Working Paper Series —— 14/2020

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Czech National Bank —— Working Paper Series —— 14/2020

CZECH NATIONAL BANK

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The Effect of Monetary Policy on House Prices – How Strong is the Transmission?

Dominika Ehrenbergerová and Josef Bajzík *

Abstract

In the current long-lasting period of low interest rates and overheating housing markets, the discussion of the effect of monetary policy on house prices has arisen again. We examine the broad empirical literature on this topic. We collect 1,447 estimates of the effect of changes in short-term interest rates on house prices. These estimates come from 31 studies and are drawn from vector autoregression models. On average, an increase in the interest rate by one percentage point causes a median decrease in house prices of 0.7 percent for the one-year horizon and 0.9 percent for the two-year horizon. Moreover, we show that at the medium-term (monetary policy) horizon, the effect of monetary policy remains significant after correcting for the publication bias present in the literature. In addition, we collect more than 40 control variables. These capture, first, the context in which the estimates were obtained, and, second, the characteristics of the economies in question. Within both groups of variables we identify several significant aspects explaining differences in the estimates reported in the literature. The most prominent drivers of the heterogeneity are the use of sign restrictions, the inclusion of additional endogenous variables in VAR models, and the level of indebtedness.

Abstrakt

V současném dlouhotrvajícím období nízkých úrokových sazeb a možného přehřátí trhu s nemovitostmi se opět začíná diskutovat vliv měnové politiky na ceny nemovitostí. Tento vztah analyzujeme zkoumáním obsáhlé empirické literatury na toto téma. Posbírali jsme 1 447 odhadů vlivu krátkodobých úrokových sazeb na ceny nemovitostí. Tyto odhady pochází z 31 studií a jsou převzaty z modelů vektorové autoregrese (VAR). Průměrně nárůst úrokové míry o jeden procentní bod sníží ceny nemovitostí o 0,7 % po jednom roce a o 0,9 % po dvou letech. Navíc naše studie ukazuje, že po očištění o publikační selektivitu ve střednědobém horizontu efekt měnové politiky zůstává signifikantní. Kromě toho jsme posbírali více než 40 kontrolních proměnných. Ty zachycují zaprvé kontext, ve kterém byly odhady získány, a za druhé charakteristiky zkoumaných ekonomik. Mezi oběma skupinami proměnných jsme identifikovali několik významných aspektů vysvětlujících rozdíly v odhadech uváděných v literatuře. Nejvýznamnější rozdíly působí užití znaménkových restrikcí, zahrnutí více proměnných do VAR modelu a úroveň zadluženosti.

JEL Codes: C83, E52, R21.
Keywords: House prices, meta-analysis, monetary policy, publication selection, transmission.

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The authors note that the paper represents their own views and not necessarily those of the Czech National Bank.
We would like to thank Jakub Matějů, Marek Rusnák, Simona Malovaná, Hana Hejlová, and other seminar participants at the Czech National Bank for useful comments. All errors and omissions remain the fault of the authors.

1. Introduction

Monetary policy conduct interacts with financial stability. Several transmission channels, such as the interest rate channel, the asset price channel, and the credit channel, have frequently been discussed in the literature (Bernanke and Gertler, 1995; Mishkin, 1996). However, the overall effect and consequences of particular steps taken by policy makers are still under investigation by researchers. Williams (2016) suggests that we cannot evaluate the interactions between monetary policy and financial stability policies and assert whether monetary policy should target financial stability without properly examining the effect of monetary policy on the variables important for the assessment of the financial cycle. It is certain that monetary policy affects house prices. However, there is no clear consensus on the exact size of the effect, i.e., on the strength of the transmission and its economic and statistical significance.

The size of the effect of monetary policy on house prices is even more important in the current long-lasting period of very low interest rates, due to its potential overheating effect on housing markets. While Taylor (2007) and Schularick and Taylor (2012) show evidence in favor of this hypothesis, Dokko et al. (2011) suggest that monetary policy cannot itself cause house prices to rise, as they find that the effect of monetary policy on house prices is not sufficiently strong to do so. Furthermore, Kohn (2007) indicate that swings in house prices might be a result of excessive optimism rather than a consequence of monetary policy. Mishkin (2007) illustrates the lack of ability of standard models to clarify housing developments. Whether the former or the latter view is more correct has significant consequences for policymakers, especially in decision making on whether macroprudential policy should be used to tame booms and busts in house prices. Moreover, the size of the effect of monetary policy on house prices is especially important in periods when financial stability goals and macroeconomic goals do not correspond. During those times, there is a trade-off between lower house price growth and the macroeconomic costs of monetary tightening (and vice versa).

While the effect of monetary policy on the overall price level has been estimated in many primary studies as well as in systematic meta-studies analyzing both the price puzzle (Rusnak et al., 2013) and transmission lags (Havranek and Rusnak, 2013), there is no systematic review of studies of the effects of monetary policy on house prices. Thus, we decide to follow the procedure implemented in Rusnak et al. (2013) to investigate the effects of monetary policy on house prices. A consensus that contractionary monetary policy leads to a decline in house prices reigns in the existing literature. However, the magnitude of the effect varies. In order to synthesize the literature, we collect data on the monetary policy-house price relationship (i.e., we collect estimates of the responses of house prices to changes in interest rates) from 31 comparable studies, altogether providing us with 1,447 estimates from 27 countries. Meta-analysis – a statistical review of previously reported findings – appears to be the most suitable method for evaluating such a broad sample of diverse results.

Using meta-analytical methods we can explore publication selection, a phenomenon where authors refrain from publishing "wrong sign" or insignificant results – a situation that is not uncommon in economic research (Stanley, 2001; Christensen and Miguel, 2018). We use several approaches to measure publication bias. We start with simple graphical visualization and follow this with formal linear tests and new nonlinear approaches designed by Furukawa (2019) and Andrews and Kasy (2019). We then examine the heterogeneity in the estimates from both the methodological and the country-specific structural points of view.

We make several robust findings. First of all, we evaluate the average impulse response of house prices reported in the literature. The maximum median (mean) decrease of house prices after a 1 pp change in the interest rate occurs after eight quarters and is equal to −0.9% (−1.2%). Second, publication bias is present in this stream of literature across all the time horizons tested. The results are in line with Ioannidis et al. (2017), who assert that the effects reported in the economic literature are exaggerated twofold on average. Moreover, we explore the key drivers of the heterogeneity in the estimates reported in the literature. Controlling for data differences, model specification, estimation and publication characteristics, and structural aspect we show that several factors systematically affect the estimates in primary studies. The most prominent drivers are sign restrictions, the inclusion of additional endogenous variables, and the country's level of indebtedness. Even after correcting for all these factors, the publication bias remains strong and significant. Lastly, we calculate implied impulse response estimates for different countries. These estimates suggest that the transmission may differ significantly across them. The maximum "true" effect is attained at the end of the medium horizon (the response after one year is around 0.2 pp lower) and varies from −0.9% to −2.7% in response to a monetary policy shock of a one percentage point increase in the interest rate. This is in line with the findings of Coibion et al. (2017) and Assenmacher-Wesche and Gerlach (2010), who suggest that the effect of monetary policy on house prices is large relative to its effects on inflation and real economic activity.

The rest of the paper has the following structure. Section 2 describes the data collection process. Section 3 scrutinizes the presence of publication bias in this field of the literature. Section 4 addresses the heterogeneity among primary studies and calculates the implied estimates. Section 5 contains a summary of the article.

2. Collecting the Dataset

There is a vast literature on monetary policy transmission that uses the class of vector autoregression (VAR) models, starting with Sims (1980), and with later studies differing in terms of estimation specifications and econometric approach. Similarly to any analysis of the transmission of monetary policy to other variables, VAR is the main empirical approach used to estimate the effect of monetary policy on house prices. A general structural VAR model has the following form, as used, for instance, in Calza et al. (2013):

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

where Y_t^i is a vector of endogenous variables for time t and country i, a^i is a constant, $A^i(L)$ and $B^i(L)$ are distributed lag polynomials, z_t is a vector of ordinary exogenous variables, and e_t^i is an error term. When the estimates are made for only one country, the model is simplified by omitting index i. The set of endogenous variables in a typical monetary VAR model usually includes measures of output, prices, and the interest rate. We are interested in the literature which, besides those, includes a measure of house prices as well. Depending on the model specification, the endogenous variables might also include other variables, for example, the exchange rate for small open economies, or a measure of consumption, the money supply, the long-run interest rate, residential investment or credit, especially when the house price channel is the primary focus of the study. In order to estimate Equation 1, authors rewrite it and estimate it in reduced form. The outputs from VAR models – the dynamic responses of the endogenous variables to structural shocks – are usually reported in impulse response function (IRF) graphs, which are easy for the reader to interpret and cover the response over several time horizons.[1]

[1] There is another approach to analyzing the effect of monetary policy on house prices, and that is the class of DSGE models (for example, Aspachs-Bracons and Rabanal, 2010; Bofinger et al., 2013). But since the estimation approaches are completely different, there are not enough DSGE model estimates to conduct a meta-analysis, and

We use Google Scholar to search for studies discussing the effect of monetary policy on house prices. We calibrate our search query in order to get the best-known relevant studies among the first hits. The algorithm scans the full text of articles regardless of title formulation. This approach extends the coverage of applicable estimates. We inspect the first 500 papers produced by the search. Furthermore, we amplify our primary search using the "snowballing" method. We investigate the references in the downloaded studies to check whether we can find other utilizable studies not detected by the primary search. We finished our search in November 2019. No newer study is considered in our research.

Each of the studies we finally included in our data set needs to fulfill the following three criteria. First, the study must use a VAR model approach for different horizons as defined in Equation 1. Second, monetary policy must be measured by the short-term interest rate. Third, the study must report confidence intervals around the mean impulse response function of house prices so that we are able to compute the standard error. Without knowing the precision of the estimates we cannot perform publication bias tests. The study selection path is shown in Figure A1 in the Appendix.

We decide to incorporate both published and unpublished studies. When only published studies are included, one can be sure that the quality of the estimates is sufficient. On the other hand, the inclusion of both published and unpublished studies enables one to study the differences between these two categories. The inclusion of unpublished articles does not affect the results of publication bias, because if journals prefer to publish particular estimates, authors will rationally adopt that preference in the early stages of research (Rusnak et al., 2013). Moreover, Doucouliagos and Stanley (2013) provide empirical evidence based on 87 meta-analyses that there is no difference in the magnitude of publication selection between unpublished and published studies. Besides, the inclusion of all suitable articles might help us detect the heterogeneity drivers better.

Finally, we identified and collected data from 31 papers which focus on the topic of our interest and which provide all the information necessary for conducting the meta-analysis. The studies included in our meta-analysis are summed up in the following Table 1. All these studies were published between 2002 and 2018. They cover the euro area and 27 individual countries, including the UK, Switzerland, Sweden, Norway, the US, Canada, Australia, China, Japan, and South Africa. In Google Scholar, the studies together receive 3,635 citations, which indicates the importance of the topic of the relationship between monetary policy and house prices. It also confirms the importance of VAR modeling in monetary economics, as already shown in Rusnak et al. (2013).

We only use studies that use the short-term interest rate as a monetary policy proxy. We also only use studies that include house prices in levels (i.e., studies that report the response of the house price level to a change in the interest rate) and we do not include studies that examine house price growth rates. We do that in order to have as homogeneous a sample as possible; if we mixed house price levels and growth rates we would not be able to examine the size effect. In so doing, we exclude some otherwise important papers on the topic (e.g., Fratantoni and Schuh, 2003; Del Negro and Otrok, 2007). Including such studies can only be delegated to future research.

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we are interested in synthesizing the empirical research, we decide to collect only the estimates from VAR models. This prevents us from including, for example, the influential contribution of Iacoviello and Neri (2010).

Table 1: Studies Included in the Meta-Analysis

Assenmacher-Wesche and Gerlach (2008a)	Carstensen et al. (2009)	Jarocinski and Smets (2008)
Assenmacher-Wesche and Gerlach (2008b)	Coibion et al. (2017)	McDonald and Stokes (2013)
Assenmacher-Wesche and Gerlach (2009)	Demary (2010)	Musso et al. (2011)
Assenmacher-Wesche and Gerlach (2010)	Elbourne (2008)	Ncube and Ndou (2011)
Bauer and Granziera (2017)	Giuliodori (2005)	Sá et al. (2011)
Belke et al. (2008)	Gupta et al. (2012a)	Sá and Wieladek (2011)
Berlemann and Freese (2013)	Gupta et al. (2012b)	Sousa (2014)
Bjørnland and Jacobsen (2010)	Iacoviello (2002)	Vargas-Silva (2008)
Bulligan (2010)	Iacoviello and Minetti (2003)	Wadud et al. (2012)
Calza et al. (2007)	Iacoviello and Minetti (2008)	Wu and Bian (2018)
Calza et al. (2013)		

Furthermore, the three aforementioned criteria prevent us from covering the literature addressing the effect of unconventional policies on house prices. This literature sprang up in the past years after monetary policy hit the zero lower bound, and it includes several influential articles. However, those studies are incomparable even mutually, due to different measures of unconventional monetary policy deployed by the authors (volumes or announcements of asset purchase programs, or increases in the total volume of central bank assets, etc.). To the best of our knowledge, the literature on transmission to house prices has not used "shadow rates" as a monetary policy proxy at all, so our dataset is not affected by that either. On the other hand, the recent focus on unconventional monetary policy measures explains why our coverage of newer studies is less numerous.

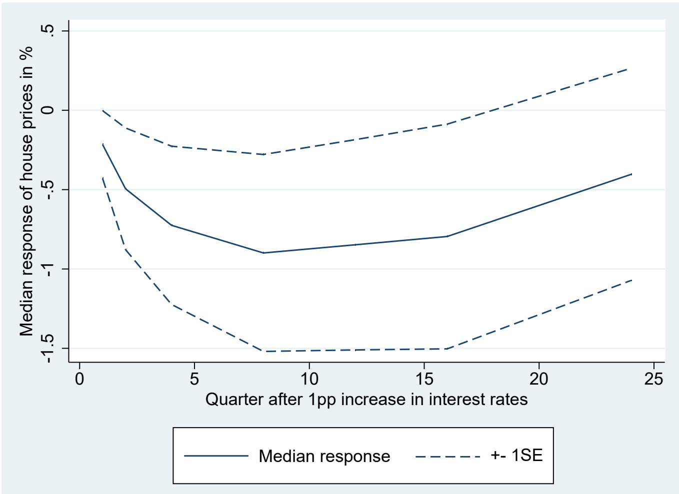
Collecting data for a meta-analysis is itself a demanding process, but the manual collection of IRFs across different time horizons is even more time consuming, because the responses and their confidence bounds have to be measured using pixel coordinates. Naturally, a measurement error arises during this procedure, but, as argued in Rusnak et al. (2013), it is random and similar to the rounding error that arises in numerical results when point estimates are collected for any meta-analysis.

From these studies we collect the responses of house prices to a change in the interest rate after one, two, four, eight, twelve, and sixteen quarters and also the longest time period for which the study computed the effect. Specifically, we gather 208 and 211 responses after one and two quarters, respectively. These together can be considered the short-term effects. For the mid-term effects we gather 221 estimates for both the four- and eight-quarter horizons. For long-term monetary policy we collect 216 estimates for the twelve-quarter and 211 estimates for the sixteen-quarter transmission lags. In all, 159 impulse responses also report longer transmission lags. On average the maximum is 24 quarters, but the longest response reported varied up to 40 quarters. Thus, we collect 1,447 estimates in total. For each of the observations we standardize the change in the monetary policy rate to a 100 bp increase and the confidence interval bands to $\pm$ 1 SE.[2]

[2] When talking about monetary policy shocks, Kuttner (2001) and Gregoriou et al. (2009) distinguish between anticipated and unanticipated changes in policy interest rates and find significant results only for unanticipated changes, i.e., true shocks. However, in our analysis we are unable to distinguish between such shocks. In the level VAR model we use, the interpretation of a "shock" to monetary policy is the same as the interpretation of a "change". Thus, in our study, "change" and "shock" are used interchangeably. If, in a primary study, a shock is, for example, one standard deviation in size, we standardize the responses so that they are equivalent to a shock of 100 bps and the responses are directly comparable across primary studies.

A total of 31 empirical papers and more than 200 impulse response functions are summarized in Figure 1. The figure plots the simple median across all studies. The literature shows that the house price falls significantly in response to a 100 bp increase in the interest rate. The immediate response is followed by a further decline. After four quarters the drop turns significantly negative. The strongest response is reached after 8 quarters, although the shape is rather flat from 8 quarters up to 16 quarters. The maximum median response is -0.9%. After reaching the maximum, the effect converges constantly toward zero.

Figure 1: Average Impulse Response Implied by the Literature

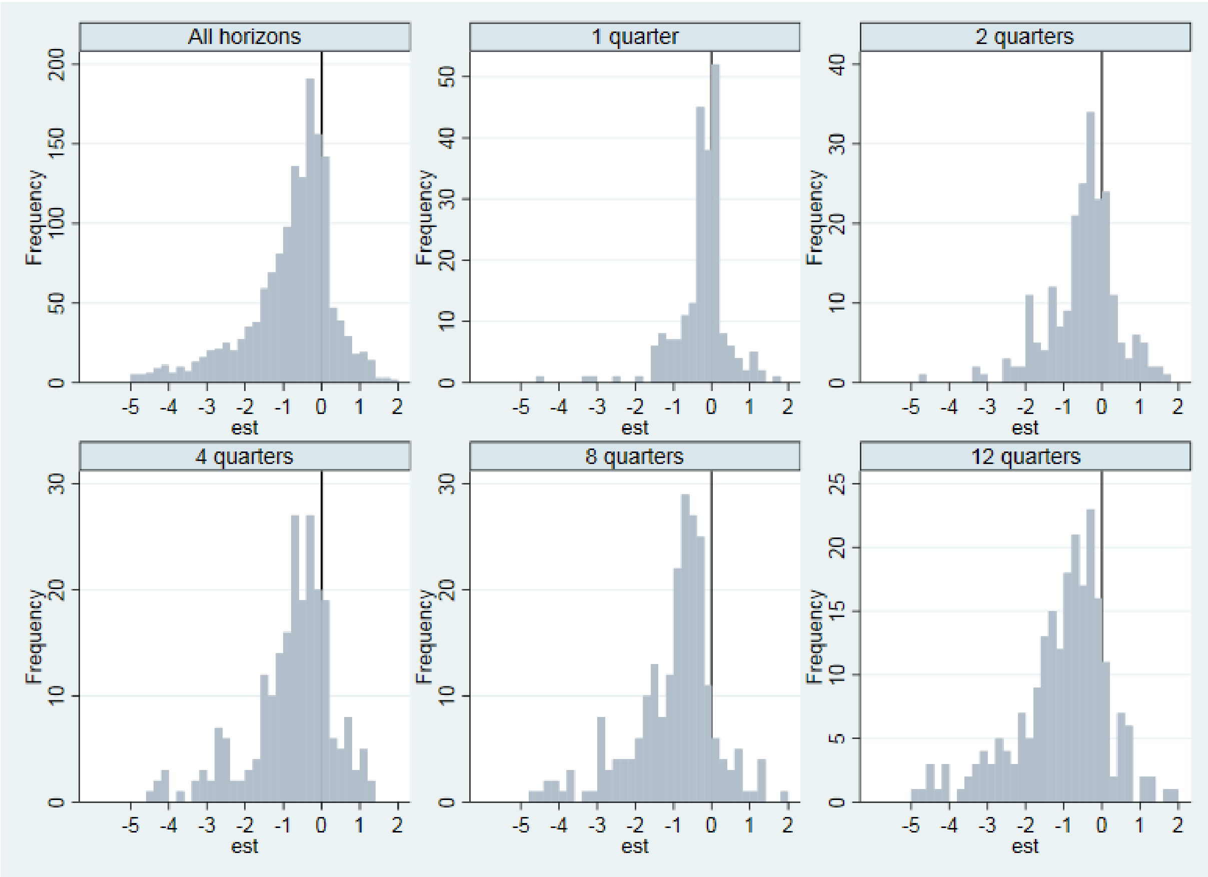


A closer view of the particular distribution of the monetary policy effect for each of the transmission lags is provided in Figure 2. The effect at one or two quarters is close to zero and almost symmetric, including positive responses, pointing to the equivalent of a "prize puzzle" in terms of house prices. At the mid-term and longer horizon, the effect is more pronounced (more negative), with the highest density of the estimates near the median being slightly above the minus one as reported above. There are no major outliers in the data, although at some horizons the lower percentiles contain rather low values of around -5%. About 87.3% of all the estimates lie between -2% and 1%. Moreover, more than 50% of all the estimates lie between -1% and 0%.

With the above two graphs, Figure 1 and Figure 2, we fulfill the first aim of the paper, namely, to provide the average effect reported across the literature. However, the simple mean or median is prone to shortcomings, so the meta-analysis does not stop there. First, the estimates might be biased by publication selection; second, when taking the simple mean or median, we do not account for the drivers of the heterogeneity of the estimates. Regarding the former, the estimates are in fact skewed, as is visible in Figure 2. This may be natural, as a negative response is theoretically plausible while a positive is not; nevertheless, we have to check the precision of the estimates and its relation to their size in order to confirm or reject the suspicion of publication selection; this will be done in the following Section 3. Regarding the latter, although we aim to have our sample as homogeneous as possible, some study designs may have a systematic influence on the results, the studies may be of different quality, and a model in a primary study may be misspecified. We will deal with the issue of heterogeneity in Section 4.

Thus, besides the responses from the IRFs, we collected more than 60 control variables that capture the specifics of each study in order to examine the heterogeneity in the estimates in the first step. After dismissing variables which prove to be highly correlated and yielding the same information and those with no variability at all, we are left with around 40 control variables. Slightly less than two thirds of the variables included are collected from primary studies themselves, while the remaining one third consist of external country-level variables included to examine structural heterogeneity and collected from typical sources such as the databases of the World Bank, the OECD, and Eurostat. Altogether, we collect about 100,000 data points to create our dataset. These data were collected by the two co-authors of this paper and cross-examined to eliminate potential mistakes arising from manual collection of the data.

Figure 2: Distribution of Effects Across Transmission Horizons



Note: The horizontal axis of the histograms captures the size of the estimated effect. The estimates are trimmed at the -5 and $+2$ thresholds for visual purposes, but they are included in subsequent estimations. For the same reason, we do not plot the horizons of 16 quarters and longer.

3. Publication Bias

Research can be affected to a great extent by publication bias. This phenomenon arises from authors' efforts to publish results that have the expected sign or size and are statistically significant. Naturally, a researcher is expected not to report estimates of the "wrong" sign if they come from evidently incorrect specifications. Nevertheless, the censoring of estimates of unexpected sign (or constant efforts to report specific estimates) distorts the literature as a whole: discarding near-zero and imprecise estimates but reporting large (in absolute terms) and imprecise estimates creates an upward bias. Ioannidis et al. (2017) suggest that the estimates reported in economics are usually overvalued twofold due to publication bias. The subject has also been addressed by McCloskey and Ziliak (2019), who liken publication selection to the Lombard effect, a term originally from biology: the higher the noise (in our case, the standard errors), the higher the vocal effort needed in speaking to be audible (in our case, carrying on specification searches with the desire to publish larger estimates in order to overcome the noise). Given the large number of degrees of freedom in empirical economic research, there is almost always a possibility to produce estimates that are of the “correct” sign and significance (Gechert et al., 2020).

Publication bias will be examined via both a visual approach and a formal econometric approach, including linear and nonlinear tests. Visually, publication selection can be examined in a funnel plot (Stanley and Doucouliagos, 2010). This common tool for detecting the extent of publication selection was proposed by Egger et al. (1997). The horizontal axis of the funnel plot captures the size of the estimated effect. On the vertical axis, the precision (the inverse of the standard error) is displayed. In the absence of publication bias, the data points should be distributed symmetrically around the mean “true” effect, with high dispersion at lower levels of precision and smaller dispersion at higher levels of precision, forming the shape of an inverted funnel. The case of asymmetry, especially when a portion of less precise estimates of the unexpected sign is missing, indicates publication selection. In other words, an asymmetric funnel plot points to the fact that the authors discarded estimates of a particular magnitude or sign. In the case of a hollow funnel plot, rejection of statistically insignificant estimates is to blame as the source of publication selection. In the worst case, the funnel plot is both hollow and asymmetric Egger et al. (1997).

Figure 3 provides us with a clear message about potential publication selection in the case of estimates of the effect of monetary policy on house prices. The distribution for all time horizons at once is captured in the upper left corner. The rest of the panels relate to individual periods. In all of these funnel plots we can see evidence of publication bias, since none of them are symmetric – they are all skewed to the left and a portion of the positive insignificant estimates is missing.

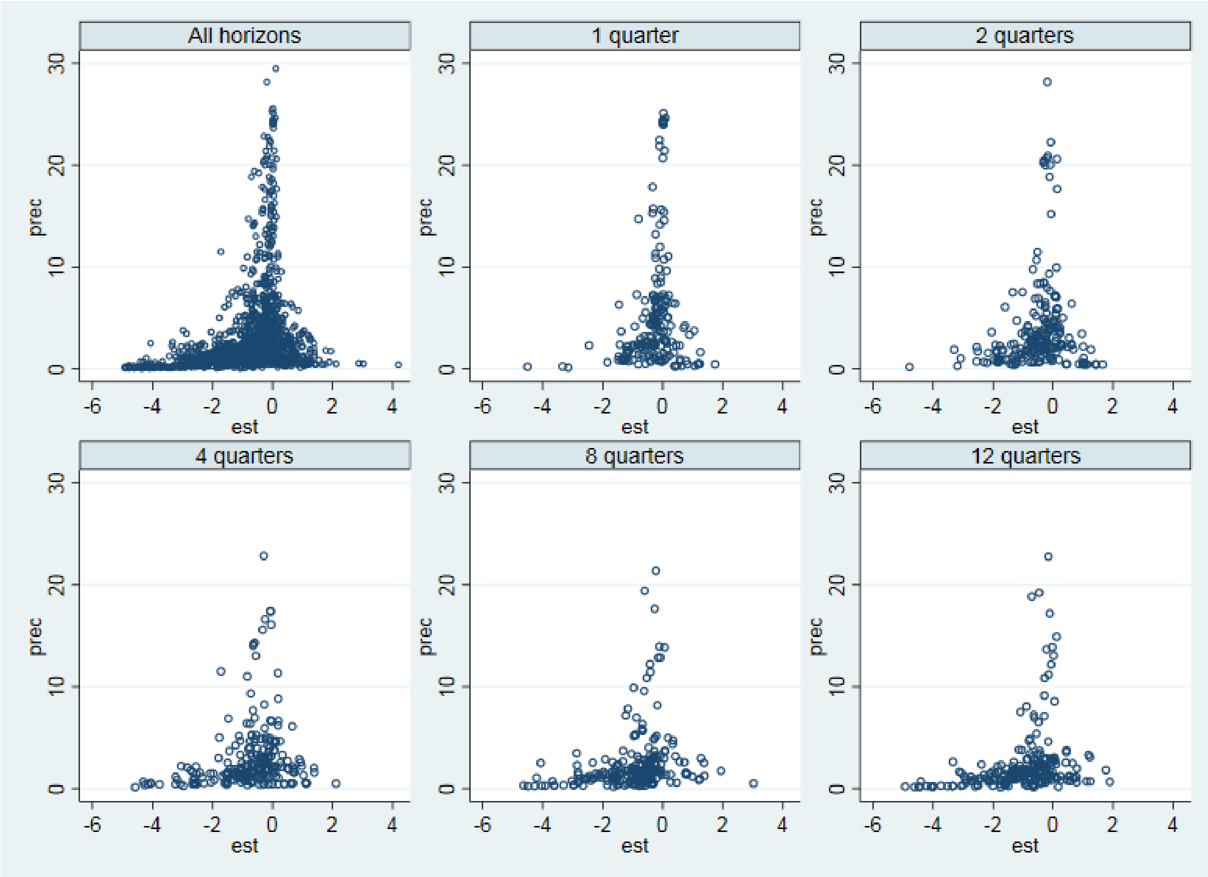
After the simple visual test in the form of the funnel plot, we proceed with more rigorous tests. Using the same intuition, we test for publication bias in formal tests – first linear ones, then nonlinear and non-parametric ones. Based on these tests, we can identify publication bias and correct the estimates for its extent. The linear test is based on the assumption that an estimate and its standard error should be independent quantities. However, a researcher may succumb to an analogy of the Lombard effect as described above: in such case, we would observe a positive relationship between the two quantities (the estimate and its standard error). The form of the linear test is specified in the following Equation 2 regressing the estimate on its standard error, as put forward by Card and Krueger (1995) and formalized by Stanley (2005):

$$\hat{x}_{i,j} = \alpha_0 + \beta SE_{i,j} + \varepsilon_{i,j} \tag{2}$$

where $\hat{x}_{i,j}$ is the estimated effect i in study j, and SE is its standard error. Parameter α_0 is the effect beyond bias, while β represents the intensity of the publication bias. In the case of no publication bias, there would be no significant relationship between the estimate and its standard error, i.e., the estimate of coefficient β would not be significantly different from zero. First, we use a simple OLS estimate with standard errors clustered at the study and country levels. Then, we also employ a weighting scheme – more specifically, we use $1/SE_{ij}$, i.e., the precision, as the weight in order to address heteroskedasticity by giving more weight to more precise estimates. The weighted least squares approach is used, for instance, by Stanley and Doucouliagos (2017), Baker and Jackson (2013), and Havranek et al. (2017).[3]

[3] The validity of Equation 2 requires a symmetrical distribution of the estimated coefficient in the absence of publication bias. An obstacle arises in VAR models from the fact that the confidence intervals around the

Figure 3: Publication Bias



Note: The horizontal axis of the funnel plot captures the size of the estimated effect. On the vertical axis, the precision is displayed. In the absence of publication bias the scatter plot should resemble an inverted funnel that is symmetric around the most precise estimates. Estimates smaller than -6 and larger than 4 are excluded from the figure for ease of exposition but are included in all the statistical tests.

After performing the linear tests, we employ new nonlinear methods which have appeared recently, presumably as a consequence of the increasing awareness about publication selection in economics. First, we employ the selection model proposed by Andrews and Kasy (2019). The authors nonparametrically identify the probability of publication as a function of a study's results (with the probability changing at the conventional p-value thresholds) and then show how to correct for publication bias knowing this conditional publication probability. The second nonlinear approach employed in this study follows the method of Furukawa (2019), also known as the "stem-based" method. The approach is fully data-dependent and non-parametric and relies on the most precise estimates. The procedure is based on minimizing the trade-off between the variance of the most precise estimates and their bias. The approach is robust under various assumptions about the form

transmission estimates are often asymmetric, for instance, in the case of BVAR models. If the asymmetry is not random through the individual estimates, the IRFs will be distributed asymmetrically even in the absence of publication bias. The linear tests for publication bias would thus not be valid. A significant difference between the average distance from the IRF's estimated coefficient to the lower and upper confidence bounds would provide evidence of such asymmetry. Following Rusnak et al. (2013) we choose the distances from the mean to the 16% and 84% confidence bounds. The maximum difference is very small – only about 8% – and even if it is significant at some horizons, it cannot explain the asymmetry in the estimated effects. Thus, with some caution we can use the classical linear formal tests for publication bias.

of publication bias. Since both nonlinear methods are quite new, there are no direct comparisons between them or relevant simulations in the literature. We incline to the suggestion by Carter et al. (2019) to use various methods for publication bias and compare their results. The results of the stem-based method are more conservative than those of the linear ones (Gechert et al., 2020). For more information about the nonlinear techniques, see Andrews and Kasy (2019) and Furukawa (2019).

The results of all the approaches employed are summarized in Table 2. It presents the outcomes of the four estimation techniques not only for the pooled set of all estimates, but also for individual horizons. Panel A runs linear regressions on both the unweighted and weighted data. The baseline OLS model exposes publication bias from the first to the eighth quarter. The effect beyond bias is negative, but less strong than the uncorrected mean. It is significant for the eighth to sixteenth quarter. The peak effect is equal to -0.7 and is reached after twelve quarters. The second linear model employs the regression weighted by the inverse of the standard error. This weighting assigns a greater weight to more precise estimates. Running the weighted regression reveals strong evidence of publication bias across all horizons. The strongest effect beyond bias appears after eight quarters, but from the original mean response of -1.2 reported in the literature, the effect shrinks to -0.33 at this horizon. The magnitude of the effect and the publication bias is similar at the horizon of four quarters. Except for the immediate horizon, the effect of a change in interest rates on house prices remains significantly negative even when corrected for publication bias.

Panel B, containing the nonlinear methods, indicates the same conclusions as those described above. The corrected mean effects are significant and negative. The results of the stem-based method are very close to the results of the weighted linear regression. The results of the selection model suggest that the correction is not necessarily as large, and the mean effect beyond bias estimated by this method is closer to the mean responses reported in the literature: correction for publication bias weakens the response by around one third.

We further provide an extended analysis of the potential sources of publication bias. Until now, we have covered the publication bias due to the effect size. However, publication bias may also appear at p-values (significance level). The practice of p-hacking suggests that researchers search for specifications until they find results that are significant at an acceptable significance level. In the absence of publication bias, the distribution of statistically significant p-values should be right-skewed. The identification of publication bias based on the distribution of p-values was put forward by Simonsohn et al. (2014b) and Simonsohn et al. (2014a) in their *p-curve* method, and by van Assen et al. (2015) and van Aert et al. (2016) in their *p-uniform* or *p-uniform** method. The distribution of the p-values and additional extensions to our publication bias analysis are available in Appendix B.

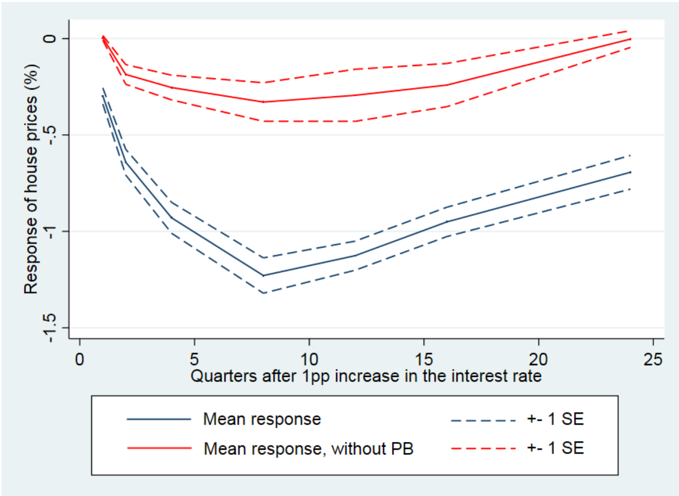
Table 2: Tests of Publication Bias and the True Effect

	Horizon						
	All horizons	1 quarter	2 quarters	4 quarters	8 quarters	12 quarters	16 quarters
PANEL A: Linear estimation							
OLS							
Publication bias	-0.584***	-0.751***	-1.099***	-1.280***	-0.990***	-0.451	-0.281
	(0.176)	(0.238)	(0.378)	(0.456)	(0.288)	(0.281)	(0.182)
	[-1.310, -0.096]	[-1.227, -0.034]	[-1.962, -0.304]	[-2.344, -0.295]	[-2.027, -0.038]	[-1.397, 0.116]	[-0.832, 0.173]
Effect beyond bias	-0.402***	-0.034	-0.055	-0.094	-0.402**	-0.699***	-0.648***
	(0.107)	(0.074)	(0.189)	(0.256)	(0.175)	(0.202)	(0.167)
	[-0.644, -0.099]	[-0.209, 0.137]	[-0.452, 0.465]	[-0.712, 0.691]	[-0.923, 0.179]	[-1.176, -0.124]	[-1.043, -0.175]
Weighted by the inverse of the standard error							
Publication bias	-1.002***	-0.838***	-0.853***	-1.036***	-1.078***	-0.879***	-0.659***
	(0.130)	(0.165)	(0.148)	(0.204)	(0.214)	(0.250)	(0.197)
	[-1.338, -0.692]	[-1.269, -0.422]	[-1.210, -0.505]	[-1.591, -0.547]	[-1.658, -0.464]	[-1.546, -0.167]	[-1.277, -0.097]
Effect beyond bias	-0.083**	-0.004	-0.186***	-0.254***	-0.329***	-0.294**	-0.241**
	(0.032)	(0.012)	(0.051)	(0.064)	(0.100)	(0.135)	(0.112)
	[-0.188, -0.001]	[-0.0340, 0.020]	[-0.375, 0.006]	[-0.471, 0.052]	[-0.671, 0.101]	[-0.702, 0.141]	[-0.548, 0.129]
PANEL B: Non-linear estimation							
Stem-based method (Furukawa, 2019)							
Effect beyond bias	-0.011	-0.006	-0.231	-0.375	-0.324	-0.171	-0.120
	(0.020)	(0.009)	(0.064)	(0.073)	(0.165)	(0.133)	(0.089)
Selection model (Andrews and Kasy, 2019)							
Effect beyond bias	-0.652	-0.319	-0.524	-0.579	-0.836	-0.811	-0.935
	(0.041)	(0.069)	(0.070)	(0.067)	(0.090)	(0.125)	(0.163)
Observations	1,447	208	211	221	221	216	211

Note: Standard errors in single brackets. Confidence intervals from wild bootstrap clustering in square brackets. We use the wild bootstrap as recommended by Cameron et al. (2008), since the number of our clusters (studies) is relatively small (31) and standard errors from clustered inference may exhibit downward bias. The procedure is implemented via the *boottest* package in Stata (see Roodman et al., 2019). $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

We sum up the findings of this section by saying that, first, publication bias appears to be present in the majority of horizons of monetary policy transmission examined. Second, the response beyond bias remains significantly negative, but its magnitude decreases by 20% to 60%. Third, after correcting for publication bias, the strongest response is reached at the end of the medium-term horizon. The mean response corrected for publication bias, as identified by the weighted linear regression, is captured in Figure 4. Nevertheless, the correction for publication bias does *not* necessarily show us the *true* effect, since the effect might also be influenced by other misspecifications in the estimation, or affected by structural factors, which we will discuss in the next section.

Figure 4: The Effect Beyond Publication Bias



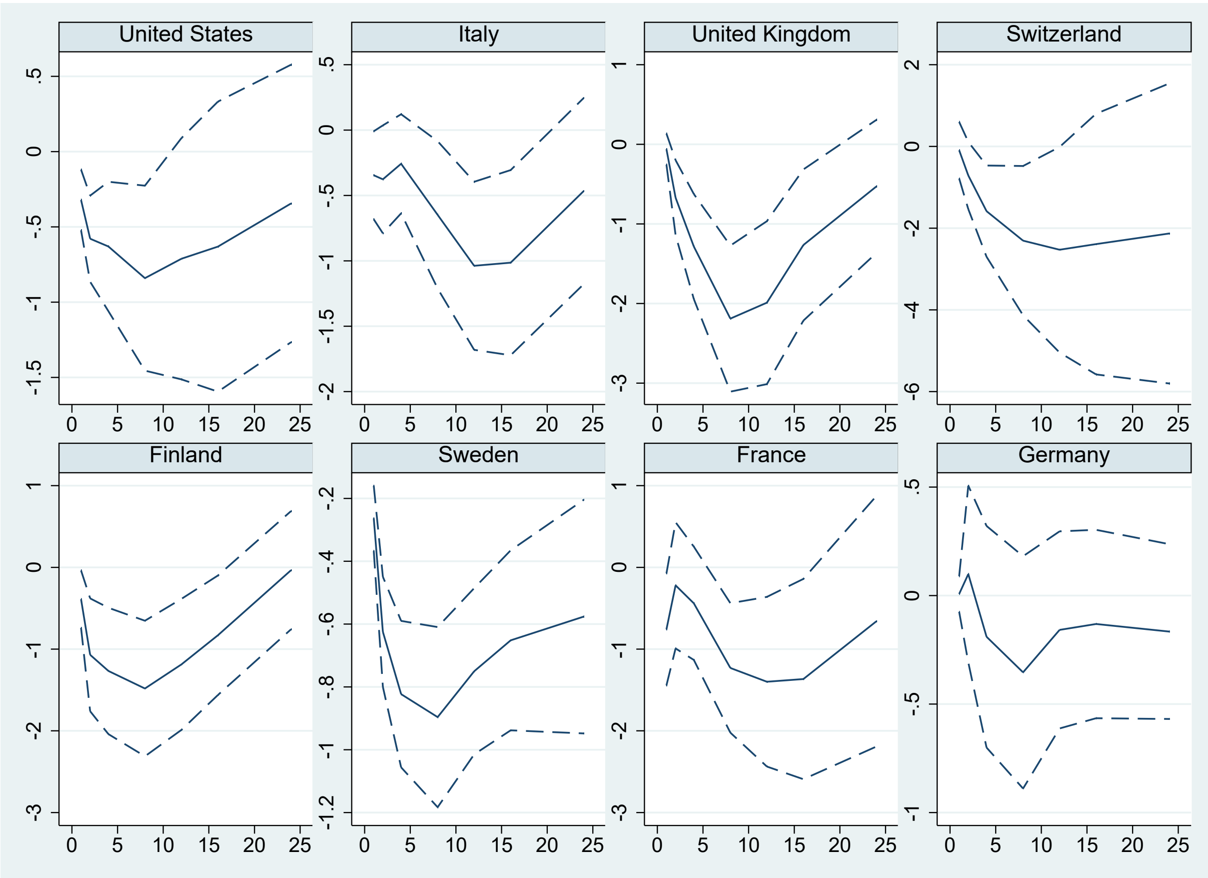
4. Drivers of Heterogeneity

In Figure 2 we saw that the estimates of the strength of the transmission from monetary policy to house prices vary widely. There are two main factors that may lead to differences in estimates. First, there are data and method choices, i.e., the context in which authors of primary studies obtain their results. They include (i) data characteristics (the time period, the length of the data sample, the frequency and dimension of the data, etc.), (ii) the estimation specification (the type of model estimated and the variables included), and (iii) various aspects of the econometric approach applied. Although not chosen by authors themselves, publication characteristics such as journal quality, publication year, and publishing in a journal instead of a working paper series, may also be systematically linked to the varying magnitude of the results reported in the literature. Second, structural characteristics across countries may be related to the differences in the strength of transmission (Calza et al., 2013; Iacoviello and Minetti, 2008; Assenmacher-Wesche and Gerlach, 2010). In other words, there may be genuine differences in the transmission across countries due to countries' structural characteristics. To cover those, we include several country-level macroeconomic and financial variables (also referred to as external variables).

In Figure 5 we provide the motivation for further analysis of the drivers of heterogeneity in the estimated impulse responses, especially for the examination of structural heterogeneity. We plot the mean responses from the eight countries that have been covered most frequently in the literature: the United States, Italy, the United Kingdom, Switzerland, Finland, Sweden, France, and Germany. We observe considerable differences across countries. The strongest response is seen in Switzerland, followed by the United Kingdom; the maximum decrease in these two countries is below -2%, which is two times the average over all countries. At the opposite end there are countries with a below-average response, especially Germany and the United States. Those two, however, also show the widest confidence intervals, leading to insignificant results over all horizons for Germany and long horizons for the United States. The rest of the countries show impulse responses relatively close to the whole sample average.

Besides differences in the size of the responses, we also observe differences with respect to the dynamics – the transmission lags of the responses. The transmission in some countries, for example, Sweden and Finland, is faster than in others – in both cases the responses bottom after eight quarters, but the response is already near (or even below) minus one after four quarters. On the other hand, the transmission is slower in countries such as Italy and France – they do not reach the maximum decrease until 12 or 16 quarters. Also, the convergence back to zero is slower (or does not even happen) in some countries (Switzerland) than in others (Finland and the United Kingdom). We find those differences intriguing although they still have the same shortcoming as the mean impulse response function from Section 2, i.e., they are not corrected for publication bias and methodological differences or misspecifications.

Figure 5: Cross-Country Heterogeneity



Note: The effect of a 1 percentage point change in the interest rate on the house price level (y-axis) across different time horizons in quarters (x-axis).

4.1 Variables

Data Characteristics. We control for some characteristics of the data used in the primary studies. First, regarding data frequency, only around 10% of the estimates come from studies that use monthly data; the rest are based on quarterly data. Second, we code for whether simple time series data are used in the primary studies (80% of all observations) or panel data are used in panel VARs. We are also interested in whether the strength of the transmission changes over time; we thus include the middle point of the dataset used. By doing so, we control for the potential change in the transmission not accounted for by structural heterogeneity, which will be described below. We also test whether the length of the sample and the number of observations used in the primary studies systematically affect the estimates – the longer the sample or the higher the number of observations, the more precise the estimates can be expected to be.

Specification characteristics. We cover the specification of the estimation equations in the second group of control variables. Rusnak et al. (2013), when assessing the effect of monetary policy on the overall price level, find that the study design has a significant effect on the results; for instance, they find that including the output gap as a measure of output in VAR or including commodity prices besides overall prices systematically affects the results. In a similar way, we specify dummies for single endogenous variables included in VAR models: we include a dummy equal to one if the GDP deflator is used instead of the usual consumer price index (only two studies do not include the general price level at all). Next, we include dummy variables if a measure of credit (usually total real credit to the private sector or mortgage loans) is used (26% of cases), if the long-run interest rate is used (17% of cases), and if consumption, residential investment, equity prices (or other asset prices), the money supply, the exchange rate, and the foreign interest rate (separately) are included. Last but not least, we distinguish between nominal and real house prices: interestingly, nominal house prices are used in only around 5% of the studies. All of the studies use the short-term (usually 3-month) money market rate, so there is no variability regarding this variable. We only include studies which use residential house prices, not commercial house prices, land prices, or rent prices. As far as the remaining aspects of the estimation specification are concerned, we control for the number of lags included in the VAR model. The number of lags affects the persistence in the impulse responses and can thus also affect the strength of transmission.

Estimation characteristics. Another important dimension over which estimates may differ is the estimation technique (the econometric specification). Studies typically use a reduced-form VAR employing the standard methods of OLS or MLE, and they usually employ recursive ordering as an identification scheme (Cholesky decomposition; 77% of all estimates); these are our reference categories. Next, we control for the situation where sign restrictions were used in order to identify a structural VAR. Since the use of sign restrictions can differ across papers (the restriction may not be imposed on all variables in the same direction), we distinguish between two cases that are important for the transmission to house prices. First, we include a dummy variable equal to one if sign restrictions were imposed on the house price variable, guaranteeing the expected sign. Second, we include a dummy if sign restrictions were imposed on any other variables, but not house prices, which we assert would be a better practice if the primary aim was to examine the transmission to house prices. We then code for other nonrecursive identification (long-run restrictions) and, regarding the estimation procedure, we also code for a dummy variable equal to one if a Bayesian VAR is estimated (around 10% of estimates).

Publication characteristics. In a state-of-the-art meta-analysis, publication characteristics are also controlled for when examining drivers of heterogeneity. We are especially interested in whether the results reported in journals with an impact factor differ systematically from those reported in working papers – for that, we include a dummy variable equal to one for all estimates from journals. Next, we examine whether the level of the impact factor itself is linked to different results. Further, we include a variable capturing the number of citations of a study, normalized by the number of years since the study's first appearance in Google Scholar. The relationship between publication characteristics variables and the estimated impulse responses, however, should be interpreted with caution (as correlation rather than causality). Last but not least, we collect the publication year, although it is probably correlated with the middle year of the sample and may be dropped at a later stage. While the middle year is included to capture possible changes in the transmission beyond those explained by structural heterogeneity, the publication year is more suitable for capturing potential changes in methodology, trends in publication (patterns of publication selection), or improvements in knowledge of the topic. Those are difficult to codify by other variables, but at the same time they might be relevant to the estimates.

Structural heterogeneity. The following group of variables covering structural heterogeneity is numerous, as we think it may be important for policy-makers and have a practical impact on the implementation of monetary policy. We include a wide range of these external variables (marked with the prefix "Country-level") to cover important macroeconomic, financial, demographic, and housing supply factors. First, we include a measure of economic development – disposable income per capita. We include this variable in logarithms of levels in order to examine whether a country's level of economic development matters for the transmission. In order to examine whether the phase of the economic cycle matters, we include separate dummy variables equal to one in boom periods and in crisis periods.

Second, we include interest rate variables, which we suspect may interact with the transmission to house prices. First, we include the level of the short-term interest rate itself: the transmission may be more complete at higher ("normal") monetary policy rates, while it might be hampered at low interest rates. On the other hand, as suggested by several studies, very low interest rates or prolonged periods of very low interest rates may cause asymmetries in the transmission and lead to asset (house) price bubbles. If those suggestions have empirical support, then a prolonged period of low interest rates fueling credit and house price booms could be mirrored in a stronger reaction of house prices in those periods than in other periods. Long-term interest rates (for example, 10-year government bond yields) instead of short-term interest rates might be even more relevant for the transmission to house prices.[4] We also include the inflation rate in the country – as suggested by Rusnak et al. (2013), periods of high inflation may be associated with lower credibility of the central bank and thus weaker transmission.

Third, we include the characteristics of the lending market – we include the credit-to-GDP ratio in order to control for the indebtedness of the sector as well as the level of financial development. We assume that at high credit-to-GDP ratios the reaction of house prices may be more pronounced, pointing to a spiral between credit and house prices. We also include a variable capturing the share of mortgage loans with floating interest rates: the higher the share of floating-rate mortgages, the stronger the immediate transmission to the overall mortgage interest rate, and possibly the stronger the transmission to house prices – the transmission is probably faster even if it is not necessarily stronger. We then include the average maturity of mortgage loans in the country.

[4] However, given that we are only able to include averages over long periods, this variable proved to be highly correlated with short-term interest rates, so we do not include it in the analysis; instead, we use the spread between the long-term and short-term interest rate.

Fourth, we include demographic characteristics, captured via population growth. If, for example, population growth is high, the transmission may be weaker, as house prices may be driven by high demand as given by demographic characteristics rather than being affected by monetary policy. Last but not least, we include several characteristics of the housing sector. In order to account for house supply factors, we include the number of building permits. The higher the number of building permits, the more open the housing market, and the more complete the transmission, while a low number of building permits may indicate restricted house supply and hampered transmission from monetary policy instead.[5] We also cover the home ownership structure. We include a proxy for tourism as a demand factor rather than a housing supply one. The last "external" variable – capturing structural heterogeneity – relates to house prices themselves. In particular, we include the standardized price-to-income ratio as a proxy for overvaluation of house prices. The price-to-income ratio, available from the OECD database, is measured as the nominal house price divided by nominal disposable income per capita and can be considered a measure of affordability. If the ratio is above its long-term average, house prices are said to be overvalued. As another potential proxy to capture overvalued house prices, we include a variable capturing the number of periods house price growth is above its long-term average. The full list of control variables is presented in Table 3.

Table 3: Description and Summary Statistics of the Regression Variables

Label	*Description*	*Mean*	*SD*
Estimate	The reported effect of a 1 percentage point change in the interest rate on the house price level, in %	-0.849	1.126
Standard Error	The reported standard error of the estimate	0.765	0.878
Data characteristics			
Monthly	= 1 if the data were collected monthly (reference category: quarterly data)	0.096	0.295
Panel	= 1 if panel data were used (ref. cat.: time series)	0.208	0.406
Length	The logarithm of the length of the data sample in the primary study in years	3.102	0.283
No. of Obs.	The total number of observations (in logarithms)	5.109	1.075
Midyear	The logarithm of the mean year of the data used minus the earliest mean year in our data plus one	2.862	0.500
VAR definition			
No. of Vars.	The number of variables included in the VAR model	1.917	0.828
GDP Defl.	= 1 if the GDP deflator is included in the VAR model (ref. cat.: CPI)	0.075	0.263
Foreign IR	= 1 if the interest rate is included	0.028	0.164
Ind. Prod.	= 1 if individual production is included as a measure of economic activity (ref. cat.: GDP in logs)	0.062	0.242
Credit	= 1 if credit is included	0.283	0.451
Consumption	= 1 if consumption is included	0.294	0.456
Res. Invest	= 1 if a measure of residential investment is included	0.185	0.388
Money Supply	= 1 if a measure of the money supply is included	0.191	0.393
Exch. Rate	= 1 if the exchange rate is included	0.233	0.423

Continued on next page

[5] The number of building permits acts as a proxy for housing supply. Other variables could serve this purpose, for example, the number of dwellings; however, the inclusion of this variable led to serious multicollinearity in our dataset. A potential candidate would be an estimate of the sensitivity of house prices to the supply side. However, we are strongly restricted by availability over the wide cross-country sample used in our paper. Therefore, we stick to the number of building permits as a house supply proxy often used in the literature (e.g., Grimes and Aitken, 2010; Paciorek, 2013)

Table 3: Description and Summary Statistics of the Regression Variables (Continued)

Label	*Description*	*Mean*	*SD*
Long-run IR	= 1 if the long-run interest rate (in addition to the short-run IR as a measure of MP) is included	0.168	0.374
Real HP	= 1 if real house prices are included (ref. cat.: nominal HP)	0.950	0.218
Lags	The number of lags (in quarters) included in the model	3.256	1.265
Time Trend	= 1 if the primary data are detrended or a time trend is added to the regression	0.395	0.489
Estimation technique			
BVAR	= 1 if a Bayesian VAR model is employed in the primary study	0.095	0.294
Sign Restr. HP	= 1 if sign restrictions are used in the VAR model and are imposed on the house price variable (ref. cat.: Cholesky decomposition)	0.052	0.222
Sign Restr. Other	= 1 if sign restrictions are used in the VAR model but are not imposed on the house price variable (ref. cat.: Cholesky decomposition)	0.029	0.169
Nonrecursive	= 1 if other nonrecursive identification is used in the VAR model (ref. cat.: Cholesky decomposition)	0.037	0.188
Publication characteristics			
Pub. Year	The logarithm of the publication year minus the earliest publication year in our data plus one	2.068	0.480
Citations	The logarithm of the number of citations of the study per year since its first appearance in Google Scholar	8.123	8.936
Impact	The recursive discounted RePEc impact factor of the outlet.	0.478	0.504
Journal	= 1 if the study is published in a journal with an impact factor	0.366	0.482
Structural heterogeneity (labeled "Country-level:" later on)			
Crisis	The number of years during which a bank crisis occurred	3.356	2.679
IR	The average 3M short-term interest rate, OECD	7.225	2.495
Prolonged Low IR	The number of consecutive years during which the short-term interest rate was below its long-run average.	8.578	4.744
10Y Gov. Bond	The average long-term interest rate on 10-year government bonds, OECD	7.643	2.141
Spread	The average difference between short-term and long-term interest rates	0.660	0.495
Floating	The share of floating IR loans	50.958	27.445
Income	Average disposable income per household per capita in US dollars, OECD	9.753	0.362
Credit-to-GDP	The credit-to-GDP ratio, BIS	124.785	33.476
Popul. Growth	Average annual population growth, World Bank	0.608	0.400
Tourism YoY	The growth rate of the number of arrivals per capita	3.463	5.032
PTI	The standardized price-to-income ratio	94.112	8.895
Prolonged High HP	The number of periods with above-average house price growth	12.180	4.662
Permits	The number of building permits issued in comparison to its long-run average.	101.394	21.977
Maturity	The average maturity of mortgage loans	22.287	4.933
Ownership	The share of home ownership	61.017	9.046
Econ. Boom	The number of periods during which an economic boom (measured as a positive output gap) occurred	5.484	3.367
Observations		1,447	

4.2 Estimation

The base equation for exploring the drivers of heterogeneity consists of regressing an estimate on its standard error (as in the publication bias regression) and on the comprehensive set of control variables. The regression is captured in the following Equation 3:

$$x_{i,j} = \alpha_0 + \beta SE_{i,j} + \sum_{l=1}^{n} \gamma_l Z_{l,ij} + \varepsilon_{i,j} \tag{3}$$

where $x_{i,j}$ is the estimate's effect, $SE_{i,j}$ is its standard error, and $Z_{l,ij}$ is the l-th control variable for the i-th estimate from the j-th study.

We employ model averaging techniques in order to explore the drivers of heterogeneity. Specifically, we use Bayesian model averaging (BMA) and frequentist model averaging (FMA) in various specifications. The advantage of this approach is that it deals robustly with the model uncertainty arising from a large number of explanatory variables. Model averaging helps resolve the issue of multiple competing model specifications. In addition, it deals with omitted variable bias in a systematic manner. BMA was pioneered in the social sciences by Raftery (1995) and Raftery et al. (1997). Recently, its use has become widespread in economics (Moral-Benito, 2015). FMA, by contrast, is a relatively novel approach in meta-analysis – the application of FMA has been pioneered by Havranek et al. (2017), Gechert et al. (2020), or Bajzik et al. (2020).

In our computations we prefer BMA to FMA for several reasons. Given the large number of control variables with respect to the number of observations, and given the multicollinearity of the control variables, BMA with its dilution prior specifically dealing with the collinearity issue is a more suitable approach. The posterior model probabilities and posterior inclusion probabilities of BMA provide more information than the simple point estimates derived from FMA (Steel, 2020), while still being easy to interpret. A detailed description of the techniques is provided in Magnus et al. (2010), Amini and Parmeter (2012), and Steel (2020), for instance.

BMA does not exclude any variable in advance. Rather, since there is no theoretical reason why any of the variables may not affect the transmission, the variables are considered as controls. BMA may run potentially 2^{41} regressions with all the possible model combinations. Such a time-consuming process is avoided by use of the Markov Chain Monte Carlo process and its Metropolis-Hastings algorithm (Zeugner and Feldkircher, 2015), which goes through the most probable models. The posterior model probability expresses the probability of inclusion of each model. The estimated coefficients for every variable are weighted by the posterior model probability through all models. For each variable we obtain a posterior inclusion probability (PIP), which denotes the sum of the posterior model probabilities of all the models in which the variable is included.

In the baseline specification the unit information g-prior (UIP) recommended by Eicher et al. (2011) gives each model the same prior weight. It constitutes our benchmark setting, addressing the lack of knowledge regarding the parameter values. Moreover, the dilution prior provides us with the benchmark model prior. This setting – discussed by George (2010) – tackles the collinearity problem that might arise from using 41 explanatory variables. The dilution prior adjusts the model probabilities by pre-multiplying them by the correlation matrix's determinant of the variables included in the particular model. The use of the dilution prior in economics and meta-analysis specifically was suggested, for instance, by Hasan et al. (2018) and Bajzik et al. (2020).

As a robustness check of our baseline BMA results, we estimate BMA using alternative g-priors and model priors. We use a combination of the unit information g-prior and the uniform model prior and a combination of the HQ g-prior and the random model prior (Fernandez et al., 2001; Ley and Steel, 2009). Moreover, FMA is applied, as mentioned above. In this model averaging technique we use Mallow's criterion for model averaging (Hansen, 2007). The covariate space is orthogonalized using the approach of Amini and Parmeter (2012).

4.3 Results

As described in the previous subsection, in order to control for model uncertainty, we use BMA for our baseline results. A visualization of the BMA results is provided in Figure 6. The variety of the best models is ordered from left to right by the posterior model probability of the individual models captured by the columns. The posterior model probability is expressed by the width of the column. The rows sort the explanatory variables based on their posterior inclusion probability from top to bottom, with the most significant variables at the top. The blue cells indicate a positive sign of the variable on the coefficient of interest. Red implies a negative relationship. When a variable is not included in a particular model, the corresponding cell remains white. Numerically, the results are captured in Table 4.

In Figure 6 we report the results on what drives the heterogeneity in the estimated response at the horizon of four quarters. We choose this horizon for our baseline results since the medium-term horizon is of the most interest to us as central bankers. In the short term, the transmission is closer to zero, while in the medium term the responses attain their peaks. Below, we will comment on the results for other horizons as well. The results show us several significant drivers of the heterogeneity in the reported impulse responses. However, the first and foremost result is the significance of the standard error, which is negatively related to the response of house prices, dragging the results away from zero and toward significantly negative numbers. This confirms the results from Section 3 – publication bias is present, and the significance and direction of the results in Section 3 is not caused by the omitted variable problem, as those results hold even after a wide range of other variables are included.

Data characteristics. Regarding data characteristics as our first category of control variables, the length of the sample included in a study matters systematically for the results, with a longer sample leading to less strong results. The other data characteristics are either just below our threshold for variables with a significant effect (as a rule-of-thumb threshold we use a posterior inclusion probability of 0.5, i.e., the variable is included in 50% of all the models), for example, the use of panel data has a low posterior inclusion probability. The mid-year of the sample does not seem to matter for the reported results after controlling for all other variables.

Specification characteristics. Second, BMA detects that the specification of the VAR model matters significantly for the reported results. The reported transmission to house prices is less strong when the long-run interest rate or the money supply is used as another endogenous variable in the model. Interestingly, the inclusion of a measure of credit does not meet the threshold for significance, as its posterior inclusion probability is below 0.5. Altogether, the results prove that proper specification and the inclusion of other variables is crucial for estimating the response of house prices, due to their endogeneity. The papers from our dataset that are published in the highest-quality journals and are highly cited also tend to include larger VAR models with a wider range of endogenous variables. The number of lags included in the VAR specification does not seem to play a statistically significant role in explaining the differences.

Estimation characteristics. Third, the estimation technique is crucial for the reported results. While the use of Bayesian estimation does not alter the results, the use of sign restrictions, when imposed on house prices, alters them significantly from both the statistical and economic point of view. At the horizon of four quarters, the posterior mean of this variable is close to -0.5, which means that if sign restrictions are used instead of Cholesky decomposition, the reported result is *ceteris paribus* 0.5 pp lower. As the sign restrictions are imposed on house prices directly, the direction of this result is rather trivial. However, the magnitude is material. Similar importance of the identification scheme is reported in Rusnak et al. (2013), who find a significant negative influence of SVAR modeling (nonrecursive identification) and conclude that it is a remedy for the price puzzle often appearing in the literature on the transmission of monetary policy to the overall price level. On the other hand, imposing sign restrictions on other variables but not on house prices does not significantly affect the results for the one-year horizon.

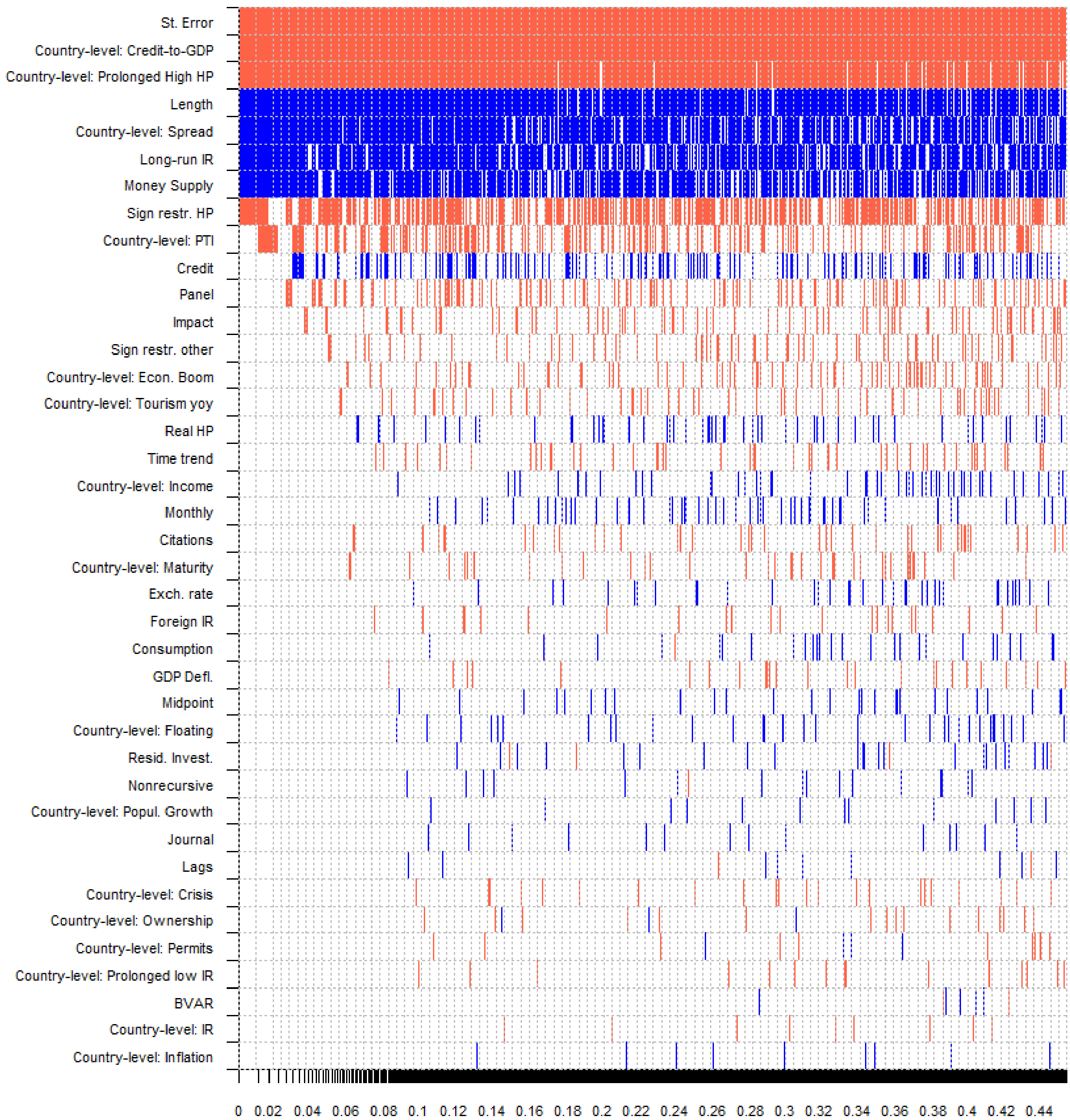
Publication characteristics. Our fourth category of control variables covers publication characteristics. A journal's impact factor records a negative posterior mean, i.e., studies published in journals with a high impact factor report more negative results, but the posterior inclusion probability is below the required threshold. Altogether, publication characteristics do not drive the systematic differences in the results reported in primary studies at this horizon.

Structural heterogeneity. Finally, we comment on the results for structural heterogeneity. Of all the external variables we included, the measure of indebtedness – the credit-to-GDP ratio – seems to be the most prominent driver of the heterogeneity of the country-specific results. The results indicate that in countries with higher credit-to-GDP ratios, the transmission from monetary policy is stronger. When the credit-to-GDP ratio is considered as a measure of financial development, the result is in line with Calza et al. (2013), who propose that the transmission is "stronger in countries with larger flexibility/development of mortgage markets". Second, the longer the period of excessive (above-average) house price growth, the stronger the transmission revealed. The other measure of overvaluation of house prices – the price-to-income ratio – yields a negative sign as well, suggesting that the transmission is stronger when house prices are overvalued; however, the posterior inclusion probability is just below our threshold. Last but not least, the country-level interest rate spread (defined as the difference between the 10-year government bond rate and the 3-month money market rate) affects the transmission toward less pronounced results – at higher term spreads, the estimated transmission from policy interest rates to house prices is less strong.

4.4 Robustness Checks

In order to provide robust evidence for our baseline results we run several other BMA specifications. First, combinations of different g-priors and model priors are examined. The comparison of the baseline specification of the dilution prior and UIP with the combination of the UIP and the uniform model prior and the combination of the HQ g-prior and the random model prior is captured in Figure 7. The figure indicates relatively stable posterior inclusion probabilities across different prior settings.

Figure 6: Baseline Results – Explaining the Differences in the Reported Results at the Four-Quarter Horizon



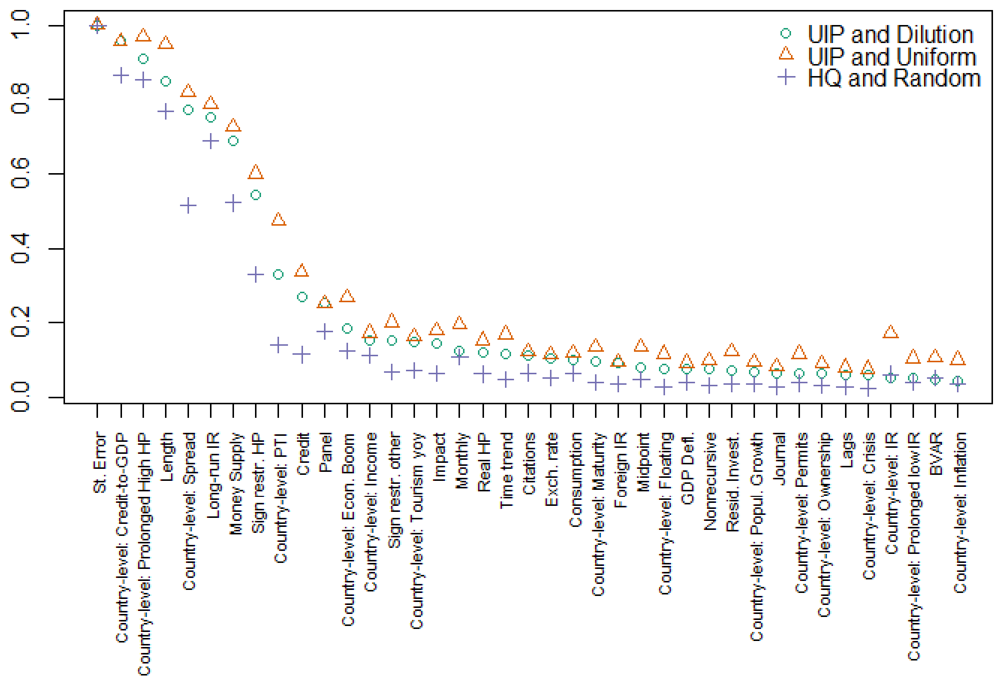
Note: The response variable is the estimated effect of a 1 percentage point change in the interest rate on the house price level after four quarters. Columns denote individual models; the variables are sorted by posterior inclusion probability in descending order. The horizontal axis denotes the cumulative posterior model probabilities; the 10,000 best models are shown. To ensure convergence we employ 3 million iterations and 1 million burn-ins. Blue color (darker in grayscale) = the variable is included and the estimated sign is positive, i.e., the transmission is weaker. Red color (lighter in grayscale) = the variable is included and the estimated sign is negative, i.e., the transmission is stronger. No color = the variable is not included in the model. The numerical results of the BMA exercise are reported in Table 4. A detailed description of all the variables is available in Table 3.

Table 4: Results of Bayesian Model Averaging, Four Quarters, Dilution Prior

	Variable	PIP	Post. mean	Post. SD
Publication bias	**SE**	**1.000**	**-1.510**	**0.143**
Data characteristics	Monthly	0.062	0.029	0.138
	Panel	0.212	-0.067	0.150
	Length	**0.941**	**1.511**	**0.560**
	Midpoint	0.036	0.008	0.056
Specification characteristics	GDP Defl.	0.036	-0.008	0.062
	Foreign IR	0.045	-0.014	0.104
	Credit	0.246	0.077	0.155
	Consumption	0.040	0.006	0.050
	Resid. Invest.	0.034	0.008	0.071
	Money Supply	**0.809**	**0.489**	**0.303**
	Exch. rate	0.046	0.007	0.050
	Long-run IR	**0.822**	**0.471**	**0.284**
	Real HP	0.075	0.032	0.143
	Lags	0.027	0.000	0.010
	Time trend	0.069	-0.013	0.060
Estimation characteristics	BVAR	0.016	0.003	0.056
	Sign restr. HP	**0.581**	**-0.465**	**0.460**
	Sign restr. other	0.106	-0.070	0.247
	Nonrecursive	0.031	0.004	0.052
Publication characteristics	Citations	0.057	-0.007	0.039
	Impact	0.119	-0.025	0.082
	Journal	0.028	0.002	0.029
Structural heterogeneity	Country-level: Crisis	0.026	0.000	0.004
	Country-level: IR	0.015	-0.001	0.011
	Country-level: Prolonged low IR	0.021	0.000	0.003
	Country-level: Spread	**0.872**	**0.378**	**0.200**
	Country-level: Floating	0.036	0.000	0.001
	Country-level: Tourism yoy	0.097	-0.002	0.007
	Country-level: Income	0.066	0.044	0.204
	Country-level: Inflation	0.012	0.000	0.006
	Country-level: Credit-to-GDP	**0.997**	**-0.010**	**0.003**
	Country-level: Popul. Growth	0.028	0.005	0.050
	Country-level: PTI	0.360	-0.007	0.010
	Country-level: Prolonged High HP	**0.968**	**-0.104**	**0.032**
	Country-level: Permits	0.021	0.000	0.001
	Country-level: Maturity	0.053	-0.017	0.099
	Country-level: Ownership	0.024	0.000	0.002
	Country-level: Econ. Boom	0.103	-0.005	0.016
Observations	209			

Note: PIP = posterior inclusion probability. SD = standard deviation. Variables with a posterior inclusion probability higher than 0.5 are shown in bold. A detailed description of all the variables is available in Table 3.

Figure 7: Posterior Inclusion Probabilities Across Different Prior Settings



Note: UIP (unit information prior) and uniform model prior = priors according to Eicher et al. (2011). The HQ prior asymptotically mimics the Hannan-Quinn criterion. PIP = posterior inclusion probability.

The deployment of frequentist model averaging yields a similar conclusion to the baseline result. The coefficients are very similar in size to those estimated via BMA. However, the number of variables that are found to be significant on the conventional level is lower. The coefficient on standard error (publication bias), the country-level variable for indebtedness (the credit-to-GDP ratio) and the country-level variable for the number of periods of above-average house price growth are all significant according to FMA. On the other hand, the p-values of the other previously significant variables, for example, the long-run interest rate as an endogenous variable in VAR models and the use of sign restrictions, are now slightly above the 10% threshold. The results of the FMA are captured in Table C1. We suggest that the baseline BMA method is more suitable in our case of a large number of variables and a relatively small number of observations.

Next, we run the BMA using the responses at all horizons at once, while controlling for each horizon by including separate dummies. In doing so, we implicitly assume that the drivers of heterogeneity are the same for each horizon and that the remaining differences across horizons can be captured by the dummy variables for the respective horizons (i.e., by a horizon-specific constant). The results of this estimation, with the same dilution model prior as above, are displayed in Figure C1. This approach naturally detects more stable and significant variables than that with one horizon only – with the full sample of more than 1,400 estimates we have a much larger number of degrees of freedom.

The main results, however, remain the same: the standard error is always the first and foremost variable when ordered according to posterior inclusion probability. Second, the use of sign restrictions yields a more negative (stronger) response. Interestingly, this result holds irrespective of whether sign restrictions are imposed on other variables in the model or also on house prices – both approaches lead to a significantly stronger effect of monetary policy on house prices. Third, countries with higher credit-to-GDP ratios experience stronger transmission, as do countries with higher house price growth. Fourth, the variable capturing prolonged periods of above-average house price growth is important here as well. As in the baseline results, the inclusion of endogenous variables in the VAR model matters for the estimated impulse responses: in addition to the long-run interest rate, the inclusion of residential investment and consumption systematically affects the results. Additional variables that prove to be important drivers of the heterogeneity in the estimates, in contrast to the horizon of four quarters only, include BVAR estimation (leading to more negative responses) and the impact factor and the number of citations – more frequently cited studies are associated with more negative responses, as are studies published in journals with a higher impact factor. As far as structural heterogeneity is concerned, a country's share of floating-rate loans yields a posterior inclusion probability above 0.5. The direction of the "floating" variable, however, is opposite to our initial expectations, as described in Section 4.1. The dummy variables for horizons of 2, 4, 8, 12, and 16 quarters are significant as expected. Altogether, although several variables are newly discovered to be significant, the most important variables detected in the single-horizon estimation and the multiple-horizon estimation are the same, suggesting that the drivers of heterogeneity do not necessarily differ across horizons.

A similar view is provided when we estimate Equation 3 by a simple frequentist check for each horizon separately, as reported in Table C2 in the Appendix. The results reported in this table are estimated using simple OLS; this simultaneously provides a robustness check of our BMA results. First, looking at the four-quarter horizon, we see the same variables marked as significant, plus some additional variables. Second, the results for the eight-quarter horizon are very similar to those for four quarters, both from the point of view of direction and size and as regards statistical significance. As far as the other horizons are concerned, there are naturally some variables for which the significance differs, for example, the inclusion of credit in the VAR model, nonrecursive identification, and even the use of sign restrictions on house prices. These mostly have an effect at short and medium horizons and not longer ones, while imposing sign restrictions on variables other than house prices is significant and has a stronger effect at longer horizons. The other main results hold across all horizons: the standard error is negatively and significantly related to the estimated impulse responses, and the inclusion of the long-term interest rate shows congruent results as well. As far as structural heterogeneity is concerned, the credit-to-GDP variable is negative and significant consistently across all medium and long horizons, and stronger transmission is linked to longer periods of above-average house price growth.

4.5 The Implied Response to a Change in Monetary Policy

We have identified several drivers of the heterogeneity of the estimates in the existing literature, both on the part of method and data choices and on the part of structural heterogeneity. Those drivers can be used to create an overall synthetic study and calculate the *implied effect*, i.e., the effect as given by all the evidence available in the literature but weighted by the appropriateness of the variables. To estimate the implied effect, the effect beyond publication bias and beyond systematic drivers of heterogeneity, we need to specify preferred values for all the control variables, i.e., to create a synthetic study, and calculate the fitted value of Equation 3. We plug maxima (in the case of dummy variables) or 90th percentile values (in the case of numerical variables) for the variables we consider to be the best practice as far as data and method choices are concerned, means for the variables we do not possess any strong opinion about, and minima (or the 10th percentile, alternatively) for the variables that deviate from best practice.

We put more weight on highly cited studies, journal publications, and journals with a high impact factor. We also prefer newer studies and studies with a longer time span included. We prefer the

BVAR model, as it is considered to be superior in many cases. We allow for sign restrictions and prefer them to recursive ordering, but only if a restriction is put on other endogenous variables in the model; we do not allow sign restrictions to be imposed directly on house prices. For most of the variables, especially for dummies capturing the inclusion of specific endogenous variables in the VAR model, we follow the three best papers in the literature. We specify the best papers according to the following criteria: (i) the discounted recursive impact factor of the journal, and (ii) the article's number of citations per year. Where even the best papers are inconclusive, we use average values. By doing so, we prefer models that include residential investment as an additional endogenous variable, and models that include the GDP deflator as a price measure (given by the top papers), although we calculate an alternative implied estimate along this dimension as well. In the first step, we also use mean values for all external variables capturing structural heterogeneity. The only external variables where we deviate from mean values are those capturing whether there is a crisis, a boom, or a period of prolonged low interest rates – for all these cases, we insert zero and thus calculate the effect in a "normal" period. We also include the 10th percentile value for the interest rate to capture the already long period of low interest rates, as a short-term interest rate of 7% – the average over the long sample included in our dataset – no longer seems relevant. However, we also provide implied estimates for other ways of creating a synthetic study.

The results of this exercise are displayed in Table 5, with the baseline implied estimate of the effect of monetary policy on house prices displayed in the first row. We assert that the implied effect is slightly lower (i.e., stronger) than the mean effect reported across the literature. The effect is statistically different from zero even in the first quarters, implying that the effect of monetary policy on house prices may in fact be even stronger than reported in the literature on average. The strongest response appears at the eight-quarter horizon and is equal to a 1.6% decline in house prices after a 1 pp increase in interest rates; the decline is equal to 1.4% at the four-quarter horizon.

However, the results differ when credit and the long-run interest rate are also included as endogenous variables in the VAR model. Rather surprisingly, they are not uniformly included in the top papers, so they were not included in the baseline implied estimate. Nevertheless, we assert that they are important variables with an endogenous relationship both to monetary policy and to house prices, hence they should be considered when estimating the transmission. In such case, the mean implied response of house prices is *less* strong, reaching around −0.9% at the lowest point. It is not statistically significant at the short horizon (although this may also be due to the wide confidence intervals caused by the inclusion of a large number of significant and insignificant variables in the calculation of the implied responses). We also provide the implied estimate for another case where more weight is put on studies with fewer citations and a lower impact factor. In so doing, we control for the fact that the best publication characteristics may not automatically be the "best practice" approach. Instead, they may carry the inherent bias that stronger and more significant responses are published more readily. In fact, less cited studies and studies in journals with lower impact factors do provide responses that are around one quarter smaller; however, the response remains significant at most of the horizons. We also provide an example of what happens when house prices are already overvalued, showing that the response is even stronger than in the baseline.

In the second part of Table 5, we offer implied estimates for the eight countries that most frequently appear in the literature (the same countries were shown in Figure 5). For that, we insert the newest values for all the available country-level variables (for the rest of the variables the same setting as in the baseline implied estimate is used). The most important driver as far as structural heterogeneity is concerned is the level of indebtedness, dividing countries into two notional groups. Countries with relatively low credit-to-GDP ratios (for example, Italy and Germany) are associated with weaker transmission of monetary policy to house prices, and countries with high credit-to-GDP ratios (for example, Switzerland, France, and Sweden) are associated with stronger transmission. The average response in the euro area seems to be slightly stronger than that in the US. The implied response in the Czech Republic is similar to the mean value reported in the literature. We can see that in almost all countries the implied effect is slightly stronger than the mean effect reported in the literature.

Table 5: Results of the Synthetic Study

	Horizon					
	1 quarter	2 quarters	4 quarters	8 quarters	12 quarters	16 quarters
Implied response – baseline	-0.921**	-1.153**	-1.367***	-1.598***	-1.463***	-1.219**
With credit and LR int. rate	-0.185	-0.417	-0.632	-0.862**	-0.726*	-0.483
With worse publication char.†	-0.545	-0.777**	-0.992**	-1.222***	-1.087**	-0.843**
With overvalued house prices‡	-1.074*	-1.306***	-1.521***	-1.751***	-1.616***	-1.372***
Finland	-0.697	-0.929**	-1.143***	-1.374***	-1.239***	-0.995**
France	-2.016***	-2.249***	-2.463***	-2.694***	-2.559***	-2.315***
Germany	-0.343	-0.576	-0.790	-1.021*	-0.886*	-0.642
Italy	-0.619	-0.851	-1.067	-1.297	-1.162	-0.918
Sweden	-1.737*	-1.968***	-2.183***	-2.414**	-2.279***	-2.035***
Switzerland	-1.734***	-1.966***	-2.180***	-2.411***	-2.277***	-2.032***
United Kingdom	-1.670***	-1.932***	-2.147***	-2.377***	-2.242***	-1.998* **
United States	-1.105**	-1.338***	-1.552***	-1.783***	-1.647***	-1.404***
European Union	-1.241**	-1.473***	-1.687***	-1.918***	-1.783***	-1.539***
Czech Republic	-0.759	-0.991*	-1.205**	-1.436**	-1.301**	-1.057*

Note: The values represent the percentage response of house prices to a 1 percentage point increase in the interest rate. ***, **, and * denote significance at the 1%, 5%, and 10% level.
† = lower centile (10th instead of 90th) of number of citations and impact factor used. ‡ = upper centile (90th) instead of mean value of price-to-income ratio.

5. Conclusion

In this paper we examined the effect of monetary policy on house price levels. We did so by reviewing and synthesizing empirical results from 31 studies estimating the effect in 27 countries. We collected more than 200 graphical impulse responses of VAR models, the main approach used to estimate the transmission of monetary policy. For each impulse response function we collected responses at different horizons. By means of meta-analysis, we were able to synthesize the existing literature and estimate the true (underlying) effect of monetary policy on house prices while correcting for potential publication bias and various data and method choices.

First, while the existing literature can be assessed as homogeneous in the direction of the effect, it is not unified in the strength and significance of the transmission – in this regard, the estimates in the literature vary considerably. Thus, in the first part of the paper, we evaluated the simple means and medians of all the estimates and provided overall and country-specific synthesized impulse response functions.

Second, we examined the extent of publication bias by means of linear and nonlinear methods based on assumptions about the relationship between the estimates and their standard errors, i.e., linear OLS regression and the stem-based method by Furukawa (2019), but also methods not requiring any assumption about the relationship between these two quantities, such as the caliper test by Gerber and Malhotra (2008a) and the selection model by Andrews and Kasy (2019), all of them state-of-the-art methods of the current meta-analysis approach in economics (Havranek et al., 2020). Our results provide evidence of publication selection: the linear tests suggest that publication selection is omnipresent across all horizons. The transmission of monetary policy beyond bias is less strong than the effect presented in the literature.

Third, we examined the drivers of the heterogeneity in the results. As potential drivers, we identified data, specification, estimation, and publication characteristics, as well as structural (country-specific) factors. Our results show that at least one variable in each category systematically affects the impulse responses reported in the primary studies. We identified the use of sign restrictions, the inclusion of additional endogenous variables, and a country's level of indebtedness as the most prominent drivers of the heterogeneity. However, even after correcting for all these factors, the evidence of publication selection (the relationship between the estimate and its standard error) remains strong and significant. Last but not least, we calculated the implied impulse responses over various dimensions. The transmission differs significantly across countries. The maximum effect, attained at the end of the medium horizon, varies from -0.9% to -2.7% in response to a monetary policy shock of a one percentage point increase in the interest rate. Although correcting for publication selection makes the implied impulse responses considerably less strong than those originally presented in the literature, after controlling for other misspecifications in the data and method choices as specified in our synthetic study, the resulting true effect of monetary policy is even slightly stronger than that reported in the literature on average.

To summarize, we have provided the first quantitative synthesis of the large literature on the transmission between monetary policy and house prices. However, at least two caveats are in order with respect to our results. First, the calculation of the implied impulse responses the synthetic study is subjective, although we try to eliminate this problem by calculating the implied effects over several dimensions and by referring to the objectively highest-quality papers in the literature. Second, in order to make our collected estimates directly comparable, we only include studies using house price levels and not growth rates. Thus, there is room for further research comparing the direction and significance of our results with those based on this second strand of literature.

The outcome of the project may have significant policy implications. The effects of accommodative monetary policy have been discussed a great deal during the last decade both in the public arena and in the academic field, and the discussion is still ongoing, especially given the recent further easing of monetary policy in the euro area. At the same time, house prices are rising at a high pace in many countries both inside and outside the euro area, together with rising levels of indebtedness. The prolonged period of accommodative monetary policy and negative real interest rates has created potential for rising vulnerabilities in the financial system. The results of the project could be used in discussions regarding the interactions of monetary and macroprudential policies and their joint conduct. Large space for future research remains open – it would be interesting to study this topic using the literature on house price inflation rates and to examine the effect of unconventional monetary policy on house prices.

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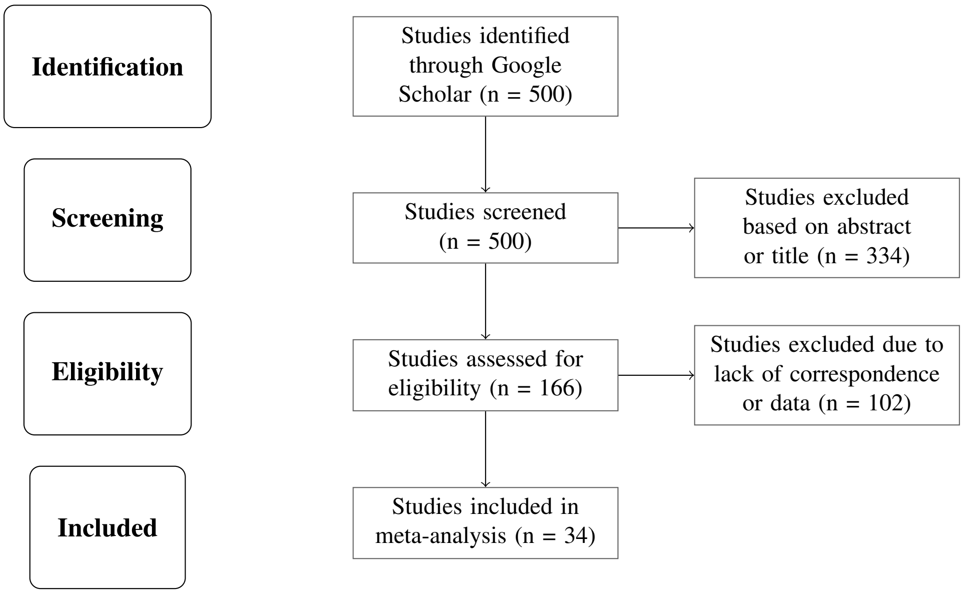
Appendix A: Studies Included

Figure A1: PRISMA Flow Diagram



Appendix B: Extensions to the Publication Bias Tests

B.1 The Caliper Test

As an extension to the previous results on publication bias, we proceed with the caliper test as proposed in Gerber and Malhotra (2008a) and Gerber and Malhotra (2008b) and recently implemented in Bruns et al. (2019). The caliper test is based on the analysis of discontinuities in reported t-statistics: if no selective reporting is present, there should be no discontinuities around the conventional significance thresholds. In other words, the number of t-statistics reported in the literature just above the threshold ("over caliper") should not be statistically different from the number of reported t-statistics just below the threshold ("under caliper"). The test does not tell us the true effect beyond bias but serves as an indicator of whether publication selection exists, providing us with a check of the previous results. The results are presented in Table B1. Primarily, we examine the significance threshold of a 68% confidence interval: although the threshold is usually much stricter in the empirical literature featuring point estimates, in the case of VAR models and impulse response functions the 68% confidence interval is the most frequently reported (almost 70% of our estimates), so we suspect that publication selection could happen around this threshold. We use caliper sizes of 0.1, 0.3, and 0.5. The results show that publication selection is present at the horizons of eight quarters and one quarter. If we test the parameter against the value of 0.4 (i.e., a 60:40 distribution around the thresholds, instead of 50:50, as reasoned in Bruns et al. (2019)), then evidence of publication selection is also present at the horizon of four quarters and when all the horizons are tested together. This is broadly in line with our previous results on publication selection.

Table B1: Caliper Test

			Horizon					
	Caliper size	All	1 quarter	2 quarters	4 quarters	8 quarters	12 quarters	16 quarters
	0.1	*0.521*	**0.722**	0.500	0.444	0.429	0.467	0.471
	(95% LCI)	*(0.436)*	**(0.533)**	(0.289)	(0.118)	(0.035)	(0.231)	(0.252)
	0.3	*0.527*	**0.625**	0.512	*0.556*	**0.625**	0.477	0.434
68%	(95% LCI)	*(0.482)*	**(0.507)**	(0.379)	*(0.430)*	**(0.516)**	(0.349)	(0.319)
	0.5	*0.509*	**0.613**	0.452	*0.560*	**0.595**	0.486	0.422
	(95% LCI)	*(0.474)*	**(0.521)**	(0.354)	*(0.464)*	**(0.506)**	(0.387)	(0.331)

Note: Caliper test for various caliper sizes (0.1, 0.3, 0.5). The reported numbers represent the share of observations in a narrow interval around the significance threshold. The test parameter is the following: $C = \frac{n_{oc}}{n_{oc}+n_{uc}}$, where n_{oc} and n_{uc} stand for the number of observations with t-statistics in the interval above the threshold ("over caliper") and below the threshold ("under caliper"), respectively. The one-sided hypothesis $H_0 : C \leq 0.5$ is tested against $H_1 : C > 0.5$. 95% lower confidence intervals for the test parameters are reported in parenthesis. Significant caliper test results when $H_0 : C \leq 0.5$ are shown in bold; significant caliper test results when $H_0 : C \leq 0.4$ are indicated in italics.

B.2 Publication Bias in P-Values

Second, we focus our attention on the bias in p-values and ask whether authors only publish estimates which are significant. To answer that question, we can apply a new technique in the field of meta-analysis that evaluates the distribution of p-values to find out whether there is publication selection and whether (or what) is the evidential value in the existing literature. Based on Figure B1, we would assert that publication bias is not present – the distribution of the p-values is strongly right-skewed, and most of the p-values are very low, although there is an increase at the 0.05 level. A right-skewed distribution suggests there is evidential value, while a left-skewed distribution suggest strong p-hacking. The latter is not the case with our dataset. The procedure finds evidential value in the dataset, but the p-curve is overestimated in comparison to other methods. In contrast to Figure B1, we plot the whole distribution of p-values (not only those significant up to the 5% significance level) to see that there is distinct jump at close-to-zero p-values but no abnormal jumps at p-values of 5% (which could be a potential attractor for p-hacking).

Figure B1: P-Curve – Distribution of Significant P-Values

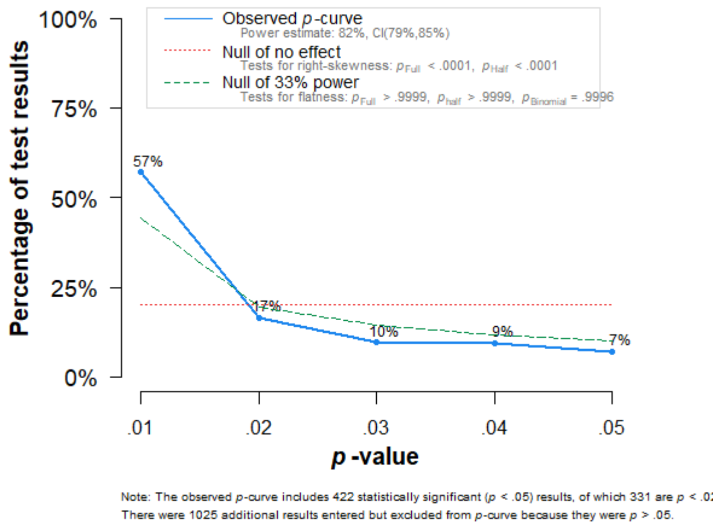
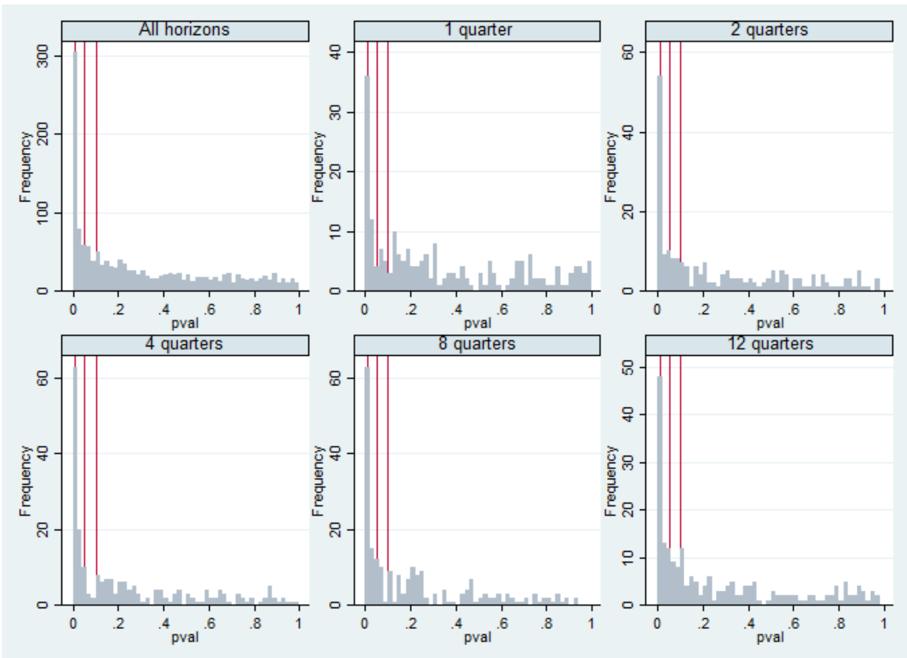


Figure B2: Distribution of All P-Values



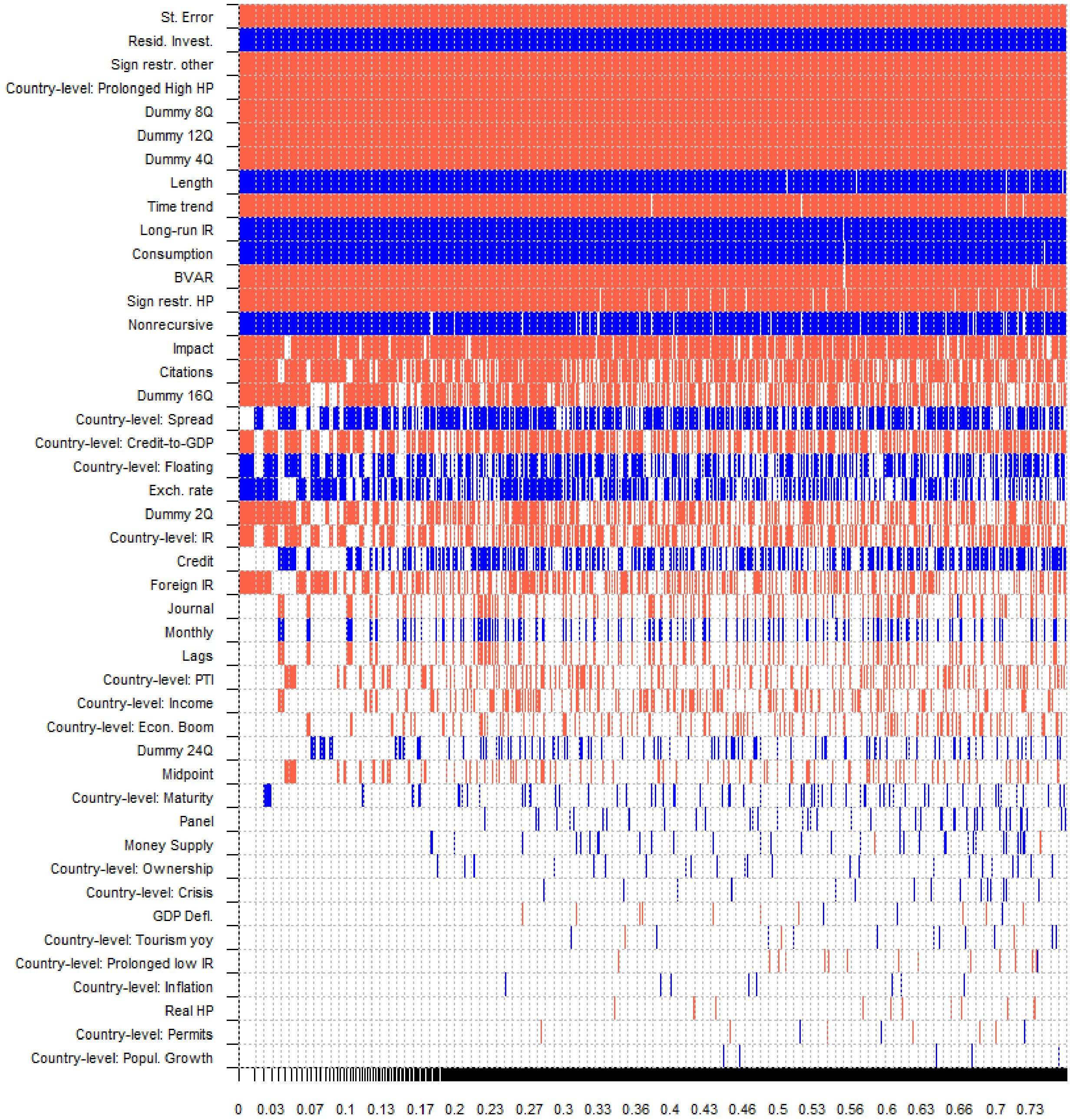
Appendix C: Additional Results

Table C1: Results of Frequentist Model Averaging, Four Quarters

	Variable	Coef.	Std. Er.	*P*-value
Publication bias	**SE**	**-1.555**	**0.183**	**0.000**
Data characteristics	Monthly	0.185	0.552	0.737
	Panel	-0.026	0.284	0.927
	Length	0.887	0.711	0.212
	Midpoint	-0.024	0.287	0.934
Specification characteristics	GDP Defl.	0.075	0.303	0.805
	Foreign IR	-0.108	0.370	0.770
	Credit	0.275	0.222	0.215
	Consumption	0.249	0.293	0.395
	Resid. Invest.	0.415	0.390	0.287
	Money Supply	0.473	0.352	0.179
	Exch. rate	0.230	0.270	0.393
	Long-run IR	*0.495*	*0.322*	*0.125*
	Real HP	0.191	0.347	0.582
	Lags	0.030	0.085	0.726
	Time trend	-0.211	0.218	0.333
Estimation characteristics	BVAR	-0.727	0.627	0.246
	Sign restr. HP	*-0.685*	*0.484*	*0.157*
	Sign restr. other	-1.232	0.860	0.152
	Nonrecursive	0.102	0.387	0.792
Publication characteristics	Citations	-0.023	0.242	0.925
	Impact	-0.235	0.243	0.332
	Journal	-0.116	0.266	0.663
Structural heterogeneity	Country-level: Crisis	0.032	0.042	0.450
	Country-level: IR	-0.166	0.113	0.140
	Country-level: Prolonged low IR	-0.023	0.031	0.455
	Country-level: Spread	0.238	0.218	0.274
	Country-level: Floating	0.005	0.004	0.284
	Country-level: Tourism yoy	-0.009	0.015	0.557
	Country-level: Income	0.448	0.687	0.515
	Country-level: Inflation	0.068	0.069	0.325
	Country-level: Credit-to-GDP	**-0.013**	**0.007**	**0.077**
	Country-level: Popul. Growth	0.513	0.543	0.345
	Country-level: PTI	-0.030	0.019	0.127
	Country-level: Prolonged High HP	**-0.069**	**0.041**	**0.093**
	Country-level: Permits	0.005	0.007	0.451
	Country-level: Maturity	-0.248	0.474	0.601
	Country-level: Ownership	-0.017	0.019	0.368
	Country-level: Econ. Boom	-0.025	0.035	0.472
Observations	209			

Note: Our frequentist model averaging (FMA) exercise employs Mallow's weights (Hansen, 2007) and the orthogonalization of the covariate space suggested by Amini and Parmeter (2012). Results significant at the 10% level are shown in bold; results that were significant in BMA and have a p-value lower than 0.15 are indicated in italics.

Figure C1: Explaining the Differences in the Reported Results Across All Horizons



Note: The response variable is the estimated effect of a 1 percentage point change in the interest rate on the house price level across all horizons. Columns denote individual models; the variables are sorted by posterior inclusion probability in descending order. The horizontal axis denotes the cumulative posterior model probabilities; the 10,000 best models are shown. To ensure convergence we employ 3 million iterations and 1 million burn-ins. Blue color (darker in grayscale) = the variable is included and the estimated sign is positive, i.e., the transmission is weaker. Red color (lighter in grayscale) = the variable is included and the estimated sign is negative, i.e., the transmission is stronger. No color = the variable is not included in the model. The numerical results of the BMA exercise are reported in Table 4. A detailed description of all the variables is available in Table 3.

Table C2: Explaining the Differences in the Reported Impulse Responses, OLS

	Variable	1 quarter	2 quarters	4 quarters	8 quarters	12 quarters	16 quarters
Publication bias	SE	-0.805***	-1.215***	-1.615***	-1.553***	-0.665***	-0.365***
		(0.204)	(0.294)	(0.278)	(0.168)	(0.230)	(0.122)
Data characteristics	Panel	-0.314***	-0.228	0.0151	0.166	0.439	0.752**
		(0.0822)	(0.141)	(0.186)	(0.268)	(0.286)	(0.294)
	Length	-0.386	0.874	1.170*	1.831***	0.644*	0.171
		(0.371)	(0.533)	(0.631)	(0.641)	(0.387)	(0.294)
Specification characteristics	GDP Defl	0.395***	0.638**	0.0965	-0.264	-0.244	-0.212
		(0.150)	(0.313)	(0.197)	(0.206)	(0.194)	(0.212)
	Credit	-0.0615	0.126*	0.299*	0.532***	0.0426	0.101
		(0.0475)	(0.0686)	(0.166)	(0.193)	(0.0858)	(0.108)
	Consumption	0.115	0.295***	0.381**	0.514*	0.799***	0.811**
		(0.0718)	(0.0722)	(0.176)	(0.295)	(0.307)	(0.364)
	Resid. invest.	0.569***	0.610***	0.516**	0.494	0.918***	0.973***
		(0.176)	(0.188)	(0.223)	(0.324)	(0.241)	(0.272)
	Money Supply	-0.229**	-0.0266	0.527***	0.747***	0.0456	-0.102
		(0.117)	(0.152)	(0.191)	(0.221)	(0.351)	(0.377)
	Exchange rate	-0.0528	0.175***	0.350***	0.529***	0.741***	0.550
		(0.0751)	(0.0556)	(0.0491)	(0.133)	(0.105)	(.)
	Long-run IR	0.0225	0.266**	0.577***	0.762***	0.574***	0.325***
		(0.0698)	(0.107)	(0.156)	(0.212)	(0.0582)	(0.0343)
	Real HP	-0.443**	-0.230	0.138	0.279**	-0.426	-0.404
		(0.202)	(0.196)	(0.134)	(0.136)	(0.352)	(0.429)
	Lags	-0.0839**	-0.0469	0.0491	-0.0286	-0.0975**	-0.184***
		(0.041)	(0.047)	(0.031)	(0.058)	(0.040)	(0.044)
	Time trend	-0.119	-0.308**	-0.315***	-0.637***	-0.531***	-0.500***
		(0.114)	(0.122)	(0.110)	(0.178)	(0.142)	(0.159)
Estimation characteristics	BVAR	-0.610*	-1.054**	-0.813*	-0.724	-1.488***	-1.038***
		(0.332)	(0.478)	(0.442)	(0.460)	(0.463)	(0.352)
	Sign restr. HP	-0.692***	-1.021***	-0.830*	-0.208	-0.313	0.275
		(0.185)	(0.369)	(0.428)	(0.471)	(0.476)	(0.392)
	Sign restr. other	0.232	-0.629*	-1.567***	-2.613***	-2.948***	-2.787***
		(0.167)	(0.330)	(0.303)	(0.728)	(0.480)	(0.500)
	Nonrecursive	0.704***	0.761**	0.305	0.164	1.307***	1.331***
		(0.190)	(0.306)	(0.293)	(0.309)	(0.398)	(0.446)
Publication characteristics	Citations	-0.159*	-0.153	-0.132	-0.164	-0.582***	-0.543***
		(0.095)	(0.183)	(0.225)	(0.215)	(0.153)	(0.203)
	Impact	-0.082	-0.252	-0.289*	-0.480	-0.660***	-0.639***
		(0.092)	(0.155)	(0.165)	(0.325)	(0.143)	(0.163)
Structural heterogeneity	Country-level: IR	-0.142**	-0.259**	-0.233**	-0.261***	-0.237***	-0.195***
		(0.070)	(0.111)	(0.110)	(0.066)	(0.029)	(0.017)
	Country-level: Spread	-0.255***	-0.108	0.287*	0.436***	0.419***	0.236*
		(0.097)	(0.202)	(0.157)	(0.122)	(0.124)	(0.142)
	Country-level: Floating	-	0.004	0.004	0.008***	0.008**	0.009**
		-	(0.005)	(0.006)	(0.003)	(0.003)	(0.004)
	Country-level: Tourism YoY	-0.023*	-0.030**	-0.007	0.004	0.005	-0.009
		(0.012)	(0.015)	(0.017)	(0.014)	(0.009)	(0.011)
	Country-level: Inflation	0.056	0.144***	0.096**	0.032	0.051	0.000
		(0.044)	(0.041)	(0.039)	(0.041)	(0.038)	(0.033)
	Country-level: Credit-to-GDP	-0.001	-0.012	-0.016*	-0.017***	-0.016***	-0.011***
		(0.005)	(0.007)	(0.008)	(0.005)	(0.000)	(0.002)
	Country-level: Maturity	0.776***	0.0632	-0.329	-0.259	0.808***	1.181***
		(0.240)	(0.711)	(0.717)	(0.611)	(0.237)	(0.391)
	Country-level: PTI	-0.019*	-0.024	-0.039***	-0.046***	-0.016**	-0.011
		(0.010)	(0.023)	(0.014)	(0.005)	(0.007)	(0.008)
	Country-level: Prolonged High HP	-0.012	-0.083**	-0.090**	-0.105**	-0.115***	-0.090***
		(0.009)	(0.033)	(0.039)	(0.041)	(0.018)	(0.020)
	Observations	196	199	209	209	204	203

Note: Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Due to space limitations, variables which are not significant at any horizon or are significant at a maximum of one horizon are excluded from the table.

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ISSN 1803-7070