Working Paper Series 12/2025

Credit Shocks Fade, Output Shocks Persist: A Meta-Analysis of 2,600 VAR Estimates Across 63 Countries

Jan Janků, Simona Malovaná, Josef Bajzík, Klára Moravcová, Ngoc Anh Ngo

Czech National Bank —— Working Paper Series —— 12/2025

CZECH NATIONAL BANK

www.cnb.cz

Reviewed by: Jerome Geyer-Klingeberg (Augsburg University)

Josef Simpartl (Czech National Bank)

Project Coordinator: Martin Hodula

Credit Shocks Fade, Output Shocks Persist: A Meta-Analysis of 2,600 VAR Estimates Across 63 Countries

Jan Janků, Simona Malovaná, Josef Bajzík, Klára Moravcová, and Ngoc Anh Ngo *

Abstract

Credit cycles have become longer and more pronounced since the mid-1980s, often amplifying business cycle downturns. While many studies examine the interplay between credit and output, they disagree on the strength and persistence of these effects. We conduct a meta-analysis of over 2,600 point estimates extracted from impulse response functions reported in 68 VAR-based studies across 63 countries. We find that output reacts quickly but briefly to credit shocks, whereas credit responses to output shocks are larger and more persistent, especially in advanced economies. Publication bias inflates reported effects, yet adjusted estimates remain economically significant. We also document substantial heterogeneity, with stronger responses in European samples, studies of corporate credit, and models using Bayesian methods or sign restrictions. These findings help clarify the typical dynamics between credit and output, informing monetary and macroprudential policy design.

JEL Codes: C32, C83, E32, G21.
Keywords: Credit cycles, impulse response functions, meta-analysis, output, publication bias.

* Jan Janků, Czech National Bank and Technical University of Ostrava, jan.janku@cnb.cz
Simona Malovaná, Czech National Bank and Charles University in Prague, simona.malovana@cnb.cz
Josef Bajzík, Anglo-American University in Prague, josef.bajzik@aauni.edu
Klára Moravcová, Masaryk University in Brno, klara.moravcova@econ.muni.cz
Ngoc Anh Ngo, Technical University of Ostrava
The views expressed are those of the authors and not necessarily those of the Czech National Bank. We gratefully acknowledge the comments and suggestions from Jerome Geyer-Klingeberg, Josef Simpartl, Martin Hodula, Josef Švéda and Tomáš Havránek. We are also thankful to the participants of the Czech National Bank research seminars and 2022 MAER-Net Colloquium in Kyoto. All errors and omissions remain the fault of the authors.

1. Introduction

The relationship between credit and the business cycle is a cornerstone of both macroeconomic theory and policy. Economic models have long emphasized the dual role of credit: as a driver of economic expansions and as a source of amplification during downturns (Bernanke and Gertler, 1995; Borio, 2014). Credit supports productive investment, innovation, and consumption smoothing. However, excessive credit growth can heighten systemic vulnerabilities, leading to deeper recessions when shocks hit (Schularick and Taylor, 2012; Jordà et al., 2017).

Yet despite extensive theoretical and empirical research, substantial uncertainty remains about the magnitude, persistence, and even the direction of the effects linking credit and output. Studies differ widely in how they measure these interactions – using different models, data samples, time horizons, and transformation choices – resulting in a fragmented picture that complicates robust inference. The asymmetry and timing of these effects remain especially contested: while stronger economic growth clearly improves credit demand (Bernanke and Blinder, 1988; Kashyap et al., 1992; Stepanyan and Guo, 2011), the lasting impact of credit shocks on output is less certain, often seen as short-lived due to debt overhangs or financial vulnerabilities (Borio, 2014; Claessens et al., 2012).

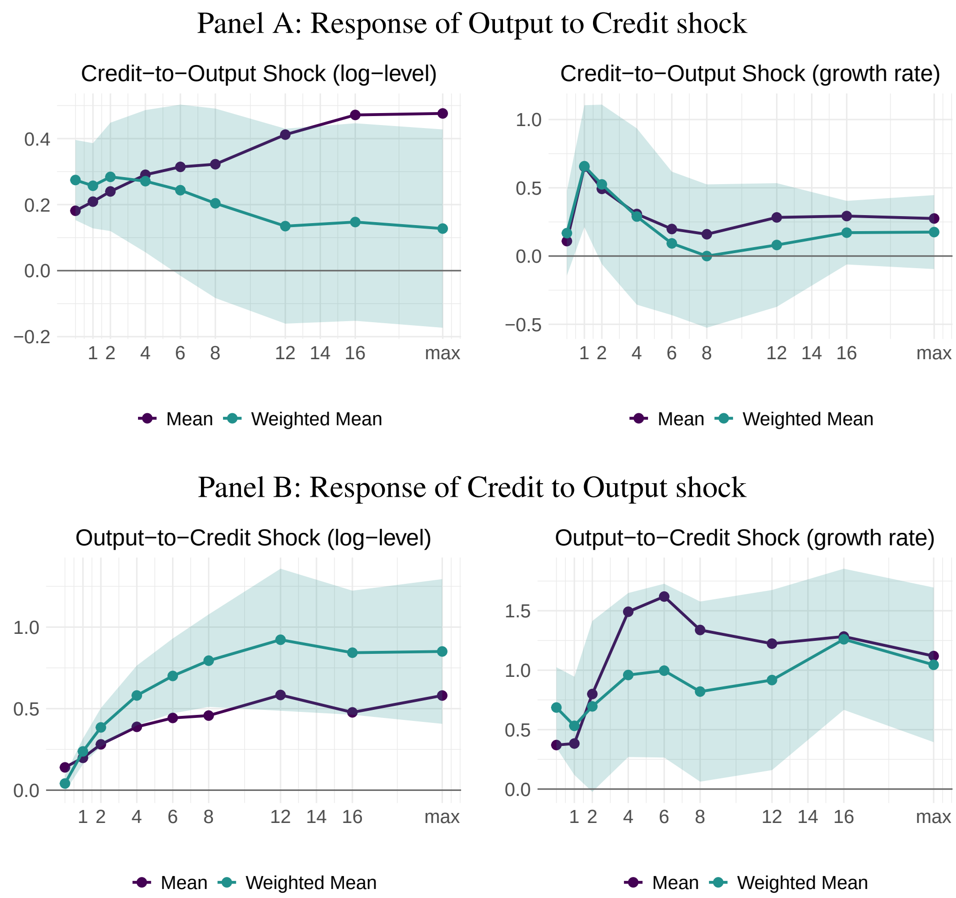
To bring greater clarity, this paper conducts a comprehensive meta-analysis of semi-elasticities from impulse response functions reported in the empirical literature. We focus on estimates from vector autoregressive (VAR) models, which provide a common empirical framework for tracing the dynamic interaction between credit and output. Our meta-analysis synthesizes 2,629 point estimates extracted from 68 studies across 63 countries, covering data from 1861 to 2019.

We make three main contributions. First, we systematically quantify the responses of output to credit shocks and of credit to output shocks across different time horizons, distinguishing between growth-rate and log-level transformations. We find that while the response of output to credit shocks peaks at short horizons and dissipates over time, the response of credit to output shocks is highly persistent, with the largest effects appearing at long horizons. Figure 1 illustrates these average impulse responses. It shows that credit-to-output shocks generally have their strongest impact on output growth after one quarter, whereas output-to-credit shocks peak after about six quarters and remain significant for up to sixteen quarters.

Second, we investigate publication bias. This bias arises when studies selectively report results that align with expected signs or are statistically significant, while findings lacking significance or contradicting established theories are underrepresented. Such tendencies may distort the shape of the median impulse responses and influence broader perceptions of the relationships being analyzed. Both simple statistical and graphical tests as well as more sophisticated empirical methods reveal that published studies systematically overstate the average effect. Even after adjusting for this bias, however, we still find that the relationship between credit and output remains positive and statistically significant, though somewhat attenuated in magnitude.

Third, we explore heterogeneity across studies. While the mean impulse responses provide a useful summary, they can mask important differences driven by country-specific factors, data quality, or methodological choices. For example, strong credit effects in some economies may be offset by negligible impacts elsewhere, and variations in financial development or estimation sample length can also play a role. We show that many of these differences account for a substantial share of the variance in reported effects. Studies explicitly focused on identifying the links between credit and output tend to find stronger and more persistent impacts, compared to studies primarily examining other relationships (e.g., responses to monetary policy shocks). Effects are also systematically shaped by the type of credit, the region studied, and the identification strategy used – most notably, Bayesian methods and sign restrictions tend to yield larger effects, especially at short horizons.

Figure 1: Mean Impulse Response Functions



Note: The figure displays the average response of credit and output to a one-percentage-point increase in credit or output, along with the average 68% confidence interval. The panels compare the simple mean and the weighted mean, where the weights are the inverse of the number of collected estimates per study. Panel A shows the response to a credit-to-output shock, while Panel B shows the response to an output-to-credit shock. In the log-level specification, both series are expressed in logarithms, so an impulse response represents the percent change in the dependent variable following a 1% increase in the shock variable. In the growth-rate specification, both series are entered as rates of change; thus, the impulse response reports the percentage-point change in the dependent variable after a one-percentage-point change in the shock variable.

By systematically combining VAR-based impulse responses from a broad set of studies, our paper fills a gap left by earlier meta-analyses that mostly focused on long-run growth regressions or credit-to-GDP ratios (Arestis et al., 2015; Valickova et al., 2015; Bijlsma et al., 2018). This lets us clearly quantify the typical size, persistence, and asymmetry of credit–output links over the business cycle. These insights are directly relevant for policymakers calibrating interest rates, capital buffers, or targeted credit tools to support both macroeconomic and financial stability (Borio, 2014; Cerutti et al., 2017a; Galí, 2015).

The remainder of the paper is structured as follows. Section 2 reviews the key theoretical and empirical literature on the links between credit and the business cycle. Section 3 describes our data collection, the construction of the semi-elasticity dataset, and key stylized facts. Section 4 examines publication bias and its impact on estimated effects. Section 5 investigates study-level heterogeneity and the factors driving variation in impulse responses. Section 6 concludes with policy implications and suggestions for future research. An extensive appendix is included as well, providing detailed summaries of all analyses.

2. Literature Review

The link between financial and real economic activity has stood at the center of economic inquiry for over a century, dating back to seminal contributions by Schumpeter (1934), Gurley and Shaw (1955), and Tobin and Brainard (1963). Over time, researchers have employed a wide array of theoretical models, empirical methods, and historical datasets to understand how credit markets influence, and are influenced by, economic output (Levine, 2021).

The Global Financial Crisis (GFC) renewed attention on these dynamics, underscoring how credit booms often precede crises and how credit busts exacerbate recessions (see, e.g., Reinhart and Rogoff, 2009; Jordà et al., 2011; Claessens et al., 2012; Gourinchas and Obstfeld, 2012; Schularick and Taylor, 2012; Aikman et al., 2015). Although early work by Minsky (1975) and Kindleberger (1978) had already highlighted feedback loops between the financial system and the real economy, the concept of a distinct "financial cycle" gained prominence only after the GFC (Borio, 2014). Empirical studies show that financial cycles – often measured through credit aggregates or asset prices – tend to be longer and more pronounced than traditional business cycles, with peaks commonly aligned with financial crises and contractions lasting several years (Drehmann et al., 2012; Claessens et al., 2011). These dynamics are central not only to understanding business cycle fluctuations but also to designing effective monetary and macroprudential policies. Because credit expansions often outpace GDP growth over extended periods (Cecchetti et al., 2011; Schularick and Taylor, 2012), credit dynamics now feature prominently in risk assessments by central banks and international institutions (IMF, 2000; BIS, 2001; Hirschbühl and Spitzer, 2021).

One strand of literature focuses on how credit affects output. Early contributions emphasized the role of banks in mobilizing savings to finance productive investment and growth (Bagehot, 1873; Fisher, 1933; Goldsmith, 1969; Brunner and Meltzer, 1972). This theme was formalized by King and Levine (1993), whose cross-country analysis linked financial depth to long-run economic growth. Later work showed how credit can amplify cyclical fluctuations, acting either as a propagation channel for external shocks (Blinder, 1987; Gertler and Bernanke, 1989; Bernanke et al., 1999; Brunnermeier et al., 2012), as an independent source of shocks (Bernanke and Gertler, 1995; Kiyotaki and Moore, 1997; Christiano et al., 2010), or even as a central driver of macroeconomic cycles (Prieto et al., 2016). A complementary strand of literature highlights non-linearities: excessive credit growth beyond certain thresholds may dampen output or increase crisis risk (see, among others, Arcand et al., 2015; Cecchetti and Kharroubi, 2019).

A parallel line of research explores the reverse relationship – how output influences credit dynamics. This "demand-following" view of financial development – highlighted in early works by Robinson (1952), Gurley and Shaw (1967), and Chandler (1977) – posits that rising output increases income and creditworthiness, thereby boosting borrowing for consumption and investment. Empirical studies support this channel: output growth leads to higher credit volumes through improved borrower conditions and business opportunities (Bernanke and Blinder, 1988; Kashyap et al., 1992; Calza et al., 2003; Apergis et al., 2007). Evidence also suggests that this output-to-credit dynamic may dominate in advanced economies, whereas developing countries often exhibit more supply-constrained credit markets (Jung, 1986; Roubini and Sala-i Martin, 1992; King and Levine, 1993).

3. The Semi-Elasticity Dataset and Stylized Facts

3.1 Paper Selection Procedure and Collection of Semi-Elasticities

We collect estimates of semi-elasticity that show the relationship between credit and output. The literature uses various approaches and models for this estimation, such as VAR and DSGE models, and simple linear and panel data regressions. We focus only on estimates based on VAR models to avoid comparing apples and oranges. Limiting our sample to VAR estimates has several benefits. Compared to DSGE models, VARs are generally more data-driven, and therefore better suited for meta-analysis methods, while the structure of both is founded on theory. 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 credit and output, 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 ε_t is a vector of structural shocks.[1]

Besides credit and output, the set of endogenous variables typically includes consumer prices and the monetary policy rate. Depending on the specific model, it may also incorporate other variables like house prices, exchange rates, and long-term interest rates. We also collect information on these variables and use it to analyze whether the effects differ with respect to the set of endogenous variables in the model (see Section 5.3). To estimate equation (1), researchers convert it into a reduced-form representation. The main outputs of VAR models, the responses of endogenous variables to structural shocks, are often shown as impulse response functions in graphs. These visual aids help readers understand the responses over various time horizons.

Our paper selection procedure follows well-established approaches in the meta-analysis literature (see, e.g., Stanley and Doucouliagos, 2012; Havránek et al., 2020; Irsova et al., 2024) and is described in full detail in Appendix A.1. Briefly, we screened over 1,050 studies and, after applying specific criteria – restricting to VAR models, requiring reported confidence intervals, using GDP as the output variable, and excluding ratios such as credit-to-GDP[2] – selected 68 studies. This process ensured methodological consistency essential for meta-analysis.

To provide initial insights into the studies we have pooled, we present some characteristics in Table 1 and Figure 2. Most studies in our sample are published in peer-reviewed journals. Among the unpublished studies, the majority are working papers from central banks and international organizations such as the ECB, BIS, IMF, Federal Reserve Banks, and national central banks. A smaller portion of the papers are working papers from other organizations like SUERF, MPRA, CESifo, or Cambridge.[3] The average publication year of the papers is 2013, ranging from 1995 to

[1] Note that in panel VAR models, these equation elements would include an index i and the constant would be a matrix of country-specific fixed effects.

[2] This excluded literature strands covered by previous meta-analytical papers (Arestis et al., 2015; Valickova et al., 2015; Bijlsma et al., 2018; Anwar and Iwasaki, 2023a; Iwasaki, 2022; Ono and Iwasaki, 2022; Anwar and Iwasaki, 2023b).

[3] We included both published and unpublished studies, such as working papers, in our analysis. The inclusion of unpublished studies in a meta-analysis is debated. While published studies might offer higher quality estimates,

2022. On average, the studies in our sample have 76 citations. The data used in each primary study varies significantly, covering different periods and countries. Altogether, the 68 studies in our sample use unbalanced data from 63 countries, spanning from 1861[4] to 2019. In Section 5, we analyze in more detail how the differences in data and methodology used in primary studies affect the estimated impulse response functions.

Table 1: Basic Summary Statistics of the Included Primary Studies

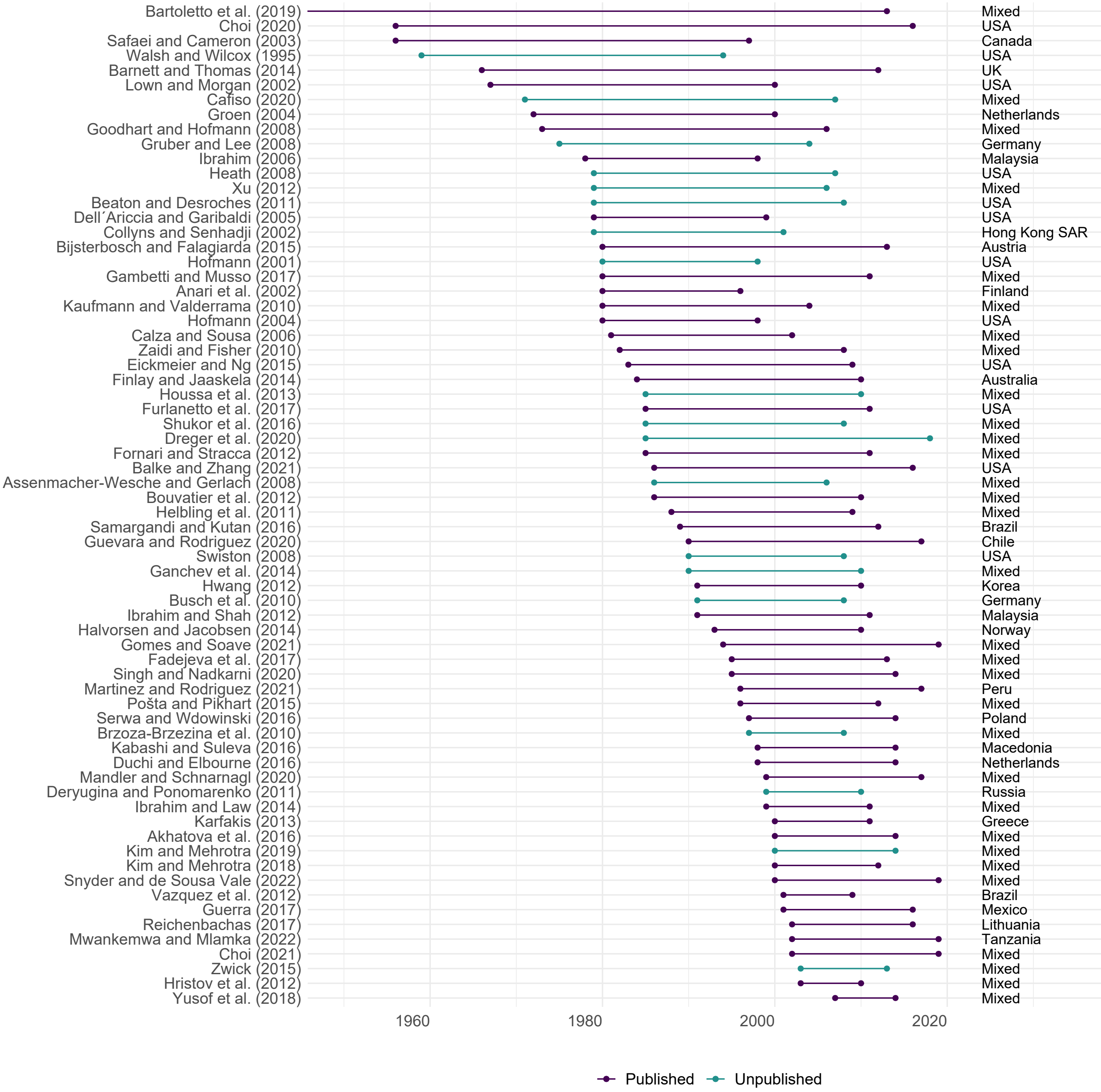
Variable transformation	Studies	IRFs	Estimates	Published (% share)	Publication year (mean)	Impact factor (mean)	Citations (mean)
Total							
Both transf.	68	339	2,629	72	2013	0.39	76
Output-to-credit shock							
Growth rate	4	18	147	50	2013	0.07	41
Log-level	5	43	283	80	2012	1.06	80
Credit-to-output shock							
Growth rate	18	77	613	83	2015	0.25	50
Log-level	20	99	797	60	2011	0.44	79
Both causalities							
Growth rate	11	63	485	73	2014	0.23	152
Log-level	10	39	304	80	2013	0.51	44

Additionally, we analyzed the abstracts of collected studies to understand the shocks they aim to identify. Word clouds for three groups of studies are presented in Figure 3: studies on credit's response to output shocks, output's response to credit shocks, and studies exploring both interactions. Across all groups, the focal point is credit, either as a fluctuation source or a link between the real economy and the financial sector. In examining the abstracts, the term "supply" is most frequently associated with "shock." Particularly in studies about output responding to credit shocks, "supply" is a common term, with "supply shock" mentioned in 15 abstracts. Other frequently paired terms with "shock" are "credit," "lending," and "financial." In comparison, connections between "demand" and "shock" are very limited. This suggests that most authors, especially in studies on credit shocks, intend to identify and estimate the impact of supply shocks.

including both types offers insights into the differences between them. As Rusnák et al. (2013) suggest, including unpublished articles is unlikely to affect results related to publication bias. This is because authors might align their research early on with journal preferences. Studies like Doucouliagos and Stanley (2013) indicate no significant difference in publication selection bias between unpublished and published studies.

[4] One study in our sample dates back to 1861 (Bartoletto et al., 2019), while the next closest in terms of timeline begins its sample in 1956.

Figure 2: Primary Study Datasets: Periods and Countries Covered



Note: The figure shows the timeline of studies included in the meta-analysis, categorized by publication status (published or unpublished) and country of focus. The range reflects the time coverage of the data used in each study, highlighting the diversity of geographic and methodological approaches. "Mixed" refers to studies analyzing multiple countries or regions.

Figure 3: Word Clouds of Abstracts

Note: The word clouds in Figure 3 highlight key terms from abstracts, illustrating thematic differences: "financial" and "macroeconomic" dominate output-to-credit studies, "monetary" and "supply" appear in credit-to-output studies, while both causalities emphasize "credit", "prices" and "shocks". "Islamic" surfaces in the output-to-credit-shock word cloud because several studies compare Islamic with conventional banking; yet in collecting impulse-response functions, we omit Islamic-banking IRFs.

From the sample of 68 studies, we extracted 2,629 point estimates (semi-elasticities) reported in 339 impulse response functions. These semi-elasticities capture how credit responds to output shocks and how output responds to credit shocks across multiple horizons. Specifically, we collected responses at 0, 1, 2, 4, 6, 8, 12, and 16 quarters, as well as at the maximum horizon reported in each study, to reflect both short-term dynamics and longer-run effects. For each horizon, we also recorded the corresponding confidence intervals.

To extract the numerical values from impulse response graphs, we used a web-based tool that allowed precise measurement of pixel coordinates. Each point estimate was independently verified by at least one other researcher to minimize recording errors and ensure the accuracy of the final dataset.

Our dataset distinguishes between two common transformations of credit and output: growth rates and log-levels. This distinction is crucial for standardizing the semi-elasticities and interpreting the effects. About 70% of the estimates capture how output responds to credit shocks, while the remaining 30% show how credit responds to output shocks. The split between growth rate and log-level transformations is roughly even. We standardize all estimates to reflect the response to a one percentage point or one percent increase in the shock variable and calculate standard errors from the reported confidence intervals. In this context, a response in log-levels represents a percentage change in the response variable following a 1% change in the shock variable, whereas a response in growth rates indicates a percentage point change after a 1 percentage point change in the shock variable.

3.2 Stylized Facts

We divided our responses into three groups: short-, medium-, and long-term horizons. 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. This grouping approach aligns with common practices in the empirical literature, particularly in studies examining the dynamic effects of macroeconomic shocks, such as monetary policy shocks (Christiano, 1999; Ramey, 2016). The primary motivation for this grouping is to increase the number of observations for estimation and to facilitate the economic interpretation of effects. Similar to monetary policy studies, where the impacts of interest rate changes differ across short-, medium-, and long-term horizons, the responses of both credit and output to shocks are expected to vary in strength over time.

Tables 2 and 3 offer a detailed overview of effect sizes across different horizons, presenting both simple unweighted statistics and those weighted by the inverse of the number of estimates collected from each study. Since the number of impulse response functions we extracted from each study varies from 1 to 26, with an average of 4, the weighted statistics ensure a more balanced initial view of the average effect size.

Table 2: Summary Statistics of Collected Semi-Elasticities: Credit-to-Output Shock

				Unweighted				Weighted			
Horizon		Obs.	Studies	Mean	5%	95%	Skew.	Mean	5%	95%	Skew.
Panel A: Growth Rate Transformation											
Short-term	1	107	28	0.654	-0.185	2.410	3.049	0.658	-0.313	6.200	3.059
	2	108	29	0.492	-0.340	1.512	3.260	0.525	-0.423	5.171	2.968
Medium-term	4	108	29	0.308	-0.310	1.497	1.589	0.289	-0.472	1.176	1.336
	6	108	29	0.198	-0.647	1.631	0.973	0.093	-1.370	1.333	-0.046
	8	108	29	0.160	-0.624	1.451	0.837	-0.001	-1.703	1.118	-0.350
Long-term	12	95	24	0.283	-0.320	1.285	2.567	0.081	-1.370	0.936	2.132
	16	91	21	0.293	-0.443	1.310	3.503	0.171	-0.305	0.956	4.628
	Max	86	18	0.275	-0.428	1.133	3.531	0.176	-0.397	0.963	4.324
Panel B: Log-Level Transformation											
Short-term	1	114	29	0.209	-0.214	0.988	0.888	0.257	-0.054	0.983	0.798
	2	114	29	0.240	-0.190	1.091	0.860	0.284	-0.141	1.067	0.757
Medium-term	4	117	30	0.290	-0.245	1.364	1.113	0.271	-0.130	1.353	1.204
	6	117	30	0.314	-0.209	1.539	1.684	0.244	-0.228	1.438	1.992
	8	117	30	0.322	-0.299	1.658	1.783	0.204	-0.307	1.570	2.175
Long-term	12	96	28	0.412	-0.400	2.000	1.424	0.135	-0.600	1.244	1.818
	16	93	27	0.472	-0.302	2.325	1.471	0.147	-0.403	1.375	2.596
	Max	76	24	0.476	-0.197	2.777	1.710	0.127	-0.143	1.125	3.671

Note: The table presents summary statistics of the collected semi-elasticities on the response of output growth to one-percentage-point increase of credit growth (Panel A) and the response of log-level of output to a one-percent change in credit (Panel B). The responses of output are in percent, while the responses of output 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. Therefore, responses at each study's maximum horizon were compiled into a "Max" category.

We have identified several key characteristics of the effect sizes. Firstly, the relationship between credit and output is strongly positive, regardless of variable transformation and the direction of causality. The response of output to credit shocks peaks at shorter horizons, whereas the response of credit to output shocks reaches its peak at significantly longer horizons. The most pronounced positive response in credit occurs at the maximum horizon, which averages six and a half years. Consequently, we can consider the response of credit to output shocks to be highly persistent and long-lasting.

Table 3: Summary Statistics of Collected Semi-Elasticities: Output-to-Credit Shock

Horizon		Obs.	Studies	Unweighted Mean	Unweighted 5%	Unweighted 95%	Unweighted Skew.	Weighted Mean	Weighted 5%	Weighted 95%	Weighted Skew.
Panel A: Growth Rate Transformation											
Short-term	1	47	13	0.383	-2.689	2.858	-0.331	0.532	-2.783	3.408	-0.245
	2	49	15	0.800	-1.064	3.621	0.411	0.696	-1.406	4.160	0.459
Medium-term	4	49	15	1.492	-1.241	4.847	0.561	0.960	-1.448	3.398	0.367
	6	49	15	1.620	-1.057	5.384	0.583	0.996	-1.788	4.761	0.606
	8	49	15	1.339	-0.896	5.363	0.675	0.820	-2.197	5.380	0.874
Long-term	12	41	12	1.224	-1.386	6.461	1.648	0.917	-1.963	6.461	1.856
	16	35	9	1.283	-1.371	7.065	1.526	1.260	-0.215	7.923	1.908
	Max	35	9	1.119	-1.209	5.987	1.472	1.045	-0.623	6.139	1.898
Panel B: Log-Level Transformation											
Short-term	1	61	14	0.198	-0.201	0.845	0.677	0.238	-0.417	1.141	0.574
	2	61	14	0.281	-0.043	0.992	1.012	0.385	-0.470	1.205	0.287
Medium-term	4	64	15	0.388	-0.146	1.619	1.117	0.581	-0.494	1.964	0.444
	6	64	15	0.443	-0.243	2.168	1.227	0.700	-0.299	2.393	0.536
	8	64	15	0.457	-0.192	2.270	1.324	0.794	-0.226	2.844	0.641
Long-term	12	46	13	0.584	-0.775	2.243	0.898	0.922	-1.113	3.485	0.510
	16	30	11	0.477	-0.564	2.488	1.128	0.843	-0.699	3.132	0.711
	Max	28	11	0.580	-0.436	2.473	1.142	0.851	-0.752	2.486	0.499

Note: The table presents summary statistics of the collected semi-elasticities on the response of credit growth to one-percentage-point increase of output growth (Panel A) and the response of log-level of credit to a one-percent change in output (Panel B). The responses of credit are in percent, while the responses of output 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. Therefore, responses at each study's maximum horizon were compiled into a "Max" category.

Secondly, the distribution of effect sizes is positively skewed. Across both relationships and variable transformations, the distribution becomes more asymmetrical at longer horizons. Specifically, the distribution of response of output to credit shock is more skewed than that of the opposite causality.

Thirdly, the majority of estimates fall within a relatively narrow range: across all horizons, approximately 85% of effect sizes lie within one standard deviation of the mean, suggesting that large responses of credit or output to shocks are rare. For the log-level transformation of credit and output, values typically range between -0.5% and 2%. For the growth rate transformation of both responses, the range is generally between -0.3 p.p. and 3 p.p.[5] The positive and long-lasting responses of outstanding credit and credit growth to output shocks have different interpretations.[6] In the latter case, the credit level not only increases in response to the shock, but its growth trajectory is also enhanced for a longer period. While the economy might absorb a one-time rise in credit levels over time, a persistent increase in credit growth can signal either a prolonged boost to

[5] There are only a few large responses, with the most significant being 10 pp response of credit and 6 pp response of output in the growth rate transformation. For the log-level of both variables, the maximum response reaches less than 4%.
[6] The vast majority of the studies use outstanding loans. Only one study (12 semi-elasticities extracted from 2 impulse response functions) refers to new credit. These responses do not present an outlier compared to the rest of the sample.

the economy and increased entrepreneurial activity or excessive lending and an unhealthy credit boom.

We explore several potential explanations for the persistent credit responses to output shocks. Detailed results, including summary tables and charts, are provided in Appendices A.2 to A.4.

First, one concern is that estimates at the maximum horizon may be biased if primary studies report impulse responses only until they converge to zero, implicitly assuming no further effects. This suggests that effects at longer horizons could be omitted under the assumption that they equate to zero. However, many studies in our sample report horizons of at least four to five years, with about half extending to six years or more. We find that the response of output to credit shocks typically fades to zero regardless of the reported horizon, whereas the response of credit to output shocks remains strong and persistent even at longer horizons.

Second, we compared studies using nominal versus real credit measures to check whether inflation might artificially inflate the persistence. While effects are somewhat weaker for real credit, the overall trajectories are remarkably similar, and the long-lasting pattern of credit responses is still evident. Moreover, studies using nominal variables typically include controls for prices, partly addressing this concern.

Finally, we examined whether the main analytical focus of the primary studies influenced the results. Studies explicitly aimed at estimating the response of credit to output shocks tend to report larger and more statistically significant effects than those where this relationship is only a secondary outcome. This suggests that studies concentrating on a specific channel might emphasize or better identify these effects.

Taken together, these checks indicate that the persistent credit response to output shocks is not driven solely by reporting horizons, inflation effects, or study focus, but appears to be a consistent feature across a wide range of data and model specifications. In the following sections, we further examine whether these patterns hold after accounting for publication bias and exploring study-specific heterogeneity.

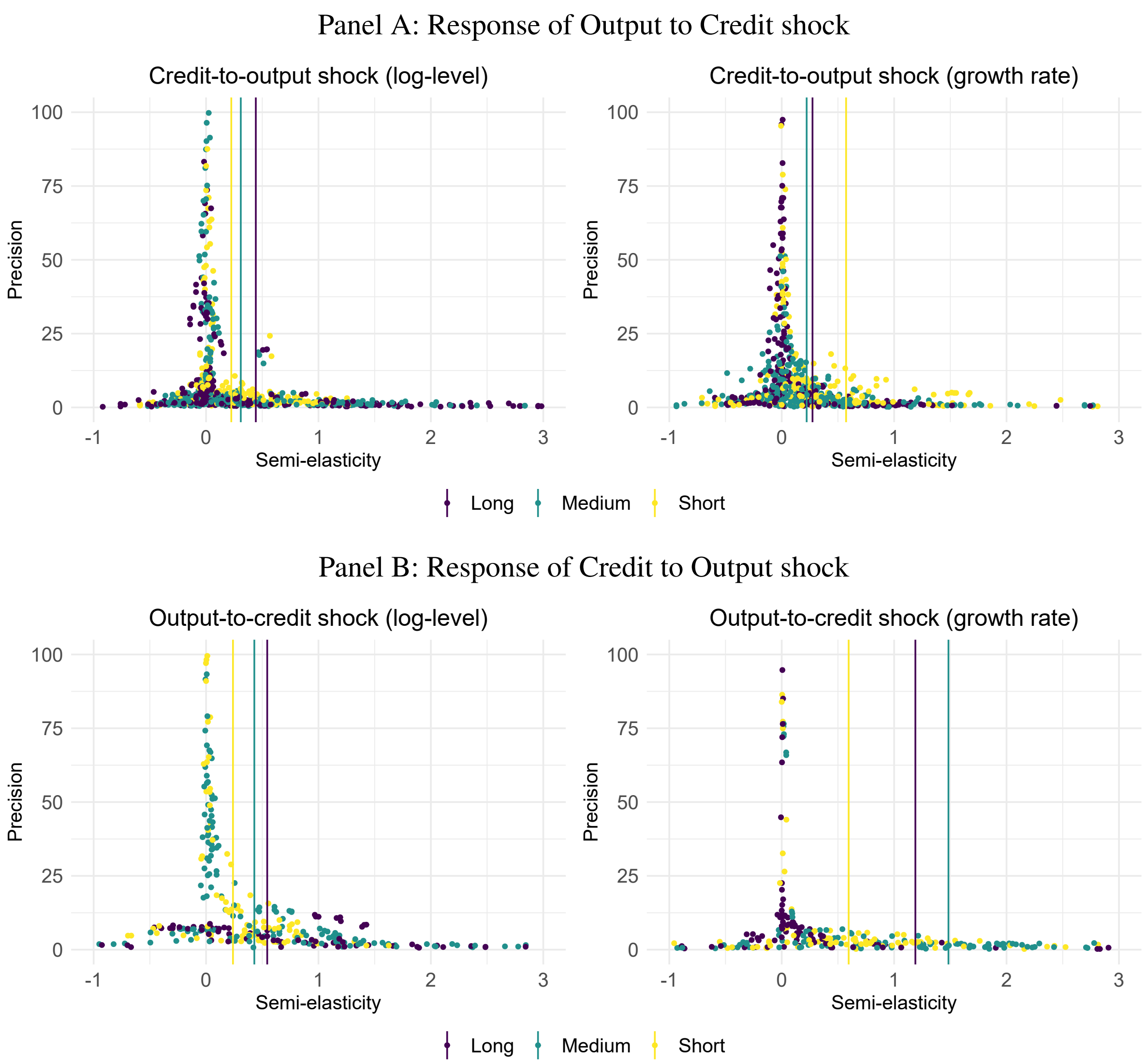
4. Publication Bias

Publication bias refers to the systematic difference between the distribution of results produced during research and those that are actually published. This often means that only significant results or those with the expected sign are published, leading to an exaggeration of reported estimates and a notable impact on the field of economics (Ioannidis et al., 2017; Astakhov et al., 2019; Campos et al., 2019; Gechert et al., 2022). Ignoring unlikely results can artificially inflate the reported effect size, making the average estimate larger than the "true" effect (Stanley, 2008; Stanley et al., 2010).

Mainstream literature generally expects a positive relationship between credit and output (Schumpeter, 1934; Bernanke and Gertler, 1995; King and Levine, 1993; Rajan and Zingales, 1998; Greenwood and Jovanovic, 1990). Given this expectation, researchers might disregard or question estimates that do not align with this economic logic, particularly those indicating a negative relationship. This could lead to a situation where researchers are biased towards confirming their preconceptions, especially in the face of imprecise or noisy data. This tendency can prompt researchers to "try harder" to produce results that meet these expectations, potentially skewing the findings towards more favorable outcomes (Card and Krueger, 1995; McCloskey and Ziliak, 2019).

The analysis of publication bias typically rests on the assumption that the estimated effect size and its standard error are independent. This means the observed effect size should not be influenced by its precision or by the size of the study. An independent relationship indicates no systematic bias, while any observed correlation could suggest publication bias. For instance, smaller, less precise studies often showing larger effects may indicate a bias towards publishing more significant results. In contrast, larger, more precise studies showing smaller effects are generally seen as more credible.

Figure 4: Funnel Plots of Credit-to-Output Shocks and Output-to-Credit Shocks



Note: In the absence of publication bias, the plots form inverted funnels symmetric around the most precise estimates. Estimates with a precision greater than 100 or a magnitude below -1 or above +3 are excluded from the figure for ease of exposition but are included in the statistical analyses.

The funnel plot is a graphical tool used to depict this principle. It plots effect size against precision (inversely related to the standard error), ideally creating a funnel-shaped distribution. In an unbiased scenario, more precise studies (typically larger ones) should cluster near the true effect size, and less precise studies should scatter symmetrically around this center, forming the funnel's broad base. Any asymmetry in this plot, particularly where smaller studies disproportionately show larger effects, serves as a visual indicator of potential publication bias, highlighting a preference for publishing more significant findings (Egger et al., 1997; Stanley et al., 2010).

Figure 4 presents funnel plots for all four samples, covering both shocks and variable transformations, with different response horizons highlighted in various colors. The vertical lines indicate the average effect size for each horizon group. Notably, the distribution is shifted to the right across the horizons. Ideally, if the true effect equaled the simple mean of the collected effects, the most precise estimates would cluster near these vertical lines. However, the rightward shift and the presence of fat tails suggest publication bias, with smaller studies – those with larger standard errors – reporting higher effect sizes and possibly excluding contrary results.

While publication bias is a concern, significant variability in effect sizes could also explain this pattern. Such differences may stem from variations in data and methods among smaller studies. We will formally correct for publication bias in this section and will investigate the drivers of heterogeneity in Section 5.

4.1 Methods

The literature provides several empirical methods to identify and adjust for publication bias, often based on the assumption that the estimated effect size and its standard error are independent. A well-established method is the Precision-Effect Estimate with Standard Error (PEESE), which is recommended when the effect size is believed to be non-zero.[7]

The PEESE method adjusts the usual linear relationship between effect size and its standard error by squaring the standard error. This adjustment is based on the observation that smaller studies, which often have larger standard errors, tend to report exaggerated effect sizes. The PEESE approach is increasingly used in economic and business meta-analyses (see, e.g., Costa-Font et al., 2011; Haelermans and Borghans, 2012; Doucouliagos et al., 2014; Zigraiova et al., 2021). The model is represented by the equation:

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

where the coefficients α and γ stand for the effect beyond bias and the intensity of publication bias, respectively.

In line with best practices in meta-analysis (see, e.g., Dettori et al., 2022; Xue et al., 2022; Ma et al., 2023), we apply a three-level (3L) model to estimate this equation. This model captures variability in effect sizes within and between studies and introduces a third level to address heterogeneity within clusters of studies. This additional layer helps account for differences in primary study characteristics, such as data or methods:

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

where δ_j is a random effect for between-study variability, $\varepsilon_{i,j,k}$ accounts for within-study error (random error), and $\kappa_{j,k}$ is a random effect for between-study variability within cluster k. All effects

[7] 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).

are assumed to be normally distributed with zero mean. The model is estimated with weights equal to the inverse of the estimate's variance in order to control for heteroskedasticity (Stanley et al., 2013; Stanley, 2005).

Another common estimation method for equations (2) is the random effects model. The main difference between this model and the three-level model is in their assumptions about the distribution of effect sizes across studies and potentially across other levels of variation. The random effects model assumes that variation in effect sizes comes from only two sources: within studies and between studies.

In presenting and interpreting publication bias results, we prefer PEESE over PET because we believe that credit/output shocks have non-zero effects on output/credit. Additionally, we favor the three-level model for its ability to handle the considerable variability within individual estimates and across studies. For context, however, we also provide PET estimates and those from the random effects model in Appendix B.1, as these are commonly referenced in the literature.

4.2 Results

Table 4 shows the results of the three-level PEESE model, which identifies publication bias across all four samples, including both shocks and transformations. The results confirm that the literature typically overestimates the average effect at the majority of response horizons. However, even after correcting for this bias, the relationship between credit and output remains positive and statistically significant, although the persistence of the shocks' effects differs depending on the direction of the causality.

For credit shocks to output, publication bias is evident at every horizon and in both variable transformations. After adjusting for this bias, the largest effects emerge at short horizons and taper off over time. In the log-level studies, the bias-corrected response of output falls from about 0.14% at short horizons to roughly 0.05% at long horizons after a 1% increase in credit. In growth-rate studies, the corresponding response drops from about 0.2 pp at short horizons to a value that is statistically indistinguishable from zero at long horizons after a one-percentage-point rise in credit growth.

The pattern reverses for output shocks to credit. Here, publication bias is concentrated in the short run, while bias-adjusted effects strengthen at medium and long horizons, signalling a persistent credit response. In log-level specifications, the bias-corrected response of credit climbs from virtually zero at short horizons to about 0.8% at long horizons following a 1% increase in output. In growth-rate specifications, a one-percentage-point rise in output boosts credit by roughly 0.4 pp in the short run and by around 0.7 pp. in the long run.[8] Additionally, we re-estimated the model for individual response horizons (quarters), rather than grouping them into short-, medium-, and long-term categories. Estimating the model horizon by horizon (Figure 5) yields the same qualitative results.

[8] Comparing the PEESE and PET models, PET emphasizes publication bias more, often adjusting the effect size nearly to zero (Tables B2–B5 in the Appendix). As discussed above, this tendency was noted by Stanley and Doucouliagos (2014), who therefore recommends to use PEESE when expecting a non-zero effect. Additionally, the random effects model gives more weight to publication bias than the three-level model most likely due to its inability to adequately account for variance between effect sizes, leading to an overestimation of bias.

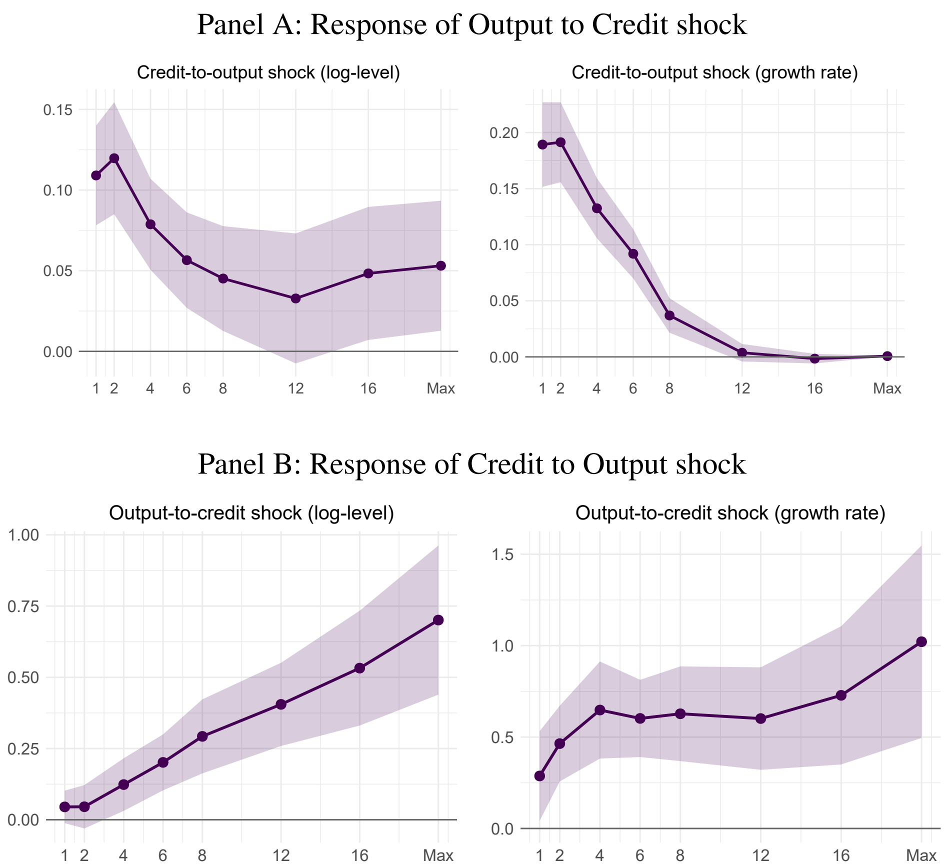
Table 4: Estimation of Publication Bias

	Short	Medium	Long
Panel A: Credit-to-output shock (log-levels)			
Effect beyond bias (const.)	0.141***	0.091***	0.058**
	(0.026)	(0.022)	(0.028)
Publication bias (SE^2)	0.495***	0.475***	0.290***
	(0.167)	(0.081)	(0.053)
I^2 level 1 (%)	0	0	0
I^2 level 2 (%)	0	0	0
I^2 level 3 (%)	100	100	100
Observations	228	351	270
Studies	29	30	28
Panel B: Credit-to-output shock (growth rate)			
Effect beyond bias (const.)	0.185***	0.092***	0.008
	(0.027)	(0.016)	(0.006)
Publication bias (SE^2)	0.444***	0.095**	0.239***
	(0.098)	(0.048)	(0.054)
I^2 level 1 (%)	0	26	11
I^2 level 2 (%)	39	18	5
I^2 level 3 (%)	61	56	84
Observations	215	324	273
Studies	29	29	24
Panel C: Output-to-credit shock (log-levels)			
Effect beyond bias (const.)	0.047	0.358**	0.778***
	(0.056)	(0.145)	(0.226)
Publication bias (SE^2)	3.949***	1.709*	0.112
	(1.012)	(0.877)	(0.562)
I^2 level 1 (%)	0	0	0
I^2 level 2 (%)	0	0	0
I^2 level 3 (%)	100	100	100
Observations	122	192	106
Studies	14	15	13
Panel D: Output-to-credit shock (growth rate)			
Effect beyond bias (const.)	0.464**	0.585***	0.738*
	(0.207)	(0.184)	(0.407)
Publication bias (SE^2)	0.111	0.400**	0.036
	(0.138)	(0.169)	(0.208)
I^2 level 1 (%)	0	0	0
I^2 level 2 (%)	43	3	0
I^2 level 3 (%)	56	97	100
Observations	96	147	112
Studies	15	15	12

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). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The analysis also highlights sources of heterogeneity as measured by the I^2 statistic, which quantifies the proportion of total variance due to heterogeneity across three levels: sampling error (level 1), within-study differences (level 2), and between-study differences (level 3). In our findings, the majority of the variance is attributed to differences between studies, particularly with the log-level transformation showing 100% variance from this source. In the growth rate transformation, both within-study differences and sampling errors are also influential at some horizons. However, the main heterogeneity in all datasets stems from the between-study differences, reflecting distinct estimation techniques and data sources of the primary studies. The substantial heterogeneity between studies further points to why the mean effects beyond bias are considerably weaker than the sample means. Further discussion on this heterogeneity and its implications for specific subsets of studies will follow in the next section.

Figure 5: Mean Impulse Response Functions After Correction for Publication Bias: Individual Horizons



Note: The figure shows the average effect beyond bias (corrected for publication bias) of output and credit after a one-percentage-point (growth rate transformation) or one-percent (log-level transformation) increase in the opposite variable, along with the average 68% confidence interval. The horizons are in quarters. The "beyond bias" estimates come from a three-level meta-analysis using Precision-Effect Estimate with Standard Error (PEESE), with standard errors clustered at the study level and weights equal to the inverse of each estimate's variance. Because some samples include few observations, estimates at certain horizons (e.g., the maximum horizon) combine data from adjacent horizons.

4.3 Additional Tests of Publication Bias

Thus far, we have found that publication bias inflates effects for both causalities and most horizons, which 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.

We start by examining the distribution of t-statistics, focusing on clustering around significance thresholds. In the absence of publication bias, t-statistics should follow an approximately normal distribution. Two likely causes for a non-normal distribution of t-statistics around zero are: a preference for positive estimates in both directions, and a tendency to publish statistically significant results.

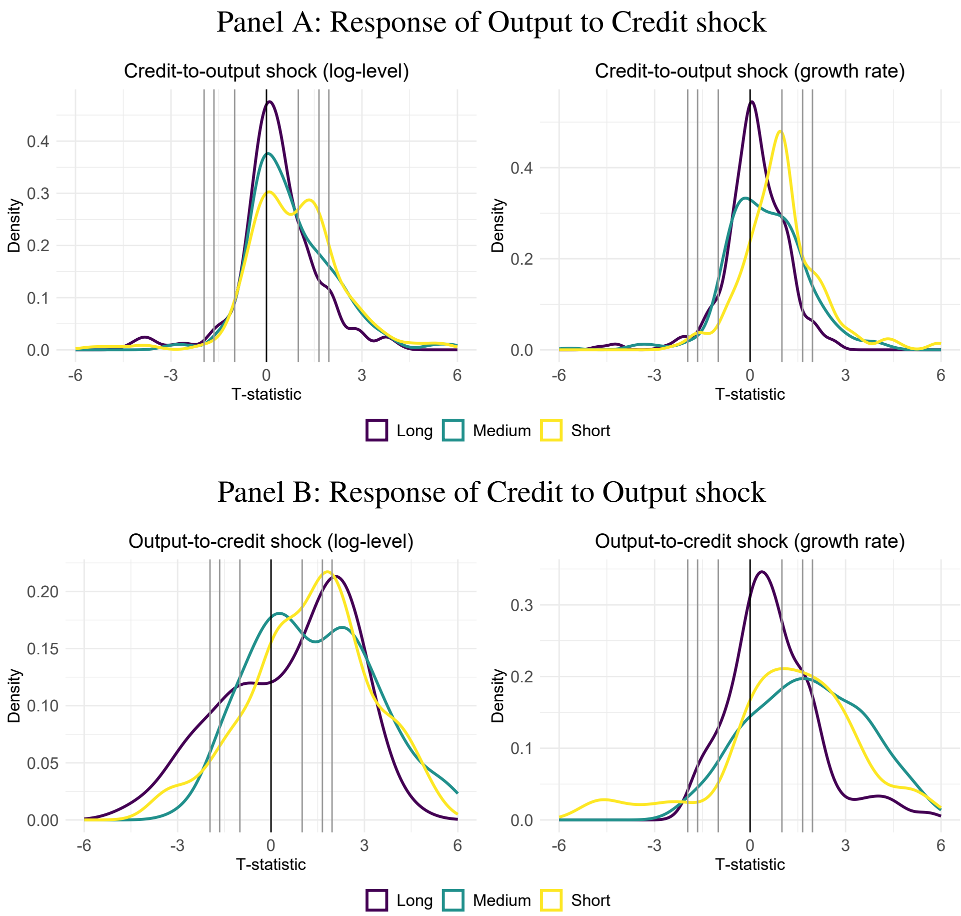
Figure 6 shows densities of the t-statistics for both transformations and both relationships, with vertical lines marking critical values for 5%, 10%, and 32% statistical significance. Looking at the credit-to-output shock relationship in Panel A, positive t-statistics near critical significance values appear over-represented. In both transformations and across all horizons, a hump is seen near 2, corresponding to critical values for 10% and 5% significance, and also around 1 for 32% significance – critical value commonly used in Bayesian estimates.

For the output-to-credit shock relationship (Panel B), similar patterns are evident. The distribution is centered around 2, with negative and non-significant values underrepresented. Unlike the credit-to-output shock relationship, values are not as often centered around 1. This may be explained by the fact that only 12% of estimates in the growth rate transformation of the output-to-credit shock relationship are Bayesian, compared to more than 28% for the credit-to-output shock. Worth noting are the positive and comparably large t-statistics for both transformations at medium and long horizons, indicating that the output-to-credit shock is more persistent compared to the credit-to-output shock.

Next, recent literature introduces a range of novel estimation methods that further relax the linearity assumption. These approaches are based on the idea that more precise estimates are less likely to suffer from publication bias. Accordingly, they focus on isolating the most precise estimates to compute an unbiased average effect (see, e.g., Stanley et al., 2010; Ioannidis et al., 2017; Furukawa, 2019; Bom and Rachinger, 2019; Andrews and Petroulakis, 2019).[9] While useful in many applications, this strategy may not always be appropriate – particularly in our context – due to substantial heterogeneity in the underlying studies. Most of these methods apply a precision threshold, either exogenously or endogenously determined, below which estimates are excluded. As a result, many estimates with larger standard errors are omitted, even though these may reflect genuine differences in study design or data quality rather than selection bias alone.

[9] 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 uses it to correct for publication bias. The underlying intuition involves jumps in publication probability at conventional p-value cut-offs.

Figure 6: Distribution of T-statistics: Density Plots



Note: The vertical lines denote the critical value associated with 5%, 10% and 32% statistical significance. In the absence of publication bias, the distribution of the t-statistics should be approximately normal.

Nevertheless, we present results of many of these techniques in Appendix B.1.1 and they generally support the baseline results. For the credit-to-output shock relationship, the results show small but mostly significant effects at short and medium horizons, while no effects are observed at longer horizons. In contrast, for the output-to-credit shock relationship, the effects are consistently larger and more significant, particularly at medium and long horizons, highlighting a stronger and more persistent transmission mechanism.

Furthermore, our estimation framework assumes that the standard error is exogenous. However, this assumption may not always hold, as publication bias can influence both point estimates and standard errors, which are calculated differently across studies. Stanley (2005) notes that because standard errors are themselves estimated, effect sizes may be subject to attenuation bias.

To assess the validity of the exogeneity assumption, we employ two additional approaches. First, we implement the Caliper test, a method designed to be robust to standard error endogeneity (Gerber and Malhotra, 2008a,b). This test evaluates whether t-statistics cluster just above conventional significance thresholds – such as 1.65 for the 10% level or 1.96 for the 5% level – which would suggest selective reporting. In the absence of publication bias, we would expect a symmetric distribution of t-statistics around these cutoffs. We run the test using three thresholds (1.00, 1.65, 1.96) and three caliper widths (0.1, 0.2, 0.3) to define the region around each threshold.

The results, presented in Appendix B.3, indicate signs of publication bias, especially in the credit-to-output shock relationship at short and medium horizons, where t-statistics are disproportionately concentrated just above the critical values. By contrast, the output-to-credit relationship shows a more balanced distribution, with significant responses more prevalent at longer horizons.

Second, we follow recent meta-analytic studies that recommend the use of instrumental variable (IV) estimators to mitigate both publication bias and endogeneity. Specifically, we instrument the squared standard error using the number of countries in each study or its inverse transformation $1/\sqrt{n}$. As shown in Appendix B.2, IV estimates reinforce our baseline findings: the bias-corrected effects of credit-to-output shocks are limited to the short term, whereas output-to-credit shocks yield larger and more persistent responses at medium and long horizons. This pattern is particularly pronounced in log-level specifications. Growth-rate estimates, while directionally consistent, are less precise – likely due to the smaller number of available observations.

5. Drivers of Heterogeneity

In the previous section, we observed notable variations across different studies, which cannot be solely attributed to publication bias. Typically, in meta-analyses aimed at estimating a structural parameter from a model accurately reflecting the data-generating process, observed heterogeneity is minimal. However, when the focus is on a reduced-form parameter or the employed model fails to accurately capture the data-generating mechanism, we witness a significant increase in the variability of the estimates. This variability is impacted by the specifics of the model and the data's characteristics. In this section, we will explore the main study characteristics to describe the roots of this heterogeneity.

Before turning to the formal model-based analysis, we first examine whether heterogeneity in elasticities can be explained by simple patterns across studies. We consider variation by authorship, country coverage, time period, and publication year. While certain differences emerge – such as greater dispersion in regional aggregates or smaller growth-rate elasticities in more recent studies – these patterns are not sufficiently consistent to explain the observed heterogeneity. This is likely due to the fact that many study characteristics co-vary, making it difficult to isolate their effects using descriptive methods alone. To address this limitation, we proceed with a multivariate approach that accounts for the joint influence of study-level factors. Results of the descriptive analysis, including summary statistics, average impulse responses, and boxplots, are available upon request.

5.1 Study Characteristics

We collected information on the data characteristics, model specification and estimation methods, and publication details of each study to better understand the discrepancies between them. A list of all variables is provided in Tables 5, while summary statistics can be found in Tables C1–C4 in the Appendix.

In terms of *data characteristics*, we collect and dummy-code information on the type of credit examined, distinguishing between household and firm credit from the borrower's perspective. Household credit primarily affects consumption (Mian and Sufi, 2018), while firm credit influences investment (Braun and Larrain, 2005); both play key roles in driving the business cycle (Gertler and Kiyotaki, 2010). At the same time, macroeconomic conditions – such as income, employment, and interest rates – affect all types of credit (Dell'Ariccia and Garibaldi, 2005). We also account for the geographical focus of each study, differentiating between those centered on North America, Europe, and the rest of the world (including mixed-country panels). This distinction may shed light on how regional structural differences shape the credit–output relationship over time, especially given the global relevance of credit markets (Imbs, 2010; Cetorelli and Goldberg, 2011; Bruno and Shin, 2015; Eickmeier and Ng, 2015; Cerutti et al., 2017b). In addition, we dummy-code whether a study uses real and seasonally adjusted credit data, and whether it relies on lower-frequency observations. Finally, we control for the length of the primary dataset (i.e., the number of years covered) and its breadth (i.e., the number of countries included).

Table 5: Primary Study Characteristics – Variable Definitions

Variable	Definition
Semi-Elasticity	The reported effect of a one-percentage-point increase in credit on output or vice versa
SE2	The reported or implied squared standard error of the estimate.
Data characteristics	
Corporate Credit	= 1 if corporate credit is used
Real Output/Credit	= 1 if real instead of nominal credit/output is used
SA Output/Credit	= 1 if credit/output is seasonally adjusted
Study Length (Y)	The logarithm of the length of the data sample used in the primary study (in years).
Multiple Countr.	= 1 if the data are from two or more countries
North America	= 1 if the study covers a country or group of countries from North America
Europe	= 1 if the study covers a country or group of countries from Europe
Model specification and estimation characteristics	
Controls: House Prices	= 1 if house prices are included
Controls: Short IR	= 1 if the short-term interest rate is included
Controls: Long IR	= 1 if the long-term interest rate is included
Sign Rest. Ident.	= 1 if sign restrictions are used in the VAR model
No. of Lags	number of lags used in the VAR model
Simple VAR	= 1 if the model is simple VAR
Bayesian Est.	= 1 if the model is Bayesian VAR
Publication characteristics	
Published	= 1 if the study is published in a peer-reviewed journal.
Pub. Year	Year of the publication of the primary study
Impact Factor	The recursive discounted RePEc impact factor of the outlet.
Citations	Logarithm of the number of Google Scholar citations normalized by the number of years since the publication year plus one
Main Focus	= 1 if the main focus of the primary study is the relationship between credit and output and/or vice versa

Regarding *model specification and estimation*, we control for the type of model used – distinguishing between simple VARs and other specifications, including panel VARs and non-linear VARs – as well as the method of shock identification (e.g., sign restrictions versus other approaches, primarily Cholesky decomposition). We also account for the estimation technique, differentiating between Bayesian and frequentist methods. In addition, we include controls for the number of lags and the inclusion of specific variables in the VAR model, such as house prices and short- and long-term interest rates. Numerous previous meta-analyses in economics have demonstrated that both model specification and the chosen estimation method significantly contribute to the heterogeneity of results across studies (Rusnák et al., 2013; Zigraiova et al., 2021; Ehrenbergerova et al., 2023; Malovaná et al., 2025, 2024).

To account for the influence of the article's *publication characteristics*, we include five additional variables. First, we capture the number of Google Scholar citations, which serves as a proxy for the study's influence (Bjork et al., 2014). We also include a dummy indicating whether the study was published in a peer-reviewed journal, the year of publication, and the RePEc discounted recursive impact factor of the publishing outlet. Drawing on evidence from prior meta-analyses (Valickova et al., 2015; Araujo et al., 2024; Bajzik et al., 2020), we examine how these characteristics affect the estimated effect size and direction. Additionally, since we recorded the primary focus of each study, we incorporate this information into the heterogeneity analysis – acknowledging that researchers may approach the credit – output relationship with greater rigor when it is the central topic of their study.

5.2 Methodology

In our analysis, we rely on Bayesian model averaging (BMA), a widely recognized method in economics and meta-analyses (Fernandez et al., 2001; Havranek and Rusnak, 2013; Moeltner and Woodward, 2009; Havránek et al., 2020). This approach addresses the issue of model uncertainty that arises when working with a large number of explanatory variables. Including all variables in a single model would lead to overfitting and lack of parsimony. Instead of using a standard OLS regression, we employ BMA to estimate many regressions with different combinations of variables and then compute a weighted average of these models (Havranek et al., 2015).

To handle the vast number of potential model combinations, we apply the Markov chain Monte Carlo (MCMC) method with the Metropolis-Hastings algorithm. This process efficiently identifies the most likely models (Zeugner and Feldkircher, 2015). Each model's likelihood is used to calculate a posterior model probability (PMP), which serves as a weight. The posterior inclusion probability (PIP) for each variable is then derived by summing the PMPs of all models in which the variable appears.

A key challenge in BMA is specifying priors for the regression coefficients (g-priors) and the models themselves (model priors). For our baseline analysis, we follow Eicher et al. (2011) and use a unit information g-prior (UIP) and a uniform model prior, reflecting minimal prior knowledge about parameter probabilities. To ensure the robustness of our findings, we also test alternative priors in sensitivity analyses.

We employ several robustness checks to ensure the reliability of our results. First, we use the dilution prior proposed by George (2010), which adjusts model probabilities based on the determinant of the correlation matrix of the included variables. This approach assigns lower weights to models with highly correlated variables and higher weights to models with low correlation. Additionally, we apply the Hannan-Quinn (HQ) g-prior to account for data quality and pair it with a random model prior (Fernandez et al., 2001; Stanley et al., 2010; Feldkircher and Zeugner, 2012; Zigraiova et al., 2021). We also test the BRIC g-prior, which minimizes the influence of priors on results, alongside the random model prior that assigns equal probability to all model sizes (Zeugner and Feldkircher, 2015; Gechert et al., 2022).

5.3 Results

This section presents the BMA results for all three horizons across both transformations (growth rate and log-level) and relationships (credit-to-output and output-to-credit shocks). In all BMA regressions, we additionally include a dummy variable called *Large SE*, which equals 1 for elasticities falling into the top 10% of standard errors. This approach addresses the possibility that very large standard errors may signal higher uncertainty or lower data quality. Including *Large SE* thus helps account for heterogeneity in precision and prevents these potentially imprecise observations from unduly affecting the overall results.[10]

We focus on the log-level transformation of credit-to-output shocks (Table C5, Figure C1) while referencing results for the growth rate transformation (Table C6, Figure C2) in the Appendix. For output-to-credit shocks, we highlight key differences, with detailed results for the log-level (Table C7, Figure C3) and growth rate transformations (Table C8, Figure C4). Following Eicher et al. (2011), we interpret variables with a posterior inclusion probability (PIP) above 0.5, classifying them as decisive (PIP > 0.99), strong (0.95–0.99), substantial (0.75–0.95), or weak (0.5–0.75).

Credit-to-output shocks: Our analysis confirms the presence of publication bias in credit-to-output shocks across all horizons, even when accounting for primary study characteristics in the multivariate BMA setting. This is reflected in the *SE2* variable, which represents the squared standard error. Compared to the univariate regressions in Section 4.2, publication bias appears slightly higher, with *SE2* coefficients around 0.7 for short horizons and 0.4 for medium and long horizons, each with PIP values near 1. For growth rate transformations, *SE2* coefficients consistently have a PIP of 1, and the bias increases at longer horizons. However, to fully capture the overall effect of publication bias, the interaction term *Large SE * SE2* must be also included. It generally reduces bias, particularly at the medium horizon in growth rate studies.

Several characteristics of the primary studies significantly influence the positive shocks from credit to output. For the log-level transformation, at short horizons, the positive impact of credit on output decreases when output is measured in real terms (as discussed in Section 3). Conversely, when credit is measured in real terms, its impact on output is higher, especially at medium to long horizons.[11] The effect of credit on output is also stronger in Europe and, to a lesser extent (with a weaker PIP), in North America (primarily studies on U.S. data), compared to other regions. This likely reflects greater financial depth and higher levels of financial development in these economies, enabling more efficient credit allocation and greater responsiveness of output to credit changes. Furthermore, higher financial integration and stability in these regions amplify the credit-to-output impact by better supporting sustained economic growth.

Studies employing sign restriction identification (as opposed to Cholesky identification), Bayesian methods, and more recent studies with higher citation counts tend to report larger effects of credit shocks on output. Bayesian methods show stronger effects primarily at short and medium horizons in log-level studies, likely due to their ability to incorporate prior information and effectively capture immediate and medium-term dynamics, though they may be less precise in forecasting

[10] Interested readers can further assess the total effect of standard errors by summing up the relevant coefficients in our regressions. We follow a similar strategy to Opatrny et al. (2023), who interacts standard errors with additional variables to manage heterogeneity.

[11] This may be a mechanical effect: when credit is measured in real terms (deflated by an inflation index), a 1 percentage point increase reflects a genuine rise in credit volume. In contrast, a 1 percentage point increase in nominal credit includes both credit volume growth and price increases.

long-term effects due to greater uncertainty and model complexity. Additionally, sign restrictions, which impose constraints aligned with economic theory at short horizons, may amplify observed credit-to-output shocks. In growth rate studies, sign restrictions show a positive effect at short horizons but a negative and significant effect at medium and long horizons. This could occur because constraints overestimate short-term effects, followed by a correction or reversal as the model accounts for extended dynamics (Kilian and Murphy, 2012).

Figures C5 and C6 present prior sensitivity analysis results across short, medium, and long horizons for Bayesian Model Averaging (BMA) applied to credit-to-output shock studies. The analysis compares prior settings, including UIP with Uniform model prior (baseline), UIP with Dilution model prior, HQ with Random model prior, and BRIC with Random model prior. Key variables, particularly *SE2*, consistently exhibit high Posterior Inclusion Probabilities (PIPs) across different prior settings and horizons, demonstrating robustness and independence from specific prior choices in Bayesian Model Averaging.

Output-to-credit shocks: We confirm the presence of publication bias across all horizons for output-to-credit shocks, even in the multivariate Bayesian model averaging framework that accounts for primary study characteristics. For the log-level transformation (Table C7), the *SE2* variable reveals significant publication bias, particularly at short and medium horizons (PIP = 1). At longer horizons, the bias diminishes, reflected by a lower coefficient and a PIP of 0.61. Importantly, only when the interaction term *Large SE * SE2* is added to *SE2* is the overall publication bias effect fully captured.

In log-level studies, real credit demonstrates a negative influence on output-to-credit shocks at short and medium horizons, likely due to adjustments in credit markets responding to temporary output fluctuations. However, at long horizons, seasonally adjusted output exhibits a strong positive effect (PIP = 1), reflecting the cumulative impact of sustained economic growth on credit demand. More recent studies report larger short- and medium-term effects of output shocks on credit, but these effects diminish over time, possibly due to evolving credit market conditions or data quality improvements in newer research.

Multiple-country studies show negative effects at short and medium horizons but positive and significant effects in the long term (PIP = 1), likely driven by cross-country heterogeneity. European data-based studies display particularly strong long-term effects of output on credit, which may reflect the region's higher financial integration and well-developed credit markets that amplify the long-term relationship. Corporate credit consistently exhibits stronger effects in log-level studies, highlighting its critical role in transmitting output shocks, as firms adjust borrowing to align with long-term investment opportunities. Compared with other credit categories – particularly household and government credit – the corporate-credit response to an output shock is roughly 0.32 pp larger at short and medium horizons and about 0.13 percentage points larger at long horizons. In growth rate studies, the focus shifts to household credit, as there are only a limited number of corporate credit studies available.[12] Studies on household credit reveal delayed but positive effects at long horizons, reflecting the slower adjustment of household borrowing behavior compared to the more dynamic nature of corporate credit.

Bayesian methods, used primarily in log-level studies, show negative effects of output shocks on credit at short and medium horizons but positive impacts in the long term (PIP = 1). Interestingly,

[12] Following our inclusion criteria outlined above, corporate credit studies are grouped with other credit types as a baseline category.

this pattern contrasts with the credit-to-output relationship, where Bayesian estimates indicated positive effects at short and medium horizons. This divergence may reflect differences in how Bayesian methods handle dynamics in the two relationships. Controlling for short-term interest rates enhances effects at short horizons but reverses at longer horizons in both transformations, suggesting that monetary policy conditions could play a role in shaping these dynamics.

As in the case of the credit-to-output shocks, Figures C7 and C8 present prior sensitivity analyses for output-to-credit shocks across short, medium, and long horizons. The results confirm that key variables, particularly *SE2*, maintain high PIPs across different prior settings and horizons.

5.4 Multivariate Effect Beyond Bias

In the previous sections, we demonstrated that both credit-to-output and output-to-credit shocks are shaped by publication bias and key drivers of heterogeneity. In this section, we estimate the mean effect beyond bias, explicitly accounting for these influences.

Unlike the univariate approach used in Section 4, this analysis employs the complete meta-regression output from the BMA to account for the multivariate context, capturing the main sources of heterogeneity. Good practice in this context also includes providing a corresponding estimate of the overall mean effect, serving as a multivariate analogue to the univariate "effect beyond bias". The effect beyond bias is calculated using fitted values based on the BMA regression results, incorporating the estimated coefficients of all primary study characteristics, except for the slope coefficient on the square of the standard error (SE^2), which is set to zero to correct for publication bias. Alongside the corrected means, we report 32/68 credible intervals derived from predictive densities, reflecting the best models identified by the BMA. Both unweighted and weighted results are presented, with weights equal to the inverse of the number of estimates per study.

Table 6: Effect Beyond Bias: Credit-to-Output Shock, Log-Levels

	Short horizon	Medium horizon	Long horizon
Unweighted			
Simple mean	0.252	0.293	0.428
Corrected mean	0.139	0.156	0.243
32/68 credible intervals	(0.025, 0.120)	(0.068, 0.204)	(0.111, 0.362)
Weighted			
Simple mean	0.266	0.207	0.147
Corrected mean	0.171	0.096	0.123
32/68 credible intervals	(0.046, 0.224)	(-0.069, 0.194)	(-0.053, 0.162)
Observations	345	234	270
Studies	30	30	28

Note: The table compares the bias-corrected effect (and its credible intervals) with the simple uncorrected mean across three horizons. The bias-corrected effect is derived from a Bayesian Model Averaging (BMA) meta-regression, setting the slope on the squared standard error (SE^2) to zero to remove publication bias. Credible intervals come from predictive densities, which are mixtures of the most likely BMA models. Weighted statistics are computed using inverse-frequency weights based on the number of estimates per study. The same applies also for Tables 7– 9.

Table 6 through Table 9 summarize the effect beyond bias for the four key relationships analyzed: credit-to-output and output-to-credit shocks, for both the log-level and growth rate transformations of output and credit. The results indicate that effects corrected for publication bias are generally lower than the simple means calculated from the sample, but they remain positive. For credit-to-output shocks, the corrected effects are most pronounced at short horizons, gradually diminishing in both significance and magnitude at medium and long horizons (Table 6 and Table 7). In contrast, output-to-credit shocks demonstrate the opposite pattern: their effects are larger, more significant, and increase in magnitude at medium and long horizons (Table 8 and Table 9).

Table 7: Effect Beyond Bias: Credit-to-Output Shock, Growth Rates

	Short horizon	Medium horizon	Long horizon
Unweighted			
Simple mean	0.421	0.184	0.213
Corrected mean	0.255	0.109	0.084
32/68 credible intervals	(0.173, 0.347)	(-0.059, 0.258)	(-0.028, 0.217)
Weighted			
Simple mean	0.356	0.119	0.061
Corrected mean	0.241	0.097	0.060
32/68 credible intervals	(0.182, 0.266)	(-0.007, 0.173)	(-0.027, 0.187)
Observations	323	216	273
Studies	29	29	24

The observed patterns in credit-to-output and output-to-credit shocks highlight the asymmetric dynamics of their interactions. Output-to-credit shocks exhibit longer-lasting and more persistent effects. As hypothesized in the introduction, this persistence could be attributed to the cumulative impact of sustained economic growth, which enhances income levels, profitability, and creditworthiness. These improvements increase access to credit and the utilization of borrowing, resulting in more prolonged effects on credit dynamics.

Table 8: Effect Beyond Bias: Output-to-Credit Shock, Log-Levels

	Short horizon	Medium horizon	Long horizon
Unweighted			
Simple mean	0.292	0.453	0.520
Corrected mean	0.153	0.224	0.417
32/68 credible intervals	(0.030, 0.198)	(0.044, 0.342)	(0.077, 0.728)
Weighted			
Simple mean	0.395	0.726	0.827
Corrected mean	0.121	0.274	0.639
32/68 credible intervals	(0.026, 0.250)	(-0.014, 0.523)	(0.164, 0.872)
Observations	146	98	112
Studies	15	15	12

Table 9: Effect Beyond Bias: Output-to-Credit Shock, Growth Rates

	Short horizon	Medium horizon	Long horizon
Unweighted			
Simple mean	0.876	1.500	1.177
Corrected mean	0.482	0.860	1.165
32/68 credible intervals	(0.063, 1.296)	(0.120, 1.620)	(0.064, 1.440)
Weighted			
Simple mean	0.745	1.003	0.834
Corrected mean	0.427	0.489	0.732
32/68 credible intervals	(0.051, 0.906)	(0.002, 0.997)	(0.064, 0.852)
Observations	146	98	112
Studies	15	15	12

5.5 Implied Effect

We now turn to *implied effect*, which, like the effect beyond bias, sets ($SE^2 = 0$) to correct for publication bias. However, it also imposes a *common empirical strategy* across all primary studies. Table 10 shows the results when all studies are assumed to use the frequentist approach. Our BMA findings indicate that "Bayesian vs. frequentist" estimation is a major source of heterogeneity in reported elasticities, so removing Bayesian methods can meaningfully shift the estimates.[13] Like the effect beyond bias, the implied estimates in Table 10 are presented as bias-corrected means with 32/68 credible intervals, derived from BMA predictive densities and weighted by the inverse of each study's number of estimates.

Table 10: Implied Effect: Credit-to-Output Shock, Frequentist Approach

	Short horizon	Medium horizon	Long horizon
Credit-to-Output Shock, Log-Levels			
Corrected & Implied mean	0.118	0.023	0.122
32/68 credible intervals	(-0.035, 0.251)	(-0.082, 0.310)	(-0.015, 0.499)
Credit-to-Output Shock, Growth Rates			
Corrected & Implied mean	0.228	0.100	0.061
32/68 credible intervals	(0.015, 0.472)	(-0.069, 0.291)	(-0.216, 0.382)

Note: This table shows the mean implied effect and its credible intervals under a frequentist approach. The implied effect reflects the mean semi-elasticity if all studies followed this single common strategy. As with the bias-corrected estimates, we derive fitted values from a complete BMA meta-regression, and the credible intervals come from predictive densities blending the most likely BMA models. The weighted statistics are used, calculated using a weight equal to the inverse of the number of estimates collected per study. The same applies also for Table 11.

Comparing Table 10 with Tables 6–7 shows that forcing frequentist methods generally lowers the estimated credit-to-output elasticities, while preserving the finding that the shock is strongest at

[13] Also, we examined three leading studies for each relationship (credit-to-output shock and output-to-credit shock), identified by the journal's impact factor. We find that all three studies in both relationships use sign restrictions, and two of the three employ a frequentist approach. The relevant articles are Choi (2021); Furlanetto et al. (2019); Gambetti and Musso (2017) for the credit-to-output shock and Choi (2021); Dell'Ariccia and Garibaldi (2005); Fornari and Stracca (2012) for the output-to-credit shock. We cannot investigate the separate influence of sign identification for output-to-credit shocks because only a few studies in this category use sign restrictions. *The Sign Rest.Ident.* variable did not meet our BMA inclusion criteria, which require appearing in at least 5% of the sample, being represented in more than four studies, and having correlation below 0.8 with other variables.

short horizons. The BMA results suggest that Bayesian estimation often employs more informative priors and incorporates richer short- and medium-term dynamics, typically inflating elasticity estimates. By contrast, the frequentist approach may yield more conservative estimates because it does not embed these priors, and thus reduces the "implied" elasticity even beyond the bias-correction. Still, as before, the effect of credit on output tends to fade at medium and long horizons, indicating that methodological changes do not overturn the basic time profile of credit-to-output shocks.

For output-to-credit shocks, Table 11 reveals that frequentist-only assumptions can push implied estimates above or below the bias-corrected values, depending on the horizon. Despite these fluctuations, the tendency for more substantial output-to-credit effects at medium to long horizons (when compared to credit-to-output shocks) remains intact. This pattern aligns with our BMA results, which highlight that study characteristics – such as estimation strategy – can shift elasticity estimates without reversing the broader temporal dynamics.

Table 11: Implied Effect: Output-to-Credit Shock, Frequentist Approach

	Short horizon	Medium horizon	Long horizon
Output-to-Credit Shock, Log-Levels			
Corrected & Implied mean	0.198	0.467	0.319
32/68 credible intervals	(0.192, 0.409)	(0.423, 0.755)	(0.101, 0.596)
Output-to-Credit Shock, Growth Rates			
Corrected & Implied mean	0.579	0.494	0.586
32/68 credible intervals	(0.249, 1.278)	(0.401, 1.336)	(0.387, 1.401)

We further explore two additional high-PIP features identified by the BMA. First, imposing sign restrictions on credit-to-output studies (Table C9) raises short-horizon estimates under log-level specifications, but leads to negative implied effects at longer horizons for growth-rate data. This suggests that sign restriction identification can amplify initial shocks, possibly because it encodes theory-based constraints in the model's structure – but those effects may taper off or reverse over time once the model accounts for extended dynamics. Second, making output-to-credit the explicit *Main Focus* of a study (Table C10) has the opposite tendency, substantially boosting short- and medium-horizon estimates in log-level data and also generating pronounced long-term effects in growth-rate studies. These findings imply that when researchers specifically concentrate on output-to-credit shocks, they often report larger elasticities, a result that also correlates with the BMA evidence on *Main Focus*.

In the broader context of meta-analysis, these results reinforce that removing bias (by setting ($SE^2 = 0$) is only the first step. Methodological and thematic choices – such as Bayesian vs. frequentist estimation, sign restriction identification, or a strong focus on output-to-credit shocks – can still meaningfully raise or lower reported elasticities. Consequently, while bias-correction is crucial, the "implied effect" approach underscores that a single uniform adjustment cannot capture all the heterogeneity introduced by different modeling decisions and research priorities.

6. Conclusions

Our meta-analysis of more than 2,600 point estimates from impulse response functions reported in 68 VAR-based studies covering 63 countries offers three key insights into the credit–output nexus. First, we find that credit shocks tend to have an immediate but short-lived impact on output, whereas output shocks lead to more persistent credit responses, pointing to notable asymmetries between financial and real variables. Second, publication bias appears to inflate reported effects, though even after correcting for it, the underlying relationships remain statistically and economically relevant. Third, we document substantial heterogeneity across studies, shaped by differences in regional focus, credit types, and methodological choices – highlighting the need to consider context when interpreting empirical estimates. Taken together, these findings offer a comprehensive and data-driven perspective to support the calibration of monetary and macroprudential policies aimed at reducing cyclical vulnerabilities and preserving financial stability.

From a policy perspective, our meta-analysis consolidates decades of fragmented empirical research into a structured set of semi-elasticities that are directly applicable in practice. By aggregating and bias-adjusting results across studies, we provide robust benchmarks that can be used in stress-testing exercises, growth-at-risk frameworks, and early-warning models – capturing how credit and output respond to shocks across both short- and longer-term horizons. These calibrated responses help identify where countercyclical tools, such as capital buffers or targeted credit restrictions, may be most effective. Beyond providing an empirical baseline, our findings underline the importance of regularly revisiting these elasticities as financial systems evolve – supporting the integration of systematic evidence into forward-looking supervision and macroeconomic analysis.

Bibliography

AIKMAN, D., A. G. HALDANE, AND B. D. NELSON (2015): "Curbing the Credit Cycle." *The Economic Journal,* 125(585):1072–1109.

AKHATOVA, M., M. P. ZAINAL, AND M. H. IBRAHIM (2016): "Banking Models and Monetary Transmission Mechanisms in Malaysia: Are Islamic Banks Different?" *Economic Papers: A Journal of Applied Economics and Policy,* 35(2):169–183.

ANARI, A., J. KOLARI, S. PYNNÖNEN, AND A. SUVANTO (2002): "Further Evidence on the Credit View: The Case of Finland." *Applied Economics,* 34(3):267–278.

ANDREWS, D. AND F. PETROULAKIS (2019): "Breaking the Shackles: Zombie Firms, Weak Banks and Depressed Restructuring in Europe." ECB Working Paper No. 2240, European Central Bank.

ANWAR, A. AND I. IWASAKI (2023): "The Finance–Growth Nexus in the Middle East and Africa: A Comparative Meta-Analysis." *International Journal of Finance & Economics,* 28(4): 4655–4683.

ANWAR, A. AND I. IWASAKI (2023): "The Finance–Growth Nexus in Asia: A Meta-Analytic Approach." *Asian Development Review,* 40(1):13–48.

APERGIS, N., I. FILIPPIDIS, AND C. ECONOMIDOU (2007): "Financial Deepening and Economic Growth Linkages: A Panel Data Analysis." *Review of World Economics,* 143:179–198.

ARAUJO, J., M. PATNAM, A. POPESCU, F. VALENCIA, AND W. YAO (2024): "Effects of Macroprudential Policy: Evidence from over 6000 Estimates." *Journal of Banking & Finance,* 169:107273.

ARCAND, J. L., E. BERKES, AND U. PANIZZA (2015): "Too Much Finance?" *Journal of Economic Growth,* 20(2):105–148.

ARESTIS, P., G. CHORTAREAS, AND G. MAGKONIS (2015): "The Financial Development and Growth Nexus: A Meta-Analysis." *Journal of Economic Surveys,* 29(3):549–565.

ASSENMACHER-WESCHE, K. AND S. GERLACH (2008): "Monetary Policy, Asset Prices and Macroeconomic Conditions: A Panel-VAR Study." NBB Working Paper No. 149, National Bank of Belgium.

ASTAKHOV, A., T. HAVRANEK, AND J. NOVAK (2019): "Firm Size and Stock Returns: A Quantitative Survey." *Journal of Economic Surveys,* 33(5):1463–1492.

BAGEHOT, W. (1873): *Lombard Street: A Description of the Money Market.* HS King & Company.

BAJZIK, J., T. HAVRANEK, Z. IRSOVA, AND J. SCHWARZ (2020): "Estimating the Armington Elasticity: The Importance of Data Choice and Publication Bias." *Journal of International Economics,* 127(C):103383.

BALKE, N. S., Z. ZENG, AND R. ZHANG (2021): "Identifying Credit Demand, Financial Intermediation, and Supply of Funds Shocks: A Structural VAR Approach." *The North American Journal of Economics and Finance,* 56:101375.

BARNETT, A. AND R. THOMAS (2014): "Has Weak Lending and Activity in the UK Been Driven by Credit Supply Shocks?" *The Manchester School,* 82:60–89.

BARTOLETTO, S., B. CHIARINI, E. MARZANO, AND P. PISELLI (2019): "Business Cycles, Credit Cycles, and Asymmetric Effects of Credit Fluctuations: Evidence from Italy for the Period of 1861–2013." *Journal of Macroeconomics,* 61:103130.

BEATON, K. AND B. DESROCHES (2011): "Financial Spillovers across Countries: The Case of Canada and the United States." Bank of Canada Working Paper No. 2011-1, Bank of Canada.

BERNANKE, B. S. AND A. S. BLINDER (1988): "Credit, Money, and Aggregate Demand." *The American Economic Review,* 78(2):435–439.

BERNANKE, B. S. AND M. GERTLER (1995): "Inside the Black Box: The Credit Channel of Monetary Policy Transmission." *Journal of Economic Perspectives,* 9(4):27–48.

BERNANKE, B. S., M. GERTLER, AND S. GILCHRIST (1999): "The Financial Accelerator in a Quantitative Business Cycle Framework." *Handbook of Macroeconomics,* 1:1341–1393.

BIJLSMA, M., C. KOOL, AND M. NON (2018): "The Effect of Financial Development on Economic Growth: A Meta-Analysis." *Applied Economics,* 50(57):6128–6148.

BIJSTERBOSCH, M. AND M. FALAGIARDA (2015): "The Macroeconomic Impact of Financial Fragmentation in the Euro Area: Which Role for Credit Supply?" *Journal of International Money and Finance,* 54:93–115.

BIS (2001): *BIS 71st Annual Report.* Bank for International Settlements. Press Library Services: Switzerland.

BJORK, S., A. OFFER, AND G. SÖDERBERG (2014): "Time Series Citation Data: The Nobel Prize in Economics." *Scientometrics,* 98:185–196.

BLINDER, A. S. (1987): "Credit Rationing and Effective Supply Failures." *The Economic Journal,* 97(386):327–352.

BOM, P. R. AND H. RACHINGER (2019): "A Kinked Meta-Regression Model for Publication Bias Correction." *Research Synthesis Methods,* 10(4):497–514.

BORIO, C. (2014): "The Financial Cycle and Macroeconomics: What Have We Learnt?" *Journal of Banking & Finance,* 45:182–198.

BOUVATIER, V., A. LÓPEZ-VILLAVICENCIO, AND V. MIGNON (2012): "Does the Banking Sector Structure Matter for Credit Procyclicality?" *Economic Modelling,* 29(4):1035–1044.

BRAUN, M. AND B. LARRAIN (2005): "Finance and the Business Cycle: International, Inter-Industry Evidence." *The Journal of Finance,* 60(3):1097–1128.

BRUNNER, K. AND A. H. MELTZER (1972): "Money, Debt, and Economic Activity." *Journal of Political Economy,* 80(5):951–977.

BRUNNERMEIER, M. K., T. M. EISENBACH, AND Y. SANNIKOV (2012): "Macroeconomics with Financial Frictions: A Survey." NBER Working Paper No. 18102, National Bureau of Economic Research.

BRUNO, V. AND H. S. SHIN (2015): "Cross-Border Banking and Global Liquidity." *The Review of Economic Studies,* 82(2):535–564.

BRUNS, S. B., I. ASANOV, R. BODE, M. DUNGER, C. FUNK, S. M. HASSAN, J. HAUSCHILDT, D. HEINISCH, K. KEMPA, AND J. KÖNIG (2019): "Reporting Errors and Biases in Published Empirical Findings: Evidence from Innovation Research." *Research Policy,* 48 (9):103796.

BRZOZA-BRZEZINA, M., T. CHMIELEWSKI, AND J. NIEDŹWIEDZIŃSKA (2010): "Substitution between Domestic and Foreign Currency Loans in Central Europe: Do Central Banks Matter?" ECB Working Paper No. 1187, European Central Bank.

BUSCH, U., M. SCHARNAGL, AND J. SCHEITHAUER (2010): "Loan Supply in Germany during the Financial Crisis." DBB Working Paper No. 05/2010, Deutsches Bundesbank

CAFISO, G. (2020): "The Loan Puzzle. A Study of Loans to Different Groups in the USA." CESifo Working Paper No. 8175, Center for Economic Studies and ifo Institute (CESifo).

CALZA, A. AND J. SOUSA (2006): "Output and Inflation Responses to Credit Shocks: Are There Threshold Effects in the Euro Area?" *Studies in Nonlinear Dynamics & Econometrics,* 10 (2).

CALZA, A., C. GARTNER, AND J. SOUSA (2003): "Modelling the Demand for Loans to the Private Sector in the Euro Area." *Applied Economics,* 35(1):107–117.

CAMPOS, N. F., J. FIDRMUC, AND I. KORHONEN (2019): "Business Cycle Synchronisation and Currency Unions: A Review of the Econometric Evidence Using Meta-Analysis." *International Review of Financial Analysis,* 61:274–283.

CARD, D. AND A. B. KRUEGER (1995): "Time-Series Minimum-Wage Studies: A Meta-Analysis." *The American Economic Review,* 85(2):238–243.

CECCHETTI, S., M. MOHANTY, AND F. ZAMPOLLI (2011): "The Real Effects of Debt." BIS Working Paper No. 352, Bank for International Settlements.

CECCHETTI, S. G. AND E. KHARROUBI (2019): "Why Does Credit Growth Crowd Out Real Economic Growth?" *The Manchester School,* 87:1–28.

CERUTTI, E., S. CLAESSENS, AND L. LAEVEN (2017): "The Use and Effectiveness of Macroprudential Policies: New Evidence." *Journal of Financial Stability,* 28:203–224.

CERUTTI, E., S. CLAESSENS, AND L. RATNOVSKI (2017): "Global Liquidity and Cross-Border Bank Flows." *Economic Policy,* 32(89):81–125.

CETORELLI, N. AND L. S. GOLDBERG (2011): "Global Banks and International Shock Transmission: Evidence from the Crisis." *IMF Economic Review,* 59(1):41–76.

CHANDLER, A. D. (1977): *The Visible Hand: The Managerial Revolution in American Business.* Cambridge, MA: Harvard University Press.

CHOI, S. (2020): "Changes in the Effects of Bank Lending Shocks and Development of Public Debt Markets." *Finance Research Letters,* 33:101225.

CHOI, S. (2021): "Bank Lending Standards, Loan Demand, and the Macroeconomy: Evidence from the Korean Bank Loan Officer Survey." *International Journal of Central Banking,* 17 (5):1–46.

CHRISTIANO, L., R. MOTTO, AND M. ROSTAGNO (2010): "Financial Factors in Economic Fluctuations." ECB Working Paper No. 1192, European Central Bank

CHRISTIANO, L. J. (1999): "Monetary Policy Shocks: What Have We Learned and to What End." *Handbook of Macroeconomics,* No. 1.

CLAESSENS, S., M. A. KOSE, AND M. E. TERRONES (2011): "Financial Cycles: What? How? When?" In *NBER International Seminar on Macroeconomics,* volume 7, number 1. University of Chicago Press Chicago, IL.

CLAESSENS, S., M. A. KOSE, AND M. E. TERRONES (2012): "How Do Business and Financial Cycles Interact?" *Journal of International Economics,* 87(1):178–190.

COLLYNS, C. AND A. SENHADJI (2003): "Lending Booms, Real Estate Bubbles, and the Asian Crisis." Working Paper No. 02/020, International Monetary Fund.

COSTA-FONT, J., M. GEMMILL, AND G. RUBERT (2011): "Biases in the Healthcare Luxury Good Hypothesis?: A Meta-Regression Analysis." *Journal of the Royal Statistical Society Series A: Statistics in Society,* 174(1):95–107.

DELL'ARICCIA, G. AND P. GARIBALDI (2005): "Gross Credit Flows." *The Review of Economic Studies,* 72(3):665–685.

DERYUGINA, E. B. AND A. A. PONOMARENKO (2011): "Identifying Structural Shocks behind Loan Supply Fluctuations in Russia." BOFIT Working Paper No. 20/2011, Bank of Finland, BOFIT Institute for Economies in Transition.

DETTORI, J. R., D. C. NORVELL, AND J. R. CHAPMAN (2022): "Fixed-Effect vs Random-Effects Models for Meta-Analysis: 3 Points to Consider." *Global Spine Journal,* 12(7): 1624–1626.

DOUCOULIAGOS, C. AND T. D. STANLEY (2013): "Are All Economic Facts Greatly Exaggerated? Theory Competition and Selectivity." *Journal of Economic Surveys,* 27(2):316–339.

DOUCOULIAGOS, H., T. D. STANLEY, AND W. K. VISCUSI (2014): "Publication Selection and the Income Elasticity of the Value of a Statistical Life." *Journal of Health Economics,* 33: 67–75.

DREGER, C., D. GERDESMEIER, AND B. ROFFIA (2020): "The Impact of Credit for House Price Overvaluations in the Euro Area: Evidence from Threshold Models." MPRA Working Paper No. 99523, Munich Personal RePEc Archive.

DREHMANN, M., C. E. BORIO, AND K. TSATSARONIS (2012): "Characterising the Financial Cycle: Don't Lose Sight of the Medium Term!" Working Paper 380, Bank for International Settlements.

DUCHI, F. AND A. ELBOURNE (2016): "Credit Supply Shocks in the Netherlands." *Journal of Macroeconomics,* 50:51–71.

EGGER, M., G. D. SMITH, M. SCHNEIDER, AND C. MINDER (1997): "Bias in Meta-Analysis Detected by a Simple, Graphical Test." *BMJ,* 315(7109):629–634.

EHRENBERGEROVA, D., J. BAJZIK, AND T. HAVRANEK (2023): "When Does Monetary Policy Sway House Prices? A Meta-Analysis." *IMF Economic Review,* 71(2):538–573.

EICHER, T. S., C. PAPAGEORGIOU, AND A. E. RAFTERY (2011): "Default Priors and Predictive Performance in Bayesian Model Averaging, with Application to Growth Determinants." *Journal of Applied Econometrics,* 26(1):30–55.

EICKMEIER, S. AND T. NG (2015): "How Do US Credit Supply Shocks Propagate Internationally? A GVAR Approach." *European Economic Review,* 74:128–145.

FABO, B., M. JANČOKOVÁ, E. KEMPF, AND L. PÁSTOR (2021): "Fifty Shades of QE: Comparing Findings of Central Bankers and Academics." *Journal of Monetary Economics,* 120:1–20.

FADEJEVA, L., M. FELDKIRCHER, AND T. REININGER (2017): "International Spillovers from Euro Area and US Credit and Demand Shocks: A Focus on Emerging Europe." *Journal of International Money and Finance,* 70:1–25.

FELDKIRCHER, M. AND S. ZEUGNER (2012): "The Impact of Data Revisions on the Robustness of Growth Determinants – A Note on 'Determinants of Economic Growth: Will Data Tell?'." *Journal of Applied Econometrics,* 27(4):686–694.

FERNANDEZ, C., E. LEY, AND M. F. J. STEEL (2001): "Model Uncertainty in Cross-Country Growth Regressions." *Journal of Applied Econometrics,* 16(5):563–576.

FINLAY, R. AND J. P. JÄÄSKELÄ (2014): "Credit Supply Shocks and the Global Financial Crisis in Three Small Open Economies." *Journal of Macroeconomics,* 40:270–276.

FISHER, I. (1933): "The Debt-Deflation Theory of Great Depressions." *Econometrica: Journal of the Econometric Society,* 337–357.

FORNARI, F. AND L. STRACCA (2012): "What Does a Financial Shock Do? First International Evidence." *Economic Policy,* 27(71):407–445.

FURLANETTO, F., F. RAVAZZOLO, AND S. SARFERAZ (2019): "Identification of Financial Factors in Economic Fluctuations." *The Economic Journal,* 129(617):311–337.

FURUKAWA, C. (2019): "Publication Bias under Aggregation Frictions: Theory, Evidence, and a New Correction Method." ZBW Working Paper, ZBW – Leibniz Information Centre for Economics, Kiel, Hamburg.

GALÍ, J. (2015): *Monetary Policy, Inflation, and the Business Cycle: An Introduction to the New Keynesian Framework and Its Applications.* Princeton University Press.

GAMBETTI, L. AND A. MUSSO (2017): "Loan Supply Shocks and the Business Cycle." *Journal of Applied Econometrics,* 32(4):764–782.

GANCHEV, G., V. TSENKOV, AND E. STAVROVA (2014): "Exploring the Relationship between Credit and Nominal GDP." In Proceedings of the Conference: Money, Regulation and Growth: Financing New Growth in Europe.

GECHERT, S., T. HAVRANEK, Z. IRSOVA, AND D. KOLCUNOVA (2022): "Measuring Capital-Labor Substitution: The Importance of Method Choices and Publication Bias." *Review of Economic Dynamics,* 45:55–82.

GEORGE, E. I. (2010): "Dilution Priors: Compensating for Model Space Redundancy." Borrowing Strength: Theory Powering Applications – A Festschrift for Lawrence D. Brown. IMS Collections, Vol. 6., Institute of Mathematical Statistics.

GERBER, A. AND N. MALHOTRA (2008): "Do Statistical Reporting Standards Affect What is Published? Publication Bias In Two Leading Political Science Journals." *Quarterly Journal of Political Science,* 3(3):313–326.

GERBER, A. S. AND N. MALHOTRA (2008): "Publication Bias in Empirical Sociological Research: Do Arbitrary Significance Levels Distort Published Results?" *Sociological Methods & Research,* 37(1):3–30.

GERTLER, M. AND B. BERNANKE (1989): "Agency Costs, Net Worth and Business Fluctuations." *American Economic Review,* 79:14–31.

GERTLER, M. AND N. KIYOTAKI (2010): "Financial Intermediation and Credit Policy in Business Cycle Analysis." *Handbook of Monetary Economics,* 3:547–599.

GOLDSMITH, R. W. (1969): *Financial Structure and Development.* Yale University Press, New Haven, US.

GOMES, F. A. R. AND G. P. SOAVE (2021): "The Role of Credit and Housing Shocks in Emerging Economies." *Applied Economics Letters,* 28(18):1552–1557.

GOODHART, C. AND B. HOFMANN (2008): "House Prices, Money, Credit, and the Macroeconomy." *Oxford Review of Economic Policy,* 24(1):180–205.

GOURINCHAS, P.-O. AND M. OBSTFELD (2012): "Stories of the Twentieth Century for the Twenty-First." *American Economic Journal: Macroeconomics,* 4(1):226–65.

GREENWOOD, J. AND B. JOVANOVIC (1990): "Financial Markets in Development, and the Development of Financial Markets." *Journal of Economic Dynamics And Control,* 14(1): 607–622.

GROEN, J. J. (2004): "Corporate Credit, Stock Price Inflation and Economic Fluctuations." *Applied Economics,* 36(18):1995–2006.

GRUBER, J. AND G. LEE (2008): "Bank Lending Effect on German Commercial Property Prices." Working Papers in Business, Economics and Management Information Systems 428, University of Regensburg.

GUERRA, E. A. R. (2017): "The Economic Growth and the Banking Credit in Mexico: Granger Causality and Short-Term Effects, 2001Q1–2016Q4." *Economía Informa,* 406:46–58.

GUEVARA, C. AND G. RODRÍGUEZ (2020): "The Role of Credit Supply Shock in Pacific Alliance Countries: A TVP-VAR-SV Approach." *The North American Journal of Economics and Finance,* 52(101140).

GURLEY, J. G. AND E. S. SHAW (1955): "Financial Aspects of Economic Development." *American Economic Review,* 45(4):515–538.

GURLEY, J. G. AND E. S. SHAW (1967): "Financial Structure and Economic Development." *Economic Development and Cultural Change,* 15(3):257–268.

HAELERMANS, C. AND L. BORGHANS (2012): "Wage Effects of On-the-Job Training: A Meta-Analysis." *British Journal of Industrial Relations,* 50(3):502–528.

HALVORSEN, J. I. AND D. H. JACOBSEN (2014): "How Important Can Bank Lending Shocks Be for Economic Fluctuations?" *The North American Journal of Economics and Finance,* 29: 104–123.

HAVRANEK, T. AND M. RUSNAK (2013): "Transmission Lags of Monetary Policy: A Meta-Analysis." *International Journal of Central Banking,* 9(4):39–76.

HAVRANEK, T., R. HORVATH, Z. IRSOVA, AND M. RUSNAK (2015): "Cross-Country Heterogeneity in Intertemporal Substitution." *Journal of International Economics,* 96(1): 100–118.

HAVRÁNEK, T., T. STANLEY, H. DOUCOULIAGOS, P. BOM, J. GEYER-KLINGEBERG, I. IWASAKI, W. R. REED, K. ROST, AND R. VAN AERT (2020): "Reporting Guidelines for Meta-Analysis in Economics." *Journal of Economic Surveys,* 34(3):469–475.

HEATH, E. (2008): "Do Credit Variables Affect Real Output and Inflation? If So, Which Ones?" West Virginia University. Working Paper.

HELBLING, T., R. HUIDROM, M. A. KOSE, AND C. OTROK (2011): "Do Credit Shocks Matter? A Global Perspective." *European Economic Review,* 55(3):340–353.

HIRSCHBÜHL, D. AND M. SPITZER (2021): "International Medium-Term Business Cycles." Working Paper 2536, European Central Bank.

HOFMANN, B. (2001): "The Determinants of Private Sector Credit in Industrialised Countries: Do Property Prices Matter?" WP, BIS Working Paper No. 108

HOFMANN, B. (2004): "The Determinants of Bank Credit in Industrialized Countries: Do Property Prices Matter?" *International Finance,* 7(2):203–234.

HOUSSA, R., J. MOHIMONT, AND C. OTROK (2013): "Credit Shocks and Macroeconomic Fluctuations in Emerging Markets." CESifo Working Paper No. 4281, Center for Economic Studies and ifo Institute (CESifo), Munich.

HRISTOV, N., O. HÜLSEWIG, AND T. WOLLMERSHÄUSER (2012): "Loan Supply Shocks during the Financial Crisis: Evidence for the Euro Area." *Journal of International Money and Finance,* 31(3):569–592.

HWANG, Y. (2012): "Sources of Housing Price Fluctuations: An Analysis Using Sign-Restricted VAR." *Korea and the World Economy,* 13(2):249–283.

IBRAHIM, M. H. (2006): "Stock Prices and Bank Loan Dynamics in a Developing Country: The Case of Malaysia." *Journal of Applied Economics,* 9(1):71–89.

IBRAHIM, M. H. AND S. H. LAW (2014): "House Prices and Bank Credits in Malaysia: An Aggregate and Disaggregate Analysis." *Habitat International,* 42:111–120.

IBRAHIM, M. H. AND M. E. SHAH (2012): "Bank Lending, Macroeconomic Conditions and Financial Uncertainty: Evidence from Malaysia." *Review of Development Finance,* 2(3-4): 156–164.

IMBS, J. (2010): "The First Global Recession in Decades." *IMF Economic Review,* 58(2):327–354.

IMF (2000): *World Economic Outlook, May 2000 : Asset Prices and the Business Cycle. International Monetary Fund Research Dept.* International Monetary Fund, IMF.

IOANNIDIS, J. P., T. STANLEY, AND H. DOUCOULIAGOS (2017): "The Power of Bias in Economics Research." *Economic Journal,* 127(605):236–265.

IRSOVA, Z., H. DOUCOULIAGOS, T. HAVRANEK, AND T. D. STANLEY (2024): "Meta-Analysis of Social Science Research: A Practitioner's Guide." *Journal of Economic Surveys,* 38(5): 1547–1566.

IWASAKI, I. (2022): "The Finance–Growth Nexus in Latin America and the Caribbean: A Meta-Analytic Perspective." *World Development,* 149:105692.

JORDÀ, Ò., M. SCHULARICK, AND A. M. TAYLOR (2011): "Financial Crises, Credit Booms, and External Imbalances: 140 Years of Lessons." *IMF Economic Review,* 59(2):340–378.

JORDÀ, Ò., M. SCHULARICK, AND A. M. TAYLOR (2017): "Macrofinancial History and the New Business Cycle Facts." *NBER Macroeconomics Annual,* 31(1):213–263.

JUNG, W. S. (1986): "Financial Development and Economic Growth: International Evidence." *Economic Development and Cultural Change,* 34(2):333–346.

KABASHI, R. AND K. SULEVA (2016): "Loan Supply Shocks in Macedonia: A Bayesian SVAR Approach with Sign Restrictions." *Croatian Economic Survey,* 18(1):5–33.

KARFAKIS, C. (2013): "Credit and Business Cycles in Greece: Is There Any Relationship?" *Economic Modelling,* 32:23–29.

KASHYAP, A. K., J. C. STEIN, AND D. W. WILCOX (1992): "Monetary Policy and Credit Conditions: Evidence from the Composition of External Finance." WP, NBER, Working Paper 4015.

KAUFMANN, S. AND M. T. VALDERRAMA (2010): "The Role of Credit Aggregates and Asset Prices in the Transmission Mechanism: A Comparison between the Euro Area and the USA." *The Manchester School,* 78(4):345–377.

KILIAN, L. AND D. P. MURPHY (2012): "Why Agnostic Sign Restrictions are not Enough: Understanding the Dynamics of Oil Market VAR Models." *Journal of the European Economic Association,* 10(5):1166–1188.

KIM, S. AND A. N. MEHROTRA (2018): "Effects of Monetary and Macroprudential Policies – Evidence from Four Inflation Targeting Economies." *Journal of Money, Credit and Banking,* 50(5):967–992.

KIM, S. AND A. N. MEHROTRA (2019): "Examining Macroprudential Policy and Its Macroeconomic Effects – Some New Evidence." Working Paper 825, Bank for International Settlements.

KINDLEBERGER, C. P. (1978): *Manias, Panics and Crashes: A History of Financial Crisis.* Macmillan, London, UK.

KING, R. G. AND R. LEVINE (1993): "Finance and Growth: Schumpeter Might be Right." *The Quarterly Journal of Economics,* 108(3):717–737.

KIYOTAKI, N. AND J. MOORE (1997): "Credit Cycles." *Journal of Political Economy,* 105(2): 211–248.

LEVINE, R. (2021): "Finance, Growth, and Inequality." IMF Working Paper No. 21/164, International Monetary Fund.

LOWN, C. S. AND D. P. MORGAN (2002): "Credit Effects in the Monetary Mechanism." *Economic Policy Review,* 8(1).

MA, W., S. HONG, W. R. REED, J. DUAN, AND P. LUU (2023): "Yield Effects of Agricultural Cooperative Membership in Developing Countries: A Meta-Analysis." *Annals of Public and Cooperative Economics.*

MALOVANÁ, S., M. HODULA, J. BAJZÍK, AND Z. GRIC (2024): "Bank Capital, Lending, and Regulation: A Meta-Analysis." *Journal of Economic Surveys,* 38(3).

MALOVANÁ, S., M. HODULA, Z. GRIC, AND J. BAJZÍK (2025): "Borrower-Based Macroprudential Measures and Credit Growth: How Biased is the Existing Literature?" *Journal of Economic Surveys,* 39(1).

MANDLER, M. AND M. SCHARNAGL (2020): "Bank Loan Supply Shocks and Alternative Financing of Non-Financial Corporations in the Euro Area." *The Manchester School,* 88: 126–150.

MARTÍNEZ, J. AND G. RODRÍGUEZ (2021): "Macroeconomic Effects of Loan Supply Shocks: Empirical Evidence for Peru." *Latin American Economic Review,* 30:1–24.

MCCLOSKEY, D. AND S. T. ZILIAK (2019): "What Quantitative Methods Should We Teach to Graduate Students? A Comment on Swann's 'Is Precise Econometrics an Illusion?'." *The Journal of Economic Education,* 50(4):356–361.

MIAN, A. AND A. SUFI (2018): "Finance and Business Cycles: The Credit-Driven Household Demand Channel." *Journal of Economic Perspectives,* 32(3):31–58.

MINSKY, H. (1975): *John Maynard Keynes.* Columbia University Press, New York, US.

MOELTNER, K. AND R. WOODWARD (2009): "Meta-Functional Benefit Transfer for Wetland Valuation: Making the Most of Small Samples." *Environmental and Resource Economics,* 42:89–108.

MWANKEMWA, L. P. AND B. MLAMKA (2022): "Effects of Monetary Policy on Bank's Credit Dynamics in Tanzania." *African Journal of Economic Review,* 10(2):13–39.

ONO, S. AND I. IWASAKI (2022): "The Finance–Growth Nexus in Europe: A Comparative Meta-Analysis of Emerging Markets and Advanced Economies." *Eastern European Economics,* 60(1):1–49.

OPATRNY, M., T. HAVRANEK, Z. IRSOVA, AND M. SCASNY (2023): "Publication Bias and Model Uncertainty in Measuring the Effect of Class Size on Achievement." ZBW Working Paper, ZBW – Leibniz Information Centre for Economics, Kiel, Hamburg Suggested Citation

POŠTA, V. AND Z. PIKHART (2015): "Financial Risk and Real Variables: Evidence Based on a SVAR Analysis of the Czech Economy." *Prague Economic Papers,* 24(5):516–537.

PRIETO, E., S. EICKMEIER, AND M. MARCELLINO (2016): "Time Variation in Macro-Financial Linkages." *Journal of Applied Econometrics,* 31(7):1215–1233.

RAJAN, R. G. AND L. ZINGALES (1998): "Financial Dependence and Growth." *The American Economic Review,* 88(3):559–586.

RAMEY, V. A. (2016): "Macroeconomic Shocks and Their Propagation." *Handbook Of Macroeconomics,* 2:71–162.

REICHENBACHAS, T. (2017): "Credit-Related Shocks in VAR Models: The Case of Lithuania." *Ekonomika,* 96(3):7–19.

REINHART, C. M. AND K. S. ROGOFF (2009): *This Time Is Different: Eight Centuries of Financial Folly.* Princeton University Press.

ROBINSON, J. (1952): *The Rate of Interest and Other Essays,* chapter The Generalization of the General Theory. Macmillan, London, UK.

ROUBINI, N. AND X. SALA-I MARTIN (1992): "Financial Repression and Economic Growth." *Journal of Development Economics,* 39(1):5–30.

RUSNÁK, M., T. HAVRANEK, AND R. HORVÁTH (2013): "How to Solve the Price Puzzle? A Meta-Analysis." *Journal of Money, Credit and Banking,* 45(1):37–70.

SAFAEI, J. AND N. E. CAMERON (2003): "Credit Channel and Credit Shocks in Canadian Macrodynamics: A Structural VAR Approach." *Applied Financial Economics,* 13(4):267–277.

SAMARGANDI, N. AND A. M. KUTAN (2016): "Private Credit Spillovers and Economic Growth: Evidence from BRICS Countries." *Journal of International Financial Markets, Institutions and Money,* 44:56–84.

SCHULARICK, M. AND A. M. TAYLOR (2012): "Credit Booms Gone Bust: Monetary Policy, Leverage Cycles, and Financial Crises, 1870–2008." *American Economic Review,* 102(2): 1029–1061.

SCHUMPETER, J. A. (1934): *The Theory of Economic Development: An Inquiry into Profits, Capital, Credit, Interest, and the Business Cycle.* Harvard University Press, Cambridge, US.

SERWA, D. AND P. WDOWIŃSKI (2017): "Macro-Financial Linkages in the Polish Economy: Combined Impulse-Response Functions in SVAR Models." *Central European Journal of Economic Modelling and Econometrics,* 4:323–357.

SHUKOR, N. B. B. M., R. B. SAID, AND R. B. A. MAJID (2016): "The Relationship between Housing Finance and Macroeconomics Variables in Malaysia." In *MATEC Web of Conferences,* volume 66. EDP Sciences.

SINGH, B. AND A. R. NADKARNI (2020): "Role of Credit and Monetary Policy in Determining Asset Prices: Evidence from Emerging Market Economies." *The North American Journal of Economics and Finance,* 51(100874).

SNYDER, T. C. AND S. VALE (2022): "House Prices and Household Credit in the Eurozone: A Single Monetary Policy with Dissonant Transmission Mechanisms." *The Quarterly Review of Economics and Finance,* 84:243–256.

STANLEY, T. D. (2008): "Meta-Regression Methods for Detecting and Estimating Empirical Effects in the Presence of Publication Selection." *Oxford Bulletin of Economics and Statistics,* 70(1):103–127.

STANLEY, T. D. (2005): "Beyond Publication Bias." *Journal of Economic Surveys,* 19(3):309–345.

STANLEY, T. D. AND H. DOUCOULIAGOS (2012): *Meta-Regression Analysis in Economics and Business.* Routledge.

STANLEY, T. D. AND H. DOUCOULIAGOS (2014): "Meta-Regression Approximations to Reduce Publication Selection Bias." *Research Synthesis Methods,* 5(1):60–78.

STANLEY, T. D., S. B. JARRELL, AND H. DOUCOULIAGOS (2010): "Could It be Better to Discard 90% Of the Data? A Statistical Paradox." *The American Statistician,* 64(1):70–77.

STANLEY, T. D., H. DOUCOULIAGOS, M. GILES, J. H. HECKEMEYER, R. J. JOHNSTON, P. LAROCHE, J. P. NELSON, M. PALDAM, J. POOT, AND G. PUGH (2013): "Meta-Analysis of Economics Research Reporting Guidelines." *Journal of Economic Surveys,* 27 (2):390–394.

STEPANYAN, V. AND K. GUO (2011): "Determinants of Bank Credit in Emerging Market Economies." IMF Working Paper No. 2011/051. International Monetary Fund.

SWISTON, A. (2008): "A U.S. Financial Conditions Index: Putting Credit Where Credit is Due." Working Paper No. 161/2008, International Monetary Fund.

TOBIN, J. AND W. C. BRAINARD (1963): "Financial Intermediaries and the Effectiveness of Monetary Controls." *American Economic Review,* 53(2):383–400.

VALICKOVA, P., T. HAVRANEK, AND R. HORVATH (2015): "Financial Development and Economic Growth: A Meta-Analysis." *Journal of Economic Surveys,* 29(3):506–526.

VAZQUEZ, F., B. M. TABAK, AND M. SOUTO (2012): "A Macro Stress Test Model of Credit Risk for the Brazilian Banking Sector." *Journal of Financial Stability,* 8(2):69–83.

WALSH, C. E. AND J. A. WILCOX (1995): "Bank Credit and Economic Activity." In *Conference Series,* volume 39. Federal Reserve Bank of Boston.

XU, T. (2012): "The Role of Credit in International Business Cycles." Cambridge Working Papers in Economics 1202, Faculty of Economics, University of Cambridge.

XUE, X., W. R. REED, AND R. C. VAN AERT (2022): "Social Capital and Economic Growth: A Meta-Analysis." Working Papers in Economics No. 22/20, University of Canterbury, Department of Economics and Finance

YUSOF, R. M., F. H. USMAN, A. A. MAHFUDZ, AND A. S. ARIF (2018): "Macroeconomic Shocks, Fragility and Home Financing in Malaysia: Can Rental Index Be the Answer?" *Journal of Islamic Accounting and Business Research.*

ZAIDI, M. A. S. AND L. A. FISHER (2010): "Monetary Policy and Foreign Shocks: A SVAR Analysis for Malaysia." *Korea and the World Economy,* 11(3):527–550.

ZEUGNER, S. AND M. FELDKIRCHER (2015): "Bayesian Model Averaging Employing Fixed and Flexible Priors: The BMS Package for R." *Journal of Statistical Software,* 68(4):1–37.

ZIGRAIOVA, D., T. HAVRANEK, Z. IRSOVA, AND J. NOVAK (2021): "How Puzzling Is the Forward Premium Puzzle? A Meta-Analysis." *European Economic Review,* 134:103714.

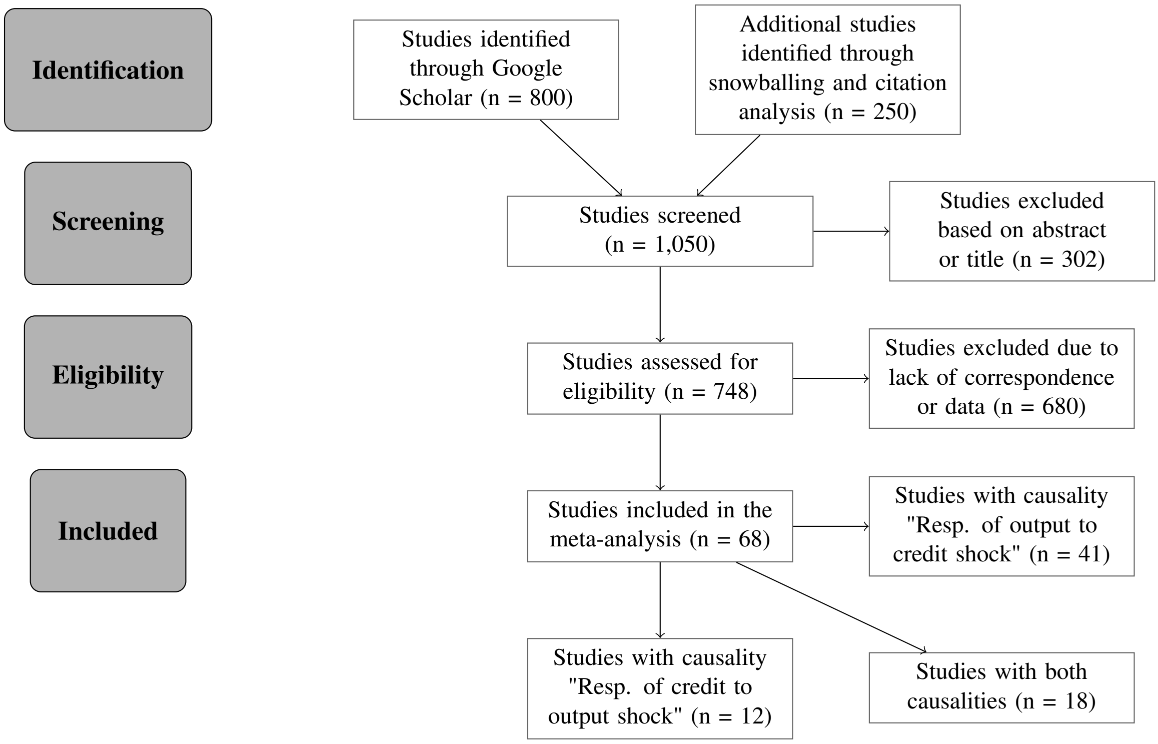
ZWICK, L. (2015): "International Liquidity Shocks and Domestic Loan Supply in the Euro Area." Ruhr Economic Paper No. 564, Rheinisch-Westfälisches Institut für Wirtschaftsforschung.

Appendix A: Data Collection and Fragmentation

A.1 Paper Selection Procedure

Figure A1 depicts the overall paper selection process for this meta-analysis, including the identification of relevant papers, the initial screening for relevance and eligibility, and the final inclusion of papers. Next, we describe each phase in detail.

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



Note: Figure A1 illustrates the overall process we employed for the selection of primary studies. First, we examined the first 800 items returned by Google Scholar using our tailor-made string (see Section 3). After that, we looked through all the references in each of the relevant study and all papers which cited such studies and identified additional 250 articles. All in all, we obtained 1,050 articles for *screening*. Second, we reviewed all the titles and abstracts, as we wanted to pinpoint the studies which are not relevant for our analysis even from the high-level perspective. In this stage, we eliminated 302 studies. In the *eligibility* phase, we went through each of the remaining 748 studies in a more detailed way and excluded 680 studies due to lack of correspondence or data. The main assessment criteria were: (1) the measure of credit is not expressed as a ratio to another continuous variable, such as total loans, total assets or GDP; (2) the estimates are reported in impulse response functions with an exact confidence interval; (3) the shock to the independent variable must be expressed numerically. We ended with 68 primary studies that were *included* in the meta-analysis. Lastly, we divided these studies based on the direction of the causality they include (output-credit, credit-output or both).

We used Google Scholar as our main search database to collect relevant primary studies. Google Scholar searches the full text of articles, as well as titles, abstracts, and keywords. Following the recommendations of Havránek et al. (2020), we screened the top 800 articles from a query related to credit and output:

> *“credit” OR “lending” OR “loan” AND “GDP” OR “output” OR “economic growth”*

We focused on studies examining both directions of causality between credit and output, including studies reporting the response of credit to output shock, the response of output to credit shock, or both. To maximize our coverage and informative value, we did not limit our sample by variable transformation, data coverage, or identification, as we plan to explore the impact of these factors in this paper. We initially read the abstracts of Google Scholar results to remove irrelevant studies. Additionally, we used forward and backward citation searching[14] to identify more articles. This helped us add 250 potentially relevant studies, increasing our total screened studies to 1,050 articles. We then selected the relevant studies for our meta-analysis from this expanded database.

To include studies in our analysis, we applied several criteria. Studies must: (1) use a VAR model for consistency in semi-elasticity comparison, (2) provide confidence intervals around the mean impulse response function for calculation of standard error, (3) use GDP as the output measure, and (4) avoid using credit as a ratio to another continuous variable, such as total assets or GDP[15]. Following these decision rules, we identified 68 primary studies for our analysis. We add the last study in September 2022.

For each paper excluded from our sample, we recorded the reason (Table A1). Initially, we removed 302 articles based on title and abstract for being irrelevant to our meta-analysis topic. Since our focus is on empirical studies using VAR models, we excluded 223 articles using different methodologies, primarily time series or panel data regression, and 82 theoretical studies. We further refined the list based on criteria for the main dependent variable, shock variable, and the presence of an uncertainty measure (standard error, confidence interval, etc.). Additionally, we checked if a journal version existed for each working paper in our sample, replacing it with the peer-reviewed version when available. The relatively small percentage of studies ultimately included, out of the very high total number of studies reviewed, is typical of recent meta-analyses that strive to screen all potentially relevant articles in the field (Ehrenbergerova et al., 2023; Fabo et al., 2021; Zigraiova et al., 2021, see also).

We identified 68 relevant studies (see Tables A2 and A3), from which we gathered 2,629 point estimates of credit and output responses to credit and output shocks across various time horizons. Specifically, we collected immediate responses where available (zero quarters), and responses after one, two, four, six, eight, twelve, and sixteen quarters. We also included responses at the maximum horizon reported by each study to assess the persistence and potential long-term effects of shocks. In each case we carefully measure pixel coordinates to recover the numerical estimate as precisely as possible using web-based tool.[16] This involved extracting each impulse response function from the articles, uploading it to the web tool, aligning axes, and selecting points (estimates and confidence intervals) at each horizon. The data was then downloaded into an Excel file for compilation. One author meticulously recorded each data point, with a more experienced colleague verifying it to ensure accuracy. The team collaboratively resolved any coding ambiguities, minimizing human errors.

[14] Forward citation search 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 search is the opposite process. In 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.

[15] This excluded literature strands covered by previous meta-analytical papers (Arestis et al., 2015; Valickova et al., 2015; Bijlsma et al., 2018; Anwar and Iwasaki, 2023a; Iwasaki, 2022; Ono and Iwasaki, 2022; Anwar and Iwasaki, 2023b), which focused on credit to GDP ratio and other financial sector proxies as dependent variables.

[16] For this purpose, we utilized the web tool https://automeris.io/WebPlotDigitizer/.

Table A1: Paper Selection Procedure: Reasons for Exclusion of Articles

Reason for exclusion	Number of articles
Studies excluded based on abstract or title	302
Studies excluded due to lack of correspondence or data	
Unsuitable methodology (not VAR)	223
Theoretical model or simulation	82
Unsuitable dependent variable	221
Unsuitable shock variable	114
Missing uncertainty measure (standard error, confidence intervals, etc.)	20
Working paper excluded due to existing journal version*	13
Size of the shock not retrievable	7
Total excluded articles	*982*

We collected responses to two shocks: credit shock and output shock. Additionally, we distinguish between two transformations of credit and output variables: growth rate and log-level transformation. We standardize each point estimate to show a reaction to a one percentage point or one percent increase in the shock variable, and we calculate the standard error of each estimate using the reported confidence intervals. Before proceeding with the analysis, we also winsorized the effect sizes for each horizon and transformation at 1% from both sides.

Table A2: Primary Studies Included in the Meta-Analysis: Response of Credit to Output Shock

A) Key Variables Expressed in Log-Levels		
Akhatova et al. (2016)	Hofmann (2004)	Kim and Mehrotra (2019)
Cafiso (2020)	Choi (2021)	Lown and Morgan (2002)
Dell'Ariccia and Garibaldi (2005)	Ibrahim (2006)	Serwa and Wdowiński (2017)
Fornari and Stracca (2012)	Ibrahim and Shah (2012)	Yusof et al. (2018)
Gruber and Lee (2008)	Kim and Mehrotra (2018)	Zaidi and Fisher (2010)
B) Key Variables Expressed in Growth Rates		
Bartoletto et al. (2019)	Ganchev et al. (2014)	Pošta and Pikhart (2015)
Bouvatier et al. (2012)	Goodhart and Hofmann (2008)	Shukor et al. (2016)
Brzoza-Brzezina et al. (2010)	Guerra (2017)	Singh and Nadkarni (2020)
Collyns and Senhadji (2003)	Ibrahim and Law (2014)	Snyder and Vale (2022)
Dreger et al. (2020)	Kaufmann and Valderrama (2010)	Vazquez et al. (2012)

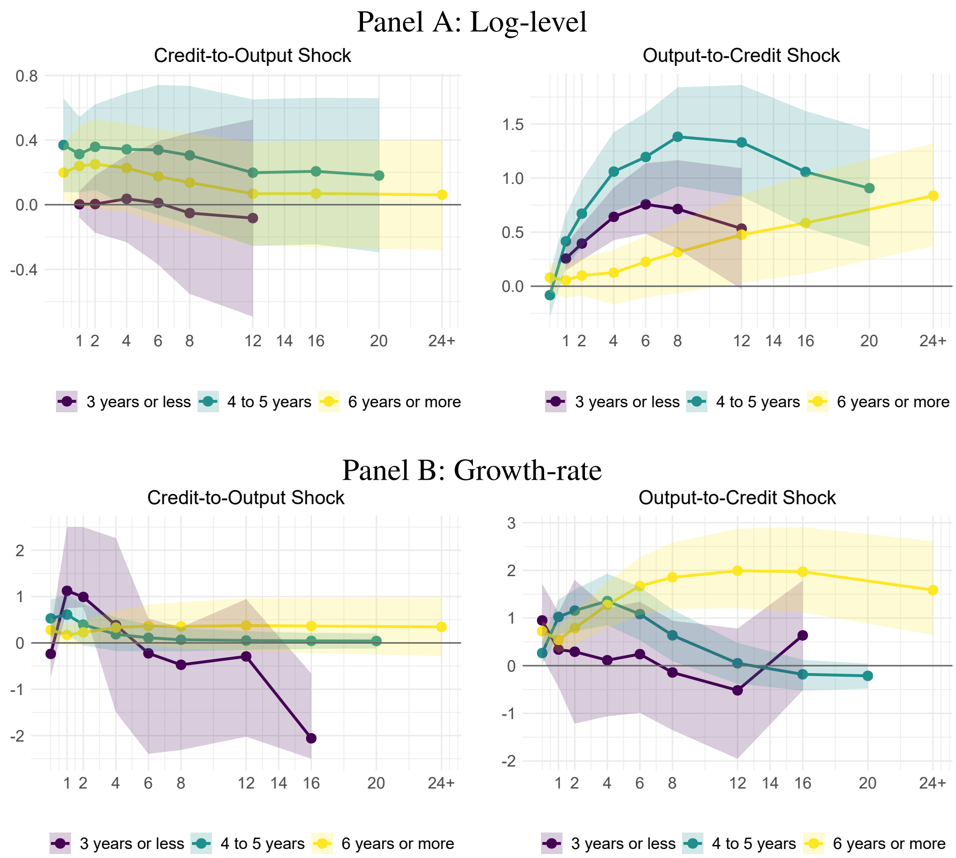
Table A3: Primary Studies Included in the Meta-Analysis: Response of Output to Credit Shock

A) Key Variables Expressed in Log-Levels		
Akhatova et al. (2016)	Halvorsen and Jacobsen (2014)	Lown and Morgan (2002)
Anari et al. (2002)	Hofmann (2001)	Mandler and Scharnagl (2020)
Assenmacher-Wesche and Gerlach (2008)	Hristov et al. (2012)	Safaei and Cameron (2003)
Beaton and Desroches (2011)	Hwang (2012)	Samargandi and Kutan (2016)
Busch et al. (2010)	Choi (2020)	Serwa and Wdowiński (2017)
Deryugina and Ponomarenko (2011)	Choi (2021)	Swiston (2008)
Eickmeier and Ng (2015)	Ibrahim (2006)	Walsh and Wilcox (1995)
Furlanetto et al. (2019)	Ibrahim and Shah (2012)	Zaidi and Fisher (2010)
Groen (2004)	Kim and Mehrotra (2018)	Zwick (2015)
Gruber and Lee (2008)	Kim and Mehrotra (2019)	
B) Key Variables Expressed in Growth Rates		
Balke et al. (2021)	Gambetti and Musso (2017)	Kabashi and Suleva (2016)
Barnett and Thomas (2014)	Ganchev et al. (2014)	Karfakis (2013)
Bartoletto et al. (2019)	Gomes and Soave (2021)	Kaufmann and Valderrama (2010)
Bijsterbosch and Falagiarda (2015)	Goodhart and Hofmann (2008)	Martínez and Rodríguez (2021)
Calza and Sousa (2006)	Guerra (2017)	Mwankemwa and Mlamka (2022)
Collyns and Senhadji (2003)	Guevara and Rodríguez (2020)	Reichenbachas (2017)
Dreger et al. (2020)	Heath (2008)	Singh and Nadkarni (2020)
Duchi and Elbourne (2016)	Helbling et al. (2011)	Snyder and Vale (2022)
Fadejeva et al. (2017)	Houssa et al. (2013)	Vazquez et al. (2012)
Finlay and Jääskelä (2014)	Ibrahim and Law (2014)	Xu (2012)

A.2 The Role of 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 that 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 Figure A2 and Table A4 in the Appendix. Firstly, many studies report effects up to 4 to 5 years (35 studies) and 6 years or more (33 studies). The response of output to credit shock diminishes to zero, regardless of the variable transformation and the maximum reported horizon. The only exception is the three studies reporting a strongly negative effect at the 16th horizon which, given the number of studies in this category, we consider an outlier. Regarding the response of credit to output, we observe a strongly positive and long-lasting response in log-credit regardless of the maximum reported horizon. For credit growth, this long-lasting response is only observed in the category of studies reporting up to 6 years or more. As such, this provides mixed evidence on the permanence or long-lasting nature of the credit response. Taking into account the number of studies in each group, we have 21 studies showing a positive and long-lasting response in credit to output shock and 9 studies, ending at shorter horizons, showing a transitory response.

Figure A2: Mean Impulse Response Functions: Breakdown by Maximum Reported Horizon



Note: The figure displays weighted average response of credit and output to a one-percentage-point increase in the credit or output, accompanied by the average 68% confidence interval. The weight is the inverse of the number of collected estimates per study. Panel A shows log-level transformation of credit and output, while Panel B shows growth-rate transformation of both variables.

Table A4: Summary Statistics of Collected Semi-Elasticities: Maximum Reported Horizon

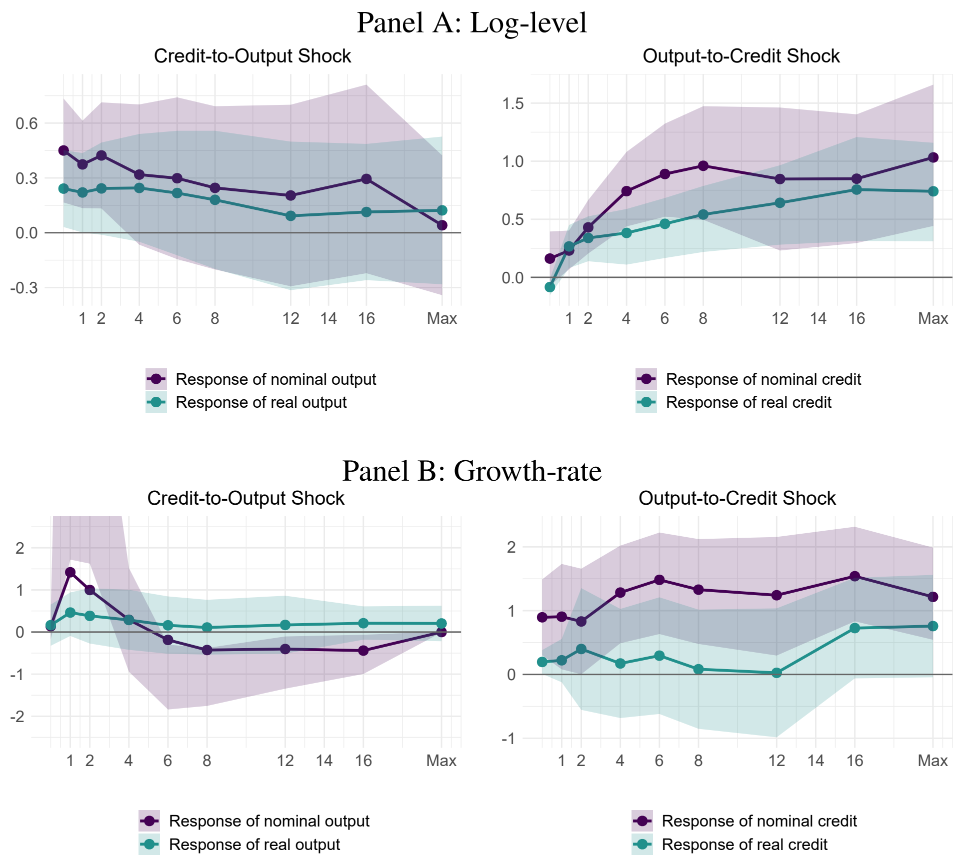
Max. horizon	Obs.	Studies	W. Mean	W. 5%	W. 95%	W. Skew
Credit-to-Output Shock: Log-level						
3 years or less	24	3	-0.08	-0.75	0.35	-0.35
4 to 5 years	38	14	0.24	-0.13	1.61	3.09
6 years or more	50	13	0.08	-0.81	0.80	2.29
Credit-to-Output Shock: Growth Rate						
3 years or less	17	8	-0.26	-2.06	0.43	-1.60
4 to 5 years	57	13	0.04	-0.21	0.35	3.33
6 years or more	34	8	0.35	-1.02	4.10	2.77
Output-to-Credit Shock: Log-level						
3 years or less	34	4	0.53	-1.11	2.50	1.30
4 to 5 years	8	5	0.91	0.01	2.18	0.70
6 years or more	20	6	0.97	-0.67	2.59	0.27
Output-to-Credit Shock: Growth Rate						
3 years or less	14	6	-0.42	-3.41	0.63	-1.12
4 to 5 years	21	3	-0.21	-1.40	0.04	-2.83
6 years or more	14	6	1.59	-1.18	6.09	1.20

Note: The table presents summary statistics of the collected semi-elasticities on the response of credit and output to shocks to output or credit. Only the maximum reported horizons of the impulse response functions are summarized.

A.3 Real vs. Nominal Credit and Output

Another reason for observing a long-lasting response of credit could be that the studies focus on nominal rather than real credit. The long-lasting impact on nominal credit would not be surprising, given that inflation can significantly inflate nominal credit, creating the appearance of increased economic activity and lending that does not accurately reflect the real economic conditions. As such, studies focusing on nominal data may overestimate the strength and persistence of relationships between output and credit. Nevertheless, when we compare studies that use nominal credit with those using real credit, we find that their results are very similar, especially for log-level transformations (see Figure A3). Generally, the effects are weaker for real credit, but the trajectory is very similar, and the long-lastingness of the response persists. Moreover, the studies on credit responses are divided roughly half-and-half between those using nominal and real credit (see Table A5). On the other hand, studies on output response are mostly conducted with real credit. Additionally, the studies using nominal credit and output always control for price changes in their model, which should at least to some extent mitigate the issue of potential effect overestimation when considering nominal variables.

Figure A3: Mean Impulse Response Functions: Real or Nominal Response Variable



Note: The figure displays weighted average response of credit and output to a one-percentage-point increase in the credit or output, accompanied by the average 68% confidence interval. The weight is the inverse of the number of collected estimates per study. Panel A shows log-level transformation of credit and output, while Panel B shows growth-rate transformation of both variables.

Table A5: Number of Studies Reporting Real or Nominal Response Variable

	Credit-to-Output Shock				Output-to-Credit Shock			
	Log-level		Growth rate		Log-level		Growth rate	
	Obs.	Studies	Obs.	Studies	Obs.	Studies	Obs.	Studies
Nominal response	112	4	79	7	192	8	198	8
Real response	826	26	776	22	254	7	192	7

A.4 Main Focus of the Primary Studies

Last but not least, we may observe a long-lasting effect on credit partly because some studies may lack precise identification, particularly when their primary focus is not on estimating credit responses. Our meta-analysis includes all studies reporting responses of credit and output to output and credit shocks, even those whose main interest lies elsewhere, merely treating these responses as secondary outcomes. To address this, we divide all studies in our dataset into those that primarily focus on estimating the credit or output response and those that focus on something else. Out of the 30 studies reporting credit responses to output shocks, 15 primarily focus on identifying and estimating this relationship. Regarding the second relationship, the majority (38 studies) focus on the output response to credit shocks, while the rest (21 studies) focus on the transmission of central bank policies, the housing market, or determinants of credit supply (see Tables A6—A8 for details). Figure A4 shows the mean impulse response functions for both groups of studies. The comparison shows that studies primarily focusing on the identification and estimation of the credit response exhibit larger and more statistically significant responses than those focusing on other issues. This pattern may indicate that studies primarily concentrating on a specific subject tend to report more substantial effects for their main interest. Furthermore, over time, it seems that main focus studies more frequently present the characteristic hump-shaped impulse response function, which is markedly distinct from those in other studies.

Table A6: Main Focus of Primary Studies: Overview

Focus of the Primary Study	Number of articles
Panel A: Response of Credit to Output Shock	
Credit and/or Business Cycle	15
Monetary and/or Macroprudential Policy Transmission	7
House Prices and/or Real Estate Market Developments	6
Determinants and Characteristics of Loan Supply	2
Panel B: Response of Output to Credit Shock	
Credit and/or Business Cycle	38
Monetary and/or Macroprudential Policy Transmission	9
House Prices and/or Real Estate Market Developments	6
Determinants and Characteristics of Loan Supply	6

Table A7: Difference in Means Between Studies With Main and Other Focus - Credit to Output Shock

Panel A: Growth Rate Transformation

Horizon	Main focus	Other focus	Diff. in means
Short-term	0.567	0.327	0.24*
Medium-term	0.121	0.291	-0.17***
Long-term	0.229	0.373	-0.144***

Panel B: Log-Level Transformation

Horizon	Main focus	Other focus	Diff. in means
Short-term	0.286	0.16	0.126**
Medium-term	0.427	0.093	0.334***
Long-term	0.573	-0.017	0.59***

Note: Column “Diff. in means” is the difference between means of studies with the main focus and studies with the other focus. Alongside this difference, we report statistical significance (***, **, *) of this difference using Wilcoxon rank sum test with the null hypothesis that means between the two groups are equal.

Table A8: Difference in Means Between Studies With Main and Other Focus - Output to Credit Shock

Panel A: Growth Rate Transformation

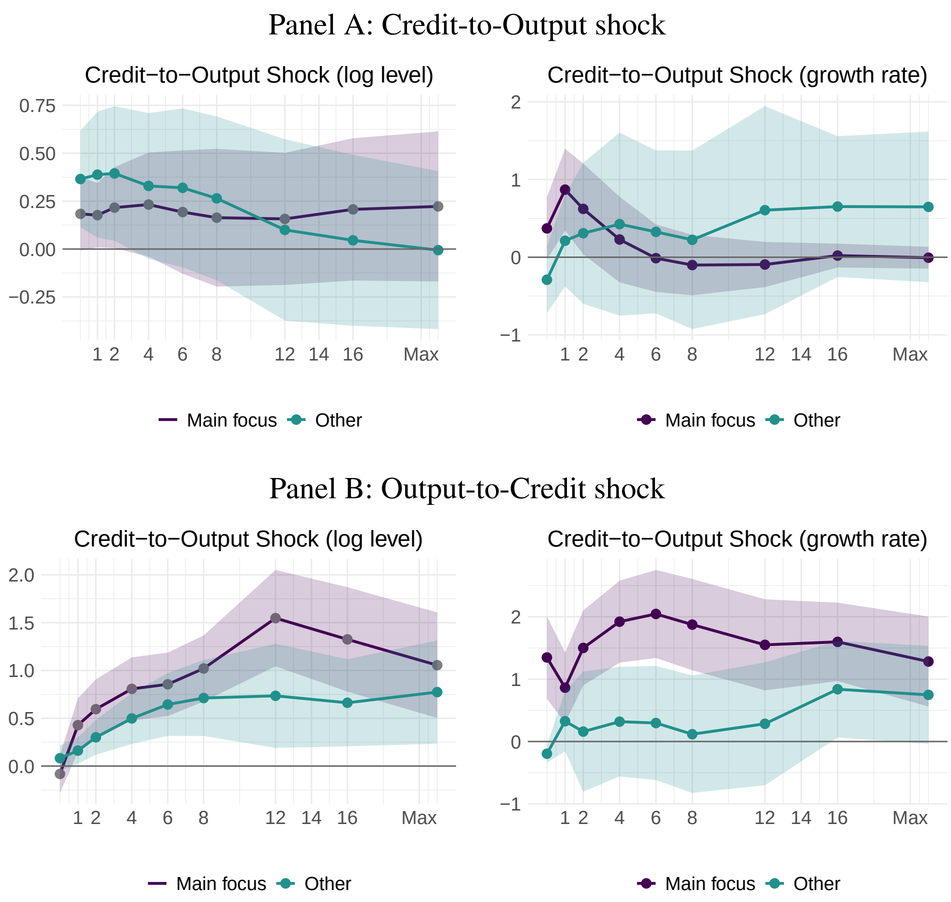
Horizon	Main focus	Other focus	Diff. in means
Short-term	1.431	0.285	1.146***
Medium-term	2.697	0.102	2.595***
Long-term	1.739	-0.023	1.762***

Panel B: Log-Level Transformation

Horizon	Main focus	Other focus	Diff. in means
Short-term	0.522	0.261	0.26
Medium-term	1.008	0.382	0.627
Long-term	1.282	0.421	0.861***

Note: Column “Diff. in means” is the difference between means of studies with the main focus and studies with the other focus. Alongside this difference, we report statistical significance (***, **, *) of this difference using Wilcoxon rank sum test with the null hypothesis that means between the two groups are equal.

Figure A4: Mean Impulse Response Functions: Main and Other Focus



Note: The figure displays the weighted average response of credit and output to a one-percentage-point increase in the credit or output, accompanied by the average 68% confidence interval. The panels compare the mainfocus studies with the non-mainfocus studies. Panel A shows credit-to-output shock, Panel B shows output-to-credit shocks.

Appendix B: Additional Information on Publication Bias Analysis

B.1 Additional Regression Results

Table B1: Effect Corrected for Publication Bias in Comparison to Average Effect from the Literature

	Short	Medium	Long
Panel A: Effect corrected for publication bias			
Credit-to-output shock (log-level)	0.255	0.224	0.128
Credit-to-output shock (growth rate)	0.610	0.109	0.095
Output-to-credit shock (log-level)	0.319	0.679	0.857
Output-to-credit shock (growth rate)	0.583	0.851	1.019
Panel B: Corrected effect as percentage of weighted uncorrected mean			
Credit-to-output shock (log-level)	55%	41%	45%
Credit-to-output shock (growth rate)	30%	84%	8%
Output-to-credit shock (log-level)	15%	53%	91%
Output-to-credit shock (growth rate)	80%	69%	72%

Table B2: Other Estimations of Publication Bias: Credit-to-Output Shock, Log-Levels

	Short	Medium	Long
Panel A: Precision-effect test (PET)			
Meta-analysis random effects			
Effect beyond bias (const.)	0.001**	0.002	-0.001
	(0.000)	(0.002)	(0.001)
Publication bias (SE)	1.06***	0.704***	0.494**
	(0.246)	(0.168)	(0.208)
I^2	55.566	43.79	8.366
Three-level meta-analysis model			
Effect beyond bias (const.)	0.077	0.021	-0.016
	(0.063)	(0.024)	(0.035)
Publication bias (SE)	0.686	0.649***	0.544***
	(0.549)	(0.092)	(0.114)
I^2 level 1 (%)	0.001	0.124	0.085
I^2 level 2 (%)	0.017	0.066	0.003
I^2 level 3 (%)	99.982	99.81	99.912
Panel B: Precision-effect estimate with standard error (PEESE)			
Meta-analysis random effects			
Effect beyond bias (const.)	0.001***	0.006***	0.000
	(0.000)	(0.002)	(0.001)
Publication bias (SE^2)	1.049**	0.669***	0.383***
	(0.464)	(0.226)	(0.087)
I^2	57.977	55.229	9.142
Three-level meta-analysis model			
Effect beyond bias (const.)	0.141***	0.091***	0.058**
	(0.026)	(0.022)	(0.028)
Publication bias (SE^2)	0.495***	0.475***	0.290***
	(0.167)	(0.081)	(0.053)
I^2 level 1 (%)	0	0.096	0.08
I^2 level 2 (%)	0.013	0.061	0.004
I^2 level 3 (%)	99.986	99.843	99.917
Observations	228	351	270
Studies	29	30	28

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: Other Estimations of Publication Bias: Credit-to-Output Shock, Growth Rates

	Short	Medium	Long
Panel A: Precision-effect test (PET)			
Meta-analysis random effects			
Effect beyond bias (const.)	0.146	0.009	0.000
	(0.102)	(0.03)	(0.001)
Publication bias (SE)	0.822***	0.445*	0.165
	(0.138)	(0.222)	(0.186)
I^2	99.998	55.795	0
Three-level meta-analysis model			
Effect beyond bias (const.)	0.048	0.022	-0.008***
	(0.035)	(0.021)	(0.003)
Publication bias (SE)	0.900***	0.414***	0.312***
	(0.168)	(0.099)	(0.065)
I^2 level 1 (%) I2-1	0.005	29.222	44.656
I^2 level 2 (%)	36.279	19.054	21.192
I^2 level 3 (%)	63.717	51.723	34.151
Panel B: Precision-effect estimate with standard error (PEESE)			
Meta-analysis random effects			
Effect beyond bias (const.)	0.277**	0.066*	0.000
	(0.121)	(0.036)	(0.001)
Publication bias (SE^2)	0.415***	0.154	0.248***
	(0.112)	(0.105)	(0.040)
I^2	99.998	58.792	0.000
Three-level meta-analysis model			
Effect beyond bias (const.)	0.185***	0.092***	0.008
	(0.027)	(0.016)	(0.006)
Publication bias (SE^2)	0.444***	0.095**	0.239***
	(0.098)	(0.048)	(0.054)
I^2 level 1 (%)	0.004	25.895	10.503
I^2 level 2 (%)	38.85	18.108	5.324
I^2 level 3 (%)	61.146	55.997	84.173
Observations	215	324	273
Studies	29	29	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 B4: Other Estimations of Publication Bias: Output-to-Credit Shock, Log-Levels

	Short	Medium	Long
Panel A: Precision-effect test (PET)			
Meta-analysis random effects			
Effect beyond bias (const.)	0.028	0.030	0.076
	(0.043)	(0.066)	(0.201)
Publication bias (SE)	1.714***	2.014***	1.406**
	(0.444)	(0.577)	(0.611)
I^2	99.946	99.975	99.996
Three-level meta-analysis model			
Effect beyond bias (const.)	0.018	0.277	1.060
	(0.082)	(1.087)	(5.793)
Publication bias (SE)	1.297**	1.199	-0.474
	(0.505)	(4.098)	(13.558)
I^2 level 1 (%)	0.032	0.005	0.001
I^2 level 2 (%)	0.006	0	0
I^2 level 3 (%)	99.963	99.995	99.999
Panel B: Precision-effect estimate with standard error (PEESE)			
Meta-analysis random effects			
Effect beyond bias (const.)	0.094	0.167	0.224
	(0.087)	(0.121)	(0.210)
Publication bias (SE^2)	3.824***	2.306***	1.044*
	(0.714)	(0.718)	(0.578)
I^2	99.962	99.984	99.996
Three-level meta-analysis model			
Effect beyond bias (const.)	0.047	0.358**	0.778***
	(0.056)	(0.145)	(0.226)
Publication bias (SE^2)	3.949***	1.709*	0.112
	(1.012)	(0.877)	(0.562)
I^2 level 1 (%)	0.037	0.005	0.001
I^2 level 2 (%)	0.001	0	0
I^2 level 3 (%)	99.962	99.995	99.999
Observations	122	192	106
Studies	14	15	13

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: Other Estimations of Publication Bias: Output-to-Credit Shock, Growth Rates

	Short	Medium	Long
Panel A: Precision-effect test (PET)			
Meta-analysis random effects			
Effect beyond bias (const.)	0.355	0.527*	-0.005
	(0.204)	(0.272)	(0.006)
Publication bias (SE)	0.394	1.092	0.837
	(0.632)	(0.650)	(0.519)
I^2	99.649	99.562	0.434
Three-level meta-analysis model			
Effect beyond bias (const.)	0.321	-0.181	0.574
	(0.250)	(0.290)	(1.353)
Publication bias (SE)	0.422	1.590***	0.319
	(0.373)	(0.378)	(2.017)
I^2 level 1 (%)	0.305	0.305	0.039
I^2 level 2 (%)	42.311	0	0
I^2 level 3 (%)	57.384	99.695	99.961
Panel B: Precision-effect estimate with standard error (PEESE)			
Meta-analysis random effects			
Effect beyond bias (const.)	0.480**	1.047**	0.004**
	(0.214)	(0.376)	(0.002)
Publication bias (SE^2)	0.136	0.311	0.586*
	(0.193)	(0.254)	(0.275)
I^2	99.653	99.657	0.215
Three-level meta-analysis model			
Effect beyond bias (const.)	0.464**	0.585***	0.738*
	(0.207)	(0.184)	(0.407)
Publication bias (SE^2)	0.111	0.400**	0.036
	(0.138)	(0.169)	(0.208)
I^2 level 1 (%)	0.304	0.318	0.035
I^2 level 2 (%)	43.376	2.836	0
I^2 level 3 (%)	56.32	96.846	99.965
Observations	96	147	112
Studies	15	15	12

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.1.1 Nonlinear Publication Bias Estimates

Table B6: Nonlinear Estimates: Credit-to-Output Shocks, Log-Levels

Method	Short	Medium	Long
Stanley et al. (2010)	0.001**	0.005***	-0.009.
	(0.000)	(0.001)	(0.005)
Ioannidis et al. (2017)	0.000	0.005***	0.000
	(0.000	(0.001)	(0.001)
Furukawa (2019)	0.000	0.004*	-0.002
	(0.001)	(0.002)	(0.005)

Note: Effects beyond the bias (const.) are shown. Standard errors in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01.

Table B7: Nonlinear Estimates: Credit-to-Output Shocks, Growth Rates

Method	Short	Medium	Long
Stanley et al. (2010)	0.031*	0.022	-0.001
	(0.016)	(0.016)	(0.003)
Ioannidis et al. (2017)	-0.046	0.011	0.003.
	(0.046)	(0.016)	(0.001)
Furukawa (2019)	0.122***	0.017	0.000
	(0.035)	(0.038)	(0.001)

Note: Effects beyond the bias (const.) are shown. Standard errors in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01.

Table B8: Nonlinear Estimates: Output-to-Credit Shocks, Log-Levels

Method	Short	Medium	Long
Stanley et al. (2010)	0.000	0.002**	0.002***
	(0.001)	(0.001)	(0.000)
Ioannidis et al. (2017)	0.000	0.001**	0.002***
	(0.001)	(0.000	(0.000
Furukawa (2019)	0.044***	0.048***	0.241***
	(0.005)	(0.008)	(0.043)

Note: Effects beyond the bias (const.) are shown. Standard in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01.

Table B9: Nonlinear Estimates: Output-to-Credit Shocks, Growth Rates

Method	Short	Medium	Long
Stanley et al. (2010)	0.063.	0.096*	0.005***
	(0.035)	(0.047)	(0.001)
Ioannidis et al. (2017)	0.016**	0.018***	0.006***
	(0.006)	(0.001)	(0.001)
Furukawa (2019)	0.017	0.743***	0.006
	(0.069)	(0.137)	(0.026)

Note: Effects beyond the bias (const.) are shown. Standard errors in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01.

B.2 IV Estimates

Tables B10–B13 present 2SLS precision–effect estimates that instrument the squared standard error, SE^2, with (i) the number of countries in each VAR and (ii) its alternative $1/\sqrt{n}$. In every specification the SE^2 coefficient is positive and significant, confirming upward bias in less precise estimates. After correction, credit-to-output semi-elasticities remain positive—especially at short horizons—whereas output-to-credit semi-elasticities show increasing effect beyond bias with longer horizons (especially in log-level studies). Results are similar across both instruments and support our conclusions from baseline results.

Table B10: 2SLS IV Estimates: Credit-to-Output Shocks, Log-Levels

	Short	**Medium**	**Long**
Instrument: n(countries)			
Effect beyond bias (const.)	0.097***	0.087*	0.092
	(0.036)	(0.052)	(0.083)
Publication bias (SE^2)	0.588***	0.453***	0.300***
	(0.166)	(0.107)	(0.062)
Instrument: $1/\sqrt{n}$			
Effect beyond bias (const.)	0.145***	0.060	0.074
	(0.037)	(0.048)	(0.081)
Publication bias (SE^2)	0.399**	0.506***	0.316***
	(0.165)	(0.113)	(0.058)
Observations	345	234	270
Studies	29	30	28

Note: Instruments: `n(countries)` is the number of countries in the VAR; $1/\sqrt{n}$ is the square root of the total number of observations in the VAR. Standard errors, clustered at the study level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B11: 2SLS IV Estimates: Credit-to-Output Shocks, Growth Rates

	Short	Medium	Long
Instrument: $n(countries)$			
Effect beyond bias (const.)	0.149**	0.036**	0.003
	(0.097)	(0.015)	(0.015)
Publication bias (SE^2)	0.400**	0.141*	0.244***
	(0.175)	(0.074)	(0.062)
Instrument: $1/\sqrt{n}$			
Effect beyond bias (const.)	0.181	0.063	−0.001
	(0.382)	(0.042)	(0.080)
Publication bias (SE^2)	0.363	0.117*	0.249**
	(0.477)	(0.067)	(0.109)
Observations	320	214	270
Studies	29	29	24

Note: Instruments: `n(countries)` is the number of countries in the VAR; $1/\sqrt{n}$ is the square root of the total number of observations in the VAR. Standard errors, clustered at the study level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B12: 2SLS IV Estimates: Output-to-Credit Shocks, Log-Levels

	Short	Medium	Long
Instrument: $n(countries)$			
Effect beyond bias (const.)	0.036**	0.120***	0.300***
	(0.017)	(0.043)	(0.090)
Publication bias (SE^2)	1.057***	2.467***	0.780**
	(0.670)	(0.531)	(0.333)
Instrument: $1/\sqrt{n}$			
Effect beyond bias (const.)	0.066**	0.216***	0.340***
	(0.032)	(0.074)	(0.102)
Publication bias (SE^2)	1.453***	1.754**	0.651
	(0.936)	(0.793)	(0.429)
Observations	186	128	106
Studies	14	15	13

Note: Instruments: `n(countries)` is the number of countries in the VAR; $1/\sqrt{n}$ is the square root of the total number of observations in the VAR. Standard errors, clustered at the study level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B13: 2SLS IV Estimates: Output-to-Credit Shocks, Growth Rates

	Short	Medium	Long
Instrument: $n(countries)$			
Effect beyond bias (const.)	0.428	0.582***	−0.543
	(0.341)	(0.189)	(0.500)
Publication bias (SE^2)	0.482**	0.583***	0.854***
	(0.192)	(0.152)	(0.221)
Instrument: $1/\sqrt{n}$			
Constant	0.366**	0.424***	−0.545
	(0.180)	(0.161)	(0.416)
Publication bias (SE^2)	0.548***	0.686***	0.855***
	(0.207)	(0.154)	(0.226)
Observations	146	98	112
Studies	15	15	12

Note: Instruments: `n(countries)` is the number of countries in the VAR; $1/\sqrt{n}$ is the square root of the total number of observations in the VAR. Standard errors, clustered at the study level, are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B.3 The Caliper Test

Caliper tests examine the distribution of reported t-statistics around standard significance thresholds (e.g., 1.65 for 10% significance, 1.96 for 5% significance). The number of t-statistics just above a threshold ("over caliper") should ideally be balanced with those just below ("under caliper"), resulting in an over-to-under ratio of 0.5 or less. According to Bruns et al. (2019), a more lenient ratio of 0.4 (60:40 distribution) is acceptable, given the low statistical power in economic studies. Without bias, t-statistic frequencies should decrease as their magnitude increases.

Tables B14 to B21 present results for significance levels corresponding to 68%, 90%, and 95% confidence intervals, using caliper sizes of 0.1, 0.2, and 0.3. Results are shown for all semi-elasticities as well as for a subset of positive semi-elasticities. The findings, covering both credit-to-output and output-to-credit relationships, reveal significant evidence of publication selection bias, particularly for positive estimates at the 68% confidence level and for credit-to-output shock elasticities. For output-to-credit shocks, the most significant estimates are observed at the 95% confidence interval.[17] We reject the null hypothesis of no p-hacking in many cases, both at the strict 0.5 threshold and the lenient 0.4 threshold.

Table B14: Caliper Test: Credit-to-Output Shocks, Log-Levels – All Effects

T-stat	C	All		Short horizon		Medium horizon		Long horizon	
	0.1	0.384	(0.288)	0.267	(0.127)	0.333	(0.078)	**0.632**	**(0.434)**
1.96	0.2	0.383	(0.309)	0.273	(0.159)	*0.441*	*(0.295)*	**0.5**	**(0.321)**
	0.3	0.357	(0.298)	0.308	(0.22)	*0.435*	*(0.311)*	0.351	(0.217)
	0.1	*0.418*	*(0.317)*	*0.419*	*(0.266)*	*0.438*	*(0.213)*	0.385	(0.134)
1.65	0.2	*0.449*	*(0.379)*	*0.413*	*(0.308)*	**0.5**	**(0.342)**	0.367	(0.215)
	0.3	*0.422*	*(0.365)*	0.378	(0.292)	*0.452*	*(0.322)*	*0.42*	*(0.302)*
	0.1	**0.509**	**(0.393)**	**0.609**	**(0.43)**	0.364	(0.088)	**0.5**	**(0.254)**
1	0.2	**0.487**	**(0.408)**	**0.556**	**(0.43)**	0.375	(0.202)	**0.5**	**(0.353)**
	0.3	*0.455*	*(0.391)*	**0.538**	**(0.434)**	0.341	(0.215)	*0.422*	*(0.297)*

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. Applies also for tables B15-B21.

[17] We had to combine medium and long horizons for output-to-credit shocks when testing positive estimates only, due to the lower number of observations, in order to ensure reliable tests.

Table B15: Caliper Test: Credit-to-Output Shocks, Log-Levels – Only Positive Effects

T-stat	C	All		Short horizon		Medium horizon		Long horizon	
	0.1	0.391	(0.293)	0.25	(0.108)	0.333	(0.078)	**0.667**	**(0.468)**
1.96	0.2	0.395	(0.318)	0.268	(0.15)	*0.441*	*(0.295)*	**0.545**	**(0.358)**
	0.3	0.371	(0.309)	0.315	(0.224)	*0.432*	*(0.305)*	*0.4*	*(0.245)*
	0.1	*0.419*	*(0.314)*	*0.433*	*(0.277)*	*0.438*	*(0.213)*	0.333	(0.023)
1.65	0.2	*0.464*	*(0.39)*	*0.424*	*(0.315)*	**0.517**	**(0.357)**	0.391	(0.213)
	0.3	*0.439*	*(0.378)*	*0.381*	(0.292)	*0.487*	*(0.35)*	*0.463*	*(0.331)*
	0.1	**0.649**	**(0.514)**	**0.706**	**(0.507)**	0.375	(0.028)	**0.778**	**(0.504)**
1	0.2	**0.583**	**(0.493)**	**0.647**	**(0.506)**	*0.412*	*(0.197)*	**0.593**	**(0.428)**
	0.3	**0.54**	**(0.466)**	**0.62**	**(0.504)**	0.367	(0.215)	**0.5**	**(0.353)**

Table B16: Caliper Test: Credit-to-Output Shocks, Growth Rates – All Effects

T-stat	C	All		Short horizon		Medium horizon		Long horizon	
	0.1	**0.54**	**(0.421)**	**0.52**	**(0.346)**	**0.5**	**(0.229)**	**0.636**	**(0.361)**
1.96	0.2	*0.44*	*(0.353)*	*0.444*	*(0.319)*	0.391	(0.213)	**0.526**	**(0.322)**
	0.3	0.399	(0.331)	0.387	(0.292)	*0.444*	*(0.303)*	0.393	(0.233)
	0.1	*0.468*	*(0.345)*	0.348	(0.173)	**0.583**	**(0.316)**	**0.556**	**(0.229)**
1.65	0.2	*0.456*	*(0.374)*	*0.442*	*(0.326)*	*0.455*	*(0.268)*	*0.462*	*(0.291)*
	0.3	0.393	(0.331)	*0.423*	*(0.329)*	0.39	(0.26)	0.333	(0.218)
	0.1	**0.528**	**(0.44)**	**0.537**	**(0.404)**	*0.478*	*(0.295)*	**0.591**	**(0.406)**
1	0.2	**0.535**	**(0.469)**	**0.552**	**(0.45)**	**0.512**	**(0.379)**	**0.556**	**(0.43)**
	0.3	*0.489*	*(0.434)*	**0.517**	**(0.428)**	*0.438*	*(0.333)*	**0.508**	**(0.403)**

Table B17: Caliper Test: Credit-to-Output Shocks, Growth Rates – Only Positive Effects

T-stat	C	All		Short horizon		Medium horizon		Long horizon	
	0.1	**0.548**	**(0.417)**	**0.565**	**(0.384)**	**0.556**	**(0.229)**	**0.5**	**(0.142)**
1.96	0.2	*0.442*	*(0.347)*	*0.488*	*(0.355)*	0.421	(0.219)	0.385	(0.134)
	0.3	*0.407*	*(0.331)*	*0.418*	*(0.317)*	*0.433*	*(0.277)*	0.353	(0.144)
	0.1	*0.459*	*(0.319)*	0.316	(0.126)	**0.556**	**(0.229)**	**0.667**	**(0.242)**
1.65	0.2	*0.476*	*(0.383)*	*0.432*	*(0.305)*	0.421	(0.219)	**0.625**	**(0.406)**
	0.3	0.393	(0.326)	*0.414*	*(0.315)*	0.361	(0.224)	0.364	(0.22)
	0.1	*0.479*	*(0.379)*	*0.486*	*(0.341)*	*0.412*	*(0.197)*	**0.529**	**(0.312)**
1	0.2	**0.54**	**(0.466)**	**0.542**	**(0.433)**	**0.517**	**(0.357)**	**0.559**	**(0.413)**
	0.3	**0.517**	**(0.455)**	**0.532**	**(0.437)**	**0.5**	**(0.369)**	**0.5**	**(0.38)**

Table B18: Caliper Test: Output-to-Credit Shocks, Log-Levels – All Effects

T-stat	C	All		Short horizon		Medium horizon		Long horizon	
1.96	0.1	**0.615**	**(0.501)**	**0.833**	**(0.676)**	**0.5**	**(0.229)**	**0.526**	**(0.322)**
	0.2	**0.562**	**(0.478)**	**0.605**	**(0.47)**	*0.455*	*(0.268)*	**0.607**	**(0.447)**
	0.3	**0.556**	**(0.484)**	**0.574**	**(0.46)**	*0.485*	*(0.335)*	**0.605**	**(0.47)**
1.65	0.1	**0.6**	**(0.458)**	*0.467*	*(0.232)*	**0.727**	**(0.472)**	**0.857**	**(0.58)**
	0.2	*0.478*	*(0.377)*	0.333	(0.185)	**0.556**	**(0.346)**	**0.688**	**(0.478)**
	0.3	**0.539**	**(0.462)**	*0.469*	*(0.349)*	**0.615**	**(0.449)**	**0.625**	**(0.478)**
1	0.1	**0.522**	**(0.339)**	**0.545**	**(0.26)**	**0.5**	**(0.049)**	**0.5**	**(0.049)**
	0.2	*0.432*	*(0.305)*	**0.5**	**(0.289)**	0.333	(0.078)	*0.462*	*(0.205)*
	0.3	*0.439*	*(0.337)*	*0.478*	*(0.295)*	0.316	(0.126)	**0.579**	**(0.377)**

Table B19: Caliper Test: Output-to-Credit Shocks, Log-Levels – Only Positive Effects

T-stat	C	All		Short horizon		Medium&Long horizon	
1.96	0.1	**0.628**	**(0.502)**	**0.8**	**(0.612)**	**0.56**	**(0.387)**
	0.2	**0.577**	**(0.483)**	**0.594**	**(0.444)**	**0.579**	**(0.442)**
	0.3	**0.579**	**(0.5)**	**0.565**	**(0.441)**	**0.608**	**(0.492)**
1.65	0.1	**0.615**	**(0.449)**	*0.462*	*(0.205)*	**0.909**	**(0.744)**
	0.2	**0.519**	**(0.402)**	0.36	(0.192)	**0.727**	**(0.56)**
	0.3	**0.598**	**(0.51)**	**0.5**	**(0.361)**	**0.714**	**(0.596)**
1	0.1	**0.5**	**(0.142)**	0.333	(0.64)	**0.6**	**(0.078)**
	0.2	0.333	(0.152)	0.25	(0.06)	*0.417*	*(0.15)*
	0.3	0.378	(0.242)	0.308	(0.07)	**0.5**	**(0.302)**

Table B20: Caliper Test: Output-to-Credit Shocks, Growth Rates – All Effects

T-stat	C	All		Short horizon		Medium horizon		Long horizon	
1.96	0.1	*0.447*	*(0.309)*	*0.4*	*(0.169)*	**0.6**	**(0.301)**	*0.4*	*(0.101)*
	0.2	*0.405*	*(0.31)*	*0.444*	*(0.278)*	**0.55**	**(0.353)**	0.25	(0.078)
	0.3	*0.417*	*(0.338)*	**0.524**	**(0.393)**	**0.5**	**(0.342)**	0.192	(0.058)
1.65	0.1	*0.412*	*(0.267)*	**0.545**	**(0.26)**	*0.429*	*(0.036)*	0.273	(0.017)
	0.2	*0.457*	*(0.357)*	**0.545**	**(0.358)**	*0.438*	*(0.213)*	*0.417*	*(0.24)*
	0.3	*0.474*	*(0.39)*	**0.531**	**(0.379)**	**0.5**	**(0.321)**	*0.419*	*(0.266)*
1	0.1	0.375	(0.156)	0.25	(-0.338)	**0.75**	**(0.162)**	0.286	(-0.073)
	0.2	0.378	(0.242)	0.3	(0.02)	**0.636**	**(0.361)**	0.267	(0.059)
	0.3	0.373	(0.267)	*0.438*	*(0.213)*	**0.562**	**(0.338)**	0.286	(0.111)

Table B21: Caliper Test: Output-to-Credit Shocks, Growth Rates – Only Positive Effects

T-stat	C	All		Short horizon		Medium&Long horizon	
	0.1	*0.484*	*(0.329)*	0.364	(0.088)	**0.588**	**(0.373)**
1.96	0.2	*0.433*	*(0.326)*	*0.409*	*(0.224)*	*0.469*	*(0.317)*
	0.3	*0.448*	*(0.359)*	**0.5**	**(0.353)**	*0.413*	*(0.29)*
	0.1	*0.48*	*(0.306)*	*0.444*	*(0.118)*	*0.462*	*(0.205)*
1.65	0.2	**0.5**	**(0.385)**	**0.526**	**(0.322)**	*0.467*	*(0.309)*
	0.3	**0.5**	**(0.405)**	*0.481*	*(0.314)*	**0.5**	**(0.372)**
	0.1	0.308	(0.07)	0.25	(-0.338)	0.333	(0.023)
1	0.2	*0.414*	*(0.255)*	0.3	(0.02)	*0.474*	*(0.27)*
	0.3	*0.422*	*(0.297)*	*0.438*	*(0.213)*	*0.444*	*(0.278)*

Appendix C: Additional Information on Heterogeneity Analysis

C.1 Summary Statistics of the Main Study Characteristics

Table C1: Summary Statistics: Credit-to-Output Shocks, Log-Levels

	Unweighted						Weighted					
	Short		Medium		Long		Short		Medium		Long	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Semi-Elasticity	0.25	(0.41)	0.29	(0.54)	0.43	(0.79)	0.26	(0.38)	0.19	(0.47)	0.13	(0.49)
SE2	0.21	(0.36)	0.44	(0.74)	0.92	(1.88)	0.15	(0.28)	0.29	(0.54)	0.37	(1.03)
Data Characteristics												
Real Output	0.88	(0.33)	0.88	(0.33)	0.89	(0.32)	0.86	(0.35)	0.87	(0.34)	0.87	(0.34)
SA Output	0.51	(0.50)	0.50	(0.50)	0.61	(0.49)	0.55	(0.50)	0.54	(0.50)	0.58	(0.50)
Real Credit	0.42	(0.49)	0.43	(0.50)	0.34	(0.47)	0.43	(0.50)	0.44	(0.50)	0.44	(0.50)
SA Credit	0.36	(0.48)	0.35	(0.48)	0.44	(0.50)	0.28	(0.45)	0.27	(0.44)	0.30	(0.46)
Pub. Year	2.77	(0.58)	2.77	(0.58)	2.95	(0.45)	2.68	(0.67)	2.68	(0.66)	2.78	(0.59)
Study Length (Y)	3.06	(0.29)	3.06	(0.29)	3.08	(0.30)	3.00	(0.37)	3.01	(0.37)	2.99	(0.38)
Multiple Countr.	0.19	(0.39)	0.19	(0.39)	0.23	(0.42)	0.13	(0.33)	0.12	(0.33)	0.14	(0.35)
North America	0.32	(0.47)	0.32	(0.47)	0.30	(0.46)	0.38	(0.49)	0.37	(0.48)	0.33	(0.47)
Europe	0.30	(0.46)	0.32	(0.47)	0.26	(0.44)	0.33	(0.47)	0.35	(0.48)	0.35	(0.48)
Corporate Credit	0.13	(0.34)	0.15	(0.35)	0.13	(0.34)	0.27	(0.44)	0.29	(0.45)	0.27	(0.45)
Model Specification and Estimation												
Controls: House Prices	0.19	(0.39)	0.21	(0.40)	0.09	(0.29)	0.16	(0.37)	0.18	(0.39)	0.17	(0.37)
Controls: Short IR	0.75	(0.43)	0.74	(0.44)	0.66	(0.47)	0.90	(0.30)	0.87	(0.33)	0.87	(0.34)
Controls: Long IR	0.23	(0.42)	0.23	(0.42)	0.24	(0.43)	0.17	(0.38)	0.17	(0.37)	0.15	(0.36)
Sign Rest. Ident.	0.33	(0.47)	0.32	(0.47)	0.39	(0.49)	0.35	(0.48)	0.34	(0.47)	0.39	(0.49)
No. of Lags	2.81	(1.55)	2.79	(1.55)	2.73	(1.52)	3.53	(2.23)	3.46	(2.23)	3.39	(2.10)
Simple VAR	0.57	(0.50)	0.58	(0.49)	0.49	(0.50)	0.87	(0.33)	0.88	(0.33)	0.87	(0.34)
Bayesian Est.	0.16	(0.36)	0.15	(0.36)	0.20	(0.40)	0.26	(0.44)	0.25	(0.43)	0.30	(0.46)
Publication Characteristics												
Published	0.68	(0.47)	0.67	(0.47)	0.81	(0.40)	0.67	(0.47)	0.65	(0.48)	0.70	(0.46)
Impact Factor	0.49	(0.45)	0.49	(0.45)	0.38	(0.43)	0.43	(0.48)	0.42	(0.48)	0.38	(0.49)
Citations	1.89	(1.00)	1.84	(1.05)	1.63	(1.06)	1.43	(1.13)	1.36	(1.17)	1.21	(1.15)
Main Focus	0.69	(0.46)	0.68	(0.47)	0.78	(0.42)	0.58	(0.49)	0.56	(0.50)	0.57	(0.50)

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 C2: Summary Statistics: Credit-to-Output Shocks, Growth Rates

	Unweighted						Weighted					
	Short		Medium		Long		Short		Medium		Long	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Semi-Elasticity	0.42	(0.57)	0.18	(0.51)	0.21	(0.75)	0.36	(0.56)	0.12	(0.45)	0.06	(0.68)
SE2	0.47	(0.80)	0.66	(1.28)	0.93	(2.20)	0.53	(0.94)	0.69	(1.46)	0.81	(2.06)
Data Characteristics												
Real Output	0.90	(0.30)	0.90	(0.30)	0.94	(0.24)	0.75	(0.43)	0.74	(0.44)	0.80	(0.40)
SA Output	0.61	(0.49)	0.61	(0.49)	0.62	(0.49)	0.59	(0.49)	0.58	(0.50)	0.57	(0.50)
Real Credit	0.58	(0.49)	0.57	(0.50)	0.59	(0.49)	0.55	(0.50)	0.54	(0.50)	0.59	(0.49)
SA Credit	0.54	(0.50)	0.54	(0.50)	0.55	(0.50)	0.49	(0.50)	0.48	(0.50)	0.51	(0.50)
Pub. Year	3.04	(0.28)	3.04	(0.28)	3.09	(0.20)	2.99	(0.30)	2.99	(0.30)	3.06	(0.22)
Study Length (Y)	3.26	(0.32)	3.26	(0.32)	3.30	(0.30)	3.15	(0.49)	3.15	(0.49)	3.21	(0.46)
Multiple Countr.	0.20	(0.40)	0.20	(0.40)	0.17	(0.38)	0.25	(0.43)	0.26	(0.44)	0.23	(0.42)
North America	0.37	(0.48)	0.37	(0.48)	0.39	(0.49)	0.21	(0.41)	0.21	(0.41)	0.21	(0.41)
Europe	0.42	(0.49)	0.43	(0.50)	0.45	(0.50)	0.45	(0.50)	0.45	(0.50)	0.52	(0.50)
Corporate Credit	0.24	(0.43)	0.24	(0.43)	0.27	(0.45)	0.08	(0.28)	0.08	(0.27)	0.10	(0.30)
Model Specification and Estimation												
Controls: House Prices	0.21	(0.41)	0.21	(0.41)	0.16	(0.37)	0.29	(0.45)	0.28	(0.45)	0.23	(0.42)
Controls: Short IR	0.82	(0.39)	0.81	(0.39)	0.89	(0.32)	0.61	(0.49)	0.60	(0.49)	0.67	(0.47)
Controls: Long IR	0.31	(0.46)	0.31	(0.46)	0.30	(0.46)	0.26	(0.44)	0.25	(0.44)	0.19	(0.39)
Sign Rest. Ident.	0.37	(0.48)	0.37	(0.48)	0.42	(0.49)	0.30	(0.46)	0.30	(0.46)	0.38	(0.48)
No. of Lags	2.32	(0.81)	2.31	(0.81)	2.26	(0.80)	2.38	(0.87)	2.37	(0.87)	2.23	(0.83)
Simple VAR	0.71	(0.45)	0.71	(0.45)	0.71	(0.46)	0.79	(0.41)	0.79	(0.41)	0.80	(0.40)
Bayesian Est	0.29	(0.45)	0.29	(0.45)	0.30	(0.46)	0.31	(0.46)	0.31	(0.46)	0.33	(0.47)
Publication Characteristics												
Published	0.85	(0.35)	0.85	(0.36)	0.89	(0.31)	0.80	(0.40)	0.79	(0.41)	0.81	(0.39)
Impact Factor	0.34	(0.34)	0.33	(0.34)	0.33	(0.35)	0.22	(0.25)	0.22	(0.25)	0.17	(0.24)
Citations	2.03	(1.25)	2.02	(1.25)	2.06	(1.25)	1.36	(1.33)	1.33	(1.34)	1.15	(1.35)
Main Focus	0.66	(0.48)	0.66	(0.48)	0.69	(0.46)	0.70	(0.46)	0.71	(0.46)	0.80	(0.40)

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: Output-to-Credit Shocks, Log-Levels

	Unweighted						Weighted					
	Short		Medium		Long		Short		Medium		Long	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Semi-Elasticity	0.29	(0.42)	0.45	(0.68)	0.52	(0.86)	0.39	(0.52)	0.73	(0.88)	0.83	(0.92)
SE2	0.04	(0.08)	0.11	(0.23)	0.27	(0.50)	0.08	(0.10)	0.20	(0.26)	0.47	(0.63)
Data Characteristics												
Real Credit	0.55	(0.50)	0.56	(0.50)	0.57	(0.50)	0.43	(0.50)	0.46	(0.50)	0.52	(0.50)
SA Output	0.47	(0.50)	0.45	(0.50)	0.25	(0.44)	0.57	(0.50)	0.54	(0.50)	0.44	(0.50)
Pub. Year	2.09	(0.88)	2.09	(0.87)	2.48	(0.54)	2.12	(0.90)	2.11	(0.88)	2.40	(0.64)
Study Length (Y)	3.12	(0.37)	3.12	(0.36)	3.09	(0.38)	3.01	(0.38)	3.03	(0.38)	2.96	(0.38)
Multiple Coun.	0.34	(0.47)	0.33	(0.47)	0.59	(0.49)	0.37	(0.49)	0.35	(0.48)	0.48	(0.50)
North America	0.66	(0.47)	0.64	(0.48)	0.75	(0.44)	0.64	(0.48)	0.61	(0.49)	0.56	(0.50)
Europe	0.23	(0.42)	0.25	(0.43)	0.11	(0.32)	0.16	(0.37)	0.21	(0.41)	0.20	(0.40)
Corporate Credit	0.19	(0.40)	0.22	(0.42)	0.19	(0.39)	0.17	(0.38)	0.21	(0.41)	0.17	(0.38)
Model Specification and Estimation												
Controls: House Prices	0.32	(0.47)	0.34	(0.48)	0.17	(0.38)	0.26	(0.44)	0.30	(0.46)	0.29	(0.46)
Controls: Short IR	0.95	(0.22)	0.92	(0.27)	0.92	(0.28)	0.88	(0.32)	0.84	(0.37)	0.89	(0.32)
No. of Lags	2.70	(1.16)	2.66	(1.17)	2.34	(0.89)	2.69	(1.11)	2.61	(1.13)	2.51	(1.08)
Bayesian	0.26	(0.44)	0.25	(0.43)	0.21	(0.41)	0.14	(0.35)	0.14	(0.35)	0.11	(0.32)
Publication Characteristics												
Impact Factor	0.61	(0.48)	0.59	(0.48)	0.63	(0.47)	0.61	(0.68)	0.58	(0.67)	0.41	(0.50)
Citations	1.72	(0.77)	1.64	(0.87)	1.50	(0.92)	1.57	(0.79)	1.44	(0.92)	1.30	(0.98)
Main Focus	0.11	(0.32)	0.11	(0.31)	0.14	(0.35)	0.41	(0.49)	0.38	(0.49)	0.42	(0.50)

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: Summary Statistics: Output-to-Credit Shocks, Growth Rates

	Unweighted						Weighted					
	Short		Medium		Long		Short		Medium		Long	
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Semi-Elasticity	0.88	(1.42)	1.50	(1.86)	1.18	(2.34)	0.75	(1.38)	1.00	(1.65)	0.83	(2.10)
SE2	0.72	(1.08)	1.32	(1.53)	1.83	(3.12)	0.75	(1.16)	1.07	(1.37)	1.48	(2.60)
Data Characteristics												
Real Credit	0.52	(0.50)	0.51	(0.50)	0.47	(0.50)	0.51	(0.50)	0.47	(0.50)	0.45	(0.50)
SA Output	0.71	(0.46)	0.69	(0.46)	0.71	(0.46)	0.72	(0.45)	0.68	(0.47)	0.64	(0.49)
Pub. Year	2.30	(0.76)	2.30	(0.76)	2.48	(0.33)	2.35	(0.78)	2.36	(0.78)	2.58	(0.32)
Study Length (Y)	3.08	(0.39)	3.08	(0.39)	3.16	(0.37)	3.03	(0.61)	3.04	(0.60)	3.24	(0.62)
Multiple Coun.	0.61	(0.49)	0.61	(0.49)	0.62	(0.49)	0.62	(0.49)	0.63	(0.49)	0.68	(0.47)
North America	0.41	(0.49)	0.41	(0.49)	0.51	(0.50)	0.47	(0.50)	0.46	(0.50)	0.61	(0.49)
Europe	0.04	(0.20)	0.04	(0.20)	0.04	(0.21)	0.12	(0.33)	0.12	(0.33)	0.15	(0.36)
Household Credit	0.10	(0.30)	0.10	(0.30)	0.05	(0.23)	0.18	(0.39)	0.20	(0.41)	0.21	(0.41)
Model Specification and Estimation												
Controls: House Prices	0.68	(0.47)	0.67	(0.47)	0.68	(0.47)	0.49	(0.50)	0.46	(0.50)	0.40	(0.50)
Controls: Short IR	0.77	(0.42)	0.76	(0.43)	0.89	(0.31)	0.55	(0.50)	0.51	(0.51)	0.61	(0.49)
No. of Lags	2.56	(0.90)	2.55	(0.91)	2.67	(0.96)	2.47	(0.88)	2.44	(0.90)	2.43	(0.92)
Bayesian	0.12	(0.33)	0.12	(0.33)	0.16	(0.37)	0.06	(0.25)	0.06	(0.24)	0.10	(0.30)
Publication Characteristics												
Impact Factor	0.15	(0.14)	0.15	(0.14)	0.12	(0.09)	0.16	(0.18)	0.16	(0.18)	0.11	(0.11)
Citations	1.58	(1.23)	1.55	(1.24)	1.46	(1.18)	1.33	(1.55)	1.25	(1.57)	0.93	(1.54)
Main Focus	0.54	(0.50)	0.55	(0.50)	0.71	(0.45)	0.46	(0.50)	0.49	(0.51)	0.74	(0.44)

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.

C.2 Bayesian Model Averaging

C.2.1 Full Regression Tables and Model Inclusion Charts

Table C5: Bayesian Model Averaging: Credit-to-Output Shocks, Log-Levels

	Short horizon			Medium horizon			Long horizon		
	P. mean	P. SD	PIP	P. mean	P. SD	PIP	P. mean	P. SD	PIP
SE2	**0.6803**	0.1116	1	**0.4390**	0.1132	1	**0.3828**	0.1952	0.9820
Large SE	-0.1353	0.1681	0.4356	-0.0090	0.0484	0.0793	**0.2618**	0.1036	0.9639
Large SE * SE2	**-0.2459**	0.2129	0.6167	-0.0247	0.0830	0.1266	-0.1355	0.2147	0.3643
(Intercept)	**0.2494**		1	**-0.3251**		1	**-2.2728**		1
Data Characteristics									
Real Output	**-0.2323**	0.0593	0.9977	-0.0687	0.0919	0.4274	-0.0075	0.0373	0.1088
SA Output	0.0165	0.0464	0.1625	0.0351	0.0715	0.2523	0.0066	0.0277	0.0986
Real Credit	0.0650	0.0809	0.4750	**0.2367**	0.0885	0.9675	0.0420	0.0700	0.3389
SA Credit	0.0239	0.0606	0.1899	0.0160	0.0520	0.1386	0.0067	0.0336	0.0974
Pub. Year	**0.1384**	0.0850	0.8165	0.0102	0.0446	0.1010	0.0083	0.0313	0.1248
Study Length (Y)	-0.0197	0.0482	0.1936	0.0412	0.0754	0.2968	**0.1804**	0.1019	0.8199
Multiple Countr.	**-0.2076**	0.0653	0.9922	**-0.1314**	0.1015	0.7208	-0.0092	0.0318	0.1213
North America	0.0053	0.0264	0.0865	0.0190	0.0566	0.1539	0.0792	0.1126	0.4139
Europe	0.0336	0.0557	0.3290	0.0309	0.0667	0.2383	**0.1899**	0.0724	0.9554
Corporate Credit	0.0031	0.0175	0.0713	0.0009	0.0144	0.0471	**-0.0683**	0.0735	0.5378
Model Specification and Estimation									
Controls: House Prices	0.0022	0.0296	0.0819	-0.0219	0.0726	0.1418	-0.0001	0.0148	0.0585
Controls: Short IR	0.0031	0.0272	0.0749	0.0119	0.0580	0.1307	-0.0036	0.0259	0.0710
Controls: Long IR	0.0803	0.0992	0.4727	0.0040	0.0316	0.0684	-0.0006	0.0208	0.0559
Sign Rest. Ident.	**0.1785**	0.1075	0.8131	**0.1801**	0.1218	0.7652	**0.2058**	0.0909	0.9057
No. of Lags	0.0011	0.0140	0.0532	0.0034	0.0207	0.0654	-0.0119	0.0356	0.1488
Simple VAR	-0.0111	0.0432	0.1176	**-0.2929**	0.1217	0.9323	-0.0478	0.0968	0.2651
Bayesian Est.	**0.1768**	0.0835	0.9074	**0.1819**	0.0974	0.8617	0.0021	0.0172	0.0642
Publication Characteristics									
Published	-0.0032	0.0214	0.0699	0.0285	0.0695	0.2000	0.0615	0.0966	0.3626
Impact Factor	0.0079	0.0314	0.1044	0.0663	0.1036	0.3649	0.0064	0.0378	0.0976
Citations	0.0012	0.0142	0.0523	0.0580	0.0909	0.3574	**0.1960**	0.0865	0.9056
Main Focus	-0.0007	0.0134	0.0512	-0.0003	0.0156	0.0439	-0.0003	0.0203	0.0598

Note: 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. Variable names and posterior means in bold for variables with PIP > 0.5.

Table C6: Bayesian Model Averaging: Credit-to-Output Shocks, Growth Rates

	Short horizon			Medium horizon			Long horizon		
	P. mean	P. SD	PIP	P. mean	P. SD	PIP	P. mean	P. SD	PIP
SE2	**0.5103**	0.0819	1	**0.8889**	0.1684	1	**0.6139**	0.1482	1
Large SE	-0.0100	0.0458	0.0845	**0.6600**	0.3565	0.8636	**-0.2475**	0.1315	0.8636
Large SE * SE2	-0.0149	0.0597	0.0973	**-1.6198**	0.3952	1	-0.0124	0.1073	0.0866
(Intercept)	**0.3525**		1	**0.0363**		1	**0.1873**		1
Data Characteristics									
Real Output	0.0002	0.0104	0.0433	0.0177	0.0476	0.1758	0.0085	0.0302	0.1165
SA Output	0.0017	0.0213	0.0614	**0.1569**	0.1554	0.5948	0.0464	0.1017	0.2482
Real Credit	-0.0007	0.0178	0.0571	0.0215	0.0626	0.1573	0.0419	0.0786	0.2794
SA Credit	0.0054	0.0248	0.0854	0.0062	0.0353	0.0871	0.0083	0.0332	0.1039
Pub. Year	-0.0016	0.0163	0.0576	0.0262	0.0685	0.1888	-0.0020	0.0206	0.0615
Study Length (Y)	-0.0002	0.0111	0.0441	-0.0021	0.0182	0.0619	-0.0003	0.0133	0.0496
Multiple Countr.	-0.0034	0.0208	0.0683	-0.0205	0.0580	0.1697	-0.0172	0.0532	0.1528
North America	-0.0033	0.0212	0.0692	0.0004	0.0207	0.0575	0.0026	0.0228	0.0656
Europe	0.0088	0.0315	0.1128	0.0192	0.0496	0.1826	0.0017	0.0175	0.0563
Corporate Credit	-0.0012	0.0144	0.0522	0.0223	0.0583	0.1830	-0.0001	0.0157	0.0535
Model Specification and Estimation									
Controls: House Prices	-0.0170	0.0467	0.1647	**-0.1081**	0.1173	0.5418	-0.1012	0.1269	0.4830
Controls: Short IR	0.0140	0.0391	0.1607	**0.2501**	0.0951	0.9489	0.0112	0.0362	0.1321
Controls: Long IR	-0.0170	0.0465	0.1659	0.0036	0.0247	0.0683	0.0011	0.0254	0.0682
Sign Rest. Ident.	**0.0922**	0.0926	0.5640	**-0.5075**	0.1271	0.9999	**-0.1845**	0.1567	0.7033
No. of Lags	-0.0112	0.0372	0.1260	**-0.1094**	0.1124	0.5724	-0.0042	0.0283	0.0751
Simple VAR	0.0001	0.0153	0.0515	0.0099	0.0421	0.1118	-0.0034	0.0212	0.0689
Bayesian Est.	0.0357	0.0670	0.2761	-0.0042	0.0318	0.0781	0.0041	0.0310	0.0710
Published	-0.0025	0.0169	0.0617	0.0349	0.0703	0.2669	-0.0033	0.0214	0.0705
Publication Characteristics									
Impact Factor	0.0246	0.0572	0.2060	-0.0707	0.1106	0.3593	0.0068	0.0389	0.0821
Citations	0.0015	0.0180	0.0577	**-0.1195**	0.1155	0.5857	0.0004	0.0166	0.0545
Main Focus	0.0095	0.0357	0.1100	0.0092	0.0441	0.1012	-0.0022	0.0267	0.0641

Note: 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. Variable names and posterior means in bold for variables with PIP > 0.5.

Table C7: Bayesian Model Averaging: Output-to-Credit Shocks, Log-Levels

	Short horizon			Medium horizon			Long horizon		
	P. mean	P. SD	PIP	P. mean	P. SD	PIP	P. mean	P. SD	PIP
SE2	**0.6187**	0.0617	1	**0.7139**	0.1478	1.0000	**0.1813**	0.1758	0.6145
Large SE	-0.0001	0.0236	0.0756	-0.0156	0.0646	0.1379	-0.0400	0.1563	0.2208
Large SE ∗ SE2	-0.0014	0.0354	0.0825	-0.0486	0.1364	0.1857	0.1080	0.2184	0.3678
(Intercept)	**-1.2560**		1	**-1.4905**		1	**1.0823**		1
Data Characteristics									
Real Credit	**-0.1131**	0.0863	0.7258	**-0.0962**	0.0889	0.6292	**-0.1788**	0.1724	0.6217
SA Output	**0.1687**	0.1185	0.7647	0.0455	0.0861	0.2975	**1.0604**	0.1777	1
Pub. Year (ln)	**0.8335**	0.1695	1	**1.0898**	0.1612	1	**-0.5072**	0.3107	0.8048
Study Length (Y)	0.0175	0.0483	0.1864	-0.0111	0.0543	0.1865	**2.3366**	0.5613	1.0000
Multiple Coun.	**-0.7680**	0.1758	1.0000	**-1.0239**	0.1801	1.0000	**3.3578**	0.6101	1
North America	-0.0051	0.0370	0.1050	-0.0174	0.0596	0.1693	0.1426	0.7140	0.4637
Europe	-0.0003	0.0181	0.0753	-0.0030	0.0287	0.1010	**2.6310**	0.4477	1
Corporate Credit	**0.3315**	0.0605	1.0000	**0.3133**	0.0697	0.9998	**0.1325**	0.1227	0.6308
Model Specification and Estimation									
Controls: House Prices	-0.0211	0.0550	0.1889	-0.0736	0.1055	0.4119	0.0009	0.0295	0.0918
Controls: Short IR	**0.4752**	0.0669	1.0000	**0.5635**	0.1089	0.9990	**-2.2762**	0.7200	0.9969
No. of Lags	-0.0015	0.0151	0.0768	-0.0057	0.0254	0.1192	**-0.3435**	0.2735	0.8278
Bayesian	**-0.5955**	0.2212	0.9753	**-0.9191**	0.2268	0.9922	**4.1909**	0.6477	1
Publication Characteristics									
Impact Factor	**0.4303**	0.0845	0.9990	**0.3325**	0.1128	0.9589	**-1.7358**	0.7334	0.9617
Citations (ln)	-0.0029	0.0480	0.1118	0.0735	0.1275	0.3494	**5.1522**	1.4019	0.9999
Main Focus	-0.0305	0.0670	0.2530	-0.0049	0.0477	0.1560	**2.2202**	0.4289	1

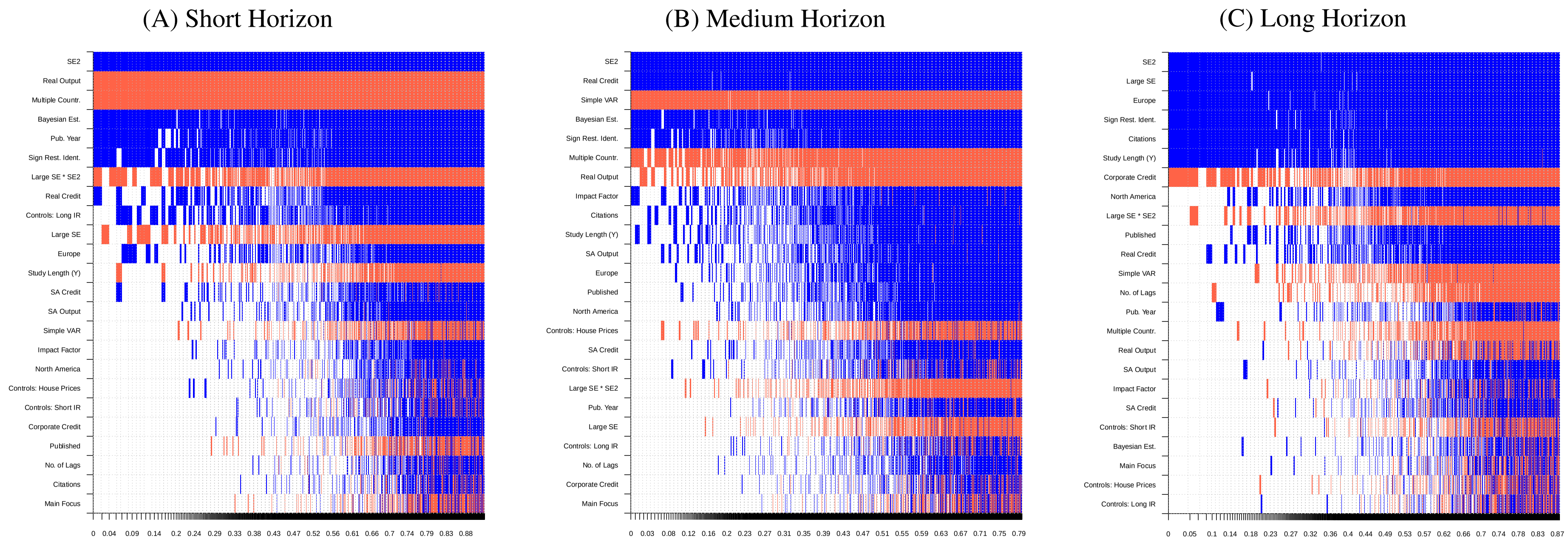
Note: The BMA procedure employs a combination of the uniform model prior and the unit information g-prior recommended by Eicher et al. (2011). The coefficients are standardized. P. mean, P. SD, and PIP stand for posterior mean, posterior standard deviation, and posterior inclusion probability, respectively. Variable names and posterior means in bold for variables with PIP > 0.5.

Table C8: Bayesian Model Averaging: Output-to-Credit Shocks, Growth Rates

	Short horizon			Medium horizon			Long horizon		
	P. mean	P. SD	PIP	P. mean	P. SD	PIP	P. mean	P. SD	PIP
SE2	**0.5764**	0.2029	1	**0.5675**	0.1345	1.0000	0.0035	0.0325	0.1144
Large SE	-0.0871	0.1727	0.3066	**-0.2426**	0.2144	0.6221	0.0018	0.0192	0.0986
Large SE * SE2	**-0.2364**	0.2451	0.5706	-0.2093	0.2578	0.4680	0.0070	0.0302	0.1325
(Intercept)	**-1.4513**		1	**-1.4852**		1	**2.2910**		1
Data Characteristics									
Real Credit	**0.3930**	0.2268	0.8208	-0.0066	0.0581	0.1481	0.0027	0.1667	0.3150
SA Output	-0.0141	0.0533	0.1338	-0.0649	0.0918	0.4238	-0.0015	0.0200	0.0929
Pub. Year (ln)	-0.0594	0.1957	0.2469	-0.0169	0.0931	0.1913	**-0.7709**	0.4163	0.8548
Study Length (Y)	0.0129	0.0462	0.1358	0.0053	0.0316	0.1225	-0.0563	0.1407	0.3394
Multiple Coun.	-0.0080	0.0485	0.1149	0.0484	0.0774	0.3672	0.0366	0.0698	0.3256
North America	-0.0145	0.0491	0.1441	-0.0536	0.0831	0.3781	-0.0390	0.0702	0.3334
Europe	-0.0037	0.0337	0.0951	-0.0349	0.0636	0.3165	0.0846	0.1257	0.4531
Household Credit	-0.0116	0.0527	0.1160	0.0229	0.0628	0.2092	**0.3232**	0.0694	0.9997
Model Specification and Estimation									
Controls: House Prices	0.0022	0.0386	0.0900	0.0145	0.0454	0.1783	-0.0492	0.1501	0.3664
Controls: Short IR	**0.3343**	0.1125	0.9664	**0.2817**	0.1069	0.9522	-0.0462	0.1243	0.4401
No. of Lags	0.0916	0.1768	0.3104	**0.1652**	0.1540	0.6666	**0.7769**	0.3721	0.8724
Bayesian	**-0.5242**	0.1496	0.9898	-0.0087	0.0391	0.1376	**0.2758**	0.1986	0.8212
Publication Characteristics									
Impact Factor	**0.5257**	0.2622	0.8537	**0.3381**	0.1531	0.9056	0.1796	0.3062	0.3710
Citations (ln)	**-0.4006**	0.1366	0.9607	0.0065	0.0414	0.1220	**-0.1534**	0.2411	0.7258
Main Focus	**0.7250**	0.1614	0.9998	**0.6335**	0.0841	1	**0.1447**	0.1729	0.5348

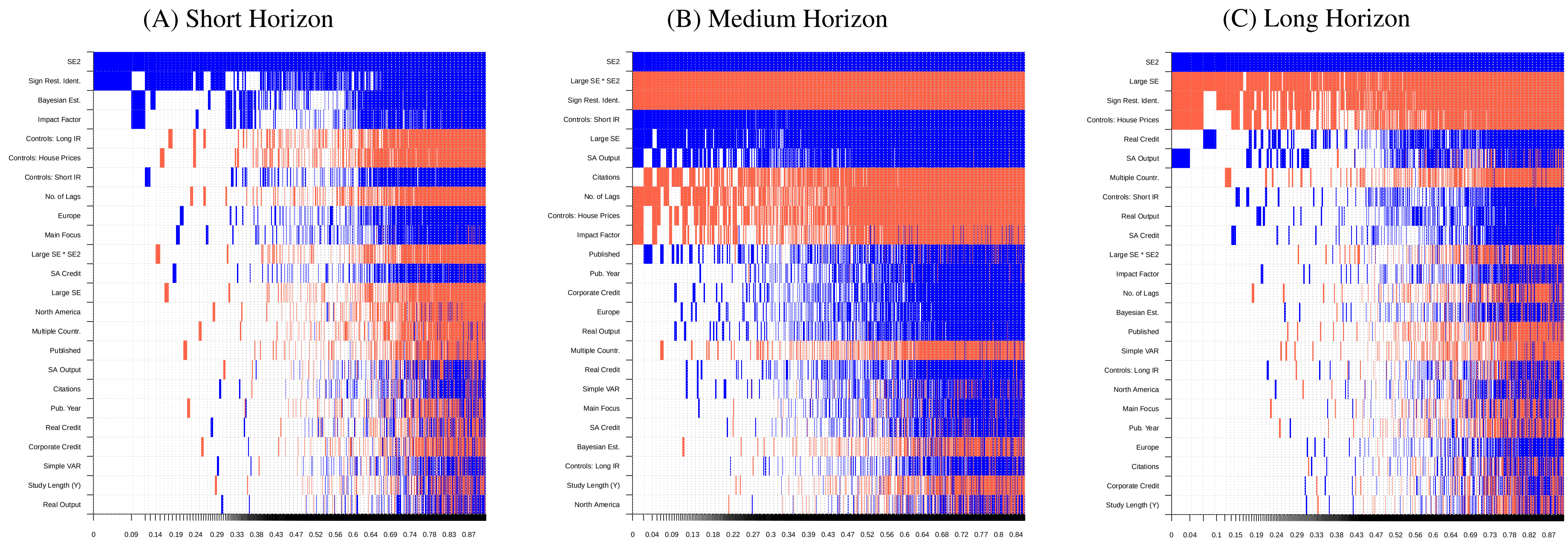
Note: The BMA procedure employs a combination of the uniform model prior and the unit information g-prior recommended by Eicher et al. (2011). The coefficients are standardized. P. mean, P. SD, and PIP stand for posterior mean, posterior standard deviation, and posterior inclusion probability, respectively. Variable names and posterior means in bold for variables with PIP > 0.5.

Figure C1: Model Inclusion in Bayesian Model Averaging: Credit-to-Output Shocks, Log Levels, Across Horizons



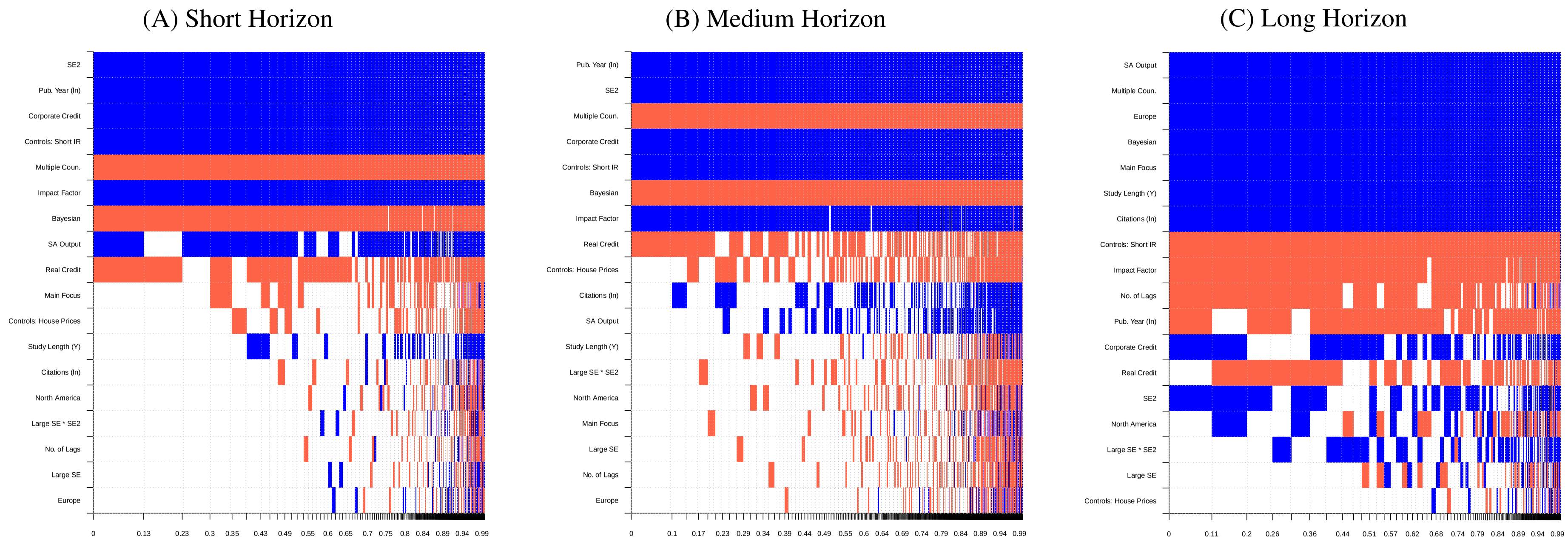
Note: Columns denote individual models; the variables are sorted by posterior inclusion probability in descending order. The horizontal axis denotes the cumulative posterior model probabilities; the 10,000 best models are shown. To ensure convergence, we employ 3 million iterations and 1 million burn-ins.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

Figure C2: Model Inclusion in Bayesian Model Averaging: Credit-to-Output Shocks, Growth Rates, Across Horizons



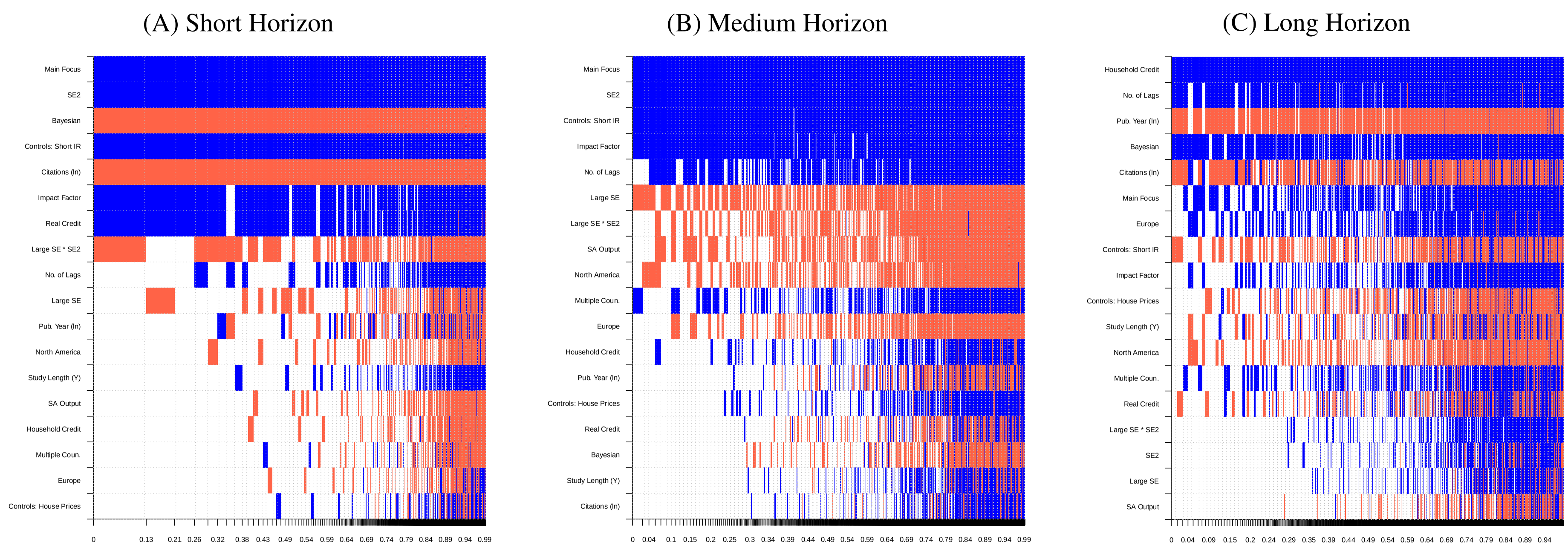
Note: Columns denote individual models; the variables are sorted by posterior inclusion probability in descending order. The horizontal axis denotes the cumulative posterior model probabilities; the 10,000 best models are shown. To ensure convergence, we employ 3 million iterations and 1 million burn-ins.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

Figure C3: Model Inclusion in Bayesian Model Averaging: Output-to-Credit Shocks, Log Levels, Across Horizons



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 effect is stronger, given that the mean effect is positive. Red color (lighter in grayscale) indicates that the variable is included and the estimated sign is negative, i.e., the effect is weaker, given that the mean effect is positive. No color indicates that the variable is not included in the model.

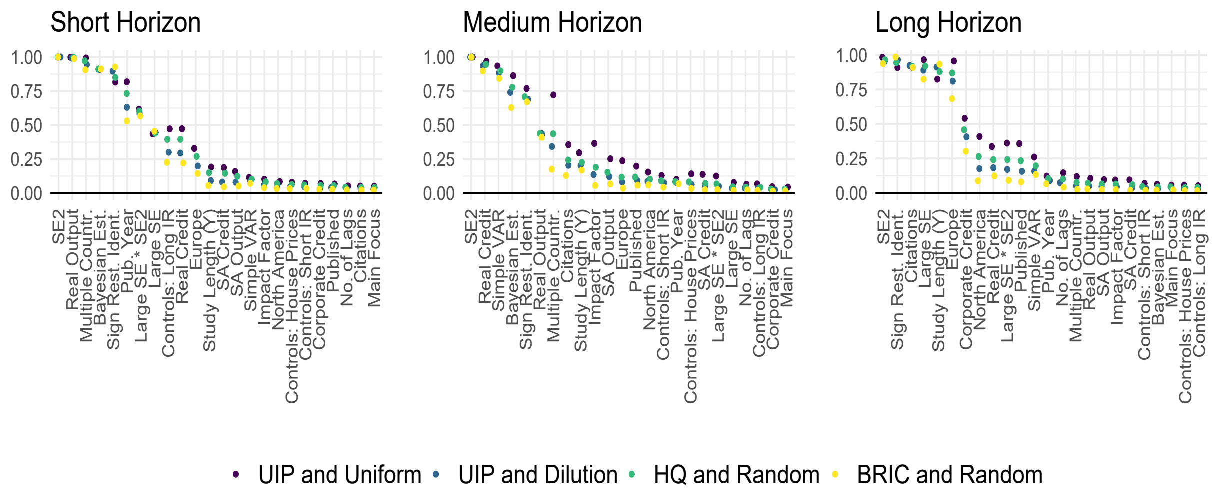
Figure C4: Model Inclusion in Bayesian Model Averaging: Output-to-Credit Shock, Growth Rates, Across Horizons



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 effect is stronger, given that the mean effect is positive. Red color (lighter in grayscale) indicates that the variable is included and the estimated sign is negative, i.e., the effect is weaker, given that the mean effect is positive. No color indicates that the variable is not included in the model.

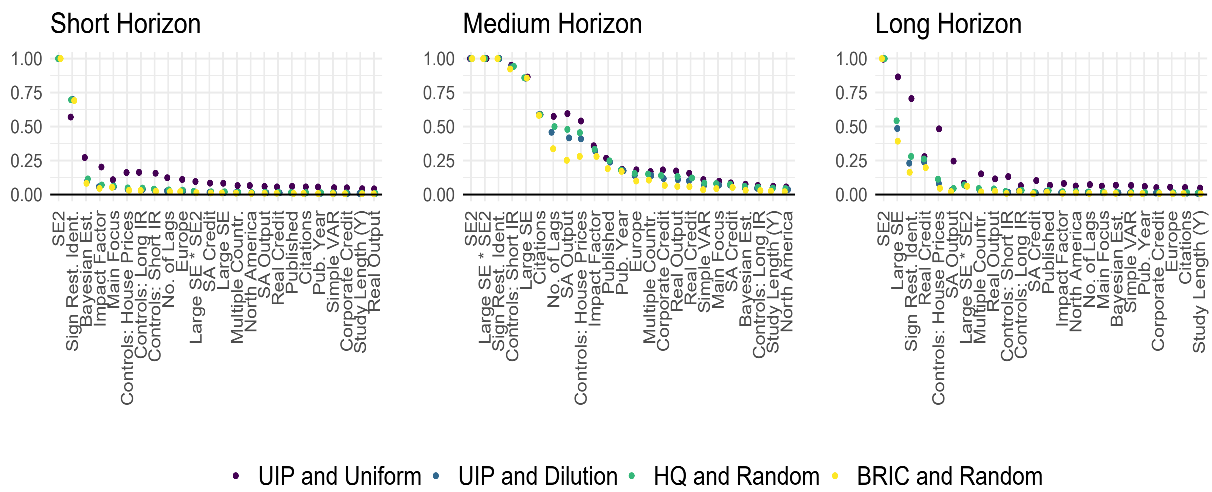
C.2.2 Prior Sensitivity Check

Figure C5: Prior Sensitivity Check in Bayesian Model Averaging: Credit-to-Output Shocks, Log Levels



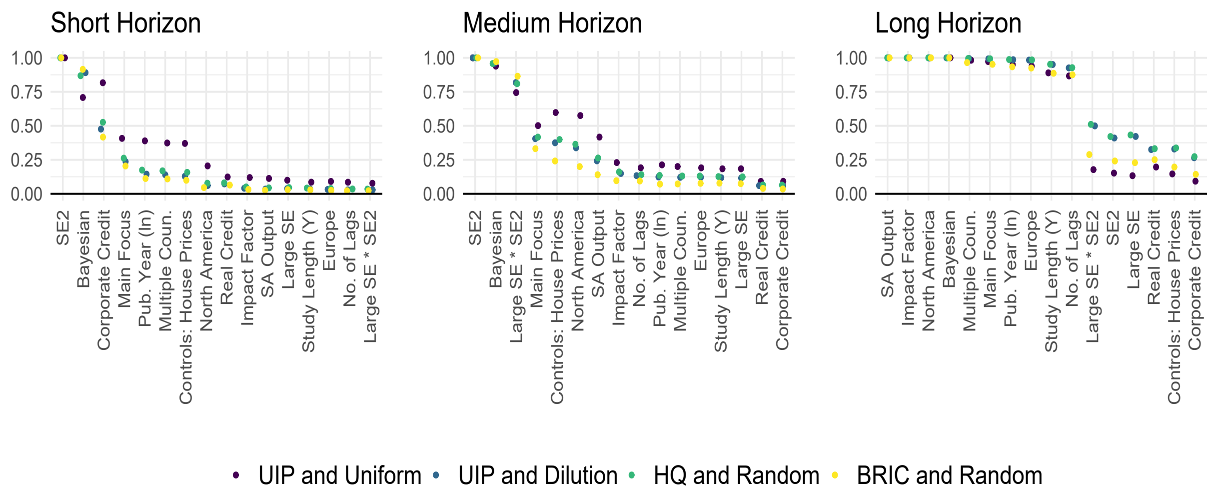
Note: Each panel shows posterior inclusion probabilities (PIPs) under various Bayesian Model Averaging priors—UIP (Unit Information Prior) with Uniform/Dilution, HQ (Hannan–Quinn), and BRIC—in short, medium, and long horizons. The horizontal axis lists candidate regressors; the vertical axis plots the PIP under each prior, illustrating how different prior assumptions affect variable selection and model robustness.

Figure C6: Prior Sensitivity Check in Bayesian Model Averaging: Credit-to-Output Shocks, Growth Rates



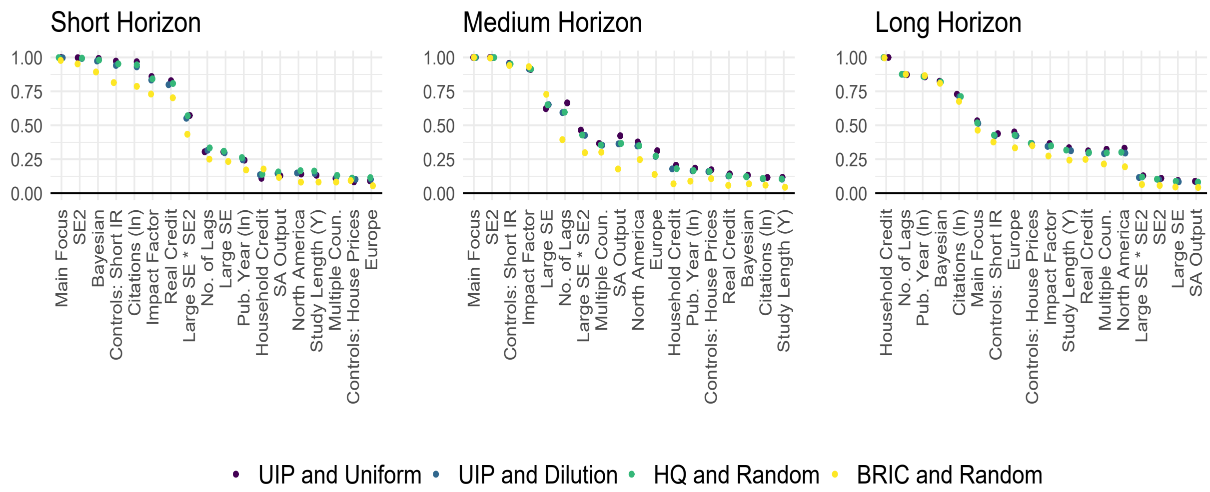
Note: Each panel shows posterior inclusion probabilities (PIPs) under various Bayesian Model Averaging priors—UIP (Unit Information Prior) with Uniform/Dilution, HQ (Hannan–Quinn), and BRIC—in short, medium, and long horizons. The horizontal axis lists candidate regressors; the vertical axis plots the PIP under each prior, illustrating how different prior assumptions affect variable selection and model robustness.

Figure C7: Prior Sensitivity Check in Bayesian Model Averaging: Output-to-Credit Shocks, Log Levels



Note: Each panel shows posterior inclusion probabilities (PIPs) under various Bayesian Model Averaging priors—UIP (Unit Information Prior) with Uniform/Dilution, HQ (Hannan–Quinn), and BRIC—in short, medium, and long horizons. The horizontal axis lists candidate regressors; the vertical axis plots the PIP under each prior, illustrating how different prior assumptions affect variable selection and model robustness.

Figure C8: Prior Sensitivity Check in Bayesian Model Averaging: Output-to-Credit Shocks, Growth Rates



Note: Each panel shows posterior inclusion probabilities (PIPs) under various Bayesian Model Averaging priors—UIP (Unit Information Prior) with Uniform/Dilution, HQ (Hannan–Quinn), and BRIC—in short, medium, and long horizons. The horizontal axis lists candidate regressors; the vertical axis plots the PIP under each prior, illustrating how different prior assumptions affect variable selection and model robustness.

C.2.3 Implied Effects

Table C9: Implied Effect: Credit-to-Output Shocks, Sign Restrictions

	Short horizon	Medium horizon	Long horizon
Credit-to-Output Shock, Log-Levels			
Corrected & Implied mean	0.273	0.232	0.338
32/68 credible intervals	(0.099, 0.391)	(0.098, 0.501)	(0.196, 0.717)
Credit-to-Output Shocks, Growth Rates			
Corrected & Implied mean	0.324	-0.271	-0.117
32/68 credible intervals	(0.100, 0.562)	(−0.412, 0.012)	(−0.389, 0.223)

Note: This table shows the mean implied effect and its credible intervals under the assumption that all studies use a sign-restriction identification. The implied effect reflects the mean semi-elasticity if all studies followed this single common strategy. As with the bias-corrected estimates, we derive fitted values from a full BMA meta-regression, and the credible intervals come from predictive densities that blend the most likely BMA models. The weighted statistics are used, calculated using a weight equal to the inverse of the number of estimates collected per study.

Table C10: Implied Effect: Output-to-Credit Shocks, Main Focus

	Short horizon	Medium horizon	Long horizon
Output-to-Credit Shock, Log-Levels			
Corrected & Implied mean	1.411	1.588	0.971
32/68 credible intervals	(0.928, 1.966)	(1.447, 2.389)	(0.855, 1.866)
Output-to-Credit Shock, Growth Rates			
Corrected & Implied mean	0.097	0.267	2.001
32/68 credible intervals	(0.014, 0.226)	(0.059, 0.368)	(1.001, 3.059)

Note: This table shows the mean implied effect and its credible intervals under the assumption that all studies focus on output-to-credit shock elasticities. The implied effect reflects the mean semi-elasticity if all studies followed this single common strategy. As with the bias-corrected estimates, we derive fitted values from a full BMA meta-regression, and the credible intervals come from predictive densities that blend the most likely BMA models. The weighted statistics are used, calculated using a weight equal to the inverse of the number of estimates collected per study.

CNB Working Paper Series (since 2024)

WP 12/2025	Jan Janků Simona Malovaná Josef Bajzík Klára Moravcová Ngoc Anh Ngo	*Credit shocks fade, output shocks persist: A meta-analysis of 2,600 VAR estimates across 63 countries*
WP 11/2025	Martin Hodula Simona Malovaná	*When foreign rates matter more: Domestic investor responses in a small open economy*
WP 10/2025	Volha Audzei Jan Brůha Ivan Sutóris	*Does firms' financing in foreign currency matter for monetary policy?*
WP 9/2025	Volha Audzei Sergey Slobodyan	*Dynamic sparse adaptive learning*
WP 8/2025	Michal Franta Jan Vlček	*Inflation at risk: The Czech case*
WP 7/2025	František Brázdik Karel Musil Tomáš Pokorný Tomáš Šestořád Jaromír Tonner Jan Žáček	*Upgrading the Czech National Bank's core forecasting model g3+*
WP 6/2025	Raphael Auer David Köpfer Josef Švéda	*The rise of generative AI: Modelling exposure, substitution, and inequality effects on the US labour market*
WP 5/2025	Josef Švéda	*Navigating banking resilience: Bail-ins & bailouts in the Czech banking sector*
WP 4/2025	Ivan Trubelík Tomáš Karhánek Simona Malovaná Aleš Michl	*Instant payments in Czechia: Adoption and future trends*
WP 3/2025	Daniel Štodt	*Non-linearity of government spending multiplier: The case of a small open economy*
WP 2/2025	Soňa Benecká	*Forecasting disaggregated producer prices: A fusion of machine learning and econometric techniques*
WP 1/2025	Martin Hodula Lukáš Pfeifer	*Payment holidays, credit risk, and borrower-based limits: Insights from the Czech mortgage market*
WP 9/2024	Jaromír Baxa Tomáš Šestořád	*Economic policy uncertainty in Europe: Spillovers and common shocks*
WP 8/2024	Martin Hodula Jan Janků Simona Malovaná Ngoc Anh Ngo	*Geopolitical risks and their impact on global macro-financial stability: Literature and measurements*

WP 7/2024	Jan Brůha Adam Ruschka Jan Šolc	*LUCI: Your best friend for measuring labor market tightness*
WP 6/2024	Aleš Bulíř Jan Vlček	*The mirage of falling R-stars*
WP 5/2024	Natálie Dvořáková Tomáš Šestořád	*Origins of post-COVID-19 inflation in central European countries*
WP 4/2024	Volha Audzei Ivan Sutóris	*A heterogeneous agent model of energy consumption and energy conservation*
WP 3/2024	František Brázdik Tatiana Keseliová Karel Musil Radek Šnobl Jan Šolc Stanislav Tvrz Jan Žáček	*Understanding inflation expectations: Data, drivers and policy implications*
WP 2/2024	Jiří Gregor	*A stress test approach to the calibration of borrower-based measures: A case study of the Czech Republic*
WP 1/2024	Michal Franta Jan Vlček	*Wage-price spirals: A risk-based approach*

CZECH NATIONAL BANK
Na Příkopě 28
115 03 Praha 1
Czech Republic

ECONOMIC RESEARCH DIVISION
Tel.: +420 224 412 321
Fax: +420 224 412 329
http://www.cnb.cz
e-mail: research@cnb.cz

ISSN 1803-7070