A Tale of Different Capital Ratios: How to Correctly Assess the Impact of Capital Regulation on Lending

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Abstract

For almost two decades, quantifying the effect of changes in bank capital and capital regulation on lending has been one of the most important research questions. Yet, the empirical literature has remained largely fragmented in terms of the estimated parameters. In this paper, we collect more than 1,600 estimates on the relationship between bank capital and lending and construct 40 variables that reflect the context in which researchers obtain such estimates. After accounting for potential publication bias, the effect of a 1 percentage point (pp) change to the capital (regulatory) ratio on annual credit growth is set at around 0.3 pp, while the effect of changes to capital requirements is about -0.7 pp. Using Bayesian and frequentist model averaging, we expose the additional layers of fragmentation observed in our results. First, we show that the relationship between bank capital and lending changes over time, reflecting the post-crisis period of increasingly demanding bank capital regulation and subdued profitability. Second, we find the reported estimates of elasticities to be significantly affected by the researchers' choice of empirical approach.

Keywords: Bank capital, bank lending, capital regulation, meta-analysis, publication bias
JEL Classification: C83, E58, G21, G28

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1. Introduction

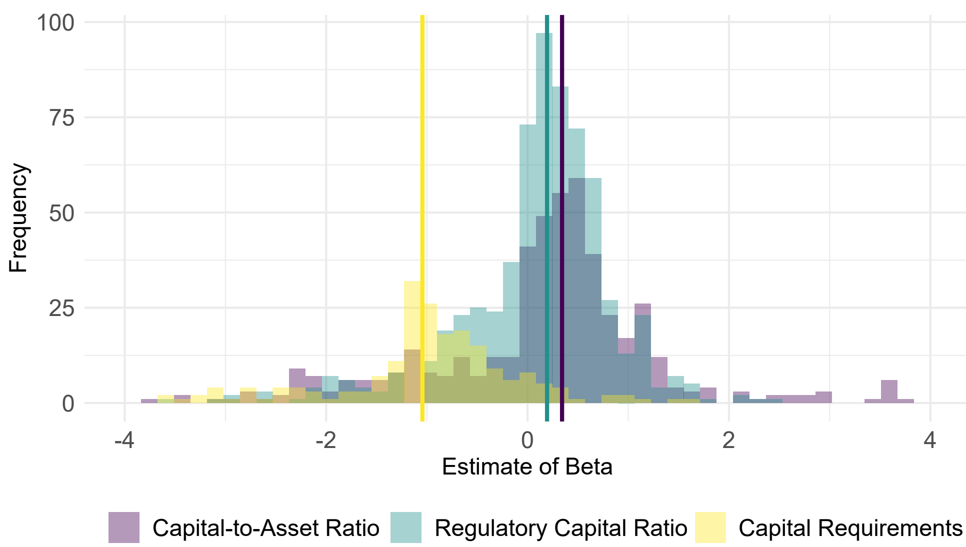
There is extant empirical literature assessing the effect of changes to bank capital on the extension of bank credit. The importance of quantifying this relationship has been one of the most pivotal research questions for almost two decades. The topic was given particular attention following the onset of the 2007–2009 Global Financial Crisis (GFC), when the likelihood of a credit crunch was under debate and again when the first quantitative easing programs were gradually implemented. The question has reemerged more recently with the gradual implementation of Basel III and an increasing use of macroprudential policy instruments. Following the implementation of Basel III, the observed minimum capital requirements effectively rose from 8% to 10.5%. However, due to all the additional prudential buffers, the capital requirements were able to reach as much as 20% (BCBS, 2010). It is thus not surprising that the current research concerning the relationship between bank capital and lending has shifted towards assessing the effects of capital regulation on bank lending capacity.

Yet, the literature has not been entirely successful in rigorously quantifying this relationship and, rather worryingly, has displayed significant fragmentation in terms of the estimated coefficients (Figure 1). In principle, there is a wide range of possible outcomes when quantifying the impact of changes in bank capital on bank lending. On the one hand, an increase in the bank capital ratio due to the introduction of a new capital regulation may dampen bank lending activities as a bank would try to avoid the higher costs of financing loans by capital. In this context, those banks that maintain a constant capital ratio in particular may experience a "capital crunch" (Bernanke et al., 1991; Hancock and Wilcox, 1993). On the other hand, a general increase in the bank capital (equity) ratio due to, for example, bank profit accumulation should be reflected in an increase in lending, suggesting a positive elasticity (Berrospide and Edge, 2010b).

A conspicuous feature of the literature on the relationship between bank capital and lending is that the bank capital ratio may change for various reasons, ranging from regulatory (Peek and Rosengren, 1997; De Jonghe et al., 2020) to economic and managerial (Houston et al., 1997; Berrospide and Edge, 2010b; Gambacorta and Marques-Ibanez, 2011a). This aspect affects the practical importance of these studies for policymakers, who are primarily concerned with the effects of capital regulation. In fact, very few studies capture the "pure" effects of changes in bank capital due to a newly endorsed capital regulation. Those few rely mostly on (semi)natural experiments, while the vast majority of studies rely on more or less precise identification strategies.

In this paper, we aim to explain the prevailing fragmentation of the empirical literature on the effects of changes to bank capital on credit dynamics. To this end, we collect more than 1,600 estimates of the relationship between bank capital and lending from 46 papers. To explain the differences, we collect an additional 40 variables that reflect the context in which the estimates were produced. The newly created database allows us not only to derive an "average" effect but also to explain why the estimates vary across different studies and to describe what the most commonly employed empirical strategy is. We use state-of-the-art meta-analytic techniques to estimate the true effect of changes to bank capital on bank lending, as well as the model averaging methods used to identify the significant drivers of the heterogeneity of the observed estimates.

Figure 1: Distribution of the Collected Estimates



Note: The figure depicts histograms of the collected estimates on the effect of a 1 pp increase in capital ratio on bank annual credit growth (see equation 1). The regulatory capital ratio represents the ratio between regulatory capital (Common Equity Tier 1, Additional Tier 1 and Tier 2 items) and risk-weighted exposures. Capital requirements represent the ratio between various categories of capital requirements (minimum, Pillar 2 add-ons and capital buffers) and risk-weighted exposures. The solid vertical lines indicate the median. For ease of exposition, extreme outliers are excluded from the figure but included in all the statistical tests.

In taking a panoramic view of the estimates collected, we expose three stylized facts: First, the majority of estimates of the relationship between bank capital and lending regresses credit growth on the level of the capital ratio and a set of controls. Given the variable transformation, the reported results represent semi-elasticity with a 5% confidence bound surrounding the mean value ranging from -2.61 to 1.68. Second, it appears that the large variance in the reported results can seemingly be well explained by the researcher's initial choice regarding how to express the bank capital ratio. We find that using a simple capital-to-assets ratio generates on average positive estimates of the relationship between bank capital and lending. Conversely, employing the regulatory (risk-weighted) capital ratio or capital requirements generates estimates skewed towards more negative values. Third, the strength of the estimated relationship between bank capital and lending decreases over time.

The meta-analytic techniques allow us to estimate the effect beyond bias, sometimes referred to as the true effect. The collected estimates imply that a 1 pp increase in the simple capital-to-asset ratio is associated with a 0.3 pp increase in credit growth. Considering the regulatory capital ratio, the average estimated elasticity decreases to about 0.2 pp. Neither of these two exhibits signs of publication bias and thus the estimated true effect is very close to a simple average across the collected elasticities. In stark contrast, correcting for publication bias shrinks the mean elasticity on capital requirements from -1.4 pp to about -1 pp.

Next, our findings indicate that various study characteristics are systematically associated with the reported results. Among the 40 variables we construct, the most important for model averaging are those related to data, the estimation technique and cross-country or regional differences. Specifically, we find that single-country studies with larger sample sizes exhibit a positive correlation with the collected elasticities, while studies shielded from omitted variable bias with more favorable publication characteristics are generally negatively correlated with the reported estimates. Apart from data characteristics, estimates of the effect of changes to the simple capital-to-asset ratio are also found to be dependent on the variables reflecting the macro-financial characteristics of the countries analyzed. The heterogeneity in the estimates based on the regulatory capital ratio can thus mostly be explained by model specification. In the case of the literature on capital requirements, the standard error is the most important variable in terms of explaining the variation in the reported estimates. Large standard errors are associated with more negative estimates, supporting the existence of publication bias in this category.

Finally, we use the information obtained from the collected studies and the heterogeneity analyses to compute the mean elasticity of the relationship between bank capital and lending based on the design of the most reliable studies. Using this, we attempt to show what the mean elasticity would be if all studies used the same strategy as our preferred approach. Given its high policy relevance, we favor such primary study characteristics that represent a consistent and unbiased estimator and could better capture changes in bank capital due to capital regulation rather than other factors. We find that elasticities implied by significant heterogeneity drivers are distinctly positive for the simple capital-to-asset ratio and negative for the regulatory capital ratio. More specifically, the implied elasticity of changes to the simple capital-to-asset ratio on bank lending is estimated at 1.8 pp, while the implied elasticity of changes to the regulatory bank capital ratio on bank lending is estimated at -0.7 pp. Interestingly, both elasticities turn negative when a prolonged period of low interest rates is considered.

Our paper relates to two strands of literature. First, this paper contributes to the broader empirical literature on the effects of changes to bank capital on the financial sector. The literature has focused predominantly on testing the implications of the bank capital level on the probability of crises (Demirguc-Kunt et al., 2013; Jordà et al., 2021) and finding the optimal capital level (Miles et al., 2013; Thakor, 2014; Schwert, 2018). Our paper provides a novel (and, to the authors' knowledge, the first) comprehensive synthesis of the empirical literature on the relationship between bank capital and lending. Previous related studies include VanHoose (2007) and Kashyap et al. (2010) who provide a narrative review of the theoretical and empirical literature. Second, our paper also contributes to the emerging literature on the effects of bank capital regulation on the real economy and the financial sector. Two recent meta-analytic studies include Araujo et al. (2020) who estimate the average effects of macroprudential policy on bank credit, house prices and the real economy, and Fidrmuc and Lind (2020) who present a meta-analysis of the impact of higher capital requirements on macroeconomic activity. In both cases, the meta-analyses are performed on a set of studies that rely on various dummy-coded indices capturing changes to bank capital regulation. A typical shortcoming associated with the dummy approach is the inability to actually quantify the effects of regulatory policies which is generally a key issue for policymakers (Alam et al., 2019). In our paper, we opt for the time-series approach where we select only those papers capturing continuous changes to bank capital.

We perceive the contribution of this paper to be threefold. First, quantifying the effect of changes to bank capital on the supply of credit is of utmost importance to policymakers. Obtaining a comprehensive overview of the findings of the literature goes well beyond the scope of individual studies that are, by nature, very selective. Second, we show the caveats associated with modelling the relationship between bank capital and lending as well as inform about the most commonly employed practices. Third, we present some indications that the relationship is changing over time and discuss the implications this would have for correctly estimating and assessing the impact of capital regulation.

The remainder of this paper is structured as follows: Section 2 introduces the interplay between bank capital, capital requirement and bank lending. Section 3 describes how we collect data from primary studies. Section 4 tests for publication bias and estimates the effect beyond bias. Section 5 explores the heterogeneity of the estimated elasticities and Section 6 concludes.

2. Bank Capital, Capital Requirements and Lending

The level of capital is central to bank lending decisions, at least under the conditions of an imperfect market for bank equity and the existence of minimum capital requirements. Consider a well-capitalized bank with access to additional sources of capital. Such a bank will be able to accommodate any capital losses without having to reduce its assets (and hence its lending). Now consider the polar case where a bank actively manages its portfolio in order to maintain a constant capital ratio. For such a bank, with an observed capital ratio of 8%, a dollar reduction in capital would lead to a \$12.5 reduction in its assets, including loans. Raising the bank capital ratio to 10.5% – the minimum level under Basel III – should therefore lead to a \$9.5 reduction in assets during, let's say, times of crises, i.e. a reduction of almost 24%. This may well be the longer-term effect of raising the bank capital level. How much we would deviate from this idealized scenario in real terms and what the intermediate effect of increasing the bank capital ratio would be is, of course, an empirical question, and not an easy one, as the reason behind the increase in the bank capital ratio is often not directly observable.

The fact that the reasons behind changes in the bank capital ratio may vary largely complicates the empirical efforts to measure the effects of changes to bank capital on bank lending. With a reasonable degree of simplification, bank capital may change due to a regulatory change (e.g. a change in the minimum capital requirements) or for any other managerial or economic reason. A large body of research is focused on the former, i.e. bank behavior under capital regulation which cannot be removed from the relationship between bank capital and bank lending. In fact, early studies date way back to the 1990s when Basel I was introduced. Back then, many observers debated whether the newly introduced capital regulations were inhibiting lending (Bernanke et al., 1991; Hancock and Wilcox, 1994; Berger and Udell, 1994). This issue was reinstated following the most recent Basel Accord (Basel III), with the debate shifting to the costs associated with stricter capital requirements as compared to the benefits arising from greater financial and macroeconomic stability (Beltratti and Stulz, 2012; Berger and Bouwman, 2013; Thakor, 2014).

With the ever-increasing use of both micro- and macro-prudential policy instruments, it is no wonder that researchers are urged to incorporate the regulatory constraints faced by banks in their estimates of the relationship between bank capital and lending. In fact, macroprudential policy, which is aimed at ensuring financial sector stability and resilience, has slowly gained prominence as a third economic policy. To achieve its goal, (macro)prudential policy has several tools at its disposal. For banks, to which such policy most commonly applies, these tools include capital- and borrower-based measures. Capital-based measures have been frequently used in both advanced and emerging market economies. Their importance and frequency of use increased significantly following the emergence of the GFC (Cerutti et al., 2017a; Alam et al., 2019). Capital-based measures encompass capital requirements aimed at increasing the loss-absorbing capacity of banks and the overall financial sector resilience to shocks of a different nature. By altering banks' funding costs, capital-based measures may also affect credit intermediation by banks. While there is no official macroprudential policy target, the immediate focus on credit dynamics is justified by the well-documented fact that credit booms typically precede crises (Jordà et al., 2011; Schularick and Taylor, 2012).

A regulatory shock to the bank capital ratio is expected to be decisive for bank lending decisions if two conditions hold true: First, a bank – in response to heightened capital requirements – changes its funding structure in favor of equity. A contrasting case would be a bank holding capital well in excess of the minimum capital requirement, i.e. one which maintains a "capital surplus". Under such circumstances, increasing additional capital requirements may have a limited effect on the capital adequacy ratio of a bank simply because it would use the extra capital, thus shrinking the surplus.[2] Second, the observed change in the structure of bank funding increases bank funding costs. This builds on the presumption that equity is more costly than debt which is a generally accepted condition (Kashyap et al., 2010).

There is widespread agreement in the theoretical academic literature that the immediate effects of constraining capital standards are likely to be a reduction in total lending (VanHoose, 2007). Empirical evidence is far more inconclusive. This stems from the several perils associated with the modelling of the relationship between capital and lending. For one, there are the identification issues. Since many capital-based regulatory measures are taken in response to developments in the financial sector, researchers are always running on edge with the risk of reverse causality that might bias their estimates. Regulatory actions may well coincide with faster growth in lending due to the focus of the policy itself. And even in the literature exploring the relationship between bank capital and lending beyond the regulatory cap, it is hard to tell if a change in capital is causing a change in lending, rather than reflecting it. During an economic recession, banks generally record larger losses on their existing loan portfolios, and this ultimately reduces their capital stock. Needless to say, during a recession, there are worse lending opportunities too. Thus, even in this strand of literature, it is the goal of researchers to make sure that

[2]In fact, we can assume than even a bank holding a capital surplus would change its lending behavior following a regulatory tightening. A bank faces internal or implicit costs of funds, which are set on a consolidated basis. Further, a bank often sets up internal capital ratio targets above the minimum level dictated (Berrospide and Edge, 2010b).

their estimates are purged of these bias-inducing effects. The other broad challenge is to separate changes in bank capital due to a change in capital regulation from changes stemming from economic conditions or management decisions. As we outlined in this section, the source of the change in bank capital can be decisive for bank lending, with the two likely going in the opposite direction.

In the current strand of literature, the most valued studies use "natural experiments" where a shock to bank capital is perceived as exogenous and uncorrelated with any lending opportunities. For instance, Peek and Rosengren (1997) exploit a regulatory change concerning the US branches of Japanese banks to identify how shocks to capital impact loan supply. Nowadays, these natural experiments are often backed-up by detailed credit registry data which allow us not only to identify exogenous shocks to capital but also to dismantle credit supply/demand movements (see, for example, De Jonghe et al., 2020). Another approach to dealing with endogeneity issues is to separate banks according to different characteristics and compare the sub-samples. In this respect, Bernanke et al. (1991) is representative of the many studies that compare different groups of banks to assess the importance of capital shocks on lending. Other studies typically divide banks according to their capital level or capital surplus and hypothesize that banks with a low level of capitalization will base their lending decisions more on changes in capital. Hancock and Wilcox (1993) and Hancock and Wilcox (1994) have conducted prominent studies in which they estimate bank capital target functions and find significant correlations between capital relative to its target and subsequently to bank lending.

3. Collection Process and Formation of the Dataset

The main purpose of this paper is to explore the effect of bank capital and capital regulation on lending. As such, we do not limit our analysis to the relationship between capital requirements and loan supply, but also explore the impact of overall bank capitalization, both risk-sensitive and insensitive. Understanding the role of bank capitalization is integral to correctly assessing and anticipating the transmission of additional capital requirements. Our general knowledge of the existing literature on this topic and the first bird's-eye view of some prominent studies gave us the impression that we would potentially face significant heterogeneity in the variable definition, their transformation and the identification strategy. Therefore, we decided to provide a comprehensive overview which would inform the reader not only about the true effect but also about the predominant model specification, including the role of the variable definition, data characteristics and the researchers' preferred estimation approach.

In our selection procedure, we considered all the empirical studies involving some form of bank capital or capital requirements on the right-hand side of the relationship and lending on the left-hand side, regardless of the variable transformation. However, we were pleasantly surprised that the majority of collected estimates (85%) was based on the same transformation – credit growth and the level of capital ratio (see Table 2). Even in this category we encounter some heterogeneity, especially with respect to the definition of the capital ratio, which we explore further in the paper. Nevertheless, one prevailing variable transformation allows us to directly quantify the effect of changes to bank capital on lending which provides a significant benefit compared to similar meta-analytic studies relying on dummy-coded macroprudential indices. Such findings will enable us to draw more convincing conclusions, not only on the true direction of the analyzed effect but also on its true size.

As a result, the estimated elasticities $\hat{\beta}$ entering the analysis in sections 4 and 5 refer to the following equation:

$$\%\Delta L_{it} = \hat{\beta} CR_{it} + \gamma X_{it} + \epsilon_{it} \tag{1}$$

where $\%\Delta L_{it}$ is annual credit growth, CR_{it} is bank capital ratio and X_{it} is a vector of control variables for time t and unit i (country or bank).

In the absence of a "unified policy function", the literature includes several variations of equation 1, involving a different type and definition of credit variable or capital ratio, a different set of control variables or a different estimation approach. For instance, our sample studies consider three different types of capital ratios: a simple capital to asset ratio (bank capital over total assets), a regulatory capital ratio (Common Equity Tier 1, Tier 1 and Tier 2 capital over risk-weighted exposures), and capital requirements (capital requirements over risk-weighted exposures). We use state of the art meta-analytic techniques to construct summaries of the estimated elasticities, aiming to verify the presence of publication bias, as well as to explain why the estimates may vary.

3.1. Paper Selection Procedure

Similarly to other meta-analyses in economics, we searched for the most relevant primary studies via Google Scholar using the following query:

> "bank capital regulation" OR "capital requirements" OR "bank capital" OR "capital surplus" OR "capital ratio" OR "macroprudential regulation" OR "macroprudential policy" AND "lending" OR "credit" OR "loans"

We limit our search to studies published in 2010 and later. This is to account for the fact that capital regulation has been used more extensively since the GFC, plateauing around 2012 (Alam et al., 2019). We scan the first 300 papers returned in the search results. After initial screening, we expand our search by investigating the references from the relevant studies.[3] The most recent study was added in November 2020 when we concluded our search.

Table 1: Journal Articles and Working Papers Included in the Meta-analysis

	Journal articles		Working papers	Do they differ?
1	Aiyar et al. (2014a)		-	-
2	Aiyar et al. (2016)	1	Aiyar et al. (2014b)	Y (M)
3	Akram (2014)	2	Akram (2012)	Y (M, P)
4	Auer et al. (2018)		-	-
5	Berrospide and Edge (2010b)	3	Berrospide and Edge (2010a)	Y (M)
	-	4	Berrospide et al. (2016)	-
	-	5	Bridges et al. (2014)	-
6	Brei et al. (2013)	6	Brei et al. (2011)	Y (A)
7	Buch and Prieto (2014)	7	Buch and Prieto (2012)	N
8	Carlson et al. (2013)	8	Carlson et al. (2011)	Y (M, P)
9	Cohen and Scatigna (2016)	9	Cohen (2013)	Y (C)
10	De Jonghe et al. (2020)	10	De Jonghe et al. (2016)	Y (M)
11	Deli and Hasan (2017)	11	Deli et al. (2017)	N
	-	12	De Nicolò (2015)	-
12	Drehmann and Gambacorta (2012)		-	-
	-	13	Galac et al. (2010)	-
13	Gambacorta and Marques-Ibanez (2011a)	14	Gambacorta and Marques-Ibanez (2011b)	N
14	Gambacorta and Shin (2018)	15	Gambacorta and Shin (2016)	N
15	Huang and Xiong (2015)		-	-
16	Imbierowicz et al. (2018)	16	Kragh and Rangvid (2016)	Y (M, P)
	-	17	Joyce and Spaltro (2014)	-
	-	18	Kanngiesser et al. (2017)	-
	-	19	Kolcunová and Malovaná (2019)	-
17	Kim and Sohn (2017)		-	-
18	Košak et al. (2015)	20	Košak et al. (2014)	N
	-	21	Labonne and Lamé (2014)	-
	-	22	Lambertini and Mukherjee (2016)	-
19	Malovaná and Frait (2017)		-	-
20	Meeks (2017)		-	-
21	Mésonnier and Stevanovic (2017)	23	Mésonnier and Stevanovic (2012)	Y (M, P)
22	Mora and Logan (2012)	24	Mora and Logan (2010)	Y (M)
23	Naceur et al. (2018)	25	Naceur et al. (2017)	Y (A)
24	Noss and Toffano (2016)	26	Noss and Toffano (2014)	N
	-	27	Olszak et al. (2014)	-
25	Roulet (2018)		-	-
	-	28	Wang and Sun (2013)	-
26	Watanabe (2010)	29	Watanabe (2006)	N

Note: Y/N – journal version and working paper do/do not differ; M – journal version and working paper use different model or methodology; P – the versions differ in time period examined; C – different number of countries is studied. Estimates that differ between journal article and working paper enter the meta-analysis; A – additional estimates that are not reported in the working paper; in this case only journal articles enter the analysis. If the estimates are the same, they enter the meta-analysis only once. Hence, the final set of studies comprises 26 journal articles and 20 working papers (29 working papers minus 7 that do not differ from the journal version, minus 2 that include fewer estimates than the journal version).

We collected 1,639 estimates from 46 studies[4], encompassing both articles published in refereed journals and working papers (see Table 1).[5] We identified several studies whose

[3]We screen all the references from the selected studies to identify additional relevant studies ("snowballing" method).

[4]Our sample size is comparable to similar meta-analyses. For example, Fidrmuc and Lind (2020) employed 312 estimates out of 48 primary studies on the effect of capital regulation on macroeconomic activity while Araujo et al. (2020) worked with 58 primary studies and more than 6,000 estimates in their extensive study of the effects of macroprudential policies on credit, household credit, and house prices.

[5]Besides the estimates and corresponding standard errors, t-statistics, p-value or confidence intervals, we collected about 40 other primary study characteristics. First, the data were collected and cross-examined by two of the co-authors of this paper. Afterwards, the two other co-authors cross-checked the whole dataset

journal and working paper version differ in some aspects. Therefore, we collected both versions for each article when available and indicated the reason for the difference (data coverage, model specification or estimation technique). Only unique estimates then entered our final analysis.

Each primary study included in our final data set meets two other criteria. First, the study reports some measure of uncertainty of its estimates (standard error, t-statistics, p-value or confidence intervals). Second, changes to bank capital ratios are measured continuously. For example, we do not consider studies using categorical or discrete variables to capture changes to bank capital ratios stemming from a regulatory shock. We are interested in the sensitivity of the credit dynamics to changes in the capital ratio in real terms, not in the frequency of use of the regulatory (prudential) policy (Cerutti et al., 2017a) or its direction (Cerutti et al., 2017b; Akinci and Olmstead-Rumsey, 2018).[6] This is one of the main distinctions between our paper and Araujo et al. (2020), who perform meta-analysis relying on studies using various dummy-coded macroprudential indices. The dummy-coding of policy actions does not allow for an estimation of the quantitative effects of policies which is generally a key issue for policymakers (Alam et al., 2019). The study selection path is captured in the Appendix in Figure A1.

There are ten articles in our sample which report impulse response functions instead of regression elasticities. In these cases we recover the numerical estimates and their confidence intervals via pixel coordinates. Specifically, we collect the immediate response, the effect after one period of time and the maximum response to the capital ratio shock. Following Fidrmuc and Korhonen (2006), we then treat each response collected as an estimate and differentiate between the contemporaneous, lagged and maximal effect by employing the corresponding dummies.

As mentioned earlier, the majority (85%) of the estimates collected refers to one variable transformation of the relationship between bank lending and capital, that is the growth-ratio transformation. The remaining estimates are scattered between different transformations, ranging from growth-change or log-ratio to log-log, which makes a direct comparison impossible. In order to be able to summarize the results from all the primary studies collected, we use the partial correlation coefficients (PCCs) method to calculate

in several rounds to identify systematic or idiosyncratic errors and to ensure the consistency of the whole dataset.

[6] Existing cross-country databases use an almost predominantly dummy-coding approach which lacks information on the intensity of the change. A 2 percentage point change of say the counter-cyclical buffer is effectively treated at par with a 0.25 pp change. Notable exceptions of databases which capture changes to the regulatory policy continuously include Vandenbussche et al. (2015) and Richter et al. (2019); Alam et al. (2019) for loan-to-value (LTV) limits.

standardized effect sizes. By doing so, we can compare the partial correlations between lending and capital from all primary studies, controlling for the potential impact of the different variable transformations preferred by the researchers (see Table 2). Summary statistics across different groups of PCCs show that the definition of capital ratio (i.e. the comparison between the simple capital-to-asset ratio, the risk sensitive capital regulatory ratio and capital requirements) plays a more significant role in the mean effect and its distribution than the preferred variable transformation.

Using the PCCs method has one important caveat. Specifically, we would lose the information about the true size of the effect. Thus, in what follows, we focus only on the estimates stemming from the prevailing growth-ratio transformation which allows us to distil the true effect and quantify its size. We believe that this approach will be of more use in the policy-making process. The growth-ratio specification was used in 32 studies encompassing a total of 1,395 estimates. Given the variable transformation, the estimated relationship shows a unit change in the capital ratio leading to a β-sized percentage point change in credit growth or, in other words, the semi-elasticity of credit growth with respect to the bank capital ratio.[7]

Table 2: Partial Correlation Coefficients for Different Variable Transformations and Capital Ratios

	Obs.	Articles	Mean	Median	5%	95%	Skewness
Total	*1,639*	*46*	*0.004*	*0.005*	*-0.106*	*0.114*	*-0.20*
Variable transformation							
Credit growth $\sim \beta \times$ capital ratio	1,395	32	0.008	0.010	-0.098	0.090	-0.03
Other transformations	244	15	-0.024	-0.045	-0.283	0.213	0.28
Different capital ratios (all transformations)							
Capital-to-asset ratio	559	20	0.023	0.016	-0.083	0.153	0.69
Regulatory capital ratio	710	22	0.000	0.016	-0.112	0.088	-1.59
Capital requirements	337	9	-0.029	-0.009	-0.114	0.012	-1.34

Note: The partial correlation coefficient (PCC) from i^{th} estimate of the j^{th} study can be derived from the t-statistics of the reported estimates and residual degrees of freedom: $PCC_{ij} = t_{ij}/\sqrt{(t_{ij}^2 - df_{ij})}$. The regulatory capital ratio represents the ratio between regulatory capital (Common Equity Tier 1, Additional Tier 1 and Tier 2 items) and risk-weighted exposures. Capital requirements represent the ratio between various categories of capital requirements (minimum, Pillar 2 add-ons and capital buffers) and risk-weighted exposures. Some articles include multiple different variable transformations and capital variables; therefore, the sum of articles across different categories reported in the third column exceeds the number of primary studies included. In addition, some articles use the level of capital (not expressed in relation to banks' assets or risk-weighted exposures); therefore, the sum of observations across different capital ratios in the second column is lower than the total number of collected estimates.

We need to make a few more adjustments to render the estimated elasticities and corresponding standard errors comparable. First, we calculate the standard errors when t-statistic, p-value or confidence intervals are provided. Second, we adjust the elasticities and corresponding standard errors to reflect annual changes. For example, if the elasticity

[7] Suppose, for instance, that β equals -1.5. Then, a 1 percentage point increase in capital ratio (say from 10% to 11%) is associated with a decrease in credit growth of 1.5 percentage points.

refers to a non-annualized quarterly change in credit, we multiply it by four. Likewise, we multiply the standard error. Third, we divide the cumulative effects by the respective number of periods. Fourth, some model specifications contain interaction terms with the respective capital ratio or additional lags of the capital ratio. We approach these elasticities in the same way as the other, i.e. “stand-alone” elasticities. We define respective dummy variables and analyze their significance in the model averaging exercise to control for the potential heterogeneity introduced by employing interaction terms or additional lags (see section 5).[8] All elasticities are expressed in percentage points.[9] Lastly, a few extreme outliers appear in the dataset, and we thus winsorize the estimates at the 2.5% level from each side.

3.2. Early View of the Fragmentation

A bird’s-eye view of the collected elasticities (Table 3 as well as Figure A2) suggest four stylized facts. First, estimates on the relationship between bank capital and lending vary substantially, ranging from positive to negative values with a mean elasticity of -0.1 but with median elasticity of 0.15. For example, a central banker, wishing to incorporate the bank capital-lending elasticity into a stress-testing framework, would have a difficult job finding the “correct” elasticity value. The increased variance in the estimated elasticities provides solid ground for a systematic evaluation of the published results which is shown in the next two sections.

Table 3: Breakdown into Categories of Different Capital Ratios

	Obs.	Articles	Mean	Median	5%	95%	Skewness
Total	*1,395*	*32*	*-0.093*	*0.149*	*-2.269*	*1.538*	*-2.72*
Capital-to-asset ratio	514	17	0.345	0.342	-2.221	3.794	0.60
Regulatory capital ratio	652	18	0.138	0.194	-1.383	1.114	0.10
Capital requirements	229	5	-1.737	-1.044	-8.926	0.302	-2.58

Note: The table presents summary statistics of the collected estimates on the effect of a 1 pp increase in capital ratio on bank annual credit growth (see equation 1) winsorized at the 2.5% level from each side. Some articles include multiple different capital ratios; therefore, the sum of the articles across the different categories reported in the third column (the sum of 17, 18 and 5) exceeds the total number of primary studies included (32). The regulatory capital ratio represents the ratio between regulatory capital (Common Equity Tier 1, Additional Tier 1 and Tier 2 items) and risk-weighted exposures. Capital requirements represent the ratio between various categories of capital requirements (minimum, Pillar 2 add-ons and capital buffers) and risk-weighted exposures.

Second, differences in the elasticity values can seemingly be well explained by the researcher’s initial choice on how to express the bank capital ratio (CR_{it}). Table 3 makes it

[8]We consider only interaction terms with dummy variables where the effect can be easily separated. We do not include elasticities linked to interaction with continuous variables since we would not be able to separate the effect of changes in capital ratio from changes in the continuous variable.

[9]We collected information on units in which variables were expressed from the primary studies which we then used to correctly transform the elasticities.

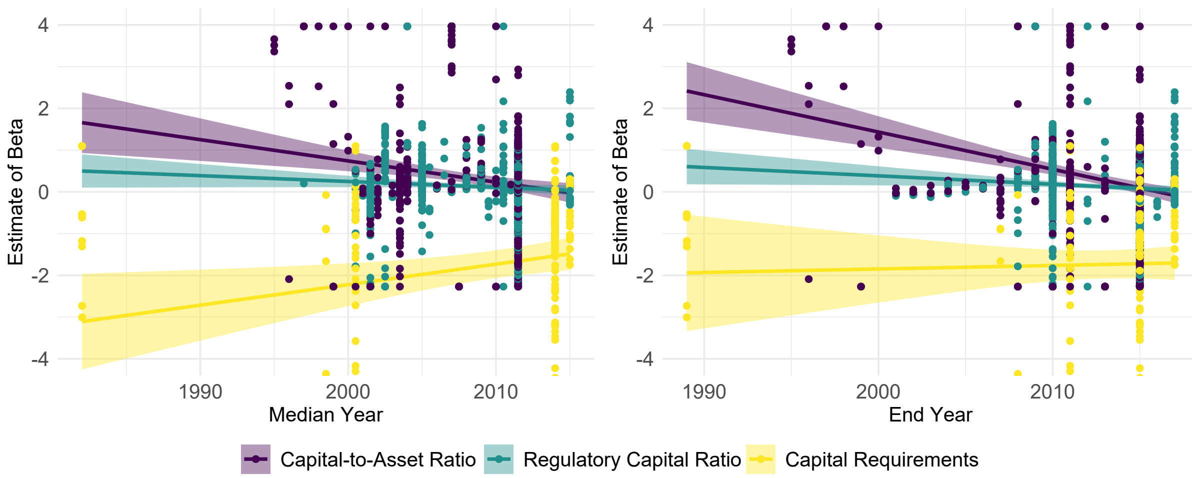
apparent that the use of a capital-to-asset ratio generates elasticity estimates skewed towards the positive spectrum of the elasticity distribution. The mean elasticity value comes in at 0.35. A simple capital-to-asset ratio is generally used to capture banks' capital position (capitalization) and therefore, a positive effect on bank lending is expected. In stark comparison, using capital requirements generates negative elasticity estimates centered around a mean value of -1.74. We collected estimates from five studies on capital requirements which mostly examine the impact of Pillar 2 capital requirements but also the changes to overall capital requirements, including various capital buffers (see Table A1). Considering regulatory capital ratios (e.g. the Tier 1 Basel regulatory ratio) then generates mean elasticity estimates of 0.14 which are slightly more skewed to negative values than a simple capital-to-asset ratio. Apparently, studies where the capital ratio takes into account the riskiness of banks' operations are more likely to capture the effects of changes to bank capital under capital regulation where a negative effect can be expected.

Third, it might be surprising that even when considering changes to Basel regulatory ratios, the literature does not paint a clear picture of the effects on bank lending. This could be due to several factors that might be in play. First, banks can change their actual capital ratio quite frequently and for various reasons that are not necessarily linked to changes in the capital regulation (see, for example, Guidara et al., 2013; Almazan et al., 2015; Bahaj et al., 2016). Thus, estimates based on observed capital ratios are noisier indicators of what regulatory changes may imply than estimates based on regulatory requirements. Second, banks typically hold capital in excess of what is required by the regulator (capital surplus), the level of which has been shown to be decisive for the response of bank lending to a shock to capital (Berrospide and Edge, 2010b). It is therefore less likely for a shock to the bank capital ratio to be binding for a bank with a high capital surplus, simply because it would use the extra capital and allow the capital surplus to shrink (Kolcunová and Malovaná, 2019).

Fourth, the reported elasticity follows an interesting pattern in time (Figure 2). Studies incorporating more recent data tend to report elasticity estimates at closer to zero. This is found to be true for both groups of studies, i.e. those using a simple capital-to-asset ratio and those with a regulatory capital ratio. One possible explanation would be that shocks to bank capital are getting smaller through time. In another words, a one unit change to a bank capital ratio (observed or regulatory) demands a lower response in terms of bank lending. In case of a regulatory shock, this would mean that it was historically more costly for banks to raise external equity. Studies performed on earlier datasets would thus represent the upper bound of the possible effects of regulatory shocks on bank capital. Another explanation is that we see the empirical manifestation of a "model risk", stemming from the alleged procyclicality of the Basel accords (Le Leslé and Avramova, 2012). One of the new features of banking regulation under the latest Basel III is that banks can determine risk-weights themselves, using internal models (IRB) rather than pre-set values for a given asset class. Model risk posits that under the IRB, the risk-weights are smaller and more pro-cyclical. Linked to the reported elasticity, the existence of a model risk may weaken the effects of capital regulation. Yet another explanation would be that bank regulators have exploited the phasing-in of the new capital requirements more often, giving banks enough time to take the newly required capital out of retained earnings instead of cutting back on lending. In case of a shock to bank capitalization, the Basel II and III accords have led to a general increase in bank capital levels and surpluses. A negative shock to bank capital is thus expected to demand a lower response rate in terms of lending dynamics.

In the analyses to come, we focus on each of the capital ratio expressions separately given their directly observable differences.

Figure 2: Reported Estimates Change Over Time



Note: The figure depicts a scatter plot of the collected estimates on the effect of a 1 pp increase in capital ratio on bank annual credit growth (see equation 1) relative to the median (left panel) and end (right panel) year. The median and end year are calculated for each primary study and capital ratio based on the time period of the data sample used in the estimation. The regulatory capital ratio represents the ratio between regulatory capital (Common Equity Tier 1, Additional Tier 1 and Tier 2 items) and risk-weighted exposures. Capital requirements represent the ratio between various categories of capital requirements (minimum, Pillar 2 add-ons and capital buffers) and risk-weighted exposures. For ease of exposition, extreme outliers are excluded from the figure but included in all the statistical tests.

4. Publication Bias

Publication bias occurs when there is a systematic difference between the distribution of the results produced and those reported by the researchers. Even the best-published study in our meta-analytic data set[10], Gambacorta and Marques-Ibanez (2011a), admits that publication bias may be an issue in the literature on bank capital and lending:

> *"The coefficient on the standard capital-to-asset ratio often has an incorrect negative sign, which casts some doubt on the role of this indicator in capturing the effect of a bank's capital position on bank lending."*

On the same note, the theoretical literature seems to have set up the "correct" relationship between more stringent capital regulation and lending (VanHoose, 2007):

> *"There is widespread agreement in the theoretical academic literature that the immediate effects of constraining capital standards are likely to be a reduction in total lending."*

The findings that do not fit the generally accepted narrative, i.e. where the researcher estimates the effect to be positive or not statistically significant, may thus be sensitive to publication bias.[11] We use two well-established strategies to examine publication bias – graphical and econometric tests. Publication bias can be graphically examined by a funnel plot that relates the estimated elasticity to its precision, measured by the inverse of the estimated standard error. The interpretation is simple: if the estimated elasticities are symmetrically distributed around the mean effect, there is no publication bias and the mean is the "true" mean effect.[12]

Publication bias can be econometrically tested using a wide range of methods from simple OLS to sophisticated non-linear techniques. Regarding linear techniques, we selected four techniques commonly used in the meta-analysis literature: unweighted and weighted OLS models, fixed effects models and random effects models. Regarding non-linear techniques, we opted for the five that are considered state-of-the-art: Top 10 Method by Stanley et al. (2010), Weighted Average of Adequately Powered (WAAP) by Ioannidis et al. (2017), Selection Model by Andrews and Kasy (2019), Stem-based Method by Furukawa (2019) and Kinked

[10]We identify the "best-published" study based on the recursive impact factor and number of citations in Google Scholar.

[11]This behavior is described as the Lombard effect (McCloskey and Ziliak, 2019), originally defined within the field of biology. In economics, the effect refers to a situation where researchers need to try harder in order to achieve the estimates consistent with their intuition if the data are imprecise or noisy.

[12]A funnel plot, as a simple graphical measure of publication bias, was first proposed by Egger et al. (1997).

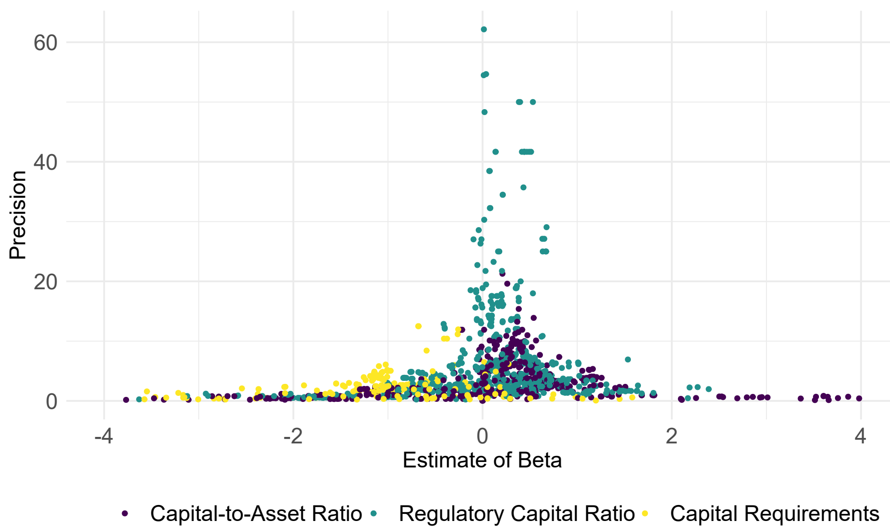
Method by Bom and Rachinger (2019). The non-linear techniques, in contrast to the linear ones, do not focus on quantifying publication bias itself but rather on estimating the effect beyond bias, often called the true effect.[13]

Linear estimation techniques are used to test for publication bias by exploiting the association between the estimated elasticity $\hat{\beta}_{i,j}$ and its standard error $SE_{i,j}$ for each study j (Stanley et al., 2013; Stanley, 2005):

$$\hat{\beta}_{i,j} = \alpha + \gamma SE_{i,j} + \epsilon_{i,j} \tag{2}$$

where α is the effect beyond bias (true effect) and γ is the intensity of publication bias. If the γ coefficient is statistically significant, publication bias is present. The underlying mean effect corrected for publication bias is then captured in the coefficient α.

Figure 3: Funnel Plot



Note: The figure depicts a funnel plot of the collected estimates on the effect of a 1 pp increase in capital ratio on bank annual credit growth (see equation 1). Precision is calculated as the inverse of standard error. The regulatory capital ratio represents the ratio between regulatory capital (Common Equity Tier 1, Additional Tier 1 and Tier 2 items) and risk-weighted exposures. Capital requirements represent the ratio between various categories of capital requirements (minimum, Pillar 2 add-ons and capital buffers) and risk-weighted exposures. For ease of exposition, extreme outliers are excluded from the figure but included in all the statistical tests.

The elasticities linked to capital requirements appear to be heavily skewed towards negative values and form an asymmetrical funnel, indicating the presence of publication bias (Figure 3). However, the funnel gives a mixed message if we look at the simple

[13]For more details on the individual non-linear techniques see, for example, Cazachevici et al. (2020) or Gechert et al. (2020). A short explanation is also provided in Table 4 below.

capital-to-asset ratio or regulatory capital ratio. In both cases, the most precise estimates are centered around slightly positive values. For simple capital-to-asset ratios, the right portion of the funnel might be a little heavier than the left one while for the regulatory capital ratio, the distribution is skewed more towards negative values. Nevertheless, the funnel plot is only a simple visual test, and the dispersion of the estimates could suggest heterogeneity in data and methods, the other systematic factor driving the estimated coefficients. To support our initial impression, we complement the visual analysis with a battery of formal regression-based tests.

Empirical tests based on linear estimation support our intuition gained from visually inspecting the funnel plot. The estimates collected on the relationship between capital requirements and bank lending show negative publication bias while we do not find any signs of significant publication bias for the two other capital ratios (Table 4). One explanation consistent with this result is that there are generally no strong *a priori* views on the direction of the relationship in the simple capital-to-asset ratio and regulatory capital ratio categories. This only highlights the need to further examine the sources of heterogeneity in the estimated elasticities, as we can rule out the problem of publication selection.

We find some evidence of publication bias in the sample of elasticities linked to capital requirements. This is not that surprising due to strong prior intuition on the potential effects of changes to capital regulation on bank lending. However, the bias estimated using linear techniques, albeit economically meaningful, is not always statistically significant. This may be due to a relatively small sample size and the potentially non-linear relationship between the collected estimates and their standard errors. The latter might speak in favor of relying on non-linear techniques which estimate the effect beyond bias, i.e. the mean effect corrected for publication bias, at between -0.5 pp and -0.8 pp. Estimates are statistically significant at 1% and are robust across the different methods employed. The uncorrected mean linked to studies using capital requirements is -1.7 which is significantly above the corrected mean, and it suggests that the effect of changing the capital requirements on bank lending might be systemically exaggerated. Nevertheless, given that this particular literature is relatively scarce due to data limitations, it is important to interpret our findings cautiously. It can be viewed as a potential area for future investigation when a greater number of studies concentrating on changes to capital requirements are available.

Table 4: Publication Bias is Negligible Even When Distinguishing Between Capital Ratios

	Capital-to-Asset Ratio	Regulatory Capital Ratio	Capital Requirements
	Panel A: Linear Techniques		
Simple OLS			
Constant (effect beyond bias)	0.311	0.091	-0.871*
	(0.197)	(0.091)	(0.330)
	[0.040, 0.908]	[-0.052, 0.314]	[-2.197, 0.410]
SE (publication bias)	0.027	0.097	-0.569
	(0.022)	(0.136)	(0.370)
	[-0.074, 0.314]	[-1.06, 0.220]	[-1.507, 0.169]
Weighted OLS (by the inverse of the standard error)			
Constant (effect beyond bias)	0.350***	0.255***	-0.593**
	(0.102)	(0.083)	(0.137)
	[0.163, 0.570]	[0.090, 0.377]	[-0.800, 0.253]
SE (publication bias)	-0.004	-0.238	-0.752**
	(0.232)	(0.328)	(0.239)
	[-0.402, 0.616]	[-0.729, 0.463]	[-1.194, -0.015]
Study-level fixed effects			
Constant (effect beyond bias)	0.415*	0.289***	-0.927***
	(0.213)	(0.104)	(0.307)
SE (publication bias)	-0.057***	-0.307	-0.533
	(0.017)	(0.289)	(0.343)
Study-level random effects			
Constant (effect beyond bias)	0.580	0.300***	-0.683***
	(0.386)	(0.115)	(0.196)
SE (publication bias)	0.006	0.011	-0.531
	(0.032)	(0.174)	(0.348)
	Panel B: Advanced Non-linear Techniques		
Top 10 method (Stanley et al., 2010)			
Effect beyond bias	0.252***	0.221***	-0.608***
	(0.026)	(0.028)	(0.094)
WAAP (Ioannidis et al., 2017)			
Effect beyond bias	0.263***	0.181***	-0.750***
	(0.037)	(0.023)	(0.076)
Stem-based method (Furukawa, 2019)			
Effect beyond bias	0.196*	0.021	-0.651***
	(0.107)	(0.187)	(0.082)
Kinked method (Bom and Rachinger, 2019)			
Effect beyond bias	0.287***	0.240***	-0.482***
	(0.023)	(0.013)	(0.043)
Observations	514	652	229
Studies	16	18	5
Observations per study (mean)	32	36	46

Note: **Panel A:** Standard errors, clustered at the study level, are reported in parentheses. Whenever possible, 90% confidence intervals from wild bootstrap clustering are reported in square brackets; the procedure was implemented via the *boottest* procedure in R. **Panel B:** The Top 10 Method by Stanley et al. (2010) and the Weighted Average of Adequately Powered (WAAP) by Ioannidis et al. (2017) focus on estimates with adequate statistical power. The Top 10 Method removes 90% of the least precise estimates and the WAAP method employs only those estimates whose statistical power exceed 80%. The number of observations under the Top 10 and WAAP methods was reduced to 52 and 33 for the capital-to-asset ratio subsample, 66 and 101 for the regulatory capital ratio subsample, and 23 and 36 for the capital requirements subsample. The stem-based method by Furukawa (2019) builds on a trade-off between bias (squared) and variance: the most precise estimates are the least biased and omitting estimates increases the variance. Hence optimizing this trade-off might lead to the desired "true effect" as well. The kinked method by Bom and Rachinger (2019) searches for the precision threshold above which publication bias is unlikely. * p ¡ 0.10, ** p ¡ 0.05, *** p ¡ 0.01.

5. Drivers of Heterogeneity

The empirical literature on the relationship between bank capital and lending shows a high degree of heterogeneity that goes beyond the different expressions of the bank capital ratio. While the impact of raising capital requirements on bank lending proves to be sizeable and negative, the effect of changes to the simple capital-to-asset ratio and the regulatory capital ratio remains unclear in terms of its direction and size, as we have shown earlier in Figure 1 and Figure 3. Publication bias was proved not to explain this large variation.

Meta-analytic studies usually have to deal with some heterogeneity of the collected estimates. In principle, the heterogeneity should be low if the subject of the meta-analysis is a deep (structural) parameter obtained from a model that correctly describes the data generating process. In such a case, some heterogeneity can be driven by an econometric approach to estimating the model. However, if the subject of the meta-analysis is a reduced-form parameter or the model does not correctly follow the data generating process, the heterogeneity of the collected estimates is expected to be high and driven by data characteristics and model specification. The latter is a case for modelling bank behavior, suggesting that model specification and data characteristics will play an important role in the derived effect.

In this section, we control for 40 variables to better understand the differences between studies.[14] About three quarters of the variables come from the primary studies while the rest are structural (external) variables capturing cross-country or regional differences. These are usually collected from first-rate databases such as those of the World Bank, the OECD or Eurostat. We provide the description and summary statistics of all variables entering the analysis in Table A2.

Given the model uncertainty inherent in such an exercise, we use both the Bayesian and frequentist model averaging. Our goals are threefold. First, we aim to identify the aspects that are the most effective in explaining the differences among the reported elasticities. Second, we calculate a mean elasticity implied by significant heterogeneity drivers. Third, we aim to show how different model choices by researchers can influence the final estimate of elasticity.

5.1. Characteristics of the Primary Study

Data characteristics. The literature provides little prior knowledge regarding the impact of different data characteristics on the relationship between bank capital and lending.

[14] We initially collected more than 60 variables which can potentially impact the reported elasticities. However, we decided to drop about 20 of these based on the dummy variable trap and due to high correlation.

Nevertheless, the existing meta-analytic studies on monetary policy transmission identify significant discrepancies caused by different data frequency and length of the data sample (Havranek and Rusnak, 2012; Ehrenbergerova et al., 2021). Given the character of the relationship studied, we extended this set of control variables significantly. Specifically, we account for the type of credit used as a dependent variable, data frequency, the number of observations, the midpoint of the data, the region of the analysis, and data confidentiality. We also distinguish between different panel-data structures: bank-level vs macro-level and multi-country vs single-country.

Model specification and estimation. As a next step, we control for different aspects of the econometric approach used in the primary study. A number of meta-analytic studies proved that these factors play a significant role in the direction and size of the estimated elasticities (Zigraiova et al., 2021). First, we are interested in the model specification. We distinguish between a static and dynamic model[15], models with different lag structures or models missing some key control variables. We also search for more specific factors, such as the presence of additional capital variables[16] or interaction term[17]. Second, we explore the impact of different estimation techniques. As a part of that, we distinguish between specifications including time and unit fixed effects.

Publication characteristics. This group of characteristics is expected to correlate with unobserved features related to the relevance and quality of the primary study. These include the year of publication, an indication of whether the study was published in a journal, the discounted recursive impact factor, the number of citations and an indication of whether this or some other version of the study was published by a central bank. The reasons to control for these characteristics are supported by the literature. For instance, Araujo et al. (2020) find that journal articles show some signs of publication bias while non-published studies do not. Moreover, the lower the impact factor, the higher the publication bias. In other words,

[15] A dynamic model contains a lagged dependent variable, in our case credit growth.

[16] Additional capital variables included in the same estimation equation, on top of the capital ratio, may distort the relationship between bank capital and lending studied. Specifically, the presence of two related variables may result in the effects going in opposite direction, which makes it difficult to distill the correct sign and size of the relationship between capital and lending. For example, the same estimation equation may include, among other combinations, both a simple capital-to-asset ratio and regulatory capital ratio or regulatory capital ratio and capital requirements. In each case, we consider the ratio more related to the capital regulation to be our main elasticity (i.e. the regulatory capital ratio in the first example and capital requirements in the second example).

[17] By imposing interaction terms, the researcher explores heterogeneity in the effect analyzed. Distinguishing between crisis and non-crisis periods is among the most frequent interactions implemented. So far, the results in the literature on the impact of the crisis period on the relationship between bank capital and lending have been mixed. Some studies find the relationship to be strong and statistically significant only in crisis periods (Gambacorta and Marques-Ibanez, 2011a; Carlson et al., 2013; Kim and Sohn, 2017), others show quite the opposite in the same equation (Bridges et al., 2014; Naceur et al., 2018).

the effect may be overestimated in the series with lower quality. Further, Fabo et al. (2021) show that reported findings may be affected by the institution which publishes them.

Reference model. We have defined our control variables against a reference model which is based on the predominant characteristics of the primary studies. For example, we define three dummy variables capturing the estimation technique (GMM, FE and other techniques) while we treat OLS, the predominant technique, as the reference group. We take the liberty of choosing the baseline characteristics more freely when the difference between the groups is not large or when we want to make the interpretation more intuitive. The predominant characteristics can be found in Table A2.

5.2. Structural (External) Variables

On top of the primary study characteristics, we consider nine external variables capturing cross-country or cross-regional differences.[18] Some of these factors may not have been accounted for in the primary study, and therefore, may play a significant role in the heterogeneity observed among reported estimates.

First, we include four variables closely connected with the monetary policy stance: the three-month interbank rate, the spread between the ten-year government bond yield and the three-month interbank rate, a variable measuring the number of consecutive years during which interest rates are very low, and an index of central bank independence. In general, studies show that the monetary policy stance may significantly affect the relationship between bank capital and lending (Gambacorta and Marques-Ibanez, 2011a; De Jonghe et al., 2020) and that monetary and macroprudential policy are not independent, as they affect both the monetary and credit conditions via their effect on credit growth (Malovaná and Frait, 2017; Akinci and Olmstead-Rumsey, 2018; Gambacorta and Murcia, 2020). Furthermore, a prolonged period of low interest rates may lead to a build-up of financial vulnerabilities (Malovaná et al., 2020) to which the macroprudential policy has to react by tightening its stance while, at the same time, monetary policy may become less effective in stimulating credit growth (Borio and Gambacorta, 2017). In this respect, the independence of central banks promotes financial stability (Klomp and De Haan, 2009) which, in its core, underlines the overall demand for macroprudential measures and might affect their effectiveness.

Second, we employ some macroeconomic and macro-financial variables: unemployment rate, deviation of consumer inflation from its long-run trend[19] and growth in house prices.

[18]Some other potentially relevant external variables (for example, GDP growth, ratio of exports and imports to GDP, credit growth, financial development index, or measure of regulatory quality) were excluded due to high multicollinearity.

[19]We approximate the long-run trend of inflation using its mean value and we set the band around which it deviates to +1/-1 pp.

We include the first two variables to control for the overall economic conditions and the third one to capture macro-financial linkages and the average position in the financial cycle. Multiple studies document that the reaction of bank lending to changes in bank capitalization differs over the course of the business and financial cycle (Gambacorta and Marques-Ibanez, 2011a; Brei et al., 2013). Moreover, (Fitzpatrick and McQuinn, 2007; Anundsen et al., 2016) document a mutually reinforcing relationship between credit growth and house prices.

Third, we explore the impact of two additional variables linked to the character of the domestic financial system: the ratio of bank assets to GDP (as a proxy for the size of the banking sector) and an index of financial openness. De Jonghe et al. (2020) show that the larger the banking sector in terms of its assets, the more detrimental the effect of increasing bank capital on credit growth. Also, several studies find differences between the impact of capital regulation on credit between advanced and emerging market economies (Cerutti et al., 2017a; Akinci and Olmstead-Rumsey, 2018; Alam et al., 2019). They offer various explanations but one may suspect that the relative size of the financial sector could also be important. The degree of financial development and openness also affects the relationship studied. On the one hand, Deli and Hasan (2017) show that a country's financial development and openness reinforces the link between the capitalization of banks and credit growth as these countries are less constrained in raising the capital. On the other, Cerutti et al. (2017a) find evidence of weaker associations between capital regulation and credit in financially more open or developed economies.

Most external variables enter the analysis as a simple average calculated for the same time period as was employed in the primary study.[20] There are two exceptions: the low for long variable and the deviation of consumer inflation from its long-run trend. These two variables represent a number of consecutive periods in a given time frame that meet a certain criteria (see Table A2).

5.3. Estimation

Given the large number of control variables, we cannot use simple OLS to regress the collected elasticities on these variables as we would face substantial model uncertainty. To address this issue, we employ model averaging techniques, both Bayesian and frequentist. The Bayesian model averaging allows us to estimate the probability that an individual explanatory variable would be included in the underlying model. The frequentist model

[20] For example, if the elasticity of the relationship between bank capital and lending comes from a primary study employing a time period between 2000 and 2014, we calculate the simple average of the external variables for the time period between 2000 and 2014 as well. For multi-country panel data, we either used an aggregate provided by a respective database or we calculated one using single-country data series.

averaging then serves as a useful robustness check. The model averaging techniques do not reject any explanatory variable in advance as the best model approaches. This is crucial for our analysis as we aim to explain the heterogeneity of the studies.

The goal of Bayesian model averaging (BMA) is to find the best possible approximation of the distribution of each regression parameter. Our data can provide 2^{40} variable combinations to run as regressions which would be potentially very time-consuming. To cut the estimation time, we use a Markov chain Monte Carlo process with the Metropolis-Hastings algorithm which only goes through the most likely models (Zeugner and Feldkircher, 2015). The probability of each model is then turned into the respective weight and the weight of each model is captured by a measure called posterior model probability (PMP). The estimated coefficients for each variable are equal to the weighted sum of the variable coefficients through all the models based on the PMP of each model. This estimated coefficients have assigned posterior inclusion probability (PIP) representing the sum of the posterior model probabilities through all the models, where the variable is included.

BMA requires explicit priors concerning the model (model prior) and regression coefficients (g-prior). Following Eicher et al. (2011), we use a combination of unit information g-prior (UIP) and uniform model prior as a baseline. This setting expresses our lack of knowledge regarding the particular probabilities of individual parameter values as the prior assigns the same weight to the regression coefficient of zero and to the observation in the data. As a robustness check, we analyze the sensitivity of our results to different prior choices. For instance, we should account for the fact that we employ a relatively high number of explanatory variables which may succumb to collinearity even though we discarded several of them upfront. Therefore, we also employ the dilution model prior proposed by George (2010), adjusting the model probabilities by the determinant of the correlation matrix of the particular variables included in the suggested model.[21] Further, we also employ a combination of the Hannan-Quinn (HQ) g-prior and random model prior (Fernandez et al., 2001; Ley and Steel, 2009) and a combination of the BRIC g-prior and random model prior. The HQ g-prior adjusts data quality and is recommended, for instance, by Feldkircher and Zeugner (2012) or Zigraiova et al. (2021). The BRIC g-prior, which is widely used in literature, minimizes the prior effect on the results (Zeugner and Feldkircher, 2015). The use of random model priors thus means that equal prior probability is given to every model size (Gechert et al., 2020). This way we show a

[21] In case of high correlation, the determinant is close to one and the model receives little weight and vice versa. This prior was used in meta-analysis for instance by Bajzik et al. (2020).

lack of prior knowledge about the model's distribution. All robustness checks that focus on the different priors are reported in Figure B4 in Appendix B.

In the following sub-section, we interpret only the BMA means with a posterior inclusion probability (PIP) of above 0.5, following the approach proposed by Jeffreys (1961) and Havranek et al. (2021). They further divide the interpretation into the following groups: the effect is deemed weak if the PIP is between 0.5 and 0.75, substantial if the PIP is between 0.75 and 0.95, strong if the PIP is between 0.95 and 0.99, and decisive if the PIP is greater than 0.99. The Bayesian approach is our baseline estimation technique and the frequentist approach serves as a robustness check. In addition to the frequentist model averaging (FMA), we run a simple OLS regression including only variables with a PIP of above 0.5. The portion of variance of the collected estimates explained by chosen characteristics, captured by adjusted R-squared, reaches between 50% and 70%. The numerical results of FMA and frequentist checks are reported in Tables B1–B3 in Appendix B.

5.4. Results

The numerical results of BMA for all three capital ratios are shown in Table 5. The corresponding graphical output is reported in Figures B1–B3 in Appendix B, confirming the conclusions drawn from the table. We first consider two samples of studies split on whether they consider the simple capital-to-asset ratio or regulatory capital ratio. The third sample of studies, which uses capital requirements, is described as a special case bearing in mind the lower number of elasticities available and the relatively uniform estimation approach employed in primary studies.

The absence of publication bias in the estimates of the relationship between bank capital and lending is supported by evidence across the two baseline samples of the risk insensitive and risk sensitive capital ratio. The posterior inclusion probability of the standard error never crosses the threshold of 0.5 in any of the two samples which means it is not important in explaining the differences in the reported results. However, standard error plays a crucial role in explaining elasticities in our third sample on the relationship between capital requirements and lending, supporting the presence of publication bias. Furthermore, we show that approximately half of our other variables are included in the best models, and the signs of these variables are robust across specifications.

Table 5: What Drives the Heterogeneity of Collected Estimates – Bayesian Model Averaging

	Capital-to-asset ratio			Regulatory capital ratio			Capital requirements		
	P. mean	P. SD	PIP	P. mean	P. SD	PIP	P. mean	P. SD	PIP
Constant	-3.555	-	1.000	-2.947	-	1.000	-1.936	-	1.000
St. error	0.002	0.008	0.087	0.001	0.011	0.068	-0.249	0.095	0.959
Data characteristics									
No. of observations	0.262	0.045	1.000	0.245	0.024	1.000	0.199	0.080	0.946
Confidential data	6.269	0.994	1.000	-2.047	0.689	0.942			
Other region	5.312	1.091	0.999	0.538	0.508	0.663			
US	-2.910	1.257	0.962	-1.323	0.702	0.923			
Single-country	3.811	1.298	0.951	2.133	0.779	0.962			
Midpoint	-3.561	1.967	0.917	0.219	0.591	0.194	-0.074	0.178	0.207
Corporate credit	0.514	0.316	0.751	-0.156	0.150	0.691			
Household credit	-0.182	0.306	0.298	-0.020	0.113	0.186	3.596	0.595	1.000
Quarterly frequency	-0.562	1.108	0.259	2.971	0.685	0.999			
Macro-level data	-0.001	0.045	0.047	0.000	0.031	0.054			
Total credit							2.649	0.864	0.976
Model specification and estimation									
Missing control variables	3.160	0.438	1.000	0.053	0.175	0.128			
Missing interest rate in eq.	-2.574	0.463	1.000	0.467	0.186	0.916			
Other method	-7.692	2.424	0.979	-1.158	0.401	0.994			
Fixed-effects method	-1.966	0.907	0.918	-0.747	0.378	0.852			
Lagged by 1Y or more	2.127	1.131	0.826	0.481	0.298	0.794	-0.024	0.118	0.092
Contemporaneous	-0.654	1.257	0.269	0.017	0.121	0.076			
Add. lag in eq.	-0.177	1.356	0.171	2.079	0.749	0.956	0.037	0.242	0.088
Time fixed effects incl.	0.024	0.078	0.129	0.006	0.031	0.069			
Add. capital in eq.	-0.021	0.098	0.080	0.960	0.276	0.992	-0.022	0.299	0.067
Dynamic model	0.019	0.112	0.068	-0.760	0.432	0.824			
Other interaction	0.003	0.032	0.049	-0.001	0.019	0.046			
Some interaction in eq.	0.002	0.044	0.045	-0.001	0.023	0.042	-0.352	0.412	0.499
GMM method	0.002	0.038	0.045	-0.011	0.049	0.082			
Crisis	0.002	0.037	0.044	0.006	0.033	0.071			
Publication characteristics									
Published	3.316	0.771	1.000	-0.006	0.091	0.095			
Impact factor	-1.454	0.235	1.000	-0.513	0.111	0.999			
Citations	-1.684	0.621	0.989	-0.051	0.176	0.126			
Central bank publication	2.574	0.785	0.972	-0.424	0.630	0.419			
Publication year	-0.963	0.823	0.691	-0.264	0.292	0.525	-0.304	0.634	0.247
External variables									
Ext: inflation deviation	0.336	0.044	1.000	0.007	0.020	0.219			
Ext: low for long	-0.460	0.066	1.000	-0.031	0.054	0.378	-0.169	0.036	0.998
Ext: fin. openness	6.499	1.253	1.000	0.265	0.339	0.534			
Ext: bank size	-0.004	0.015	0.179	-0.003	0.007	0.218			
Ext: 3M interest rate	-0.040	0.208	0.094	0.024	0.080	0.177			
Ext: spread	0.046	0.271	0.090	0.008	0.099	0.168			
Ext: unemployment	-0.004	0.075	0.080	0.062	0.085	0.442			
Ext: house price growth	0.001	0.009	0.073	-0.001	0.005	0.118			

Note: The table presents the estimation results of a collected estimate of the beta coefficient on the primary study characteristics and external (structural) variables, searching for potential sources of heterogeneity. Bayesian model averaging employs a combination of the uniform model prior and the unit information g-prior recommended by Eicher et al. (2011). P. mean – posterior mean, P. SD – posterior standard deviation, PIP – posterior inclusion probability. The regulatory capital ratio represents the ratio between regulatory capital (Common Equity Tier 1, Additional Tier 1 and Tier 2 items) and risk-weighted exposures. Capital requirements represent the ratio between the various categories of capital requirements (minimum, Pillar 2 add-ons and capital buffers) and risk-weighted exposures. The set of characteristics for the subset of elasticities on capital requirements is reduced due to little heterogeneity in the studies and the multicollinearity of some variables.

5.4.1. Capital-to-Asset Ratio

The sample of studies using a simple capital-to-asset ratio to explain changes in bank lending has a mean elasticity of 0.35, with a significant variation between the top and bottom 5% reported (-2.22 and 3.79 respectively). Such studies generally aim to capture the effects of the bank capital position or the level of bank capitalization. As such, prior intuition and the true effect identified in the previous sections of our paper is that higher bank capitalization should inflate lending.

Data characteristics. We find sample size to be one of the most important factors influencing the estimates of the relationship between bank capital and lending. Elasticities estimated using larger datasets tend to be higher which may be the result of higher in-sample variation captured by more observations or by a change in the relationship over time. The latter is supported by the fact that studies performed on more recent datasets, as captured by the midpoint of the estimation period, exhibit a less positive or indeed even a negative effect of bank capital on lending. The shift in the estimated relationship over time may, to some extent, reflect increasingly demanding (and binding) capital regulation, especially in Europe and the US. The decisively positive effect of studies conducted on countries outside Europe, together with the substantially negative effect of studies performed solely on US data, speak in favor of this interpretation. Some structural characteristics give additional credit to this hypothesis (see below).

More generally, single-country studies deliver higher positive elasticity estimates, even more so when using confidential data sources.[22] Due to more detailed data, these kinds of studies can theoretically be more successful than others in correctly identifying shocks to bank capital that are plausibly unrelated to lending opportunities. As such, they are less likely to suffer from endogeneity bias.[23]

We further find substantial evidence that primary studies focusing on the impact of changes in the bank capital position on corporate credit produce more positive elasticity estimates than those focusing on the impact on total credit (or household credit). This suggests that corporate credit is far more sensitive to changes to a bank's capital position than credit extended to other economic sectors. This echoes back to the literature which shows that when faced with a change in capital requirements, banks will make trade-offs between different assets and between the different options of how to move to a higher capital

[22] In our sample, studies employing confidential data are exclusively single-country studies and are generally performed by central bank researchers.

[23] For example, lending growth and capital can be endogenously determined through the performance of borrowers. Confidential loan-level data may allow for the inclusion of borrower characteristics that would significantly reduce potential bias.

ratio. Not surprisingly, banks will often choose to reduce their high-risk exposures (Akram, 2014; Mendicino et al., 2018). Empirical studies find that banks tend to shrink their portfolios of corporate loans, which generally attract a higher risk weight, more than their portfolios of domestic loans (Bridges et al., 2014; Bahaj et al., 2016).

Model specification and estimation. Another important way in which estimates differ is the estimation technique used. Studies that try to shield the estimates from the omitted variable bias by using a fixed effect estimator tend to report lower elasticity values than those using simple OLS. Similarly, studies that include both bank-specific and macroeconomic variables in their specification report lower elasticities than those missing some controls. The one exception is interest rates which, if missing in the model, translate into a generally weaker relationship between capital and lending. In our stream of literature, the omitted variable can take the form of various unobserved managerial or regulatory decisions that if negatively correlated with the bank capital ratio would downward-bias the estimated elasticities. Overall, it seems that studies that try to address these issues report less positive or negative estimates which would mean the endogeneity bias would generally work upwards for this particular relationship between capital and lending. We also find that studies which consider a short-term relationship (up to one year) between bank capital and lending report lower elasticity than studies considering a capital ratio lagged by one year or more. This may imply that changes to bank capital affect lending more negatively over a shorter horizon and more positively over a longer horizon.

Publication characteristics. Our results indicate a strong association between four publication characteristics (year of publication, publication in a peer-reviewed journal, journal impact factor and the number of citations) and the reported results. We interpret this association as the potential effect of quality: while studies published in refereed journals tend to report generally more positive bank capital-lending elasticities, more recent and higher-quality studies (those with a higher impact factor and more citations) tend to report decisively fewer positive or potentially even negative elasticities. This means that the effect may be overestimated in the lower-quality series, while the negative association between the year of publication and the estimated elasticity lends support to our hypothesis of the changing relationship over time. Interestingly, working papers published by central banks tend to report more positive elasticities.

External variables. We identified three external variables which play a decisive role in the relationship between bank capital and lending. First, the positive relationship is stronger in countries with worsened economic conditions, which is captured by high or unstable inflation. A negative shock to bank capital during less tranquil times in countries with unfavorable macroeconomic conditions could significantly decrease credit. Second, a prolonged period of low interest rates, as captured by the "low for long" variable, weakens the positive effect or potentially reverses it. The variable refers mostly to the period after the GFC when bank profitability was subdued and capital regulation was tightened. For many banks, capital requirements became binding as their capital buffers (capital above minimum capital requirements) became depleted. Therefore, additional capital requirements may require banks to increase their capital position without extending additional credit to the economy or may even depress lending. The "low for long" variable could capture the change in the relationship between bank capital and lending after the GFC, which may have become weaker or even negative. Finally, we find that the degree of financial openness of a given country considered in the analysis is an important factor in explaining heterogeneity. Studies performed on a more financially open country tend to report more positive elasticities. A more financially open banking sector is more prone to international spillovers of various shocks which foster the dependence of bank lending decisions on the level of capitalization.

5.4.2. Regulatory Capital Ratio

The sample of studies using a regulatory capital ratio to explain movements in bank lending has a mean elasticity of 0.14 with the variation between the top and bottom 5% reported more in favour of the negative spectrum of the elasticity distribution (-1.38 and 1.11 respectively). Studies considering the risk-sensitive capital ratio are generally interested in the estimation of the relationship between bank capital and lending under the umbrella of bank capital regulation. While the distribution of elasticities on the simple capital-to-asset ratio is skewed to the right (i.e. more towards positive values), the distribution of elasticities on the regulatory capital ratio is skewed to the left (i.e. more towards negative values).

Contrary to the risk insensitive capital-to-asset ratio, prior intuition linked to explaining a shock to regulatory capital ratio is rather fuzzy. Studies considered in our sample tend to understand such a shock in different ways ranging from a plain shock to bank capitalization (Brei et al., 2013) to the imposition of a regulatory tax (Naceur et al., 2018) to a direct proxy for macroprudential policy (Wang and Sun, 2013). The absence of clear prior intuition is actually a good motivation for looking at the factors explaining the heterogeneity of reported elasticities. It could be that different researchers have different priors on the analyzed effect and this may be reflected in their choice of the data, model and estimation technique.

Since the simple capital-to-assets ratio and regulatory capital ratio share some commonalities in terms of calculation, it is not surprising that we also find several common factors that explain the heterogeneity for both. Specifically, single-country studies with a larger sample size are positively correlated with the reported elasticities in both sub-samples. Similarly, studies shielded from omitted variable bias with more favorable publication characteristics are negatively correlated with the reported elasticities in both sub-samples. However, contrary to the sub-sample of the simple capital-to-asset ratio, we find a much smaller role for external factors.

Data characteristics. On top of the factors described above, we find that larger data frequency is positively correlated with the estimated elasticities: elasticities estimated using datasets with annual frequency tend to be smaller than those employing quarterly or monthly data for estimation. We further find that US-based studies tend to report substantially smaller elasticities than other regions, even more so when using confidential data sources. Contrary to the previous subset of data, the effect of a higher regulatory capital ratio on lending is less positive or even negative if corporate loans are considered.

Model specification and estimation. We found that a number of characteristics were strongly associated with the model specification and estimation. First, the reliance on a dynamic model specification tends to shift the distribution of elasticity estimates to negative territory. Not surprisingly, including the lagged dependent variable may explain a lot of the variance in the credit dynamics initially captured by the capital variable. Given the generally high persistence in the stock of credit, not including the persistence term may significantly overestimate the reported elasticity linked to the capital ratio. In addition, a rich lag structure and an additional capital variable included in the same model shifts the elasticity towards more positive values. This effect can be viewed from two angles. The inclusion of additional lags of the reported capital ratio or different capital variables may absorb the variation in credit dynamics, and potentially act in the opposite direction as the capital ratio primarily studied in this sub-section. In other words, the positive correlation may be a result of multicollinearity in the primary study. On the other hand, assuming that the researchers checked and corrected for potential variable multicollinearity, a richer lag structure and additional capital variable may allow the model to account for period-to-period persistence in the level of capital, e.g. adjustments banks may make in advance of planned changes in their balance sheets.

Finally, we find that the inclusion of interest rates in a model is detrimental to the reported elasticity. Studies that are missing interest rates from their model specification report have more positive elasticities. Interest rates are generally meant to capture changes to monetary policy which can directly affect bank lending and bank capital. Numerous studies show that monetary policy tightening depresses lending via the bank lending channel (Kishan and Opiela, 2000; Disyatat, 2011; Albrizio et al., 2020). At the same time, higher interest rates improve the profitability of a bank which can increase its capital via retained earnings (Borio et al., 2017; Altavilla et al., 2018). This would suggest that the omitted-variable bias associated with not including the interest rate into the model works upwards.

5.4.3. Capital requirements

Studies considering changes to bank capital requirements offer the most precise approximation of changes to bank capital due to a change in capital regulation. Unfortunately, owing to a lack of hindsight and detailed supervisory data, few such studies exist, giving us only a handful of elasticity estimates (Table A1). Moreover, the studies show little heterogeneity in terms of data characteristics and econometric approach, significantly reducing the set of control variables entering the analysis. BMA estimates thus need to be interpreted with care as they are meant to serve as a first attempt to perform a heterogeneity analysis in this field of literature. Changes to capital requirements, if binding, are expected to decrease bank lending due to the immediate effect on bank funding costs. If bank capital is costly, reliance on such funding can lead to a decrease in the supply of credit. Prior intuition thus strongly favors a negative effect of rising capital requirements on bank lending.

The presence of publication bias in the estimates of the effect of capital requirements is supported by evidence across all the models we run. Thus, the reported elasticities are found to be systematically exaggerated due to publication bias even if we control for the additional characteristics of the individual studies. The bias is found to work downwards, therefore studies tend to favor more negative elasticity estimates which are in line with economic intuition.

We further find that studies based on smaller samples report more negative elasticities. This association may simply reflect that studies performed on more recent datasets, that are naturally shorter, will identify a stronger negative effect of capital regulation, since changes to bank capital requirements took place predominately after the GFC. A negative correlation with the "low for long" variable speaks in the favor of this interpretation. Similarly to the previous two capital to lending relationships, we found that the type of bank credit entering the analysis played a significant role. The recurring theme is a stronger reaction of corporate credit to changes in capital ratios, which may be either be more positive (changes in the simple capital-to-asset ratio) or more negative (changes in the regulatory capital ratio or capital requirements).

5.5. Economic Significance of Key Variables and Implied Elasticity

The model averaging analysis suggests that the reported elasticities for the two subsets linked to the simple capital-to-asset ratio and the regulatory capital ratio are mainly affected by the researchers' choice of empirical approach. Many of these choices may arguably lead to a bias, not to mention the changing nature of the relationship over time. Therefore, we calculate a mean elasticity based on the variables deemed important by the BMA analysis (PIP above 0.5)[24] and our definition of best practice methodology in the literature. With that, we attempt to show what the mean elasticity would be if all the studies used the same strategy as the one that we prefer. We tend to favor such characteristics of the primary study that, in our opinion, represent a consistent and unbiased estimator and could better capture changes in bank capital due to capital regulation (i.e. more recent single-country studies performed using confidential data). Since best practice is subjective, we show how the mean elasticity deviates if we alter some of the key variables. Unlike other studies, we calculate the mean elasticity as fitted values based directly on the BMA output.[25] Given that the portion of variance of the collected estimates explained by characteristics used to derive the "best-practice" specification reaches up to 70%, we consider this exercise to be robust and informative.[26]

Our subjective definition of best practice is as follows. We prefer single-country studies performed on confidential data samples with higher frequency and more observations. Regarding model specification, we favor dynamic models including both bank-level (supply-side) and macroeconomic (demand-side) control variables and estimated using the fixed effect regression method. We also prefer studies with more favorable publication characteristics, i.e. more recent publications in refereed journals with a higher impact factor and more citations. Finally, we include external variables if they were selected by the BMA. In addition, we distinguish between corporate and household credit in our definition of best practice. As a first alternation to best practice, we calculate mean elasticities for an inferior empirical approach that misses some key control variables, includes the additional capital variable or lag of respective capital ratio and is estimated by OLS. Finally, we change the value of the "low for long variable" to its 90th percentile to shows the full manifestation of a prolonged period of low interest rates on the relationship between bank capital and lending. Table 6 reports the results for all three subsets of the collected estimates. However, as the studies on capital requirements are quite homogeneous

[24]We do not consider all control variables when calculating the mean elasticity because we want to account only for the identified drivers of heterogeneity and show how the mean elasticity changes if we alter these key drivers. We see this exercise merely as an extension of the model averaging analysis: in the model averaging analysis, we focused on the statistical significance; here we show the economic significance of the key drivers.

[25]We set variables with a PIP below 0.5 and non-preferred variables to zero and then we use function *predict* supplied by *BMS* package in R which calculate fitted values based on the MCMC frequencies of all models. The confidence intervals are retrieved from the predictive densities of the fitted values calculated using function *pred.density* from the same package.

[26]We report the adjusted R^2 as the goodness-of-fit measure of the best-practice specification that controls for the variables used to calculate implicit mean elasticities. These specifications are referred to as frequentist checks and presented in Tables B1–B3 in the Appendix.

in terms of the methodology used, and consequently, the set of significant heterogeneity drivers is limited, we cannot report implicit elasticity for an inferior empirical approach.

Table 6: Mean Elasticities Implied by Significant Heterogeneity Drivers

	Capital-to-Asset Ratio		Regulatory Capital Ratio		Capital Requirements	
	Estim.	68% CI	Estim.	68% CI	Estim.	68% CI
Baseline (“best practice”)	0.43	(-0.81, 1.08)	-0.62	(-0.75, 0.22)	-0.72	(-1.91, 0.27)
Corporate credit	0.61	(-0.60, 1.24)	-0.65	(-0.79, 0.19)	-0.97	(-2.16, 0.02)
Household credit	0.39	(-0.84, 1.01)	-0.62	(-0.76, 0.22)	-0.55	(-1.73, 0.44)
Inferior empirical approach	0.26	(-0.98, 0.91)	0.52	(0.51, 1.50)	-	-
Prolonged period of low interest rates*	-2.42	(-3.68, -1.83)	-1.08	(-1.20, -0.24)	-3.55	(-4.68, -2.53)

Note: The table presents the mean estimate of the elasticity of the relationship between bank capital and lending implied by the BMA, the collected estimates and our definition of best practice in the first row, as well as changes to key variables in the remaining rows. That is, the table attempts to show what the mean elasticity would be if all studies used the same strategy as the one that we prefer (“best practice”). In addition, we attempt to show the economic significance of the key variables by calculating the mean elasticity for different sub-groups of characteristics. The mean elasticity is calculated as the fitted values based on the BMA output and a matrix of chosen study characteristics (only variables with a PIP of above 0.5 are considered). We report 68% confidence intervals in brackets which are retrieved from the predictive densities of the fitted values. The predictive density is a mixture density based on the best models identified by the BMA. *We replaced the values of the low for long variable with its 90th percentile to see the full manifestation of this effect. The low for long variable was deemed decisive (PIP ¿ 0.99) in the subset of capital-to-asset ratios and capital requirements but unimportant (PIP = 0.38) in the subset of regulatory capital ratios; thus, estimates for the latter should be interpreted with caution.

The “best practice” estimate of the relationship between the capital-to-asset ratio and bank credit growth is 0.4 pp while that of the relationship between the regulatory capital ratio and bank credit growth is -0.6 pp. The latter is in stark contrast to both the simple average and the mean effect corrected for publication bias calculated across collected elasticities (around 0.1–0.2 pp for both subsets). Generally, elasticities implied by significant heterogeneity drivers are distinctly on the positive side for the simple capital-to-asset ratio and on the negative side for the regulatory capital ratio. This supports our view that the relationship between the regulatory capital ratio and bank lending reflects the impact of changes to bank capital regulation whereas the simple capital-to-asset ratio reflects changes to bank capitalization. Part of the regulatory policy is contained in the denominator of the regulatory capital ratio. Unlike the simple capital-to-asset ratio, changes to the regulatory capital ratio can be caused by changes in regulation (i.e. the method of calculation of risk-weighted exposures) and the riskiness of the underlying exposures. A slightly stronger reaction, both positive and negative, was derived for corporate credit relative to household credit. On the contrary, the inferior empirical approach brings both elasticities slightly closer to zero, suggesting that the correct model specification and estimation technique is key in identifying the true effect.

The “best practice” estimate of a 1 pp increase in capital requirements, corrected for publication bias, and a preference for larger data samples, slows down bank annual credit growth, on average, by 0.7 pp. Similarly to the previous two ratios, the effect is stronger for corporate credit relative to household credit. Given the limited heterogeneity in the approach to estimating this effect, the implied elasticity of the relationship between capital requirements and bank lending serves merely as a robustness check for our results on publication bias. Reassuringly, the calculated elasticity stays at the same interval between -0.5 and -1 pp which conforms to the estimates of the effect beyond bias (Table 4), which is substantially above the uncorrected mean of -1.7 pp. Importantly, the effect is very close to that of the regulatory capital ratio, supporting our intuition presented above that the relationship between the regulatory capital ratio and bank lending reflects the impact of changes to bank capital regulation. This may be relevant for at least two reasons. First, it is in line with the literature highlighting the role of bank capitalization and the risk sensitivity of capital regulation in the transmission of capital requirements. Second, it allows for proxy changes in capital regulation using the regulatory capital ratio if the data on capital requirements are not available to the researcher. In such a case, the choice of a suitable model specification and estimation approach would be crucial.

Interestingly, implied elasticities for all three capital ratios are brought down by a period of low interest rates. The relationship between bank capital and lending seems to be significantly affected by the post-crisis period of highly accommodative monetary policy and more demanding capital regulation. Even risk-insensitive bank capitalization is no longer positively associated with bank lending.

6. Concluding Remarks

We present the first quantitative synthesis of the vast empirical literature on the effects of changes to bank capital on the extension of bank credit. The great importance of the relationship between bank capital and lending was highlighted during the 2007–2010 financial crisis, and again, during the global uncertainty caused by the Covid-19 pandemic which put pressure on bank balance sheets. Yet, prior to the emergence of the GFC, most central banks tended to overlook the role of banks as a potential source of friction and did not regularly include the banking sector in their macroeconomic models. Years after the crisis, the situation has changed, and the estimation of the relationship between bank capital and lending, especially due to the impact of rising regulatory policies, is now at the centerpiece of policy debates and academic interest. Yet, despite the high importance of quantifying the relationship, the empirical literature displays a high degree of fragmentation in terms of the estimated coefficients. To tackle this issue, we synthesize the empirical evidence from a unique dataset of more than 1,600 estimates of the elasticity between bank capital and lending collected from 46 primary studies. We use state-of-the art meta-analytic techniques as well as Bayesian and frequentist model averaging methods to identify the sources of the fragmentation.

We provide the following key findings. First, the researcher's initial choice on how to express the bank capital ratio has an important impact on the estimated effect. We divide all collected elasticities into three groups based on whether they consider changes to a simple capital-to-asset ratio, a regulatory capital ratio or capital requirements. While elasticities linked to simple capital-to-asset ratio tend to be more on the positive side, elasticities on regulatory capital ratios are more skewed to negative values and elasticities on capital requirements are strongly negative. Taking a simple average of the collected estimates, a one percentage point increase in the simple capital-to-asset ratio or regulatory capital ratio is found to increase lending by about 0.3 pp or 0.1 pp respectively. On the contrary, a 1 pp increase in minimum capital requirements is found to decrease lending by 1.7 pp. Correcting for publication bias brings the latter effect to between -0.5 pp and -0.9 pp. However, the first two groups of elasticities do not show any signs of publication bias.

Next, we continue to find significant heterogeneity in the reported estimates linked to the capital-to-asset ratio and the regulatory capital ratio. We control for an additional 40 variables that reflect the context in which the estimates were obtained in the primary study and cross-country or regional differences. Since the simple capital-to-assets ratio and regulatory capital ratio share some commonalities, it is not surprising that we also identify several common factors that explain the heterogeneity for both, especially those linked to model specification and estimation. On the contrary, external factors such as macro-financial and institutional characteristics only play a prominent role in the relationship with the capital-to-assets ratio.

Interestingly, we find a few signs that the relationship between bank capital and lending has changed in the post-crisis period. First, if we plot a simple median elasticity per study against the median year of the estimation sample, the relationship weakens over time. Next, a prolonged period of low interest rates weakens the positive effect and also potentially reverses it. The external variable that captures this period is calculated as a number of consecutive quarters during which the short-term interest rate is below its first quartile. Hence, it refers mostly to the period after the GFC of increasingly demanding bank capital regulation and subdued bank profitability. In such an environment, 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. In addition, elasticities estimated using smaller datasets tend to be smaller which may lend additional support to the changing relationship over time. The more negative effect seen in studies conducted on Europe or the US relative to the rest of the world also supports this interpretation, given that more stringent capital regulation was applied especially in these regions.

To some extent, it can be considered surprising that the literature finds such a stark difference in the effects of changes to bank regulatory capital ratio and capital requirements. It seems that the observed regulatory capital ratio is a rather noisy indicator of regulatory changes to bank capital and may capture both changes to overall bank capitalization or to capital regulation. However, taking into consideration some key economic drivers identified by the model averaging techniques reveal that these two have significantly more in common. If we consider single-country studies performed on confidential data samples with higher frequency and a superior empirical approach (i.e. dynamic model specification with the lagged effect of capital on lending including both bank-level and macroeconomic control variables and estimated with unit fixed effects), the mean elasticity turns negative. This suggests that the final estimate of elasticity is strongly affected by the researchers' choice of empirical approach. Nevertheless, researchers aiming to estimate the effect of changes to bank capital as a result of changing capital regulation are still better off considering changes directly to capital requirements.

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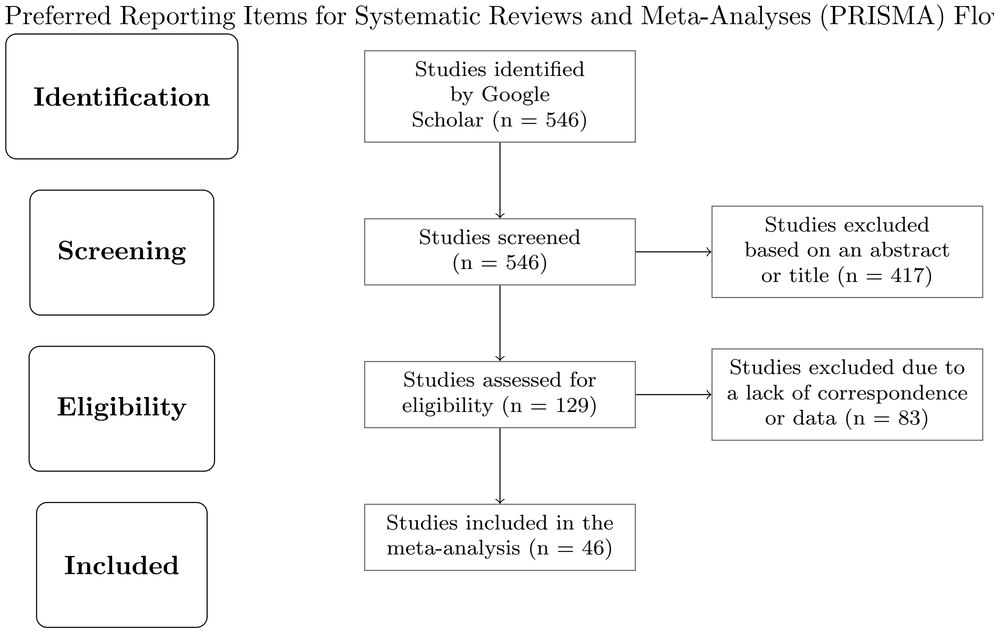
Appendix A. Data Collection and Fragmentation

Appendix A.1. PRISMA Diagram

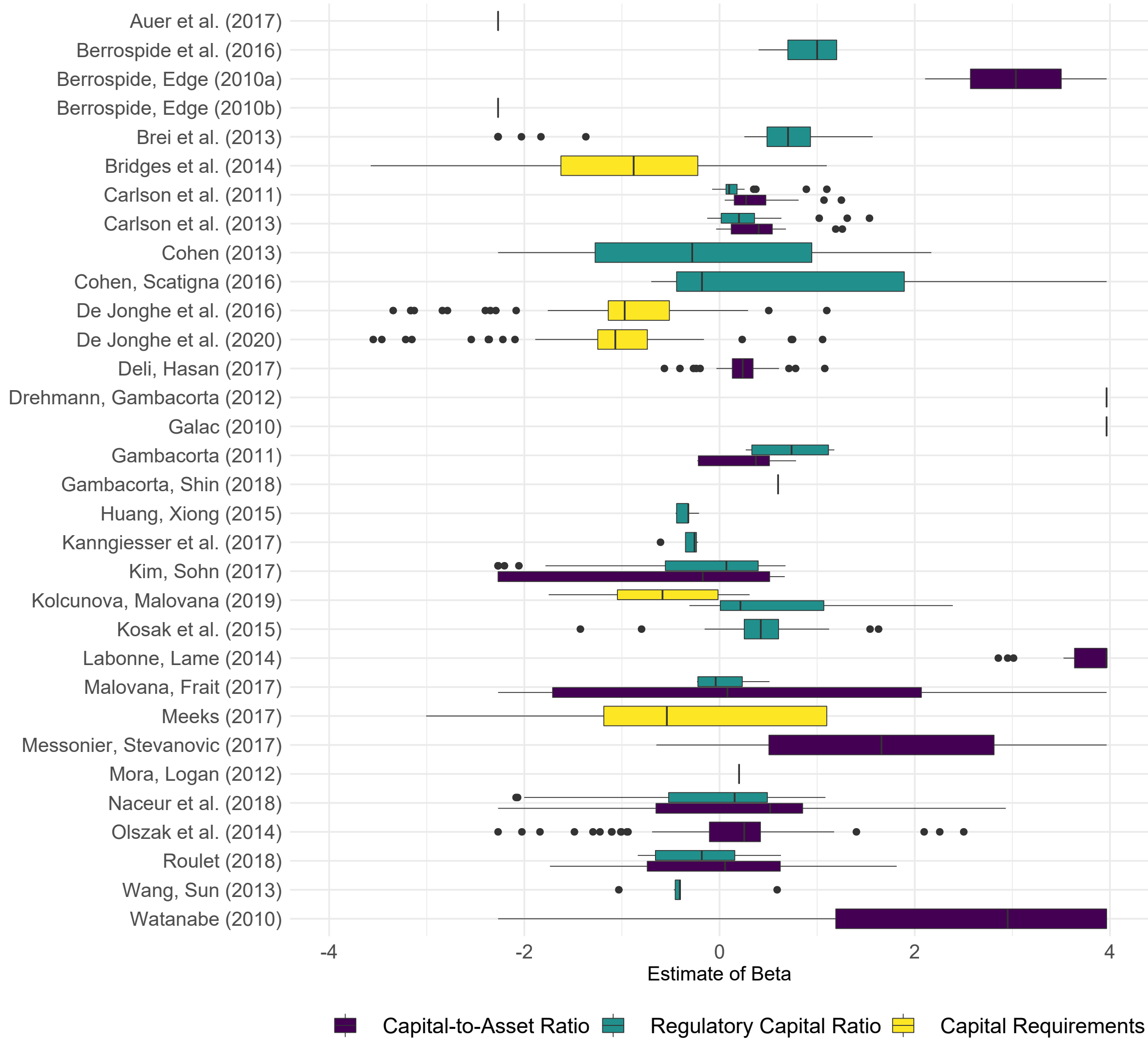
Figure A1 depicts the overall process employed in the selection of primary studies. In the *identification* phase, we scanned the first 300 research articles returned by Google Scholar using a tailor-made search query, limiting our search to papers published in or after 2010 (see Section 3.1). We then went through all the citations in each of the relevant studies and identified an additional 246 articles, bringing us to a total of 546 articles for *screening*. In the next step, we reviewed all the titles and abstracts with the aim of effectively identifying studies that were not acceptable, even from a high-level perspective. In doing so, we eliminated 417 studies and assessed the remaining 129 for *eligibility*. During this step, we went through each article in more detail and filtered out 83 studies due to a lack of correspondence or data. The main elimination criteria were: (1) the study must report numerical results; (2) estimated elasticities must be presented together with the corresponding test statistic – standard error, t-statistic, p-value or exact confidence interval; (3) the effect is not a cross-boarder effect; (4) the measure of lending cannot be expressed as a ratio to some other continuous variable such as total loans or total bank assets; and finally, (5) the variable capturing capital cannot be expressed by a dummy-coded index (such as in Cerutti et al. 2017b). All in all we ended up with 46 primary studies *included* in the meta-analysis.

Figure A1: Preferred Reporting Items for Systematic Reviews and Meta-Analyses (PRISMA) Flow Diagram



Appendix A.2. Additional Summary Statistics According to Different Characteristics

Figure A2: Reported Estimates Vary Both Within and Across Studies



Note: The figure depicts a boxplot of the collected estimates on the effect of a 1 pp increase in capital ratio on bank annual credit growth (see equation 1). The regulatory capital ratio represents the ratio between regulatory capital (Common Equity Tier 1, Additional Tier 1 and Tier 2 items) and risk-weighted exposures. Capital requirements represent the ratio between various categories of capital requirements (minimum, Pillar 2 add-ons and capital buffers) and risk-weighted exposures. The length of each box represents the interquartile range (P25-P75), and the dividing line inside the box is the median value. The whiskers represent the highest and lowest data points within 1.5 times the range between the upper and lower quartiles. The vertical line denotes unitary elasticity. For ease of exposition, extreme outliers are excluded from the figure but included in all the statistical tests.

Table A1: Which Capital Requirements Were Explored in Primary Studies

Article	Capital requirements	Country	Period	Mean (95% CI)
Bridges et al. (2014)	Required capital ("trigger") ratio	UK	1990–2014	-3.98 (-11.56, 0.26)
De Jonghe et al. (2016)	Pillar 2 capital requirements	BE	2013–2015	-1.05 (-3.15, 0.20)
De Jonghe et al. (2020)	Pillar 2 capital requirements	BE	2013–2015	-1.69 (-4.85, -0.16)
Kolcunová and Malovaná (2019)	Overall capital requirements	CZ	2013–2017	-0.57 (-1.60, 0.27)
Meeks (2017)	Required capital ("trigger") ratio	UK	1975–2008	-0.80 (-3.48, 1.10)

Note: The required capital ("trigger") ratio is an analogy to Pillar 2 capital requirements, i.e. bank-specific capital requirements set by the Bank of England (before 2001) and the FSA. In 2001, the trigger ratio was renamed Individual Capital Guidance (ICG) and became part of the Pillar 2 process under Basel II (Bridges et al., 2014, see). Overall, regulatory capital requirements refer to a combination of minimum capital requirements, Pillar 2 capital add-ons and additional capital buffers (the systemic risk buffer and countercyclical capital buffer).

Table A2: Description and Summary Statistics of Meta-Regression Variables

Variable	**Description**	**Mean (St. Dev.)**					
		Capital-to-Asset Ratio		Regulatory Capital Ratio		Capital Requirements	
Estimate	The reported estimate of the beta coefficient.	0.34	(1.38)	0.14	(0.80)	-1.74	(2.59)
St. error	The reported standard error of the beta coefficient.	1.22	(4.58)	0.49	(1.24)	1.52	(2.37)
Data characteristics							
Total credit	= 1 if total credit is used as a dependent variable.	0.38	(0.49)	0.56	(0.50)	0.10	(0.30)
Corporate credit	= 1 if corporate credit is used as a dependent variable.	0.35	(0.48)	0.21	(0.41)	0.79	(0.41)
Household credit	= 1 if household credit is used as a dependent variable.	0.27	(0.45)	0.22	(0.42)	0.12	(0.32)
Midpoint	The logarithm of the midpoint of the data sample.	3.27	(0.17)	3.28	(0.16)	3.22	(0.79)
No. of observations	The logarithm of the total number of observations.	7.53	(1.63)	7.67	(1.60)	10.56	(3.66)
Quarterly frequency	= 1 if data frequency is quarterly.	0.11	(0.31)	0.21	(0.41)	1.00	(0.00)
Confidential data	= 1 if confidential (supervisory) data are used (as opposed to publicly available data).	0.07	(0.26)	0.19	(0.39)	0.33	(0.47)
Macro-level data	= 1 if macro-level data are used (as opposed to bank-level data).	0.39	(0.49)	0.31	(0.46)	0.00	(0.00)
Single-country	= 1 if the study covers a single country (as opposed to a cross-country study).	0.45	(0.50)	0.54	(0.50)	1.00	(0.00)
US	= 1 if the study covers only US.	0.34	(0.47)	0.44	(0.50)	0.00	(0.00)
Other region	= 1 if the study covers countries outside the US and Europe or the sample includes a mix of countries from different regions.	0.15	(0.36)	0.25	(0.44)	0.00	(0.00)
Model specification and estimation							
GMM method	= 1 if the general method of moments (GMM) is used.	0.33	(0.47)	0.13	(0.34)	0.17	(0.38)
Fixed-effects method	= 1 if the fixed-effects (FE) regression method is used.	0.17	(0.37)	0.35	(0.48)	0.68	(0.47)
Other method	= 1 if a method other than the OLS, GMM or FE method is used.	0.04	(0.19)	0.15	(0.35)	0.15	(0.36)
Time fixed effects incl.	= 1 if time fixed effects are included.	0.42	(0.49)	0.23	(0.42)	0.85	(0.36)
Dynamic model	= 1 if the model is dynamic, i.e., contains a lagged dependent variable.	0.46	(0.50)	0.44	(0.50)	0.33	(0.47)
Lagged by 1Y or more	= 1 if the estimate is lagged by a year (4 quarters) or more.	0.69	(0.46)	0.77	(0.42)	0.38	(0.49)
Contemporaneous	= 1 if the estimate is contemporaneous (not lagged at all).	0.21	(0.40)	0.03	(0.18)	0.03	(0.16)
Missing control variables	= 1 if the model is missing either supply-side (banks-specific) or demand-side (macroeconomic) control variables.	0.18	(0.39)	0.21	(0.40)	0.85	(0.36)
Missing interest rate in eq.	= 1 if the model is missing interest rate as a control variable.	0.42	(0.49)	0.21	(0.41)	0.85	(0.36)
Crisis	= 1 if the estimate is interacted with a crisis dummy variable.	0.10	(0.30)	0.14	(0.35)	0.02	(0.13)
Other interaction	= 1 if the estimate is interacted with another dummy variable.	0.18	(0.38)	0.23	(0.42)	0.23	(0.42)
Add. capital in eq.	= 1 if the model contains an additional capital variable on top of the studied capital ratio.	0.27	(0.44)	0.38	(0.49)	0.98	(0.13)
Add. lag in eq.	= 1 if the model contains additional lag(s) of the studied capital ratio.	0.01	(0.11)	0.01	(0.08)	0.62	(0.49)
Some interaction in eq.	= 1 if the model contains some interaction term (discrete or continuous) with the studied capital ratio.	0.33	(0.47)	0.39	(0.49)	0.52	(0.50)

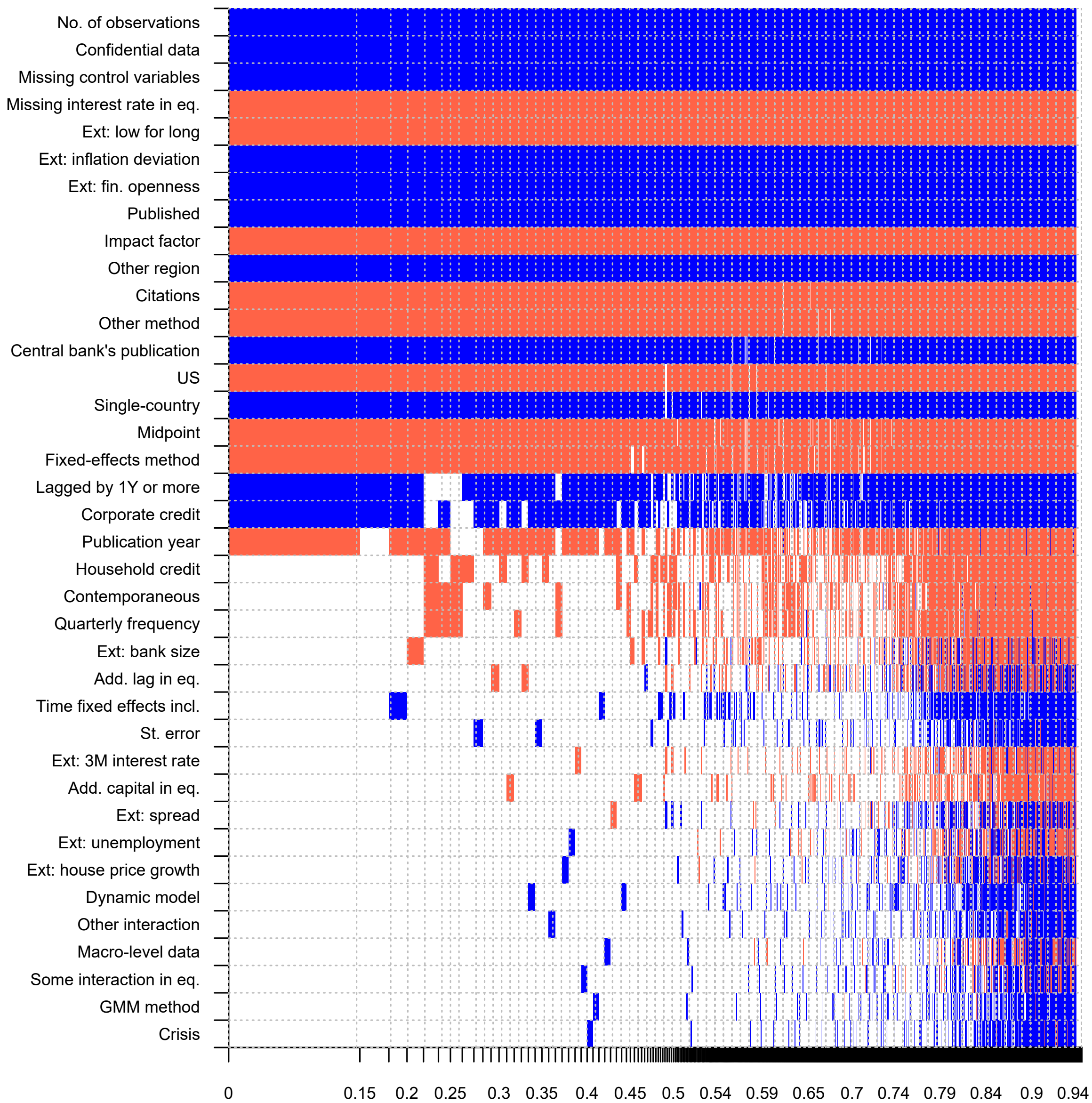
Continued Table A2.

Variable	Description	Mean (St. Dev.)					
		Capital-to-Asset Ratio		Regulatory Capital Ratio		Capital Requirements	
Publication characteristics							
Publication year	The logarithm of the publication year of the primary study minus the earliest publication year in our dataset plus one.	1.85	(0.56)	1.88	(0.44)	2.10	(0.29)
Impact factor	The recursive impact factor.	0.68	(0.63)	0.67	(0.57)	1.07	(0.84)
Citations	The logarithm of the number of citations divided by the number of years from its publication until 2021.	2.38	(0.81)	2.79	(0.56)	2.77	(0.51)
Published	= 1 if the primary study was published in a journal with an impact factor.	0.75	(0.43)	0.84	(0.37)	0.43	(0.50)
Central bank publication	= 1 if the primary study was published by a central bank.	0.26	(0.44)	0.25	(0.44)	1.00	(0.00)
External variables							
Ext: 3M interest rate	The average 3-month interest rate in percent.	2.71	(2.04)	2.52	(1.65)	1.81	(3.16)
Ext: spread	The average spread (difference between 10-year government bond yield and 3-month interest rate) in percentage points.	-1.80	(0.59)	-1.69	(0.65)	-1.05	(0.77)
Ext: low for long	The number of consecutive quarters during which the 3M interest rate is below its first quartile.	9.80	(4.59)	9.53	(4.62)	6.73	(7.08)
Ext: inflation deviation	The number of consecutive quarters during which CPI inflation is outside the +/- 1 percentage point band around its long-term mean.	21.11	(14.23)	14.81	(6.77)	12.04	(19.50)
Ext: house price growth	The average annual house price growth in percent.	1.90	(4.38)	1.98	(4.93)	2.70	(3.28)
Ext: unemployment	The average unemployment rate in percent.	8.10	(1.56)	7.50	(1.58)	7.94	(1.22)
Ext: bank size	The average ratio between banking sector assets and GDP in percent.	89.00	(33.21)	78.53	(19.48)	84.14	(20.94)
Ext: fin. openness	The average financial openness index (Chinn and Ito, 2008).	2.00	(0.37)	1.99	(0.60)	2.28	(0.23)

Appendix B. Extensions to the Analysis of Heterogeneity

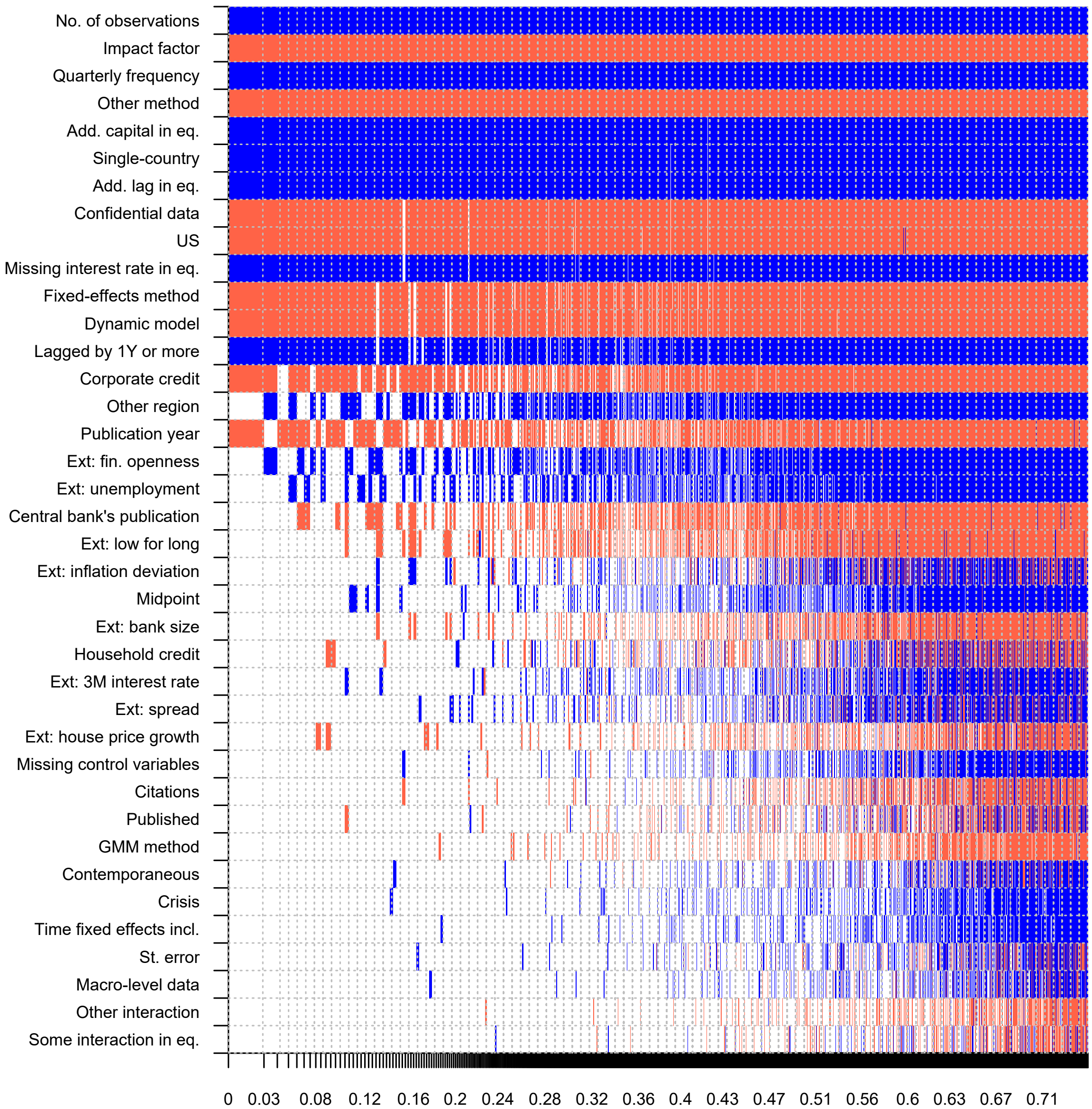
Appendix B.1. Bayesian Model Averaging

Figure B1: BMA Results – Capital-to-Asset Ratio



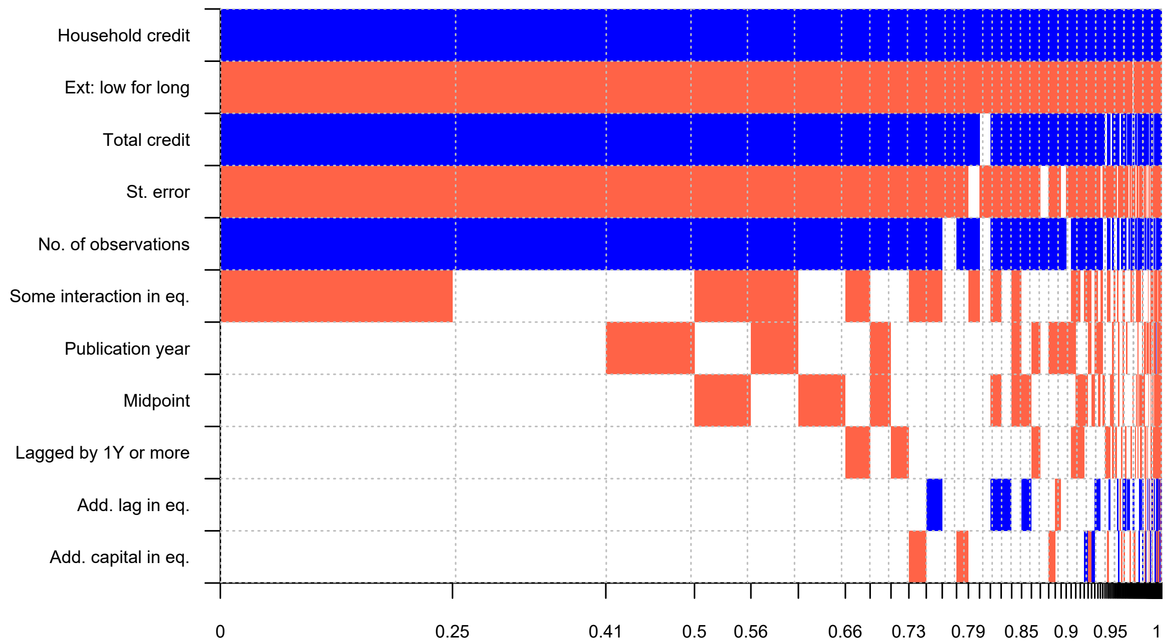
Note: The response variable is the estimated effect of a 1 percentage point change in capital-to-asset ratio on credit growth. The columns denote the individual models; the variables are sorted by posterior inclusion probability in descending order. The horizontal axis denotes the cumulative posterior model probabilities; the 10,000 best models are shown. To ensure convergence we employ 3 million iterations and 1 million burn-ins. Blue (darker in the grayscale) indicates that the variable is included and the estimated sign is positive, i.e., the transmission is stronger, given that the mean effect is positive. Red (lighter in the grayscale) indicates that the variable is included and the estimated sign is negative, i.e., the transmission is weaker, given that the mean effect is positive. No color indicates that the variable is not included in the model.

Figure B2: BMA Results – Regulatory Capital Ratio



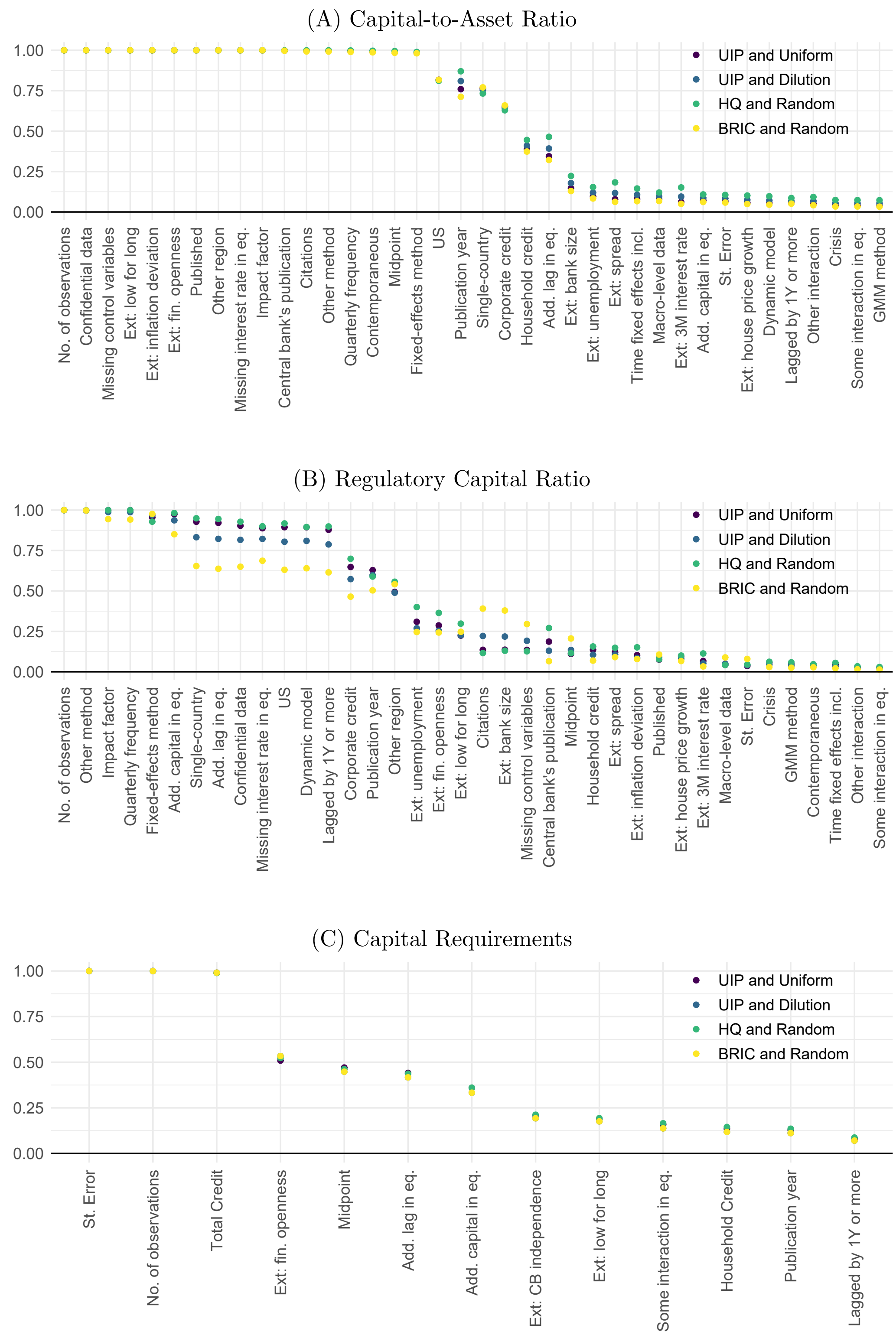
Note: The response variable is the estimated effect of a 1 percentage point change in the regulatory capital ratio on credit growth. The columns denote the individual models; the variables are sorted by posterior inclusion probability in descending order. The horizontal axis denotes the cumulative posterior model probabilities; the 10,000 best models are shown. We employ 3 million iterations and 1 million burn-ins to ensure convergence. Blue (darker in the grayscale) indicates that the variable is included and the estimated sign is positive, i.e., the transmission is weaker, given that the mean effect is negative. Red (lighter in the grayscale) indicates that the variable is included and the estimated sign is negative, i.e., the transmission is stronger, given that the mean effect is negative. No color indicates that the variable is not included in the model.

Figure B3: BMA Results – Capital Requirements



Note: The response variable is the estimated effect of a 1 percentage point change in capital requirements on credit growth. The columns denote the 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. We employ 3 million iterations and 1 million burn-ins to ensure convergence. Blue (darker in the grayscale) indicates that the variable is included and the estimated sign is positive, i.e., the transmission is weaker, given that the mean effect is negative. Red (lighter in the grayscale) indicates that the variable is included and the estimated sign is negative, i.e., the transmission is stronger, given that the mean effect is negative. No color indicates that the variable is not included in the model.

Figure B4: Bayesian Model Averaging – Prior Sensitivity



Note: The figures show posterior inclusion probability for different prior combinations. In our baseline, we use a unit information g-prior (UIP) and a uniform model prior which reflects our lack of prior knowledge. The uniform model prior gives each model the same prior probability, and the unit information g-prior provides the same information as one observation from the data. As a robustness check, we use a dilution model prior, as proposed by (George, 2010), to account for potential collinearity between explanatory variables. Next, we also employ a combination of the Hannan-Quinn (HQ) g-prior and random model prior and a combination of the BRIC g-prior and random model prior. The HQ g-prior adjusts data quality while the BRIC g-prior minimizes the prior effect on the results. The random model prior gives equal prior probability to every model size (Gechert et al., 2020).

Appendix B.2. Frequentist Model Averaging and Frequentist Check

Table B1: FMA and OLS Results – Capital-to-Asset Ratio

	Frequentist model averaging			Frequentist check (OLS)		
	Coef.	SE	p-value	Coef.	SE	p-value
Constant	21.918	15.240	0.150	-3.880	6.142	0.536
St. Error	0.006	0.019	0.750			
Data characteristics						
No. of observations	0.264	0.049	0.000	0.259	0.054	0.000
Confidential data	6.940	1.434	0.000	6.624	0.774	0.000
Quarterly frequency	-2.107	1.002	0.035			
Other region	3.780	1.802	0.036	5.515	0.998	0.000
Midpoint	-9.750	5.209	0.061	-3.673	2.324	0.134
Corporate credit	0.473	0.430	0.271	0.707	0.258	0.015
Macro-level data	-0.188	0.214	0.382			
US	-1.465	2.512	0.560	-2.352	0.672	0.003
Household credit	-0.237	0.422	0.575			
Single-country	0.452	2.275	0.842	3.450	0.848	0.001
Model specification and estimation						
Missing control variables	2.980	0.638	0.000	3.243	0.757	0.001
Missing interest rate in eq.	-2.398	0.646	0.000	-2.625	0.945	0.013
Other method	-11.121	4.258	0.009	-8.861	1.935	0.000
Fixed-effects method	-3.854	1.574	0.014	-1.929	0.446	0.001
Contemporaneous	-1.843	1.067	0.084			
Lagged by 1Y or more	1.351	0.828	0.103	2.859	0.796	0.002
Add. lag in eq.	-4.631	3.043	0.128			
Time fixed effects incl.	0.197	0.134	0.142			
Crisis	0.400	0.476	0.400			
Other interaction	0.364	0.448	0.417			
Dynamic model	0.283	0.355	0.426			
GMM method	0.147	0.202	0.469			
Some interaction in eq.	-0.178	0.323	0.581			
Add. capital in eq.	0.183	0.442	0.679			
Publication characteristics						
Impact factor	-1.440	0.417	0.001	-1.513	0.259	0.000
Published	3.539	1.069	0.001	3.579	0.639	0.000
Citations	-2.504	0.836	0.003	-1.912	0.397	0.000
Central bank publication	3.427	1.622	0.035	2.564	0.341	0.000
Publication year	-1.660	0.813	0.041	-1.259	0.545	0.035
External variables						
Ext: fin. openness	7.943	2.413	0.001	6.593	0.889	0.000
Ext: inflation deviation	0.230	0.100	0.022	0.332	0.057	0.000
Ext: low for long	-0.284	0.142	0.045	-0.447	0.094	0.000
Ext: 3M interest rate	-0.745	0.531	0.160			
Ext: spread	1.035	0.834	0.215			
Ext: house price growth	-0.022	0.030	0.453			
Ext: unemployment	0.129	0.270	0.634			
Ext: bank size	-0.001	0.026	0.960			
			Adj. R^2		0.690	

Note: The table presents the estimation results of the collected estimate of the beta coefficient on the primary study characteristics and external (structural) variables, searching for potential sources of heterogeneity. The estimation is based on 514 observations from 17 studies. Frequentist model averaging applies Mallow's weights (Hansen, 2007) using the orthogonalization of covariate space suggested by Amini and Parmeter (2012) to reduce the number of estimated models. The frequentist check (OLS) includes the variables with posterior inclusion probability (PIP) estimated by Bayesian model averaging (BMA) above 0.5 and is estimated using standard errors clustered at the study level. A description and summary of all the variables is provided in Table A2.

Table B2: FMA and OLS Results – Regulatory Capital Ratio

	Frequentist model averaging			Frequentist check (OLS)		
	Coef.	SE	p-value	Coef.	SE	p-value
Constant	-13.024	5.862	0.026	-2.046	0.623	0.004
St. Error	-0.054	0.040	0.177			
Data characteristics						
No. of observations	0.242	0.026	0.000	0.236	0.040	0.000
Quarterly frequency	3.541	0.657	0.000	3.138	0.457	0.000
Confidential data	-2.340	0.715	0.001	-2.194	0.318	0.000
Single-country	3.709	1.077	0.001	1.993	0.182	0.000
US	-2.090	0.677	0.002	-1.187	0.217	0.000
Other region	0.872	0.327	0.008	0.398	0.214	0.081
Corporate credit	-0.361	0.172	0.035	-0.175	0.068	0.020
Midpoint	2.452	1.371	0.074			
Household credit	-0.200	0.171	0.240			
Macro-level data	-0.072	0.116	0.536			
Model specification and estimation						
Add. capital in eq.	0.835	0.227	0.000	1.009	0.166	0.000
Lagged by 1Y or more	0.808	0.254	0.001	0.513	0.211	0.026
Add. lag in eq.	3.094	0.924	0.001	1.794	0.186	0.000
Other method	-0.790	0.311	0.011	-1.337	0.163	0.000
Contemporaneous	0.599	0.377	0.112			
Fixed-effects method	-0.418	0.286	0.143	-0.884	0.193	0.000
Time fixed effects incl.	0.099	0.085	0.247			
Crisis	0.253	0.220	0.251			
Missing interest rate in eq.	0.230	0.205	0.263	0.562	0.152	0.002
Dynamic model	-0.259	0.278	0.353	-1.068	0.122	0.000
GMM method	-0.095	0.116	0.412			
Other interaction	0.164	0.215	0.446			
Some interaction in eq.	-0.146	0.225	0.516			
Missing control variables	0.132	0.261	0.614			
Publication characteristics						
Impact factor	-0.428	0.115	0.000	-0.555	0.095	0.000
Central bank publication	-1.712	0.634	0.007			
Publication year	-0.812	0.440	0.065	-0.252	0.175	0.168
Citations	-0.319	0.270	0.238			
Published	0.321	0.340	0.345			
External variables						
Ext: fin. openness	0.968	0.366	0.008	0.210	0.164	0.218
Ext: low for long	-0.102	0.047	0.030			
Ext: 3M interest rate	0.340	0.192	0.077			
Ext: spread	-0.360	0.278	0.196			
Ext: inflation deviation	0.024	0.021	0.252			
Ext: unemployment	0.097	0.109	0.371			
Ext: house price growth	0.007	0.011	0.526			
Ext: bank size	0.006	0.011	0.579			
			Adj. R^2		0.539	

Note: The table presents the estimation results of the collected estimate of the beta coefficient on the primary study characteristics and external (structural) variables, searching for potential sources of heterogeneity. The estimation is based on 652 observations from 18 studies. Frequentist model averaging applies Mallow's weights (Hansen, 2007) using the orthogonalization of covariate space suggested by Amini and Parmeter (2012) to reduce the number of estimated models. The frequentist check (OLS) includes the variables with posterior inclusion probability (PIP) estimated by Bayesian model averaging (BMA) above 0.5 and is estimated using standard errors clustered at the study level. A description and summary of all the variables is provided in Table A2.

Table B3: FMA and OLS Results – Capital Requirements

	Frequentist model averaging			Frequentist check (OLS)		
	Coef.	SE	p-value	Coef.	SE	p-value
Constant	-1.430	1.451	0.324	-2.715	1.382	0.121
St. error	-0.366	0.158	0.020	-0.245	0.318	0.485
Data characteristics						
Total credit	2.726	1.956	0.163	2.301	0.717	0.033
No. of observations	0.153	0.121	0.203	0.175	0.088	0.118
Household credit	1.963	1.621	0.226	3.743	0.857	0.012
Midpoint	-0.475	0.441	0.281			
Model specification and estimation						
Add. lag in eq.	0.937	0.965	0.332			
Some interaction in eq.	-0.413	0.453	0.362			
Lagged by 1Y or more	0.060	0.281	0.831			
Add. capital in eq.	0.120	0.906	0.895			
Publication characteristics						
Publication year	-0.248	0.741	0.738			
External variables						
Ext: low for long	-0.048	0.114	0.673	-0.172	0.041	0.014
			Adj. R^2		0.619	

Note: The table presents the estimation results of the collected estimate of the beta coefficient on primary study characteristics and external (structural) variables, searching for potential sources of heterogeneity. The estimation is based on 229 observations from 5 studies. Frequentist model averaging applies Mallow's weights (Hansen, 2007) using the orthogonalization of covariate space suggested by Amini and Parmeter (2012) to reduce the number of estimated models. The frequentist check (OLS) includes the variables with posterior inclusion probability (PIP) estimated by Bayesian model averaging (BMA) above 0.5 and is estimated using standard errors clustered at the study level. A description and summary of all the variables is provided in Table A2.