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# Monetary Policy Has a Long-Lasting Impact on Credit: Evidence from 91 VAR Studies

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## Abstract

We synthesized 3,175 estimates (454 impulse responses) of the semi-elasticity of credit with respect to changes in the monetary policy rate from 91 vector autoregression studies. We found that monetary policy tightening consistently yields a negative and long-lasting response in both credit volume and credit growth. Several factors contribute to the substantial heterogeneity of the effect sizes in this literature. First, publication selectivity significantly exaggerates the mean reported estimate, because insignificant results are under-reported. Second, researchers' choice of estimation design has a significant impact on the estimated response. Studies using Bayesian methods and including house prices report a smaller decline in credit, while studies with sign restrictions show a larger drop than those using recursive identification.

## Abstrakt

Syntetizovali jsme 3 175 odhadů (454 impulzních odezev) semielasticity úvěrů ve vztahu ke změnám měnověpolitické sazby z 91 studií využívajících vektorovou autoregresi. Zjistili jsme, že zpřísnění měnové politiky konzistentně vede k negativní a dlouhotrvající odezvě objemu úvěrů i růstu úvěrů. K výrazně různorodé velikosti efektů podle této odborné literatury přispívá několik faktorů. Zaprvé, publikační selektivita výrazně nadhodnocuje průměrný uváděný odhad, protože nevýznamné výsledky jsou uváděny méně. Zadruhé, zvolený způsob odhadu má významný vliv na odhadnutou odezvu. Studie využívající bayesovské metody a zahrnující ceny domů vykazují menší pokles úvěrů, zatímco studie se znaménkovými restrikcemi uvádějí výraznější snížení než ty, které používají rekurzivní identifikaci.

**JEL Codes:** C83, E52, R21.
**Keywords:** Bayesian model averaging, credit, interest rates, meta-analysis, monetary policy transmission, publication bias.

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# 1. Introduction

Understanding the credit channel of monetary policy is fundamental for achieving and maintaining both macroeconomic and financial stability. This channel serves as the linchpin in transmitting monetary policy decisions, directly influencing key economic variables such as consumption, investment, and overall growth. By determining market interest rates, monetary policy influences the borrowing costs of all economic sectors. When banks adjust their lending and deposit rates based on policy changes, business investments and household spending decisions are directly impacted.

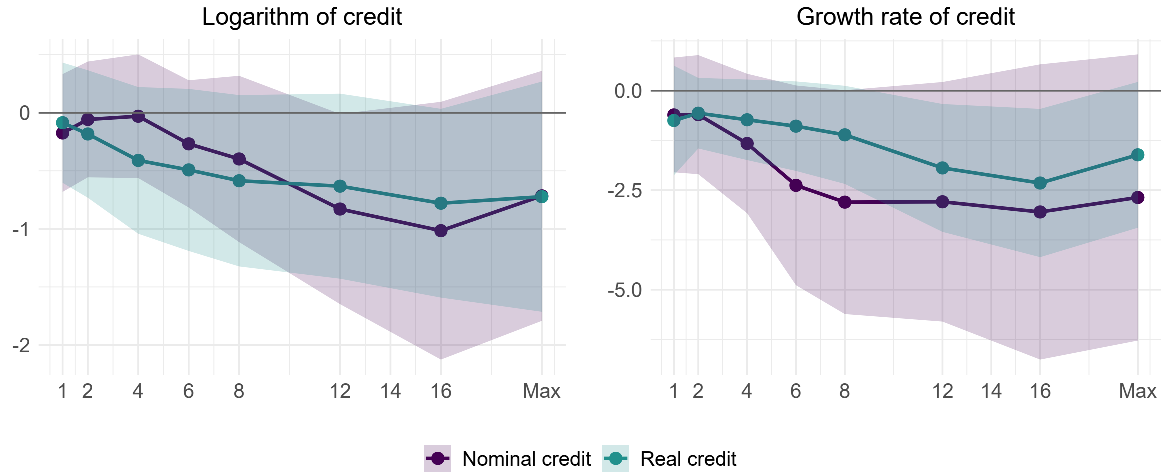
The credit channel of monetary policy is an umbrella term that includes several sub-channels, most notably the balance sheet channel, the bank lending channel, and the risk-taking channel. The balance sheet channel suggests that tighter monetary policy can depress asset prices, reducing borrowers' net worth and hampering firms' ability to secure funding, which curtails their investment (Bernanke and Gertler, 1989, 1995; Kiyotaki and Moore, 1997; Bernanke et al., 1999). This dynamic also influences households, especially those whose wealth is tied to house prices (Iacoviello, 2005), resulting in more stringent credit conditions for them as well. On the other hand, the bank lending channel describes how tighter monetary policy reduces the supply of bank loans, mainly due to increased funding costs (Bernanke and Blinder, 1992). Banks may also be less willing to lend when monetary policy rates are high because of heightened borrower-lender agency costs (Holmstrom and Tirole, 1997; Jiménez et al., 2012) and bank balance sheet constraints (Kishan and Opiela, 2000; Van den Heuvel, 2002). Additionally, the risk-taking channel of monetary policy (Adrian and Shin, 2009; Acharya and Naqvi, 2012) encourages banks to make riskier investments when interest rates are low, leading to less monitoring (Dell'Ariccia et al., 2017) and potential over-allocation to high-risk assets. In terms of the credit channel, banks may be encouraged to expand their balance sheets through leverage and originate excessive amounts of lower-quality credit because of softened lending standards. However, a decline in risk tolerance can reverse this, reducing the available credit.

The general consensus in the literature is that the effects of monetary policy on the real economy, including credit, take time to manifest fully and that the lag can range from several months to over two years. However, there is less agreement on the precise timing of the peak impact, the intensity of the credit response, and the long-term effects beyond the two-year horizon. A related meta-analysis by Ehrenbergerova et al. (2022) synthesizes the impulse responses of house prices to monetary policy shocks from 31 studies. Their findings suggest that the effect bottoms out after two years then gradually returns to zero. However, they draw attention to substantial heterogeneity in the results, influenced by the primary study's estimation design, the prevailing economic conditions, and country-specific factors. Given the even vaster body of literature examining the link between monetary policy and credit, the discrepancies in the findings are even more marked in this case, leading to a range of possible policy implications. Yet there is no comprehensive review pinpointing consistent themes or patterns among these diverse findings. Our paper aims to bridge this gap. The transmission lag, the strength and duration of the effect, the heterogeneity in the literature, and what underpins this heterogeneity are crucial factors for central banks. This is because the calibration of monetary policy should factor in the intricate interplay between monetary policy, the financial system, the broader economy, and inflation.

To examine the credit channel of monetary policy, we collect 454 impulse response functions from 91 studies. The studies use collectively unbalanced data from 68 countries between 1955 and 2019. The impulse responses, derived from vector autoregressions (VARs), are the primary outcomes of these studies. We focus on VAR studies because the impact of monetary policy on credit is typically not immediate but is expected to manifest over the medium to long term. Moreover, our aim is to explore the delay in transmission and the peak effect, which would be challenging if we synthesized research using diverse methodologies. By concentrating on VAR studies, we can evaluate effects within a relatively standardized framework that accommodates multiple endogenous variables. This captures the dynamic relationships and feedback loops among the variables without requiring a strict causal ordering. The ability of VARs to capture the dynamic relationships in a multi-variable system makes them particularly well-suited to analyzing the complex interplay between monetary policy and credit, making them a frequent choice among researchers.

Our meta-analysis yields several key findings. Most notably, the impact of a monetary policy shock on credit is negative and long-lasting. Regardless of whether we consider the growth rate or the log-level transformation of credit, the overall effect is consistently negative and strongest at longer horizons. To put this into perspective, a one percentage point increase in the monetary policy rate leads, on average, to about a 0.9% decrease in outstanding credit and a 2.7 percentage point (pp) decrease in outstanding credit growth after four years. At the maximum horizon reported by researchers, namely, six to seven years, the effects change slightly to about -0.7% and -2.1 pp, respectively.

***Figure 1: Mean Impulse Response Functions: Real vs. Nominal Credit***




***Note:*** The figure displays the average response of credit and credit growth to a one percentage point increase in the monetary policy rate, accompanied by the average 68% confidence interval. The horizons are in quarters. The figure compares studies that use real and nominal credit variables as the main response variables. All responses are calculated as a weighted mean, with the weight being the inverse of the number of estimates collected per study. For context, 55% of the primary studies (65% of the semi-elasticities) in our sample use real credit.

One of the first arguments we usually encounter when presenting these results is that studies should focus on nominal rather than real credit. A long-lasting impact on nominal credit would not be surprising, given that monetary policy is expected primarily to influence interest rates and inflation, both of which directly affect the nominal value of credit. Higher interest rates can dampen borrowing and increase the cost of existing debt, leading to a reduction in the nominal amount of credit. However, this effect should be smaller in real terms, as changes in inflation can mitigate the impact on the real value of credit. Nevertheless, when we compare studies that use nominal credit with those using real credit, we find that their results are very similar (see Figure 1).

The negative and long-lasting responses of credit and credit growth have different interpretations and policy implications. When we assess the response of the logarithm of credit to a monetary policy shock, we find that tighter monetary policy diminishes the actual level of credit by a certain percentage. This is consistent with what we might expect: if borrowing becomes more expensive due to tighter monetary policy, the demand for and supply of credit might fall. The long-lasting effect implies that the decline in the level of credit does not just fade away over time, but persists. This might reflect structural shifts in borrowing behavior or sustained changes in the supply of credit. The long-lasting negative effect on credit growth is rather more profound. It suggests that not only does the credit level drop in response to the shock, but its growth trajectory is also stunted for longer.

A permanent fall in the level of credit in the economy due to a monetary policy action could be worrisome, especially if credit plays a critical role in driving economic activity. Such a fall would suggest that the policy has long-term implications for credit-driven sectors, such as housing and investment. If the monetary policy shock is intended to cool down an overheated credit market, it might be seen as a desired effect. However, if the shock inadvertently depresses credit levels in a struggling economy, it could be a cause for concern. A long-lasting negative effect on credit growth might be even more concerning for policymakers. While a one-time drop in credit levels might be absorbed by the economy, a permanent reduction in credit growth can signal prolonged economic sluggishness, reduced entrepreneurial activity, or hindered economic dynamism. This kind of response might be particularly concerning in economies where growth is heavily reliant on expanding credit.

As informative as the mean impulse response is in summarizing all 91 studies, it may suffer from several issues, which we address in the paper. First, publication bias, stemming from the selective reporting of results, is a concern in meta-analysis. Researchers may unintentionally favor results that align with expected outcomes, particularly in studies using VARs, which often work with limited data sets. Occasionally, these VARs yield unexpected results, such as credit not responding to policy rates or even increasing after a monetary tightening. While some anomalies might indicate model issues, others arise merely by chance due to small sample sizes. It is challenging to pinpoint misleadingly large effects, especially as there is a natural bias towards these effects over null results. Although this does not imply dishonesty, it does reflect the challenges faced in observational research. Adjusting for unexpected outcomes can even enhance the accuracy of an individual study. The identification of shocks in vector autoregressions via sign restrictions follows this principle. Yet the broader academic literature might lean too heavily towards these large, favorable estimates, making it difficult to discern how much individual studies may be influenced by this publication bias.

In the context of the effect of monetary policy on credit, there is a strong expectation of a negative relationship. When results deviate from this expectation, researchers might discard or overlook them, assuming they are based on flawed data or models. Such practices can result in the Lombard effect,[1] where scholars make more effort to align their findings with prevailing beliefs, especially when dealing with ambiguous data. Multiple studies, spanning from economics to finance, have identified significant publication bias. To detect this bias, it is pivotal to ensure an independent relationship between an effect's size and its standard error. Any dependency detected could point to publication bias, where smaller studies with pronounced effects or larger ones with subtle impacts are preferentially published.

[1] The Lombard effect, originally defined in biology, refers to a situation where researchers emphasize significant results or complex methods in order to stand out in a crowded field, similar to speaking louder in a noisy room. McCloskey and Ziliak (2019) suggest that this behavior can lead to overstated findings.

To test and correct for publication bias, we use Precision-Effect Estimate with Standard Error (PEESE) as proposed by Stanley and Doucouliagos (2014), which accounts for the fact that smaller studies might report larger effects by regressing the effect size on the square of the standard error. In order to account for multiple sources of heterogeneity in the estimation of publication bias, we use both the random effects and three-level meta-analysis models (Cheung, 2014; Gao et al., 2017). Aside from PEESE, we also employ the Caliper test, which relaxes the assumption that the standard error is exogenous. This assumption of exogeneity may not always hold, given that publication bias can influence both the point estimates and the standard errors (Stanley, 2005). All the techniques confirm the presence of publication bias, with the exaggeration being at least twofold in the medium term (responses after four to eight quarters) and the long term (responses after twelve or more quarters). Despite that, we still identify a statistically significant negative effect of monetary policy tightening on credit in both the medium and long term.

Second, the mean impulse response can conceal significant differences between primary studies regarding estimation design. Credit dynamics and their response to monetary policy shocks might be influenced by factors such as the phase of the business cycle (Assenmacher-Wesche and Gerlach, 2010), (macro)prudential regulations (Iacoviello and Minetti, 2003; Malovaná and Frait, 2017; Vollmer, 2022), and the current monetary policy stance (Borio and Gambacorta, 2017; Cao et al., 2023). Additionally, measurement issues such as recursive identification and the omission of important variables such as house prices (Hofmann, 2004) can affect the mean response. We bring a unique angle with our meta-analysis, drawing from a comprehensive dataset and covering a broader range of countries and time periods than prior research. Unlike individual studies, our sample of more than 450 impulse responses produced in various contexts allows us to examine the heterogeneity in the transmission systematically.

To better understand the heterogeneity between the effect sizes, we collected information on the data characteristics (e.g., the type of credit or region and the period of analysis), the model specification and estimation (e.g., the inclusion of house prices and identification via sign restrictions), and publication details (e.g., the number of citations per year) of each study. In total, we collected about 30 variables that reflect the context in which the impulse responses were estimated. To address model uncertainty when relating the estimated semi-elasticities to these explanatory variables, we use Bayesian model averaging (Raftery et al., 1997; Eicher et al., 2011; Steel, 2020). First and foremost, the publication bias remains present and statistically significant even when accounting for individual heterogeneity drivers.

Regarding data characteristics, our results suggest that the type of lender and borrower significantly shapes the credit response to monetary policy changes. Monetary policy shocks influence household credit significantly more. Lending to non-financial firms is also more strongly affected, especially in the long term. Furthermore, bank credit reacts less sensitively to interest rate changes than non-bank credit does. Significant findings also emerge from the model specification and estimation, with studies employing Bayesian estimation and integrating house prices showing a less negative response. On the other hand, studies employing sign restrictions find a larger decrease in credit than those using recursive identification. Lastly, publication characteristics, notably the paper being published in a peer-reviewed journal, influence the reported effects, with the responses being generally less negative.

We contribute to the existing literature in several ways. First, apart from clearly adding to the debate on the credit channel of monetary policy and the determinants of transmission heterogeneity, our meta-analysis enriches the literature on monetary policy neutrality and hysteresis. Typically, these concepts pertain to the long-term impact on *output*. Yet the modern literature closely associates the business cycle with the evolution of *credit* (Jordà et al., 2013, 2015, 2016; Mian and Sufi, 2018; Mian et al., 2021), creating an essential link between leverage and real economic activity.[2] The earlier empirical literature mainly supports the long-run neutrality of money, suggesting that changes in the money supply do not have persistent effects on real variables such as output (King and Watson, 1997). Nonetheless, empirical evidence also indicates potential deviations from this neutrality, particularly under specific conditions or economic specifications (King and Watson, 1997; Fisher and Seater, 1993). Some studies highlight the real effects of money, especially when factors such as financial intermediaries and broader credit markets are considered (Cooley and Hansen, 1995). More recent research, focusing on the long-run effects of transitory shocks, documents persistent effects of monetary policy shocks (Jorda et al., 2023), as well as government spending shocks (Antolin-Diaz and Surico, 2022) and transitory corporate tax shocks (Cloyne et al., 2022).

Second, our study contributes to the literature on publication selectivity. Going back to Rosenthal (1979), researchers have highlighted the issue that many studies with non-significant or null results often remain unpublished, residing in researchers' file drawers. Our contribution builds upon the seminal works in this domain, confirming that publication selectivity remains a significant concern in empirical research. The effects in the economic literature are typically exaggerated by a factor of two (Ioannidis et al., 2017).[3] Recent meta-analyses on the impact of monetary policy have detected this publication bias, identifying significant bias in the reported effects of monetary policy on the consumer price level (Rusnák et al., 2013; Nguyen et al., 2021), output (Nguyen, 2020), and house prices (Ehrenbergerova et al., 2022). This bias has been confirmed not only in economics and certain bank-related studies (Zigraiova and Havranek, 2016; Campos et al., 2019; Gechert et al., 2022), but also in market-based finance (Astakhov et al., 2019; Kim et al., 2019; Gric et al., 2023).

Third, our study contributes to the literature that emphasizes the significance of design and estimation techniques in obtaining accurate results. The influential paper by Christiano et al. (1999) describes how different models and shock identifications can lead to varied interpretations and magnitudes of the response to a shock. A more recent paper by Baumeister and Hamilton (2015) discusses the use of sign restrictions and compares it to other methods. Essentially, the authors argue for the importance of using sign restrictions with informative priors in Bayesian estimation to yield meaningful and economically consistent results in VAR analyses. By adopting this approach, researchers can prevent a multitude of implausible outcomes that might emerge when relying exclusively on sign restrictions. Numerous other meta-analyses in economics consistently demonstrate that model specification and the selected estimation technique significantly influence the heterogeneity observed between effects and studies (Rusnák et al., 2013; Zigraiova et al., 2021; Ehrenbergerova et al., 2022; Malovaná et al., 2024, 2023).

[2] Using 140 years of historical macrofinancial data, Jordà et al. (2013, 2015) show that financial crises lead to costlier recessions and that expansions driven by excessive credit result in deeper downturns and slower recoveries. Jordà et al. (2016) show that loose monetary conditions can lead to surges in house prices and mortgage loans, making financial crises more likely, particularly in the postwar period. Mian and Sufi (2018) posit that household debt and financial conditions play a critical role in driving business cycles, especially through their impact on household demand. Mian et al. (2021) highlight the role of increasing indebtedness in shaping demand patterns, suggesting that high debt levels can stifle consumer demand and have broader macroeconomic implications.

[3] Ioannidis et al. (2017) studied more than 64,000 estimates from 159 economic survey papers and found that the adequately powered estimates are exaggerated, typically by a factor of two and with one-third inflated by a factor of four or more.

## 2. The Semi-Elasticity Dataset and Stylized Facts

We gather semi-elasticity estimates of how changes in monetary policy rates impact credit. To avoid comparing disparate variables, we concentrate solely on estimates based on VAR models.[4] A general structural VAR model takes the following form:

$$AY_t = \sum_{i=1}^{p} B_i Y_{t-i} + \varepsilon_t, \tag{1}$$

where $Y_t$ is a vector of endogenous variables, including the monetary policy rate and credit, at time $t$, $A$ and $B_i$ are impact matrices that capture the contemporaneous and lagged relationships between the variables in $Y_t$ for each lag $i$, and $\varepsilon_t$ is a vector of structural shocks. Alongside the monetary policy rate and credit, the set of endogenous variables usually contains output and consumer prices. Depending on the specification, it may also include other variables, such as house prices, exchange rates, and long-term interest rates. To estimate equation (1), researchers rewrite it into a reduced-form representation. The primary outputs of VAR models – the responses of the endogenous variables to structural shocks – are commonly presented as impulse response functions in graphical format. These visual representations facilitate interpretation for readers and encompass responses over multiple time horizons.

To gather relevant primary studies, we employed Google Scholar as our main search database, as it searches the full text of articles, in addition to titles, abstracts, and keywords. Following the recommendations of Havránek et al. (2020), we screened the top 800 articles returned by a specific query related to credit, interest rates, and monetary policy (for more details on our search strategy, including the exact query, see Appendix A.1). We included all studies with credit as an endogenous variable, regardless of its transformation. However, studies using the credit-to-GDP ratio were excluded, though they constituted only a small fraction of the studies identified.

The initial screening involved reading the abstracts of the Google Scholar results to exclude irrelevant studies. Subsequently, we employed two supplementary literature search techniques to identify additional relevant articles: forward citation searching and backward citation searching.[5] Through the use of both, we were able to extend our database and ensure more comprehensive and robust coverage of the literature related to the relationship between interest rates and loans. Through forward and backward citation searching, we identified an additional 127 potentially relevant studies, bringing our total of studies screened to 927 articles. After screening these additional ones, we selected the relevant studies for our meta-analysis.

Before incorporating studies into our analysis, we followed several inclusion criteria. Studies must: (1) use a VAR model approach for quantitative comparability of the semi-elasticities collected, (2) report confidence intervals around the mean impulse response function of credit to enable standard error calculations, (3) use the monetary policy rate or some other short-term interest rate as a proxy for monetary policy, and (4) preferably be published in a peer-reviewed

[4] Such estimates are commonly derived from two types of models: dynamic stochastic general equilibrium (DSGE) models and VAR models. While the structure of both is founded on theory, VAR models are generally more data-driven and therefore better suited for meta-analysis methods.

[5] Forward citation searching involves examining the references cited within relevant studies that we have already identified. This process helps uncover other potentially useful articles that may not have been detected by our initial search, as they could be connected to the topic through a chain of citations. Backward citation searching is the opposite process. With this technique, we explore the articles that have cited the relevant studies we initially identified. By doing so, we can locate more recent articles that build upon or extend the research of the primary studies.

journal, with working papers replaced by the corresponding journal publications where available. After applying these decision rules and not restricting the time period or location of the estimations, we identified 91 primary studies for our analysis, which collectively use unbalanced data from 68 countries between 1955 and 2019.

***Table 1: Paper Selection Procedure: Reasons for Exclusion***

| Reason for exclusion | Number of articles |
|---|---|
| Studies excluded based on abstract or title | 303 |
| Studies excluded due to lack of correspondence or data | |
| Unsuitable methodology (not VAR) | 157 |
| Theoretical model or simulation | 154 |
| Unsuitable credit variable | 107 |
| Unsuitable shock variable (not short-term interest rate) | 69 |
| Missing uncertainty measure (standard error, confidence intervals, etc.) | 21 |
| Working paper excluded due to existing journal version* | 11 |
| International spillover analysis | 10 |
| Size of shock not retrievable | 4 |
| Total excluded articles | 836 |

***Note:*** The table presents the reasons for omitting papers from our analysis. While some papers might have multiple reasons for exclusion, we highlight only the most relevant one here. *For each working paper we assessed as appropriate to include in the meta-analysis, we checked whether a journal version exists. We did not do the opposite, i.e., check for working paper versions of journal articles.

For each paper we decided to leave out of our sample, we coded a reason for the exclusion (Table 1). Based on the title and abstract, we excluded 303 articles that we immediately identified as not relevant to the topic of our meta-analysis. As we are interested in empirical analyses using vector autoregression models only, we excluded 157 articles which use different methodology, mostly time series or panel data regression, and 154 theoretical studies. The list was further narrowed down based on our criteria for credit and shock variables and because of missing precision variables (standard error, confidence interval, etc.). As the last step, we verified whether a journal version was available for each of the working papers in our sample and, where necessary, replaced the working paper with its peer-reviewed published paper version. For a detailed overview of the selection process and the papers included, please refer to Appendix A.1.

From these 91 studies, we collected 3,175 point estimates of the response of credit to a monetary policy shock at different time horizons. Specifically, we collected responses after one, two, four, six, eight, twelve, and sixteen quarters. Additionally, we collected the response at the maximum horizon reported by the study, which could help us identify the persistence of the shock's impact and possible long-lasting effects. In each case, we carefully measured the pixel coordinates to recover the numerical estimate as precisely as possible using a web-based tool.[6] Each data point was meticulously recorded by one author and verified by a more experienced colleague to ensure

[6] For this purpose, we used the web tool https://automeris.io/WebPlotDigitizer/. Specifically, we extracted each impulse response function from the article of interest and uploaded it into the web tool. We then aligned the axes and selected the desired points (the estimates and confidence intervals at each horizon) in the figure. Points that were imprecisely selected were adjusted using the zoom tool. Finally, we downloaded the selected points into an Excel file and combined them with the others.

accuracy. Ambiguities in coding were collaboratively resolved by the team, reducing the likelihood of human error.

For each horizon except the maximum one, we collected between 319 and 454 responses. At the maximum horizon, which is 26 quarters on average, we collected 255 responses from 49 studies. The largest numbers of responses were recorded after four and six quarters, which are the horizons most relevant to monetary policy transmission.

We categorized our responses into three distinct groups: short, medium, and long horizons. Following the approach of previous meta-analyses on monetary policy (Ehrenbergerova et al., 2022; Rusnák et al., 2013), the short horizon comprises responses in the first and second quarters, the medium horizon includes responses after four, six, and eight quarters, and the long horizon encompasses those after twelve and sixteen quarters. The primary motivation for this grouping is to increase the number of observations for estimation and to facilitate economic interpretation of the effects, as the impact of monetary policy changes is expected to vary in strength across different horizons.[7]

As the next step, we standardize the effects of each impulse response to show the reaction to a one percentage point increase in the policy rate, and we calculate the standard error of each estimate using the reported confidence intervals. We distinguish between two transformations of credit variables that appear in our dataset: growth rate and log-level transformation. The majority of the responses collected – about 70% – have log-level transformation, meaning that the response is interpreted as the percentage change in credit after a 1 pp increase in the monetary policy rate. The remaining 30% have growth rate transformation of credit, meaning that the response is interpreted as the percentage point change in annual credit growth after a 1 pp increase in the monetary policy rate. Before delving into the analysis, we winsorized the effect sizes for each horizon and transformation at 2.5% from both sides.

Table 2 provides a detailed view of the distribution of the effect sizes across various horizons. We present both simple unweighted statistics and statistics weighted by the inverse of the number of estimates collected from each study. Given that we obtained a different number of estimates from each study, ranging from 3 to 192 with an average of 81, these weighted statistics offer a more balanced initial view of the average effect size.

We identified several important characteristics of the effect sizes. Firstly, regardless of the credit transformation, the effect is, on average, negative and strongest at the long horizon. The average effect strengthens until the sixteen-quarter horizon and then diminishes slightly at the maximum horizon. Since the maximum horizon is between six and seven years, we can consider the impact of a monetary policy shock on credit to be highly persistent and long-lasting.

Secondly, the distribution of the effect sizes is negatively skewed. In the case of log-level transformation of credit, the distribution is increasingly asymmetrical with longer horizons. When we look at the growth rate of credit, there is a significant skew in the distribution of the semi-elasticities at all horizons, but this skewness lessens with longer horizons.

[7] Monetary policy actions generally focus on the medium to long horizon, as indicated by Taylor and Wieland (2012) and demonstrated by ECB (2021) and FOMC (2022). Recent debates on policy integration highlight the necessity of extending the monetary policy horizon for this policy to exert a more substantial influence on the economy, as noted by Borio et al. (2022). However, the empirical data up to this point does not provide a clear conclusion and varies across countries. According to Havranek and Rusnak (2013), the degree of financial development is a critical factor in determining the duration of the transmission delay.

***Table 2: Summary Statistics of Semi-Elasticities Collected***

| Horizon | | Obs. | Studies | Unweighted Mean | Unweighted 5% | Unweighted 95% | Unweighted Skew. | Weighted Mean | Weighted 5% | Weighted 95% | Weighted Skew. |
|---|---|---|---|---|---|---|---|---|---|---|---|
| Panel A: Log-level of Credit | | | | | | | | | | | |
| Short | 1 | 286 | 62 | 0.04 | -1.78 | 1.87 | 0.84 | -0.13 | -2.16 | 0.93 | -0.69 |
| | 2 | 295 | 64 | 0.07 | -1.48 | 1.86 | 1.71 | -0.12 | -2.59 | 1.09 | 0.19 |
| Medium | 4 | 299 | 64 | -0.01 | -2.05 | 3.78 | 0.92 | -0.23 | -2.59 | 1.03 | -0.22 |
| | 6 | 309 | 64 | -0.23 | -2.74 | 2.75 | 0.05 | -0.39 | -3.59 | 0.57 | -1.34 |
| | 8 | 295 | 61 | -0.38 | -3.69 | 2.60 | -0.59 | -0.50 | -5.02 | 0.80 | -1.69 |
| Long | 12 | 289 | 57 | -0.63 | -4.88 | 1.72 | -1.79 | -0.72 | -5.06 | 0.69 | -2.22 |
| | 16 | 229 | 46 | -0.88 | -5.61 | 1.55 | -1.80 | -0.87 | -5.49 | 0.52 | -2.08 |
| | Max | 192 | 33 | -0.75 | -4.44 | 1.25 | -1.86 | -0.72 | -4.02 | 0.68 | -2.26 |
| Panel B: Growth Rate of Credit | | | | | | | | | | | |
| Short | 1 | 118 | 24 | -1.39 | -14.93 | 2.29 | -2.96 | -0.68 | -3.87 | 2.82 | -3.87 |
| | 2 | 146 | 28 | -0.90 | -8.34 | 1.44 | -2.46 | -0.58 | -4.63 | 1.37 | -2.64 |
| Medium | 4 | 146 | 28 | -1.07 | -10.27 | 1.29 | -3.06 | -1.01 | -11.08 | 1.26 | -2.84 |
| | 6 | 145 | 27 | -1.18 | -11.88 | 1.28 | -3.26 | -1.51 | -15.81 | 0.97 | -2.75 |
| | 8 | 145 | 27 | -1.35 | -11.76 | 0.57 | -3.13 | -1.81 | -15.49 | 0.89 | -2.47 |
| Long | 12 | 128 | 24 | -1.58 | -11.06 | 0.47 | -2.37 | -2.36 | -12.96 | 0.72 | -1.73 |
| | 16 | 90 | 20 | -1.88 | -11.00 | 0.41 | -2.00 | -2.71 | -14.03 | 0.73 | -1.51 |
| | Max | 63 | 16 | -1.45 | -8.91 | 0.32 | -1.92 | -2.12 | -9.59 | 0.44 | -1.30 |

***Note:*** The table presents summary statistics of the collected semi-elasticities on the response of credit and annual credit growth to a one percentage point increase in the monetary policy rate. The responses of credit are in percent, while the responses of credit growth are in percentage points. The horizons are in quarters. A significant number of studies report the impulse response function up to the 16th quarter, after which the number of reported effects decreases and the maximum reported horizons vary. The responses at each study's maximum horizon were therefore compiled into a "Max" category.

Thirdly, the majority of the estimates lie within one standard deviation from the mean, indicating very few instances of large responses of credit to monetary policy shocks. Collectively, across all horizons, 86% of all the effect sizes reside within one standard deviation. For the log-level transformation of credit, this typically ranges between -2% and 1%. For the growth rate transformation of credit, it is generally between -5% and 2%.[8]

The negative and long-lasting responses of outstanding credit and credit growth have different interpretations.[9] In the latter case, not only does the credit level drop in response to the shock, but its growth trajectory is also stunted for longer. While a one-time drop in credit levels might be absorbed by the economy over time, a permanent reduction in credit growth can signal prolonged economic sluggishness, reduced entrepreneurial activity, or hindered economic dynamism. To better understand the impact of a monetary policy rate increase, we collected annual credit growth data from the same time period, region, and credit definition used in each primary study's analysis (i.e., the exact data entering every regression analysis).

[8] There are only a few large responses, the most significant being -8.5% for the log-level of credit and -19% for the growth rate of credit, but these are isolated cases.

[9] The vast majority of the studies use outstanding loans. Only one study (21 elasticities extracted from three impulse response functions) refers to new credit. These responses are generally larger but do not present an outlier with respect to the rest of the sample.

Table 3 presents summary statistics for observed credit growth and credit growth after a one percentage point increase in the monetary policy rate. The average annual credit growth from these studies is positive, with a slight skew to the right. A monetary policy tightening, assuming it has a long-lasting effect, would shift the distribution towards more negative values. However, most studies would still indicate positive annual growth.[10] This implies that the given estimates are not unreliable; they would not thrust the economy into an immediate credit crunch. Instead, they suggest that transitory monetary policy shocks would slowly propagate through the economy, potentially causing long-lasting changes.

***Table 3: Average Annual Credit Growth Across Studies: Observed and Shocked at the Long Horizon***

| | Unweighted | | | | Weighted | | | |
|---|---|---|---|---|---|---|---|---|
| | Mean | 5% | 95% | Skew. | Mean | 5% | 95% | Skew. |
| Observed credit growth | 7.46 | 1.82 | 11.85 | 0.67 | 8.60 | 3.92 | 13.58 | 1.21 |
| Credit growth after MP shock | 5.85 | -2.74 | 11.92 | -1.07 | 6.30 | -6.82 | 13.70 | -0.65 |

***Note:*** The first row of the table presents summary statistics of average annual credit growth across all primary studies that focus on the response to credit growth. Average credit growth was calculated using the same time series that entered the regression analysis in each study. The second row shows summary statistics of the difference between average annual credit growth and the estimated response of annual credit growth to a one percentage point increase in the monetary policy rate over the long horizon.

Figure 2 displays the mean response of credit to a one percentage point increase in the short-term monetary policy rate for both transformations. The figure also includes the 68% confidence interval, corresponding to one standard deviation on either side of the mean, a convention in the VAR literature.[11] The left panels compare the simple and weighted means calculated using all effect sizes, and their trajectories are comparable. The impulse responses bottom out after four years with a 0.9% decrease in credit and a 2.7% decrease in credit growth following a one percentage point increase in the policy rate.

The mean response, however, is barely statistically significant, even with 68% confidence intervals. This can be attributed to several factors, one of which is the sample size. Generally, as the sample size decreases, the standard errors of the regression coefficients increase. A larger standard error for a coefficient means that there is more variability in the possible values for that coefficient. A wider spread of potential values makes it harder to reject the null hypothesis that the coefficient is equal to zero, implying a lower statistical significance. Thus, in the right panels of Figure 2, we compare the mean response of single-country studies to panel data studies, which generally have larger sample sizes. The effect is clearly statistically significant for panel data studies with larger sample sizes, aligning with our expectation.[12,13]

[10] Such a shock would likely push only a handful of studies into negative credit growth territory.

[11] Approximately half of all the studies in our sample report 68% (or one standard deviation) confidence intervals, 21% of studies report 90% confidence intervals, and 26% of studies report 95% (or two standard deviations) confidence intervals. The remaining few studies report specific confidence intervals, such as 80% or 84%.

[12] We observe a similar trend when categorizing studies by the total number of observations into small-sample (bottom quartile), large-sample (top quartile), and medium-sample (all others). See Figure A2 in the Appendix for details.

[13] The other reasons why we identify more statistically significant results for studies with larger samples are that small samples exhibit greater variability, are more susceptible to outlier influence, and often do not represent the population accurately. Additionally, they typically have reduced statistical power to detect an effect.

***Figure 2: Mean Impulse Response Functions***


***Note:*** The figure displays the average response of credit and credit growth to a one percentage point increase in the monetary policy rate, accompanied by the average 68% confidence interval. The horizons are in quarters. The left panels compare the simple and weighted means, with the weight being the inverse of the number of estimates collected per study. The right panels show weighted averages for both the sub-sample of single-country studies and panel data studies.

Interestingly, studies conducted on larger samples, while reporting more statistically significant results, also report generally weaker effects. This may suggest publication bias in our dataset. The analysis of publication bias usually assumes that the effect size and its standard error are independent. If smaller studies consistently show larger effects, it might indicate a bias towards impactful results. We address this formally in Section 3.

Another issue with the mean impulse response is that it hides key distinctions between the primary studies in terms of estimation design, such as data used, model specification and estimation, and shock identification. A meta-analysis, unlike primary studies, offers insights into how these factors affect transmission strength. We explore their impact in Section 4.

### 2.1 The Role of the Maximum Reported Horizon

One could argue that the estimates at the maximum horizon might be biased if authors of primary studies only report impulse response functions up to the point of convergence to zero. This suggests that effects at longer horizons could be omitted, under the assumption they equate to zero. To address this concern, we analyze the effects at the maximum reported horizon from each impulse response function in every primary study, as summarized in Table 4.

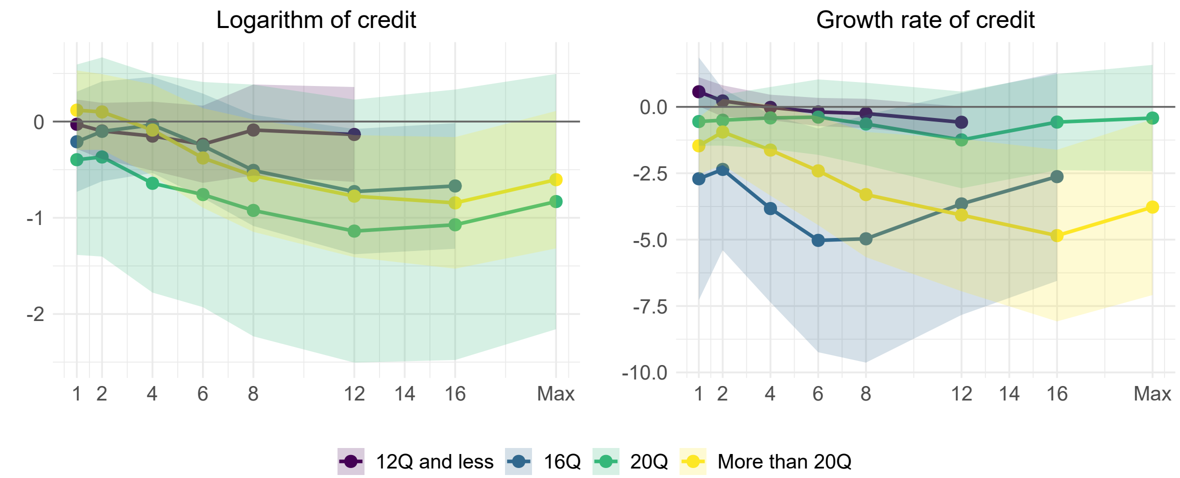
Firstly, most studies report effects up to the 20th quarter, with a strongly negative average effect at this horizon. Studies with a maximum horizon shorter than 20 quarters tend to show a less pronounced effect, yet far from zero and mostly negative. From this, one might infer that if the impulse response functions in these studies were extended beyond the 20-quarter horizon, the reported effect could be expected to intensify, similar to those studies reporting effects at longer horizons. Furthermore, when comparing the trajectories of the average impulse response functions across different maximum reported horizons, it is evident that studies ending the responses before the 20-quarter horizon rarely report convergence to zero (see Figure 3).

***Table 4: Summary Statistics of Semi-Elasticities Collected: Maximum Reported Horizon***

| | | | Unweighted | | | | Weighted | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| Max horizon | Obs. | Studies | Mean | 5% | 95% | Skew. | Mean | 5% | 95% | Skew. |
| Log-level of Credit | | | | | | | | | | |
| 6 | 16 | 4 | -0.29 | -1.36 | 0.23 | -0.99 | -0.17 | -1.36 | 0.20 | -1.70 |
| 8 | 6 | 4 | 0.17 | -1.78 | 2.10 | -0.06 | 0.18 | -2.32 | 1.93 | 0.11 |
| 12 | 59 | 11 | -0.22 | -1.37 | 0.49 | -1.69 | -0.13 | -1.42 | 0.49 | -2.17 |
| 16 | 20 | 5 | -0.66 | -5.14 | 0.21 | -2.29 | -0.80 | -5.51 | 0.10 | -2.07 |
| 20 | 68 | 17 | -0.99 | -7.33 | 1.30 | -1.51 | -0.83 | -7.33 | 0.93 | -1.87 |
| 24 | 59 | 7 | -0.44 | -4.42 | 2.12 | -1.11 | -0.31 | -3.35 | 0.93 | -1.74 |
| 32 | 57 | 9 | -0.71 | -2.58 | 0.18 | -1.27 | -0.71 | -3.18 | 0.00 | -1.60 |
| Growth Rate of Credit | | | | | | | | | | |
| 4 | 1 | 1 | 0.06 | 0.06 | 0.06 | - | 0.06 | 0.06 | 0.06 | - |
| 8 | 17 | 3 | 0.01 | -0.11 | 0.16 | 2.58 | 0.18 | -0.10 | 0.47 | 0.65 |
| 12 | 32 | 4 | -0.44 | -1.80 | 0.54 | -2.39 | -0.58 | -4.77 | 0.34 | -2.31 |
| 16 | 2 | 2 | -0.06 | -0.09 | -0.04 | 0.00 | -0.06 | -0.09 | -0.04 | 0.00 |
| 20 | 31 | 8 | -0.57 | -3.10 | 0.45 | -1.48 | -0.42 | -3.06 | 0.49 | -1.58 |
| 24 | 8 | 4 | -3.55 | -9.37 | 0.32 | -0.38 | -3.92 | -9.60 | 0.31 | -0.30 |
| 32 | 24 | 4 | -1.89 | -9.42 | 0.22 | -1.36 | -3.61 | -9.60 | 0.03 | -0.38 |

***Note:*** The table presents summary statistics of the collected semi-elasticities on the response of credit and annual credit growth to a one percentage point increase in the monetary policy rate. The responses of credit are in percent, while the responses of credit growth are in percentage points. The horizons are in quarters. Only the maximum reported horizons of the impulse response functions are summarized.

***Figure 3: Mean Impulse Response Functions: Maximum Reported Horizon***




***Note:*** The figure displays the average response of credit and credit growth to a one percentage point increase in the monetary policy rate, accompanied by the average 68% confidence interval. The horizons are in quarters. The figure differentiates between studies based on the maximum reported horizon of the impulse response function in each primary study. All responses are calculated as a weighted mean, with the weight being the inverse of the number of estimates collected per study.

## 3. Publication Bias

Publication bias can be defined as a systematic difference between the distribution of results produced during research and the distribution of published results. It usually means that only those estimates which are significant or have the "correct" sign get published. Such selection leads to exaggeration of the reported estimates and greatly affects the field of economics (e.g., Ioannidis et al., 2017; Astakhov et al., 2019; Campos et al., 2019; Gechert et al., 2022). While it makes little sense to build papers on unlikely results, systematically ignoring unlikely results leads to an upward bias that tends to increase the reported magnitude of effects. This makes the average substantially larger than the "true" effect (Stanley, 2008; Stanley et al., 2010).

The mainstream literature expects the effect of monetary policy shocks on credit to be negative (Gertler and Gilchrist, 1993; Barraza et al., 2019). As the prior knowledge about the direction of the estimated effect is strong, researchers might question or even throw away estimates that do not comply with this economic logic. Estimates above zero might have a psychological effect, suggesting that the data or model specification is incorrect. As a result, such estimates might be discarded. Researchers may also have similar feelings about estimates that are statistically weaker or smaller in magnitude, even though they have the expected sign.

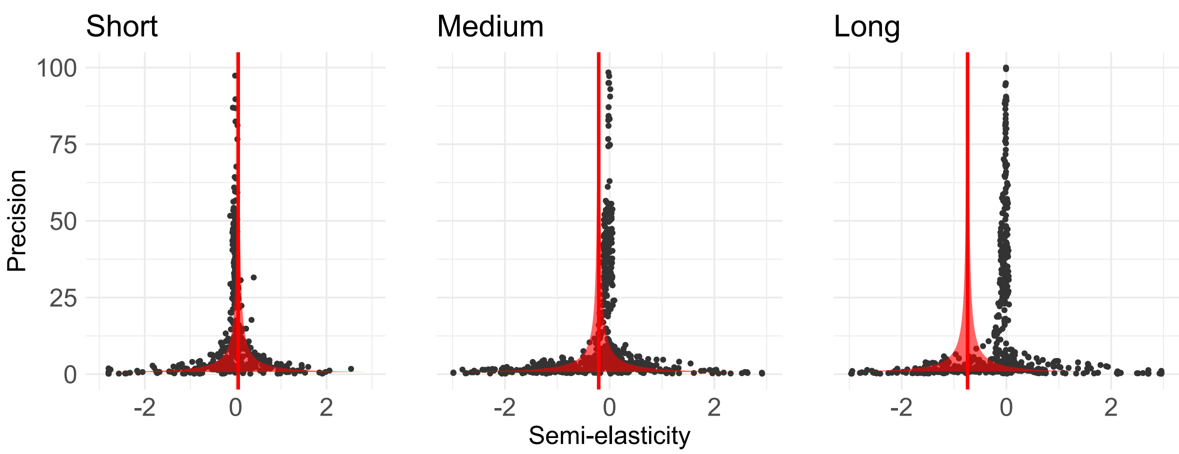
Consequently, findings that do not fit the generally accepted narrative, i.e., where the researcher estimates the effect to be positive or not statistically significant, may thus succumb to publication bias. This may lead to the Lombard effect, i.e., to a situation where researchers try harder to achieve estimates consistent with their intuition (McCloskey and Ziliak, 2019). This happens especially if the data are imprecise or noisy. Given the numerous options available as regards both study design and estimation approach, one can always "try harder" to find significance or estimates with the "correct" sign (Card and Krueger, 1995).

The analysis of publication bias is typically based on the assumption that the estimated effect size and its standard error are independent. This foundational assumption ensures that the observed effect size is not influenced by its precision or by the size of the sample from which it was derived. An independent relationship suggests no systematic bias, while any detected relationship could indicate publication bias. For example, smaller, less precise studies that consistently show larger effects might be indicative of publication bias in favor of impactful results. Conversely, larger, more precise studies with smaller effects could be more readily accepted in publications.

The funnel plot is a visual tool designed to illustrate this very principle. By plotting the effect size against its precision (measured by the inverse of its standard error), the resulting graph ideally forms a funnel-shaped distribution. In the absence of publication bias, studies with high precision (usually represented by larger sample sizes) will cluster around the true effect size, while less precise studies will scatter more widely but symmetrically around this mean, forming the wide base of the funnel. Any noticeable asymmetry in the plot, especially if smaller studies skew towards showing larger effects, is a visual cue of potential publication bias. This means that certain results, often those which are more significant, are preferentially published, thus violating the core assumption of independence between effect size and precision (Egger et al., 1997; Stanley et al., 2010).

Figure 4 displays the funnel plot of the log-level transformation of credit across all three horizons (represented by black points), while Appendix B1 performs a similar analysis for the growth rate transformation. The stylized red funnel outlines the ideal distribution of effect sizes given the sample mean. The vertical red line marks the mean value of our collected effect sizes for each horizon, and the shaded region portrays its 95% confidence bands. Studies within this red region exhibit effect sizes that align, in terms of statistical variability, with the average effect. Conversely, effects outside this region suggest potential publication bias, as their sizes deviate significantly from their precision (standard error).

***Figure 4: Funnel Plots: Log-Level of Credit***




***Note:*** In the absence of publication bias, the plots form inverted funnels symmetric around the most precise estimates. The individual panels depict the funnel plots for the different horizons: Panel A – short, Panel B – medium, and Panel C – long. Estimates with a precision greater than 100 or a magnitude below -3 or above +3 are excluded from the figure for ease of exposition but are included in the statistical analyses.

Notably, the distribution is shifted to the right across the medium and especially the long horizon. If the true effect was equal to the simple mean of the collected effects, the individual observations would fall within the red funnel. Their position outside it, along with the apparent shift in the distribution mean and the fat tails, indicates publication bias. It seems that smaller studies – those with larger standard errors – tend to report higher effect sizes (and potentially discard opposite ones). While publication bias is a concern, significant heterogeneity in the effect sizes could also account for this pattern. Such disparities might arise from variations in data and methodology among the primary studies, i.e., from smaller studies systematically using different data and methodology. We will delve deeper into this by formally testing and correcting for publication bias in the following sub-section and subsequently exploring heterogeneity in Section 4.

### 3.1 Empirical Tests

The literature offers various empirical methods to test and correct for publication bias. As mentioned earlier, these methods typically assume that the estimated effect size and its standard error are independent. A well-established method is Precision-Effect Estimate with Standard Error (PEESE), which is recommended when the effect size is believed to be non-zero. This method has been shown to outperform the older linear Precision Effect Test (PET), which, unlike PEESE, is sensitive to non-zero true effects (Stanley and Doucouliagos, 2014). For instance, if theories, such as those related to the bank lending channel of monetary policy (Gertler and Gilchrist, 1993; Barraza et al., 2019), suggest a non-zero impact of monetary policy shocks on credit, PEESE would be preferable to PET. Next, we describe both correction approaches alongside the models that are used to estimate them.

The PET, introduced by Egger et al. (1997), is the simplest method for correcting for publication bias. It is based on a simple linear association between the effect size $\hat{\beta}_{i,j}$ and the corresponding standard error $\hat{SE}_{i,j}$ for each study $i$ and effect $j$:

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

where $\alpha$ captures the effect beyond bias (also referred to as the corrected effect or true effect) and $\gamma$ expresses the intensity of publication bias. In the case of no publication bias, the effect sizes and their standard errors are not correlated. If the $\gamma$ coefficient is significant, publication bias is present.

The PEESE method relaxes the assumption of linearity between the effect size and its standard error. The reason for squaring the standard error is that small studies, i.e., those with larger standard errors, are more liable to report inflated effect sizes. The PEESE approach has recently been popularized in economic and business meta-analyses by, for example, Costa-Font et al. (2011), Haelermans and Borghans (2012), Doucouliagos et al. (2014), and Zigraiova et al. (2021). The corresponding equation is as follows:

$$\hat{\beta}_{i,j} = \alpha + \gamma \hat{SE}^2_{i,j} + \varepsilon_{i,j} \tag{3}$$

where the coefficients $\alpha$ and $\gamma$ stand for the effect beyond bias and the intensity of publication bias, respectively.

Following the guidelines and practices in the existing meta-analytical literature (Duan et al., 2020; Dettori et al., 2022; Xue et al., 2022; Ma et al., 2023), we employ two models to estimate equations (2) and (3): the random effects (RE) model and the three-level (3L) model. The primary distinction between these models lies in their assumptions regarding the distribution of effect sizes across studies and potentially across other levels of variation, such as between studies, within studies, or within clusters of studies. They model the variance and structure of the data.

The RE model assumes that there is variation in the effect sizes both within and between studies, accounting for both types of variance to derive an overall effect size estimate. The model is expressed as:

$$\hat{\beta}_{i,j} = \alpha + \delta_j + \gamma \hat{SE}^2_{i,j} + \varepsilon_{i,j}, \tag{4}$$

where $\delta_j$ is a random effect for between-study variability while $\varepsilon_{i,j}$ accounts for within-study error (random error). Both effects are assumed to be normally distributed with zero mean.

The three-level model extends the random effects model and adds a third level of analysis, accounting for within-cluster heterogeneity. This variability within groups of studies could be caused by differences in primary study characteristics, such as data or methods:

$$\hat{\beta}_{i,j,k} = \alpha + \delta_k + \kappa_{j,k} + \gamma \hat{SE}^2_{i,j,k} + \varepsilon_{i,j,k} \tag{5}$$

where $\kappa_{j,k}$ is a random effect for between-study variability within cluster $k$ while $\delta_k$ is a random effect for between-cluster variability. $\varepsilon_{i,j,k}$ accounts for within-study error. Similarly to the random effects model, all three effects are assumed to be normally distributed with zero mean. All models are estimated with weights equal to the inverse of the estimate's variance in order to control for heteroskedasticity (Stanley et al., 2013; Stanley, 2005).

### 3.2 Results

When presenting and interpreting the results of publication bias, we favor PEESE over PET. This is because we believe that the impact of a monetary shock on credit dynamics is non-zero, at least over certain horizons. Moreover, we prefer the three-level model's estimation method, as there is evident high heterogeneity not only among the individual effect estimates, but also among the studies. The three-level model can better account for such heterogeneity. The results for the log-level credit transformation are provided in Table 5. We present the results for the growth rate credit transformation in Appendix B2. For comparison, we also include PET estimates and those using the random effects model in Appendix B1, as these are frequently used in the literature.

The three-level PEESE model reveals a publication bias at both the medium and long horizons. Specifically, the coefficient on the standard error, which measures the bias, is about -0.07 and statistically significant in the medium term and increases to -0.16 (still statistically significant) over the long term. As a result, the statistically significant mean effect beyond bias stands at -0.11 in the medium term and -0.14 in the long term. For the short term, neither an effect beyond bias, nor any publication bias is evident. Although the effects are weaker when we correct for publication bias, their temporal pattern remains consistent, showing the least impact in the short term and the largest effect over the long term. The pattern for the growth rate transformation of credit is very similar. The mean effect beyond bias is significant for both the medium and long horizon, with the biggest negative impact occurring in the long term, and negative publication bias is present across all horizons.

Comparing the PEESE to the PET model, PET gives more weight to publication bias, effectively bringing the corrected effect close to zero. As discussed above, this tendency was noted by Stanley and Doucouliagos (2014), who therefore suggest using PEESE when the effect is believed to be non-zero. Moreover, comparing the random effects model to the three-level model, even stronger emphasis is given to publication bias in the former. This is probably due to unaccounted for variance between effects, which is not sufficiently factored in by the random effects model, leading to overestimation of the publication bias.

The results also provide valuable insights into the sources of heterogeneity. This is best reflected in the $I^2$ statistic, which measures the amount of effect heterogeneity as a percentage of the total variance. Within the context of the three-level model, $I^2$ represents the variance due to sampling error (level 1), the heterogeneity within studies (level 2), and the heterogeneity between studies (level 3). Notably, in our results, the entire variance is attributed to between-study heterogeneity across all horizons. This confirms that the main source of heterogeneity in our dataset arises from differences between the primary studies, due largely to their different estimation techniques and data sources. The substantial heterogeneity between studies further elucidates why the mean effects beyond bias are considerably weaker than the sample means. We delve deeper into this heterogeneity in the next section, presenting implicit effects for specific subsets of the primary studies with distinct characteristics.

***Table 5: Estimation of Publication Bias: Log-Level of Credit***

| | Short | Medium | Long |
|---|---|---|---|
| Effect beyond bias (constant) | -0.002 | -0.107*** | -0.142*** |
| | (0.012) | (0.020) | (0.027) |
| Publication bias ($SE^2$) | 0.024 | -0.066*** | -0.158*** |
| | (0.024) | (0.022) | (0.015) |
| $I^2$ level 1 (%) | 0 | 0 | 0 |
| $I^2$ level 2 (%) | 0 | 0 | 0 |
| $I^2$ level 3 (%) | 100 | 100 | 100 |
| Observations | 581 | 903 | 710 |
| Studies | 64 | 65 | 57 |

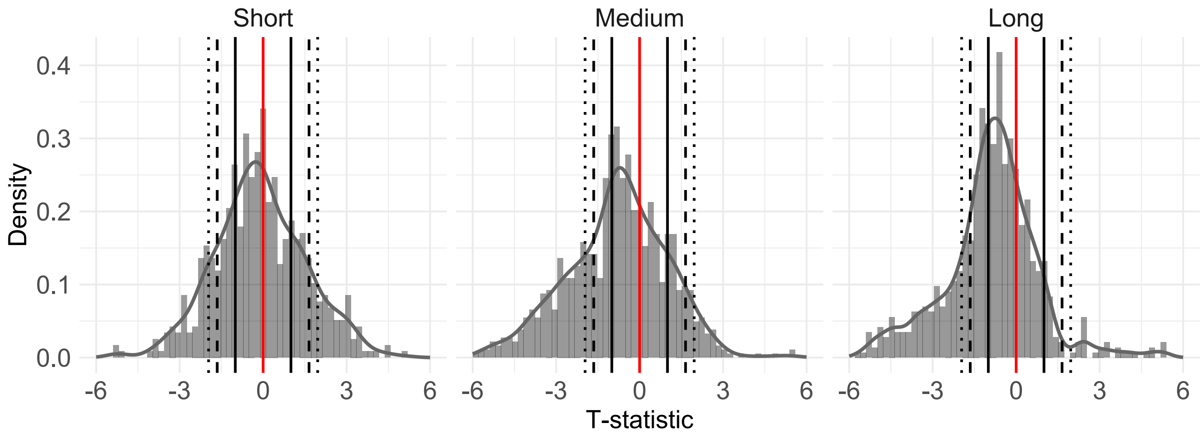
***Note:*** This table presents the results of Precision-Effect Estimate with Standard Error (PEESE) using the three-level meta-analysis model. Additional specifications can be found in the Appendix. Standard errors, clustered at the study level, are reported in parentheses. The model is estimated with weights equal to the inverse of each estimate's variance to control for heteroskedasticity. $I^2$ quantifies the effect heterogeneity as a percentage of the total variance. Within the context of the three-level model, $I^2$ captures the variance attributed to sampling error (level 1), within-study heterogeneity (level 2), and between-study heterogeneity (level 3). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Publication bias can arise from multiple sources, and while our baseline regression provides valuable insights on the presence of this bias, it does not thoroughly investigate its underlying causes. Primarily, there are two likely causes: one, researchers or publishing entities may have a preference for estimates showing the "correct" negative sign; and two, there may be a propensity to publish estimates that are statistically significant. We investigate these channels further.

To uncover the sources of publication bias, we first examine the distribution of the t-statistics, paying close attention to how the estimates cluster around significance thresholds. Figure 5 presents a histogram of the t-statistics, with vertical lines marking the critical values for 5% (dotted line), 10% (dashed line), and 32% (solid line) statistical significance. In the absence of publication bias, the t-statistics should follow an approximately normal distribution (Egger et al., 1997). This seems to hold for the short horizon. However, the distribution becomes significantly skewed for the medium and long horizons, suggesting a preference for negative estimates. The mode of the distribution for these two horizons shifts towards negative values, which is consistent with the effect being negative. However, the number of estimates on the right side of the distribution falls sharply, particularly near the significance thresholds, resulting in a distinct, fat left tail. Furthermore, there is pronounced over-reporting of negative estimates near the 10% and 5% significance levels, suggesting that researchers search for significant effects.

When we examine the t-statistics distribution for the growth rate credit transformation (reported in Appendix B2), the patterns become even more evident. For the medium and long horizons, the distribution starts to form multiple modes, and both positive and non-significant values appear to be under-reported. Overall, we find that both sources of publication bias might be at work.

***Figure 5: Distribution of T-Statistics: Log-Level of Credit***




***Note:*** The vertical lines denote the critical value associated with 5% (dotted line), 10% (dashed line) and 32% (solid line) statistical significance. Approximately half of all the studies report 68% (or one standard deviation) confidence intervals, 21% of studies report 90% confidence intervals, and 26% of studies report 95% (or two standard deviations) confidence intervals. The remaining few studies report specific confidence intervals, such as 80% or 84%. We exclude estimates with large t-statistics from the figure but include all in the regressions. In the absence of publication bias, the distribution of the t-statistics should be approximately normal.

We next conduct an empirical analysis to further support our findings that the publication bias is due to an inclination towards reporting both negative estimates and significant findings. First, we extend equation (5) by adding a dummy variable which equals one if the estimate is statistically significant at the 10% level. Second, we alter the dummy to equal one if the estimate has a negative sign. Lastly, we integrate both these conditions into a combined dummy. We regress the estimate against the respective dummy and its interaction with the squared standard error of the estimate. In each specification, the parameter of the interaction term captures the strength of the publication bias. The results in Table 6 confirm our previous findings. The documented publication bias appears to be influenced by both the negative sign of estimates and their statistical significance. The impact of the negative sign is larger in the medium term (as seen in column 2 vs. column 5), while at longer horizons, the emphasis shifts to statistical significance (as is evident when comparing columns 3 and 6).

***Table 6: Estimation of Publication Bias With Interaction Terms: Log-Level of Credit***

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| | Dummy: I(t-stat<1.65) | | | Dummy: I($\beta$ <0) | | | Dummy: I(t-stat<1.65, $\beta$ <0) | | |
| | Short | Medium | Long | Short | Medium | Long | Short | Medium | Long |
| Constant | -0.009 | -0.082*** | -0.102*** | -0.003 | -0.116*** | -0.148** | 0.011 | -0.073*** | -0.101*** |
| | (0.011) | (0.023) | (0.028) | (0.011) | (0.041) | (0.072) | (0.010) | (0.019) | (0.028) |
| $SE^2$ | -0.006 | -0.048** | -0.112*** | 0.415*** | 0.405*** | 0.210*** | 0.054** | 0.021 | -0.106*** |
| | (0.020) | (0.021) | (0.012) | (0.064) | (0.040) | (0.043) | (0.025) | (0.022) | (0.012) |
| Dummy | 0.000 | -0.005 | -0.009 | -0.003 | -0.006 | -0.012 | -0.003 | -0.007 | -0.012 |
| | (0.004) | (0.032) | (0.040) | (0.003) | (0.052) | (0.089) | (0.006) | (0.033) | (0.050) |
| $SE^2 \times$Dummy | 0.720*** | -0.531** | -0.961*** | -0.600*** | -0.694*** | -0.440*** | -4.567*** | -2.226*** | -1.090*** |
| | (0.200) | (0.224) | (0.156) | (0.068) | (0.049) | (0.045) | (0.795) | (0.405) | (0.175) |
| $I^2$ level 1 (%) | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| $I^2$ level 2 (%) | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| $I^2$ level 3 (%) | 100 | 100 | 100 | 100 | 100 | 100 | 100 | 100 | 100 |

***Note:*** This table presents the results of Precision-Effect Estimate with Standard Error (PEESE) using the three-level meta-analysis model. Additional specifications can be found in the Appendix (Tables B3–B6). Standard errors, clustered at the study level, are reported in parentheses. The model is estimated with weights equal to the inverse of each estimate's variance to control for heteroskedasticity. $I^2$ quantifies the effect heterogeneity as a percentage of the total variance. Within the context of the three-level model, $I^2$ captures the variance attributed to sampling error (level 1), within-study heterogeneity (level 2), and between-study heterogeneity (level 3). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

### 3.3 Additional Tests of Publication Bias

Thus far, we have determined that publication bias is driven by a preference for reporting negative estimates and significant findings. This trend could reflect a researcher's strong personal a priori belief or pressure to publish significant estimates aligned with prevailing theories or dominant literature streams. We will now explore other tests frequently used in the literature to detect publication bias and assess their applicability in our context.

First, our estimation framework assumes the standard error is exogenous. However, this assumption of exogeneity may not always hold, given that publication bias can influence both point estimates and standard errors. These are computed differently across various studies. Stanley (2005) posits that since the standard error is itself estimated, the effect sizes might be susceptible to attenuation bias. To test the exogeneity condition, we use the Caliper test, a method robust to standard error endogeneity, as introduced by Gerber and Malhotra (2008a,b). The Caliper test examines the distribution of the t-statistics near conventional significance levels, such as 1.65 for 10% significance and 1.96 for 5% significance. In the absence of publication bias, we would expect the t-statistics to be evenly distributed around these thresholds. We implement the test with three t-statistic thresholds (1, 1.65, 1.96) and three caliper sizes denoting the width of the test region around these thresholds (0.1, 0.2, 0.3). The results can be found in Appendix B.2. The findings indicate that publication selection exists across all estimates and horizons for both credit transformations. The evidence is especially pronounced if we restrict the test sample only to negative estimates.[14]

[14] Other methods robust to endogeneity of the standard error can be used to test for publication bias. These include the p-curve method introduced by Simonsohn et al. (2014a,b) and the tests by Elliott et al. (2022). However, these methods assume no heterogeneity among the estimates, which contrasts sharply with our sample. As demonstrated in Table 6, our sample displays significant heterogeneity, making these methods inappropriate for our context.

Second, recent literature introduces a range of novel estimation methods that further relax the linearity assumption. Central to these methods is the assumption that more precise estimates are less prone to publication bias. As such, the focus is on isolating these precise estimates to compute the average effect (see Ioannidis et al., 2017; Furukawa, 2019; Bom and Rachinger, 2019; Andrews and Petroulakis, 2019).[15] While this approach has proved useful in many studies (e.g., Bajzik et al., 2020; Havranek et al., 2021; Malovaná et al., 2023), it is not appropriate for our scenario due to the presence of two sources of publication bias and the substantial heterogeneity in our sample. Most of these methods operate by determining a precision threshold, either exogenously or endogenously, below which estimates are discarded. Consequently, many estimates with larger standard errors are omitted. These estimates, though, could be indicative of the primary study's data and design rather than merely being a byproduct of selection bias.

## 4. Drivers of Heterogeneity

In the prior section, we identified significant heterogeneity across studies, which is not merely a result of publication bias.[16] Generally, when the aim of a meta-analysis is to focus on a structural parameter derived from a well-constructed model that correctly describes the data-generating process, the heterogeneity remains on the low side. On the other hand, if the meta-analysis revolves around a reduced-form parameter or when the model does not correctly represent the data-generation process, the heterogeneity of the collected estimates tends to be pronounced, largely influenced by the model's specifics and the nature of the data. This phenomenon is particularly evident when modeling bank behavior, indicating that data characteristics and model specification hold considerable sway on the effect under examination (Malovaná et al., 2024, 2023). Moving forward in this section, we delve deeper into the tools offered by meta-analysis to understand the origins of the heterogeneity.

### 4.1 Data and Methodology

In this sub-section, we outline the main study characteristics used to explain the observed heterogeneity, along with our estimation approach for identifying the most significant factors behind it. We collected information on the data characteristics, model specification and estimation, and publication details of each study to better understand the discrepancies between them.[17] A list of all the variables, along with their definitions and summary statistics, can be found in Tables C1, C2, and C3 in the Appendix.

[15] Ioannidis et al. (2017) propose a procedure that focuses only on estimates with statistical power above 80%. The stem-based method by Furukawa (2019) suggests using only the stem of the funnel plot, that is, a portion of the most precise estimates. This portion is determined by minimizing the trade-off between bias (raising the number of imprecise estimates that are included) and variance (reducing the number of estimates included). The kinked method proposed by Bom and Rachinger (2019) builds on the idea that estimates are automatically reported if they cross a certain precision threshold; therefore, they introduce an "endogenous kink" technique that estimates this threshold. The selection model by Andrews and Petroulakis (2019) first identifies the "conditional publication probability" (the probability of publication as a function of a study's results) and then use it to correct for publication bias. The underlying intuition involves jumps in publication probability at conventional p-value cut-offs.

[16] The variability both across and within studies is clearly visible in Figures A3 and A4 in the Appendix.

[17] Initially, our collection included a broader set of potential characteristics, but around twenty were omitted because they only appeared in a few studies, not representing a significant portion of the literature. Additionally, five variables were dropped due to high correlation with others. Such selection criteria are consistent with typical practices in meta-analyses (Bajzik et al., 2020; Bajzik, 2021). A list of the variables excluded is available upon request.

In terms of *data characteristics*, we collect and dummy-code information on the type of credit from the perspective of the borrower (household, firm, total) and lender (bank, non-bank financial institution, total). We suspect that some credit categories might be more sensitive to monetary policy shocks than others, given the similar pattern identified for macroprudential policy in a meta-analysis by Malovaná et al. (2023). Moreover, several studies indicate that when extending credit, non-bank financial institutions might respond to monetary policy shocks differently than banks (Nelson et al., 2017; Hodula and Libich, 2023; Holm-Hadulla et al., 2023). We also take into account the geographical focus of the study, distinguishing between papers centered on Europe, Asia, the USA, and the rest of the world (including mixed panels). This differentiation could highlight how varying regional structural characteristics influence the credit channel of monetary policy. Furthermore, we dummy-code studies using real, detrended, and seasonally adjusted credit, and studies with lower data frequency. Lastly, we control for the primary study's data length (the number of years it spans), its depth (the number of countries analyzed), and its midpoint year.

Regarding *model specification and estimation*, we control for the type of model (simple VAR, panel VAR, other) and the shock identification method (sign restrictions, Cholesky decomposition, other). We also take into account the estimation approach (whether it is Bayesian or frequentist). Moreover, we factor in the number of lags and specific variables included in the VAR model, such as asset prices, exchange rates, and interest rates other than the monetary policy rate. Numerous previous meta-analyses in economics have consistently shown that the model specification and the chosen estimation technique contribute significantly to the heterogeneity observed between effects and studies (Rusnák et al., 2013; Zigraiova et al., 2021; Ehrenbergerova et al., 2022; Malovaná et al., 2024, 2023).

Finally, to account for the influence of the article's *publication characteristics*, we include four additional variables. First, we capture the number of citations each study has received on Google Scholar, as it can proxy for the influence of the study (Bjork et al., 2014). We also include a dummy reflecting the publication of the primary study in a peer-reviewed journal and the RePEc discounted recursive impact factor for the outlet where the study was published. Given evidence from prior meta-analytical studies (Valickova et al., 2015; Araujo et al., 2020; Bajzik et al., 2020), we test how these characteristics impact the size and direction of the effect. Additionally, since we gathered details on the primary studies' focus, we add this to the heterogeneity analysis, noting that researchers may be more careful in identifying the credit channel of monetary policy when it is the study's main concern.

To avoid the dummy variable trap, we omit one variable within each group of characteristics. For example, while retaining the dummy variables for credit taken out by households and non-financial firms, we drop the dummy for total credit. Typically, we exclude the most prevalent characteristic in each group, aiming to pinpoint differences from the most frequently adopted approach in the existing literature. This results in 27 explanatory variables.

Given the model uncertainty in assessing such a large number of characteristics, we employ Bayesian model averaging (BMA) to identify significant heterogeneity drivers. While a simple OLS model can be straightforward to solve, it can lack parsimony if all the variables are included or face bias if some are omitted. This can lead to issues of model uncertainty and best model selection. BMA avoids prematurely rejecting any variable crucial for our heterogeneity-exploring analysis. BMA estimates a number of models with different combinations of explanatory variables and then calculates a weighted average across those models, with the weights being proportional to the model fit and complexity. As such, it allows for the possibility that different models may be appropriate for different subsets of the data and that the results may be sensitive to the choice of model specification. BMA was pioneered in the social sciences by Raftery (1995) and Raftery et al. (1997) and is regularly used in the recent meta-analytical literature (Ehrenbergerova et al., 2022; Gechert et al., 2022; Malovaná et al., 2023). Through BMA, we seek to: (1) discern what chiefly explains the differences between studies, (2) compute a mean elasticity based on significant drivers, and (3) demonstrate how researchers' model-setting choices impact the final estimate.

BMA aims to provide the best possible approximation of the distribution of each regression parameter by using all of the possible model combinations and comparing them directly. Since 27 explanatory variables enter the analysis, including the standard error, they can provide $2^{27}$ possible model combinations, which makes the estimation time-consuming. To cut the estimation time, we employ the Markov chain Monte Carlo (MCMC) process with the Metropolis-Hastings algorithm, which only goes through the most probable models (Zeugner and Feldkircher, 2015). The probability of each model is then turned into a weight, and a posterior model probability (PMP) is assigned to each model. This measure indicates how good each model is in comparison with the others. Based on the weights of the models and the variables included in them, a posterior inclusion probability (PIP) is also derived. It represents the sum of the posterior model probabilities over all the models where the variable is included.

BMA requires explicit priors for both the model (the model prior) and the regression coefficients (the g-prior). Following Eicher et al. (2011), we use a combination of the unit information g-prior (UIP) and the uniform model prior as a baseline. This combination expresses our lack of knowledge about the specific probabilities of the individual parameter values, since the prior assigns the same weight to the regression coefficient of zero and to the observed data. To ensure robustness, we follow recent meta-analyses in economics (Bajzik et al., 2020; Gechert et al., 2022; Malovaná et al., 2023) and evaluate the influence of different prior choices on our results. 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.[18] Further, we also employ a combination of the Hannan-Quinn (HQ) g-prior and the random model prior (Fernandez et al., 2001; Ley and Steel, 2009) and a combination of the BRIC g-prior and the random model prior. The HQ g-prior adjusts data quality and is recommended, for instance, by Feldkircher and Zeugner (2012) and Zigraiova et al. (2021). The BRIC g-prior, which is widely used in the 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., 2022). This way, we show a lack of prior knowledge about the model's distribution. The results presented below remain robust to our choice of priors. For more details, see Figures C7 and C8 in the Appendix.

Following the approach proposed in Jeffreys (1961), Eicher et al. (2011), and Havránek et al. (2020), we only interpret variables with a PIP above 0.5, with the following classification. A variable is classified as decisive if its PIP is higher than 0.99, strong if it is between 0.95 and 0.99, substantial if it is between 0.75 and 0.95, and weak if it is between 0.5 and 0.75.

[18] In the case of high correlation, the determinant is close to one and the model receives little weight. The opposite holds in the case of low correlation. This prior has been used in meta-analysis by Bajzik et al. (2020), for instance.

## 4.2 Results

In this section, we present the numerical BMA results for all three horizons, considering the log-level transformation of credit (see Table 7). Numerical results for the growth rate of credit, alongside the corresponding visual outputs, can be found in Table C4 and Figures C1–C6 in the Appendix. We refer to these results in the main text as necessary.

***Table 7: Bayesian Model Averaging: Log-Level of Credit***

| | Short horizon | | | Medium horizon | | | Long horizon | | |
|---|---|---|---|---|---|---|---|---|---|
| | P. mean | P. SD | PIP | P. mean | P. SD | PIP | P. mean | P. SD | PIP |
| Constant | -0.711 | | 1.000 | -1.538 | | 1.000 | 0.144 | | 1.000 |
| $SE^2$ | -0.325 | 0.040 | 1.000 | -0.349 | 0.030 | 1.000 | -0.616 | 0.031 | 1.000 |
| **Data characteristics** | | | | | | | | | |
| Bank credit | 0.116 | 0.069 | 0.823 | 0.208 | 0.036 | 1.000 | 0.224 | 0.041 | 1.000 |
| Non-bank credit | 0.016 | 0.035 | 0.211 | 0.009 | 0.023 | 0.165 | 0.004 | 0.015 | 0.089 |
| Credit to firms | -0.006 | 0.023 | 0.105 | -0.002 | 0.012 | 0.065 | -0.065 | 0.054 | 0.664 |
| Credit to households | -0.159 | 0.045 | 0.992 | -0.204 | 0.030 | 1.000 | -0.070 | 0.055 | 0.695 |
| Asia | -0.005 | 0.020 | 0.090 | 0.0001 | 0.006 | 0.033 | -0.0003 | 0.007 | 0.034 |
| USA | 0.055 | 0.074 | 0.431 | 0.001 | 0.010 | 0.042 | -0.005 | 0.022 | 0.089 |
| Other countries | -0.135 | 0.087 | 0.781 | -0.212 | 0.042 | 1.000 | -0.001 | 0.015 | 0.044 |
| Real credit | 0.015 | 0.037 | 0.181 | -0.003 | 0.014 | 0.076 | 0.003 | 0.014 | 0.071 |
| SA credit | 0.001 | 0.013 | 0.044 | -0.002 | 0.012 | 0.053 | -0.0005 | 0.010 | 0.042 |
| Detrended credit | -0.122 | 0.050 | 0.935 | -0.0003 | 0.006 | 0.034 | 0.001 | 0.009 | 0.045 |
| Lower data frequency | -0.003 | 0.017 | 0.066 | -0.033 | 0.045 | 0.408 | -0.033 | 0.045 | 0.411 |
| No. of countries | -0.053 | 0.083 | 0.345 | -0.001 | 0.010 | 0.039 | 0.006 | 0.023 | 0.090 |
| No. of years | -0.003 | 0.022 | 0.063 | 0.000 | 0.008 | 0.038 | -0.038 | 0.049 | 0.435 |
| Midyear | -0.0002 | 0.011 | 0.042 | 0.003 | 0.015 | 0.068 | -0.004 | 0.019 | 0.067 |
| **Model specification and estimation** | | | | | | | | | |
| Panel VAR | -0.034 | 0.061 | 0.288 | -0.003 | 0.015 | 0.060 | 0.013 | 0.038 | 0.144 |
| Other VAR | 0.004 | 0.020 | 0.078 | -0.007 | 0.021 | 0.120 | 0.001 | 0.008 | 0.039 |
| Bayesian estimation | 0.629 | 0.058 | 1.000 | 0.566 | 0.044 | 1.000 | 0.125 | 0.054 | 0.925 |
| Sign restrictions | -0.306 | 0.050 | 1.000 | -0.195 | 0.037 | 1.000 | -0.001 | 0.010 | 0.042 |
| Controls: Asset prices | 0.162 | 0.052 | 0.971 | 0.190 | 0.033 | 1.000 | 0.092 | 0.054 | 0.827 |
| Control: Exchange rate | -0.002 | 0.013 | 0.049 | 0.003 | 0.015 | 0.072 | -0.001 | 0.008 | 0.039 |
| Controls: Other IR | 0.006 | 0.022 | 0.107 | 0.004 | 0.016 | 0.085 | 0.0004 | 0.007 | 0.034 |
| No. of lags | 0.018 | 0.040 | 0.212 | 0.000 | 0.007 | 0.035 | -0.005 | 0.020 | 0.099 |
| **Publication characteristics** | | | | | | | | | |
| Citations | 0.001 | 0.010 | 0.040 | -0.003 | 0.014 | 0.067 | 0.001 | 0.008 | 0.038 |
| Impact factor | -0.020 | 0.045 | 0.215 | -0.062 | 0.053 | 0.652 | -0.001 | 0.010 | 0.045 |
| Main focus | 0.179 | 0.057 | 0.977 | 0.176 | 0.034 | 1.000 | 0.003 | 0.015 | 0.068 |
| Published | 0.207 | 0.043 | 1.000 | 0.216 | 0.032 | 1.000 | 0.172 | 0.034 | 1.000 |

***Note:*** The table presents the estimation results of the collected semi-elasticities on the primary study characteristics for specifications with log-level transformation of the credit variable. The BMA procedure employs a combination of the uniform model prior and the unit information g-prior recommended by Eicher et al. (2011). The coefficients are standardized. P. mean, P. SD, and PIP stand for posterior mean, posterior standard deviation, and posterior inclusion probability, respectively.

We have confirmed the existence of *publication bias* across all horizons, even in the multivariate BMA setting that accounts for primary study characteristics. Compared with the univariate regressions discussed in Section 3, the publication bias identified here is substantially larger, but with a similar escalating trend over longer horizons. The coefficient capturing the publication bias is around -0.3 for both the short and medium term but surges to -0.6 over the long term. All of these coefficients have a PIP of 1.0. This is broadly in line with the findings of Ehrenbergerova et al. (2022) (the effect of monetary policy on house prices) and Rusnák et al. (2013) (the effect of monetary policy on the price level).

Several *data characteristics* significantly influence the effect. Most notably, lender type (be it banks, non-banking financial institutions, or both) and borrower type (whether households, firms, or all sectors) are important. Studies focusing on bank-originated credit generally report a less negative effect across all horizons than those that consider lending from all financial sectors. This result indicates that non-bank credit is more sensitive to changes in interest rates than bank credit. Although the research on monetary policy transmission through non-banks is limited in comparison to studies involving banks, it is broadly in line with our findings. The existing evidence suggests that non-banks reduce the effectiveness of the bank lending channel of monetary policy both in the US (Elliott et al., 2023) and in Europe (Cucic and Gorea, 2022). Next, monetary policy shocks have a stronger effect on household credit than on borrowing by other sectors. The impact on lending to non-financial firms is also more negative, though less significant in the short and medium term. In the long term, monetary policy appears to have a stronger negative influence on lending to both households and firms.

Regarding other data characteristics, studies using detrended credit variables exhibit a significantly more negative response in the short term. This may reflect the removal of some distortions from the data that affect the results unnecessarily. Moreover, credit decreases more in response to monetary policy shocks in “other countries”, which in our case form a disparate group composed of both advanced countries and emerging markets, including developing countries from Africa and Latin America. Given that the other three regional groups consist largely of advanced countries, the stronger negative effect of the “other” group may be driven by the existence of some less developed countries in the mix. This aligns with the literature showing that the credit channel tends to be dominant among the monetary policy transmission mechanisms in low-income countries due to their lower level of financial development (Mishra et al., 2012; Mishra and Montiel, 2013; Abuka et al., 2019). Another reason for the larger negative response of “other countries” to monetary policy shocks might be that these studies encompass greater variation within their samples.

Among the *model specification and estimation characteristics*, three emerge as particularly significant. Firstly, studies employing Bayesian estimation consistently yield less negative estimates than those using the frequentist approach. Secondly, models that integrate asset prices, including residential and commercial real estate prices, similarly show less negative effects. This is aligned with some recent studies pointing to the importance of asset (house) prices in the nexus between credit, asset prices, and monetary policy. For example, Ehrenbergerova et al. (2022) identify a strong significant effect of monetary policy on house prices using a meta-analysis of 31 studies covering 45 countries and 69 years. Their analysis also shows amplified effects in countries with more developed mortgage markets. Furthermore, Kim and Mehrotra (2018) demonstrated that when house prices are accounted for in the VAR model, the impact of monetary policy on credit tends to weaken. The recent literature recognizes that asset prices play an important role in the transmission and usually includes asset prices in the VAR model (Barraza et al., 2019; Hanisch, 2019; Franz, 2020).

Thirdly, the negative effect of a monetary policy shock on credit is larger if the shock was identified using sign restrictions as opposed to Cholesky decomposition (as in the majority of the remaining cases) or other approaches. In this case, we focus strictly on the sign restrictions imposed on the credit variable, which are negative in all cases. This finding is in line with Ehrenbergerova et al. (2022), who identified a significant impact of identification via sign restrictions on the effect of monetary policy on house prices.

The impact of all three factors is biggest in the short to medium term but diminishes over the long term. The effect of identification through sign restrictions on the strength of monetary policy transmission is not significant in the long term. This aligns with the general intuition behind sign restrictions, which primarily affect the immediate aftermath of a shock. Over the longer horizon, endogenous mechanisms should play a more important role.

The effect is significantly influenced by two *publication characteristics*. Firstly, results published in a peer-reviewed journal tend to be less negative. In line with this, Malovaná et al. (2023) noted the influence of publication on the interplay between bank capital and lending, while Bajzik et al. (2020) found similar trends in their Armington elasticity research and Ugur et al. (2020) in the area of R&D spillovers and productivity. However, it should be mentioned that this is not a rule and many other meta-analytical studies do not identify a significant difference between working papers and journal papers (Doucouliagos and Stanley, 2013; Havranek and Kokes, 2015; Havranek et al., 2018b,a). This may be an indication either of limited bias in both working papers and journal publications, or of the selection process being so strong that certain results are not even communicated in unpublished manuscripts (Doucouliagos and Stanley, 2013; Bruns et al., 2019).

Secondly, studies focusing primarily on the identification of the credit channel of monetary policy generally exhibit less negative effects, particularly in the short and medium term. Yet for the long term, the effects they identify align closely with studies that have a different primary focus (cf. Table 11 in Section 5). This suggests that, irrespective of the central theme of the research, the impact of a monetary policy shock on credit remains persistently negative in the long term.

The results regarding the growth rate of credit are qualitatively similar, although statistically less significant. The key variables in heterogeneity analysis include publication bias and the type of credit. Differentiating between the Bayesian and frequentist estimation approaches yields significantly different results in the short term only. The remaining variables then affect the strength of the credit channel mainly in the long term. For more details on these results, see Table C4 in the Appendix.

### 4.3 Model Comparison

In Section 3, we identified both publication bias and significant effect heterogeneity, largely due to variations between primary studies. We then determined key characteristics of these studies as major contributors to this heterogeneity. It remains to be seen whether these characteristics can enhance the fit of the model sufficiently. Thus, we incorporate the substantial heterogeneity drivers (i.e., those with a PIP above 0.75) into the univariate models from Section 3. We then evaluate this enriched (full) model against the original (reduced) one to assess any improvements and better data fitting.

In Table 8, we present test statistics comparing the two models over three horizons. The results show that the expanded model with the identified drivers offers a significantly better explanation of effect heterogeneity beyond just the publication bias. Notably, the Likelihood Ratio Test statistic consistently shows positive values, significant at the 1% level, implying a superior fit of the full model over all horizons. The QE test statistic also indicates enhanced performance when additional variables are factored in. Additionally, the information criteria mostly lean towards the full model, with smaller values suggesting a more optimal fit relative to the number of parameters.[19]

[19] The results for the sample on the growth rate of credit are less straightforward, though they generally favor the extended model. The QE statistic indicates lower residual heterogeneity in the full model across all horizons. The

***Table 8: Model Comparison: Log-Level of Credit***

| | Short horizon | | Medium horizon | | Long horizon | |
|---|---|---|---|---|---|---|
| | Full model | Reduced model | Full model | Reduced model | Full model | Reduced model |
| Meta-analysis random effects | | | | | | |
| Number of parameters | 11 | 3 | 11 | 3 | 7 | 3 |
| AICc | 67 | 131 | 1,327 | 1,375 | 1,487 | 1,559 |
| BIC | 115 | 144 | 1,379 | 1,390 | 1,519 | 1,572 |
| QE | 1,711 | 1,802 | 2,914 | 3,303 | 2,251 | 2,511 |
| LRT | | 80*** | | 65*** | | 79*** |
| Three-level meta-analysis model | | | | | | |
| Number of parameters | 12 | 4 | 12 | 4 | 8 | 4 |
| AICc | -173 | -166 | 131 | 153 | 428 | 440 |
| BIC | -121 | -148 | 189 | 173 | 464 | 458 |
| QE | 1,711 | 1,802 | 2,914 | 3,303 | 2,251 | 2,511 |
| LRT | | 23*** | | 38*** | | 201*** |

***Note:*** The table presents results from a comparison between a reduced model, as estimated in Table B1, and an extended model including additional moderators with a PIP above 0.8, as estimated in Table 7. AICc, BIC, and LRT stand for corrected Akaike Information Criterion, Bayesian Information Criterion, and Likelihood Ratio Test, respectively. QE is the test statistic of the test for residual heterogeneity. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

## 4.4 Effect Beyond Bias and Implied Effect

In the previous sections, we confirmed that the effect is influenced both by publication bias, especially over the medium to long term, and by several significant drivers of heterogeneity. Therefore, in this section we will calculate the mean effect that is both corrected for publication bias and takes into account the primary study characteristics. In contrast to the effect beyond bias estimated in Section 3 in a univariate context, we will now provide an estimate based on the multivariate regression output. Furthermore, we will explore how certain key primary characteristics, such as the estimation approach and data, influence the mean effect, by calculating the implied effect.

We calculate *the effect beyond bias* and *the implied effect* (the effect implied by significant heterogeneity drivers) using the fitted values based on the complete meta-regression output of the BMA. These values consider the estimated coefficients on all the primary study characteristics, with the exception of the slope coefficient on the square of the standard error $SE^2$, which is set to zero to correct for publication bias. In the case of the implied effect, we aim to show what the mean effect would be if all the studies used the same strategy. We explore two cases reflecting the significant heterogeneity drivers identified: first, where the effects are estimated using the frequentist approach, with shocks identified through sign restrictions; and second, where the credit variable is credit granted to the non-financial sector (both firms and households) by both banks and non-banks. To implement this, we set the relevant dummy variables to one across all the studies (i.e., indicating similar methodological and data approaches) and re-calculate the fitted values. All other remaining variables are kept as they are, i.e., representing the observed values. Alongside the effect beyond bias and the implied effect, we report 32/68 credible intervals derived from the predictive densities, which are mixture densities based on the best models identified by the BMA.

Likelihood Ratio Test statistic is also positive across all horizons and is statistically significant, especially at the medium and long horizons. However, the information criteria yield mixed results.

We report unweighted and weighted results, with weights equal to the inverse of the number of estimates per study.

***Table 9: Effect Beyond Bias: Log-Level of Credit***

| | Short horizon | Medium horizon | Long horizon |
|---|---|---|---|
| Unweighted | | | |
| Simple mean | 0.056 | -0.208 | -0.742 |
| Corrected mean | 0.181 | -0.019 | -0.322 |
| 32/68 credible intervals | (-0.252, 0.613) | (-0.604, 0.567) | (-0.987, 0.344) |
| Weighted | | | |
| Simple mean | -0.124 | -0.369 | -0.764 |
| Corrected mean | 0.023 | -0.161 | -0.377 |
| 32/68 credible intervals | (-0.412, 0.457) | (-0.748, 0.426) | (-1.047, 0.293) |
| Observations | 581 | 903 | 710 |
| Studies | 64 | 65 | 57 |

***Note:*** The table compares the mean effect beyond bias and its credible intervals with the simple uncorrected mean at all three horizons. The effect beyond bias is calculated using the fitted values based on the complete meta-regression output of the BMA. These values consider the estimated coefficients on all the primary study characteristics, with the exception of the slope coefficient on the square of the standard error ($SE^2$ is set to zero to correct for publication bias). The credible intervals are derived from the predictive densities, which are mixture densities based on the best models identified by the BMA. To calculate the effect beyond bias and its credible intervals, we use the *predict* and *pred.density* functions provided by the BMS package in R. The weighted statistics are calculated using a weight equal to the inverse of the number of estimates collected per study.

*The effect beyond bias* in Table 9 confirms what we previously identified. The effect remains negative in the medium term and intensifies further in the long term. At the same time, the effect persists, meaning it does not revert to zero over the long horizon. The mean effect beyond bias is smaller than the simple mean, approximately half when considering the weighted average (which we deem more credible than the unweighted one). This aligns with what we determined in the univariate regression in Section 3, where the effect, when adjusted for publication bias, was also weaker. In the case of multivariate regression, when we also factor in the characteristics of primary studies, the effect is stronger in the long term compared to univariate regression.

*The implied effects* in Table 10 show that the effect is negative and considerably stronger when the impact of monetary policy shocks on credit is estimated using the frequentist approach (as opposed to the Bayesian approach) and when the shock is identified through sign restrictions. In this case, the effect is significantly stronger across all three horizons, but the biggest relative intensification occurs in the short and medium term. In other words, if all the studies were estimated using the frequentist approach and used sign restrictions for identification, then a 1 percentage point positive monetary policy shock would lead to a decrease in credit of approximately 0.9% in the short term and 1.1% in the medium term. In the long term, the effect corrects to -0.5%, but it remains negative and persistent.

If all the studies were to estimate the impacts on loans granted to the household sector (i.e., ignoring loans granted to other sectors), the effect would also have significantly stronger negative values. In this scenario, a monetary policy shock of 1 percentage point implies a decrease in loans by 1% at the medium horizon and a long-lasting decline of 0.6%.

Regarding the growth rate of credit, for which the results can be found in Tables C6 and C7 in the Appendix, we observe very similar patterns. First, *the effect beyond bias* derived from the complete BMA results remains consistently negative and becomes larger in the long term, but it is notably weaker than the simple mean. However, this effect reaches -1.3 pp over the long horizon, indicating that a 1 pp increase in the monetary policy rate results in a sustained decrease in annual credit growth of more than 1 pp after accounting for publication bias and variability among primary studies. Second, when examining *the implied effect*, the response is markedly stronger if all the studies employed the frequentist approach with identification through sign restrictions and centered on credit to both households and firms, as also observed with the log-level credit transformation.

***Table 10: Implied Effect: Log-Level of Credit***

| | Short horizon | Medium horizon | Long horizon |
|---|---|---|---|
| Frequentist approach and sign restrictions | | | |
| Mean | -0.870 | -1.140 | -0.525 |
| 32/68 credible intervals | (-1.305, -0.434) | (-1.728, -0.553) | (-1.192, 0.142) |
| W. Mean | -0.863 | -1.074 | -0.510 |
| W. 32/68 credible intervals | (-1.299, -0.426) | (-1.662, -0.485) | (-1.180, 0.160) |
| Credit to households | | | |
| Mean | -0.268 | -0.825 | -0.569 |
| 32/68 credible intervals | (-0.704, 0.169) | (-1.413, -0.238) | (-1.242, 0.103) |
| W. Mean | -0.433 | -0.979 | -0.621 |
| W. 32/68 credible intervals | (-0.871, 0.005) | (-1.568, -0.390) | (-1.296, 0.053) |

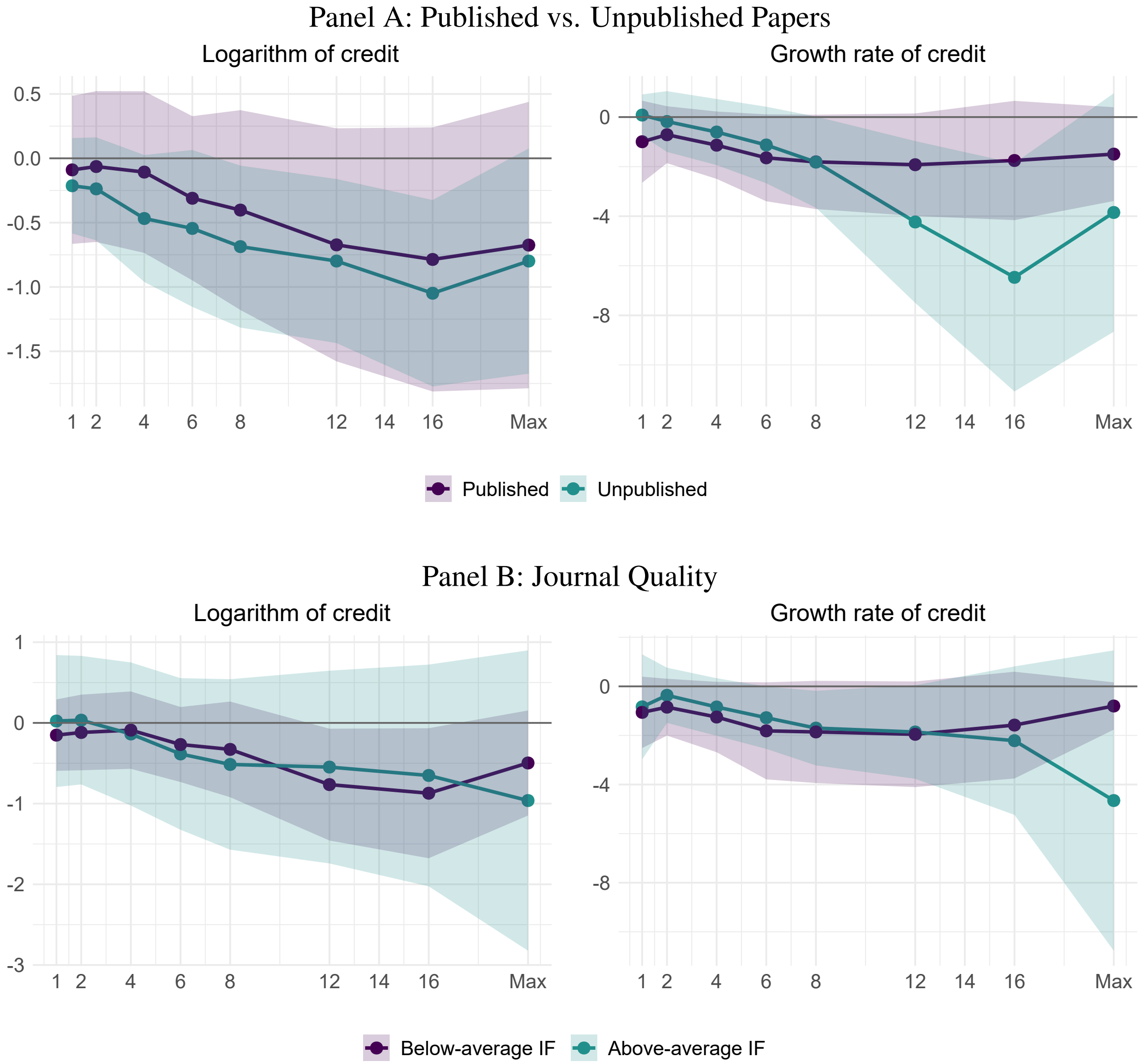
***Note:*** The table presents the mean implied effect and its credible intervals at all three horizons for two different cases: first, where the effects are estimated using the frequentist approach, with shocks identified through sign restrictions; and second, where the credit variable is credit granted to households. The implied effect shows what the mean semi-elasticity would be if all the studies used the same strategy. Similarly to the effect beyond bias in Table 9, the implied effect is calculated using the fitted values based on the complete meta-regression output of the BMA. The credible intervals are derived from the predictive densities, which are mixture densities based on the best models identified by the BMA. To calculate the implied effect and its credible intervals, we use the *predict* and *pred.density* functions provided by the BMS package in R. The weighted statistics are calculated using a weight equal to the inverse of the number of estimates collected per study.

# 5. Additional Considerations and Discussion

In this section, we conduct several robustness and sensitivity tests on our baseline results and respond upfront to some of the questions and comments we frequently get. First, we consider whether the paper was published in a peer-reviewed journal. Next, we identify studies that mainly focus on the identification of the credit channel of monetary policy. Lastly, we re-estimate the results for individual horizons, rather than using three grouped horizons. For the reader stopping here, the results are qualitatively similar, although we observe some quantitative differences.

When screening the publication outlets of the papers included in our analysis, a question may arise about the quality of the underlying studies and about mixing papers from peer-reviewed journals with unpublished working papers. Both research economists and publication outlets are highly motivated to produce and publish top-tier research. Thus, if some studies do not appear in leading journals, it could indicate methodological or data concerns. However, there has been a significant surge in the number of studies produced over recent decades, while space in top economic journals remains limited. Naturally, these top journals tend to prioritize novel work that attracts readers. As a result, studies that merely reaffirm established economic relationships, perhaps for a different country or time frame, often struggle to find a place in these journals. Yet, if methodologically sound, such studies can deepen our understanding of specific economic areas. Nonetheless, we should examine whether our results differ notably when focusing solely on studies from peer-reviewed journals and, further, when narrowing our scope to studies in journals with a higher-than-average impact factor.

***Figure 6: Mean Impulse Response Functions: Publication Characteristics***



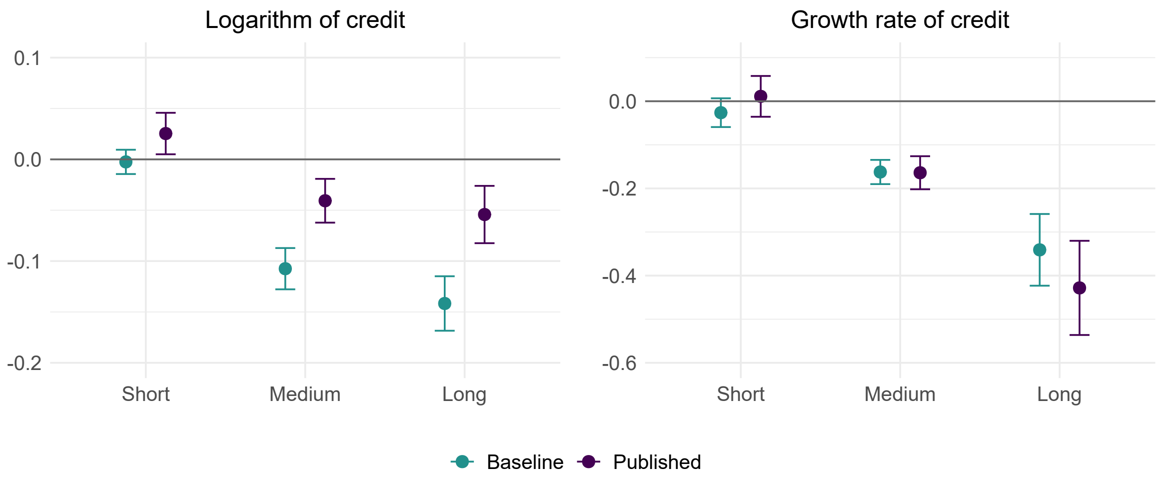

***Note:*** The figure displays the average response of credit and credit growth to a one percentage point increase in the monetary policy rate, accompanied by the average 68% confidence interval. The horizons are in quarters. Panel A compares studies published in peer-reviewed journals with unpublished papers. Panel B differentiates between studies in journals with an above- and below-average impact factor, with the average being calculated based on the journals in our sample. All the responses are calculated as a weighted mean, with the weight being the inverse of the number of estimates collected per study.

Out of the 91 papers in our sample, 63 are published in peer-reviewed journals. In Panel A of Figure 6, we compare the average responses from both published and unpublished studies. The trajectory of the credit response is largely similar, but there is a difference over the long horizon for credit growth. In the latter case, however, the average draws from just 7 unpublished papers, as opposed to 21 published ones. Panel B of Figure 6 then contrasts the responses from studies published in journals with an above-average impact factor, with the average being calculated based on the journals in our sample. In this case, the trajectory of the average response is nearly identical in both groups.[20]

[20] One could argue that not only the publication outlet, but also the year of publication of the primary study matters, given that estimation and identification methods become more sophisticated over time. As such, the hypothesis

Next, we subject the restricted sample of published studies to the empirical tests to which we also subjected the entire sample. Figure 7 compares the mean impulse responses corrected for publication bias of our baseline sample and of the sample consisting only of published papers. The effect beyond bias is very similar for the response of credit growth, while the response of credit is slightly weaker. In both cases, the effect remains negative and long-lasting.

***Figure 7: Mean Impulse Responses After Correction for Publication Bias: Publication Characteristics***




***Note:*** The figure compares the effect beyond bias estimated for the full sample (the baseline results from Table 5) and the sample of published studies. The effect beyond bias is estimated using Precision-Effect Estimate with Standard Error (PEESE) and the three-level meta-analysis model. Standard errors are clustered at the study level. The model is estimated with weights equal to the inverse of each estimate's variance to control for heteroskedasticity.

Second, based on the title and abstract of the primary studies, we collect information on whether the main focus of the primary studies is the relationship between monetary policy and credit. If the main focus of the primary study is the credit channel of monetary policy transmission, researchers might pay more attention to its identification and provide more accurate estimates. On the other hand, researchers may be also tempted to look for negative and statically significant results which are consistent with general expectations, making these studies potentially more sensitive to publication selectivity. Table 11 summarizes our classification of the main focus of our studies. Two thirds of the studies focus directly on monetary policy transmission through the credit channel. The remaining studies in our sample generally focus on the identification of other transmission channels of monetary policy, the determinants of credit supply and demand, macro-financial analysis, or the identification of macroprudential policy transmission.

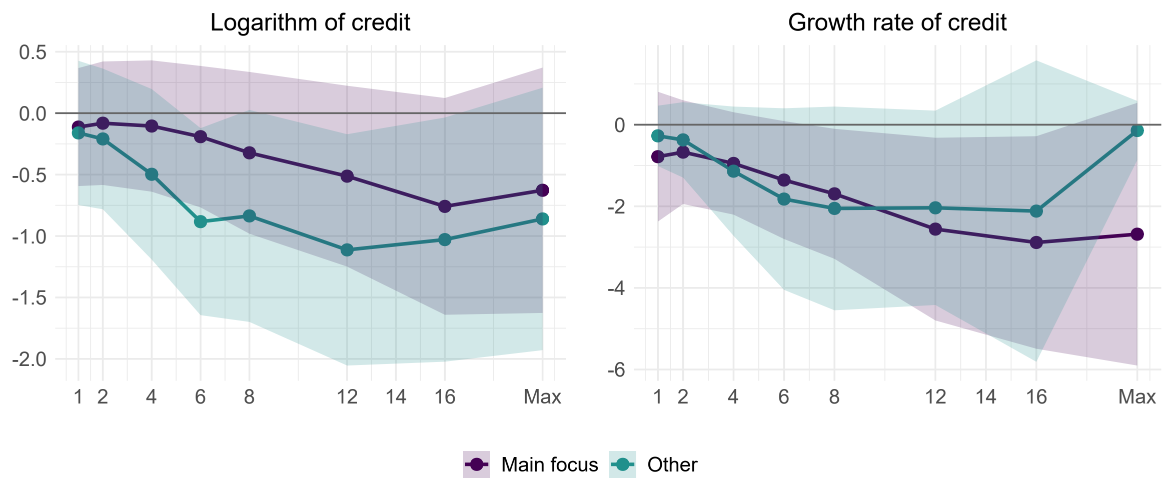
could be that responses in more recent studies are better identified, hence converging to zero over the long term, while those in older studies remain non-zero even in the long term. However, when comparing the average impulse response functions from studies with different publication years, we cannot confirm this hypothesis. Figure A5 in the Appendix shows that the responses do indeed differ based on the year of publication, but these differences hardly support the idea of identification improving over time, instead reflecting the heterogeneity between the primary studies in terms of their data and estimation approaches. While the responses in the earliest primary studies with log-level transformation of credit converge to zero at the maximum horizon, those with growth rate transformation of credit diverge significantly. Additionally, studies published between 2010 and 2017 consistently show a long-lasting negative effect of changes in monetary policy rates on credit.

***Table 11: Focus of Primary Studies: Overview***

| Focus of primary study | Number of articles |
|---|---|
| Studies with main focus: | |
| Impact of monetary policy on credit (credit channel) | 62 |
| Studies with other focus: | |
| Other channels of monetary policy transmission | 8 |
| Determinants of credit supply and demand | 7 |
| Analysis of financial conditions and their changes | 5 |
| Macroprudential policy transmission | 3 |
| House prices and/or real estate market developments | 3 |
| Relationship between credit and business cycles | 3 |

Figure 8 compares the average impulse responses of the two samples. Reassuringly, the trajectory of the responses estimated by studies focusing primarily on the identification of the credit channel is very similar to those which have another focus. The response over the long horizon remains long-lasting in those studies where we would expect the identification of this channel to be more accurate.

***Figure 8: Mean Impulse Response Functions by Focus of Primary Studies***



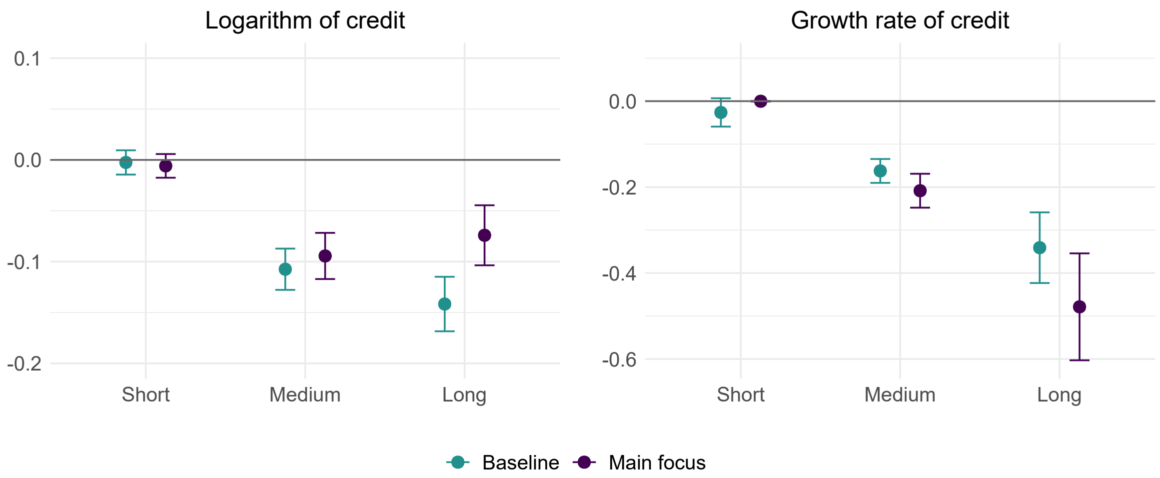

***Note:*** The figure displays the average response of credit and credit growth to a one percentage point increase in the monetary policy rate, accompanied by the average 68% confidence interval. The horizons are in quarters. The figure compares studies which focus primarily on the relationship between monetary policy and credit (identification of the credit channel) and studies with another primary focus. All the responses are calculated as a weighted mean, with the weight being the inverse of the number of estimates collected per study.

Similar to previous exercises, we subject the sample of studies which focus primarily on the identification of the credit channel to the empirical tests that we also applied to the full sample. Figure 9 compares the mean impulse responses, corrected for publication bias, between our baseline and the restricted sample of studies. The effect beyond bias is very similar for both groups.[21]

[21] The results of the BMA analysis for both sensitivity analyses (published papers and papers with the main focus) are qualitatively similar to the baseline results and are available upon request.

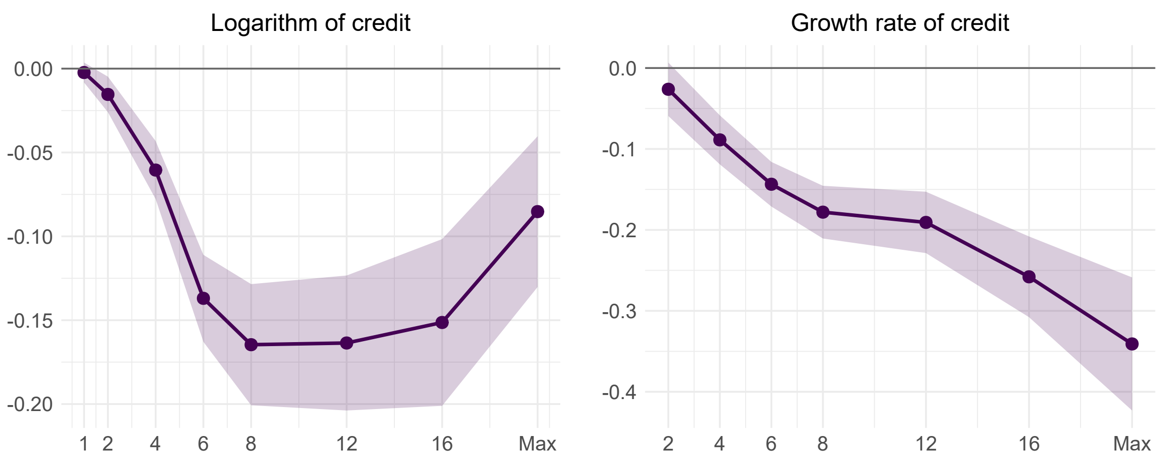
Third, we re-estimate the baseline results for all eight horizons and construct impulse responses corrected for publication bias. This exercise should help us assess whether a specific horizon is driving the results and show the adjusted response at the maximum horizon of approximately six to seven years. Figure 10 displays the final responses, which align with the baseline results, confirming a long-lasting negative effect.

***Figure 9: Mean Impulse Responses After Correction for Publication Bias: Main Focus***




***Note:*** The figure compares the effect beyond bias estimated for the full sample (the baseline results from Table 5) and the sample of studies focusing primarily on identification of the credit channel. The effect beyond bias is estimated using Precision-Effect Estimate with Standard Error (PEESE) and the three-level meta-analysis model. Standard errors are clustered at the study level. The model is estimated with weights equal to the inverse of each estimate's variance to control for heteroskedasticity.

***Figure 10: Mean Impulse Response Functions After Correction for Publication Bias***




***Note:*** The figure displays the average effect beyond bias (the response corrected for publication bias) of credit and credit growth to a one percentage point increase in the monetary policy rate, accompanied by the average 68% confidence interval. The horizons are in quarters. The effect beyond bias is estimated using Precision-Effect Estimate with Standard Error (PEESE) and the three-level meta-analysis model. Standard errors are clustered at the study level. The model is estimated with weights equal to the inverse of each estimate's variance to control for heteroskedasticity. Due to the limited number of observations for the sample on credit growth, the effect beyond bias at each horizon is also estimated using observations from the previous horizon (e.g., the effect beyond bias at the second horizon is estimated using observations from both the first and second horizons).

## 6. Concluding Remarks

Our meta-analysis has provided clear insights into the relationship between changes in monetary policy rates and credit dynamics. Primarily, we have found that when monetary policy tightens, credit experiences a significant and long-lasting decline. Specifically, a one percentage point increase in the monetary policy rate leads to about a 0.9% decrease in credit and a 2.7 percentage point drop in credit growth after four years. These changes do not quickly fade away, indicating a lasting impact on both the level and growth of outstanding credit.

Several factors contribute to the significant heterogeneity of effect sizes we observe in the literature. First, our exploration into publication bias, using methods such as PEESE and the Caliper test, revealed the presence of such bias, even when we accounted for various differences in primary studies. This suggests that some caution is needed when interpreting individual studies, as they may sometimes lean towards expected or favorable outcomes. The exaggeration due to publication bias is at least twofold, aligning with previous findings for the economic literature.

Second, various other factors influenced the effects we observed. Differences in data characteristics, model specification, and publication details played a role. Importantly, researchers' choice of estimation design has a significant impact on the estimated response. Studies using Bayesian methods and including house prices report a smaller decline in credit, while studies with sign restrictions show a larger drop than those using recursive identification. Additionally, credit to households was significantly more affected by monetary policy shocks, and the responses reported in peer-reviewed journals were generally less negative.

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# Appendix A: Data Collection and Fragmentation

## A.1 Paper Selection Procedure

Figure A1 depicts the overall paper selection process for this meta-analysis. In the first phase, we examined the first 800 items returned by Google Scholar for the following query:

> *"credit" OR "lending" OR "loan" AND "interest rate" OR "interbank rate" OR "policy rate" OR "repo rate" OR "monetary policy" OR "yield" OR "spread" OR "short rate" OR "long rate"*

After that, we reviewed all the titles and abstracts, as we wanted to pinpoint the studies that are not relevant for our analysis, even from a high-level perspective (screening phase). In the next phase, we went through each of the remaining studies in more detail and excluded studies due to a lack of correspondence or data (eligibility phase). Each study included in this meta-analysis needs to meet the following criteria. First, the study must employ a VAR model and report the results in impulse response functions with confidence intervals, as we need to retrieve the precision of the estimates for our publication bias tests. Second, the credit variable cannot be expressed as a ratio to another continuous variable (e.g., to total assets, total credit, or GDP). Third, we exclude spillover studies, such as analyses of the transmission of US monetary policy shocks to the world. Lastly, the shock to the impulse variable must be expressed numerically. In this regard, we included papers from which we were able to retrieve the shock size, i.e., where the size of the shock was reported directly (e.g., in separate impulse response functions or a data description table) and studies where we were able to retrieve the original data sources to calculate the size of the shock as reported in the studies.

Once we arrived at the list of relevant papers, we conducted additional searches through backward and forward citation searches and identified an additional 127 potentially relevant articles. These articles had to meet the same criteria as the studies identified via the Google Scholar search. All in all, we screened 927 articles, from which we identified 91 as relevant. In our search, we did not limit the sample based on the publishing date, as we wanted to include as long a time span as possible. We added the last study in September 2022. Table A1 presents all the studies included in our analysis.

We decided to incorporate both published and unpublished studies (typically working papers). For each working paper, we checked whether a journal version exists. If a journal version did exist and contained estimates for the causality of interest, we replaced the working paper with the journal version. The issue of including unpublished studies in a meta-analysis is contentious. On the one hand, when only published studies are included, the quality of the estimates is possibly higher. On the other hand, the inclusion of both published and unpublished studies enables us to study the differences between these two categories. Moreover, as Rusnák et al. (2013) state, the inclusion of unpublished articles should not affect the results of publication bias, because if journals prefer to publish particular estimates, authors will rationally adopt that preference in the early stages of research. Some studies, such as Doucouliagos and Stanley (2013), provide empirical evidence that there is no difference in the magnitude of publication selection between unpublished and published studies. Moreover, the working papers mostly come from central banks or other internationally relevant institutions (e.g., NBER, BIS), where the papers go through a review process similar to that of peer-reviewed journal publications. Thus, we include both journal articles and working papers and distinguish them by means of a dummy in our heterogeneity analysis.

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

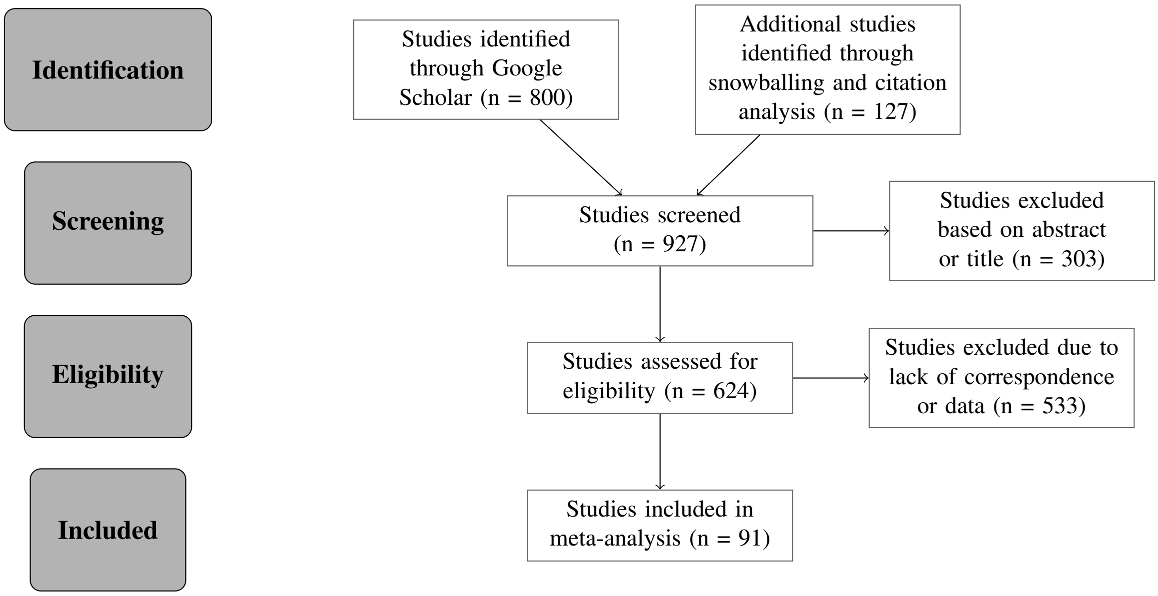


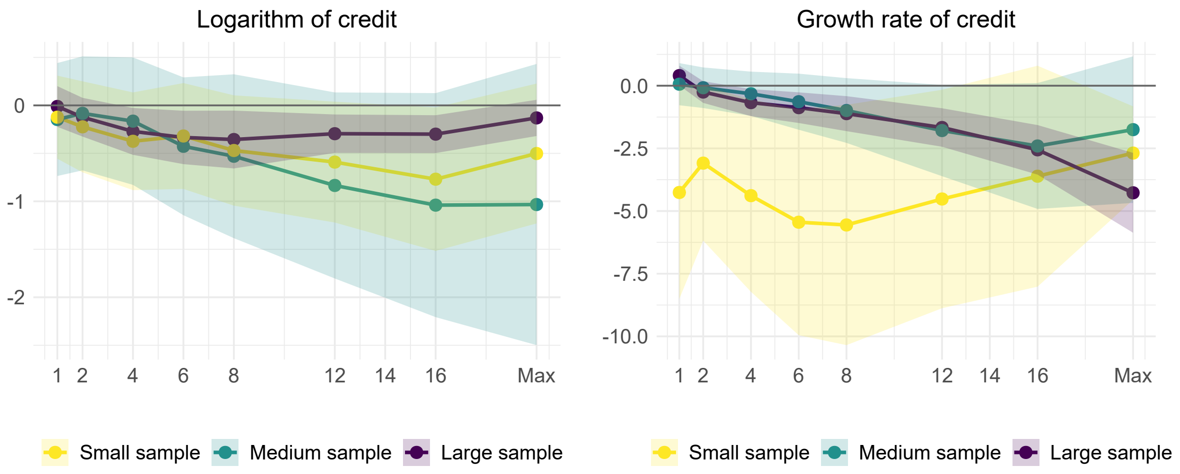
***Table A1: Primary Studies Included in the Meta-Analysis***

| | | |
|---|---|---|
| Panel A: Log-level of Credit | | |
| Afrin (2017) | Hofmann and Peersman (2017b) | Oros and Romocea-Turcu (2009) |
| Assenmacher-Wesche and Gerlach (2008) | Hristov et al. (2012) | Ouchchikh (2017) |
| Assenmacher-Wesche and Gerlach (2010) | Hulsewig et al. (2005) | Papadamou and Siriopoulos (2012) |
| Aysan et al. (2018) | Hwang (2012) | Peersman and Smets (2001) |
| Barraza et al. (2019) | Ibarra (2016) | Peersman and Wagner (2014) |
| Bäurle and Scheufele (2019) | Ibrahim and Shah (2012) | Pescatori and Sole (2016) |
| Bayardavaa et al. (2015) | Jannsen et al. (2019) | Punzi and Kauko (2015) |
| Binatli and Sohrabji (2019) | Jiang (2015) | Sá et al. (2011) |
| Busch et al. (2010) | Jung et al. (2017) | Seoela (2022) |
| Buttiglione and Ferri (1994) | Kabundi and Rapapali (2019) | Serwa and Wdowiński (2017) |
| Choi (2021) | Kakes and Sturm (2002) | Skibińska (2018) |
| Christiano et al. (1996) | Karim et al. (2006) | Stakėnas and Stasiukynaitė (2017) |
| Fornari and Stracca (2012) | Kim and Lim (2020) | Steudler and Zurlinden (1998) |
| Franz (2019) | Kim and Mehrotra (2018) | Suranjit (2016) |
| Franz (2020) | Kim and Mehrotra (2019) | Suzuki (2004) |
| Garretsen and Swank (2003) | Koivu (2009) | Tamási and Világi (2011) |
| Gertler and Gilchrist (1993) | Kubo (2008) | Tan (2012) |
| Goodhart and Hofmann (2003) | Lown and Morgan (2002) | Walsh and Wilcox (1995) |
| Greenwood-Nimmo and Tarassow (2016) | Lungu (2007) | Wrobel and Pawowska (2002) |
| Gupta (2004) | Łyziak et al. (2008) | Wu and Yang (2018) |
| Halvorsen and Jacobsen (2016) | Mertens (2008) | Zaidi and Fisher (2010) |
| Hofmann and Peersman (2017a) | Morsink and Bayoumi (2001) | |
| Panel B: Growth Rate of Credit | | |
| Auel and de Mendonça (2011) | Hanisch (2019) | Nocera and Roma (2017) |
| Belviso and Milani (2006) | Hassan (2003) | Pescatori and Sole (2016) |
| Berkelmans (2005) | Hofmann (2004) | Pool et al. (2015) |
| Bhattacharya (2014) | Huber and Punzi (2020) | Prabheesh and Rahman (2019) |
| Breitenlechner et al. (2016) | Iacoviello and Minetti (2008) | Robstad (2018) |
| Calmès and Théoret (2020) | Kabashi and Suleva (2016) | Singh and Nadkarni (2020) |
| Eickmeier et al. (2009) | Kronick and Wu (2019) | Stakėnas and Stasiukynaitė (2017) |
| Evans and Robertson (2018) | Martínez and Rodríguez (2021) | Wilhelmsson (2020) |
| Goodhart and Hofmann (2008) | Mazelis (2016) | |
| Gumata et al. (2013) | Mwankemwa and Mlamka (2022) | |

***Note:*** In our sample, two studies (Pescatori and Sole, 2016; Stakėnas and Stasiukynaitė, 2017) contain impulse response functions for both log-levels and growth rates.

## A.2 Additional Charts

***Figure A2: Mean Impulse Response Functions: Sample Size***




***Note:*** The figure displays the average response of credit and annual credit growth to a one percentage point increase in the monetary policy rate, accompanied by the average 68% confidence interval. Studies are categorized based on the data sample size used in their estimations: small (first quartile), medium (second and third quartiles), and large (fourth quartile).

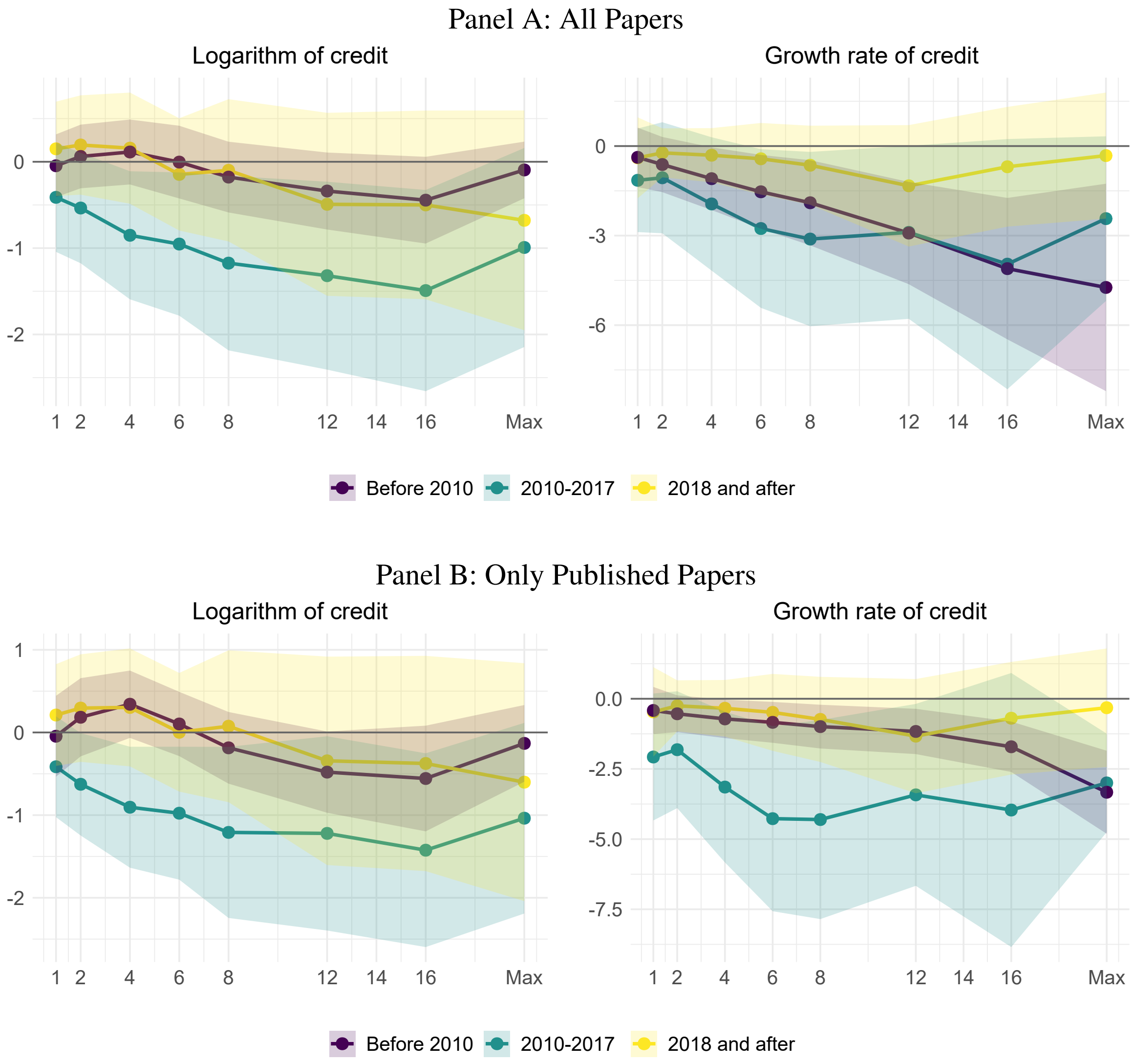
***Figure A3: Boxplot of Effect Sizes: Log-Level of Credit***


***Note:*** The figure displays a boxplot of response of credit to a one-percentage-point increase in the monetary policy rate. 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.

***Figure A4: Boxplot of Effect Sizes: Growth Rate of Credit***


***Note:*** The figure displays a boxplot of the response of annual credit growth to a one percentage point increase in the monetary policy rate. 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.

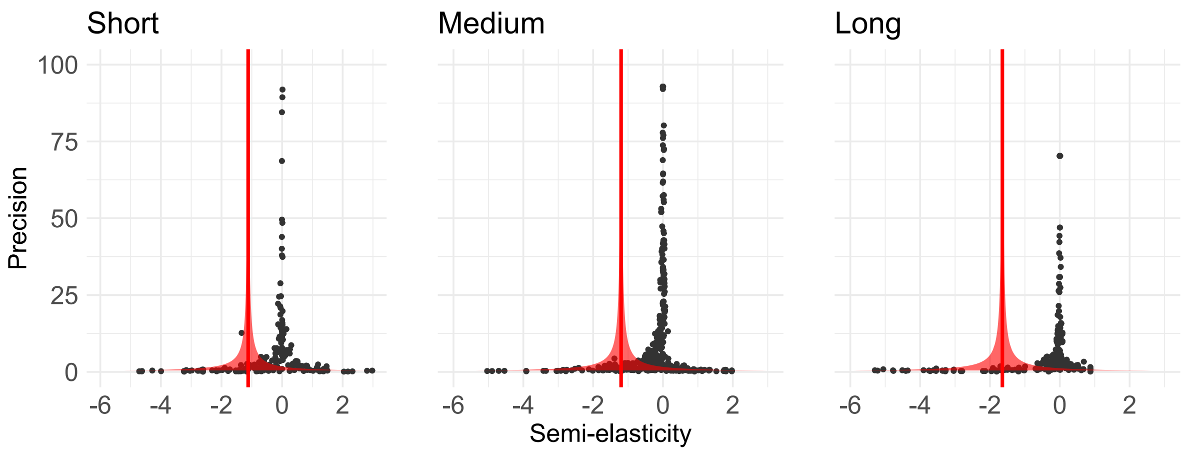
***Figure A5: Mean Impulse Response Functions: Year of Publication***




***Note:*** The figure displays the average response of credit and credit growth to a one percentage point increase in the monetary policy rate, accompanied by the average 68% confidence interval. The horizons are in quarters. Both panels compare studies based on the year when the primary study was published. Panel A contains all the studies in our dataset. Panel B contains only studies published in peer-reviewed journals. All responses are calculated as a weighted mean, with the weight being the inverse of the number of estimates collected per study.

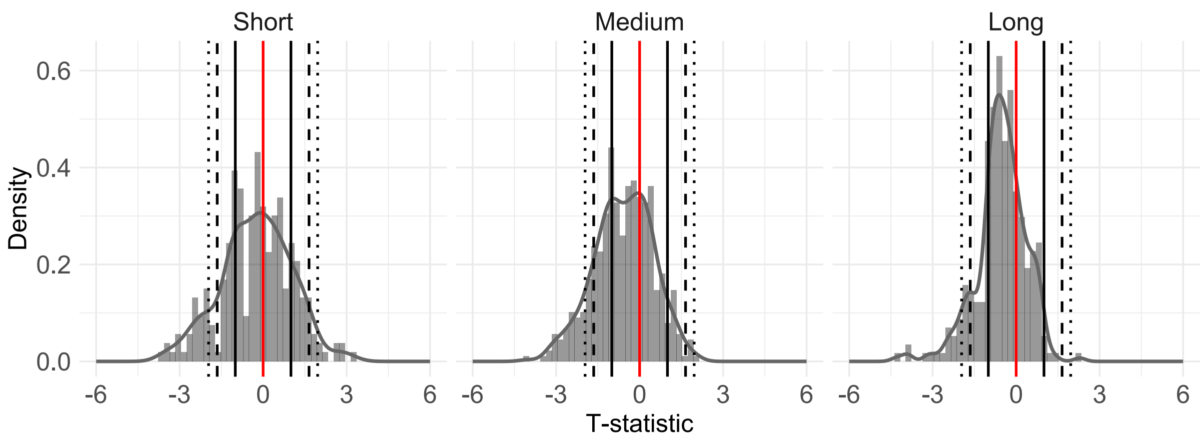
# Appendix B: Extensions to Publication Bias

***Figure B1: Funnel Plots: Growth Rate of Credit***




***Note:*** In the absence of publication bias, the plots form inverted funnels symmetric around the most precise estimates. The individual panels depict the funnel plots for the different horizons: Panel A – short, Panel B – medium, and Panel C – long. Estimates with a precision greater than 100 or a magnitude below -3 or above +3 are excluded from the figure for ease of exposition but are included in the statistical analyses.

***Figure B2: Distribution of T-Statistics: Growth Rate of Credit***




***Note:*** The vertical lines denote the critical value associated with 5% (dotted line), 10% (dashed line) and 32% (solid line) statistical significance. Approximately half of all the studies report 68% (or one standard deviation) confidence intervals, 21% of the studies report 90% confidence intervals, and 26% of the studies report 95% (or two standard deviations) confidence intervals. The remaining few studies report specific confidence intervals, such as 80% or 84%. We exclude estimates with large t-statistics from the figure but include all in the regressions. In the absence of publication bias, the distribution of the t-statistics should be approximately normal.

## B.1 Additional Regression Results

***Table B1: Estimation of Publication Bias: Log-Level of Credit***

| | Short | Medium | Long |
|---|---|---|---|
| Panel A: Precision-effect test (PET) | | | |
| Meta-analysis random effects | | | |
| Effect beyond bias (constant) | 0.000 | -0.010 | -0.010 |
| | (0.001) | (0.010) | (0.012) |
| Publication bias (SE) | -0.047 | -0.605** | -0.777*** |
| | (0.202) | (0.237) | (0.241) |
| $I^2$ (%) | 6 | 94 | 79 |
| Three-level meta-analysis model | | | |
| Effect beyond bias (constant) | -0.016 | -0.016 | 0.025 |
| | (0.020) | (0.039) | (0.045) |
| Publication bias (SE) | 0.157 | -0.439*** | -0.773*** |
| | (0.191) | (0.138) | (0.122) |
| $I^2$ level 1 (%) | 0 | 0 | 0 |
| $I^2$ level 2 (%) | 0 | 0 | 0 |
| $I^2$ level 3 (%) | 100 | 100 | 100 |
| Panel B: Precision-effect estimate with standard error (PEESE) | | | |
| Meta-analysis random effects | | | |
| Effect beyond bias (constant) | -0.001 | -0.025** | -0.030* |
| | (0.000) | (0.012) | (0.015) |
| Publication bias ($SE^2$) | 0.028 | -0.083 | -0.184*** |
| | (0.086) | (0.119) | (0.067) |
| $I^2$ (%) | 7 | 97 | 89 |
| Three-level meta-analysis model | | | |
| Effect beyond bias (constant) | -0.002 | -0.107*** | -0.142*** |
| | (0.012) | (0.020) | (0.027) |
| Publication bias ($SE^2$) | 0.024 | -0.066*** | -0.158*** |
| | (0.024) | (0.022) | (0.015) |
| $I^2$ level 1 (%) | 0 | 0 | 0 |
| $I^2$ level 2 (%) | 0 | 0 | 0 |
| $I^2$ level 3 (%) | 100 | 100 | 100 |
| Observations | 581 | 903 | 710 |
| Studies | 64 | 65 | 57 |

***Note:*** Standard errors, clustered at the study level, are reported in parentheses. All models are estimated with weights equal to the inverse of the estimate's variance to control for heteroskedasticity. $I^2$ measures the effect heterogeneity as a percentage of the total variance. For the random effects model, $I^2$ measures the between-study variance in the true effect. For the three-level model, $I^2$ measures the amount of heterogeneity variance within studies (level 2) and between studies (level 3). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

***Table B2: Estimation of Publication Bias: Growth Rate of Credit***

| | Short | Medium | Long |
|---|---|---|---|
| Panel A: Precision-effect test (PET) | | | |
| Meta-analysis random effects | | | |
| Effect beyond bias (constant) | 0.003*** | 0.008*** | 0.017** |
| | (0.001) | (0.002) | (0.006) |
| Publication bias (SE) | -0.404* | -0.710*** | -0.642*** |
| | (0.220) | (0.154) | (0.170) |
| $I^2$ (%) | 4 | 0 | 0 |
| Three-level meta-analysis model | | | |
| Effect beyond bias (constant) | 0.034 | -0.038 | 0.017*** |
| | (0.037) | (0.028) | (0.004) |
| Publication bias ($SE^2$) | -0.332*** | -0.575*** | -0.642*** |
| | (0.116) | (0.123) | (0.063) |
| $I^2$ level 1 (%) | 0 | 5 | 100 |
| $I^2$ level 2 (%) | 18 | 0 | 0 |
| $I^2$ level 3 (%) | 82 | 95 | 0 |
| Panel B: Precision-effect estimate with standard error (PEESE) | | | |
| Meta-analysis random effects | | | |
| Effect beyond bias (constant) | 0.001*** | 0.000 | -0.006 |
| | (0.000) | (0.002) | (0.004) |
| Publication bias (SE) | -0.066*** | -0.113*** | -0.138*** |
| | (0.015) | (0.028) | (0.035) |
| $I^2$ (%) | 2 | 0 | 0 |
| Three-level meta-analysis model | | | |
| Effect beyond bias (constant) | -0.026 | -0.162*** | -0.341*** |
| | (0.033) | (0.028) | (0.082) |
| Publication bias ($SE^2$) | -0.062*** | -0.097*** | -0.111*** |
| | (0.009) | (0.007) | (0.011) |
| $I^2$ level 1 (%) | 0 | 1 | 1 |
| $I^2$ level 2 (%) | 14 | 0 | 0 |
| $I^2$ level 3 (%) | 86 | 99 | 99 |
| Observations | 264 | 436 | 281 |
| Studies | 28 | 28 | 24 |

***Note:*** Standard errors, clustered at the study level, are reported in parentheses. All models are estimated with weights equal to the inverse of the estimate's variance to control for heteroskedasticity. $I^2$ measures the effect heterogeneity as a percentage of the total variance. For the random effects model, $I^2$ measures the between-study variance in the true effect. For the three-level model, $I^2$ measures the amount of heterogeneity variance within studies (level 2) and between studies (level 3). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

***Table B3: Estimation of Publication Bias With Interaction Terms: Precision-Effect Test (PET): Log-Level of Credit***

| | Dummy for significant effects: I(t-stat<1.65) | | | Dummy for negative effects: I($\beta$ <0) | | | Dummy for significant and negative effects: I(t-stat<1.65, $\beta$ <0) | | |
|---|---|---|---|---|---|---|---|---|---|
| | Short | Medium | Long | Short | Medium | Long | Short | Medium | Long |
| Meta-analysis random effects | | | | | | | | | |
| Constant | 0.000 | 0.000 | 0.000 | -0.001* | -0.001* | 0.001 | 0.000 | 0.000 | -0.001 |
| | (0.000) | (0.000) | (0.001) | (0.000) | (0.000) | (0.002) | (0.000) | (0.001) | (0.002) |
| SE | -0.109 | -0.174 | -0.379*** | 1.445*** | 1.123*** | 1.038*** | 0.430*** | 0.101 | -0.170 |
| | (0.114) | (0.118) | (0.124) | (0.144) | (0.143) | (0.321) | (0.154) | (0.166) | (0.200) |
| Dummy | -0.002* | -0.002* | -0.004 | 0.001 | 0.000 | -0.001 | 0.001** | 0.000 | 0.003 |
| | (0.001) | (0.001) | (0.007) | (0.000) | (0.001) | (0.003) | (0.000) | (0.001) | (0.003) |
| SE×Dummy | 0.167 | -1.62*** | -1.741** | -2.720*** | -2.843*** | -2.626*** | -3.121*** | -3.058*** | -2.979*** |
| | (0.479) | (0.438) | (0.700) | (0.213) | (0.274) | (0.452) | (0.246) | (0.234) | (0.306) |
| $I^2$ (%) | 8 | 3 | 10 | 0 | 2 | 4 | 0 | 0 | 5 |
| Three-level meta-analysis model | | | | | | | | | |
| Constant | -0.020 | -0.003 | 0.065 | -0.006** | -0.019 | -0.021 | 0.000** | -0.008 | 0.034 |
| | (0.022) | (0.025) | (0.073) | (0.002) | (0.016) | (0.030) | (0.000) | (0.010) | (0.035) |
| SE | 0.065 | -0.144* | -0.465** | 1.514*** | 1.016*** | 0.653*** | 0.433*** | 0.061 | -0.345*** |
| | (0.197) | (0.081) | (0.212) | (0.113) | (0.144) | (0.139) | (0.074) | (0.064) | (0.120) |
| Dummy | -0.001 | 0.000 | 0.002 | 0.001 | 0.003 | -0.003 | 0.001*** | 0.002 | 0.001 |
| | (0.009) | (0.002) | (0.034) | (0.003) | (0.011) | (0.017) | (0.000) | (0.004) | (0.020) |
| SE×Dummy | 0.402 | -1.279*** | -1.747*** | -2.651*** | -2.393*** | -1.837*** | -3.131*** | -2.683*** | -2.257*** |
| | (0.424) | (0.180) | (0.632) | (0.148) | (0.223) | (0.167) | (0.130) | (0.136) | (0.41) |
| $I^2$ level 1 (%) | 0 | 0 | 0 | 9 | 0 | 0 | 96 | 0 | 0 |
| $I^2$ level 2 (%) | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| $I^2$ level 3 (%) | 100 | 100 | 100 | 91 | 100 | 100 | 4 | 100 | 100 |

***Note:*** Standard errors, clustered at the study level, are reported in parentheses. All models are estimated with weights equal to the inverse of the estimate's variance to control for heteroskedasticity. $I^2$ measures the effect heterogeneity as a percentage of the total variance. For the random effects model, $I^2$ measures the between-study variance in the true effect. For the three-level model, $I^2$ measures the amount of heterogeneity variance within studies (level 2) and between studies (level 3). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

***Table B4: Estimation of Publication Bias With Interaction Terms: Precision-Effect Estimate With Standard Error (PEESE): Log-Level of Credit***

| | Dummy for significant effects: I(t-stat<1.65) | | | Dummy for negative effects: I($\beta$ <0) | | | Dummy for significant and negative effects: I(t-stat<1.65, $\beta$ <0) | | |
|---|---|---|---|---|---|---|---|---|---|
| | Short | Medium | Long | Short | Medium | Long | Short | Medium | Long |
| Meta-analysis random effects | | | | | | | | | |
| Constant | 0.000 | -0.003 | -0.005 | 0.001*** | 0.002** | 0.029*** | 0.000* | 0.000 | -0.003 |
| | (0.000) | (0.003) | (0.005) | (0.000) | (0.001) | (0.010) | (0.000) | (0.001) | (0.005) |
| $SE^2$ | -0.009 | -0.031 | -0.126*** | 0.439*** | 0.390*** | 0.226*** | 0.066 | 0.019 | -0.113** |
| | (0.061) | (0.081) | (0.045) | (0.099) | (0.048) | (0.065) | (0.081) | (0.092) | (0.043) |
| Dummy | -0.002** | -0.042*** | -0.053*** | -0.003*** | -0.009*** | -0.069*** | -0.003*** | -0.010*** | -0.061*** |
| | (0.001) | (0.015) | (0.019) | (0.001) | (0.002) | (0.018) | (0.001) | (0.003) | (0.017) |
| $SE^2 \times$Dummy | 0.735 | -0.725 | -1.071*** | -0.639*** | -0.772*** | -0.515*** | -4.997*** | -2.861*** | -1.361*** |
| | (0.468) | (0.647) | (0.239) | (0.148) | (0.161) | (0.118) | (0.685) | (0.637) | (0.214) |
| $I^2$ (%) | 8 | 94 | 77 | 2 | 7 | 86 | 3 | 7 | 76 |
| Three-level meta-analysis model | | | | | | | | | |
| Constant | -0.009 | -0.082*** | -0.102*** | -0.003 | -0.116*** | -0.148** | 0.011 | -0.073*** | -0.101*** |
| | (0.011) | (0.023) | (0.028) | (0.011) | (0.041) | (0.072) | (0.010) | (0.019) | (0.028) |
| $SE^2$ | -0.006 | -0.048** | -0.112*** | 0.415*** | 0.405*** | 0.210*** | 0.054** | 0.021 | -0.106*** |
| | (0.020) | (0.021) | (0.012) | (0.064) | (0.040) | (0.043) | (0.025) | (0.022) | (0.012) |
| Dummy | 0.000 | -0.005 | -0.009 | -0.003 | -0.006 | -0.012 | -0.003 | -0.007 | -0.012 |
| | (0.004) | (0.032) | (0.040) | (0.003) | (0.052) | (0.089) | (0.006) | (0.033) | (0.050) |
| $SE^2 \times$Dummy | 0.720*** | -0.531** | -0.961*** | -0.600*** | -0.694*** | -0.440*** | -4.567*** | -2.226*** | -1.090*** |
| | (0.200) | (0.224) | (0.156) | (0.068) | (0.049) | (0.045) | (0.795) | (0.405) | (0.175) |
| $I^2$ level 1 (%) | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| $I^2$ level 2 (%) | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| $I^2$ level 3 (%) | 100 | 100 | 100 | 100 | 100 | 100 | 100 | 100 | 100 |

***Note:*** Standard errors, clustered at the study level, are reported in parentheses. All models are estimated with weights equal to the inverse of the estimate's variance to control for heteroskedasticity. $I^2$ measures the effect heterogeneity as a percentage of the total variance. For the random effects model, $I^2$ measures the between-study variance in the true effect. For the three-level model, $I^2$ measures the amount of heterogeneity variance within studies (level 2) and between studies (level 3). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

***Table B5: Estimation of Publication Bias With Interaction Terms: Precision-Effect Test (PET): Growth Rate of Credit***

| | Dummy for significant effects: I(t-stat<1.65) | | | Dummy for negative effects: I($\beta$ <0) | | | Dummy for significant and negative effects: I(t-stat<1.65, $\beta$ <0) | | |
|---|---|---|---|---|---|---|---|---|---|
| | Short | Medium | Long | Short | Medium | Long | Short | Medium | Long |
| Meta-analysis random effects | | | | | | | | | |
| Constant | 0.002*** | 0.005*** | 0.009*** | 0.000 | 0.001 | 0.000 | 0.002*** | 0.006*** | 0.009*** |
| | (0.001) | (0.002) | (0.004) | (0.001) | (0.001) | (0.003) | (0.001) | (0.002) | (0.004) |
| SE | -0.136 | -0.368*** | -0.398*** | 0.999*** | 0.559*** | 0.500*** | 0.028 | -0.352*** | -0.385*** |
| | (0.140) | (0.097) | (0.087) | (0.206) | (0.073) | (0.072) | (0.197) | (0.101) | (0.090) |
| Dummy | 0.006*** | 0.037*** | 0.267*** | -0.001 | 0.005** | 0.016*** | -0.001 | -0.007 | 0.101 |
| | (0.003) | (0.004) | (0.094) | (0.002) | (0.003) | (0.007) | (0.002) | (0.005) | (0.060) |
| SE×Dummy | -1.508* | -2.085*** | -2.109*** | -2.197*** | -1.765*** | -1.481*** | -2.880*** | -1.956*** | -2.060*** |
| | (0.772) | (0.178) | (0.303) | (0.345) | (0.190) | (0.182) | (0.545) | (0.154) | (0.296) |
| $I^2$ (%) | 2 | 0 | 0 | 0 | 0 | 0 | 2 | 0 | 0 |
| Three-level meta-analysis model | | | | | | | | | |
| Constant | 0.051*** | 0.005*** | 0.009*** | -0.046 | 0.001 | 0.000 | 0.007 | 0.005*** | 0.009*** |
| | (0.026) | (0.001) | (0.003) | (0.063) | (0.001) | (0.003) | (0.028) | (0.001) | (0.003) |
| SE | -0.236*** | -0.368*** | -0.398*** | 1.071 | 0.559*** | 0.500*** | 0.108 | -0.358*** | -0.385*** |
| | (0.100) | (0.042) | (0.046) | (0.760) | (0.040) | (0.055) | (0.169) | (0.043) | (0.048) |
| Dummy | -0.017 | 0.037*** | 0.267 | -0.006 | 0.005*** | 0.016*** | -0.002 | -0.009*** | 0.101*** |
| | (0.060) | (0.010) | (0.169) | (0.026) | (0.002) | (0.004) | (0.003) | (0.004) | (0.044) |
| SE×Dummy | -0.854 | -2.085*** | -2.109*** | -1.943*** | -1.765*** | -1.481*** | -2.735*** | -1.939*** | -2.060*** |
| | (0.608) | (0.119) | (0.185) | (0.708) | (0.066) | (0.080) | (0.490) | (0.105) | (0.165) |
| $I^2$ level 1 (%) | 0 | 100 | 100 | 0 | 100 | 100 | 0 | 99 | 100 |
| $I^2$ level 2 (%) | 21 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| $I^2$ level 3 (%) | 79 | 0 | 0 | 100 | 0 | 0 | 100 | 1 | 0 |

***Note:*** Standard errors, clustered at the study level, are reported in parentheses. All models are estimated with weights equal to the inverse of the estimate's variance to control for heteroskedasticity. $I^2$ measures the effect heterogeneity as a percentage of the total variance. For the random effects model, $I^2$ measures the between-study variance in the true effect. For the three-level model, $I^2$ measures the amount of heterogeneity variance within studies (level 2) and between studies (level 3). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

***Table B6: Estimation of Publication Bias With Interaction Terms: Precision-Effect Estimate With Standard Error: Growth Rate of Credit***

| | Dummy for significant effects: I(t-stat<1.65) | | | Dummy for negative effects: I($\beta$ <0) | | | Dummy for significant and negative effects: I(t-stat<1.65, $\beta$ <0) | | |
|---|---|---|---|---|---|---|---|---|---|
| | Short | Medium | Long | Short | Medium | Long | Short | Medium | Long |
| Meta-analysis random effects | | | | | | | | | |
| Constant | 0.002*** | 0.001 | -0.005 | 0.003*** | 0.006*** | 0.018*** | 0.002*** | 0.002 | -0.005 |
| | (0.001) | (0.002) | (0.004) | (0.000) | (0.002) | (0.005) | (0.001) | (0.002) | (0.004) |
| SE | -0.053*** | -0.094*** | -0.104*** | 0.147* | 0.145*** | 0.134 | -0.051*** | -0.094*** | -0.104*** |
| | (0.006) | (0.015) | (0.015) | (0.076) | (0.049) | (0.115) | (0.006) | (0.015) | (0.015) |
| Dummy | -0.001 | -0.017 | -0.496* | -0.009** | -0.021*** | -0.037*** | -0.017*** | -0.086*** | -0.652*** |
| | (0.001) | (0.011) | (0.264) | (0.004) | (0.006) | (0.011) | (0.003) | (0.025) | (0.216) |
| SE×Dummy | -0.784*** | -0.950** | -0.413* | -0.24** | -0.282*** | -0.281** | -0.885*** | -0.903** | -0.399* |
| | (0.189) | (0.405) | (0.215) | (0.103) | (0.054) | (0.116) | (0.232) | (0.373) | (0.207) |
| $I^2$ (%) | 2 | 0 | 0 | 4 | 0 | 0 | 2 | 0 | 0 |
| Three-level meta-analysis model | | | | | | | | | |
| Constant | 0.002 | -0.127*** | -0.005* | 0.026 | -0.167*** | -0.308*** | 0.008 | -0.113*** | -0.005* |
| | (0.025) | (0.025) | (0.003) | (0.033) | (0.051) | (0.115) | (0.024) | (0.022) | (0.003) |
| SE | -0.051*** | -0.083*** | -0.104*** | 0.132*** | 0.183*** | 0.170 | -0.050*** | -0.084*** | -0.104*** |
| | (0.007) | (0.006) | (0.009) | (0.034) | (0.038) | (0.135) | (0.007) | (0.006) | (0.009) |
| Dummy | -0.055 | -0.012 | -0.496*** | -0.124*** | -0.025 | -0.064 | -0.167** | -0.073 | -0.652*** |
| | (0.062) | (0.065) | (0.164) | (0.026) | (0.063) | (0.133) | (0.079) | (0.062) | (0.146) |
| SE×Dummy | -0.673*** | -0.796*** | -0.413*** | -0.217*** | -0.304*** | -0.292** | -0.738*** | -0.773*** | -0.399*** |
| | (0.101) | (0.170) | (0.126) | (0.036) | (0.039) | (0.136) | (0.106) | (0.162) | (0.121) |
| $I^2$ level 1 (%) | 0 | 3 | 100 | 0 | 2 | 1 | 0 | 3 | 100 |
| $I^2$ level 2 (%) | 15 | 0 | 0 | 20 | 0 | 0 | 17 | 0 | 0 |
| $I^2$ level 3 (%) | 85 | 97 | 0 | 80 | 98 | 99 | 83 | 97 | 0 |

***Note:*** Standard errors, clustered at the study level, are reported in parentheses. All models are estimated with weights equal to the inverse of the estimate's variance to control for heteroskedasticity. $I^2$ measures the effect heterogeneity as a percentage of the total variance. For the random effects model, $I^2$ measures the between-study variance in the true effect. For the three-level model, $I^2$ measures the amount of heterogeneity variance within studies (level 2) and between studies (level 3). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

## B.2 The Caliper Test

Caliper tests are based on the principle that reported t-statistics should be evenly distributed around standard significance thresholds (e.g., 1.65 for 10% significance and 1.96 for 5% significance). Essentially, the count of reported t-statistics above a threshold (termed "over caliper") should be statistically comparable to those below the threshold (termed "under caliper"). Ideally, the resulting ratio should be 0.5 or less, indicating a 50:50 distribution. Bruns et al. (2019) suggests a more relaxed criterion where an over-to-under caliper ratio of 0.4 (or a 60:40 distribution) is permissible. They argue that the conventional 50:50 null hypothesis might be overly stringent, particularly as extensive evidence hints that economic research often lacks sufficient power (Ioannidis et al., 2017). Consequently, in the absence of reporting biases, the frequency of t-statistic values is expected to decline as the magnitude of these values increases.

The results can be found in Tables B7 through B10. We examine three significance thresholds, corresponding to the 68%, 90%, and 95% confidence intervals. We use three caliper sizes: 0.1, 0.2, and 0.3. The first two tables present findings for the entire sample of semi-elasticities and a subset of only negative semi-elasticities on the log-level of credit. The remaining two tables center on the growth rate of credit. These findings indicate that publication selection exists across all estimates and horizons for both samples. The evidence is particularly compelling for negative estimates: we reject the null hypothesis of no p-hacking at both the conservative threshold of 0.5 and the more lenient threshold of 0.4.

***Table B7: Caliper Test: Log-Level of Credit – All Effects***

| T-stat | C | All | | Short horizon | | Medium horizon | | Long horizon | |
|---|---|---|---|---|---|---|---|---|---|
| | 0.1 | *0.494* | *(0.428)* | **0.500** | **(0.383)** | **0.522** | **(0.421)** | *0.436* | *(0.300)* |
| 1.96 | 0.2 | *0.452* | *(0.408)* | *0.481* | *(0.399)* | *0.447* | *(0.379)* | *0.427* | *(0.335)* |
| | 0.3 | *0.412* | *(0.376)* | *0.401* | *(0.336)* | *0.432* | *(0.376)* | 0.393 | (0.323) |
| | 0.1 | *0.497* | *(0.435)* | *0.492* | *(0.382)* | **0.560** | **(0.464)** | *0.400* | *(0.276)* |
| 1.65 | 0.2 | *0.459* | *(0.414)* | *0.462* | *(0.380)* | **0.512** | **(0.438)** | 0.388 | (0.306) |
| | 0.3 | *0.415* | *(0.379)* | *0.442* | *(0.376)* | *0.452* | *(0.397)* | 0.331 | (0.268) |
| | 0.1 | *0.443* | *(0.383)* | 0.367 | (0.251) | *0.412* | *(0.322)* | **0.569** | **(0.451)** |
| 1 | 0.2 | **0.519** | **(0.477)** | *0.443* | *(0.359)* | **0.516** | **(0.450)** | **0.577** | **(0.506)** |
| | 0.3 | *0.493* | *(0.458)* | *0.449* | *(0.378)* | *0.481* | *(0.427)* | **0.540** | **(0.480)** |

***Note:*** The table shows the results of the caliper test for three caliper sizes: 0.1, 0.2, and 0.3. The reported numbers represent the share of t-statistics in the narrow interval that are above the significance threshold, i.e., the share of observations above 1.96, 1.65, or 1. Formally, the ratio $C$ is calculated as the number of t-statistics above the given significance threshold ("over caliper") over the total number of observations. We test two one-sided null hypotheses of no p-hacking: $C$ is lower than or equal to 0.5 and $C$ is lower than or equal to 0.4. Significant results are shown in bold (H0: $C \leq 0.5$) and italics (H0: $C \leq 0.4$). Lower 95% confidence intervals are reported in brackets.

***Table B8: Caliper Test: Log-Level of Credit – Only Negative Effects***

| T-stat | C | All | | Short horizon | | Medium horizon | | Long horizon | |
|---|---|---|---|---|---|---|---|---|---|
| 1.96 | 0.1 | **0.500** | **(0.423)** | **0.500** | **(0.348)** | **0.562** | **(0.441)** | *0.421* | *(0.284)* |
| | 0.2 | *0.466* | *(0.412)* | **0.533** | **(0.425)** | *0.485* | *(0.401)* | 0.390 | (0.296) |
| | 0.3 | *0.422* | *(0.378)* | *0.419* | *(0.33)* | *0.486* | *(0.416)* | 0.347 | (0.274) |
| 1.65 | 0.1 | **0.504** | **(0.427)** | *0.433* | *(0.277)* | **0.600** | **(0.476)** | *0.450* | *(0.316)* |
| | 0.2 | *0.482* | *(0.426)* | **0.500** | **(0.385)** | **0.553** | **(0.457)** | *0.411* | *(0.324)* |
| | 0.3 | *0.437* | *(0.394)* | *0.460* | *(0.37)* | *0.496* | *(0.425)* | 0.363 | (0.294) |
| 1 | 0.1 | *0.454* | *(0.378)* | 0.310 | (0.162) | 0.389 | (0.277) | **0.667** | **(0.532)** |
| | 0.2 | **0.518** | **(0.466)** | 0.393 | (0.283) | *0.490* | *(0.408)* | **0.616** | **(0.535)** |
| | 0.3 | *0.491* | *(0.448)* | *0.419* | (0.323) | 0.446 | **(0.380)** | **0.579** | **(0.509)** |

***Note:*** The table shows the results of the caliper test for three caliper sizes: 0.1, 0.2, and 0.3. The reported numbers represent the share of t-statistics in the narrow interval that are above the significance threshold, i.e., the share of observations above 1.96, 1.65, or 1. Formally, the ratio $C$ is calculated as the number of t-statistics above the given significance threshold ("over caliper") over the total number of observations. We test two one-sided null hypotheses of no p-hacking: $C$ is lower than or equal to 0.5 and $C$ is lower than or equal to 0.4. Significant results are shown in bold (H0: $C \leq 0.5$) and italics (H0: $C \leq 0.4$). Lower 95% confidence intervals are reported in brackets.

***Table B9: Caliper Test: Growth Rate of Credit – All Effects***

| T-stat | C | All | | Short horizon | | Medium horizon | | Long horizon | |
|---|---|---|---|---|---|---|---|---|---|
| 1.96 | 0.1 | *0.421* | *(0.311)* | *0.444* | *(0.235)* | *0.423* | *(0.254)* | 0.385 | (0.134) |
| | 0.2 | 0.327 | (0.253) | *0.400* | *(0.245)* | 0.291 | (0.187) | 0.320 | (0.157) |
| | 0.3 | 0.324 | (0.265) | 0.373 | (0.258) | 0.294 | (0.211) | 0.324 | (0.193) |
| 1.65 | 0.1 | **0.523** | **(0.419)** | 0.357 | (0.122) | **0.611** | **(0.472)** | *0.467* | *(0.232)* |
| | 0.2 | *0.444* | *(0.372)* | 0.351 | (0.217) | *0.486* | *(0.385)* | *0.462* | *(0.291)* |
| | 0.3 | 0.378 | (0.321) | 0.305 | (0.204) | 0.383 | (0.305) | *0.486* | *(0.341)* |
| 1 | 0.1 | **0.571** | **(0.491)** | **0.588** | **(0.443)** | **0.625** | **(0.494)** | *0.484* | *(0.329)* |
| | 0.2 | *0.498* | *(0.440)* | *0.475* | *(0.368)* | **0.598** | **(0.510)** | 0.377 | (0.273) |
| | 0.3 | *0.452* | *(0.404)* | *0.482* | *(0.390)* | **0.535** | **(0.462)** | 0.293 | (0.209) |

***Note:*** The table shows the results of the caliper test for three caliper sizes: 0.1, 0.2, and 0.3. The reported numbers represent the share of t-statistics in the narrow interval that are above the significance threshold, i.e., the share of observations above 1.96, 1.65, or 1. Formally, the ratio $C$ is calculated as the number of t-statistics above the given significance threshold ("over caliper") over the total number of observations. We test two one-sided null hypotheses of no p-hacking: $C$ is lower than or equal to 0.5 and $C$ is lower than or equal to 0.4. Significant results are shown in bold (H0: $C \leq 0.5$) and italics (H0: $C \leq 0.4$). Lower 95% confidence intervals are reported in brackets.

***Table B10: Caliper Test: Growth Rate of Credit – Only Negative Effects***

| T-stat | C | All | | Short horizon | | Medium horizon | | Long horizon | |
|---|---|---|---|---|---|---|---|---|---|
| | 0.1 | *0.440* | *(0.321)* | **0.500** | **(0.254)** | *0.435* | *(0.253)* | 0.385 | (0.134) |
| 1.96 | 0.2 | 0.352 | (0.268) | **0.556** | **(0.346)** | 0.306 | (0.195) | 0.292 | (0.129) |
| | 0.3 | 0.366 | (0.299) | **0.548** | **(0.394)** | 0.316 | (0.226) | 0.314 | (0.180) |
| | 0.1 | **0.549** | **(0.431)** | **0.667** | **(-0.307)** | **0.576** | **(0.428)** | *0.467* | *(0.232)* |
| 1.65 | 0.2 | *0.471* | *(0.390)* | *0.421* | *(0.219)* | *0.483* | *(0.375)* | *0.480* | *(0.306)* |
| | 0.3 | *0.427* | *(0.360)* | *0.400* | *(0.245)* | *0.402* | *(0.314)* | **0.515** | **(0.366)** |
| | 0.1 | **0.590** | **(0.500)** | **0.619** | **(0.432)** | **0.647** | **(0.506)** | **0.500** | **(0.336)** |
| 1 | 0.2 | **0.535** | **(0.469)** | **0.513** | **(0.376)** | **0.642** | **(0.543)** | *0.412* | *(0.295)* |
| | 0.3 | *0.469* | *(0.412)* | *0.462* | *(0.345)* | **0.559** | **(0.473)** | 0.344 | (0.244) |

***Note:*** The table shows the results of the caliper test for three caliper sizes: 0.1, 0.2, and 0.3. The reported numbers represent the share of t-statistics in the narrow interval that are above the significance threshold, i.e., the share of observations above 1.96, 1.65, or 1. Formally, the ratio $C$ is calculated as the number of t-statistics above the given significance threshold ("over caliper") over the total number of observations. We test two one-sided null hypotheses of no p-hacking: $C$ is lower than or equal to 0.5 and $C$ is lower than or equal to 0.4. Significant results are shown in bold (H0: $C \leq 0.5$) and italics (H0: $C \leq 0.4$). Lower 95% confidence intervals are reported in brackets.

# Appendix C: Extensions to the Analysis of Heterogeneity

***Table C1: Variable Definitions***

| Variable | Definition |
|---|---|
| Semi-elasticity | The reported effect of a one percentage point increase in the interest rate on credit |
| $SE^2$ | The square of the reported or implied standard error of the estimate. |
| **Data characteristics** | |
| Bank credit | = 1 if credit provided by banks is used. |
| Non-bank credit | = 1 if credit provided by non-banks is used. |
| Credit to households | = 1 if credit to households is used. |
| Credit to firms | = 1 if credit to firms is used. |
| Asia | = 1 if the study covers a country or group of countries from Asia. |
| USA | = 1 if the analyzed country is the USA. |
| Other countries | = 1 if the analyzed country is neither the USA nor from Europe or Asia. |
| Real credit | = 1 if real instead of nominal credit is used. |
| SA credit | = 1 if credit is seasonally adjusted. |
| Detrended credit | = 1 if the study uses detrended data on credit. |
| Lower data frequency | = 1 if the data were collected at quarterly or annual frequency. |
| No. of countries | The number of countries included in the analysis. |
| No. of years | The logarithm of the length of the data sample used in the primary study (in years). |
| Midyear | The logarithm of the midpoint of the data sample. |
| **Model specification and estimation characteristics** | |
| Panel VAR | = 1 if a panel VAR model is employed in the primary study. |
| Other VAR | = 1 if the primary study employs a model other than the simple, Bayesian or panel VAR model. |
| Bayesian estimation | = 1 if the VAR model is estimated using Bayesian techniques. |
| Sign restrictions | = 1 if sign restrictions are used in the VAR model. |
| Controls: Asset prices | = 1 if asset prices are included. |
| Controls: Exchange rate | = 1 if the exchange rate is included. |
| Controls: Other IR | = 1 if interest rates other than the one used as a proxy for the monetary policy rate are included (i.e., long-term interest rates, yields, lending rates, spreads). |
| No. of lags | The number of lags (in quarters) included in the model. |
| **Publication characteristics** | |
| Citations | The logarithm of the number of Google Scholar citations normalized by the number of years since the publication year plus one. |
| Impact factor | The recursive discounted RePEc impact factor of the outlet. |
| Main focus | = 1 if the main focus of the primary study is the credit channel of monetary policy transmission. |
| Published | = 1 if the study is published in a peer-reviewed journal. |

***Table C2: Summary Statistics of Meta-Regression Variables: Log-Level of Credit***

| | Unweighted | | | | | | Weighted | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Short | | Medium | | Long | | Short | | Medium | | Long | |
| | Mean | SD | Mean | SD | Mean | SD | Mean | SD | Mean | SD | Mean | SD |
| Semi-elasticity | 0.06 | 1.16 | -0.21 | 1.59 | -0.74 | 1.90 | -0.12 | 0.91 | -0.37 | 1.42 | -0.77 | 1.83 |
| $SE^2$ | 1.99 | 6.03 | 2.22 | 6.48 | 3.87 | 10.78 | 1.45 | 5.16 | 1.71 | 5.60 | 3.19 | 9.87 |
| **Data characteristics** | | | | | | | | | | | | |
| Bank credit | 0.58 | 0.49 | 0.58 | 0.49 | 0.55 | 0.50 | 0.61 | 0.49 | 0.60 | 0.49 | 0.54 | 0.50 |
| Non-bank credit | 0.01 | 0.10 | 0.01 | 0.10 | 0 | 0.06 | 0 | 0.06 | 0 | 0.06 | 0 | 0.04 |
| Credit to households | 0.14 | 0.35 | 0.14 | 0.35 | 0.15 | 0.36 | 0.12 | 0.33 | 0.12 | 0.33 | 0.15 | 0.36 |
| Credit to firms | 0.22 | 0.41 | 0.24 | 0.43 | 0.24 | 0.43 | 0.24 | 0.43 | 0.25 | 0.43 | 0.27 | 0.45 |
| Asia | 0.15 | 0.36 | 0.17 | 0.38 | 0.17 | 0.37 | 0.29 | 0.45 | 0.30 | 0.46 | 0.32 | 0.47 |
| USA | 0.33 | 0.47 | 0.32 | 0.47 | 0.35 | 0.48 | 0.18 | 0.38 | 0.18 | 0.38 | 0.21 | 0.41 |
| Other countries | 0.28 | 0.45 | 0.28 | 0.45 | 0.29 | 0.45 | 0.21 | 0.41 | 0.22 | 0.41 | 0.21 | 0.41 |
| Real credit | 0.64 | 0.48 | 0.66 | 0.47 | 0.72 | 0.45 | 0.52 | 0.50 | 0.53 | 0.50 | 0.58 | 0.49 |
| SA credit | 0.45 | 0.50 | 0.43 | 0.49 | 0.41 | 0.49 | 0.44 | 0.50 | 0.42 | 0.49 | 0.43 | 0.50 |
| Detrended credit | 0.22 | 0.41 | 0.20 | 0.40 | 0.19 | 0.39 | 0.21 | 0.41 | 0.20 | 0.40 | 0.18 | 0.39 |
| Lower data frequency | 0.80 | 0.40 | 0.82 | 0.38 | 0.90 | 0.30 | 0.69 | 0.46 | 0.72 | 0.45 | 0.81 | 0.40 |
| No. of countries | 4.51 | 7.69 | 4.53 | 7.73 | 5.30 | 8.39 | 3.27 | 6.01 | 3.38 | 6.20 | 3.70 | 6.81 |
| No. of years | 21.20 | 8.96 | 21.71 | 9.09 | 22.93 | 8.22 | 19.68 | 10.53 | 20.53 | 10.89 | 21.71 | 10.44 |
| Midyear | 1995 | 10.04 | 1995 | 10.05 | 1994 | 9.98 | 1998 | 9.46 | 1997 | 9.62 | 1997 | 9.28 |
| **Model specification and estimation** | | | | | | | | | | | | |
| Panel VAR | 0.28 | 0.45 | 0.29 | 0.45 | 0.33 | 0.47 | 0.21 | 0.41 | 0.21 | 0.41 | 0.22 | 0.42 |
| Other VAR | 0.10 | 0.30 | 0.10 | 0.30 | 0.09 | 0.28 | 0.08 | 0.27 | 0.08 | 0.27 | 0.09 | 0.28 |
| Bayesian estimation | 0.34 | 0.47 | 0.35 | 0.48 | 0.42 | 0.49 | 0.23 | 0.42 | 0.23 | 0.42 | 0.32 | 0.47 |
| Sign restrictions | 0.32 | 0.47 | 0.30 | 0.46 | 0.33 | 0.47 | 0.34 | 0.48 | 0.33 | 0.47 | 0.39 | 0.49 |
| Controls: Asset prices | 0.60 | 0.49 | 0.57 | 0.49 | 0.59 | 0.49 | 0.49 | 0.50 | 0.47 | 0.50 | 0.49 | 0.50 |
| Control: Exchange rate | 0.34 | 0.48 | 0.32 | 0.47 | 0.27 | 0.45 | 0.40 | 0.49 | 0.38 | 0.49 | 0.34 | 0.47 |
| Controls: Other IR | 0.23 | 0.42 | 0.22 | 0.42 | 0.21 | 0.41 | 0.24 | 0.43 | 0.24 | 0.43 | 0.19 | 0.40 |
| No. of lags | 2.76 | 1.38 | 2.75 | 1.36 | 2.85 | 1.32 | 2.98 | 1.78 | 2.97 | 1.77 | 3.14 | 1.83 |
| **Publication characteristics** | | | | | | | | | | | | |
| Citations | 1.53 | 0.88 | 1.53 | 0.88 | 1.49 | 0.88 | 1.45 | 0.86 | 1.44 | 0.87 | 1.43 | 0.89 |
| Impact factor | 0.49 | 0.42 | 0.49 | 0.42 | 0.51 | 0.42 | 0.38 | 0.44 | 0.38 | 0.44 | 0.40 | 0.43 |
| Main focus | 0.71 | 0.45 | 0.72 | 0.45 | 0.69 | 0.46 | 0.69 | 0.46 | 0.69 | 0.46 | 0.62 | 0.49 |
| Published | 0.60 | 0.49 | 0.61 | 0.49 | 0.61 | 0.49 | 0.68 | 0.47 | 0.67 | 0.47 | 0.65 | 0.48 |

***Note:*** The table displays the mean and standard deviation of all the primary study characteristics across all the studies. The weighted statistics are calculated using a weight equal to the inverse of the number of estimates collected per study.

***Table C3: Summary Statistics of Meta-Regression Variables: Growth Rate of Credit***

| | Unweighted | | | | | | Weighted | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Short | | Medium | | Long | | Short | | Medium | | Long | |
| | Mean | SD | Mean | SD | Mean | SD | Mean | SD | Mean | SD | Mean | SD |
| Semi-elasticity | -1.12 | 3.68 | -1.20 | 3.40 | -1.65 | 3.39 | -0.62 | 2.86 | -1.43 | 3.82 | -2.41 | 4.17 |
| $SE^2$ | 16.87 | 52.32 | 10.59 | 36.53 | 11.90 | 32.49 | 6.55 | 26.28 | 10.59 | 33.69 | 19.63 | 39.21 |
| **Data characteristics** | | | | | | | | | | | | |
| Bank credit | 0.58 | 0.49 | 0.61 | 0.49 | 0.59 | 0.49 | 0.56 | 0.50 | 0.55 | 0.50 | 0.53 | 0.50 |
| Non-bank credit | 0.20 | 0.40 | 0.19 | 0.39 | 0.18 | 0.38 | 0.12 | 0.33 | 0.12 | 0.32 | 0.10 | 0.30 |
| Credit to households | 0.30 | 0.46 | 0.28 | 0.45 | 0.29 | 0.45 | 0.18 | 0.39 | 0.19 | 0.39 | 0.16 | 0.36 |
| Credit to firms | 0.09 | 0.29 | 0.08 | 0.28 | 0.06 | 0.23 | 0.14 | 0.34 | 0.13 | 0.34 | 0.13 | 0.34 |
| Asia | 0.06 | 0.25 | 0.06 | 0.23 | 0.05 | 0.23 | 0.14 | 0.34 | 0.08 | 0.27 | 0.05 | 0.22 |
| USA | 0.36 | 0.48 | 0.36 | 0.48 | 0.37 | 0.48 | 0.25 | 0.43 | 0.26 | 0.44 | 0.27 | 0.44 |
| Other countries | 0.26 | 0.44 | 0.25 | 0.43 | 0.19 | 0.39 | 0.35 | 0.48 | 0.38 | 0.49 | 0.38 | 0.49 |
| Real credit | 0.60 | 0.49 | 0.64 | 0.48 | 0.56 | 0.50 | 0.51 | 0.50 | 0.57 | 0.50 | 0.50 | 0.50 |
| SA credit | 0.30 | 0.46 | 0.32 | 0.47 | 0.34 | 0.48 | 0.45 | 0.50 | 0.40 | 0.49 | 0.39 | 0.49 |
| Detrended credit | 0.05 | 0.22 | 0.05 | 0.21 | 0.05 | 0.23 | 0.17 | 0.38 | 0.16 | 0.37 | 0.10 | 0.30 |
| Lower data frequency | 0.86 | 0.35 | 0.87 | 0.33 | 0.99 | 0.10 | 0.73 | 0.45 | 0.79 | 0.41 | 0.94 | 0.24 |
| No. of countries | 2.98 | 5.31 | 2.80 | 5.09 | 3.47 | 5.86 | 2.52 | 4.61 | 2.45 | 4.51 | 3.21 | 5.42 |
| No. of years | 25.46 | 14.33 | 24.98 | 13.78 | 26.52 | 15.38 | 21.74 | 10.42 | 22.09 | 10.03 | 23.36 | 10.48 |
| Midyear | 1997 | 7.61 | 1997 | 7.53 | 1997 | 7.43 | 1999 | 8.14 | 1999 | 7.75 | 1998 | 7.71 |
| **Model specification and estimation** | | | | | | | | | | | | |
| Panel VAR | 0.09 | 0.29 | 0.08 | 0.28 | 0.12 | 0.33 | 0.14 | 0.34 | 0.13 | 0.34 | 0.19 | 0.39 |
| Other VAR | 0.35 | 0.48 | 0.37 | 0.48 | 0.38 | 0.49 | 0.12 | 0.33 | 0.14 | 0.35 | 0.14 | 0.35 |
| Bayesian estimation | 0.60 | 0.49 | 0.56 | 0.50 | 0.59 | 0.49 | 0.38 | 0.49 | 0.40 | 0.49 | 0.44 | 0.50 |
| Sign restrictions | 0.57 | 0.50 | 0.53 | 0.50 | 0.60 | 0.49 | 0.55 | 0.50 | 0.51 | 0.50 | 0.52 | 0.50 |
| Controls: Asset prices | 0.67 | 0.47 | 0.69 | 0.46 | 0.73 | 0.44 | 0.55 | 0.50 | 0.59 | 0.49 | 0.64 | 0.48 |
| Control: Exchange rate | 0.10 | 0.30 | 0.10 | 0.30 | 0.12 | 0.32 | 0.41 | 0.50 | 0.37 | 0.48 | 0.32 | 0.47 |
| Controls: Other IR | 0.50 | 0.50 | 0.45 | 0.50 | 0.53 | 0.50 | 0.20 | 0.41 | 0.20 | 0.40 | 0.24 | 0.43 |
| No. of lags | 2.26 | 1.28 | 2.26 | 1.28 | 2.30 | 1.27 | 2.62 | 2.26 | 2.57 | 2.24 | 2.54 | 2.29 |
| **Publication characteristics** | | | | | | | | | | | | |
| Citations | 1.46 | 0.92 | 1.51 | 0.92 | 1.66 | 0.94 | 1.46 | 0.87 | 1.48 | 0.89 | 1.67 | 0.95 |
| Impact factor | 0.27 | 0.24 | 0.27 | 0.24 | 0.30 | 0.25 | 0.19 | 0.23 | 0.20 | 0.22 | 0.23 | 0.23 |
| Main focus | 0.80 | 0.40 | 0.74 | 0.44 | 0.78 | 0.41 | 0.74 | 0.44 | 0.68 | 0.47 | 0.71 | 0.46 |
| Published | 0.67 | 0.47 | 0.70 | 0.46 | 0.80 | 0.40 | 0.73 | 0.45 | 0.74 | 0.44 | 0.79 | 0.41 |

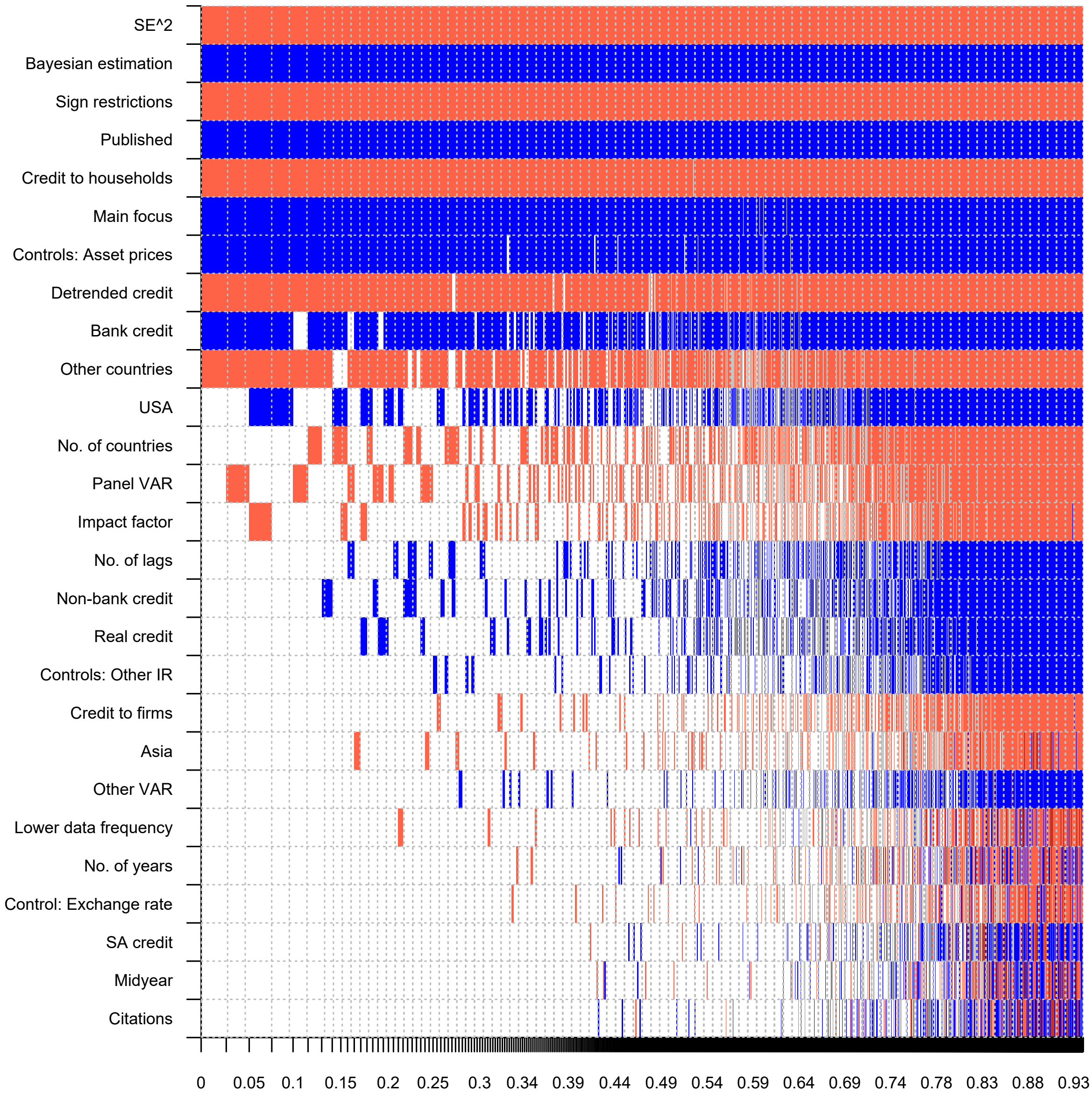
***Note:*** The table displays the mean and standard deviation of all the primary study characteristics across all the studies. The weighted statistics are calculated using a weight equal to the inverse of the number of estimates collected per study.

***Table C4: Bayesian Model Averaging: Growth Rate of Credit***

| | Short horizon | | | Medium horizon | | | Long horizon | | |
|---|---|---|---|---|---|---|---|---|---|
| | P. mean | P. SD | PIP | P. mean | P. SD | PIP | P. mean | P. SD | PIP |
| Constant | 1.844 | | 1.000 | 0.348 | | 1.000 | 1.232 | | 1.000 |
| $SE^2$ | -0.761 | 0.042 | 1.000 | -0.913 | 0.027 | 1.000 | -0.568 | 0.043 | 1.000 |
| **Data characteristics** | | | | | | | | | |
| Bank credit | 0.018 | 0.048 | 0.175 | 0.069 | 0.055 | 0.708 | 0.342 | 0.069 | 1.000 |
| Non-bank credit | 0.020 | 0.048 | 0.209 | 0.040 | 0.042 | 0.552 | 0.078 | 0.074 | 0.604 |
| Credit to households | -0.183 | 0.049 | 0.998 | 0.000 | 0.005 | 0.040 | 0.0004 | 0.010 | 0.058 |
| Credit to firms | -0.017 | 0.040 | 0.216 | -0.005 | 0.015 | 0.146 | -0.001 | 0.010 | 0.066 |
| Asia | -0.000 | 0.008 | 0.038 | 0.038 | 0.030 | 0.692 | 0.007 | 0.021 | 0.151 |
| USA | 0.008 | 0.031 | 0.101 | 0.027 | 0.047 | 0.301 | -0.432 | 0.066 | 1.000 |
| Other countries | 0.005 | 0.023 | 0.089 | 0.001 | 0.014 | 0.064 | -0.335 | 0.061 | 1.000 |
| Real credit | 0.018 | 0.047 | 0.184 | 0.023 | 0.037 | 0.343 | 0.001 | 0.012 | 0.058 |
| SA credit | 0.001 | 0.011 | 0.047 | -0.108 | 0.048 | 0.898 | -0.070 | 0.074 | 0.568 |
| Detrended credit | 0.052 | 0.056 | 0.550 | 0.122 | 0.022 | 1.000 | 0.018 | 0.040 | 0.238 |
| Lower data frequency | -0.008 | 0.029 | 0.115 | -0.004 | 0.020 | 0.090 | -0.001 | 0.011 | 0.067 |
| No. of countries | 0.013 | 0.034 | 0.181 | 0.002 | 0.012 | 0.062 | 0.001 | 0.014 | 0.061 |
| No. of years | 0.019 | 0.054 | 0.170 | 0.008 | 0.026 | 0.128 | -0.002 | 0.023 | 0.061 |
| Midyear | -0.008 | 0.031 | 0.101 | -0.001 | 0.015 | 0.062 | -0.004 | 0.024 | 0.082 |
| No. of lags | -0.001 | 0.012 | 0.046 | -0.003 | 0.016 | 0.082 | -0.010 | 0.029 | 0.159 |
| **Model specification and estimation** | | | | | | | | | |
| Panel VAR | 0.007 | 0.027 | 0.112 | 0.086 | 0.043 | 0.876 | 0.434 | 0.091 | 1.000 |
| Other VAR | -0.016 | 0.050 | 0.140 | -0.003 | 0.034 | 0.167 | 0.357 | 0.072 | 1.000 |
| Controls: Asset prices | 0.001 | 0.013 | 0.047 | 0.019 | 0.038 | 0.246 | 0.0004 | 0.013 | 0.060 |
| Control: Exchange rate | -0.001 | 0.011 | 0.047 | -0.038 | 0.040 | 0.562 | 0.355 | 0.065 | 1.000 |
| Controls: Other IR | 0.001 | 0.015 | 0.049 | 0.058 | 0.062 | 0.569 | -0.004 | 0.025 | 0.090 |
| Bayesian estimation | 0.139 | 0.059 | 0.925 | 0.034 | 0.046 | 0.442 | -0.001 | 0.014 | 0.068 |
| Sign restrictions | 0.001 | 0.012 | 0.046 | -0.005 | 0.023 | 0.093 | 0.006 | 0.029 | 0.097 |
| **Publication characteristics** | | | | | | | | | |
| Citations | -0.001 | 0.015 | 0.050 | -0.127 | 0.064 | 0.870 | -0.679 | 0.072 | 1.000 |
| Impact factor | 0.006 | 0.024 | 0.099 | 0.002 | 0.079 | 0.368 | 0.375 | 0.087 | 0.998 |
| Main focus | -0.004 | 0.017 | 0.077 | -0.150 | 0.038 | 0.988 | -0.008 | 0.031 | 0.126 |
| Published | -0.010 | 0.029 | 0.141 | 0.002 | 0.015 | 0.074 | 0.001 | 0.016 | 0.067 |

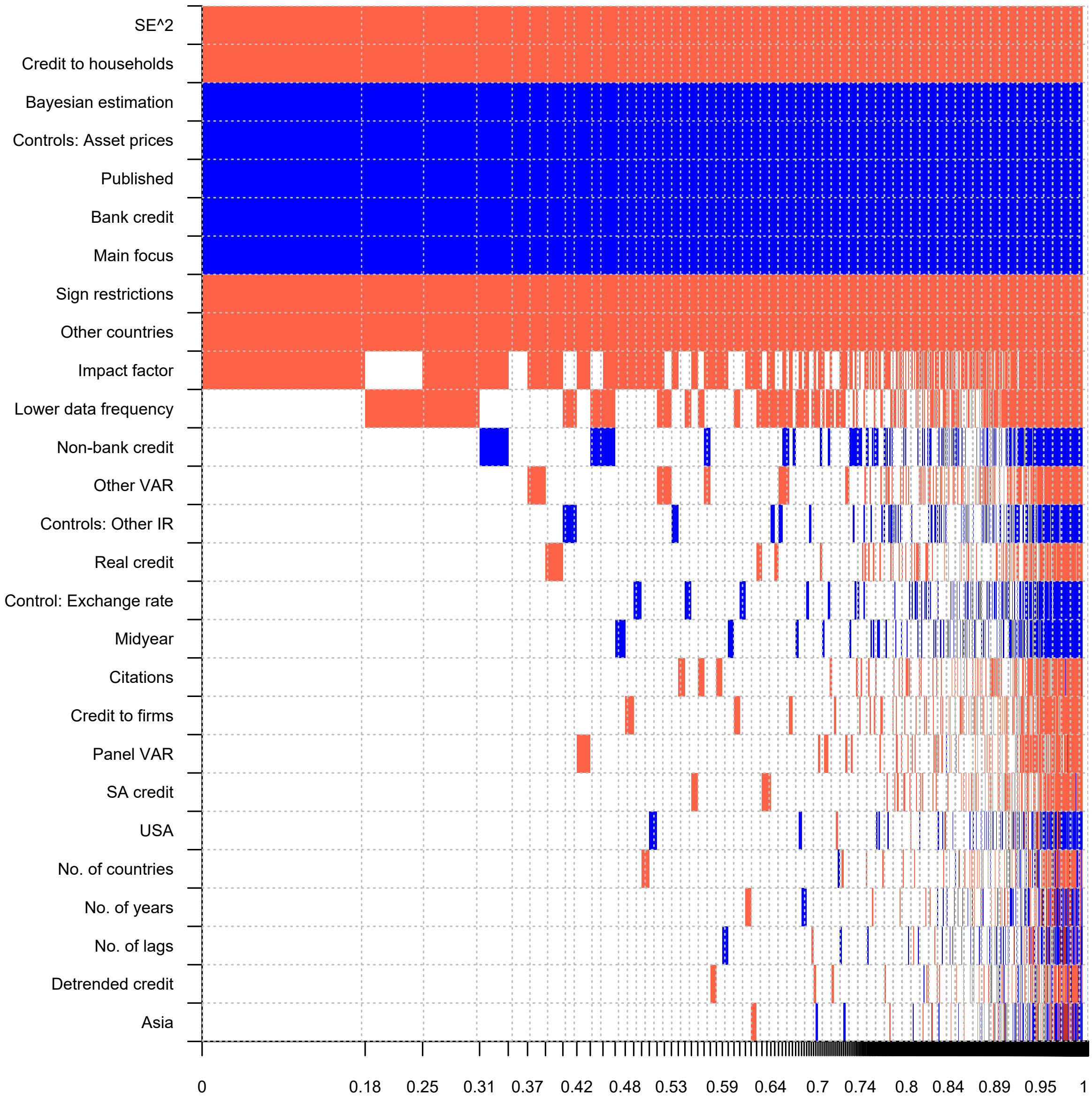
***Note:*** The table presents the estimation results of the collected semi-elasticities on the primary study characteristics for specifications with growth rate transformation of the credit variable. The BMA procedure employs a combination of the uniform model prior and the unit information g-prior recommended by Eicher et al. (2011). The coefficients are standardized. P. mean, P. SD, and PIP stand for posterior mean, posterior standard deviation, and posterior inclusion probability, respectively.

***Figure C1: Model Inclusion in Bayesian Model Averaging: Log-Level of Credit and Short-Term Horizon***



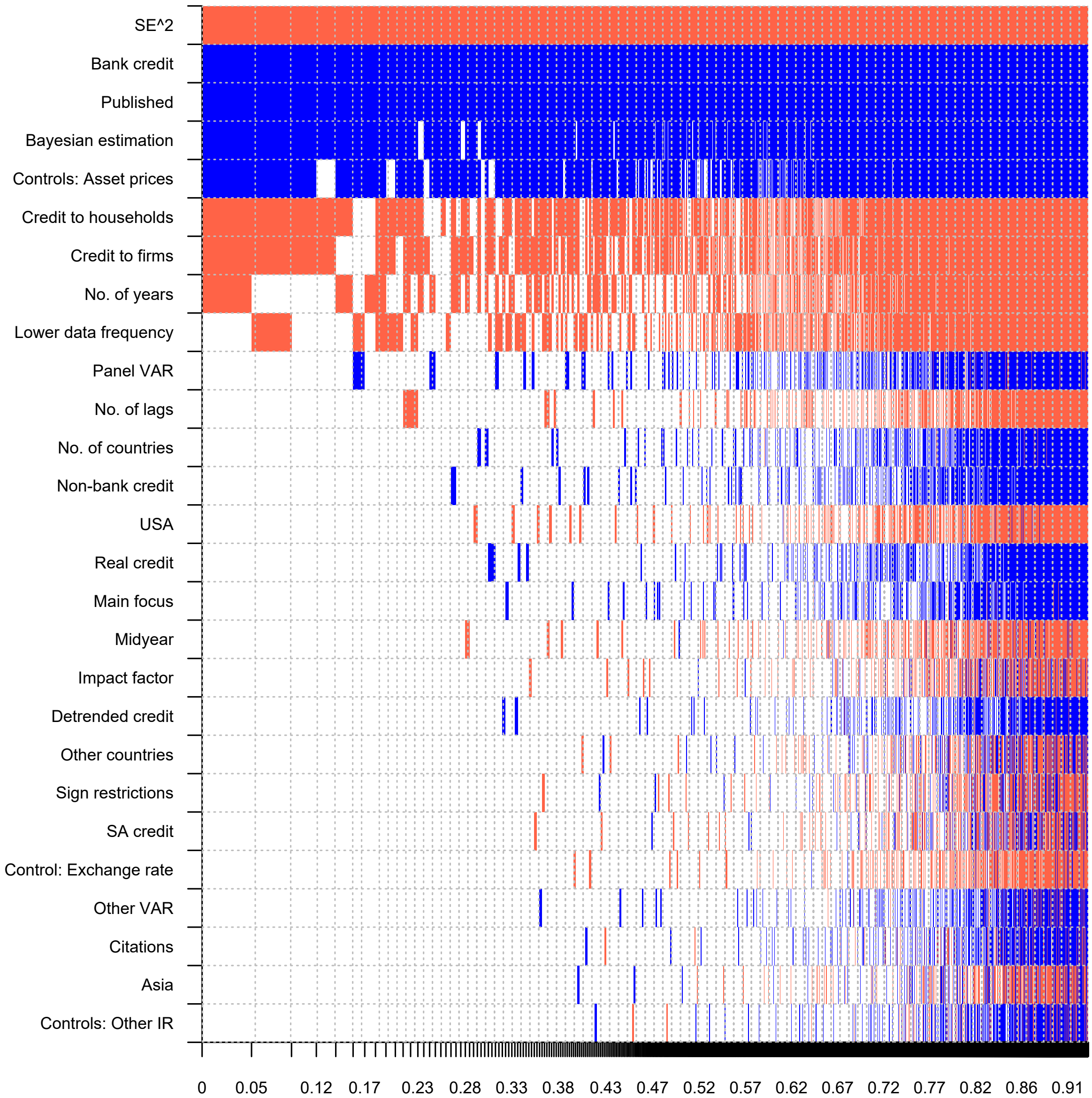

***Note:*** Columns denote individual models; the variables are sorted by posterior inclusion probability in descending order. The horizontal axis denotes the cumulative posterior model probabilities; the 10,000 best models are shown. To ensure convergence, we employ 3 million iterations and 1 million burn-ins. Blue color (darker in grayscale) 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 color (lighter in 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 C2: Model Inclusion in Bayesian Model Averaging: Log-Level of Credit and Medium-Term Horizon***



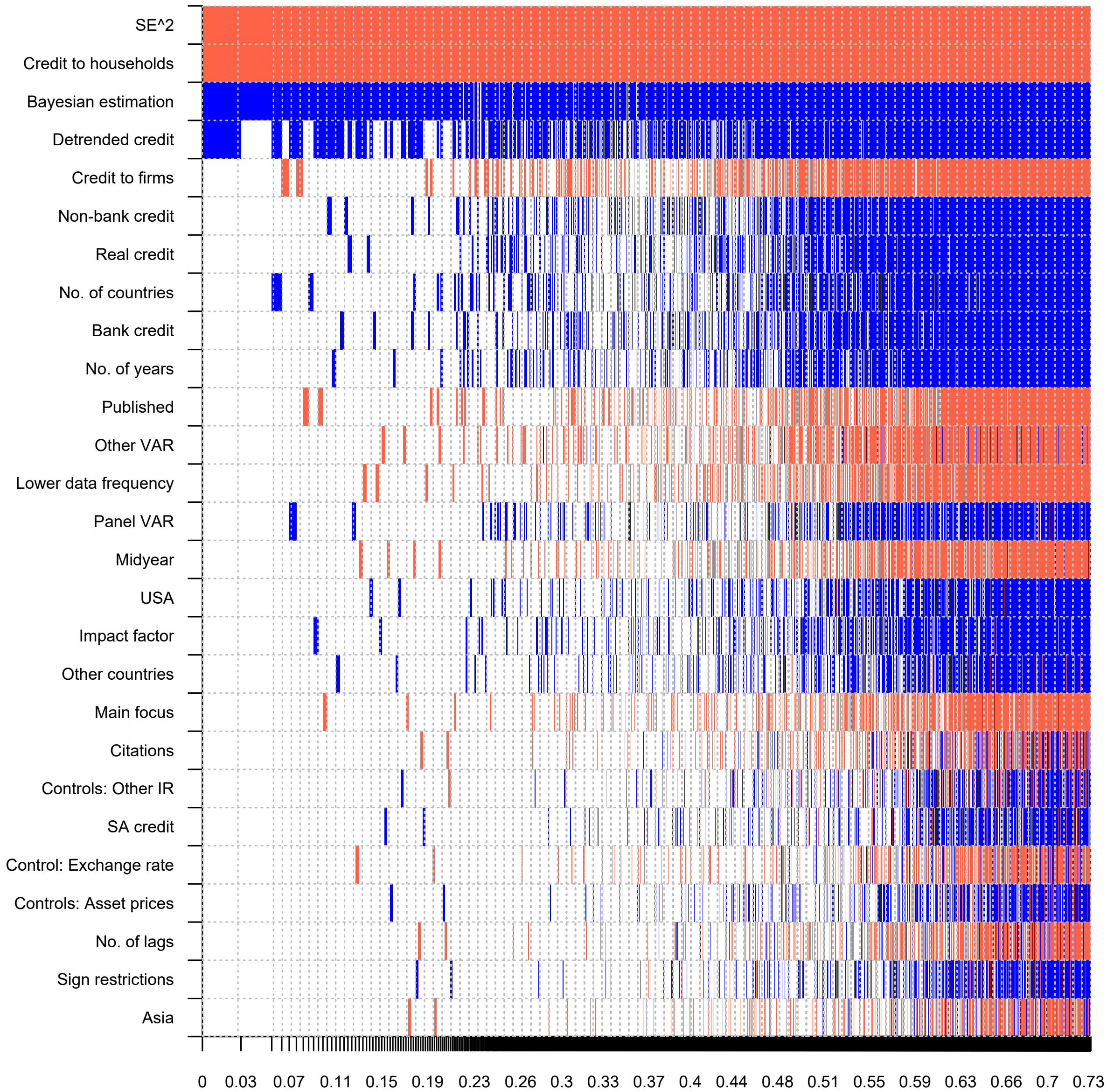

***Note:*** Columns denote individual models; the variables are sorted by posterior inclusion probability in descending order. The horizontal axis denotes the cumulative posterior model probabilities; the 10,000 best models are shown. To ensure convergence, we employ 3 million iterations and 1 million burn-ins. Blue color (darker in grayscale) 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 color (lighter in 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 C3: Model Inclusion in Bayesian Model Averaging: Log-Level of Credit and Long-Term Horizon***



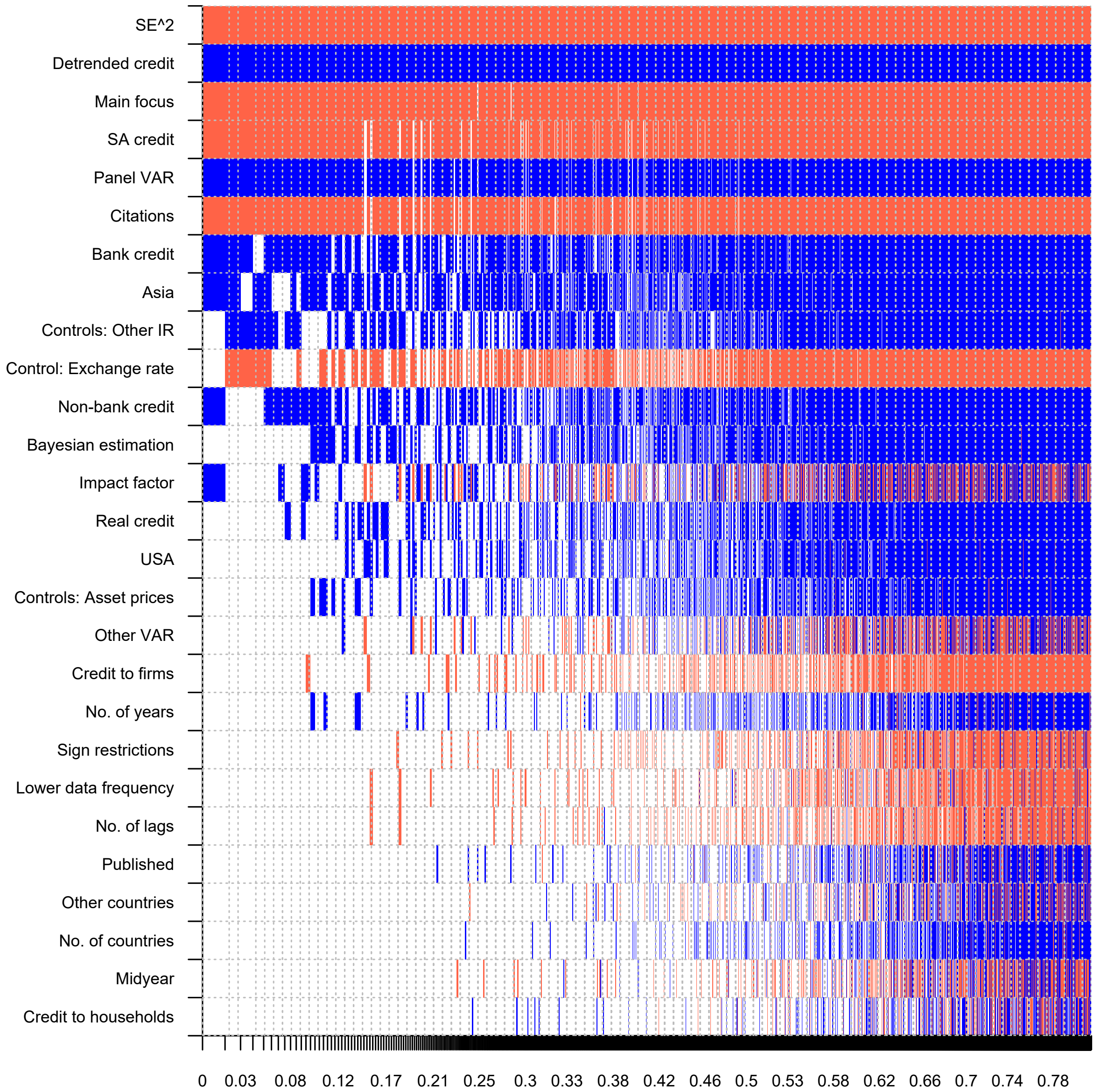

***Note:*** Columns denote individual models; the variables are sorted by posterior inclusion probability in descending order. The horizontal axis denotes the cumulative posterior model probabilities; the 10,000 best models are shown. To ensure convergence, we employ 3 million iterations and 1 million burn-ins. Blue color (darker in grayscale) 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 color (lighter in 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 C4: Model Inclusion in Bayesian Model Averaging: Growth Rate of Credit and Short-Term Horizon***



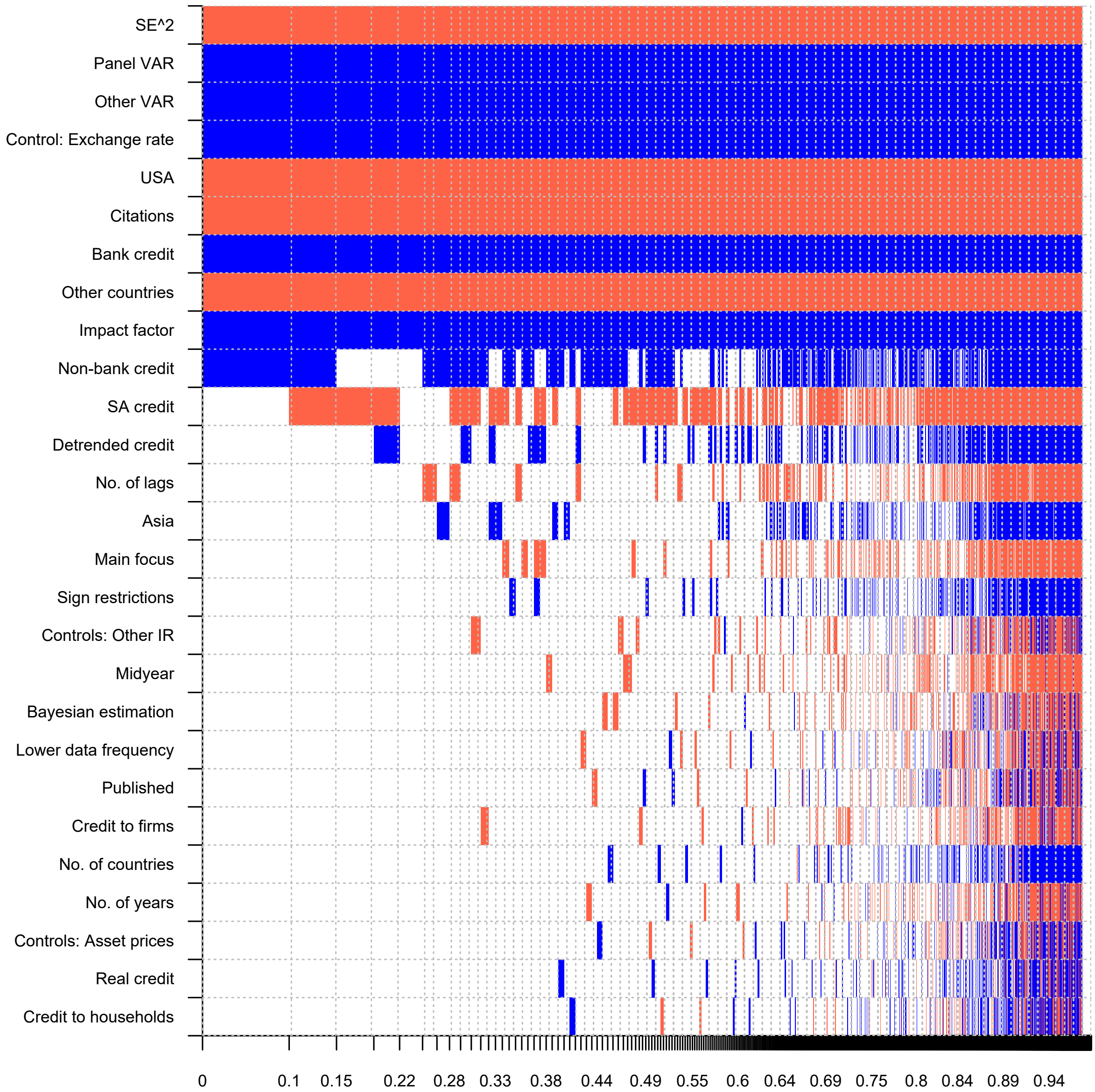

*Note:* Columns denote individual models; the variables are sorted by posterior inclusion probability in descending order. The horizontal axis denotes the cumulative posterior model probabilities; the 10,000 best models are shown. To ensure convergence, we employ 3 million iterations and 1 million burn-ins. Blue color (darker in grayscale) 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 color (lighter in 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 C5: Model Inclusion in Bayesian Model Averaging: Growth Rate of Credit and Medium-Term Horizon***




***Note:*** Columns denote individual models; the variables are sorted by posterior inclusion probability in descending order. The horizontal axis denotes the cumulative posterior model probabilities; the 10,000 best models are shown. To ensure convergence, we employ 3 million iterations and 1 million burn-ins. Blue color (darker in grayscale) 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 color (lighter in 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 C6: Model Inclusion in Bayesian Model Averaging: Growth Rate of Credit and Long-Term Horizon***




***Note:*** Columns denote individual models; the variables are sorted by posterior inclusion probability in descending order. The horizontal axis denotes the cumulative posterior model probabilities; the 10,000 best models are shown. To ensure convergence, we employ 3 million iterations and 1 million burn-ins. Blue color (darker in grayscale) 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 color (lighter in 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 C7: Prior Sensitivity of BMA Results: Log-Level of Credit***

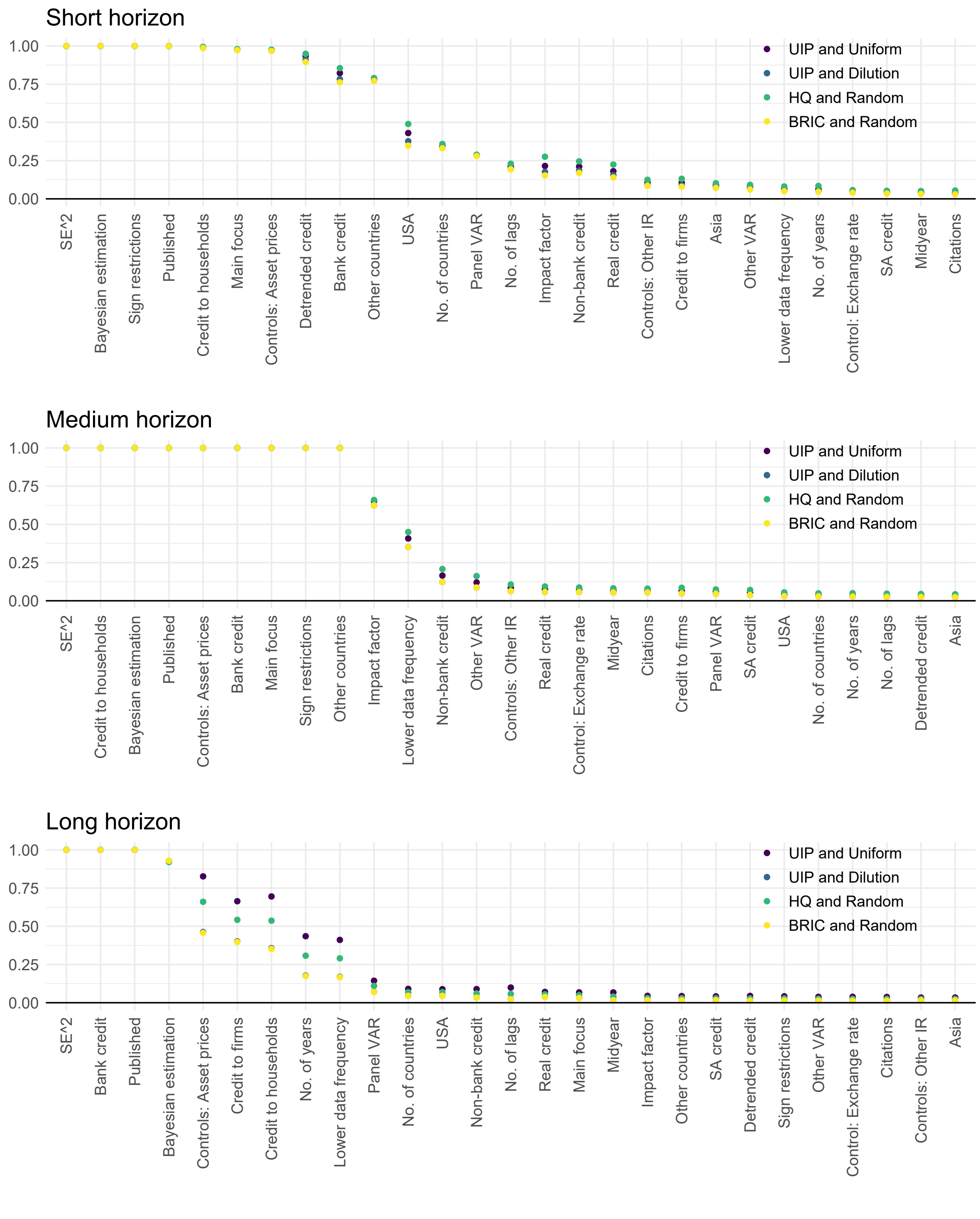


***Figure C8: Prior Sensitivity of BMA Results: Growth of Credit***

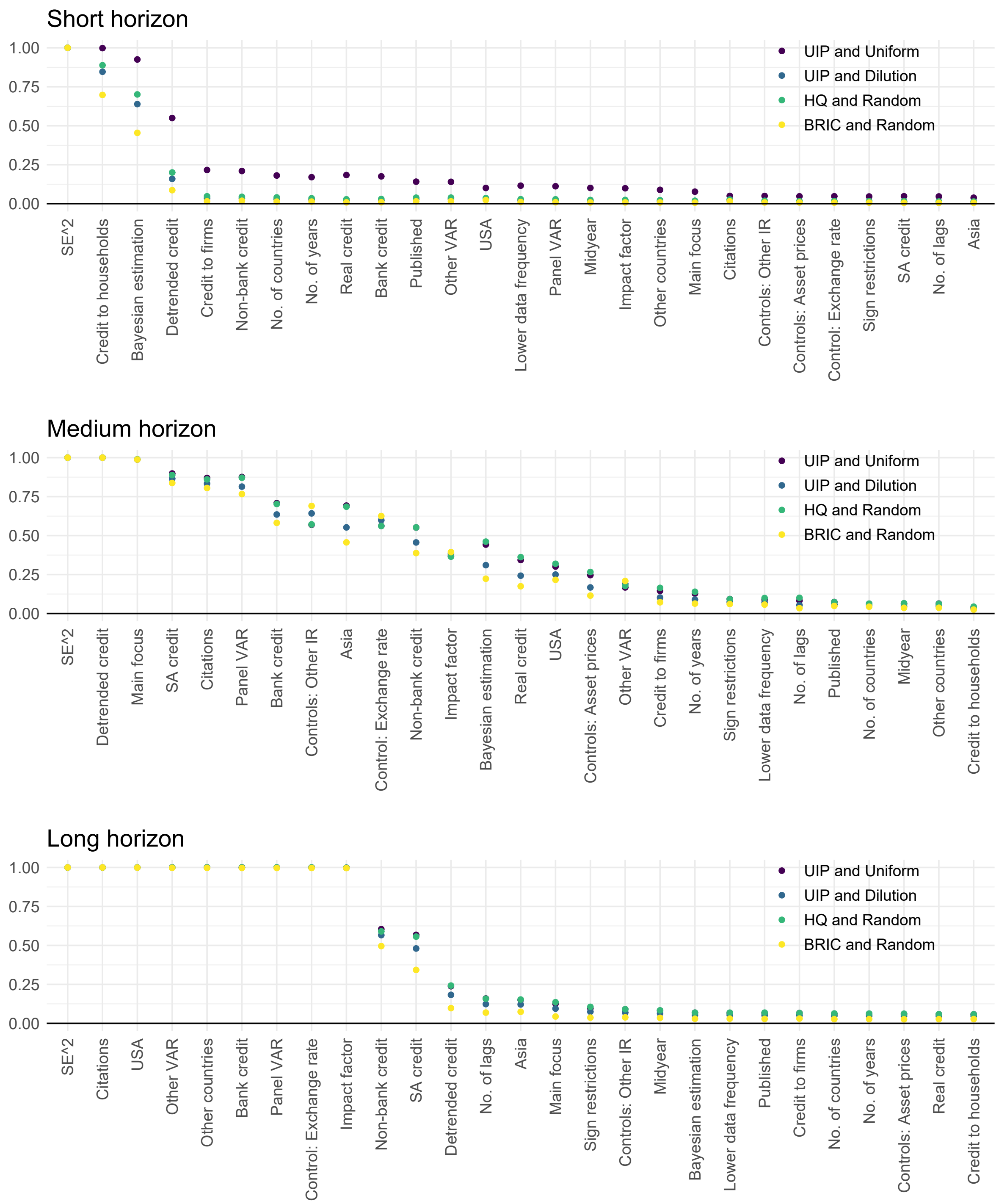


***Table C5: Model Comparison: Growth Rate of Credit***

| | Short horizon | | Medium horizon | | Long horizon | |
|---|---|---|---|---|---|---|
| | Full model | Reduced model | Full model | Reduced model | Full model | Reduced model |
| Meta-analysis random effects | | | | | | |
| Number of parameters | 5 | 3 | 7 | 3 | 11 | 3 |
| AICc | 682 | 682 | 593 | 598 | 405 | 400 |
| BIC | 670 | 692 | 621 | 610 | 444 | 411 |
| QE | 847 | 851 | 603 | 617 | 231 | 243 |
| LRT | | 4 | | 13*** | | 12 |
| Three-level meta-analysis model | | | | | | |
| Number of parameters | 6 | 4 | 8 | 4 | 12 | 4 |
| AICc | 620 | 620 | 410 | 474 | 398 | 402 |
| BIC | 641 | 634 | 442 | 490 | 440 | 417 |
| QE | 847 | 851 | 603 | 617 | 231 | 243 |
| LRT | | 3 | | 72*** | | 21*** |

***Note:*** The table presents results from a comparison between a reduced model, as estimated in Table B2, and an extended model including additional moderators with a PIP above 0.8, as estimated in Table C4. AICc, BIC, and LRT stand for corrected Akaike Information Criterion, Bayesian Information Criterion, and Likelihood Ratio Test, respectively. QE is the test statistic of the test for residual heterogeneity. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

***Table C6: Predicted Values: Growth Rate of Credit***

| | Short horizon | Medium horizon | Long horizon |
|---|---|---|---|
| Unweighted | | | |
| Simple mean | -1.121 | -1.200 | -1.645 |
| Corrected mean | -0.217 | -0.299 | -0.939 |
| 32/68 credible intervals | (-1.282, 0.848) | (-0.900, 0.303) | (-1.723, -0.155) |
| Weighted | | | |
| Simple mean | -0.843 | -1.627 | -2.594 |
| Corrected mean | -0.216 | -0.440 | -1.347 |
| 32/68 credible intervals | (-1.293, 0.860) | (-1.053, 0.175) | (-2.148, -0.548) |
| Observations | 264 | 436 | 281 |
| Studies | 28 | 28 | 24 |

***Note:*** The table compares the mean effect beyond bias and its credible intervals with the simple uncorrected mean at all three horizons. The effect beyond bias is calculated using the fitted values based on the complete meta-regression output of the BMA. These values consider the estimated coefficients on all the primary study characteristics, with the exception of the slope coefficient on the square of the standard error ($SE^2$ is set to zero to correct for publication bias). The credible intervals are derived from the predictive densities, which are mixture densities based on the best models identified by the BMA. To calculate the effect beyond bias and its credible intervals, we use the *predict* and *pred.density* functions provided by the BMS package in R. The weighted statistics are calculated using a weight equal to the inverse of the number of estimates collected per study.

***Table C7: Implicit Values: Growth Rate of Credit***

| | Short horizon | Medium horizon | Long horizon |
|---|---|---|---|
| Frequentist approach and sign restrictions | | | |
| Mean | -0.836 | -0.447 | -0.915 |
| 32/68 credible intervals | (-1.908, 0.234) | (-1.056, 0.164) | (-1.702, -0.130) |
| W. Mean | -0.621 | -0.550 | -1.322 |
| W. 32/68 credible intervals | (-1.701, 0.457) | (-1.167, 0.072) | (-2.125, -0.522) |
| Credit to households | | | |
| Mean | -1.226 | -0.292 | -0.936 |
| 32/68 credible intervals | (-2.297, -0.156) | (-0.893, 0.309) | (-1.720, -0.151) |
| W. Mean | -1.407 | -0.430 | -1.342 |
| W. 32/68 credible intervals | (-2.491, -0.323) | (-1.044, 0.185) | (-2.143, -0.542) |

***Note:*** The table presents the mean implied effect and its credible intervals at all three horizons for two different cases: first, where the effects are estimated using the frequentist approach, with shocks identified through sign restrictions; and second, where the credit variable is credit granted to households. The implied effect shows what the mean semi-elasticity would be if all the studies used the same strategy. Similarly to the effect beyond bias in Table C6, the implied effect is calculated using the fitted values based on the complete meta-regression output of the BMA. The credible intervals are derived from the predictive densities, which are mixture densities based on the best models identified by the BMA. To calculate the implied effect and its credible intervals, we use the *predict* and *pred.density* functions provided by the BMS package in R. The weighted statistics are calculated using a weight equal to the inverse of the number of estimates collected per study.

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