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Details Matter: Loan Pricing and Transmission of Monetary Policy in the Euro Area

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Details Matter: Loan Pricing and Transmission of Monetary Policy in the Euro Area*

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Does the maturity of the relevant risk-free rate influence the strength of monetary policy pass-through to interest rates on new loans? To address this question, we present novel empirical evidence on lending practices across all euro-area countries, using AnaCredit data covering nearly 7 million new loans issued to nonfinancial corporations in 2022–23. We document substantial variation in (i) the prevalence of fixed- versus floating-rate loans, (ii) rate fixation periods, and (iii) reference rates. This variation results in lending rates being exposed to different segments of the risk-free rate yield curve which, in turn, influence their sensitivity to monetary policy changes. We show that loans linked to shorter-maturity risk-free rates experience more pronounced monetary pass-through. Importantly, this effect is not purely mechanical, as part of the effect is offset by adjustments in the premium, revealing previously less-explored heterogeneity in the pass-through to lending rates.

JEL Codes: E52, E43, G21, E58.

1. Introduction

Despite the single currency and common monetary policy, credit markets across euro-area member states exhibit significant heterogeneity in several dimensions (Altavilla, Gürkaynak, and Quaedvlieg 2024; Kosekova et al. 2023). One notable example is the prevalence of fixed- versus floating-rate loans (Albertazzi, Fringuellotti, and Ongena 2024). Fixed-rate loans feature nominal interest rates that remain constant over the entire duration of the loan, whereas floating-rate loans have interest rates that change over time, typically indexed to a reference rate. The latter have been shown to be more sensitive to short-term money market rate fluctuations, amplifying the transmission of monetary policy (Ippolito, Ozdagli, and Perez-Orive 2018; Tzamourani 2021).

In this paper, we argue that the mere classification of loans into fixed or floating rate fails to fully capture their distinct sensitivity to monetary policy changes. Our results show that even within these two broad loan categories, other characteristics play a significant role in the transmission of monetary policy. First, the maturity

of the reference rate for floating-rate loans can vary substantially.¹ Loans with shorter reference rate maturities are more exposed to changes in monetary policy, as their interest rates quickly reflect shifts in short-term rates. Similarly, for fixed-rate loans, the degree of sensitivity depends on loan maturity (Gürkaynak, Karasoy-Can, and Lee 2022). Newly issued fixed-rate loans with shorter maturities are priced based on shorter-term risk-free rates which tend to closely track changes in policy rates. By contrast, longer-maturity fixed-rate loans are less responsive, as the underlying risk-free rate reacts more moderately to monetary policy shifts.² Taken together, a fixed-rate loan with, for instance, a 6-month maturity will most likely have an interest rate closer to the monetary policy rate than a floating-rate loan with a 12-month reference rate.

In this study we address the following question: does the maturity of the relevant risk-free rate affect the strength of monetary policy pass-through to interest rates on new loans? While the role of loan characteristics in monetary transmission has been examined in some depth for outstanding loans, their relevance for the pass-through to newly issued loans remains comparatively underexplored. We leverage unique loan-level information from the euro area's credit data set (AnaCredit), encompassing nearly 7 million newly issued loans to nonfinancial corporations (NFCs) in 2022–23, to examine the lending practices across all euro-area countries and investigate how these can shape the transmission of monetary policy. We first document the prevalence of fixed- and floating-rate loans, the differences in duration of rate fixation periods, as well as reference rates used in loan pricing. We build on this novel empirical evidence to move beyond the binary classification of fixed- versus floating-rate loans and propose a nuanced measure of loan sensitivity to short-term rate changes. The main advantage of our approach is that we are able to account for the maturity of the risk-free rate that is relevant for each

¹The frequency of interest rate adjustments can also vary to a large extent. However, our analysis shows that this, in most cases, aligns closely with the maturity of the underlying reference rate.

²For outstanding fixed-rate loans the residual maturity is another important factor for the sensitivity to monetary policy changes. Loans with shorter residual maturities are more exposed to the prevailing monetary policy rates if they need to be refinanced, which is particularly important during periods of sharp changes in interest rates, such as the post-pandemic monetary tightening.

loan. For fixed-rate loans, this corresponds to loan maturity at origination, while for floating-rate loans, it corresponds to the maturity of the underlying reference rate. Thus, we identify the specific segment of the risk-free rate yield curve that influences a loan's interest rate.

The fact-finding exercise provides empirical evidence about previously less-explored variation in lending practices across the euro area: (i) Floating-rate loans predominate in smaller euro-area countries (e.g., Latvia, Lithuania, Cyprus, Finland, Estonia, and Ireland), accounting for approximately 90 percent of new loans in 2022–23, whereas they account for only about one-third in the Netherlands, Belgium, and Germany. (ii) For floating-rate loans, much of the observed variation across countries stems from the maturity of reference rates, which range from overnight rates to 12-month rates. For example, although Cyprus and Estonia have a high share of floating-rate loans, the reference rate maturities differ substantially. In Cyprus, over one-third of floating-rate loans are tied to reference rates with maturities of one month or less, whereas in Estonia, the majority are benchmarked to reference rates with maturities of three to six months. (iii) For fixed-rate loans, the variation arises from differences in loan maturity which, again, varies considerably across euro-area countries. In France and Italy, fixed-rate loans account for more than half of all new loans to NFCs, yet their typical maturities differ markedly. In Italy, most fixed-rate loans have a maturity of 1 year or less, while in France the majority exceeds 5 years or even 10 years.

In light of this observed variation across euro-area countries, we provide a more granular measure of sensitivity reflecting the maturity of the relevant risk-free rate. Specifically, we decompose the loan interest rate into the relevant risk-free rate at the time of issuance and the corresponding premium.³ We show that the average maturity of the relevant risk-free rate for new loans ranges from approximately six months in Latvia and Ireland to over five

³Overnight indexed swap (OIS) rates serve as a robust proxy for risk-free rates, as they are directly linked to central bank policy rates. OIS rates reflect market expectations of monetary policy actions, since they are derived from overnight lending rates, which are closely guided by central bank interventions. Additionally, OIS markets are highly liquid and transparent, enabling real-time tracking of how monetary policy decisions influence interest rate expectations.

years in the Netherlands, Malta, and France. However, the average maturity of the relevant risk-free rates does not show a clear distinction between countries where fixed-rate loans predominate and those where floating-rate loans are more prevalent.

Since monetary policy affects risk-free rates differently depending on their maturity, we next show that differences in the maturity of relevant risk-free rates contributed to variations in how lending rates reacted during the post-pandemic monetary tightening. The interest rate increase for new loans between 2022:Q1 and 2023:Q4 was largely driven by the rise in the relevant risk-free rates. The contribution of these rates was particularly pronounced in some euro-area countries such as Latvia and Ireland, where floating-rate loans with short fixation periods predominate, as well as Italy, where shorter-maturity fixed-rate loans are common. In contrast, countries like the Netherlands and France, where longer-term risk-free rates play a more significant role in loan pricing, experienced a more moderate increase in lending rates due to smaller increases in these rates. This finding holds after controlling for a large set of loan-level variables and a rich set of fixed effects to account for differences in lending rate levels across countries, macroeconomic sectors, regions, and firm size. Importantly, the rise in risk-free rates was partially offset by a decline in the premium,⁴ which limited the overall rise in lending rates. We find that this smoothing role of the premium is consistently present throughout the sample period and is not driven by time-varying bank heterogeneity. These results suggest that banks adjusted the premium in a way that limits the increase in lending rates for loans most exposed to rising short-term risk-free rates. This helped to smooth cross-loan differences in lending rate changes that would have been larger if lending rates had moved purely in line with the relevant risk-free rates.

We go one step further and analyze new loans to NFCs issued within a six-week period around 15 European Central Bank (ECB) Governing Council (GovC) meetings held between February 2022 and October 2023 to show that pass-through of unexpected changes

⁴The term “premium” used in this study does not refer exclusively to compensation for the default risk of the borrower. Instead, it encompasses compensation for all risks a lender assumes beyond the risk-free alternative.

in monetary policy rates strengthens for new loans linked to shorter-maturity risk-free rates. Our findings reveal that loans with shorter maturities of the relevant risk-free rates—regardless of whether they are fixed or floating rate—experience a more pronounced pass-through of monetary policy surprises. However, this effect is not purely mechanical, as part of the increase in risk-free rates is offset by adjustments in the premium. Using our estimates, a straightforward calculation suggests that the variation in the extent to which loans are exposed to short-term rates has significant implications for the aggregate pass-through from monetary policy rates to lending rates. Specifically, the pass-through in the euro area is approximately 13 percent stronger than it would be if all new loans had fixed interest rates for their entire duration. Conversely, the pass-through is significantly weaker (by about 30 percent) than if all loans were tied to risk-free rates with maturities of one month or less (e.g., if all loans were floating-rate loans linked to the one-month EURIBOR, or euro-area interbank offered rate). From a monetary policy perspective, these results are important, as they reveal previously less-explored heterogeneity in the pass-through of monetary policy to lending rates within the single monetary union that depends on how monetary policy affects the shape of the yield curve.

Our study contributes to two strands of the existing literature. First, we add to previous studies that have documented significant variation in lending practices across euro-area countries. Kosekova et al. (2023) focus on the 11 largest euro-area economies, and reveal substantial cross-country differences across multiple dimensions, including the use of various loan instruments, loan maturities, the number of bank-firm relationships, and the reliance on the main bank. Additionally, they identify large cross-country differences in average lending rates, which persist even after controlling for variations in firm characteristics and loan instrument types. Similarly, Altavilla, Gürkaynak, and Quaedvlieg (2024) document substantial differences in lending rates across the 10 largest euro-area countries, which they subsequently decompose into country-, bank-, firm-, and loan-level factors. Their analysis shows that country-level fixed effects account for approximately half of the variance in lending rates at the loan level, with the remaining variation being largely explained by bank- and firm-level factors. We extend this literature by providing detailed insights into how lending rates are constructed

across euro-area countries, focusing particularly on loans' sensitivity to changes in policy rates.⁵

Although answering the question of why there are cross-country differences in lending practices falls outside the scope of this paper, the existing literature suggests that the observed variation reflects a complex set of determinants that includes various borrower and lender characteristics, macroeconomic conditions, information asymmetries, and regulatory or institutional factors. For example, Albertazzi, Fringuellotti, and Ongena (2024) find that borrower-side factors play a prominent role in driving the prevalence of fixed-rate mortgage loans. They also show that the share of new fixed-rate loans is significantly higher in countries with lower historical inflation volatility. In the context of corporate lending, Vickery (2008) shows that credit-constrained, bank-dependent firms are more likely to opt for fixed-rate debt to hedge against the risk of rising interest costs. On the supply side, banks that are sensitive to rising interest rates prefer a higher proportion of floating-rate loans. Banks' interest rate preferences also depend on capital requirements and regulatory frameworks, among other factors.⁶

Second, we add to a growing body of literature that lists reasons why monetary policy transmission might vary across countries, banks, firms, and loan types (see Altavilla, Andreeva et al. 2019; Altavilla, Canova, and Ciccarelli 2020; Beyer 2024; Bittner et al. 2022; Fricke, Greppmair, and Paludkiewicz 2024; Holton and Rodriguez d'Acri 2018; Horvath, Kotlebova, and Siranova 2018; Kashyap and Stein 2000; and Kho 2023, among many others). In particular, we add to the literature examining the role of floating-rate loans in amplifying the transmission mechanism of monetary policy. While most of the previous studies have focused on mortgage markets (Corsetti, Duarte, and Mann 2020; Di Maggio et al. 2017; Eichenbaum, Rebelo, and Wong 2022; Flodén et al. 2020; Garriga, Kydland, and Šustek 2017; Pica 2022; Tzamourani 2021), transmission to firms is relatively less explored (with a couple of notable

⁵Importantly, we also include the smaller euro-area countries in our sample, a step that further highlights the observed heterogeneity in lending practices.

⁶In a more general setting, there is an extensive body of literature on the effect of bank competition on the cost of credit; see, for example, Beck, Demirgüç-Kunt, and Maksimovic (2004); Fungáčová, Samshur, and Weill (2017).

exceptions: Core et al. 2024; Gökaynak, Karasoy-Can, and Lee 2022; Ippolito, Ozdagli, and Perez-Orive 2018).

The amplification role of floating-rate loans arises through two distinct mechanisms: first, the pricing of new loans reacts more strongly to changes in monetary policy rates, as the reference rates for floating-rate loans typically have shorter maturities; and second, the pricing of outstanding floating-rate debt adjusts periodically, directly exposing borrowers to changes in monetary policy rates. Our study focuses exclusively on the first transmission mechanism and demonstrates that it is not only the interest rate type but also specific loan characteristics that determine which segment of the risk-free rate yield curve a loan is exposed to. This, in turn, influences how sensitive interest rates on new loans are to changes in monetary policy.

In turn, the prior literature mostly emphasizes the second mechanism: the cash flow channel of monetary policy.⁷ Ippolito, Ozdagli, and Perez-Orive (2018) develop a theoretical framework where unhedged floating-rate debt for financially constrained firms means that changes in monetary policy rates directly affect a firm's interest expenses on existing debt, draining its internal liquid resources and thereby constraining its ability to finance investment. They further demonstrate that the quantitative significance of this mechanism increases with the maturity of the debt, as firms remain exposed to interest rate fluctuations for a longer period. Similarly, Gökaynak, Karasoy-Can, and Lee (2022) explore the cash flow channel by examining whether firm-level stock price responses to monetary policy changes are influenced by differences in liability structures—specifically, the proportion of fixed- versus floating-rate debt. Their findings indicate that firms with a greater share of unhedged floating-rate debt experience more pronounced stock price reactions to monetary policy changes. These firms also face stronger real effects,

⁷One potential reason why the role of fixed- and floating-rate loans in the transmission of monetary policy to new lending has received relatively limited attention is the lack of a panel structure—each newly issued loan represents a unique contract. This constrains the ability to control for loan demand and unobserved firm-time variation. However, the rich data set available in AnaCredit allows us to partially offset these limitations (see Section 3).

such as sharper declines in capital investment, total assets, and net worth.

Finally, most prior research distinguishes between fixed- and floating-rate loans, but ignores the variation within both groups which might lead to different lending rate sensitivity to changes in short-term rates. One exception is Gürkaynak, Karasoy-Can, and Lee (2022), who argue that the differences between fixed- and floating-rate debt in terms of sensitivity to monetary policy changes depend on loan maturity. Fixed-rate debt with short maturities closely resembles floating-rate debt, as both are influenced by prevailing short-term rates. However, for longer maturities, the differences become more pronounced. Floating-rate debt with long maturities effectively resets periodically, aligning with new short-term rates at each adjustment. In contrast, long-term fixed-rate debt locks in its interest rate until maturity, remaining unaffected by fluctuations in short-term rates. Building on this insight, we demonstrate that the sensitivity of loans to changes in short-term interest rates depends not only on loan maturity and rate type but also on the maturity of the underlying reference rate.

The remainder of the paper is organized as follows: Section 2 introduces the data set and highlights key stylized facts about cross-country differences in loan pricing practices across the euro area. Section 3 outlines the econometric framework and presents the study's findings. It analyzes whether differences in lending practices explain the variation in how lending rates were adjusted across euro-area countries during the post-pandemic episode of monetary tightening. It also examines how the pass-through of monetary policy changes to lending rates varies across different maturities of the relevant risk-free rates. Section 4 provides concluding remarks.

2. Data and Stylized Facts

In this section, we describe the data set and outline sample selection. We then provide some stylized facts about cross-country differences in the prevalence of fixed- and floating-rate loans, the duration of rate fixation periods, and the reference rates used in loan pricing across all euro-area jurisdictions. Building on this evidence, we propose a more nuanced measure of loan interest rate sensitivity to

short-term rates which captures the maturity of the relevant risk-free rate.

2.1 AnaCredit and Sample Selection

Our main data source is AnaCredit, a confidential data set maintained by the European System of Central Banks (ESCB) which provides harmonized transaction-level information on lending to firms, covering resident banks from all euro-area countries.⁸ Banks are required to report all loans to firms if their total exposure to a given borrower is equal to or exceeds EUR 25,000. The data are available at a monthly frequency and include a comprehensive set of variables that capture bank-level (ownership, total assets, etc.), firm-level (size, sector, etc.) and loan-level (loan amount, interest rate, maturity, etc.) characteristics.⁹

We impose several restrictions when selecting the sample for our analysis. We include only euro-denominated loans granted to NFCs domiciled in the euro area. Following Kosekova et al. (2023), we limit the sample to borrowers categorized as S.11 (nonfinancial corporations) under the ESA 2010 classification, from which we exclude firms that fall under NACE codes 64–66. These NACE categories cover a spectrum of financial activities, including traditional banking and credit services (NACE 64), insurance and pension funding (NACE 65), and auxiliary financial services (NACE 66) such as financial consulting, brokerage, fund management, and other support functions for the financial and insurance industries. By excluding these sectors, we ensure a targeted focus on NFCs.

AnaCredit covers a broad range of loan types, including financial leases, credit lines, revolving credit, trade receivables, and credit card debt, among others. For this study, we limit our sample to credit

⁸We consider the EA-19 composition of the euro area valid in 2022: Belgium, Germany, Estonia, Ireland, Greece, Spain, France, Italy, Cyprus, Latvia, Lithuania, Luxembourg, Malta, the Netherlands, Austria, Portugal, Slovenia, Slovakia, and Finland. Croatia adopted the euro on January 1, 2023, and AnaCredit data for Croatia are available starting from only 2023.

⁹For more information see the AnaCredit Manual: https://www.ecb.europa.eu/stats/ecb_statistics/anacredit/html/index.en.html.

lines, revolving credit, and other loans, as these categories encompass the majority of lending activity across all euro-area countries. Other loans include all loans that are not included in any of the other more narrowly defined categories. While this is a residual classification, it comprises typical business loans that are entirely disbursed in one installment and repaid over a set period, primarily with regular payments. Credit lines are instruments characterized by the following features that set them apart from other loan types: (i) debtors can withdraw funds up to a preapproved credit limit without prior notice to the creditor; (ii) the credit can be drawn in multiple tranches; and (iii) it is nonrevolving, meaning the available credit limit only decreases as funds are withdrawn, and repayments do not replenish the original credit amount. Revolving credit, excluding overdrafts and credit card debt, refers to any credit instrument—aside from a current account with a credit limit or credit card debt—that allows funds to be repeatedly drawn and repaid up to a specified credit limit. We follow Fricke, Greppmair, and Paludkiewicz (2024) and limit the sample to these types of instruments to ensure a reasonable level of homogeneity across the loans included in our analysis. Moreover, the three above-mentioned types of loans represent the majority of overall new loans issued to NFCs in all euro-area countries (between 65 percent and 99 percent of the new loans' value). As a robustness check, we exclude revolving loans from our analysis. The maturity of these loans is inherently ambiguous, as borrowers can draw on the credit line and repay it as long as it remains open, making it challenging to align with our focus on well-defined loan maturities.

To ensure comparability across all countries in our sample, we exclude loan exposures below EUR 25,000, as some countries report these smaller exposures while others do not. Also, we limit the sample to loans with positive interest rates and exclude any loans not reported in AnaCredit within six months of their inception date. Harmonization of the data across countries comes at the price of a substantial decline in the number of observations for some countries, although coverage remains high in terms of the loan value. Additionally, we exclude syndicated loans and loans with multiple creditors, as AnaCredit only records euro-area credit institutions that are involved in syndicated loans, regardless of the reporting institution's role as lead arranger or agent. More detailed information on

the structure of newly issued loans to NFCs in euro-area countries can be found in the appendix.¹⁰

The sample includes newly issued loans¹¹ from January 2022 to December 2023, capturing the post-pandemic period of monetary tightening. Unlike most previous studies, our sample covers banks from all euro-area countries except for Croatia, where AnaCredit data are only available starting from its euro-area accession in 2023. Table 1 presents the summary statistics for the new loans to NFCs included in our analysis.¹²

Our final sample consists of nearly 7 million newly issued loans from January 2022 to December 2023, encompassing data from 2,382 banks. Among all countries, France has the highest number of observations, contributing 26.2 percent of the total, followed by Germany with 22.8 percent and Spain with 20.7 percent. At the other end of the spectrum are Malta (0.03 percent), Latvia (0.04 percent), and Estonia (0.07 percent). The summary statistics reveal substantial variation across euro-area countries in several dimensions, including average loan size, maturity, and interest rates. These differences partially reflect the varying popularity of different loan types and the diverse sectoral and size composition of borrowers. One consistent feature across nearly all euro-area countries—except Luxembourg

¹⁰See Table A.1 and Figures A.1–A.3 in the appendix.

¹¹Loans are considered newly issued in AnaCredit when all of the following conditions are met. First, the unique combination of creditor, debtor, and instrument identifiers appear in AnaCredit for the first time. Second, the loan has not been reissued or extended as part of a renegotiation. Third, the time between the reporting date and the instrument's inception date is no more than six months. This third condition ensures that loans extended far in the past are not classified as "newly issued" simply because they are reported late due to a borrower only recently exceeding the EUR 25,000 threshold. The findings in the following sections remain robust to changes in the time difference threshold.

¹²Several adjustments were made to address discrepancies in Malta's loan data reporting, particularly in distinguishing between fixed-rate and floating-rate loans. Loans were reclassified by analyzing observed interest rate changes over their lifespan, with single irregularities treated as reporting errors and corrected to align with the dominant behavior. For loans with insufficient data, classifications were inferred based on the lending bank's broader rate-setting practices, including its typical behavior with similar loans. Mixed-rate loans were evaluated for consistency; those showing a clear alignment with a specific rate type were reclassified, while others remained classified as mixed. Overall, any discrepancies in the reported frequency rate of adjustments were corrected to the actual rate adjustment frequency inferred from the data on outstanding loans.

Table 1. Summary Statistics for the Newly Issued Loans to
NFCs in Euro-Area Countries, 2022–23

Country	Number of Loans	Total Value, Bill. of EUR	Number of Banks	Value of the Loan, Thsd. of EUR			Rate, Weighted Mean, %	Maturity, Weighted Mean, Years	Domestic, Share, %	
				Mean	Median	S.D.			Number	Status
AT	114,660	114.6	437	999.7	147.0	4,164.8	3.39	7.52	97.2	88.8
BE	301,076	138.4	44	459.5	95.6	2,559.2	3.66	3.51	98.8	89.8
CY	7,169	4.1	12	577.3	100.8	2,658.1	4.45	7.69	99.3	86.5
DE	1,580,626	511.9	861	323.9	42.0	8,189.1	3.57	3.48	98.8	92.3
EE	5,116	4.9	10	966.7	140.0	3,642.2	5.30	4.00	99.9	97.7
ES	1,436,303	309.8	127	215.7	54.3	5,821.2	3.53	3.38	99.7	98.0
FI	52,863	32.4	156	612.1	80.4	7,019.5	3.30	5.85	99.8	99.6
FR	1,815,906	484.6	175	266.9	50.0	4,164.2	3.07	7.01	99.9	97.5
GR	34,097	33.1	19	972.2	102.7	7,692.3	4.64	7.51	99.9	99.7
IE	50,165	40.5	19	806.5	56.3	5,974.1	3.94	3.54	80.9	47.4
IT	1,203,663	544.2	222	452.1	61.3	5,084.1	3.54	2.27	99.7	98.1
LT	8,892	5.6	19	629.0	80.6	3,814.6	4.95	5.20	99.9	96.4
LU	30,948	57.2	74	1,849.6	199.8	17,047.5	3.06	2.36	21.2	28.1
LV	2,835	2.6	14	900.7	125.0	3,694.8	4.79	4.03	99.1	94.1
MT	1,902	2.3	9	1,232.7	350.0	4,236.7	4.69	8.12	95.0	85.7
NL	53,807	93.1	29	1,729.7	200.0	9,272.3	3.68	7.86	94.7	69.4
PT	192,039	37.7	121	196.3	52.6	1,201.7	3.88	4.57	100.0	99.6
SI	17,209	9.0	15	522.3	100.0	1,905.1	3.46	3.49	99.0	97.9
SK	23,142	13.0	19	563.0	70.0	3,949.8	3.68	3.46	99.9	98.1
Euro Area	6,932,418	2,439.0	2,382	351.8	50.0	5,915.7	3.48	4.35	99.0	92.3

Source: AnaCredit.
Note: New loans to euro-area NFCs (excluding financial and insurance activities) issued in 2022–23, denominated in EUR, exceeding EUR 25,000, with a positive interest rate. The sample consists of the following types of loans: credit lines other than revolving credits, revolving credits, other loans, excluding syndicated loans. The first column represents the country of residence of the bank.

and, to a lesser extent, Ireland, Malta, and the Netherlands—is that the vast majority of loans are extended to domestic firms. This implies that country-specific institutional environments and lending practices could also contribute to differences in the transmission of monetary policy across euro-area countries.

2.2 Interest Rates on New Loans

In this section, we document differences in lending rates across euro-area countries. Figure 1 shows the mean interest rates on newly issued loans to NFCs, along with the one-standard-error band, for the first quarter of 2022 and the fourth quarter of 2023.¹³ The first period captures the final quarter before post-pandemic rate hikes commenced in the euro area,¹⁴ while the latter represents the first quarter following the final rate hike in the cycle. Between these two quarters, key monetary policy rates were raised by a cumulative 450 basis points. Rates were then maintained at peak levels for nine months before being lowered in June 2024.

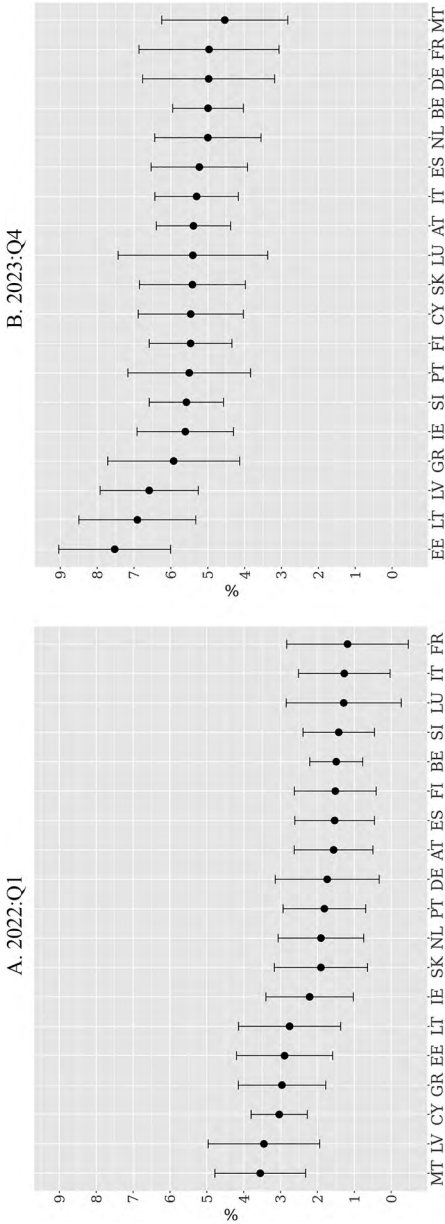
Several noteworthy observations arise from Figure 1. First, there is considerable variation in lending rates across euro-area jurisdictions in both periods. In the first quarter of 2022, the difference between the highest mean interest rate (Malta, 3.55 percent) and the lowest (France, 1.18 percent) amounted to 237 basis points. This disparity had widened by the fourth quarter of 2023, with Estonia displaying the highest mean lending rate (7.52 percent) and Malta the lowest (4.54 percent).

Second, the relative position of countries within the lending rate distribution shifted notably between the two periods. For example, Malta moved from having the highest mean lending rate in the first quarter of 2022 to the lowest in the fourth quarter of 2023. Similar shifts, although less extreme, occurred among larger economies: the

¹³Interest rates shown in Figure 1 are very similar to those obtained from the ECB, MFI Interest Rate Statistics (MIR).

¹⁴Although the initial rate increase occurred in July 2022, financial markets had largely anticipated this move by the second quarter of 2022. The first quarter of 2022 also marks the end of net asset purchases under the ECB's pandemic emergency purchase program (PEPP), which, however, had already been anticipated following the announcement in late 2021.

Figure 1. Interest Rates of the Newly Issued Loans to NFCs in Euro-Area Countries



Source: AnaCredit.
Note: New loans to euro-area NFCs (excluding financial and insurance activities) issued in 2022:Q1 and 2023:Q4, denominated in EUR, exceeding EUR 25,000, with a positive interest rate. The sample consists of the following types of loans: credit lines other than revolving credits, revolving credits, other loans, excluding syndicated loans. The black dot denotes the weighted mean; the error bar represents +/- one weighted standard error. The horizontal axis represents the country of residence of the bank. The countries are arranged according to the weighted average level of interest rates.

Netherlands and Germany, initially around the middle of the distribution, ended up among the countries with the lowest rates by the latter period. Estonia saw the largest increase in mean lending rates, with a rise of more than 450 basis points, followed by Slovenia, Lithuania, Luxembourg, and Italy, each experiencing increases of around 400 basis points. Conversely, Malta saw a modest increase of 99 basis points, while Cyprus experienced a rise of 250 basis points. Greece, Germany, and the Netherlands showed similar increases, each close to 300 basis points. Such pronounced differences in lending rate changes underscore the need to explore why the recent episode of monetary tightening had such varied impacts on lending rates across countries within a single monetary union.

It is possible that cross-country differences in mean lending rates could be partially explained by structural differences. For example, it is possible that for some countries, the composition of the loan portfolio might be tilted more toward loans to large NFCs that tend to have lower interest rates. In order to account for cross-country differences in loan portfolio structure in terms of loan types, firm size classes, and maturities, we calculate the conditional mean interest rates.¹⁵

¹⁵Like in Kosekova et al. (2023), we use the following regression to compute the mean conditional interest rate ($\alpha_{1,c}$) for the euro-area countries:

$$r_{i,t}^{loan} = \sum_{c=1}^{19} \alpha_{1,c} \cdot D_c^{country} + \sum_{k=2}^4 \alpha_{2,k} \cdot D_{k,i}^{size} + \sum_{s=2}^{21} \alpha_{3,s} \cdot D_{s,i}^{sector} + \sum_{l=2}^3 \alpha_{4,l} \cdot D_{l,i}^{type} \\ + \sum_{m=2}^6 \alpha_{5,m} \cdot D_{m,i}^{maturity} + \alpha_6 \cdot D_i^{cross-border} + u_{i,t}.$$

The interest rate of loan i issued in period t was regressed on the set of dummies representing country c ($D_c^{country}$), firm size category k ($D_{k,i}^{size}$), macroeconomic sector s ($D_{s,i}^{sector}$), loan type l ($D_{l,i}^{type}$, credit lines, revolving credit, and other loans), and loan maturity category m ($D_{m,i}^{maturity}$). Dummy variable $D_i^{cross-border}$ equals one if loan i is a cross-border loan (the country of a bank does not coincide with the country of a firm, country c corresponds to the location of the bank). The regression is estimated by weighted least squares (WLS) for the new loans to NFCs issued in 2022:Q1 and 2023:Q4 separately, using loan values as weights. Figure A.4 in the appendix compares conditional and unconditional mean interest rates across countries for both the pre- and post-tightening periods, while Figure A.5 in the appendix shows the change in these rates between the two periods.

Consistent with the findings of Kosekova et al. (2023), our results reveal that adjusting for differences in firm size, sector, and loan type has a minimal effect on cross-country variation, indicating that such structural factors explain only a small portion of the lending rate differences across euro-area countries. Similarly, the changes in mean interest rates between the first quarter of 2022 and the fourth quarter of 2023 remain largely unaffected when comparing conditional and unconditional means.

These findings suggest that other loan characteristics—beyond those typically captured by borrower or loan type—may play a more significant role in explaining cross-country differences in lending rate levels and their responsiveness to monetary policy. One such dimension is the lending rate characteristics—in particular, the prevalence of fixed- versus floating-rate contracts and the maturity of the associated reference rates. These characteristics directly shape the sensitivity of lending rates to changes in monetary policy and can vary widely across countries.

2.3 Fixed- vs. Floating-Rate Loans and Other Rate Characteristics

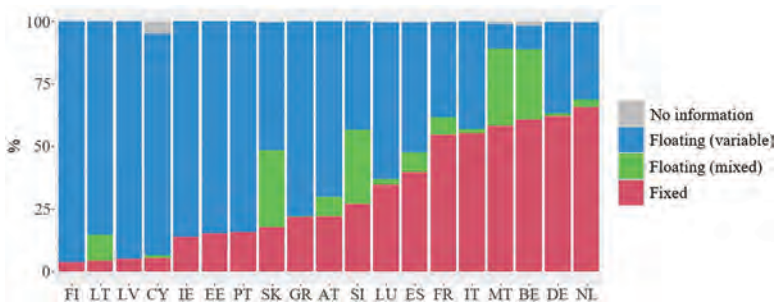
To explore this further, we leverage the unique granularity of AnaCredit data to document the cross-country differences in these lending rate characteristics to provide further context for the observed disparities in lending rate dynamics.

Figure 2 displays the composition of newly issued loans to NFCs, segmented by rate type: fixed, variable, and mixed. We treat variable- and mixed-rate loans as floating-rate loans.^{16,17} A loan is considered to be a fixed-rate loan if it has an interest rate that

¹⁶According to the AnaCredit Manual, instruments that have both a fixed and a variable interest rate over their life are classified as mixed. Therefore, we treat mixed-rate loans as floating-rate loans in our analysis.

¹⁷If results on the structure of newly issued loans for a particular country contain information regarded as confidential according to the AnaCredit rules, the following adjustments are implemented in the published results. Random numbers are added to the share of all new loan categories within a given country, ensuring that the sum of all shares remains equal to 100 percent. These random numbers cannot be smaller than 1.5 percentage point or exceed 4 percentage points in absolute terms. If the share of a particular category is very small, an additional rule ensures that the share is adjusted at least twice, but by no more than

Figure 2. Structure of the Newly Issued Loans to NFCs by Rate-Type Categories in Euro-Area Countries, 2022–23



Source: AnaCredit.

Note: New loans to euro-area NFCs (excluding financial and insurance activities) issued in 2022–23, denominated in EUR, exceeding EUR 25,000, with a positive interest rate. The sample consists of the following types of loans: credit lines other than revolving credits, revolving credits, other loans, excluding syndicated loans. Fixed-rate loans have interest rates that remain constant over the entire duration of the loan. Floating-rate loans have interest rates that adjust over time. Shares are value-weighted. The horizontal axis represents the country of residence of the bank. Countries are arranged according to the share of new loans with fixed interest rates. The results for several countries were subject to additional randomization in order to avoid the disclosure of confidential information. In such cases, the figure does not report precise results, though the randomization procedure preserves the overall trends (see footnote 17).

remains constant over the entire duration of the loan. In turn, a loan is treated as a floating-rate loan if it has an interest rate that adjusts over time, typically linked to a defined reference rate. The prevalence of each rate type varies significantly across countries. For example, in Latvia, Lithuania, Finland, and Cyprus, floating-rate loans predominate, making up over 90 percent of new loans. Fixed-rate loans make up for only a minor share of new lending in these markets. Conversely, fixed-rate loans comprise nearly two-thirds of new loans to NFCs in the Netherlands, Belgium, and Germany. Other euro-area countries display a more balanced distribution between fixed- and floating-rate loans.

50 times in relative terms. This randomization procedure prevents the disclosure of confidential information while preserving overall trends in the results.

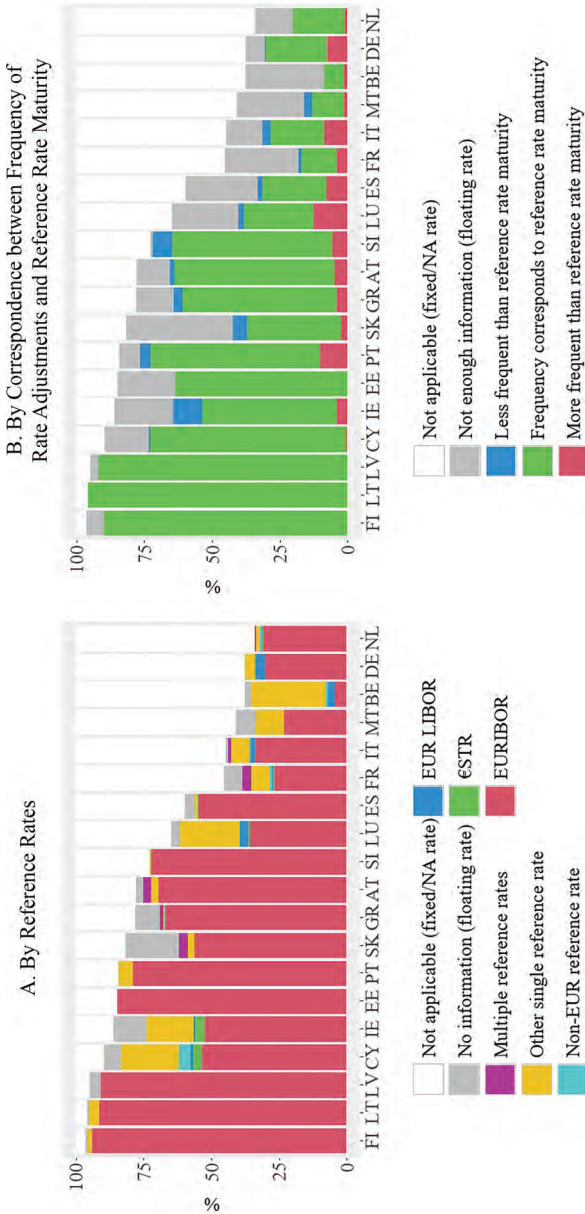
Moreover, within these two broad loan categories, there is substantial variation across several dimensions. For floating-rate loans, the reference rates used to benchmark lending rates differ notably among euro-area countries. While EURIBOR rates are the predominant benchmark in most countries (see Figure 3A), the choice of maturity varies significantly, ranging from overnight rates to 12-month rates (see Figure 4A). For example, Cyprus and Estonia both have a similar proportion of floating-rate loans, yet the specific reference rate maturities differ considerably. In Cyprus, over a third of loans are linked to a reference rate with a maturity of one month or less, and nearly three-quarters are benchmarked to a rate with a maturity of three months or less. In contrast, in Estonia, less than 10 percent of loans are based on reference rates of a month or less, with most floating-rate loans being linked to reference rates with maturities between three and six months.

Importantly, the frequency of interest rate adjustments for floating-rate loans generally aligns with the maturity of their reference rates (see Figure 3B). Consequently, countries with a higher share of floating-rate loans tied to reference rates with shorter maturities are likely to experience greater lending rate fluctuations than those where reference rates have longer maturities, even if the proportion of floating-rate loans is comparable.¹⁸

For fixed-rate loans, the interest rate is set for the entire duration of the loan, making their rate fixation period equal to loan maturity at origination. As shown in Figure 4B, the maturity structure of fixed-rate loans exhibits substantial heterogeneity across euro-area countries. France and Italy illustrate this contrast well: in both countries, fixed-rate loans comprise over half of all loans to NFCs, yet the typical maturity at origination differs markedly. In Italy, most fixed-rate loans have a maturity of 1 year or less, while in France, the majority have maturities exceeding 5 years, with many extending beyond 10 years. Consequently, the shorter maturities for

¹⁸ To address concerns that monetary policy might influence banks' preferences for certain loan types or interest rate structures, as suggested by Gambacorta (2009) and Takaoka and Takahashi (2022), we compare the structure of newly issued loans to NFCs between 2022:Q1 and 2023:Q4. Figures A.6–A.9 in the appendix reveal that the composition of loan types within each country remains highly stable over time, as does the cross-country variation, suggesting minimal impact of monetary policy on these structural preferences.

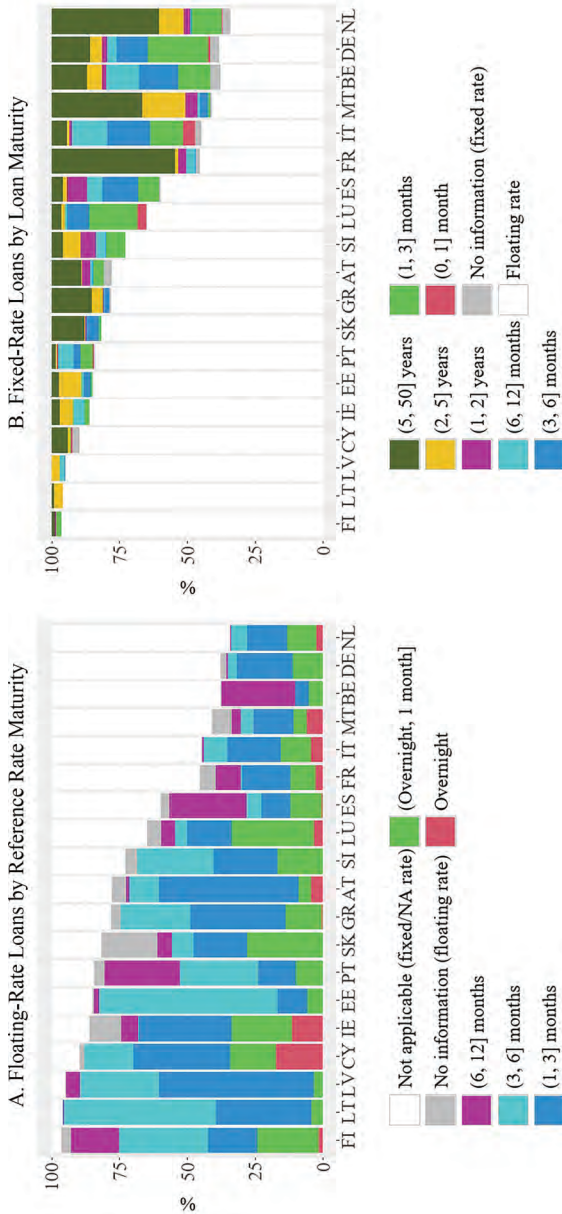
Figure 3. Structure of the Newly Issued Loans to NFCs by Euro-Area Countries, 2022–23



Source: AnaCredit.

Note: New loans to euro-area NFCs (excluding financial and insurance activities) issued in 2022–23, denominated in EUR, exceeding EUR 25,000, with a positive interest rate. The sample consists of the following types of loans: credit lines other than revolving credits, revolving credits, other loans, excluding syndicated loans. Fixed-rate loans have interest rates that remain constant over the entire duration of the loan. Floating-rate loans have interest rates that adjust over time. Shares are value-weighted. The horizontal axis represents the country of residence of the bank. Countries are arranged according to the share of new loans with fixed interest rates. The results for several countries were subject to additional randomization in order to avoid the disclosure of confidential information. In such cases, the figure does not report precise results, though the randomization procedure preserves the overall trends (see footnote 17).

Figure 4. Structure of the Newly Issued Floating-Rate Loans to NFCs by Euro-Area Countries, 2022–23



Source: AnaCredit.

Note: New loans to euro-area NFCs (excluding financial and insurance activities) issued in 2022–23, denominated in EUR, exceeding EUR 25,000, with a positive interest rate. The sample consists of the following types of loans: credit lines other than revolving credits, revolving credits, other loans, excluding syndicated loans. Fixed-rate loans have interest rates that remain constant over the entire duration of the loan. Floating-rate loans have interest rates that adjust over time. Shares are value-weighted. The horizontal axis represents the country of residence of the bank. Countries are arranged according to the share of new loans with fixed interest rates. The results for several countries were subject to additional randomization in order to avoid the disclosure of confidential information. In such cases, the figure does not report precise results, though the randomization procedure preserves the overall trends (see footnote 17).

fixed-rate loans in Italy mean that NFCs face more frequent refinancing, leaving them more vulnerable to fluctuations in lending rates.

In summary, a simple distinction between fixed- and floating-rate loans overlooks significant variation in the reference rates used for pricing loans to NFCs across the euro area. Given that monetary policy impacts risk-free rates differently depending on their maturity (and therefore also the reference rates used for pricing loans), a binary classification of loans may not fully capture the sensitivity of loan interest rates to changes in monetary policy rates. To address this, we develop a loan-level measure of interest rate exposure based on the maturity of the relevant risk-free rate.

2.4 *Obtaining the Relevant Risk-Free Rate and Premium*

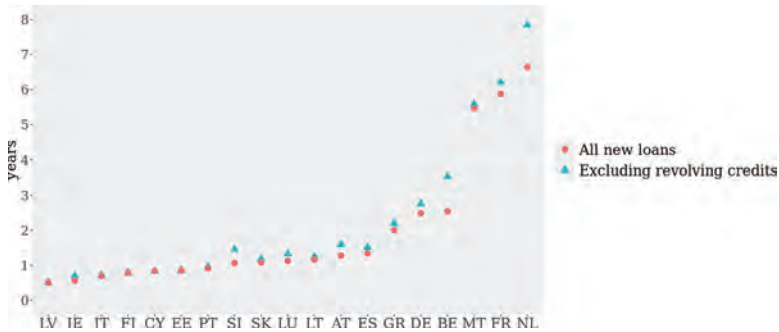
This section details our approach to identifying the relevant risk-free rate for each loan. We start by decomposing the interest rate ($r_{i,t}^{loan}$) on each loan i into two components: the relevant risk-free rate ($r_{i,t}^{risk-free}$) at the time of issuance and the corresponding premium ($r_{i,t}^{premium}$).

$$r_{i,t}^{loan} = r_{i,t}^{risk-free} + r_{i,t}^{premium}$$

For fixed-rate loans, the relevant risk-free rate is the OIS rate matched to the loan's maturity at the issuance date. For floating-rate loans, the relevant risk-free rate is the OIS rate matching the reference rate's maturity at the time of issuance, which, by definition, is shorter than the loan maturity. For example, the relevant risk-free rate for a five-year fixed-rate loan is the five-year OIS rate on the issuance date. In contrast, for a five-year floating-rate loan which has an interest rate benchmarked against a three-month EURIBOR and adjusted every three months, the relevant risk-free rate would be the three-month OIS rate. The premium is then calculated as the difference between the lending rate and the relevant risk-free rate.

The maturity of the relevant risk-free rate indicates which segment of the risk-free rate yield curve is relevant for determining the loan's interest rate. Figure 5 displays the weighted average maturity of the relevant risk-free rates for new loans to NFCs in euro-area countries.

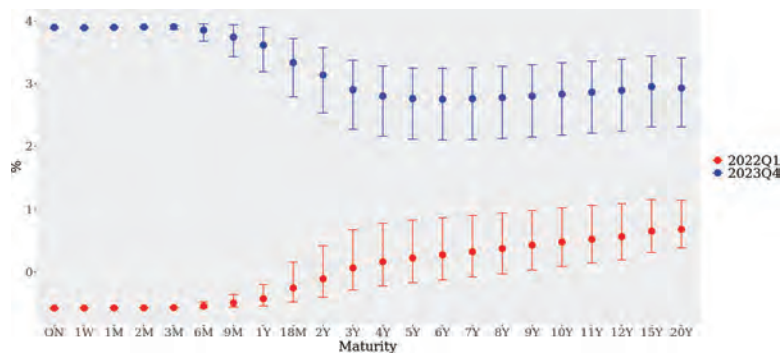
Figure 5. Weighted Average Maturity of the Relevant Risk-Free Rate for the Newly Issued Loans of NFCs in Euro-Area Countries, 2022–23



Source: AnaCredit.
Note: New loans to euro-area NFCs (excluding financial and insurance activities) issued in 2022–23, denominated in EUR, exceeding EUR 25,000, with a positive interest rate. The sample consists of the following types of loans: credit lines other than revolving credits, revolving credits, other loans, excluding syndicated loans. For fixed-rate loans, the maturity of the relevant risk-free rate corresponds to the loan’s maturity, while for floating-rate loans, it is equal to the maturity of the underlying reference rate. The horizontal axis represents the country of residence of the bank. Countries are arranged according to the weighted average maturity of the relevant risk-free rate.

At least two important observations can be made. First, there is significant variation across countries, with relevant risk-free rate maturities ranging from approximately 6 months in Latvia and Ireland to over 6.5 years in the Netherlands. Three countries—Malta, France, and the Netherlands—clearly stand out with the highest maturity of the relevant risk-free rates, which is largely due to the relatively high share of fixed-rate loans with longer maturities. These substantial differences in the maturities of the relevant risk-free rates could be one of the reasons for the cross-country variation in the extent to which lending rates changed during the post-pandemic episode of monetary tightening in the euro area. This is illustrated by comparing the OIS curve from the first quarter of 2022 with that of the fourth quarter of 2023 (see Figure 6). During this period, shorter-term OIS rates, such as the six-month rate—which reflects the average relevant risk-free rates in Latvia and Ireland—rose by approximately 440 basis points. In

Figure 6. OIS (risk-free) Rates in 2022:Q1 and 2023:Q4



Source: ECB Statistical Data Warehouse.
Note: Dots denote the average level of the OIS rate during the respective period, while bars indicate the minimum and maximum value of the rate.

contrast, OIS rates with maturities of five to seven years, which represent the average relevant risk-free rates in Malta, France, and the Netherlands, increased by only about 250 basis points.

Second, the average maturity of the relevant risk-free rates does not show a clear distinction between countries where fixed-rate loans predominate and those where floating-rate loans are more prevalent. For instance, countries with the shortest average maturity of relevant risk-free rates include Latvia, Ireland, and Cyprus, where floating-rate loans with frequent rate resets are common, as well as Italy, where fixed-rate loans predominate but tend to have shorter maturities. This observation aligns well with Gürkaynak, Karasoy-Can, and Lee (2022), who argue that fixed-rate loans with short maturities are of a very similar nature to floating-rate loans. A further illustration is Spain, where the average maturity of the relevant risk-free rates is significantly shorter than in Greece, despite fixed-rate loans being nearly twice as common. Both of the facts mentioned above remain unchanged when revolving loans are excluded from the sample.

In the following analysis, we empirically address two key questions: (i) To what extent can the cross-country variation in the rise of average interest rates on new loans between early 2022 and late 2023 be explained by differences in the evolution of relevant risk-free

rates used to price NFC loans? (ii) How is the pass-through of monetary policy rate changes to interest rates on new loans affected by the maturity of the relevant risk-free rate?

3. Analysis

3.1 *Before-and-After Analysis of Lending Rates: Cross-Country Variation*

We begin the econometric analysis by examining whether differences in lending practices help to explain the variation in how lending rates adjusted across euro-area countries during the post-pandemic episode of monetary tightening. As shown in the previous sections, lending practices differ significantly across countries in terms of the prevalence of fixed- versus floating-rate loans, loan maturities, reference rates and their maturities, and the frequency of interest rate adjustments. These differences expose borrowers to risk-free rates of varying maturities, suggesting that the impact of rising short-term monetary policy rates may not have been felt uniformly across the euro area.

To conduct this analysis, we use loan-level data on all newly issued loans to NFCs from AnaCredit, which offers significant advantages, such as providing detailed information on contract-specific characteristics at the most granular level.¹⁹ We employ a time-difference approach to analyzing changes in interest rate levels (Bredl 2025). In particular, we compare the rates of new loans issued in the

¹⁹However, using this data presents a notable limitation: the absence of a panel structure, as each newly issued loan represents a unique contract. Potentially, one may address this limitation by aggregating loan data to the bank-firm level across different credit instrument types similar to Behn, Forletta, and Reghezza (2024) or Coulier, Pancaro, and Reghezza (2024). This method does not work well for newly issued loans, however. It focuses exclusively on loans repeatedly issued by bank *a* to firm *b*, which substantially reduces the sample size (especially given the short time period covered by the AnaCredit data set), and introduces a bias toward large firms. Furthermore, even within the same bank-firm pair, loans can vary in key characteristics such as maturity and collateral value. One can aggregate new loans further across broader dimensions, such as countries, sectors, regions, or banks. While this method restores the time dimension, it is often less effective for conducting analysis in smaller euro-area countries. Limited observations in these cases often necessitate aggregation at the country level or similarly broad categories, which sacrifices the granularity of the data set.

first quarter of 2022 (2022:Q1) to those issued in the fourth quarter of 2023 (2023:Q4). This time frame captures the period before the first rate hike of the post-pandemic monetary policy tightening in July 2022 and following the final rate hike in September 2023. The time-difference regression is specified as follows:

$$\begin{aligned} \begin{bmatrix} r_{i,t \in 2022Q1}^{loan} \\ r_{i,t \in 2023Q4}^{loan} \end{bmatrix} &= \beta_0^{loan} \cdot D_c^{country} + \beta_{1,c}^{loan} \cdot D_c^{country} \cdot \begin{bmatrix} 0 \\ 1 \end{bmatrix} \\ &+ \sum_j \gamma_j^{loan} \cdot X_{i,t}^j + u_{i,t}. \end{aligned} \quad (1)$$

Variable $r_{i,t}^{loan}$ refers to the interest rate of a new loan i issued in period t . Period t corresponds to the inception date of the loan.²⁰ The regression sample includes loans issued during 2022:Q1 ($t \in 2022Q1$) and those issued during 2023:Q4 ($t \in 2023Q4$). We allow interest rate levels to differ across countries by including a country-specific dummy $D_c^{country}$ (representing the country of residence of the bank). Furthermore, the interest rate levels are regressed on a dummy variable that equals zero for loans issued in the first period and one for loans issued in the second period. To capture cross-country variation in the extent to which lending rates changed, this dummy variable is interacted with $D_c^{country}$ (note that it is not necessary to include the period dummy separately, as no country is omitted in $D_c^{country}$). The coefficient of interest $\beta_{1,c}^{loan}$ represents the

²⁰We implicitly assume that the creditor and debtor agree on the loan rate at the inception date. However, actual practices may deviate from this assumption. First, both parties can set the rate prior to the inception date. Unfortunately, AnaCredit does not provide information to identify such cases, particularly for fixed-rate loans. Second, the rate can be set after the inception date. AnaCredit contains the settlement date for most instruments. On average, loans are settled two weeks after their inception, although the average lag varies across countries, ranging from 0 to 38 days. Even under the extreme assumption that interest rates are set at the settlement date for all newly issued loans, our findings remain qualitatively unchanged. Finally, bias may arise due to the gap between the inception and reference (reporting) dates. We expect this bias to be negligible, as our sample is restricted to loans reported in AnaCredit within six months of their inception. Moreover, approximately 75 percent of newly issued loans first appear in AnaCredit within the same month or the following month of their inception. Using the reference date instead of the inception date to redefine the loan of the newly issued rate, the risk-free rate, and premium at the inception date does not qualitatively alter our findings. Supporting results are available upon request.

difference in interest rates for new loans issued before and after the monetary tightening in each country.

Changes in monetary policy rates were not the only factor influencing lending rates during the sample period. To address this, we leverage the granularity of our data set and include an extensive set of loan-level control variables $X_{i,t}^j$. Specifically, it includes a rich set of fixed effects to account for the characteristics of creditors (bank-level fixed effects), debtors (macroeconomic sector, region, and firm size fixed effects), and loan instruments (fixed effects for loan type, maturity, collateral size, reference rate type, reference rate maturity, rate adjustment frequency, multidebtor loans, cross-border loans). The large set of fixed effects is necessary to control for differences in lending rate levels across countries, macroeconomic sectors, regions, and firm sizes. Furthermore, systematic differences in lending rates are likely to exist between fixed-rate and floating-rate loans, between loans linked to the six-month EURIBOR and those linked to the three-month EURIBOR, and among other contract variations. Most of the fixed effects are interacted with country fixed effects²¹ to capture country specificity of sectoral, firm size, or contract factors. However, there is no need to include time fixed effects in this specification, as the analysis considers only two time periods. By including all the above-mentioned fixed effects simultaneously, we ensure that we compare similar loans in 2022:Q1 and 2023:Q4 in terms of creditor, debtor, and contract features. Additionally, to account for changes in credit risk, we include the loan-level information on the probability of default.²² Equation (1) is estimated by WLS, where weights are determined by the loan values.

²¹We use the bank country of residence. The only exception is the interaction with the sector of activity, where the firm country of residence is used to capture the demand effect.

²²AnaCredit provides information on the probability of default (PD) for individual loans at the time of issuance. However, coverage of this variable is incomplete (or even absent for some countries), as PD data is not required from all entities (see the AnaCredit Manual). To address this limitation, we impute missing PD values using a model that predicts PD based on the ratio of the accumulated impairment amount to the loan value, while controlling for loan maturity, firm size, macroeconomic sector, and collateral size fixed effects. Details on the country-level variation in PD coverage after imputation and the quality of the imputation (measured as the correlation between actual and predicted PD values) are presented in Table A.2 in the appendix.

The results obtained from Equation (1) should not be interpreted as causal or used to directly quantify the pass-through of monetary policy rates to lending rates in the euro-area countries. This is because the policy rate changes between 2022:Q1 and 2023:Q4 were neither exogenous nor entirely unexpected. Economic agents had anticipated the tightening to some extent even before 2022. Moreover, the onset of the war in Ukraine in February 2022, along with the associated rise in uncertainty and energy price shocks, influenced expectations. Nevertheless, the before-and-after comparison offers valuable insights into cross-country differences in lending rate responses over the longer term, as the expectations regarding monetary policy changes were likely shared by agents across all countries within the monetary union.

Equation (1) can also be applied to the components of the interest rate: the relevant risk-free rate ($r_{i,t}^{risk-free}$) and the premium ($r_{i,t}^{premium}$). Since the sample and the set of right-hand-side variables remain unchanged, the estimated conditional change in new loan rates can be decomposed into two parts: the change driven by adjustments in the relevant risk-free rates (Equation (2)) and the change attributable to shifts in the premium (Equation (3)):

$$\beta_{1,c}^{loan} = \beta_{1,c}^{risk-free} + \beta_{1,c}^{premium}.$$

$$\begin{aligned} \begin{bmatrix} r_{i,t \in 2022Q1}^{risk-free} \\ r_{i,t \in 2023Q4}^{risk-free} \end{bmatrix} &= \beta_0^{risk-free} \cdot D_c^{country} + \beta_{1,c}^{risk-free} \cdot D_c^{country} \cdot \begin{bmatrix} 0 \\ 1 \end{bmatrix} \\ &+ \sum_j \gamma_j^{risk-free} \cdot X_{i,t}^j + u_{i,t}^{risk-free} \end{aligned} \quad (2)$$

$$\begin{aligned} \begin{bmatrix} r_{i,t \in 2022Q1}^{premium} \\ r_{i,t \in 2023Q4}^{premium} \end{bmatrix} &= \beta_0^{premium} \cdot D_c^{country} + \beta_{1,c}^{premium} \cdot D_c^{country} \cdot \begin{bmatrix} 0 \\ 1 \end{bmatrix} \\ &+ \sum_j \gamma_j^{premium} \cdot X_{i,t}^j + u_{i,t}^{premium} \end{aligned} \quad (3)$$

The coefficient $\beta_{1,c}^{risk-free}$ captures the extent to which lending rates in country c changed due to shifts in the underlying risk-free rates that are relevant for loan pricing. Cross-country variation in this coefficient arises solely from differences in the maturities of the relevant risk-free rates. It is likely to be larger in countries where

short-term risk-free rates predominate, driven by a higher prevalence of floating-rate loans with shorter reference rate maturities or a greater share of fixed-rate loans with shorter maturities. The coefficient $\beta_{1,c}^{premium}$ captures changes in lending rates driven by factors other than adjustments in risk-free rates.

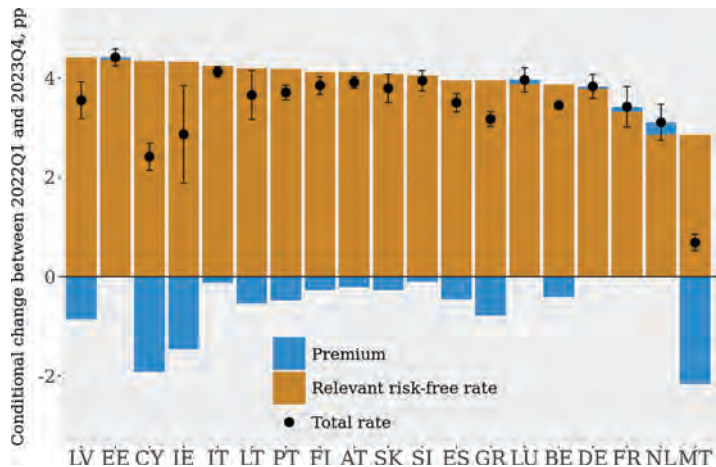
Figure 7 displays the estimated coefficients, $\beta_{1,c}^{loan}$ (shown as dots), derived from Equation (1) for each euro-area country. It also provides a decomposition of the total changes in lending rates into two key components: adjustments in the relevant risk-free rates and shifts in the premium, as outlined in Equations (2) and (3).

Consistent with the previous literature (e.g., Beyer 2024; Sørensen and Werner 2006), the estimated coefficients exhibit notable variation across euro-area countries. In 10 out of 19 countries, the coefficients fall within a relatively narrow range of 3.5 to 4.0 percentage points (pp) in conditional change between 2022:Q1 and 2023:Q4. However, some countries deviate significantly from this range. The lowest coefficients are found in Malta (0.69 pp), Cyprus (2.42 pp), Ireland (2.87 pp), and the Netherlands (3.11 pp), indicating relatively subdued adjustments in lending rates. In contrast, the highest conditional changes are observed in Estonia (4.42 pp) and Italy (4.13 pp), where lending rates responded more sharply. Across all euro-area countries, the increase in relevant risk-free rates was the primary driver of rising lending rates.²³ In fact, in many cases, lending rate changes closely mirrored the adjustments in underlying risk-free rates.

Additionally, three key observations emerge from the analysis. First, the contribution of risk-free rates to lending rate changes exhibits notable cross-country variation. Eleven countries experienced an increase in relevant risk-free rates exceeding 4 pp, with Latvia and Estonia experiencing the highest increases at 4.37–4.41 pp. In contrast, the Netherlands and Malta saw risk-free rates rise by only 2.85 pp, while France recorded an increase of 3.33 pp. These differences stem only from the varying maturities of the risk-free

²³Accounting for the heterogeneity in relevant risk-free rate changes across countries helps to explain the cross-country heterogeneity in conditional changes in the rates for new loans. The weighted sum of squares mean deviation in overall lending rate changes across countries equals 0.11, while a similar metric for the premium is 0.07.

Figure 7. Decomposition of Conditional Changes in the Rates of the Newly Issued Loans to NFCs between 2022:Q1 and 2023:Q4



Source: AnaCredit.

Note: Point depicts the estimated country-specific coefficient $\beta_{1,c}^{loan}$ from Equation (1) that shows the difference in interest rates of new loans issued before and after the monetary tightening and the 95 percent confidence bands. The brown bar is the contribution of changes in the risk-free rate $\beta_{1,c}^{risk-free}$, and the blue bar is the contribution of changes in the premium $\beta_{1,c}^{premium}$. The sample includes new loans to euro-area NFCs (excluding financial and insurance activities) issued in 2022:Q1 and 2023:Q4, denominated in EUR, exceeding EUR 25,000, with a positive interest rate. The sample consists of the following types of loans: credit lines other than revolving credits, revolving credits, other loans, excluding syndicated loans. For fixed-rate loans, the maturity of the relevant risk-free rate corresponds to the loan’s maturity, while for floating-rate loans, it is equal to the maturity of the underlying reference rate. The horizontal axis represents the country of residence of the bank. Countries are arranged according to the contribution of the relevant risk-free rate.

rates relevant for each country, as all euro-area members share the same underlying risk-free rates (OIS rates).

Second, the distinction between fixed- and floating-rate loans does not always provide a clear explanation for the observed patterns. For instance, the contribution of relevant risk-free rates was particularly pronounced in countries like Latvia and Ireland, where floating-rate loans with short fixation periods are more prevalent.

Similarly, a strong contribution of risk-free rates was observed in Italy, despite its higher reliance on fixed-rate loans, as these loans tend to have shorter maturities.

Third, a large increase in the relevant risk-free rates does not necessarily result in the largest increases in lending rates. In several countries, the rise in relevant risk-free rates was offset by a decline in the premium, which moderated the overall increase in lending rates. Apart from Malta, this compensatory effect was particularly evident in countries like Cyprus, Ireland, and Latvia, where the relevant risk-free rates have very short maturities. This suggests that changes in the premium can play a significant role in shaping the total lending rate adjustments.

3.2 *Variation in Lending Rate Changes during Post-Pandemic Tightening: The Role of Loan Characteristics*

Equations (1)–(3) provide valuable insights into the factors underlying cross-country variation in how lending rates adjusted during the post-pandemic episode of monetary policy tightening. However, these equations do not address whether lending rates responded differently across various types of loans. To investigate whether changes in lending rates were influenced by the maturity of the relevant risk-free rate, we extend the framework of Equations (1)–(3), replacing the country-specific dummies with various loan-level indicators describing contract features primarily focusing on the maturity of the relevant risk-free rate (see Equation (4)).

$$\begin{aligned} \begin{bmatrix} r_{i,t \in 2022Q1} \\ r_{i,t \in 2023Q4} \end{bmatrix} &= \beta_0 + \left(\delta_0 + \sum_k \delta_{1,k} \cdot Z_{i,t}^k + \sum_m \delta_{2,m} \cdot W_{i,t}^m \right) \cdot \begin{bmatrix} 0 \\ 1 \end{bmatrix} \\ &+ \sum_j \gamma_j \cdot X_{i,t}^j + u_{i,t} \end{aligned} \quad (4)$$

$Z_{i,t}^k$ includes a set of dummies representing the maturity buckets of the relevant risk-free rate associated with loan i issued in period t . These buckets are as follows: relevant risk-free rate maturity up to 1 month; 1 to 3 months; 3 to 6 months; 6 to 12 months; 1 to 2

years; 2 to 5 years.²⁴ The loans with the relevant risk-free rate above five years serve as a benchmark (omitted category). This approach allows us to assess whether lending rate adjustments differ based on the maturity of the relevant risk-free rate. The set of dummies $W_{i,t}^m$ represents other contract features of the loan that are potentially important in explaining the lending rate changes between 2022:Q1 and 2023:Q4; in particular, we include a dummy variable that distinguishes between fixed- and floating-rate loans. In addition, $W_{i,t}^m$ includes two dummies indicating loans in which the frequency of interest rate adjustments differs from the maturity of the reference rate (higher and lower frequency, correspondingly). We also allow for different lending rate changes in cases where the reference rate is not EUR-related, where there are multiple reference rates, or cases where the reference rate is any other single rate (EURIBOR; ESTR, or euro short-term rate; and EUR LIBOR, or London interbank offered rate, serve as benchmarks). Finally, $W_{i,t}^m$ accounts for the potential difference in rate changes for the cross-border loans. As before, $X_{i,t}^j$ stands for the large set of variables controlling for the level of lending rates. Note that all contract features captured by $Z_{i,t}^k$ and $W_{i,t}^m$ are also included into $X_{i,t}^j$.²⁵ Similarly to Equations (1)–(3), we perform the analysis for the total lending rate and the two subcomponents: the relevant risk-free rate and the premium, thus $r_{i,t}$ represents $r_{i,t}^{loan}$, $r_{i,t}^{risk-free}$, or $r_{i,t}^{premium}$.

We therefore proceed to investigate whether lending rate adjustments and factors driving them differ based on the maturity of the relevant risk-free rate. Table 2 presents the estimated coefficients from the regression analysis specified in Equation (4).²⁶

²⁴The upper bound is included in each bucket, while the lower bound is only included in the first bucket.

²⁵Although $X_{i,t}^j$ includes the relevant risk-free rate maturity $\times$ country fixed effects, we still can estimate the coefficient $\delta_{1,k}$ because of the lending rate variation between 2022:Q1 and 2023:Q4 for the same country and relevant risk-free rate maturity.

²⁶Table A.3 in the appendix provides the information about the share of outliers. For each euro-area country we exclude very large newly issued loans: top 0.1 percent, or 10 top loans, whichever measure has the highest number of observations. Also, we exclude new loans with interest rates exceeding the top 1 percentile of the distribution in the respective country. Table A.4 in the appendix reports the share of observations with missing information. The value share of observations with missing information is substantial, averaging around

Table 2. Regression Results for Conditional Change in Total Rate, Relevant Risk-Free Rate, and Premium between 2022:Q1 and 2023:Q4

Variables	Total Rate	Relevant Risk-Free Rate	Premium
	(1)	(2)	(3)
Dummy for 2023:Q4 (D_{2023Q4})	3.11*** (0.05)	2.45*** (0.02)	0.66*** (0.04)
$D_{2023Q4} \times (0, 1]$ Month Risk-Free Rate	1.01*** (0.16)	1.99*** (0.03)	-0.98*** (0.15)
$D_{2023Q4} \times (1, 3]$ Month Risk-Free Rate	0.92*** (0.13)	1.97*** (0.03)	-1.05*** (0.12)
$D_{2023Q4} \times (3, 6]$ Month Risk-Free Rate	0.80*** (0.12)	1.89*** (0.03)	-1.09*** (0.11)
$D_{2023Q4} \times (6, 12]$ Month Risk-Free Rate	0.73*** (0.09)	1.63*** (0.04)	-0.90*** (0.09)
$D_{2023Q4} \times (1, 2]$ Year Risk-Free Rate	0.36*** (0.08)	1.20*** (0.03)	-0.85*** (0.08)
$D_{2023Q4} \times (2, 5]$ Year Risk-Free Rate	0.17** (0.06)	0.35*** (0.02)	-0.18** (0.06)
$D_{2023Q4} \times \text{Floating Rate}$	0.00 (0.14)	0.07*** (0.02)	-0.07 (0.14)
$D_{2023Q4} \times \text{Adjustment More Frequent Than Reference Rate Maturity}$	0.10 (0.19)	0.00 (0.02)	0.09 (0.19)
$D_{2023Q4} \times \text{Adjustment Less Frequent Than Reference Rate Maturity}$	-0.07 (0.24)	-0.01 (0.03)	-0.06 (0.23)
$D_{2023Q4} \times \text{Non-EUR Reference Rate}$	0.26* (0.12)	-0.04 (0.02)	0.30* (0.12)

(continued)

Table 2. (Continued)

Variables	Total Rate	Relevant Risk-Free Rate	Premium
	(1)	(2)	(3)
$D_{2023Q4} \times \text{Other Single Reference Rate}$	-0.13 (0.17)	-0.02 (0.06)	-0.11 (0.16)
$D_{2023Q4} \times \text{Multiple Reference Rates}$	-0.28 (0.28)	-0.16* (0.06)	-0.12 (0.32)
$D_{2023Q4} \times \text{Cross-Border Loans}$	-0.18 (0.14)	0.01 (0.02)	-0.18 (0.15)
Probability of Default (Imputed if Missing)	9.50*** (1.12)	0.00 (0.07)	9.50*** (1.12)
Fixed Effects: Creditor Country, Bank, Loan Type $\times$ Creditor Country, Debtor Sector $\times$ Debtor Country, Debtor Region, Firm Size Class $\times$ Creditor Country, Loan Maturity $\times$ Creditor Country, Collateral Size $\times$ Creditor Country, Cross-Border Loan $\times$ Creditor Country, Multi-debtor Loan $\times$ Creditor Country, Reference Rate Type $\times$ Creditor Country, Risk-Free Rate Maturity $\times$ Creditor Country, Reference Rate Adjustment Frequency $\times$ Creditor Country			
Number of Observations	1,133,053	1,133,053	1,133,053
R^2	0.85	0.99	0.46
Within R^2	0.78	0.98	0.09
Standard Errors Clustered by	Bank	Bank	Bank
Source: AnaCredit. Note: *** denotes significance at the 99.9 percent level, ** at 99 percent, and * at 95 percent. Standard errors are reported in parentheses. The sample includes new loans to euro-area NFCs (excluding financial and insurance activities) issued in 2022:Q1 and 2023:Q4, denominated in EUR, exceeding EUR 25,000, with a positive interest rate. The sample consists of the following types of loans: credit lines other than revolving credits, revolving credits, other loans, excluding syndicated loans. For fixed-rate loans, the maturity of the relevant risk-free rate corresponds to the loan's maturity, while for floating-rate loans, it is equal to the maturity of the underlying reference rate. Results are obtained by WLS, where weights are determined by the loan values. D_{2023Q4} denotes a binary variable that equals one for 2023:Q4 and zero for 2022:Q1. Other variables (except the probability of default) are the binary (dummy) variables that equal one if a loan contract has a corresponding feature.			

The results indicate that interest rates increased between 2022:Q1 and 2023:Q4 for all loans, primarily driven by a notable rise in the relevant risk-free rates (column 2) and a smaller contribution from a change in the premium (column 3). However, the extent of the increase varies based on the maturity of the relevant risk-free rates, with more pronounced increases observed for loans tied to shorter maturities. For example, loans with relevant risk-free rate maturities of between two and five years experienced a 0.17 percentage point higher increase in interest rates compared with loans with relevant risk-free rate maturities exceeding five years. The estimated coefficients steadily rise as maturities shorten, with the highest coefficient (1.01) being observed for loans with relevant risk-free rate maturities of one month or less.

Interestingly, the coefficient for floating-rate loans is statistically insignificant. This indicates that the sensitivity of lending rates is not simply driven by whether a loan has a fixed or floating rate. Instead, it is influenced by a more nuanced set of factors that characterize the maturity of the relevant risk-free rate. Most other factors characterizing lending rates do not show a statistically significant effect on changes in lending rates. One exception is loans referenced to non-EUR reference rates, which experienced a more pronounced increase in lending rates.

It is important to note that the differences in lending rate changes are smaller than expected given the mechanical differences in risk-free rates. This is because loans linked to risk-free rates with shorter maturities saw premia decline more substantially. For instance, loans with risk-free rate maturities of one month or less saw a 0.98 percentage point greater reduction in premia compared with those with maturities exceeding five years. Consequently, although the relevant risk-free rates for these loans increased by 1.99 percentage points more, the differences in overall lending rate adjustments were less pronounced.

These results suggest that banks adjusted premia in a way that partially offset the increase in lending rates for loans most exposed to rising short-term risk-free rates. This behavior helped to smooth

35 percent. The highest shares of observations with missing data were recorded for Ireland, France, and Malta, which should be considered when interpreting the results.

cross-loan differences in lending rate changes that would have been larger if lending rates had moved purely in line with the relevant risk-free rates. To ensure that this finding is not driven solely by our choice of the “after” period and a particular configuration of the risk-free yield curve, we reestimate Equation (4) iteratively, varying the post-tightening period from 2023:Q4 to each quarter between 2022:Q2 and 2023:Q3. Our results indicate that the findings are robust to the choice of the “after” period. The smoothing role of the premium is evident throughout the sample period, as differences in lending rate adjustments consistently remain more muted than what would be expected based solely on changes in the underlying risk-free rates.²⁷

There are at least two potential reasons why the adjustment in the premium may depend on the maturity of the relevant risk-free rate. First, we argue that such adjustments may be driven by bank-specific factors that vary over time. For example, banks issuing new loans tied to shorter-maturity risk-free rates (assuming consistent pricing with outstanding loans) were likely to experience a more substantial increase in net interest income, as their loan portfolios reprice more quickly in response to monetary policy changes than their funding costs, particularly deposits (see, for example, Altunok, Arslan, and Ongena 2023; Beyer 2024). As a result, deposit spreads may have been large enough to sustain or increase banks’ profitability without the need for a high loan spread. This allowed banks

²⁷See Table A.5 in the appendix. Importantly, the offsetting mechanism goes beyond what would simply emerge as a result of a constant incomplete pass-through. If the pass-through of the relevant risk-free rate was incomplete and constant over loan categories and time, offsetting would follow “mechanically.” Imagine the pass-through was 0.8 for all loans. If the relevant risk-free rate increased by 4 percentage points, the lending rate would increase by 3.2 percentage points and the premium would decline by 0.8 percentage points. If the relevant risk-free rate for another loan category increased by 2 percentage points, lending rates in this category would increase by 1.6 percentage points and the premium would decline by 0.4 percentage points only. Hence, the premium for the category with a stronger increase in the relevant risk-free rate would “mechanically” decline more strongly. Our results suggest that the pass-through is time-varying and fosters the offsetting pattern. The pass-through for loans with relevant risk-free rate maturity exceeding five years was quite low in the second quarter 2022 ($0.57/1.16=0.49$) when relevant risk-free rates in this category had risen more than in other categories but quite strong in the fourth quarter 2023 ($3.11/2.45=1.27$) when relevant risk-free rates in this category had risen less than in other categories.

more exposed to short-term rates to benefit from stronger gains in net interest income compared with those relying more on longer-maturity benchmarks. The resulting additional income could have allowed them to reduce the premium—to gain market share, prevent a credit crunch, or support borrower resilience. To test whether the observed smoothing of lending rate changes is driven by time-varying bank heterogeneity, we reestimate Equation (4), this time including bank-period fixed effects. These fixed effects control for any variation in how individual banks were affected across the “before” and “after” period (see Table 3). Our results suggest that the smoothing effect of the premium does not disappear when bank-period fixed effects are included.

Alternatively, a premium may smooth out monetary-policy-induced increases in the risk-free rates due to “composition effects” from the supply side and the demand side. On the supply side, the literature suggests that financial institutions may change their risk-taking depending on the monetary policy conditions (e.g., Chodorow-Reich 2014). According to this view, increases in policy rates raise the hurdle rate for new investment projects, making it more costly or less feasible for projects with lower expected returns or higher variance (riskier projects) to secure external financing. Likewise, a higher interest rate environment may reduce banks’ incentives to reach for yield or may increase their risk aversion. In our context, such behavior may lead banks to shift their lending portfolios toward relatively less risky borrowers than prior to the tightening cycle. If borrowers of loans with shorter-maturity risk-free rates are more sensitive to such reallocation—consistent with their higher rollover risks or credit spreads (e.g., Chen, Xu, and Yang 2021; Diamond 1991)—these loans may have experienced a relatively sharper decline in premium. Although we control for these possible compositional effects through the inclusion of loan-specific probabilities of default, we cannot rule out that our results may to some extent be driven by these supply-side shifts. From another perspective, as borrowers on short-term loans have flexibility to manage their leverage (e.g., Dangl and Zechner 2021), reaching, for example, for higher-maturity or fixed-rate loans amid rising reference rates, banks may narrow the spreads on new shorter-maturity loans relatively more to retain their investment returns or market shares. This should be more prevalent in less concentrated banking systems

Table 3. Regression Results for Conditional Change in Total Rate, Relevant Risk-Free Rate, and Premium between 2022:Q1 and 2023:Q4, Including Bank-Quarter Fixed Effects

Variables	Total Rate	Relevant Risk-Free Rate	Premium
	(1)	(2)	(3)
Dummy for 2023:Q4 (D_{2023Q4})	—	—	—
$D_{2023Q4} \times (0, 1]$ Month Risk-Free Rate	0.57*** (0.13)	1.81*** (0.05)	-1.24*** (0.13)
$D_{2023Q4} \times (1, 3]$ Month Risk-Free Rate	0.78*** (0.10)	1.84*** (0.04)	-1.06*** (0.09)
$D_{2023Q4} \times (3, 6]$ Month Risk-Free Rate	0.73*** (0.10)	1.76*** (0.04)	-1.03*** (0.09)
$D_{2023Q4} \times (6, 12]$ Month Risk-Free Rate	0.73*** (0.10)	1.52*** (0.05)	-0.79*** (0.10)
$D_{2023Q4} \times (1, 2]$ Year Risk-Free Rate	0.35*** (0.08)	1.13*** (0.03)	-0.78*** (0.08)
$D_{2023Q4} \times (2, 5]$ Year Risk-Free Rate	0.21*** (0.06)	0.31*** (0.02)	-0.10 (0.06)
$D_{2023Q4} \times \text{Floating Rate}$	0.42*** (0.09)	0.16*** (0.03)	0.27** (0.10)
$D_{2023Q4} \times \text{Adjustment More Frequent Than Reference Rate Maturity}$	-0.04 (0.09)	0.00 (0.02)	-0.03 (0.10)
$D_{2023Q4} \times \text{Adjustment Less Frequent Than Reference Rate Maturity}$	-0.04 (0.18)	0.01 (0.06)	-0.04 (0.14)
$D_{2023Q4} \times \text{Non-EUR Reference Rate}$	-0.20*** (0.07)	-0.05 (0.03)	-0.15 (0.08)

(continued)

Table 3. (Continued)

Variables	Total Rate	Relevant Risk-Free Rate	Premium
	(1)	(2)	(3)
$D_{2023Q4} \times \text{Other Single Reference Rate}$	-0.14 (0.13)	-0.13 (0.08)	-0.01 (0.17)
$D_{2023Q4} \times \text{Multiple Reference Rates}$	0.39 (0.83)	-0.16 (0.10)	0.55 (0.92)
$D_{2023Q4} \times \text{Cross-Border Loans}$	-0.22 (0.16)	0.00 (0.02)	-0.22 (0.17)
Probability of Default (Imputed if Missing)	9.45*** (1.09)	-0.02 (0.06)	9.47*** (1.10)
Fixed Effects: Creditor Country $\times$ Quarter, Bank $\times$ Quarter, Loan Type $\times$ Creditor Country, Debtor Sector $\times$ Debtor Country, Debtor Region, Firm Size Class $\times$ Creditor Country, Loan Maturity $\times$ Creditor Country, Collateral Size $\times$ Creditor Country, Cross-Border Loan $\times$ Creditor Country, Multidebtor Loan $\times$ Creditor Country, Reference Rate Type $\times$ Creditor Country, Risk-Free Rate Maturity $\times$ Creditor Country, Reference Rate Adjustment Frequency $\times$ Creditor Country			
Number of Observations	1,133,053	1,133,053	1,133,053
R^2	0.87	0.99	0.52
Within R^2	0.07	0.64	0.07
Standard Errors Clustered by	Bank	Bank	Bank
Source: AnaCredit. Note: This table replicates results from Table 2 but with fixed effects at the bank-quarter level. As a consequence, the coefficient for D_{2023Q4} is no longer identified. *** denotes significance at the 99.9 percent level, ** at 99 percent, and * at 95 percent. Standard errors are reported in parentheses. The sample includes new loans to euro-area NFCs (excluding financial and insurance activities) issued in 2022:Q1 and 2023:Q4, denominated in EUR, exceeding EUR 25,000, with a positive interest rate. The sample consists of the following types of loans: credit lines other than revolving credits, revolving credits, other loans, excluding syndicated loans. For fixed-rate loans, the maturity of the relevant risk-free rate corresponds to the loan's maturity, while for floating-rate loans, it is equal to the maturity of the underlying reference rate. Results are obtained by WLS, where weights are determined by the loan values. D_{2023Q4} denotes a binary variable that equals one for 2023:Q4 and zero for 2022:Q1. Other variables (except the probability of default) are the binary (dummy) variables that equal one if a loan contract has a corresponding feature.			

in which deposit spreads are likely to widen relatively less after rate hikes, in the spirit of Drechsler, Savov, and Schnabl (2017). These insights on the smoothing role of loan premia amid policy tightening point to an interesting interplay between the bank lending channel and the bank risk-taking channel and the market composition which has been largely unexplored.

Finally, on the demand side, one might argue that there might have been an “opportunistic” shift in borrower demand toward loan categories for which the increase in relevant risk-free rates was relatively lower. This demand shift then might have induced an increase in the premium in these categories relative to the categories that experienced a sharper increase in relevant risk-free rates. However, our data are not well suited to directly test these hypotheses, and we leave a more detailed investigation of these mechanisms to future research.

3.3 Pass-Through of Monetary Policy Rates to Lending Rates

We next turn to the following question: Does the pass-through of monetary policy rates to lending rates depend on the maturity of the relevant risk-free rates? Since this directly addresses the transmission of monetary policy, our empirical strategy must be adjusted in two key ways. First, we turn our attention to loans issued in close proximity to ECB GovC meetings and use stacked time-difference regression instead of a simple before-and-after comparison (see Bredl 2025).

In total, there were 15 ECB GovC meetings in 2022–23.²⁸ For each GovC meeting on date τ , we compare new loans issued within a six-week period before the meeting ($t \in [\tau - 6W, \tau)$) with those issued within a six-week period after the meeting ($t \in (\tau, \tau + 6W]$). Loans issued on the meeting date are excluded from both groups. The six-week windows were chosen because ECB GovC meetings typically occur at six-week intervals. This time frame ensures that, in most cases, only a single monetary policy event affects the loans in each

²⁸Eight meetings in 2022: February 3, March 10, April 14, June 9, July 21, September 8, October 27, December 15. Seven meetings in 2023: February 2, March 16, May 4, June 15, July 27, September 14, October 26.

group.²⁹ We then combine the 15 sets of pre- and post-meeting loan data into a single data set and apply an approach similar to Equation (4). Now instead of the binary variable $[0, 1]'$ that distinguishes the loans issued before and after post-pandemic monetary tightening, the regression includes the variable $[0, \Delta DF_\tau]'$ that compares the loans issued six weeks before ($t \in [\tau - 6W, \tau)$) and six weeks after ($t \in (\tau, \tau + 6W]$) the respective GovC meeting. ΔDF_τ stands for the policy change variable and serves as a continuous treatment, since we stack several monetary policy events into one regression. After controlling for an extensive set of variables ($X_{i,t}^j$), the difference between the lending rates of new loans issued before and after the meeting is driven by the announced policy changes. Interaction terms allow for the heterogeneous pass-through of monetary policy rates depending on individual loan characteristics ($Z_{i,t}^k$ and $W_{i,t}^m$):

$$\begin{aligned} \begin{bmatrix} r_{i,t \in [\tau - 6W, \tau)} \\ r_{i,t \in (\tau, \tau + 6W]} \end{bmatrix} &= \beta_\tau + \left(\delta_0 + \sum_k \delta_{1,k} \cdot Z_{i,t}^k + \sum_m \delta_{2,m} \cdot W_{i,t}^m \right) \\ &\times \begin{bmatrix} 0 \\ \Delta DF_\tau \end{bmatrix} + \sum_j \gamma_j \cdot X_{i,t}^j + u_{i,t}, \end{aligned} \quad (5)$$

where the coefficient β_τ represents a GovC meeting-specific intercept, accounting for differences in the level of loan rates around each monetary policy event. The set of controls and fixed effects is unchanged from the earlier specification. However, since Equation (5) analyzes numerous monetary policy decisions, all fixed effects are now interacted with the GovC meeting (period) fixed effects. These interactions ensure that all possible country, creditor, debtor, and contract characteristics surrounding each GovC meeting are controlled for. In other words, this approach ensures the comparability of new loans issued before and after each monetary policy decision.

The second adjustment to our methodology concerns the variable representing policy changes, now defined as the announced change in the deposit facility rate (ΔDF_τ) at date τ . Policy rate adjustments between July 2022 and September 2023 were not exogenous and were largely anticipated by economic agents. To address this, we

²⁹It is important to note that many loans appear in the stacked regression twice: once in the “before” group and once in the “after” group.

follow the approach of Altavilla et al. (2022), estimate Equation (5) by two-stage least squares (TSLS), and instrument ΔDF_τ with monetary policy surprises, measured using high-frequency movements in asset prices around official policy announcements. These surprises are sourced from the Euro Area Monetary Policy Event-Study Database (Altavilla, Brugnolini et al. 2019), widely used in recent empirical research (e.g., Altavilla et al. 2023; Fungáčová, Kerola, and Laine 2023). We employ two instruments simultaneously: target and timing surprises. Target surprise captures changes in short-term interest rates during the press release window, while timing surprise reflects market expectations for policy changes over the next few meetings, measured during the press conference window (Altavilla, Brugnolini et al. 2019). This approach aligns broadly with Altavilla et al. (2022), who use changes in OIS rates with maturities of up to three years as instruments. However, we exclude forward guidance surprises due to their weak correlation with changes in the deposit facility rate.³⁰ The relevance of the instruments is largely driven by the choice of the policy variable—the deposit facility rate. While forward guidance and quantitative easing (QE) surprises are important for loans with long relevant risk-free rate maturities, the inclusion of these surprises in the analysis requires a different sample period and different policy variable.

We instrument ΔDF_τ rather than directly including monetary surprises in Equation (5) for two reasons. First, this simplifies the interpretation of regression results, which now describe responses to an unexpected 1 percentage point change in the deposit facility rate. Second, using multiple monetary surprises as instruments enhances the reliability of our estimates. The coefficients of interest, $\delta_{1,k}$, estimate the extent to which the pass-through of unexpected changes in the deposit facility rate to lending rates varies across maturity buckets of the relevant risk-free rate ($Z_{i,t}^k$). Individual loan value weights are applied while estimating Equation (5). As before, we also decompose the total transmission to lending rates into transmission

³⁰The correlation between changes in the deposit facility rate and the target surprise is 0.41 for 2022–23, and 0.45 for the timing surprise. In contrast, correlations with forward guidance and QE surprises are weaker at 0.13 and –0.23, respectively. Note that these correlations are based on only 15 GovC meetings during 2022–23, so they should be interpreted with caution.

to relevant risk-free rates and the premium, and $r_{i,t}$ represents $r_{i,t}^{loan}$, $r_{i,t}^{risk-free}$, or $r_{i,t}^{premium}$.

Equation (5) analyzes interest rate pass-through over a relatively short horizon (which we call $h = 1$ in the regression output tables). A period of six weeks following each GovC meeting may be insufficient to capture the full extent of the pass-through to lending rates. To examine longer-term responses to monetary policy surprises, we also estimate the effects over extended horizons: 7 to 12 weeks and 13 to 18 weeks post-monetary surprise. Specifically, we analyze new loans issued within the intervals $t \in (\tau + 6W, \tau + 12W]$ and $t \in (\tau + 12W, \tau + 18W]$, also referred to as $h = 2$ and $h = 3$, respectively.

Analyzing longer horizons of monetary transmission presents substantial challenges, as multiple GovC meetings may occur between the pre- and post-meeting periods for newly issued loans. Equation (5) attributes all changes in loan interest rates to the GovC meeting on date τ . This approach does not introduce bias over a long sample period, as monetary policy surprises—used as instruments for changes in deposit facility rates—are not autocorrelated. The absence of autocorrelation in monetary policy surprises is a natural phenomenon; market participants would detect any systematic patterns in monetary policy decisions and adjust their expectations accordingly. However, in very short samples (as is the case in this study), monetary surprises may appear to be autocorrelated, potentially leading to biased estimates of monetary transmission. This limitation should be considered when interpreting results over longer horizons.³¹

Table 4 shows the estimated coefficients from Equation (5). The pass-through from monetary policy rates to lending rates strengthens as the maturity of the relevant risk-free rates shortens. This effect becomes particularly pronounced over longer horizons (columns 1–3), i.e., when considering horizon $h = 2$ or $h = 3$ instead of our default horizon $h = 1$. The most substantial pass-through is observed for loans with risk-free rates of one month or less. For these loans, an unexpected 1 percentage point increase in the deposit facility

³¹The limited time dimension and data constraints in some cases are also the primary reasons why pass-through equations are not estimated separately for each country.

Table 4. Regression Results for the Pass-Through of Monetary Policy Rates to Lending Rates, Relevant Risk-Free Rate, and Premium

Variables	Total Rate			Relevant Risk-Free Rate			Premium		
	$h = 1$	$h = 2$	$h = 3$	$h = 1$	$h = 2$	$h = 3$	$h = 1$	$h = 2$	$h = 3$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Change in DF Rate (ΔDF Instrumented by Target and Timing Surprises)	0.17* (0.08)	0.59*** (0.14)	0.37*** (0.10)	-0.21*** (0.02)	-0.17*** (0.02)	-0.65*** (0.02)	0.38*** (0.09)	0.76*** (0.13)	1.02*** (0.11)
$\Delta DF \times (0, 1]$ Month Risk-Free Rate	0.33* (0.14)	0.78*** (0.25)	1.47*** (0.23)	1.10*** (0.05)	1.5*** (0.06)	2.25*** (0.07)	-0.76*** (0.14)	-0.75*** (0.27)	-0.77*** (0.26)
$\Delta DF \times (1, 3]$ Month Risk-Free Rate	0.19 (0.13)	0.65 (0.33)	1.24*** (0.26)	0.77*** (0.05)	1.21*** (0.05)	1.91*** (0.05)	-0.58*** (0.12)	-0.57 (0.35)	-0.67* (0.28)
$\Delta DF \times (3, 6]$ Month Risk-Free Rate	0.05 (0.11)	0.40** (0.14)	0.98*** (0.13)	0.64*** (0.04)	1.06*** (0.03)	1.66*** (0.05)	-0.60*** (0.12)	-0.66*** (0.15)	-0.68*** (0.15)
$\Delta DF \times (6, 12]$ Month Risk-Free Rate	0.22* (0.10)	0.34 (0.19)	0.59** (0.21)	0.56*** (0.05)	0.88*** (0.06)	1.35*** (0.06)	-0.34*** (0.09)	-0.54* (0.21)	-0.76*** (0.22)
$\Delta DF \times (1, 2]$ Year Risk-Free Rate	0.39* (0.16)	0.13 (0.16)	0.63*** (0.19)	0.64*** (0.04)	0.89*** (0.04)	1.34*** (0.07)	-0.26 (0.16)	-0.76*** (0.16)	-0.71*** (0.17)
$\Delta DF \times (2, 5]$ Year Risk-Free Rate	-0.10 (0.11)	-0.16 (0.19)	0.27* (0.11)	0.18*** (0.03)	0.06* (0.03)	0.53*** (0.04)	-0.27* (0.12)	-0.22 (0.19)	-0.26* (0.12)
$\Delta DF \times$ Floating Rate	0.01 (0.11)	-0.37 (0.24)	-0.40 (0.23)	-0.13** (0.05)	-0.11* (0.05)	-0.09 (0.09)	0.15 (0.09)	-0.26 (0.27)	-0.32 (0.27)
$\Delta DF \times$ Adjustment More Frequent Than Reference Rate Maturity	0.02 (0.09)	0.14 (0.13)	0.02 (0.18)	0.03 (0.03)	0.07* (0.03)	-0.10 (0.08)	-0.01 (0.08)	0.07 (0.13)	0.12 (0.22)
$\Delta DF \times$ Adjustment Less Frequent Than Reference Rate Maturity	-0.07 (0.21)	-0.09 (0.26)	0.23 (0.28)	0.03 (0.06)	0.03 (0.07)	0.10 (0.11)	-0.09 (0.20)	-0.12 (0.28)	0.13 (0.27)
$\Delta DF \times$ Non-EUR Reference Rate	-0.15* (0.06)	0.08 (0.09)	-0.11 (0.10)	0.11 (0.06)	0.09 (0.06)	0.17** (0.06)	-0.26** (0.09)	-0.010 (0.08)	-0.27* (0.11)

(continued)

rate results in a 1.473 percentage point larger rise in lending rates after 13–18 weeks compared with loans where the relevant risk-free rates have maturities exceeding five years. The pass-through becomes more muted for loans with progressively longer maturities of the relevant risk-free rates: 1 to 3 months (1.24), 3 to 6 months (0.98), 6 to 12 months (0.59), and 1 to 2 years (0.63).

The differences in the strength of pass-through are largely driven by how the risk-free rates relevant to each group of loans respond to monetary policy changes (columns 4–6). The estimated coefficients suggest that the pass-through of unexpected changes in monetary policy rates is significantly stronger for risk-free rates with shorter maturities. This reflects what can be considered the automatic component of pass-through, as shorter-maturity risk-free rates tend to adjust more directly and rapidly to shifts in policy rates.³² Consequently, loans tied to these rates exhibit a more pronounced sensitivity to monetary policy surprises.

However, this mechanical pass-through is not the only factor at play, as adjustments in premia act to smooth differences in pass-through across loan categories. Loans linked to shorter-maturity risk-free rates also experienced a comparatively smaller increase in premia (columns 7–9), which partially offsets the differences in the aggregate pass-through to lending rates. For example, for loans that have risk-free rates with maturities of one month or less, an unexpected 1 percentage point increase in the deposit facility rate leads to a 2.25 percentage point larger rise in underlying risk-free rates compared with loans with relevant risk-free rates exceeding five years. However, the difference in pass-through to lending rates is more muted (1.47 percentage points), due to a more substantial increase in premia for loans with longer-term risk-free rates. Thus, while loans linked to shorter-maturity risk-free rates experience a more pronounced pass-through of monetary policy surprises, the effect is not purely mechanical.

³²It is noteworthy that the estimated pass-through to short-term risk-free rates exceeds unity at longer horizons ($h = 2$ and $h = 3$). The regression coefficient captures the response of risk-free rates to an unexpected 1 percentage point change in the deposit facility rate. However, an unexpected increase in the deposit facility rate may signal additional changes that markets subsequently anticipate. This effect is particularly pronounced during our sample period, characterized by predominantly positive monetary surprises and a steady increase in policy rates.

Table 5. Interest Rate Pass-Through to Lending Rates from Unexpected 1 Percentage Point Increase in the Deposit Facility Rate, %

	<i>h</i> = 1	<i>h</i> = 2	<i>h</i> = 3
Existing Structure of New Loans	0.33	0.82	0.98
All New Loans Become Floating-Rate Loans with One-Month Reference Rate	0.52	1.01	1.42
Floating-Rate Loans Have One-Month Reference Rate, No Changes for Fixed-Rate Loans	0.38	0.91	1.13
All Floating-Rate Loans Become Fixed-Rate Loans, No Changes in Maturity	0.27	0.78	0.86
Source: AnaCredit. Note: Horizon <i>h</i> = 1 corresponds to the period of 6 weeks, <i>h</i> = 2 to 7–12 weeks, and <i>h</i> = 3 to 13–18 weeks following the Governing Council meeting.			

Other factors characterizing lending rates, such as a binary variable distinguishing between floating- and fixed-rate loans, do not exhibit a statistically significant effect on how monetary policy rate changes are passed through to lending rates. This underscores that the maturity of the relevant risk-free rate plays a pivotal role in explaining the variation in lending rate sensitivity to monetary policy changes, capturing much of the heterogeneity that a simple fixed-versus-floating classification fails to account for.

This variation in how loans are exposed to risk-free rates of different maturities has significant implications for the aggregate transmission of monetary policy. The first row of Table 5 shows that the estimated pass-through of an unexpected 1 percentage point increase in the deposit facility rate to lending rates is close to unity. However, this pass-through is approximately 30 percent weaker than it would be if all new loans were tied to risk-free rates with maturities of one month or less (e.g., if all loans were floating-rate loans linked to the one-month EURIBOR). Notably, even if only the floating-rate loans were indexed to a one-month reference rate, the pass-through would be about 15 percent stronger. Conversely, if all new

loans had fixed interest rates for the entire loan duration, the pass-through would be approximately 13 percent weaker. From a monetary policy perspective, these results are important, as they reveal heterogeneity in the pass-through of monetary policy to lending rates that depends on how monetary policy affects the shape of the yield curve.

3.4 *Robustness Checks*

We perform several robustness checks of our findings, both for the analysis of conditional changes in rates of new loans before and after monetary policy tightening, and for the analysis of the pass-through of monetary policy rates to lending rates. Detailed results can be found in the appendix,³³ while here we discuss the design of the robustness checks and the key results. First, we acknowledge the specific nature of revolving credits. The fact that the maturity of revolving credits is inherently ambiguous provides additional challenges in defining the relevant risk-free rate. Thus, we check the robustness of our findings to the exclusion of revolving credits. Despite a notable reduction in the number of observations, regression results provide the same story, stressing the importance of the maturity of the relevant risk-free rate for new other loans and credit lines other than revolving credits. Also, the decomposition of the total rate changes by euro-area countries remains robust.

The period of 2022–23 saw a rapid increase in interest rates across the euro area, presenting additional challenges for estimating monetary policy transmission, even over short horizons. During the weeks leading up to each GovC date τ ($t \in [\tau - 6W, \tau)$), interest rates on new loans often exhibited significant fluctuations, rarely remaining stable. This raises the possibility that estimates of pass-through from monetary policy rates to lending rates might primarily reflect the continuation of pre-surprise trends rather than the actual effect of monetary policy measures. To address this issue, we perform a robustness check by incorporating changes in the six-month OIS rate during the “before” period (from $\tau - 6W$ to $\tau - 1$) into our regressions. This variable captures the potential continuation of preexisting trends in interest rates. To ensure that this adjustment is

³³see Tables A.6–A.12 and Figure A.13 in the appendix.

appropriately applied, only new loans issued after the GovC meeting are influenced by this variable. Consequently, we include the term $\beta_2 \cdot [0, \Delta r_{\tau-6W, \tau-1}^{OIS6M}]^T$ on the right-hand side of Equation (5). Note that while the baseline specification of Equation (5) tends to overestimate the monetary policy transmission in 2022–23, the adjusted specification underestimates it. More important in the context of our research is that the results still point to the crucial role of the maturity of the relevant risk-free rate.

We account for the possibility that loans labeled as fixed-rate loans in AnaCredit are actually floating-rate loans, but the period of rate fixation exceeds one year. Unfortunately, the AnaCredit database does not provide any information in this respect. In order to test the robustness of our results to the assumption that the relevant risk-free rate maturity coincides with a loan maturity, we use an alternative assumption: we assume that for the fixed-rate loans with maturity exceeding three years, the relevant risk-free rate is the three-year OIS rate. Regression results prove the robustness of our findings to this assumption.

Finally, we rerun the regressions excluding new loans with imputed probabilities of default. Despite the selection bias introduced—only certain banks that are required to report the probability of default to AnaCredit—regression results lead to the same conclusion, stressing the importance of the maturity of the relevant risk-free rate for transmission.

4. Conclusions

This study provides new insights into the heterogeneity of lending practices across euro-area countries and the implications it has for the transmission of monetary policy. Using granular loan-level data from AnaCredit, covering nearly 7 million new loans issued to NFCs in 2022–23, we document significant variation in the prevalence of fixed- and floating-rate loans, rate fixation periods, and reference rates used for pricing loans. These differences have important implications for how lending rates respond to changes in monetary policy.

To provide a deeper understanding of interest rate sensitivity, we introduce a nuanced measure based on the maturity of the relevant risk-free rate, which identifies the segment of the risk-free rate yield

curve that influences a loan's interest rate. Our analysis reveals substantial cross-country variation in the average maturity of these relevant risk-free rates, ranging from approximately six months in countries like Latvia and Ireland to over five years in the Netherlands, Malta, and France.

Since monetary policy affects risk-free rates differently depending on their maturity, such disparities contribute to variations in how lending rates adjusted across euro-area economies during the post-pandemic episode of monetary tightening. Some of the smaller euro-area countries such as Latvia and Ireland, where floating-rate loans with short reference rate maturities predominate, and Italy, where fixed-rate loans with shorter maturities are prevalent, experienced larger increases in the relevant risk-free rates, leading to more pronounced rises in lending rates. In contrast, countries like the Netherlands and France, where longer-term risk-free rates play a significant role in loan pricing, observed more moderate adjustments in lending rates. Importantly, we found no systematic difference in lending rate changes for fixed- and floating-rate loans after accounting for the relevant risk-free rate. This indicates that the sensitivity of newly issued loan rates is not simply driven by whether a loan has a fixed a floating rate. Instead, it is influenced by a more nuanced set of factors that characterize the maturity of the relevant risk-free rate.

This study also shows that the pass-through of monetary policy changes to lending rates is strongest for loans linked to shorter-maturity risk-free rates, though this effect is not purely mechanical. Specifically, the increase in relevant risk-free rates for these loans is partially mitigated by a more muted rise in premia, compared with loans linked to longer-maturity reference rates. This behavior helped smooth cross-loan differences in lending rate adjustments that would have been more pronounced had lending rates moved solely in line with changes in the relevant risk-free rates. While the precise mechanisms underlying the differential adjustment in premia lie beyond the scope of this study, we demonstrate that this finding is robust to the choice of the sample period and is not driven by any particular configuration of the risk-free yield curve. Moreover, we show that the observed smoothing is not attributable to time-varying heterogeneity across banks, but rather reflects within-bank variation in loan pricing behavior.

Several potential explanations for these dynamics exist. One possibility is that premia smooth monetary-policy-induced increases in risk-free rates due to “composition effects” arising from both the supply and demand sides. On the supply side, financial institutions may adjust their risk-taking behavior in response to tighter monetary policy, shifting lending toward less risky borrowers. If borrowers of loans linked to shorter-maturity risk-free rates are more affected by such reallocation, these loans may have experienced a more pronounced decline in premia.

On the demand side, there may have been an opportunistic shift toward loan categories where the increase in the relevant risk-free rate was more moderate, placing upward pressure on premia in those segments. While we control for borrower risk through loan-specific probabilities of default, we cannot fully rule out the influence of such composition effects. Further research is needed to disentangle the relative contributions of these supply- and demand-side channels.

Overall, this study contributes to the literature by demonstrating how lending practices and the characteristics of loans can shape the effectiveness of monetary policy. Specifically, our findings illuminate how variations in lending practices drive cross-country differences in lending rate dynamics, offering a deeper understanding of the mechanisms underlying these divergences. Furthermore, our work opens avenues for further investigation into the drivers of cross-country heterogeneity in lending practices. While we document these differences and their implications, the underlying causes—whether rooted in institutional factors or firm- and bank-level characteristics—remain unexamined. Finally, our analysis focuses exclusively on the pass-through to interest rates on new loans, leaving the pass-through to interest rates on outstanding loans as a promising area for future research. We plan to address these questions in our future work.

Appendix. Additional Tables and Figures

Table A.1. Summary Statistics for Newly Issued Loans to NFCs by Euro-Area Country, 2022-23

Country	All Loans	...of Which Credit Lines, Revolving Credits, and Other Credits	...of Which Loans ≥25K, with a Positive Interest Rate, Reported within Six Months	...of Which Loans to NFCs Not in Financial and Insurance Sector and Nonsyndicated Loans	Loans Included in the Analysis	...as a Share of All Loans
Value of New Loans						
	Bill. of EUR	%	%	%	Bill. of EUR	%
AT	195.0	94.7	68.5	90.7	114.6	58.8
BE	350.0	97.0	49.8	81.8	138.4	39.5
CY	4.8	94.0	94.8	96.4	4.1	85.9
DE	975.2	87.2	72.1	83.5	511.9	52.5
EE	6.9	88.8	91.1	88.9	4.9	71.9
ES	611.9	80.4	73.3	85.9	309.8	50.6
FI	55.7	89.4	91.1	71.3	32.4	58.1
FR	1,935.5	79.1	41.2	76.8	484.6	25.0
GR	51.1	99.1	76.9	85.2	33.1	64.8
IE	184.4	90.0	34.9	69.8	40.5	21.9
IT	1,172.6	71.6	73.4	88.3	544.2	46.4
LT	9.3	71.9	92.9	90.3	5.6	60.3
LU	222.9	65.1	56.7	69.6	57.2	25.7
LV	3.6	84.1	88.4	94.7	2.6	70.4
MT	4.5	71.1	78.4	93.3	2.3	51.8
NL	234.2	83.6	58.1	81.9	93.1	39.7
PT	66.1	83.8	71.6	95.1	37.7	57.0
SI	14.4	81.5	91.0	84.4	9.0	62.6
SK	22.0	81.8	82.0	88.1	13.0	59.1

Table A.1. (Continued)

Country	...of Which Credit Lines, Revolving Credits, and Other Credits	...of Which Loans ≥25K, with a Positive Interest Rate, Reported within Six Months	...of Which Loans to NFCs Not in Financial and Insurance Sector and Nonsyndicated Loans	Loans Included in the Analysis	... as a Share of All Loans
Number of New Loans					
	Number	%	%	Number	%
AT	370,789	74.1	42.3	114,660	30.9
BE	927,843	76.6	44.5	301,076	32.4
CY	26,646	54.5	49.8	7,169	26.9
DE	5,233,584	75.0	40.7	1,580,626	30.2
EE	18,237	38.9	72.5	5,116	28.1
ES	9,469,269	49.3	31.0	1,436,303	15.2
FI	488,964	24.0	46.6	52,863	10.8
FR	8,588,863	51.6	42.1	1,815,906	21.1
GR	65,140	79.2	66.7	34,097	52.3
IE	229,090	61.8	36.7	50,165	21.9
IT	8,328,146	32.1	45.4	1,203,663	14.5
LT	43,405	35.1	58.8	8,892	20.5
LU	171,241	25.7	75.5	30,948	18.1
LV	9,125	36.8	84.7	2,835	31.1
MT	197,493	1.4	69.5	1,902	1.0
NL	341,831	91.3	18.1	53,807	15.7
PT	695,999	71.2	38.8	192,039	27.6
SI	147,704	22.8	51.7	17,209	11.7
SK	143,285	24.7	66.0	23,142	16.2
Source: AnaCredit.					
Note: New loans to euro-area NFCs issued in 2022–23, denominated in EUR. The first column represents the country of residence of the bank.					

Table A.2. PD and Imputed PD Data Availability for Newly Issued Loans to NFCs by Euro-Area Country, 2022–23

Country	Number of Observations	Total Value, Bill. of EUR	Coverage of PD after Imputation		Correlation, PD, and Imputed PD
			% of Number	% of Value	
AT	114,660	114.6	66.9	70.7	0.76
BE	301,076	138.4	91.0	89.2	0.38
CY	7,169	4.1	68.2	65.7	—
DE	1,580,626	511.9	57.3	79.8	0.27
EE	5,116	4.9	86.1	81.9	0.67
ES	1,436,303	309.8	66.4	72.5	0.42
FI	52,863	32.4	81.1	88.8	0.38
FR	1,815,906	484.6	93.4	83.6	0.63
GR	34,097	33.1	60.6	77.9	0.57
IE	50,165	40.5	70.3	58.5	0.37
IT	1,203,663	544.2	84.5	94.4	0.68
LT	8,892	5.6	73.2	81.8	0.70
LU	30,948	57.2	64.8	72.9	0.39
LV	2,835	2.6	89.1	88.7	0.69
MT	1,902	2.3	63.9	65.9	—
NL	53,807	93.1	94.1	70.7	0.55
PT	192,039	37.7	83.3	87.3	0.57
SI	17,209	9.0	88.3	76.8	0.81
SK	23,142	13.0	97.6	94.9	0.63
Source: AnaCredit.					
Note: New loans to euro-area NFCs (excluding financial and insurance activities) issued in 2022–23, denominated in EUR, exceeding EUR 25,000, with a positive interest rate. The sample consists of the following types of loans: credit lines other than revolving credits, revolving credits, other loans, excluding syndicated loans. The first column represents the country of residence of the bank.					

Table A.3. Summary Statistics Before and After the Exclusion of Outliers, 2022:Q1 and 2023:Q4

Country	Number of Loans	Total Value, Bill. of EUR	Share of Outliers, % of Value	W. Mean Rate, %		W. Mean Maturity, Years		Value of the Loan, Thsd. of EUR	
				All Loans	Excl. Outliers	All Loans	Excl. Outliers	All Loans	Excl. Outliers
AT	28,182	28,774.3	9.6	3.2	3.2	7.8	8.0	1,021.0	933.1
BE	85,915	41,862.8	11.9	3.8	3.7	3.3	3.3	487.3	434.2
CY	1,849	942.9	20.3	4.2	4.4	7.3	6.7	509.9	413.9
DE	374,444	119,511.6	29.8	3.5	3.5	3.8	4.6	319.2	226.5
EE	1,278	1,112.1	23.4	5.3	5.1	4.1	3.9	870.2	678.7
ES	365,703	78,062.0	23.2	3.5	3.5	3.7	3.4	213.5	165.9
FI	13,978	6,608.0	19.9	3.4	3.4	7.5	8.2	472.7	383.0
FR	448,147	123,952.4	23.6	2.8	2.9	6.8	7.9	276.6	213.7
GR	7,512	6,958.8	22.4	5.1	4.8	7.4	7.2	926.4	727.6
IE	13,414	10,476.1	17.3	3.8	4.0	3.2	3.6	781.0	657.6
IT	329,247	148,139.7	24.8	3.8	3.7	2.3	2.6	449.9	342.0
LT	1,746	1,272.2	26.9	4.8	5.1	5.1	4.7	728.6	542.0
LU	7,444	12,672.8	20.4	2.9	2.9	2.4	2.9	1,702.4	1,371.3
LV	635	487.5	29.3	5.3	5.4	4.4	4.4	767.7	564.4
MT	496	399.4	29.7	4.1	4.3	7.1	6.6	805.2	585.3
NL	14,653	23,909.7	11.6	3.8	3.7	7.5	8.2	1,631.7	1,458.8
PT	50,550	9,342.2	11.9	3.7	3.7	4.5	4.3	184.8	164.6
SI	4,059	2,164.5	13.3	3.6	3.6	3.6	3.6	533.3	468.6
SK	5,621	2,883.3	20.3	3.7	3.9	4.0	4.4	513.0	414.0

Source: AnaCredit.
Note: New loans to euro-area NFCs (excluding financial and insurance activities) issued in 2022:Q1 and 2023:Q4, denominated in EUR, exceeding EUR 25,000, with a positive interest rate. The sample consists of the following types of loans: credit lines other than revolving credits, revolving credits, other loans, excluding syndicated loans. The first column represents the country of residence of the bank.

Table A.4. Summary Statistics Before and After the
Exclusion of Loans with Missing Data, 2022:Q1 and 2023:Q4

Country	Number of Loans	Total Value, Bill. of EUR	Share of Loans with Missing Data, %		W. Mean Rate, %		W. Mean Maturity, Years		Value of the Loan, Thsd. of EUR	
			Number	Value	All Loans	Excl. Missing	All Loans	Excl. Missing	All Loans	Excl. Missing
AT	27,871	26,005.5	45.3	37.8	3.2	3.2	8.0	7.9	933.1	1,061.6
BE	84,969	36,895.2	21.3	22.5	3.7	3.8	3.3	3.2	434.2	427.5
CY	1,816	751.6	36.9	42.0	4.4	4.4	6.9	6.8	413.9	380.6
DE	370,316	83,859.8	45.4	33.9	3.5	3.6	4.6	4.9	226.5	274.4
EE	1,255	851.8	14.7	19.9	5.1	5.0	3.9	4.1	678.7	637.5
ES	361,680	59,986.3	38.1	31.1	3.5	3.7	3.4	3.4	165.9	184.6
FI	13,821	5,293.7	19.7	18.4	3.4	3.3	8.2	8.1	383.0	389.5
FR	443,043	94,680.5	37.7	54.1	2.9	2.9	7.9	6.0	213.7	157.5
GR	7,426	5,402.9	49.6	36.4	4.8	4.7	7.2	7.0	727.6	917.5
IE	13,175	8,664.0	77.2	58.2	4.0	4.0	3.6	3.5	657.6	1,204.1
IT	325,625	111,354.2	19.8	9.4	3.7	3.7	2.6	2.5	342.0	386.4
LT	1,717	930.5	20.5	22.3	5.1	4.9	4.7	4.8	542.0	529.5
LU	7,357	10,088.4	56.9	43.2	2.9	2.5	2.9	3.1	1,371.3	1,806.4
LV	611	344.9	15.5	23.8	5.4	5.2	4.4	4.5	564.4	509.5
MT	480	280.9	58.5	57.8	4.3	4.6	6.6	6.7	585.3	595.5
NL	14,483	21,128.3	14.2	39.3	3.7	3.9	8.2	9.1	1,458.8	1,032.6
PT	49,993	8,230.7	16.9	13.1	3.7	3.7	4.3	4.3	164.6	172.2
SI	4,006	1,877.2	12.2	23.1	3.6	3.7	3.6	3.8	468.6	410.6
SK	5,550	2,297.5	4.7	17.9	3.9	4.0	4.4	4.8	414.0	356.9

Source: AnaCredit.
Note: New loans to euro-area NFCs (excluding financial and insurance activities) issued in 2022:Q1 and 2023:Q4, denominated in EUR, exceeding EUR 25,000, with a positive interest rate. The sample consists of the following types of loans: credit lines other than revolving credits, revolving credits, other loans, excluding syndicated loans. The first column represents the country of residence of the bank.

Table A.5. Regression Results for Conditional Change in Total Rate, Relevant Risk-Free Rate and Premium between 2022:Q1 and 2022:Q2, 2022:Q3, . . . , 2023:Q4

		2022:Q2	2022:Q3	2022:Q4	2023:Q1	2023:Q2	2023:Q3	2023:Q4
Dummy for Post Period (D_{post})	Total Rate	0.57*** (0.04)	1.14*** (0.05)	1.99*** (0.05)	2.44*** (0.07)	2.82*** (0.04)	3.02*** (0.04)	3.11*** (0.05)
	Ref. Rate	1.16*** (0.02)	1.52*** (0.02)	2.27*** (0.02)	2.38*** (0.02)	2.47*** (0.02)	2.65*** (0.02)	2.45*** (0.02)
	Premium	-0.59*** (0.04)	-0.38*** (0.05)	-0.28*** (0.05)	0.06 (0.07)	0.34*** (0.03)	0.37*** (0.03)	0.66*** (0.04)
$D_{post} \times (0, 1]$ Month Risk-Free Rate	Total Rate	-0.51*** (0.06)	-0.60*** (0.12)	-0.36** (0.17)	0.17 (0.16)	0.49** (0.17)	0.88*** (0.16)	1.01*** (0.16)
	Ref. Rate	-1.42*** (0.04)	-1.07*** (0.05)	-0.51*** (0.04)	0.43*** (0.04)	1.13*** (0.03)	1.49*** (0.03)	1.99*** (0.03)
	Premium	0.91*** (0.07)	0.47*** (0.12)	0.15 (0.17)	-0.26 (0.16)	-0.64*** (0.17)	-0.61*** (0.16)	-0.98*** (0.15)
$D_{post} \times (1, 3]$ Month Risk-Free Rate	Total Rate	-0.53*** (0.05)	-0.63*** (0.07)	-0.34*** (0.10)	0.08 (0.11)	0.50*** (0.09)	0.79*** (0.12)	0.92*** (0.13)
	Ref. Rate	-1.26*** (0.04)	-0.71*** (0.04)	-0.12*** (0.03)	0.72*** (0.03)	1.31*** (0.03)	1.58*** (0.03)	1.97*** (0.03)
	Premium	0.73*** (0.06)	0.08 (0.07)	-0.22** (0.10)	-0.65*** (0.10)	-0.81*** (0.09)	-0.79*** (0.12)	-1.05*** (0.12)
$D_{post} \times (3, 6]$ Month Risk-Free Rate	Total Rate	-0.51*** (0.04)	-0.41*** (0.06)	-0.15 (0.08)	0.29** (0.10)	0.59*** (0.10)	0.83*** (0.12)	0.80*** (0.12)
	Ref. Rate	-1.05*** (0.03)	-0.39*** (0.03)	0.18*** (0.03)	0.97*** (0.04)	1.42*** (0.03)	1.60*** (0.03)	1.89*** (0.03)
	Premium	0.54*** (0.05)	-0.01 (0.07)	-0.33*** (0.08)	-0.67*** (0.09)	-0.82*** (0.09)	-0.77*** (0.12)	-1.09*** (0.11)

(continued)

Table A.5. (Continued)

		2022:Q2	2022:Q3	2022:Q4	2023:Q1	2023:Q2	2023:Q3	2023:Q4
$D_{post} \times (6, 12]$ Month Risk-Free Rate	Total Rate	-0.34*** (0.06)	-0.03 (0.10)	0.09 (0.12)	0.45*** (0.09)	0.69*** (0.07)	0.80*** (0.08)	0.73*** (0.09)
	Ref. Rate	-0.82*** (0.03)	-0.09** (0.03)	0.46*** (0.03)	1.08*** (0.04)	1.43*** (0.04)	1.55*** (0.04)	1.63*** (0.04)
	Premium	0.48*** (0.05)	0.06 (0.08)	-0.37*** (0.11)	-0.63*** (0.07)	-0.74*** (0.06)	-0.76*** (0.07)	-0.90*** (0.09)
$D_{post} \times (1, 2]$ Year Risk-Free Rate	Total Rate	-0.40*** (0.04)	-0.15** (0.07)	-0.03 (0.08)	0.24** (0.08)	0.38*** (0.07)	0.51*** (0.08)	0.36*** (0.08)
	Ref. Rate	-0.62*** (0.03)	-0.02 (0.03)	0.54*** (0.03)	1.04*** (0.03)	1.25*** (0.03)	1.33*** (0.02)	1.20*** (0.03)
	Premium	0.22*** (0.05)	-0.13 (0.07)	-0.57*** (0.09)	-0.80*** (0.09)	-0.86*** (0.08)	-0.82*** (0.08)	-0.85*** (0.08)
$D_{post} \times (2, 5]$ Year Risk-Free Rate	Total Rate	-0.13** (0.04)	-0.04 (0.05)	0.13* (0.05)	0.16* (0.08)	0.17* (0.08)	0.19* (0.10)	0.17** (0.06)
	Ref. Rate	-0.12*** (0.02)	-0.02 (0.02)	0.32*** (0.02)	0.51*** (0.02)	0.53*** (0.03)	0.52*** (0.02)	0.35*** (0.02)
	Premium	-0.01 (0.04)	-0.01 (0.05)	-0.19*** (0.05)	-0.35*** (0.07)	-0.36*** (0.09)	-0.33*** (0.09)	-0.18** (0.06)

Source: AnaCredit.

Note: This table replicates results from Table 2 for all post-quarters from 2022:Q2 until 2023:Q4. Only results for post-quarter dummy variables are reported for brevity. *** denotes significance at the 99.9 percent level, ** at 99 percent, and * at 95 percent. Standard errors are reported in parentheses. Ref. Rate stands for risk-free rate. The sample includes new loans to euro-area NFCs (excluding financial and insurance activities) issued in 2022:Q1 and 2023:Q4, denominated in EUR, exceeding EUR 25,000, with a positive interest rate. The sample consists of the following types of loans: credit lines other than revolving credits, revolving credits, other loans, excluding syndicated loans. For fixed-rate loans, the maturity of the relevant risk-free rate corresponds to the loan's maturity, while for floating-rate loans, it is equal to the maturity of the underlying reference rate. Results are obtained by WLS, where weights are determined by the loan values. D_{post} denotes a binary variable that equals one for a respective post-quarter and zero for 2022:Q1. Other variables (except the probability of default) are the binary (dummy) variables that equal one if a loan contract has a corresponding feature.

Table A.6. Regression Results for Conditional Change in Total Rate, Relevant Risk-Free Rate, and Premium between 2022:Q1 and 2023:Q4, Excluding Revolving Credits

Variables	Total Rate	Relevant Risk-Free Rate	Premium
	(1)	(2)	(3)
Dummy for 2023:Q4 (D_{2023Q4})	3.11*** (0.05)	2.42*** (0.02)	0.69*** (0.04)
$D_{2023Q4} \times (0, 1]$ Month Risk-Free Rate	1.13*** (0.18)	2.04*** (0.03)	-0.90*** (0.17)
$D_{2023Q4} \times (1, 3]$ Month Risk-Free Rate	0.93*** (0.13)	2.01*** (0.03)	-1.08*** (0.13)
$D_{2023Q4} \times (3, 6]$ Month Risk-Free Rate	0.82*** (0.13)	1.92*** (0.03)	-1.10*** (0.13)
$D_{2023Q4} \times (6, 12]$ Month Risk-Free Rate	0.77*** (0.09)	1.65*** (0.04)	-0.88*** (0.08)
$D_{2023Q4} \times (1, 2]$ Year Risk-Free Rate	0.31*** (0.08)	1.19*** (0.03)	-0.88*** (0.08)
$D_{2023Q4} \times (2, 5]$ Year Risk-Free Rate	0.20*** (0.06)	0.34*** (0.02)	-0.14* (0.06)
$D_{2023Q4} \times$ Floating Rate	-0.04 (0.15)	0.07** (0.02)	-0.11 (0.16)
$D_{2023Q4} \times$ Adjustment More Frequent Than Reference Rate Maturity	0.18 (0.20)	-0.01 (0.02)	0.19 (0.20)
$D_{2023Q4} \times$ Adjustment Less Frequent Than Reference Rate Maturity	-0.11 (0.25)	-0.04 (0.03)	-0.08 (0.23)
$D_{2023Q4} \times$ Non-EUR Reference Rate	0.18 (0.11)	-0.04 (0.02)	0.23* (0.11)
$D_{2023Q4} \times$ Other Single Reference Rate	-0.37 (0.29)	-0.05 (0.04)	-0.32 (0.28)
$D_{2023Q4} \times$ Multiple Reference Rates	-0.41 (0.28)	-0.08 (0.05)	-0.33 (0.30)
$D_{2023Q4} \times$ Cross-Border Loans	-0.21 (0.16)	0.01 (0.02)	-0.22 (0.16)
Probability of Default (Imputed if Missing)	9.23*** (1.15)	0.00 (0.07)	9.23*** (1.13)

(continued)

Table A.6. (Continued)

Variables	Total Rate	Relevant Risk-Free Rate	Premium
	(1)	(2)	(3)
Fixed Effects:			
Creditor Country	Yes	Yes	Yes
Bank	Yes	Yes	Yes
Loan Type × Creditor Country	Yes	Yes	Yes
Debtor Sector × Debtor Country	Yes	Yes	Yes
Debtor Region	Yes	Yes	Yes
Firm Size Class × Creditor Country	Yes	Yes	Yes
Loan Maturity × Creditor Country	Yes	Yes	Yes
Collateral Size × Creditor Country	Yes	Yes	Yes
Cross-Border Loan × Creditor Country	Yes	Yes	Yes
Multidebtor Loan × Creditor Country	Yes	Yes	Yes
Reference Rate Type × Creditor Country	Yes	Yes	Yes
Risk-Free Rate Maturity × Creditor Country	Yes	Yes	Yes
Reference Rate Adjustment Frequency × Creditor Country	Yes	Yes	Yes
Number of Observations	1,031,655	1,031,655	1,031,655
Within R^2	0.85	0.99	0.48
Standard Errors Clustered by	0.78	0.98	0.09
	Bank	Bank	Bank
Source: AnaCredit. Note: *** denotes significance at the 99.9 percent level, ** at 99 percent, and * at 95 percent. Standard errors are reported in parentheses. The sample includes new loans to euro-area NFCs (excluding financial and insurance activities) issued in 2022:Q1 and 2023:Q4, denominated in EUR, exceeding EUR 25,000, with a positive interest rate. The sample consists of the following types of loans: credit lines other than revolving credits, revolving credits, other loans, excluding syndicated loans. For fixed-rate loans, the maturity of the relevant risk-free rate corresponds to the loan's maturity, while for floating-rate loans, it is equal to the maturity of the underlying reference rate. Results are obtained by WLS, where weights are determined by the loan values. $D_{2023:Q4}$ denotes a binary variable that equals one for 2023:Q4 and zero for 2022:Q1. Other variables (except the probability of default) are the binary (dummy) variables that equal one if a loan contract has a corresponding feature.			

Table A.7. Regression Results for Conditional Change in Total Rate, Relevant Risk-Free Rate, and Premium between 2022:Q1 and 2023:Q4, Three-Year Adjustment for Fixed-Rate Loan

Variables	Total Rate	Relevant Risk-Free Rate	Premium
	(1)	(2)	(3)
Dummy for 2023:Q4 (D_{2023Q4})	3.18*** (0.12)	2.88*** (0.02)	0.29* (0.13)
$D_{2023Q4} \times (0, 1]$ Month Risk-Free Rate	0.97*** (0.13)	1.63*** (0.02)	-0.66*** (0.12)
$D_{2023Q4} \times (1, 3]$ Month Risk-Free Rate	0.87*** (0.16)	1.59*** (0.02)	-0.72*** (0.16)
$D_{2023Q4} \times (3, 6]$ Month Risk-Free Rate	0.74*** (0.11)	1.50*** (0.02)	-0.76*** (0.11)
$D_{2023Q4} \times (6, 12]$ Month Risk-Free Rate	0.68*** (0.08)	1.25*** (0.03)	-0.57*** (0.09)
$D_{2023Q4} \times (1, 2]$ Year Risk-Free Rate	0.28* (0.14)	0.86*** (0.02)	-0.58*** (0.14)
$D_{2023Q4} \times (2, 5]$ Year Risk-Free Rate	—	—	—
$D_{2023Q4} \times$ Floating Rate	-0.03 (0.12)	-0.01 (0.01)	-0.02 (0.12)
$D_{2023Q4} \times$ Adjustment More Frequent Than Reference Rate Maturity	0.11 (0.19)	0.02 (0.01)	0.09 (0.19)
$D_{2023Q4} \times$ Adjustment Less Frequent Than Reference Rate Maturity	-0.07 (0.24)	0.00 (0.03)	-0.07 (0.23)
$D_{2023Q4} \times$ Non-EUR Reference Rate	0.18 (0.11)	-0.04 (0.02)	0.23* (0.11)
$D_{2023Q4} \times$ Other Single Reference Rate	-0.12 (0.16)	-0.03 (0.04)	-0.09 (0.16)
$D_{2023Q4} \times$ Multiple Reference Rates	-0.26 (0.28)	-0.11* (0.05)	-0.16 (0.31)
$D_{2023Q4} \times$ Cross-Border Loans	-0.17 (0.14)	0.00 (0.02)	-0.17 (0.15)
Probability of Default (Imputed if Missing)	9.50*** (1.12)	-0.04 (0.06)	9.54*** (1.12)

(continued)

Table A.7. (Continued)

Variables	Total Rate	Relevant Risk-Free Rate	Premium
	(1)	(2)	(3)
Fixed Effects:			
Creditor Country	Yes	Yes	Yes
Bank	Yes	Yes	Yes
Loan Type × Creditor Country	Yes	Yes	Yes
Debtor Sector × Debtor Country	Yes	Yes	Yes
Debtor Region	Yes	Yes	Yes
Firm Size Class × Creditor Country	Yes	Yes	Yes
Loan Maturity × Creditor Country	Yes	Yes	Yes
Collateral Size × Creditor Country	Yes	Yes	Yes
Cross-Border Loan × Creditor Country	Yes	Yes	Yes
Multidebtor Loan × Creditor Country	Yes	Yes	Yes
Reference Rate Type × Creditor Country	Yes	Yes	Yes
Risk-Free Rate Maturity × Creditor Country	Yes	Yes	Yes
Reference Rate Adjustment Frequency × Creditor Country	Yes	Yes	Yes
Number of Observations	1,133,053	1,133,053	1,133,053
R^2	0.85	0.99	0.45
Within R^2	0.78	0.99	0.07
Standard Errors Clustered by	Bank	Bank	Bank
Source: AnaCredit. Note: *** denotes significance at the 99.9 percent level, ** at 99 percent, and * at 95 percent. Standard errors are reported in parentheses. The sample includes new loans to euro-area NFCs (excluding financial and insurance activities) issued in 2022:Q1 and 2023:Q4, denominated in EUR, exceeding EUR 25,000, with a positive interest rate. The sample consists of the following types of loans: credit lines other than revolving credits, revolving credits, other loans, excluding syndicated loans. For fixed-rate loans, the maturity of the relevant risk-free rate corresponds to the loan's maturity, while for floating-rate loans, it is equal to the maturity of the underlying reference rate. Results are obtained by WLS, where weights are determined by the loan values. $D_{2023:Q4}$ denotes a binary variable that equals one for 2023:Q4 and zero for 2022:Q1. Other variables (except the probability of default) are the binary (dummy) variables that equal one if a loan contract has a corresponding feature.			

Table A.8. Regression Results for Conditional Change in Total Rate, Relevant Risk-Free Rate, and Premium between 2022:Q1 and 2023:Q4, Using Non-imputed PD Observations Only

Variables	Total Rate	Relevant Risk-Free Rate	Premium
	(1)	(2)	(3)
Dummy for 2023:Q4 (D_{2023Q4})	3.11*** (0.06)	2.44*** (0.03)	0.67*** (0.05)
$D_{2023Q4} \times (0, 1]$ Month Risk-Free Rate	1.07*** (0.18)	2.01*** (0.03)	-0.94*** (0.18)
$D_{2023Q4} \times (1, 3]$ Month Risk-Free Rate	0.93*** (0.15)	1.98*** (0.03)	-1.05*** (0.14)
$D_{2023Q4} \times (3, 6]$ Month Risk-Free Rate	0.80*** (0.14)	1.91*** (0.04)	-1.10*** (0.13)
$D_{2023Q4} \times (6, 12]$ Month Risk-Free Rate	0.71*** (0.11)	1.67*** (0.05)	-0.96*** (0.10)
$D_{2023Q4} \times (1, 2]$ Year Risk-Free Rate	0.46*** (0.09)	1.24*** (0.03)	-0.77*** (0.09)
$D_{2023Q4} \times (2, 5]$ Year Risk-Free Rate	0.21** (0.08)	0.37*** (0.03)	-0.16 (0.08)
$D_{2023Q4} \times$ Floating Rate	0.02 (0.19)	0.07* (0.03)	-0.05 (0.19)
$D_{2023Q4} \times$ Adjustment More Frequent Than Reference Rate Maturity	0.29 (0.23)	0.01 (0.02)	0.28 (0.23)
$D_{2023Q4} \times$ Adjustment Less Frequent Than Reference Rate Maturity	-0.13 (0.17)	-0.04 (0.03)	-0.10 (0.16)
$D_{2023Q4} \times$ Non-EUR Reference Rate	0.10 (0.13)	-0.04 (0.02)	0.14 (0.13)
$D_{2023Q4} \times$ Other Single Reference Rate	-0.07 (0.16)	-0.05 (0.07)	-0.02 (0.15)
$D_{2023Q4} \times$ Multiple Reference Rates	-0.89** (0.27)	-0.11 (0.08)	-0.77** (0.27)
$D_{2023Q4} \times$ Cross-Border Loans	-0.38* (0.19)	-0.01 (0.03)	-0.37 (0.20)
Probability of Default (Imputed if Missing)	1.34*** (0.11)	-0.02* (0.01)	1.37*** (0.11)

(continued)

Table A.8. (Continued)

Variables	Total Rate	Relevant Risk-Free Rate	Premium
	(1)	(2)	(3)
Fixed Effects:			
Creditor Country	Yes	Yes	Yes
Bank	Yes	Yes	Yes
Loan Type × Creditor Country	Yes	Yes	Yes
Debtor Sector × Debtor Country	Yes	Yes	Yes
Debtor Region	Yes	Yes	Yes
Firm Size Class × Creditor Country	Yes	Yes	Yes
Loan Maturity × Creditor Country	Yes	Yes	Yes
Collateral Size × Creditor Country	Yes	Yes	Yes
Cross-Border Loan × Creditor Country	Yes	Yes	Yes
Multidebtor Loan × Creditor Country	Yes	Yes	Yes
Reference Rate Type × Creditor Country	Yes	Yes	Yes
Risk-Free Rate Maturity × Creditor Country	Yes	Yes	Yes
Reference Rate Adjustment Frequency × Creditor Country	Yes	Yes	Yes
Number of Observations	841,004	841,004	841,004
R^2	0.85	0.99	0.44
Within R^2	0.78	0.98	0.07
Standard Errors Clustered by	Bank	Bank	Bank
Source: AnaCredit. Note: *** denotes significance at the 99.9 percent level, ** at 99 percent, and * at 95 percent. Standard errors are reported in parentheses. The sample includes new loans to euro-area NFCs (excluding financial and insurance activities) issued in 2022:Q1 and 2023:Q4, denominated in EUR, exceeding EUR 25,000, with a positive interest rate. The sample consists of the following types of loans: credit lines other than revolving credits, revolving credits, other loans, excluding syndicated loans. For fixed-rate loans, the maturity of the relevant risk-free rate corresponds to the loan's maturity, while for floating-rate loans, it is equal to the maturity of the underlying reference rate. Results are obtained by WLS, where weights are determined by the loan values. D_{2023Q4} denotes a binary variable that equals one for 2023:Q4 and zero for 2022:Q1. Other variables (except the probability of default) are the binary (dummy) variables that equal one if a loan contract has a corresponding feature.			

Table A.9. Regression Results for the Pass-Through of Monetary Policy Rates to Lending Rates, Relevant Risk-Free Rate, and Premium, Excluding Revolving Credits

Variables	Total Rate			Relevant Risk-Free Rate			Premium		
	$h = 1$	$h = 2$	$h = 3$	$h = 1$	$h = 2$	$h = 3$	$h = 1$	$h = 2$	$h = 3$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Change in DF Rate (ΔDF Instrumented by Target and Timing Surprises)	0.19* (0.08)	0.65*** (0.14)	0.42*** (0.10)	-0.21*** (0.02)	-0.17*** (0.02)	-0.64*** (0.03)	0.40*** (0.08)	0.82*** (0.13)	1.05*** (0.10)
$\Delta DF \times [0, 1]$ Month Risk-Free Rate	0.20 (0.13)	0.68* (0.27)	1.47*** (0.23)	1.08*** (0.06)	1.54*** (0.06)	2.23*** (0.08)	-0.89*** (0.13)	-0.87*** (0.29)	-0.77*** (0.27)
$\Delta DF \times (1, 3]$ Month Risk-Free Rate	0.14 (0.14)	0.57 (0.36)	1.17*** (0.25)	0.78*** (0.05)	1.21*** (0.05)	1.89*** (0.05)	-0.63*** (0.12)	-0.64 (0.38)	-0.72*** (0.27)
$\Delta DF \times (3, 6]$ Month Risk-Free Rate	0.09 (0.12)	0.29 (0.17)	0.98*** (0.14)	0.66*** (0.04)	1.08*** (0.04)	1.65*** (0.05)	-0.57*** (0.12)	-0.79*** (0.18)	-0.67*** (0.16)
$\Delta DF \times (6, 12]$ Month Risk-Free Rate	0.30*** (0.08)	0.34 (0.18)	0.72*** (0.20)	0.58*** (0.05)	0.88*** (0.06)	1.35*** (0.06)	-0.28*** (0.08)	-0.54*** (0.21)	-0.63*** (0.21)
$\Delta DF \times (1, 2]$ Year Risk-Free Rate	0.17 (0.13)	0.04 (0.16)	0.79*** (0.19)	0.61*** (0.04)	0.85*** (0.04)	1.32*** (0.08)	-0.44*** (0.14)	-0.81*** (0.17)	-0.53*** (0.17)
$\Delta DF \times (2, 5]$ Year Risk-Free Rate	-0.18 (0.13)	-0.22 (0.21)	0.36*** (0.11)	0.16*** (0.03)	0.05 (0.03)	0.50*** (0.04)	-0.33* (0.13)	-0.28 (0.21)	-0.14 (0.11)
$\Delta DF \times$ Floating Rate	0.09 (0.11)	-0.26 (0.25)	-0.43* (0.21)	-0.13** (0.05)	-0.12* (0.05)	-0.08 (0.09)	0.22* (0.10)	-0.14 (0.28)	-0.35 (0.26)
$\Delta DF \times$ Adjustment More Frequent Than Reference Rate Maturity	0.00 (0.10)	0.15 (0.15)	0.22 (0.16)	0.03 (0.03)	0.07** (0.02)	0.01 (0.07)	-0.03 (0.10)	0.07 (0.16)	0.22 (0.21)
$\Delta DF \times$ Adjustment Less Frequent Than Reference Rate Maturity	-0.14 (0.26)	-0.12 (0.29)	0.11 (0.35)	0.05 (0.06)	0.04 (0.08)	0.09 (0.11)	-0.19 (0.24)	-0.15 (0.30)	0.03 (0.32)
$\Delta DF \times$ Non-EUR Reference Rate	-0.13* (0.06)	0.00 (0.08)	-0.13 (0.10)	0.12 (0.06)	0.09 (0.06)	0.14* (0.06)	-0.24*** (0.08)	-0.09 (0.08)	-0.27*** (0.09)
$\Delta DF \times$ Other Single Reference Rate	-0.15 (0.11)	-0.35* (0.18)	-0.61** (0.23)	-0.02 (0.06)	-0.05 (0.08)	-0.02 (0.08)	-0.13 (0.14)	-0.30 (0.30)	-0.59*** (0.22)
$\Delta DF \times$ Multiple Reference Rates	-0.56 (0.47)	-0.71* (0.34)	-0.42 (0.36)	0.23*** (0.08)	0.50* (0.19)	0.36*** (0.14)	-0.79 (0.46)	-1.21*** (0.30)	-0.78 (0.44)
$\Delta DF \times$ Cross-Border Loans	0.41*** (0.15)	0.22 (0.24)	-0.04 (0.29)	0.07* (0.03)	0.08* (0.04)	0.16* (0.07)	0.34* (0.16)	0.14 (0.25)	-0.19 (0.34)
Probability of Default (Imputed if Missing)	7.86*** (0.84)	7.78*** (0.80)	7.59*** (0.80)	-0.03 (0.05)	-0.11 (0.07)	-0.17 (0.10)	7.90*** (0.84)	7.89*** (0.80)	7.76*** (0.81)

(continued)

Table A.10. Regression Results for the Pass-Through of Monetary Policy Rates to Lending Rates, Relevant Risk-Free Rate, and Premium, Controlling for the Preexisting Trends

Variables	Total Rate			Relevant Risk-Free Rate			Premium		
	$h = 1$	$h = 2$	$h = 3$	$h = 1$	$h = 2$	$h = 3$	$h = 1$	$h = 2$	$h = 3$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Change in DF Rate (ΔDF Instrumented by Target and Timing Surprises)	0.22** (0.08)	0.70*** (0.14)	0.81*** (0.11)	-0.19*** (0.02)	-0.03 (0.02)	-0.08 (0.05)	0.41*** (0.09)	0.73*** (0.13)	0.89*** (0.11)
$\Delta DF \times (0, 1]$ Month Risk-Free Rate	0.34* (0.13)	0.78*** (0.25)	1.46*** (0.21)	1.10*** (0.05)	1.53*** (0.07)	2.23*** (0.21)	-0.77*** (0.14)	-0.75*** (0.27)	-0.77*** (0.28)
$\Delta DF \times (1, 3]$ Month Risk-Free Rate	0.19 (0.13)	0.64* (0.32)	1.15*** (0.21)	0.77*** (0.05)	1.21*** (0.06)	1.79*** (0.16)	-0.58*** (0.12)	-0.57 (0.35)	-0.63* (0.30)
$\Delta DF \times (3, 6]$ Month Risk-Free Rate	0.03 (0.11)	0.37** (0.14)	0.90*** (0.13)	0.62*** (0.04)	1.01*** (0.03)	1.54*** (0.09)	-0.59*** (0.12)	-0.64*** (0.15)	-0.64*** (0.16)
$\Delta DF \times (6, 12]$ Month Risk-Free Rate	0.22* (0.10)	0.36 (0.20)	0.57** (0.21)	0.56*** (0.05)	0.90*** (0.04)	1.31*** (0.10)	-0.34*** (0.09)	-0.55** (0.20)	-0.75*** (0.22)
$\Delta DF \times (1, 2]$ Year Risk-Free Rate	0.41* (0.16)	0.12 (0.17)	0.53* (0.22)	0.66*** (0.03)	0.87*** (0.05)	1.19*** (0.11)	-0.26 (0.16)	-0.75*** (0.15)	-0.66*** (0.17)
$\Delta DF \times (2, 5]$ Year Risk-Free Rate	-0.09 (0.11)	-0.15 (0.18)	0.22 (0.12)	0.19*** (0.02)	0.08** (0.03)	0.46*** (0.04)	-0.28* (0.12)	-0.23 (0.19)	-0.24* (0.12)
$\Delta DF \times$ Adjustable Rate	0.05 (0.11)	-0.32 (0.25)	-0.32 (0.21)	-0.08 (0.05)	-0.02 (0.06)	0.04 (0.15)	0.13 (0.10)	-0.30 (0.27)	-0.35 (0.29)
$\Delta DF \times$ Adjustment More Frequent Than Reference Rate Maturity	-0.03 (0.10)	0.10 (0.13)	0.01 (0.25)	-0.06 (0.04)	-0.02 (0.05)	-0.11 (0.16)	0.03 (0.08)	0.11 (0.13)	0.12 (0.20)
$\Delta DF \times$ Adjustment Less Frequent Than Reference Rate Maturity	-0.04 (0.21)	-0.08 (0.26)	0.30 (0.35)	0.06 (0.07)	0.04 (0.08)	0.20 (0.22)	-0.10 (0.21)	-0.12 (0.29)	0.10 (0.27)
$\Delta DF \times$ Non-EUR Reference Rate	-0.11 (0.07)	0.13 (0.11)	-0.07 (0.30)	0.16* (0.07)	0.18 (0.10)	0.23 (0.35)	-0.28** (0.10)	-0.05 (0.09)	-0.30* (0.13)
$\Delta DF \times$ Other Single Reference Rate	-0.10 (0.10)	-0.15 (0.16)	-0.47* (0.23)	-0.06 (0.06)	-0.05 (0.10)	-0.17 (0.25)	-0.04 (0.11)	-0.10 (0.20)	-0.30 (0.24)
$\Delta DF \times$ Multiple Reference Rates	-0.55 (0.41)	-0.82* (0.36)	-0.63 (0.61)	0.04 (0.09)	0.04 (0.08)	-0.24 (0.41)	-0.59 (0.47)	-0.86* (0.36)	-0.39 (0.35)
$\Delta DF \times$ Cross-Border Loans	0.23 (0.16)	0.14 (0.19)	-0.18 (0.26)	0.05 (0.03)	0.03 (0.03)	0.02 (0.10)	0.18 (0.16)	0.11 (0.20)	-0.21 (0.25)
Changes in 6M OIS Rate Before GovC Meeting	0.28*** (0.05)	0.56*** (0.15)	0.92*** (0.16)	0.56*** (0.03)	1.07*** (0.02)	1.43*** (0.02)	-0.28*** (0.06)	-0.51*** (0.15)	-0.50** (0.16)
Probability of Default (Imputed if Missing)	8.22*** (0.79)	8.12*** (0.74)	7.96*** (0.74)	0.01 (0.05)	-0.08 (0.06)	-0.15 (0.09)	8.21*** (0.80)	8.20*** (0.76)	8.11*** (0.76)

(continued)

Table A.11. Regression Results for the Pass-Through of Monetary Policy Rates to Lending Rates, Relevant Risk-Free Rate, and Premium, Three-Year Adjustment for Fixed-Rate Loans

Variables	Total Rate			Relevant Risk-Free Rate			Premium		
	$h = 1$	$h = 2$	$h = 3$	$h = 1$	$h = 2$	$h = 3$	$h = 1$	$h = 2$	$h = 3$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Change in DF Rate (ΔDF Instrumented by Target and Timing Surprises)	0.15 (0.13)	0.82** (0.31)	0.76*** (0.23)	-0.03 (0.05)	-0.08 (0.05)	-0.16* (0.07)	0.18 (0.13)	0.90** (0.32)	0.92*** (0.26)
$\Delta DFF \times (0, 1]$ Month Risk-Free Rate	0.37*** (0.11)	0.48** (0.18)	1.04*** (0.13)	0.89*** (0.03)	1.42*** (0.04)	1.76*** (0.05)	-0.52*** (0.12)	-0.95*** (0.18)	-0.73*** (0.15)
$\Delta DFF \times (1, 3]$ Month Risk-Free Rate	0.23* (0.10)	0.37 (0.23)	0.89*** (0.17)	0.57*** (0.03)	1.10*** (0.03)	1.41*** (0.05)	-0.35** (0.11)	-0.73** (0.23)	-0.59** (0.20)
$\Delta DFF \times (3, 6]$ Month Risk-Free Rate	0.08 (0.13)	0.15 (0.22)	0.57*** (0.17)	0.44*** (0.04)	0.94*** (0.04)	1.15*** (0.06)	-0.37** (0.13)	-0.80*** (0.22)	-0.58** (0.19)
$\Delta DFF \times (6, 12]$ Month Risk-Free Rate	0.25* (0.13)	0.08 (0.22)	0.18 (0.17)	0.35*** (0.07)	0.76*** (0.08)	0.84*** (0.07)	-0.10 (0.11)	-0.67** (0.22)	-0.66*** (0.19)
$\Delta DFF \times (1, 2]$ Year Risk-Free Rate	0.42* (0.19)	-0.10 (0.28)	0.24 (0.24)	0.38*** (0.05)	0.71*** (0.05)	0.77*** (0.11)	0.04 (0.20)	-0.80** (0.30)	-0.54* (0.25)
$\Delta DFF \times (2, 5]$ Year Risk-Free Rate	—	—	—	—	—	—	—	—	—
$\Delta DFF \times$ Floating Rate	0.00 (0.10)	-0.29 (0.23)	-0.35 (0.20)	-0.11* (0.04)	-0.10* (0.04)	-0.09 (0.08)	0.11 (0.09)	-0.19 (0.25)	-0.27 (0.24)
$\Delta DFF \times$ Adjustment More Frequent Than Reference Rate Maturity	0.03 (0.09)	0.10 (0.14)	-0.01 (0.18)	0.04 (0.03)	0.08* (0.03)	-0.08 (0.08)	-0.01 (0.08)	0.02 (0.14)	0.08 (0.22)
$\Delta DFF \times$ Adjustment Less Frequent Than Reference Rate Maturity	-0.07 (0.21)	-0.08 (0.26)	0.23 (0.28)	0.02 (0.05)	0.04 (0.07)	0.10 (0.11)	-0.09 (0.20)	-0.12 (0.28)	0.13 (0.27)
$\Delta DFF \times$ Non-EUR Reference Rate	-0.15* (0.06)	0.08 (0.09)	-0.11 (0.10)	0.11 (0.06)	0.09 (0.06)	0.16** (0.06)	-0.26** (0.09)	-0.01 (0.08)	-0.27* (0.11)
$\Delta DFF \times$ Other Single Reference Rate	-0.08 (0.10)	-0.16 (0.16)	-0.37 (0.20)	-0.02 (0.06)	-0.04 (0.07)	-0.01 (0.09)	-0.06 (0.11)	-0.12 (0.19)	-0.36 (0.21)
$\Delta DFF \times$ Multiple Reference Rates	-0.42 (0.46)	-0.61 (0.37)	-0.20 (0.33)	0.12 (0.07)	0.47* (0.19)	0.37** (0.13)	-0.53 (0.46)	-1.09** (0.35)	-0.57 (0.42)
$\Delta DFF \times$ Cross-Border Loans	0.23 (0.16)	0.15 (0.20)	-0.11 (0.23)	0.06 (0.03)	0.08* (0.04)	0.12 (0.07)	0.17 (0.17)	0.07 (0.20)	-0.24 (0.26)
Probability of Default (Imputed if Missing)	8.22*** (0.80)	8.12*** (0.74)	7.94*** (0.74)	0.02 (0.04)	-0.07 (0.06)	-0.14 (0.09)	8.19*** (0.80)	8.19*** (0.75)	8.08*** (0.76)

(continued)

Table A.12. Regression Results for the Pass-Through of Monetary Policy Rates to Lending Rates, Relevant Risk-Free Rate, and Premium, Using Non-imputed PD Observations Only

Variables	Total Rate			Relevant Risk-Free Rate			Premium		
	$h = 1$	$h = 2$	$h = 3$	$h = 1$	$h = 2$	$h = 3$	$h = 1$	$h = 2$	$h = 3$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Change in DF Rate (ΔDF Instrumented by Target and Timing Surprises)	0.22* (0.09)	0.71*** (0.16)	0.45*** (0.12)	-0.20*** (0.02)	-0.16*** (0.02)	-0.66*** (0.03)	0.41*** (0.10)	0.87*** (0.16)	1.11*** (0.12)
$\Delta DF \times [0, 1]$ Month Risk-Free Rate	0.24 (0.15)	0.36 (0.21)	1.22*** (0.19)	1.09*** (0.05)	1.58*** (0.06)	2.31*** (0.09)	-0.86*** (0.16)	-1.22*** (0.22)	-1.10*** (0.23)
$\Delta DF \times [1, 3]$ Month Risk-Free Rate	0.25* (0.12)	0.18 (0.21)	0.94*** (0.14)	0.77*** (0.04)	1.24*** (0.05)	1.99*** (0.05)	-0.52*** (0.13)	-1.06*** (0.20)	-1.05*** (0.15)
$\Delta DF \times [3, 6]$ Month Risk-Free Rate	0.01 (0.12)	0.20 (0.16)	0.89*** (0.15)	0.65*** (0.04)	1.07*** (0.04)	1.68*** (0.06)	-0.64*** (0.12)	-0.87*** (0.16)	-0.79*** (0.16)
$\Delta DF \times [6, 12]$ Month Risk-Free Rate	0.16 (0.11)	0.04 (0.17)	0.42* (0.18)	0.53*** (0.05)	0.89*** (0.05)	1.41*** (0.06)	-0.37*** (0.10)	-0.85*** (0.17)	-0.99*** (0.20)
$\Delta DF \times [1, 2]$ Year Risk-Free Rate	0.22 (0.20)	0.17 (0.17)	0.70** (0.21)	0.58*** (0.05)	0.86*** (0.05)	1.34*** (0.09)	-0.37 (0.21)	-0.69*** (0.18)	-0.64*** (0.19)
$\Delta DF \times [2, 5]$ Year Risk-Free Rate	-0.04 (0.14)	-0.38 (0.30)	0.19 (0.15)	0.14*** (0.04)	0.03 (0.04)	0.55*** (0.06)	-0.19 (0.15)	-0.41 (0.30)	-0.36* (0.17)
$\Delta DF \times$ Floating Rate	-0.04 (0.10)	-0.13 (0.20)	-0.26 (0.15)	-0.15** (0.05)	-0.19** (0.06)	-0.15 (0.10)	0.11 (0.10)	0.05 (0.17)	-0.11 (0.22)
$\Delta DF \times$ Adjustment More Frequent Than Reference Rate Maturity	-0.02 (0.16)	0.15 (0.17)	0.18 (0.15)	0.00 (0.05)	0.06 (0.04)	-0.08 (0.08)	-0.02 (0.15)	0.14 (0.18)	0.26 (0.19)
$\Delta DF \times$ Adjustment Less Frequent Than Reference Rate Maturity	-0.40 (0.37)	-0.27 (0.42)	-0.08 (0.43)	0.01 (0.07)	-0.06 (0.07)	0.07 (0.19)	-0.41 (0.35)	-0.21 (0.43)	-0.15 (0.37)
$\Delta DF \times$ Non-EUR Reference Rate	-0.11 (0.09)	0.15 (0.11)	-0.11 (0.13)	0.12* (0.06)	0.12* (0.06)	0.17** (0.06)	-0.23** (0.08)	0.03 (0.09)	-0.28* (0.12)
$\Delta DF \times$ Other Single Reference Rate	0.11 (0.12)	-0.18 (0.13)	-0.32 (0.23)	-0.01 (0.07)	0.04 (0.08)	-0.03 (0.09)	0.12 (0.12)	-0.22 (0.18)	-0.30 (0.25)
$\Delta DF \times$ Multiple Reference Rates	-0.36 (0.49)	-0.68* (0.34)	-0.18 (0.38)	0.25*** (0.06)	0.72*** (0.19)	0.57*** (0.14)	-0.62 (0.50)	-1.39*** (0.23)	-0.75 (0.46)
$\Delta DF \times$ Cross-Border Loans	0.44 (0.23)	0.44* (0.19)	0.29 (0.17)	0.02 (0.04)	0.04 (0.05)	0.12* (0.06)	0.42 (0.24)	0.40* (0.18)	0.17 (0.15)
Probability of Default	1.12*** (0.11)	1.08*** (0.11)	1.08*** (0.11)	0.02 (0.02)	0.03 (0.02)	0.02 (0.01)	1.10*** (0.11)	1.05*** (0.11)	1.06*** (0.11)

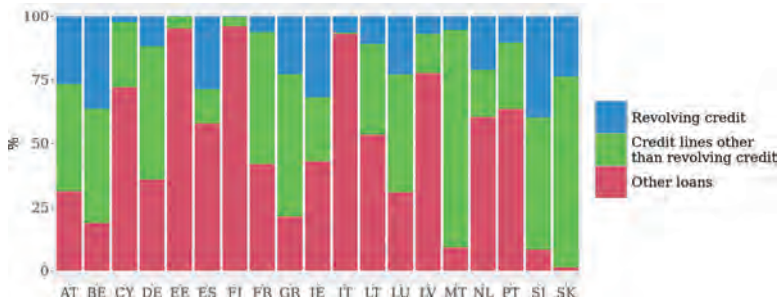
(continued)

Table A.12. (Continued)

Variables	Total Rate			Relevant Risk-Free Rate			Premium		
	$h = 1$	$h = 2$	$h = 3$	$h = 1$	$h = 2$	$h = 3$	$h = 1$	$h = 2$	$h = 3$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Fixed Effects:									
GovC Period (τ) $\times$ Creditor Country	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
GovC Period (τ) $\times$ Bank	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
GovC Period (τ) $\times$ Loan Type $\times$ Creditor Country	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
GovC Period (τ) $\times$ Debtor Sector $\times$ Creditor Country	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
GovC Period (τ) $\times$ Debtor Region	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
GovC Period (τ) $\times$ Firm Size Class $\times$ Creditor Country	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
GovC Period (τ) $\times$ Loan Maturity $\times$ Creditor Country	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
GovC Period (τ) $\times$ Collateral Size $\times$ Creditor Country	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
GovC Period (τ) $\times$ Cross-Border Loan $\times$ Creditor Country	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
GovC Period (τ) $\times$ Multidebtor Loan $\times$ Creditor Country	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
GovC Period (τ) $\times$ Reference Rate Type $\times$ Creditor Country	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
GovC Period (τ) $\times$ Reference Rate Maturity $\times$ Creditor Country	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
GovC Period (τ) $\times$ Reference Rate Adjustment Frequency $\times$ Creditor Country	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Observations	4,714,260	4,644,687	4,454,510	4,714,260	4,644,687	4,454,510	4,714,260	4,644,687	4,454,510
R^2	0.80	0.79	0.78	0.98	0.97	0.94	0.59	0.58	0.58
Within R^2	0.02	0.05	0.09	0.14	0.33	0.32	0.01	0.01	0.01
Standard Errors Clustered by	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank

Source: AnaCredit.
Note: *** denotes significance at the 99.9 percent level, ** at 99 percent, and * at 95 percent. Standard errors are reported in parentheses. The sample includes new loans to euro-area NFCs (excluding financial and insurance activities) issued in 2022 and 2023, denominated in EUR, exceeding EUR 25,000, with a positive interest rate. The sample consists of the following types of loans: credit lines other than revolving credits, revolving credits, other loans, excluding syndicated loans. For fixed-rate loans, the maturity of the relevant risk-free rate corresponds to the loan's maturity, while for floating-rate loans, it is equal to the maturity of the underlying reference rate. Horizon $h = 1$ corresponds to the period of 6 weeks, $h = 2$ to 7–12 weeks, and $h = 3$ to 13–18 weeks following the Governing Council meeting. Results are obtained by weighted TSLS, where weights are determined by the loan values. Changes in the deposit facility rate are instrumented by target and timing surprises. ΔDF denotes changes in DF rate. Other variables (except the probability of default) are the binary (dummy) variables that equal one if a loan contract has a corresponding feature.

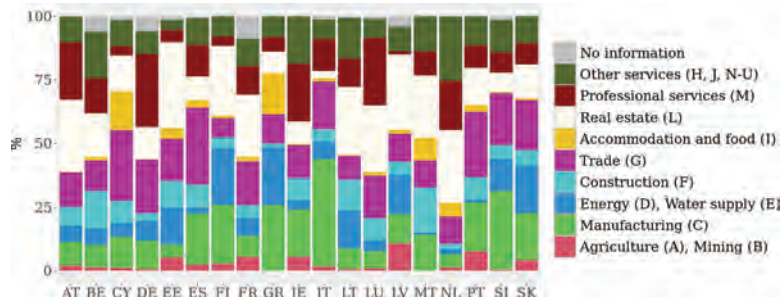
Figure A.1. Structure of Newly Issued Loans to NFCs in Euro-Area Countries by Loan Type, 2022–23



Source: AnaCredit.

Note: New loans to euro-area NFCs (excluding financial and insurance activities) issued in 2022–23, denominated in EUR, exceeding EUR 25,000, with a positive interest rate. The sample consists of the following types of loans: credit lines other than revolving credits, revolving credits, other loans, excluding syndicated loans. Shares are value-weighted. The horizontal axis represents the country of residence of the bank. The results for several countries were subject to additional randomization in order to avoid the disclosure of confidential information. In such cases, the figure does not report precise results, though the randomization procedure preserves the overall trends (see footnote 17).

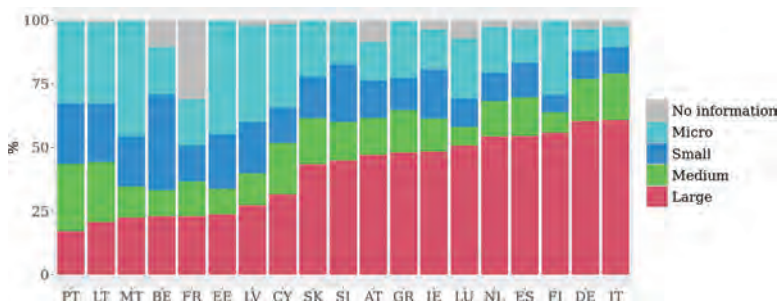
Figure A.2. Structure of Newly Issued Loans to NFCs in Euro-Area Countries by Borrower’s Macroeconomic Sector, 2022–23



Source: AnaCredit.

Note: New loans to euro-area NFCs (excluding financial and insurance activities) issued in 2022–23, denominated in EUR, exceeding EUR 25,000, with a positive interest rate. The sample consists of the following types of loans: credit lines other than revolving credits, revolving credits, other loans, excluding syndicated loans. Shares are value-weighted. The horizontal axis represents the country of residence of the bank. The results for several countries were subject to additional randomization in order to avoid the disclosure of confidential information. In such cases, the figure does not report precise results, though the randomization procedure preserves the overall trends (see footnote 17).

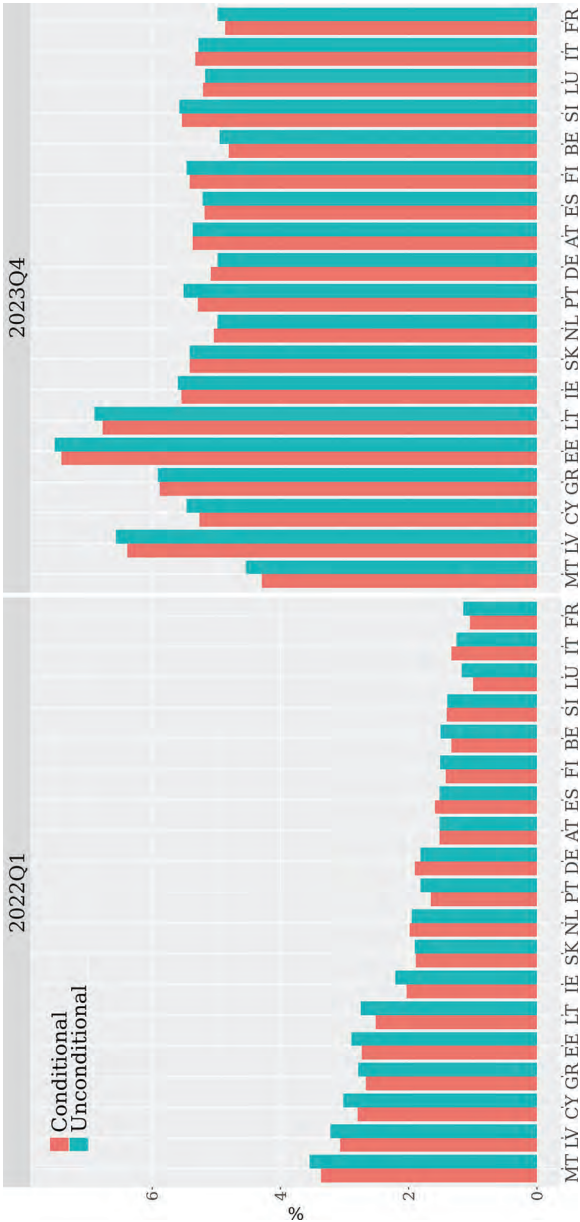
Figure A.3. Structure of Newly Issued Loans to NFCs in Euro-Area Countries by Borrower’s Size Class, 2022–23



Source: AnaCredit.

Note: New loans to euro-area NFCs (excluding financial and insurance activities) issued in 2022–23, denominated in EUR, exceeding EUR 25,000, with a positive interest rate. The sample consists of the following types of loans: credit lines other than revolving credits, revolving credits, other loans, excluding syndicated loans. Shares are value-weighted. Countries are arranged according to the share of loans to large enterprises. The horizontal axis represents the country of residence of the bank. The results for several countries were subject to additional randomization in order to avoid the disclosure of confidential information. In such cases, the figure does not report precise results, though the randomization procedure preserves the overall trends (see footnote 17).

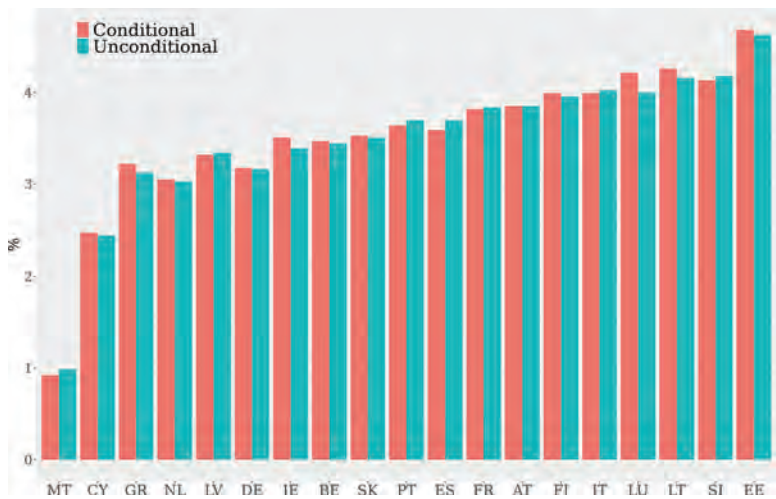
Figure A.4. Unconditional and Conditional Interest Rates of Newly Issued Loans to NFCs, 2022:Q1 and 2023:Q4



Source: AnaCredit.

Note: New loans to euro-area NFCs (excluding financial and insurance activities) issued in 2022:Q1 and 2023:Q4, denominated in EUR, exceeding EUR 25,000, with positive a interest rate. The sample consists of the following types of loans: credit lines other than revolving credits, revolving credits, other loans, excluding syndicated loans. The unconditional bar represents the unconditional weighted average, while the conditional bar depicts the weighted average controlling for the firm size category, macroeconomic sector, maturity category, loan type, and cross-border loan dummies (see footnote 15). The horizontal axis represents the country of residence of the bank. Countries are arranged according to the unconditional weighted average level of interest rates in 2022:Q1.

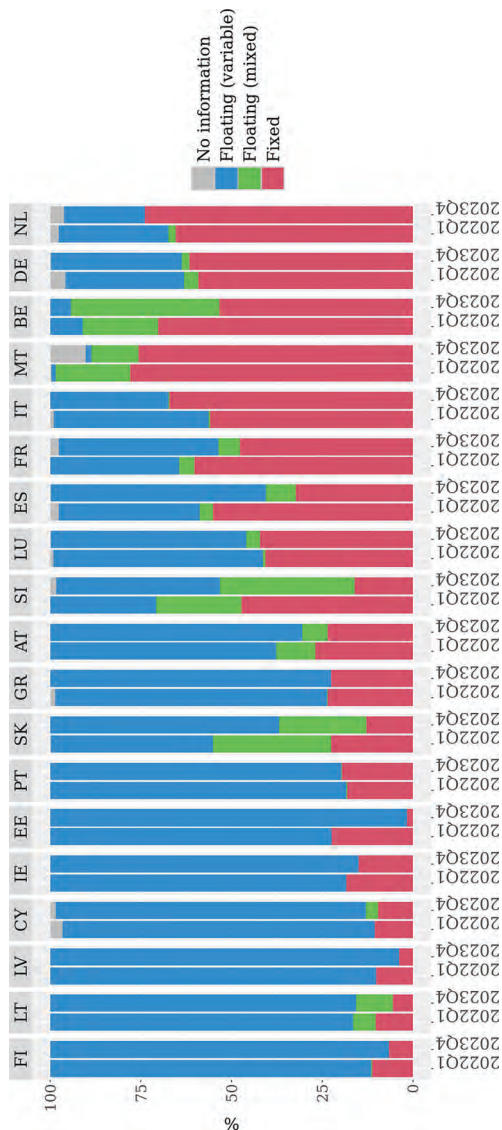
Figure A.5. Changes in Unconditional and Conditional Mean Interest Rates of Newly Issued Loans to NFCs in Euro-Area Countries between 2022:Q1 and 2023:Q4



Source: AnaCredit.

Note: New loans to euro-area NFCs (excluding financial and insurance activities) issued in 2022:Q1 and 2023:Q4, denominated in EUR, exceeding EUR 25,000, with a positive interest rate. The sample consists of the following types of loans: credit lines other than revolving credits, revolving credits, other loans, excluding syndicated loans. The unconditional bar represents the unconditional weighted average, while the conditional bar depicts the weighted average controlling for the firm size category, macroeconomic sector, maturity category, loan type, and cross-border loan dummies (estimated for 2022:Q1 and 2023:Q4 separately; see footnote 15). The horizontal axis represents the country of residence of the bank. Countries are arranged according to the change in unconditional weighted mean interest rate in 2023:Q4 compared with 2022:Q1.

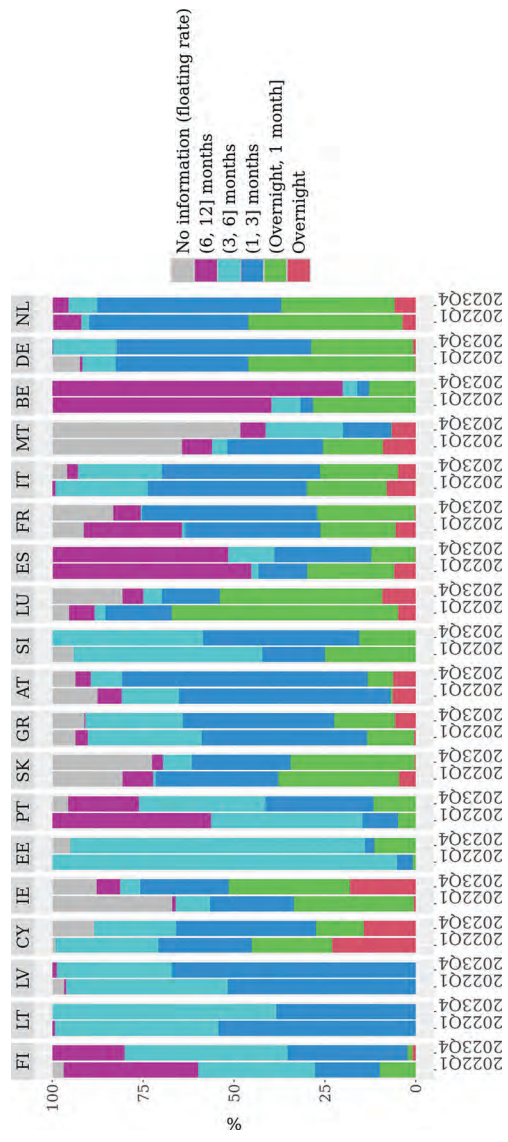
Figure A.6. Structure of Newly Issued Loans to NFCs in Euro-Area Countries by Rate-Type Category, 2022:Q1 and 2023:Q4



Source: AnaCredit.

Note: New loans to euro-area NFCs (excluding financial and insurance activities) issued in 2022:Q1 and 2023:Q4, denominated in EUR, exceeding EUR 25,000, with a positive interest rate. The sample consists of the following types of loans: credit lines other than revolving credits, revolving credits, other loans, excluding syndicated loans. Fixed-rate loans have interest rates that remain constant over the entire duration of the loan. Floating-rate loans have interest rates that adjust over time. Shares are value-weighted. The horizontal axis represents the country of residence of the bank. Countries are arranged according to the share of new loans with fixed interest rates in 2022-23. The results for several countries were subject to additional randomization in order to avoid the disclosure of confidential information. In such cases, the figure does not report precise results, though the randomization procedure preserves the overall trends (see footnote 17).

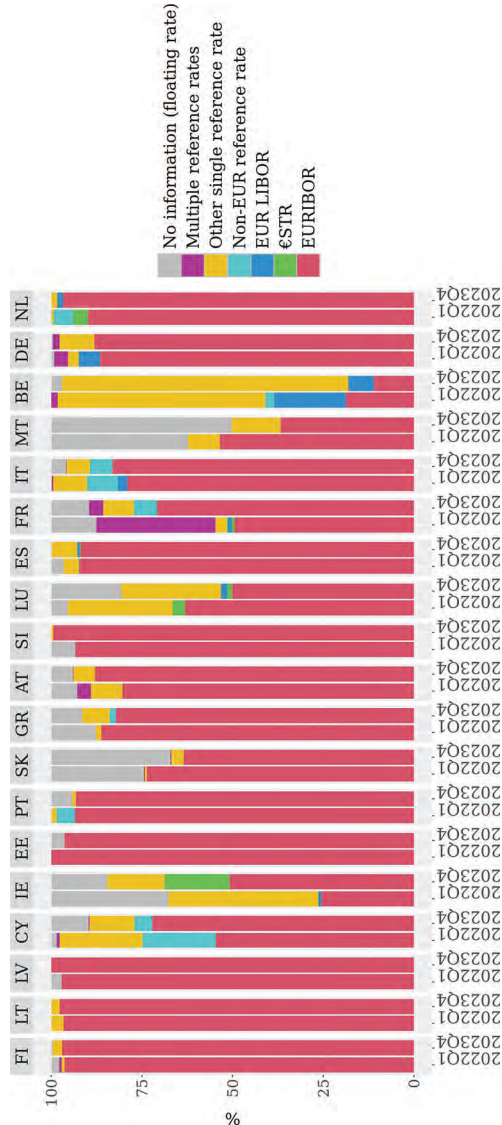
Figure A.7. Structure of Newly Issued Floating-Rate Loans to NFCs in Euro-Area Countries by Reference Rate Maturity, 2022:Q1 and 2023:Q4



Source: AnaCredit.

Note: New loans to euro-area NFCs (excluding financial and insurance activities) issued in 2022:Q1 and 2023:Q4, denominated in EUR, exceeding EUR 25,000, with a positive interest rate. The sample consists of the following types of loans: credit lines other than revolving credits, revolving credits, other loans, excluding syndicated loans. Fixed-rate loans have interest rates that remain constant over the entire duration of the loan. Floating-rate loans have interest rates that adjust over time. Shares are value-weighted. The horizontal axis represents the country of residence of the bank. Countries are arranged according to the share of new loans with fixed interest rates in 2022–23. The results for several countries were subject to additional randomization in order to avoid the disclosure of confidential information. In such cases, the figure does not report precise results, though the randomization procedure preserves the overall trends (see footnote 17).

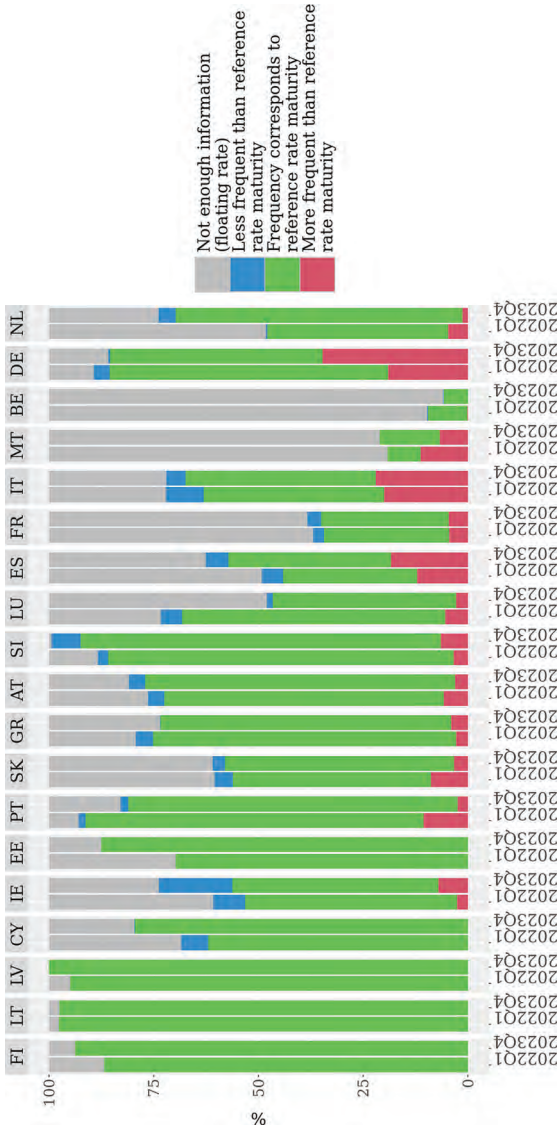
Figure A.8. Structure of Newly Issued Floating-Rate Loans to NFCs in Euro-Area Countries by Reference Rate, 2022:Q1 and 2023:Q4



Source: AnaCredit.

Note: New loans to euro-area NFCs (excluding financial and insurance activities) issued in 2022:Q1 and 2023:Q4, denominated in EUR, exceeding EUR 25,000, with a positive interest rate. The sample consists of the following types of loans: credit lines other than revolving credits, revolving credits, other loans, excluding syndicated loans. Fixed-rate loans have interest rates that remain constant over the entire duration of the loan. Floating-rate loans have interest rates that adjust over time. Shares are value-weighted. The horizontal axis represents the country of residence of the bank. Countries are arranged according to the share of new loans with fixed interest rates in 2022–23. The results for several countries were subject to additional randomization in order to avoid the disclosure of confidential information. In such cases, the figure does not report precise results, though the randomization procedure preserves the overall trends (see footnote 17).

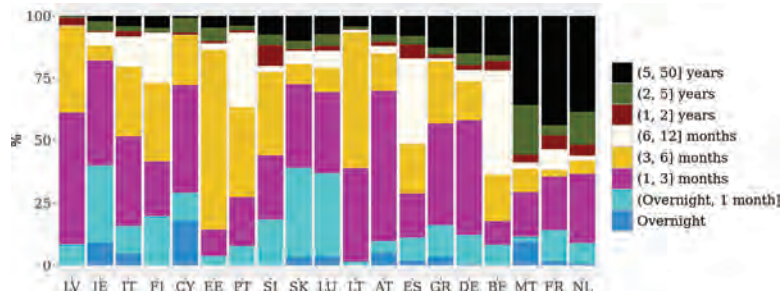
Figure A.9. Structure of Newly Issued Floating-Rate Loans to NFCs in Euro-Area Countries by Correspondence between Frequency of Rate Adjustment and Reference Rate Maturity, 2022:Q1 and 2023:Q4



Source: AnaCredit.

Note: New loans to euro-area NFCs (excluding financial and insurance activities) issued in 2022–23, denominated in EUR, exceeding EUR 25,000, with a positive interest rate. The sample consists of the following types of loans: credit lines other than revolving credits, revolving credits, other loans, excluding syndicated loans. Fixed-rate loans have interest rates that remain constant over the entire duration of the loan. Floating-rate loans have interest rates that adjust over time. Shares are value-weighted. The horizontal axis represents the country of residence of the bank. Countries are arranged according to the share of new loans with fixed interest rates in 2022–23. The results for several countries were subject to additional randomization in order to avoid the disclosure of confidential information. In such cases, the figure does not report precise results, though the randomization procedure preserves the overall trends (see footnote 17).

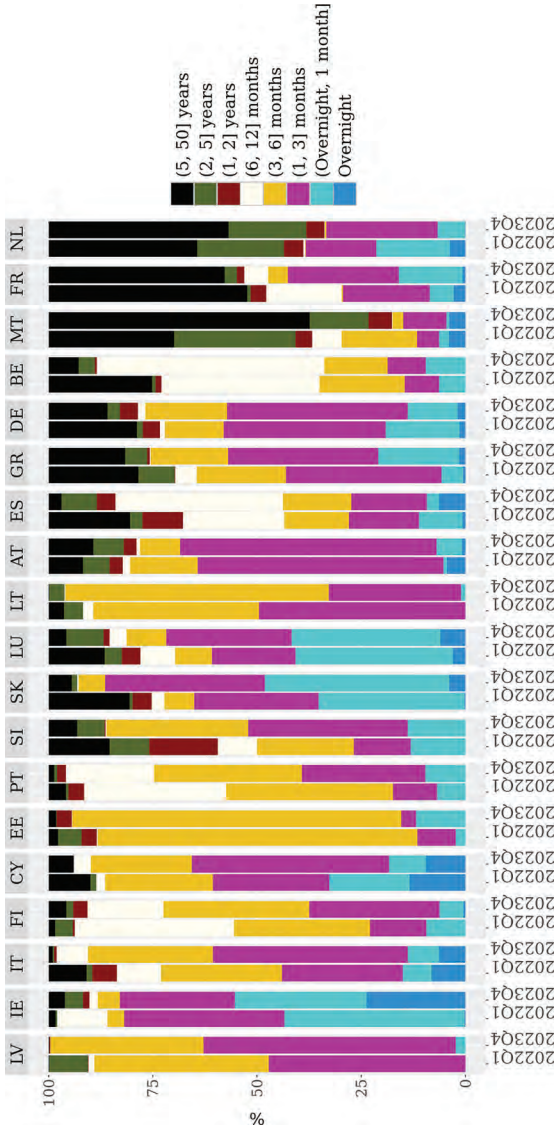
Figure A.10. Structure of Newly Issued Loans to NFCs by Maturity of the Relevant Risk-Free Rate in Euro-Area Countries, 2022–23



Source: AnaCredit.

Note: New loans to euro-area NFCs (excluding financial and insurance activities) issued in 2022–23, denominated in EUR, exceeding EUR 25,000, with a positive interest rate. The sample consists of the following types of loans: credit lines other than revolving credits, revolving credits, other loans, excluding syndicated loans. For fixed-rate loans, the maturity of the relevant risk-free rate corresponds to the loan’s maturity, while for floating-rate loans, it is equal to the maturity of the underlying reference rate. Shares are value-weighted. The horizontal axis represents the country of residence of the bank. Countries are arranged according to the weighted average maturity of the relevant risk-free rate. The results for several countries were subject to additional randomization in order to avoid the disclosure of confidential information. In such cases, the figure does not report precise results, though the randomization procedure preserves the overall trends (see footnote 17).

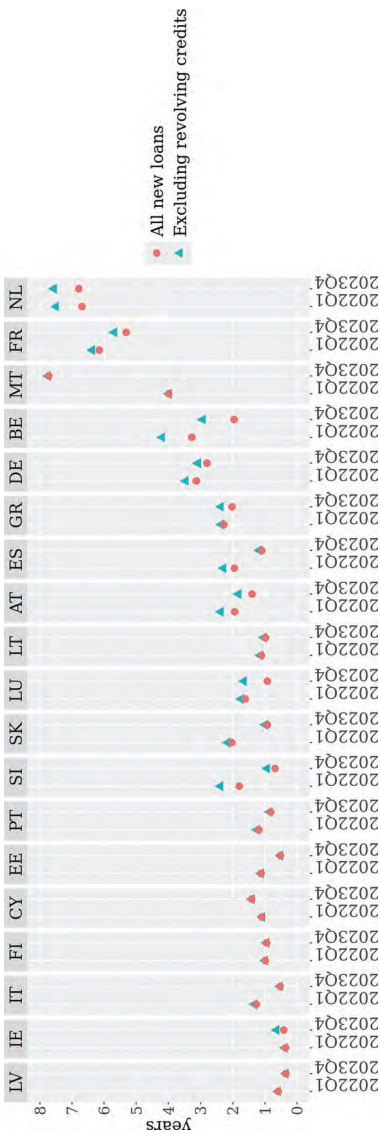
Figure A.11. Structure of Newly Issued Loans to NFCs by Maturity of the Relevant Risk-Free Rate in Euro-Area Countries, 2022:Q1 and 2023:Q4



Source: AnaCredit.

Note: New loans to euro-area NFCs (excluding financial and insurance activities) issued in 2022:Q1 and 2023:Q4, denominated in EUR, exceeding EUR 25,000, with a positive interest rate. The sample consists of the following types of loans: credit lines other than revolving credits, revolving credits, other loans, excluding syndicated loans. For fixed-rate loans, the maturity of the relevant risk-free rate corresponds to the loan's maturity, while for floating-rate loans, it is equal to the maturity of the underlying reference rate. Shares are value-weighted. The horizontal axis represents the country of residence of the bank. Countries are arranged according to the weighted average maturity of the relevant risk-free rate. The results for several countries were subject to additional randomization in order to avoid the disclosure of confidential information. In such cases, the figure does not report precise results, though the randomization procedure preserves the overall trends (see footnote 17).

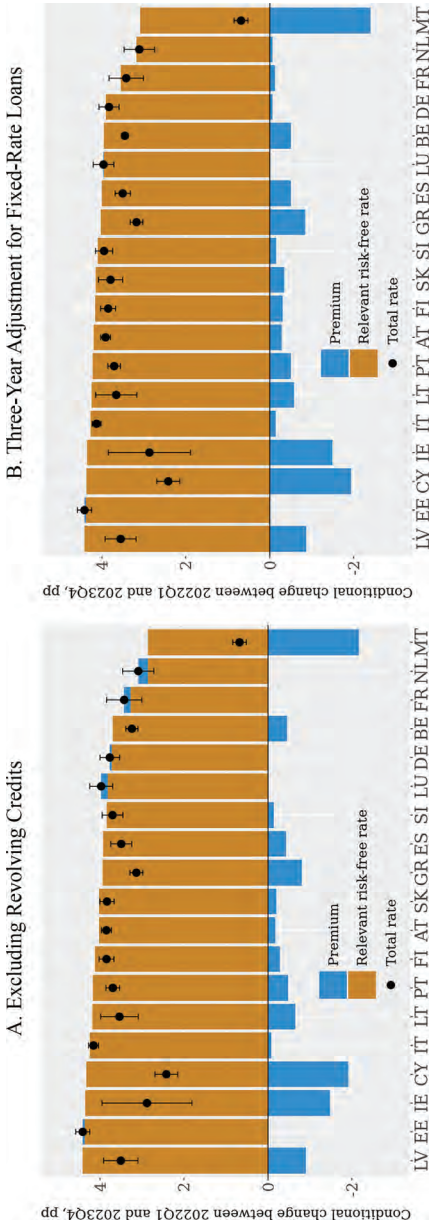
Figure A.12. Weighted Average Maturity of the Relevant Risk-Free Rate for Newly Issued Loans to NFCs in Euro-Area Countries, 2022:Q1 and 2023:Q4



Source: AnaCredit.

Note: New loans to euro-area NFCs (excluding financial and insurance activities) issued in 2022:Q1 and 2023:Q4, denominated in EUR, exceeding EUR 25,000, with a positive interest rate. The sample consists of the following types of loans: credit lines other than revolving credits, revolving credits, other loans, excluding syndicated loans. For fixed-rate loans, the maturity of the relevant risk-free rate corresponds to the loan's maturity, while for floating-rate loans, it is equal to the maturity of the underlying reference rate. The horizontal axis represents the country of residence of the bank. Countries are arranged according to the weighted average maturity of the relevant risk-free rate in 2022–23.

Figure A.13. Decomposition of Conditional Changes in the Rates of Newly Issued Loans to NFCs between 2022:Q1 and 2023:Q4, Robustness Checks



Source: AnaCredit.

Note: Points depict the estimated country-specific coefficient $\beta_{1,c}^{loan}$ from Equation (1) that shows the difference in interest rates of new loans issued before and after the monetary tightening and the 95 percent confidence bands. Brown bars show the contribution of changes in the risk-free rate $\beta_{1,c}^{risk-free}$, and blue bars show the contribution of changes in the premium $\beta_{1,c}^{premium}$. The sample includes new loans to euro-area NFCs (excluding financial and insurance activities) issued in 2022:Q1 and 2023:Q4, denominated in EUR, exceeding EUR 25,000, with a positive interest rate. The sample consists of the following types of loans: credit lines other than revolving credits, revolving credits, other loans, excluding syndicated loans. For fixed-rate loans, the maturity of the relevant risk-free rate corresponds to the loan's maturity, while for floating-rate loans, it is equal to the maturity of the underlying reference rate. The horizontal axis represents the country of residence of the bank. Countries are arranged according to the contribution of the relevant risk-free rate.

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Household Borrowing and Monetary Policy Transmission: Post-Pandemic Insights from Nine European Credit Registers*

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We study heterogeneity in households' credit across nine European countries (Belgium, Spain, Hungary, Ireland, Italy, Latvia, Lithuania, Portugal, and Slovakia) during 2022–24 using granular credit register data. We first document substantial between- and within-country variation in mortgage and consumer lending by borrower age, loan maturity, and interest rate fixation. We then quantify the pass-through of the ECB's recent tightening cycle to household borrowing costs, and assess its heterogeneous impact across households. Pass-through is nearly complete for mortgages (around 0.9) but considerably weaker for consumer credit (around 0.4). While mortgage pass-through is relatively homogeneous across countries, consumer credit shows pronounced cross-country differences that cannot be explained by borrower or loan characteristics. Younger households face stronger mortgage pass-through but weaker consumer credit pass-through relative to older borrowers, and longer maturities are associated with stronger pass-through in both credit markets.

JEL Codes: E52, G21, D14.

1. Introduction

Monetary policy shapes the consumption and investment decisions of households. An important part of the transmission mechanism is intermediated through the financial system, with borrowing decisions being altered by contractionary and easing cycles, which directly affect the cost of credit (Bernanke and Gertler 1995; Kashyap and Stein 2000). In response to a monetary tightening, households can decide to borrow less (intensive margin) or not borrow at all (extensive margin) (Mian, Rao, and Sufi 2013). However, some households may well be unresponsive to changes in the cost of credit, especially if they are among the very wealthy or very poor (Cloyne, Ferreira, and Surico 2020). Most of the evidence on the transmission of monetary policy through mortgages and consumer loans to households is based on evidence from aggregate data or single countries, notably the U.S. or the U.K.¹ Country-specific

¹For instance, see Benetton, Gavazza, and Surico (2025), Beraja et al. (2018), Berger et al. (2021), Beyer et al. (2024), Bosshardt et al. (2024), Drechsler, Savov,

studies are naturally bounded by the institutional setting that shapes the credit and housing markets.

In this paper, we undertake the unique task of collecting and jointly analyzing granular loan-level data for households in nine European countries to establish novel facts on the transmission of monetary policy to households through bank loans across different markets. By pulling together such granular data on household and consumer loans across countries in a single study, we are able to go beyond borders that may constrain the understanding of the transmission mechanism and explore common features and notable differences. The evidence that we collect shows that mortgage and consumer loan markets are highly heterogeneous, even across neighboring euro-area countries. We explore within- and across-country variation to establish how prices and quantities change in credit markets during a period that contains the steepest and fastest tightening cycle in the history of the European monetary union.

The data set includes household loans granted in eight countries in the euro area: Belgium, Ireland, Italy, Latvia, Lithuania, Portugal, Slovakia, and Spain; and one non-euro-area country, Hungary. We cover a 36-month period from January 2022 to December 2024. This sample period spans the entire post-pandemic tightening episode in the euro area, as well as the first months of easing. After a prolonged period of historically low interest rates, the European Central Bank (ECB) increased interest rates for the first time in July 2022, in response to steeply increasing inflation. Interest rates increased by 4.5 percentage points between that month and September 2023. After several months of plateauing monetary restriction, in June 2024 the ECB decreased rates. Our analysis focuses on the new loan contracts that originated within this period.

Unlike for firms, there is no harmonized euro-area credit register for households. Instead, we rely on national household credit registers from nine countries and harmonize them to enable cross-country comparison. Because of strict legal and privacy restrictions, loan-level data cannot be shared across borders. We therefore work with harmonized aggregates that preserve privacy but still allow us to document cross-country heterogeneity and study monetary policy

and Schnabl (2022), Emiriz and Koulischer (2023), Flodén et al. (2020), Indarte (2023), Liebersohn and Rothstein (2025), Pietruni and Signoretti (2017).

transmission in detail. This approach ensures robust comparability across countries while providing more granularity than existing euro-area data sets such as the individual Balance Sheet Items (iBSI) or MFI interest rate statistics (iMIR). The construction of the data set and the standardized aggregation procedure are described in detail in Sections 2 and 3.

The descriptive evidence highlights substantial heterogeneity in household credit markets across Europe in the post-pandemic period. Interest rate dispersion is far more pronounced in consumer credit, where cross-country differences can reach several percentage points, whereas mortgage rates are more tightly clustered. Loan amounts also vary markedly, with consumer credit showing greater relative dispersion than mortgage loans, consistent with the more diverse purposes and shorter horizons of these products. In addition, the number of new loan contracts differs strongly across borrower groups and loan characteristics, underscoring the diversity of household borrowing patterns.

We document systematic differences in credit allocation by borrower age. Mortgages are overwhelmingly concentrated among households aged 25–44, in line with life-cycle patterns around home purchases, while consumer credit has a more even age distribution and plays a greater role among mid- to late-career households. Young borrowers under 25 generally have limited access to credit, though in Latvia, Lithuania, and Slovakia, they account for a larger share of consumer loans.

There is also pronounced variation in loan maturities and interest rate fixation structures. For mortgages, Belgium, Hungary, Italy, and Latvia show a greater share of contracts with maturities below 20 years, while others make extensive use of very long-term contracts, such as Portugal, where loans exceeding 30 years are common. Consumer credit is typically shorter in duration, but longer-term loans such as auto credit dominate in value terms. Fixation practices differ just as sharply: Spain and Italy predominantly issue fixed-rate mortgages, whereas Portugal, Latvia, and Lithuania rely on variable-rate loans with short reset periods. These institutional differences directly shape the transmission of monetary policy.

Building on these facts, we estimate how policy rate changes feed into household borrowing costs for newly originated loans. We find that pass-through is nearly complete in mortgage markets (0.90), but

substantially weaker (0.36) and more fragmented in consumer credit. This is consistent with the findings surveyed in Gregor, Melecký, and Melecký (2021) as well as with a recent study by Beyer et al. (2024) on transmission in 30 European countries in the latest tightening cycle. We also explore heterogeneity in pass-through by interacting reference rates with borrower and contract characteristics. Transmission is stronger for younger borrowers in mortgages and for older borrowers in consumer loans. Short-maturity mortgages behave more like consumer loans, with muted sensitivity to policy changes, while long maturities and variable-rate contracts transmit policy more directly. Importantly, these results are robust across samples and show that country-level differences persist even after accounting for contract characteristics, with Ireland consistently exhibiting the lowest pass-through.

Importantly, we find that interest rate changes also affect the dispersion of borrowing costs: in mortgages, pricing remains uniform, but in consumer credit, rate increases widen the distribution of contract rates, suggesting strategic repricing by lenders. This shows that monetary policy affects not just average rates but also the distribution of financing costs across households.

Overall, our results show that monetary policy does not affect all households uniformly, but interacts with demographic factors, contract design, and national market structures. By harmonizing household credit register data across nine European countries, our study extends earlier work that relied on single-country or aggregate data and provides a unique cross-country perspective on the heterogeneous transmission of monetary policy to households. This literature has established that household heterogeneity—across income, age, mortgage structure, and credit constraints—plays a crucial role in how monetary shocks affect borrowing costs and, ultimately, spending (Beraja et al. 2018; Berger et al. 2021; Bhutta, Fuster, and Hizmo 2020; Charalambakis, Teppa, and Tsiortas 2024; Drechsler, Savov, and Schnabl 2022; Liaudinskas 2024; Liebersohn and Rothstein 2025).

The findings we present can be reconciled with several possible explanations, whose detailed investigation we leave for future work. In interpreting the results, we highlight what we view as the most plausible mechanisms, while acknowledging that other interpretations are possible. A further limitation concerns the external validity

of our evidence: we examine a very unique tightening cycle, which followed an equally unique long spell of very low interest rates. This context should be borne in mind when extrapolating the results to other settings.

Our paper adds to the literature on the effects of interest rate changes on mortgage flows and pricing, which mostly focuses on single-country settings. These include research on prepayment and path dependency in the U.S. mortgage market (Berger et al. 2021), on the distributional consequences of tightening U.S. policy on mortgage availability (Bosshardt et al. 2024), and on how interest rate pass-through varies by borrower and lender health (Indarte 2023). In Europe, evidence from Belgium (Emiris and Koulisher 2023), Italy (Pietrunti and Signoretti 2017), Sweden (Flodén et al. 2020), and the U.K. (Benetton, Gavazza, and Surico 2025; Bracke et al. 2024) discusses the roles of contractual design and financial frictions.

However, most of this literature relies on country-specific data. Our study, in contrast, seeks to integrate insights from a diverse set of countries within a harmonized methodological framework, exploiting highly granular cross-country data on new loan originations across mortgage and consumer credit markets.² Our paper complements and extends previous work studying mortgage market dynamics across countries that use more aggregated data. For instance, while Badarinza, Campbell, and Ramadorai (2018) show that the choice of rate fixation types depends on rate expectations, using data for nine countries, we show that even within a monetary union with common rate expectations there are persistent and sizable differences across countries, which have important implications on the magnitude and distribution of the effects of monetary policy decisions. By using borrower-level data instead of country-level information (as in Badarinza, Campbell, and Ramadorai 2018), we are able to shed light on the role of borrower and loan characteristics on borrowing decisions and the transmission of monetary policy. De Stefani and Mano (2025) focus more explicitly on the role of rate fixation in the transmission of monetary policy using quarterly country-level data on rate types for 35 countries over two

²When it comes to corporate credit, an expanding literature exploits contract-level data across countries—much of it enabled by AnaCredit in the euro area (see, e.g., Vilerts et al. 2026 and references therein).

decades. They find that monetary policy shapes choices on rate fixation for mortgages, which change the composition of the outstanding mortgage stock, thereby altering monetary policy transmission in the future. We extend this work by using granular loan-level data on mortgages (and consumer loans), showing that monetary policy decisions affect other borrowing choices, such as maturity and loan products, which also play a role in monetary policy transmission.³

2. Construction of the Data Set

A harmonized credit register for individuals or households is not available at the European level. Leveraging a collaboration among researchers from different central banks in Europe, within the context of the European System of Central Banks (ESCB) ChaMP Research Network,⁴ we construct a harmonized data set compliant with confidentiality and privacy legislation covering nine countries. In this section, we delineate the data commonly accessible and capable of being standardized across the participating countries.

2.1 *Sample Composition*

The sample consists of households in nine European countries, eight of which are in the euro area. The countries in the sample are Belgium (BE), Spain (ES), Hungary (HU), Ireland (IE), Italy (IT), Lithuania (LT), Latvia (LV), Portugal (PT), and Slovakia (SK). While in some countries the data span even more than two decades, in others the collection of data is more recent—starting in 2019 or later. The data used in this study span the period between January 2022 and December 2024 (June 2024 for Belgium and Lithuania). This sample period allows us to focus on the behavior of the

³In Appendix B we present an overview of selected recent household-credit-register-based studies from our sample of countries. This highlights the range of questions that household credit registers can address, but it is by no means exhaustive.

⁴The Challenges for Monetary Policy Transmission in a Changing World (ChaMP) Research Network aims to improve our understanding of how monetary policy transmits to the European economy amid unprecedented shocks, structural changes, and shifting inflation dynamics. More information on the network is available at <https://www.ecb.europa.eu/pub/research-networks/html/champ.en.html>.

household credit market in the post-pandemic period. Doing so, we avoid country-specific idiosyncrasies in pandemic-related credit market interventions as well as a potential impact of the phasing in of the credit registers. The sample period covers the steep monetary tightening episode with several interest rate hikes between July 27, 2022, and September 20, 2023. For the analysis in this paper, we are collapsing the data in six-month intervals. Our data collection focuses on the characteristics of all new contracts (flows) originated between January 2022 and December 2024.

2.2 Dimensions of the Data

Credit Types. We include two different loan types: mortgages and consumer credit.⁵ To get to comparable categories across countries, we make the following sampling choices. Concerning mortgages, we only include mortgages with collateral. The collateral or protection offered can be physical assets (typically the property) but also personal guarantees (typically senior relatives). We exclude mortgages whose purpose is not the purchase or construction of a physical property. Consumer credit includes both collateralized and uncollateralized loans.⁶ Leasing contracts are excluded from the consumer credit sample, as there is no automatic ownership transfer or obligation to purchase the leased good.

Borrowers. We only include loans to natural persons. We exclude loans that are given to individuals who are self-employed or sole proprietors. Loans can be given to a single individual, but also cosigning or codebtorship can take place (e.g., by a couple who cosigns a mortgage for financing a house purchase). The only characteristics of the borrower that are available in all of the countries in the sample are information on the age of the borrower and the location of residence. We group borrowers in six age groups, corresponding to different phases in the life cycle. The six bins are < 25

⁵In the data collection effort, we also collect information on revolving credit, which includes overdrafts and credit card debt. This third category is not described in the stylized facts nor included in the pass-through analysis due to its heterogeneity and rate stickiness. Across countries, this product category is characterized by a high number of observations but smaller loan amounts.

⁶In Slovakia, only collateralized consumer credit is available.

years, 25 to 34 years, 35 to 44 years, 45 to 54 years, 55 to 64 years, and ≥ 65 years; or missing if no information is provided.

Creditor Types. Various types of financial institutions participate in the reporting to the credit registers. In addition to credit institutions, these can also be installment sellers, other financial institutions that give consumer loans, social lenders, insurance companies, and other entities that grant mortgage loans. To be able to make accurate comparisons between countries and between financial institutions, we classify them into two groups: monetary financial institutions (MFIs) and other financial institutions (OFIs).⁷ The presence and importance of MFIs and OFIs differ not only between countries but also within credit types in a given country.⁸

Loan Contracts. For each loan contract, we have information on five characteristics. We know the authorized or committed amount, expressed in euros, and the interest rate, expressed as the annual percentage rate (APR) on the contract (at origination). Furthermore, we also have information about maturity, interest rate fixation period, and whether the loan is secured or not. We use bins of *maturity* and these differ depending on the type of credit. For mortgages, we classify loans into four groups of maturity at origination: ≤ 10 -year maturity, 10- to 20-year maturity, 20- to 30-year maturity, and > 30 -year maturity. We also consider four bins for consumer credit: ≤ 1 -year maturity, 1- to 3-year maturity, 3- to 10-year maturity, and > 10 -year maturity. For each loan type, we also have a category for missing or implausible values. We do not have information on the specific type of *collateral* for all countries, so we only use a binary indicator to determine whether a loan is secured or not.

In all countries except Belgium, we also have information on the *initial rate fixation* period. We categorize this information into four groups: loans with an interest rate fixation period of up to 12 months, loans with a fixation period between 1 and 5 years, loans with an

⁷We obtain the list of MFIs from https://www.ecb.europa.eu/stats/financial_corporations/list_of_financial_institutions/html/index.en.html.

⁸This study includes the information on OFIs in the descriptive analyses of the data set, to provide the complete picture of the data set. However, in the pass-through analyses, we focus only on the loans granted by MFIs, which is a more homogeneous and comparable set of financial institutions. We leave a detailed description of the differences between MFIs and OFIs and the cross-country heterogeneity for future analyses.

interest rate fixation period between 5 and 10 years, and loans with a fixation period longer than 10 years. For fixed-rate loans, we use the maturity of the contract to assign the fixation period. As with the previous characteristics, there is also a fifth category for cases where information is missing.

2.3 Data Coverage and Limitations

While the data set provides an unprecedented cross-country view of household credit markets, its coverage and construction are subject to several limitations that define the boundaries of the analysis. Our data collection and cleaning involve collective methodological choices and constraints that shape the interpretation of the results. The appendix provides detailed country-specific descriptions; here we summarize the main aspects.

First, coverage and availability differ across countries: Belgium does not report contractual interest rates, though we can estimate them for mortgages (but not consumer loans), and the Italian register likewise lacks rates on consumer credit. In some cases, information from multiple registers must be combined—for example, in Italy, from Centrale dei Rischi and Consorzio Tutela del Credito. Second, institutional changes create breaks in coverage, such as Belgium's transition to a new register in May 2024, which prevented us from using data thereafter. Third, data availability is incomplete in some countries, e.g., Lithuania lacks observations for the second half of 2024, due to delayed dissemination. Finally, confidentiality requirements prevent us from working with or sharing contract-level data; instead, we aggregate into multidimensional bins, dropping bins with fewer than five contracts. These steps preserve privacy but inevitably reduce granularity.

In addition, the pass-through analysis is limited to loans granted by monetary financial institutions. We impose this restriction for two reasons. First, other financial institutions are, in most countries, active only in the consumer credit market. To ensure comparability between mortgages and consumer credit, we restrict the analysis to the more homogeneous set of MFIs. Second, there is substantial cross-country variation in the presence and importance of OFIs, which would otherwise confound the interpretation of cross-country differences.

Finally, aggregate monetary and financial statistics are compiled according to harmonized rules within the euro area. These rules were used as guidance for concepts and definitions in our work whenever possible.

Taken together, these constraints imply that the harmonized data set used in this paper provides a representative but not exhaustive picture of household credit markets. They also motivate the binning approach described in the next section, which ensures comparability across countries while respecting confidentiality requirements.

2.4 Additional Data

In the context of this study, we complement the household credit register information with additional data on market interest rates. This includes EURIBOR (euro-area interbank offered rate) rates and country-level interest rate swaps (IRS) at various maturities. Namely, we have EURIBOR rates at one month, three months, six months, and 1 year, and IRS at 5, 10, 15, 20, and 30 years, as well as their counterparts for Hungary. These rates help to understand how interest rates on household credit with different characteristics change when the relevant reference rate changes due to changes in monetary policy and broader economic conditions.

3. Statistical Treatment of Confidential Data

3.1 Binning Granular Data

For each new loan granted to households, each credit register records both the committed loan amount and the interest rate at the beginning of the contract.⁹ The unit of observation is the loan contract of borrower i with institution b in country c at time t . Because contract-level household credit data cannot be shared due to strict confidentiality, we aggregate the data into standardized bins that preserve comparability across countries while safeguarding privacy.

⁹For more details, see Appendix A for country-specific information and exceptions.

For each of the dimensions described in Subsection 2.2, we create indicator variables. Subsequently, we create bins from all possible combinations of these dimensions and label these $BIN_{Country,Period,LoanType,Age,Maturity,IRF,Collateral,MFI}$. Each bin is thus defined by country, half-year period, loan type (mortgage or consumer credit), borrower age group, loan maturity, interest rate fixation (IRF) horizon, collateral status, and lender type (MFI versus non-MFI). This multidimensional classification allows us to compare contracts with similar characteristics across countries. The grouping of observations with similar characteristics in the same bin thus aims at preserving confidentiality while maximizing within-bin homogeneity.¹⁰ At the same time, after aggregation, there is still a sufficient number of different combinations across countries and over time periods.

In principle, this approach could generate up to 768 bins per country-period, though the actual number is often smaller. One reason is that some combinations of characteristics are not observed in practice (e.g., loans with maturity over 30 years in the age group over 65). The more decisive factor is the absence of a specific category in a country (e.g., there is no information on loans provided by OFIs in Slovakia, effectively reducing the set to 384 combinations).

3.2 Bin-Specific Information

For each bin, we collect several statistics on both the nominal loan amount and the annualized interest rate. First, we compute the within-bin average nominal loan amount at the time of origination and the within-bin average interest rate (APR) on new loans. While potentially losing information due to aggregation, the bins still allow for new insights not observable through other data sets (like iBSI or iMIR, or surveys).

Second, we also compute the within-bin standard deviation in amounts and interest rates. This not only gives us an indication of the homogeneity within a bin, but also allows for studying dispersion

¹⁰Studies using corporate credit register data often include industry-location-size-time (ILST) fixed effects, following Degryse et al. (2019). While their goal is different, i.e., preserving single-bank borrowers in a high-dimensional fixed effects setup, the idea is similar. An ILST bin should contain firms with similar credit needs.

in pricing in very similar contracts. To the best of our knowledge, such information is not available in other cross-country data sets.

Third, we also compute the within-bin median and within-bin interquartile range, which are robust estimators of centrality and dispersion. Furthermore, combining measures of centrality and dispersion will also allow us to compute coefficients of variation, which are unit-free and thus enhance cross-country and time comparisons.

Finally, for each bin, we also count the number of observations (i.e., the number of new loan contracts in that bin) and do that for two reasons. First, we use that information to delete bins with less than five observations, to avoid reidentification in scarcely populated bins. Second, we can also use this information as statistical or importance weights in any analysis. Statistical weights might be considered to account for the precision of the within-bin estimates. Importance weights might be considered if one is interested in a macroanalysis treating different bins or countries differently depending on their relative size and prevalence in the economy.

3.3 Summary Statistics

To provide an overview of the data set, Table 1 presents summary statistics on new loan contracts disaggregated by product type. The sample spans the period 2022:H1–2024:H2. The table contains two panels: panel A reports statistics on *mortgages*, whereas panel B reports statistics on *consumer credit*. The unit of observation is a narrowly defined bin constructed over combinations of country, period, loan type, borrower age group, maturity bin, interest rate fixation category, collateral status, and lender type. For each product type, we provide statistics (means and percentiles) on the within-bin average and dispersion in loan amounts and interest rates for newly originated contracts, as well as the number of new contracts per bin. Monetary values are expressed in euros, while interest rates are in percentage terms.

The number of bins with at least five credit contracts differs substantially across product types. There are 3,000 bins for mortgages and 5,728 bins for consumer loans. Differences between products are due to the exclusion of uncollateralized mortgages or mutually exclusive combinations (e.g., short-term consumer loans with long interest rate fixation). Moreover, in Belgium and Italy, contract-level

interest rate data are only available for mortgages, which explains the lower observation count of interest rates compared with amounts for nonmortgage credit.

For each product, the number of new contracts per predefined bin varies considerably. This mainly reflects differences in country's size. Variation within a country is mainly due to bins' characteristics. Some bins are less populated for economic reasons, e.g., long-term mortgages to people aged 65 or higher. Interestingly, comparing the number of observations per bin, we find that for the average bin, there are six times as many observations on consumer credit compared with mortgages. The observed variation in the number of contracts is also informative for empirical specification design, underscoring the importance of weighted regressions to account for bin-specific sample sizes and importance. For example, using the number of observations in a bin as weights in a regression framework would, in spirit, be similar to using the underlying contract-level data and unweighted regressions, where loans with more common characteristics will be a larger fraction of such a sample.

Across the product types, the table reveals significant heterogeneity in contract size and pricing. Average amounts for new mortgages are over €100,000 (panel A), while consumer loans average just below €15,000 (panel B). The ratio of these average values is basically the inverse of the ratio of the number of new consumer credit to new mortgages, indicating that both markets are of similar size. Interest rate differences across products are also striking: mortgages average 4.22 percent, whereas consumer loans average 11.52 percent. These differences reflect both credit risk and contract design, with consumer credit typically involving uncollateralized, flexible credit, and shorter durations. For each bin type, there is also sizable between-bin variation in both average amounts and average interest rates. This may reflect both cross-country differences and within-country variation due to contract features.

The dispersion in loan amounts is substantial for both products. Standard deviations highlight wide within-bin variation. Among consumer loans, the coefficient of variation for loan amounts is 0.89. The within-bin dispersion in mortgage credit amount points to slightly more homogeneity, relative to consumer loans. Importantly for our study, the within-bin dispersion and coefficient of variation are much smaller for interest rates. Interestingly, we observe

the reverse pattern for within-bin price dispersion, compared with within-bin amount dispersion. Mortgage pricing seems to be more standardized (within a bin) than consumer loan rate-setting.

Additional descriptive evidence is provided in Appendix C. Table C.1 reports the number of bins with at least five contracts per country and product type, offering a measure of data representativeness and granularity. The total count of observations varies across different countries, which may be attributed to certain characteristics that are either immeasurable within a specific country or lack the level of detail required by predefined bins. For instance, Belgium exhibits a generally low count due to the absence of data on interest rate fixation, a trait categorized into five distinct bins. This absence significantly limits the possible number of cells. The highest observation count is seen in Spain and Portugal, suggesting two factors: first, the data are highly detailed with comprehensive documentation of all characteristics; second, there is considerable diversity in the types of contracts available in household credit markets.

Tables C.2 and C.3 provide detailed within-bin statistics separately for mortgages and consumer credit, broken down by country. For each product type, the tables present percentiles, standard deviations, and coefficients of variation for both loan amounts and interest rates. This expanded breakdown supports cross-country comparisons of pricing dispersion and loan size heterogeneity and highlights the variation that may be masked in pooled summary statistics.

Complementary graphical evidence is provided in Appendices D, E, and F. Figures D.1–D.9 plot country-level time series of total new credit volume and number of contracts, as well as value-weighted average loan amounts and interest rates by product type. Figures E.1–E.9 further disaggregate these statistics by initial maturity category, and Figures F.1–F.9 by borrower age group. These visualizations reinforce the cross-sectional heterogeneity identified in the summary statistics.

Implications. These summary statistics reveal a high degree of heterogeneity in household credit markets across Europe—both across and within countries, and across product types. The results indicate that consumer credit is substantially less standardized than mortgage lending. This underscores the need to go beyond very aggregate measures when analyzing the transmission of monetary policy and the dynamics of household borrowing.

4. Stylized Facts: Country and Product Variation

4.1 *Composition of New Credit*

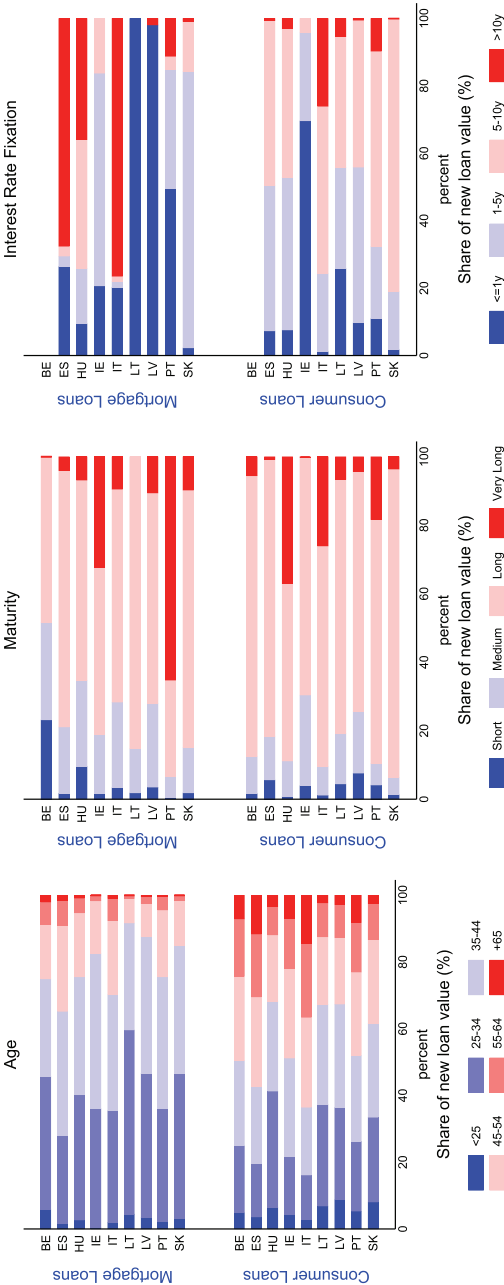
Before proceeding to a regression analysis analyzing heterogeneity in the pass-through of monetary policy, we first document a set of stylized facts on the composition of newly originated mortgage and consumer loan contracts—across borrower age, loan maturity, and interest rate fixation. These stylized facts highlight key patterns in the composition of credit within and between countries that our subsequent analyses will seek to exploit.

We infer these stylized facts from Figure 1 and Figure G.1 (in Appendix G), which are constructed in a similar fashion. Figure 1 and Figure G.1 differ in that the former is based on the value of the contracts, whereas the latter is based on the number of contracts. Each row of charts corresponds to a loan type (mortgages on top, consumer loans on the bottom), while the three columns report, respectively, the distribution of contracts by borrower age (left), by loan maturity (middle), and by interest rate fixation period (right). Within each bar, the colored segments show the percentage share of total contracts in nine euro-area countries—Belgium (BE), Spain (ES), Hungary (HU), Ireland (IE), Italy (IT), Lithuania (LT), Latvia (LV), Portugal (PT), and Slovakia (SK)—for the age groups, maturity bins, or interest rate fixation horizons. This stylized-facts figure provides a concise visual summary of how borrower demographics and contract characteristics vary both across loan types and across countries over the period January 2022–December 2024. Patterns that emerge are compatible with a wide array of potential explanations, of which we discuss only some in the text, deferring more accurate analyses to future research.

4.1.1 *Breakdown of New Loan Contracts by Age of Borrower*

We begin by examining how new loan origination varies by borrower age. This analysis complements a growing literature emphasizing demographic heterogeneity as a key driver of monetary policy transmission (see, e.g., Leahy and Thapar 2022). Age is arguably the most relevant borrower characteristic lost in aggregate data, and its distributional effects warrant detailed investigation.

Figure 1. Breakdown of New Loan Contracts (value)
by Age, Maturity, and Interest Rate Fixation Period



Note: The figure shows the distribution of new loan contracts (by value) across borrower age (left), loan maturity (middle), and interest rate fixation (right) for mortgages (top) and consumer loans (bottom) in nine European countries (2022–24).

The left panel of Figure 1 (and Figure G.1) displays the age composition of new loan contracts by product type for nine European countries over the period January 2022 to December 2024. The upper figure shows the share of the value of mortgages granted to each age group; the lower figure reports the corresponding share in total loan value of consumer credit. Each bar is disaggregated into six borrower age brackets: under 25, 25–34, 35–44, 45–54, 55–64, and over 65 years.

Mortgages exhibit the most pronounced age-based skew. The bulk of new mortgage lending is concentrated among individuals aged 25 to 44 years, who account for 65 percent to 85 percent of new contracts across countries, both in value and number of loans. This is consistent with first-time home purchases and life-cycle patterns of household formation. The 45–54 group plays a more limited but visible role, particularly in countries like Spain, Italy, and Portugal, possibly due to postponed investments by young people, potentially related to later stabilization in the employment situation, or due to second-home buyers. The youngest borrowers (under 25) represent a negligible share in most countries, though surpassing 5 percent in Belgium, Lithuania, and Latvia (in number, not in value), while the over-65 group has almost no presence in this market segment—likely reflecting the repayment horizon and past housing choices.

Consumer credit exhibits a more even age distribution than mortgages. The 25–34 and 35–44 groups dominate originations in most countries, especially in terms of total loan value. Borrowers under 25 play a relatively larger role in Lithuania, Latvia, and Slovakia, though their loan sizes are typically smaller, as evidenced by their lower share in value relative to volume (see complementary Figure G.1). The share of borrowers aged 45 to 65 years is more pronounced in consumer credit markets (compared with mortgages), suggesting that mid-career individuals continue to demand credit, possibly for consumption smoothing or household investments. Especially in Southern European countries (Spain and Italy), we see a greater share of consumer credit going to borrowers aged 45 and above, and the lowest participation rate for the youngest age group.

These patterns underscore the importance of borrower age in understanding credit access and pricing across product markets. They also highlight meaningful cross-country differences in credit market demographics, possibly linked to household structure,

income dynamics, and institutional settings such as credit scoring systems, retirement-related borrowing constraints, or homeowner-ship preferences and other social patterns.

4.1.2 Breakdown of New Loan Contracts by Maturity of Contract

We now turn to an analysis of how loan maturity structures vary by credit product and across countries, based on originations between January 2022 and December 2024. Maturities are classified separately for mortgage and consumer credit. For *mortgages*, we distinguish between short-term (≤ 10 years), medium-term (10–20 years), long-term (20–30 years), and very long-term (> 30 years) maturities. For *consumer loans*, we use four bins: short-term (≤ 1 year), medium-term (1–3 years), long-term (3–10 years), and very long-term (> 10 years). The middle charts of Figures 1 and G.1 illustrate substantial cross-country heterogeneity in the composition of loan contracts by maturity at origination.

The maturity structure of *mortgage loans* varies markedly across countries. In Hungary and Belgium, over half of the number of new mortgage lending is concentrated in maturities below 20 years, reflecting a preference for shorter repayment horizons. In contrast, countries such as Portugal, Lithuania, and Slovakia exhibit a strong reliance on long-dated mortgage contracts. In particular, Portugal stands out: over 60 percent of newly originated mortgage loans fall into the very long-term category (maturities above 30 years), and nearly 90 percent exceed 20 years. In all other countries, the 20–30 year bin is the most common. These differences likely reflect national institutional settings, such as mortgage market design, regulation on amortization periods, and the availability of fixed-rate options. Given the role of maturity structure and interest rate revision in determining household exposure to interest rate risk, such variation has direct implications for the transmission of monetary policy (Hedlund et al. 2025).

In *consumer credit*, short-term (less than one year) and medium-term maturities (one to three years) hold a notable presence, but are much less important in terms of total value. This is a reflection of the wide range of items that can be purchased with consumer credit. An important category of consumer loans is auto loans, which are large

in value and often have a maturity exceeding three years. At the shorter end of the maturity spectrum, consumer durables bought on credit have less significant values and, most often, maturities up to three years. In value terms, long-term loans with a maturity between 3 and 10 years are the dominant group in all countries.

From the angle of monetary policy transmission, the bulk of the effect for mortgages is likely going to be driven by longer-term, higher-value loans. For consumer loans instead, the maturity-value mismatch (where short-term loans dominate by number but longer-term loans dominate by value) may imply a temporal duality in transmission if rate changes are different for short- versus long-maturity consumer loans. We analyze this in a later section (Section 5.3).

4.1.3 Breakdown of New Loan Contracts by Interest Rate Fixation Period

The final breakdown looks at differences in interest rate fixation periods between products and countries. Garriga, Kydland, and Šustek (2017), for example, highlight the role of fixation period in the transmission of monetary policy. The right panel of Figure 1 reports the distribution of new loan contracts in terms of total loan value, while the right panel of Figure G.1 represents their distribution by count. Considering mortgages, we again observe substantial cross-country heterogeneity. In Spain and Italy, mortgage interest rates are predominantly fixed over the duration of the contract. If not a fixed-rate contract, the contracts exhibit very high interest rate reset frequencies. There are hardly any intermediate cases. In Portugal, nearly half of the contracts have short fixation periods (less than a year), and in Latvia and Lithuania (nearly) only such contracts are used. Slovakia is different from the above two cases, in that mortgage contracts have rates that can be reset but not at very short notice. The disparity in mortgage loan fixation periods, in particular, may have implications for household financial stability, as shorter fixation periods expose borrowers to higher interest rate risk in tightening monetary conditions (while providing relief in easing periods). At the same time, this allows for faster monetary policy transmission.

Finally, turning to consumer loans, it is important here to assess the distribution across interest rate fixation bins together with

the maturity distribution. The following interesting observations emerge. First, Ireland stands out by having nearly 70 percent of its consumer loans having reset periods within 12 months. Second, also in Latvia and Lithuania, many loan contracts are adjustable rate, as interest rates can be reset within 12 months. Third, in Italy, Portugal, and Slovakia, many consumer loans have long interest rate fixation or are never reset.

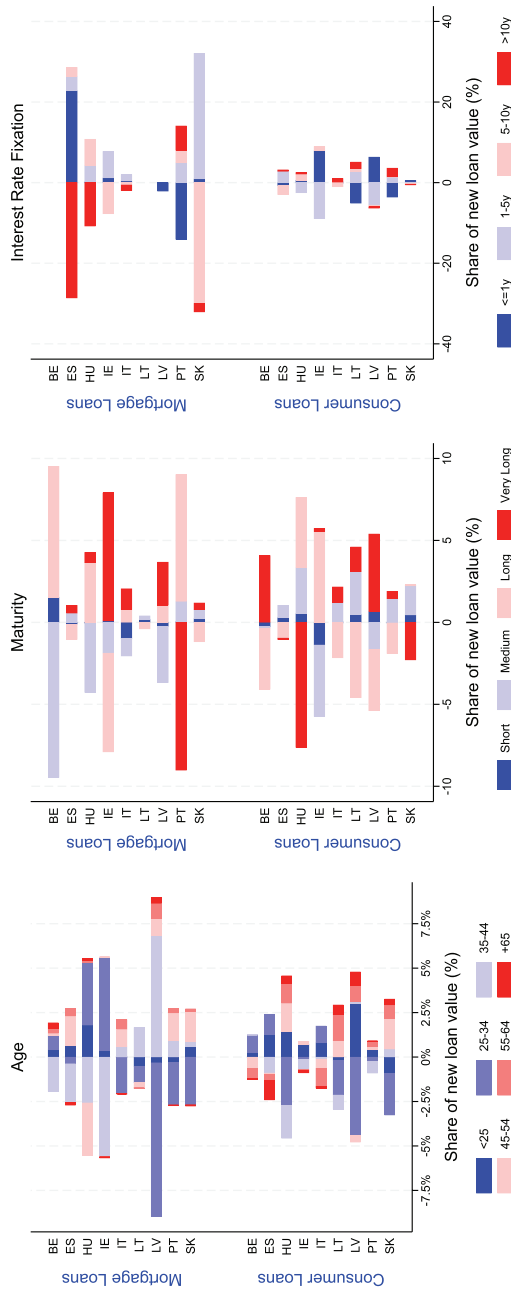
4.2 Composition Changes over a Tightening Cycle

We now turn to a graphical analysis that documents significant shifts in the composition of consumer and mortgage credit markets throughout the post-pandemic tightening cycle. Figures 2 and G.2 compare, for every country and category, the variation in relative weight (in terms of value and number of operations) in percentage points between 2022:H1 and 2023:H2. They thus capture differences between the “pre” and the “post” tightening cycle. However, it is important to note that significant shifts in composition may have already occurred just prior to 2022:H1 due to anticipation effects. Although some patterns are broadly consistent across countries, others reveal notable idiosyncrasies at the country and market segment levels.

The key findings of Figures 2 and G.2 are as follows. The composition of borrowing by age group varies across countries. At the peak of the tightening phase, younger borrowers (below age 34) account for a larger share of new mortgage issuance in Belgium, Spain, Hungary, and Ireland. However, the opposite trend is observed in Italy, Latvia, Lithuania, Portugal, and Slovakia, where borrowers younger than 34 experience a relative decline in their share of new consumer credit flows. For the latter four countries, the trend is the same in the consumer credit market. In fact, in all countries but Belgium, Spain, and Italy, the share of consumer credit to late-career borrowers increases.

In general, there is an effect of maturity lengthening in the mortgage market. The exception is Portugal, where very long-term mortgages (exceeding 30 years) are more prevalent; there is a reallocation of these ultra-long maturities to the 20–30 year segment (Figure 2). In the consumer credit segment, there is, in all countries except Ireland, a small increase in the share of short-maturity loans (Figure 2).

Figure 2. Share Changes over a Tightening Episode (value): Breakdown by Age, Maturity, and IRF



Note: The figure presents the change in the composition of the value of new loan contracts with respect to borrower age (left), loan maturity (middle), and interest rate fixation (right) across two different credit products—mortgage loans (top) and consumer loans (bottom)—for selected European countries. Each bar represents the percentage change in the share of new loan contracts by age/maturity/IRF group between two time periods (2022:H1 and 2023:H2). Positive values indicate an increase in the share of new loan contracts for a given group, while negative values indicate a decline.

In general, across countries, longer maturity consumer loans tend to lose share in terms of number of contracts (Figure G.2), though the picture is less consistent when looking in value terms, where there is also heterogeneous reshuffling between the two longest maturity bins.

The dynamics of loan composition based on interest rate fixation periods also provide important insights. In many countries, the allocation across different fixation periods appears relatively stable in consumer credit markets. In Portugal, Latvia, and Lithuania, there is a small shift (in value) from variable-rate consumer credit to slightly longer fixation period contracts. However, in the mortgage segment, there are clear shifts away from very long-term fixed-rate mortgages in Spain and Slovakia, and to a lesser extent also in Hungary and Ireland. By contrast, in Portugal, there is a reversal of this trend, with a decline in the popularity of variable-rate mortgages featuring interest rate adjustments within a year.

In general, these findings illustrate substantial compositional changes in credit markets during a tightening episode. Although certain patterns emerge consistently across countries, significant heterogeneity persists at the market segment level. Given that these changes reflect changes in the composition of newly issued loans, they have the potential to shape the medium to long-term evolution of bank credit portfolios.

5. Heterogeneity in the Transmission of Monetary Policy

5.1 *The Average Pass-Through*

To assess how monetary policy affects the pricing of household credit, we estimate pass-through regressions. The pass-through rate regression model examines how changes in the reference interest rate affect the pricing of newly originated household loans. The analysis is applied separately to mortgages and consumer credit. We estimate the following regression:

$$\begin{aligned} InterestRate_{bin,t} = & \beta_1 \times ReferenceRate_{bin,t} \\ & + \nu_{c \times mat} + \nu_{age} + \nu_{irf} + \varepsilon_{bin,t}. \end{aligned} \quad (1)$$

In our setup, this means that the dependent variable corresponds to the average interest for all new loans (in a six-month window) of a narrowly defined bin. The coefficient (β_1) represents the baseline pass-through effect of the reference rate, showing how changes in the benchmark rate translate into changes in loan pricing. We measure the reference rate at the midpoint of each six-month interval.¹¹ Importantly, our econometric approach measures the pass-through from reference rates to household loan rates. We thus focus on the contractual stage of transmission—how benchmark rates translate into borrowing costs for households. We do not estimate the first stage of transmission from monetary policy rates to reference rates, which may itself vary across maturities and countries.

Table 2 presents regression estimates, disaggregated into mortgages (columns 1–3) and consumer credit (columns 4–6). Columns 1 and 4 report unweighted regressions; columns 2–3 and 5–6 apply weights based on the number and value of loans, respectively. All specifications include country-by-maturity ($\nu_{c \times mat}$), age (ν_{age}), and IRF (ν_{irf}) fixed effects, and standard errors are clustered at the country-age level. Finally, the sample in the pass-through analysis is limited to loans granted by MFIs. We do that for two reasons. First, OFIs are in most countries only active in the consumer credit market. As we want to compare the pass-through across mortgages and consumer credit, we prefer to have the comparison on a similar set of financial institutions. Second, between countries, there is also significant variation in the presence and importance of various types of OFIs.

Pass-through to mortgage rates is nearly complete across all weighting schemes, with coefficients around 0.90. In contrast, pass-through to consumer credit is significantly weaker: unweighted estimates are around 0.36, rising to approximately 0.52 when weighted by loan value. This suggests stronger pass-through for large-value

¹¹The reference rate is assigned to each bin depending on the interest rate fixation that defines that bin and can be both product- and country-specific. For fixation periods up to one year, we use the relevant EURIBOR tenor. For fixation periods longer than one year, we use interest rate swap (IRS) rates corresponding to the fixation horizon. In the case of Hungary, we use BUBOR (Budapest inter-bank offered rate) for short-term fixation periods and BIRS (Budapest interest rate swap) rates for longer horizons.

consumer loans—such as auto loans—whereas rates on small-value products appear less sensitive to reference rate changes.¹² These estimates are broadly in line with the average magnitudes reported in the meta-analysis of pass-through studies by Beyer et al. (2024) and Gregor, Melecký, and Melecký (2021).

The pattern of adjusted R^2 values provides further insight into the heterogeneity of pass-through. In the unweighted regressions (columns 1 and 4), the fit is similar for both mortgages and consumer credit, with adjusted R^2 values around 0.77–0.81. When applying weights, the explanatory power of the regressions improves across the board, but with notable variation by product and weighting scheme. For mortgages, both number-weighted and value-weighted specifications yield adjusted R^2 values of 0.86, indicating a uniformly strong fit across loan volumes and sizes. For consumer credit, the number-weighted specification shows a similarly high adjusted R^2 of 0.87, suggesting that pricing is relatively systematic in high-frequency, small-loan bins. However, the improvement is more modest when weighting by loan value, with the adjusted R^2 rising only to 0.82. This points to slightly greater pricing dispersion among large-value consumer loans, which may reflect more idiosyncratic contract terms, borrower-specific pricing, or slower rate adjustment.

Several mechanisms may contribute to differences between products and across specifications. Lenders may adjust nonprice contract terms—such as maturity, loan size, commissions and fees, or collateral requirements—rather than headline interest rates, particularly to preserve affordability in high-rate environments (Bharath et al. 2011; Qian and Strahan 2007). Moreover, pass-through may be asymmetric: lenders might absorb part of rising funding costs during policy tightening to limit default risks or preserve borrower demand (Puglisi 2023). These behaviors reflect a mix of risk mitigation, product-specific pricing constraints, and institutional frictions or banking-sector structure and conduct.

¹²For comparison, Vilterts et al. (2026) find a pass-through coefficient for corporate credit of 0.66. They examine a similar period and also focus on new loans, but use the granular data available in AnaCredit for nonfinancial corporations (NFCs).

Taken together, these mechanisms underscore the importance of composition effects arising from heterogeneity in monetary policy pass-through. In the next section, we exploit additional features of the data set to uncover some of these mechanisms and sources of heterogeneity.

To examine heterogeneity by borrower and contract characteristics, we augment Equation (1) with interaction terms:

$$\begin{aligned}
 InterestRate_{bin,t} = & \beta_1 \times ReferenceRate_{bin,t} \\
 & + \sum_d \beta_d \times ReferenceRate_{bin,t} \\
 & \times D(Characteristic = d) \\
 & + \sum_d \gamma_d \times D(Characteristic = d) + \nu_{c \times mat} \\
 & + \nu_{age} + \nu_{irf} + \varepsilon_{bin,t},
 \end{aligned} \tag{2}$$

where for each category d , β_d measures the *heterogeneous pass-through*, i.e., how the sensitivity of loan rates to the reference rate differs when the bin exhibits the characteristic d (e.g., a specific country, age group, maturity bin, or fixation period). γ_d captures a *level shift* in the average interest rate associated with the characteristic d , independent of reference rate movements. By comparing the estimates of β_d across categories, we identify how the pass-through varies with borrower demographics, contract design, and national market structures.

5.2 Heterogeneity in the Mortgage Pass-Through

Column 1 of Table 3 repeats the average pass-through using weighted least squares (column 2 of Table 2). Column 2 introduces interactions between the reference rate and country dummies. The omitted benchmark category is Spain. We find that a 100 basis point (bps) rise in the reference rate raises new mortgage rates in Spain by 80 bps. Almost every other country exhibits a higher coefficient—implying more complete pass-through of policy changes. The only country in our sample whose pass-through is significantly lower is

Table 3. Heterogeneity in the Pass-Through: Mortgages

	(1)	(2)	(3)	(4)	(5)	(6)
	<i>Dependent Variable: Average Interest Rate on New Contract</i>					
Reference Rate	0.904*** (0.029)	0.796*** (0.009)	0.799*** (0.013)	0.816*** (0.012)	0.865*** (0.039)	0.895*** (0.050)
BE × Reference Rate		0.225*** (0.018)	0.213*** (0.028)	0.277*** (0.040)	0.155*** (0.042)	0.185*** (0.051)
ES × Reference Rate		—	—	—	—	—
HU × Reference Rate		0.242*** (0.041)	0.234*** (0.033)	0.316*** (0.033)	-0.044 (0.045)	0.033 (0.069)
IE × Reference Rate		-0.422*** (0.014)	-0.431*** (0.012)	-0.416*** (0.017)	-0.585*** (0.025)	-0.592*** (0.030)
IT × Reference Rate		0.223*** (0.056)	0.213*** (0.055)	0.232*** (0.059)	0.173*** (0.037)	0.176*** (0.037)
LT × Reference Rate		0.116*** (0.010)	0.093*** (0.015)	0.112*** (0.011)	0.047 (0.039)	0.012 (0.048)
LV × Reference Rate		0.085*** (0.011)	0.066*** (0.014)	0.096*** (0.015)	0.014 (0.039)	-0.005 (0.044)
PT × Reference Rate		0.137*** (0.019)	0.127*** (0.021)	0.140*** (0.029)	0.034 (0.037)	0.022 (0.045)
SK × Reference Rate		0.397*** (0.053)	0.382*** (0.056)	0.394*** (0.055)	0.109* (0.055)	0.097* (0.056)
Age: <25 × Reference Rate			0.073*** (0.018)			0.065*** (0.020)
Age: 25–34 × Reference Rate			0.032*** (0.009)			0.027*** (0.009)
Age: 35–44 × Reference Rate			—			—
Age: 45–54 × Reference Rate			-0.006 (0.009)			-0.002 (0.008)
Age: 55–64 × Reference Rate			-0.051** (0.021)			-0.018 (0.029)
Age: ≥65 × Reference Rate			-0.187*** (0.045)			-0.152*** (0.048)

(continued)

Ireland.¹³ This cross-country variation persists when we add borrower age (col. 3) or loan maturity (col. 4) interactions, indicating that these dimensions alone do not explain the national differences.

Borrower age effects are monotonically decreasing with age. As the cohort 35–44 is the omitted reference group, this implies that coefficients are positive for the two younger groups (<25 and 25–34) and negative for the two older groups (55–64 and ≥ 65), with only the 45–54 bracket insignificant. Younger borrowers face lower income, less stable job positions, higher debt-to-income ratios, and weaker negotiation power—so rate changes bite harder—whereas older borrowers' greater wealth and credit histories dampen repricing. From a distributional perspective, this implies that tightening cycles impose relatively larger burdens on first-time buyers. These patterns align with life-cycle models of borrowing, in which younger households are more constrained by down-payment and income considerations in mortgage markets, while mid- to late-career borrowers rely more on consumer credit. Credit-constrained households are therefore more exposed to interest rate changes, consistent with theories of balance sheet channels of monetary policy (Cloyne, Ferreira, and Surico 2020; Mian and Sufi 2010).

Loan maturity interactions reveal a threshold effect. The 10- to 20-year and >30-year bins are not statistically different from the 20- to 30-year benchmark. Short-maturity mortgages (under 10 years), on the other hand, feature significantly weaker pass-through. Short-term loans—often financing smaller or second homes with sizable down payments—carry less duration risk, giving lenders less incentive to reprice aggressively.¹⁴ These mortgages with very short total maturities appear to be treated almost like consumer loans. Table 2 showed that consumer credit exhibits a muted pass-through (≈ 0.36 unweighted), and we observe a similar dampening for these short-maturity mortgages.

Interest rate fixation is an important driver of heterogeneity in the pass-through (judged from the increase in adjusted R^2): loans

¹³This could be explained by excess liquidity, competition, and bank health. Ireland has had weaker pass-through to mortgage rates and overnight household deposits during the recent tightening than other European countries (Byrne and Foster 2023).

¹⁴Vilerts et al. (2026) also find that, for corporate loans, those with shorter residual maturity are more exposed to monetary policy rate changes.

fixed for one to five years show the strongest and most immediate pass-through, as they reset frequently. By contrast, mortgages with 5- to 10-year and >10-year fixations are far less responsive, reflecting banks' asset-liability matching incentives and the forward-looking nature of long-fixation pricing, which smooths borrower payments but blunts short-term policy impulses.

Importantly, when we include interest rate fixation (IRF) interactions (cols. 5–6), four patterns emerge. First, all else equal, Spain's pass-through rises to 0.87–0.90, revealing a stronger link after accounting for fixation structure. Second, several countries (Portugal, Hungary, Latvia, and Lithuania) no longer differ statistically from Spain—implying their earlier apparent “over-pass-through” was driven by a higher share of short-fixation loans. Third, the pass-through is still higher in Italy, Belgium, and Slovakia and indicates a one-for-one relationship between contractual rates and reference rates. Finally, Ireland remains a persistent outlier, with a muted pass-through even after controlling for age, maturity, and fixation, suggesting deeper institutional or market-structure barriers.

5.3 Heterogeneity in the Consumer Credit Pass-Through

We now move to analyze the pass-through to consumer credit. Column 1 of Table 4 repeats the average pass-through using weighted least squares (column 5 of Table 2). Column 2 interacts the reference rate with country dummies. In Spain (the omitted category), the baseline pass-through is 0.49: a 100 bps increase in the reference rate raises the average rate on new consumer loans in Spain by only 49 bps (slightly more than half the mortgage pass-through). Cross-country coefficients vary markedly: Hungary, Ireland, and Lithuania lie at the lower end with only a modest pass-through, while Latvia and Slovakia show near-complete pass-through.¹⁵ Interestingly, Portugal's pass-through closely matches Spain's, suggesting similar transmission efficiencies in those markets.¹⁶

¹⁵Despite the general similarity of the banking sectors in Latvia and Lithuania, the high pass-through in Latvia is to a large extent driven by one specific bank focusing on consumer credits.

¹⁶Recall that Belgium and Italy are excluded because comparable consumer-rate data are unavailable.

Table 4. Heterogeneity in the Pass-Through: Consumer Credit

	(1)	(2)	(3)	(4)	(5)	(6)
	<i>Dependent Variable: Average Interest Rate on New Contract</i>					
Reference Rate	0.356*** (0.059)	0.485*** (0.083)	0.450*** (0.076)	0.608*** (0.103)	0.438*** (0.086)	0.634*** (0.132)
ES × Reference Rate		—	—	—	—	—
HU × Reference Rate		−0.242** (0.090)	−0.241*** (0.084)	−0.285*** (0.083)	−0.262*** (0.090)	−0.281*** (0.076)
IE × Reference Rate		−0.429*** (0.084)	−0.428*** (0.077)	−0.456*** (0.084)	−0.384*** (0.086)	−0.500*** (0.094)
LT × Reference Rate		−0.339*** (0.107)	−0.329*** (0.103)	−0.405*** (0.108)	−0.333*** (0.113)	−0.390*** (0.110)
LV × Reference Rate		0.566*** (0.185)	0.574*** (0.186)	0.531*** (0.191)	0.551*** (0.181)	0.539*** (0.195)
PT × Reference Rate		0.051 (0.081)	0.054 (0.073)	−0.025 (0.079)	0.039 (0.080)	−0.028 (0.067)
SK × Reference Rate		0.513*** (0.095)	0.522*** (0.090)	0.465*** (0.101)	0.484*** (0.104)	0.473*** (0.098)
Age: <25 × Reference Rate			0.040 (0.061)			0.040 (0.064)
Age: 25–34 × Reference Rate			0.035* (0.018)			0.028 (0.018)
Age: 35–44 × Reference Rate			—			—
Age: 45–54 × Reference Rate			0.005 (0.020)			0.012 (0.020)
Age: 55–64 × Reference Rate			0.050* (0.026)			0.060** (0.027)
Age: >=65 × Reference Rate			0.153*** (0.033)			0.174*** (0.034)

(continued)

Column 3 adds borrower age-group interactions, revealing a modest upward slope in pass-through with age: only the oldest cohorts (55–64 and ≥ 65) exhibit statistically significant coefficients above the baseline, suggesting that late-life loans may carry higher risk premia or involve more frequent renegotiation, thereby increasing rate sensitivity.

Column 4 introduces maturity-bin interactions. Short-maturity loans (≤ 1 year) and medium-maturity loans (1–3 years) display substantially weaker pass-through—akin to the muted response seen for short-term mortgages. The very-long maturity bin (above 10 years) is statistically indistinguishable from the 3- to 10-year reference. Importantly, adjusting for maturity elevates Spain's estimated pass-through from 0.49 to about 0.61 but does not alter the ordering of country effects, implying that contract length alone cannot account for national differences. Unlike in the mortgage regressions, the role of the interest rate fixation period is less pronounced, and its inclusion does not affect the country interactions. There seems to be some effect across the longer fixation periods, though the coefficients are unstable across specifications.

Finally, our mortgage results showed that short-maturity mortgages behave like consumer loans in their muted pass-through, while long-maturity mortgages align closely with the reference rate. Similarly, within consumer credit, *long-maturity loans* (most likely financing vehicles) exhibit pass-through patterns more akin to mortgages, whereas *short-maturity products* remain as reference-rate insensitive as other small-ticket credit. Recognizing the *purpose* of each loan—whether for housing, vehicle purchase, or consumer durables—could offer further clarity and would therefore be a valuable extension.¹⁷

5.4 Rate Dispersion and Monetary Policy

Up to this point, our analysis has focused on the *average* sensitivity of newly originated loan rates to changes in the reference rate. In Table 2, our results demonstrated that mortgage rates adjust almost one-for-one with reference rate moves, while consumer credit rates

¹⁷For instance, Ivashina, Laeven, and Moral-Benito (2022) show that loan purpose on corporate loans has important implications for the bank lending channel.

exhibit significantly weaker pass-through. However, focusing solely on central-tendency measures masks an important aspect of lender behavior: how changes to reference rates affect the *cross-sectional dispersion* of rates offered to otherwise similar borrowers or similar contracts.

To capture this dimension, Table 5 reports weighted least-squares estimates that allow assessing how a 100 bps increase in the reference rate influences both the central tendency and within-bin dispersion of *newly originated* household loan rates. Panel A isolates mortgages and panel B isolates consumer credit. Column 1 replicates the baseline pass-through coefficients for the within-bin average rate. Columns 2–6 then extend the analysis to the median rate, interquartile range (IQR), standard deviation (StD), robust coefficient of variation (IQR/median), and conventional coefficient of variation (StD/mean). Each measure is computed for all contracts within a bin. By comparing these dispersion metrics across loan types, we can determine whether, and to what extent, monetary conditions amplify or compress rate heterogeneity in the origination of household credit.

Panel A (Mortgages). The estimated pass-through to new mortgage rates is nearly complete, also when using the median rate in a bin. Moreover, measures of within-bin dispersion remain essentially unchanged. The interquartile range and standard deviation within a bin increase only marginally and are economically negligible. The combined effect of the first and second moments is that both coefficients of variation decline slightly (by approximately 6 percent) when the reference rate tightens. These findings indicate that, even as reference rates fluctuate, mortgage pricing adjusts almost uniformly across borrowers and loan contracts.

Panel B (Consumer Credit). In contrast, new consumer credit rates exhibit substantially weaker pass-through. Importantly, the effect on the within-bin median is around 0.30 bps and thus smaller than the within-bin mean—reflecting already some discretionary price setting within a bin. This is confirmed in columns 3 and 4. Crucially, the interquartile range coefficient of 0.325 (statistically significant at the 1 percent level) reveals a pronounced absolute widening of the rate distribution. A 100 bps increase in the reference rate widens the gap between the 75th and 25th percentiles within a bin by 33 bps. The within-bin standard deviation also increases (by

Table 5. Impact of Reference Rates on Loan Rate Dispersion

[illegible]

0.098 bps, significant at 5 percent) when the reference rate increases. However, in relative terms, the increase in dispersion (IQR) and increase in median offset each other, resulting in an insignificant impact on the robust coefficient of variation. The conventional coefficient of variation declines by 0.009 (1 percent significance), implying that although absolute dispersion rises, relative heterogeneity decreases because the mean rate increases more rapidly than the standard deviation. However, the economic magnitude of this effect is small.

These results may suggest that monetary conditions have fundamentally different cross-sectional effects on mortgage versus consumer credit markets. Mortgage lenders, operating under standardized product menus and intense competition, adjust rates almost uniformly, thereby preserving the homogeneity of pricing (Scharfstein and Sunderam 2016). By contrast, consumer credit providers may engage in a more strategic repricing (Yannelis and Zhang 2023), raising central-tendency measures only partially while sharply expanding the absolute spread of offered rates—particularly penalizing higher-rate borrowers (in a bin)—and thus intensifying segmentation.

This widening of dispersion is consistent with theories of risk pricing and asymmetric information: consumer credit markets are less transparent, with higher monitoring costs and stronger scope for price discrimination, which can amplify heterogeneity in response to monetary policy (Indarte 2023; Stiglitz and Weiss 1981). The pronounced widening of absolute rate dispersion in new consumer credit contracts following a policy rate increase not only reflects supply-side factors but also is a combination of borrower-level, contract-level, and lender-level heterogeneity. On the borrower side, differences in financial literacy and search sophistication influence how effectively individuals compare and negotiate loan offers. More sophisticated borrowers, who tend to have higher incomes and greater financial experience, are better able to secure favorable terms, whereas less-informed or credit-constrained households face higher markups (Bhutta, Fuster, and Hizmo 2020; Lusardi and Mitchell 2014). Demographic factors such as age further shape risk assessments: older borrowers often benefit from longer credit histories and established relationships with lenders, while younger or lower-income

applicants are charged risk premia that amplify rate dispersion as reference rates climb (Argyle, Nadauld, and Palmer 2023).

Lender- and market-structure factors may also play a central role in driving cross-sectional dispersion. Heterogeneity in bank size, capitalization, and funding profiles gives rise to differential cost pass-through: well-capitalized institutions with access to stable funding can adjust rates uniformly, while smaller or less liquid banks rely on higher markups and selective repricing to maintain margins. Moreover, market concentration and the presence of nonbank or online lenders intensify competitive pressures, particularly in the consumer credit segment, where a fragmented supply base permits greater price dispersion when benchmark rates change (Kiefer, Kiefer, and Mayock 2025).

Although our collapsed data would permit testing some of these channels, they are better assessed using the underlying microdata. We see our results as suggestive evidence that could motivate a rich agenda for future work examining how borrower sophistication, contract structure, and lender health jointly determine the uneven pass-through of monetary policy through new-loan origination. Complementing average-rate indicators with *distributional metrics*—such as IQR and CoV—to capture the widening of credit-cost dispersion, particularly in consumer markets, can thus be valuable to central banks and macroprudential authorities.

6. Robustness and Variation

6.1 *Subsample Stability*

To ensure that our findings on heterogeneous mortgage pass-through are not driven by specific sample choices, we conduct a series of robustness checks in Table 6, reestimating the core specifications under alternative sample restrictions.

The mortgage pass-through estimates in Table 6 (columns 1–3) demonstrate substantial robustness to three alternative samples: omitting 2024 (col. 1), excluding Belgium and Italy—due to missing consumer credit data (col. 2)—and removing Ireland, which has a much lower baseline pass-through than all other countries (col. 3).

Baseline pass-through estimates remain stable at 0.87–0.95 across all specifications, confirming near-complete pass-through

Table 6. Heterogeneity in the Pass-Through: Robustness

Variation	Mortgages			Consumer Credit		
	(1)	(2)	(3)	(4)	(5)	(6)
	No 2024	No BE/IT	No IE	No 2024	No IE	No IE + Collateral
<i>Dependent Variable: Average Interest Rate on New Contract</i>						
Reference Rate	0.874***	0.950***	0.904***	0.686***	0.966***	1.009***
BE × Reference Rate	0.183**		0.181***			
ES × Reference Rate	—	—	—	—	—	—
HU × Reference Rate	0.023	0.063	0.065	−0.295***	−0.260***	−0.255***
IE × Reference Rate	−0.620***	−0.628***		−0.515***		
IT × Reference Rate	0.211***		0.176***			
LT × Reference Rate	0.008	−0.036	0.005	−0.430***	−0.453***	−0.437***
LV × Reference Rate	0.030	−0.049	−0.011	1.016***	0.554***	0.549***
PT × Reference Rate	0.055	−0.037	0.015	−0.094	−0.066	−0.047
SK × Reference Rate	0.024	0.085	0.141**	0.382***	0.451***	0.438***
Age: <25 × Ref. Rate	0.074***	0.063***	0.064***	0.026	−0.009	−0.014
Age: 25–34 × Ref. Rate	0.032***	0.026***	0.027***	0.028	0.028	0.026
Age: 35–44 × Ref. Rate		—	—	—	—	—
Age: 45–54 × Ref. Rate	−0.002	0.002	0.000	0.008	0.009	0.010
Age: 55–64 × Ref. Rate	−0.024	−0.017	−0.017	0.038	0.060*	0.061*
Age: ≥65 × Ref. Rate	−0.165***	−0.176***	−0.146***	0.138***	0.187***	0.184***

(continued)

Table 6. (Continued)

Variation	Mortgages			Consumer Credit		
	(1)	(2)	(3)	(4)	(5)	(6)
	No 2024	No BE/IT	No IE	No 2024	No IE	No IE + Collateral
	<i>Dependent Variable: Average Interest Rate on New Contract</i>					
Short $\times$ Ref. Rate	-0.273**	-0.431***	-0.343**	-0.459***	-0.632***	-0.676***
Medium $\times$ Ref. Rate	0.012	-0.003	0.003	-0.144	-0.271**	-0.293**
Long $\times$ Ref. Rate	—	—	—	—	—	—
Very Long $\times$ Ref. Rate	-0.027	-0.028	-0.037	0.352*	0.344	0.570**
IRF: $\leq$ 1 Year $\times$ Ref. Rate	—	—	—	—	—	—
IRF: 1-5 Years $\times$ Ref. Rate	0.472***	0.402***	0.416***	-0.085	-0.378***	-0.401***
IRF: 5-10 Years $\times$ Ref. Rate	-0.127	-0.168*	-0.201**	-0.047	-0.356***	-0.383***
IRF: >10 Years $\times$ Ref. Rate	-0.207	-0.284***	-0.209**	-0.427***	-0.671***	-0.763***
Collateralized $\times$ Ref. Rate						-0.265***
Observations	1,949	2,071	2,623	1,775	2,380	2,380
Adjusted R-squared	0.91	0.91	0.92	0.89	0.87	0.87

Note: This table presents heterogeneity in the pass-through on new household loans, distinguishing between mortgages (columns 1-3) and consumer credit (columns 4-6). The top row indicates how the specification differs from the benchmark. The main independent variable is the reference interest rate, interacted with four sets of characteristics. Variables, weighting, clustering, and fixed effects are as in 2 and 3. Robust standard errors clustered at the country-age level are reported in Table H.1 in Appendix H. Statistical significance is denoted as follows: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

regardless of sample restrictions. Country heterogeneity remains robust. Ireland's large negative interaction persists across samples, reinforcing its classification as being structurally different. Positive deviations for other countries also remain consistent, indicating that these effects are not driven by transient shocks or sample composition. The only noteworthy variation appears for Slovakia when 2024 is excluded: its country interaction tightens marginally.¹⁸ Age and maturity effects also remain robust across specifications. The more complete pass-through for the youngest borrowers (<25 years and 25–34 years) remains, while the lower pass-through for older borrowers or short-maturity loans (<10 years) is unchanged. Finally, loans fixed for 1–5 years consistently display stronger pass-through (≈ 0.40 – 0.47), while long-fixation contracts (5–10 years, >10 years) remain negative. These invariant patterns linked with interest rate fixation highlight banks' liability-management incentives as a key structural mechanism.

Overall, the core findings of a near-complete mortgage pass-through and its systematic heterogeneity by country, age, maturity, and fixation are highly robust to these alternative sample definitions.

Columns 4–6 present four variations of the consumer credit pass-through specification: a sample excluding all observations from 2024 (col. 4), a sample further omitting Ireland (col. 5), and finally the Ireland-omitted sample augmented with an interaction between the reference rate and a secured-loan dummy (col. 6).

We start with the most striking observation. When Ireland is excluded, every interest rate fixation category (1–5 years, 5–10 years, and >10 years) exhibits a significantly negative interaction relative to the ≤ 1 -year benchmark (or variable-rate contracts). This uniform downward shift suggests that longer-fixation consumer loans adjust less to reference rate changes than fully adjustable-rate contracts—likely reflecting banks' asset–liability matching priorities and the contractual frictions that delay repricing in these segments.

The change in these coefficients also affects the baseline pass-through coefficient; which correspondingly rises from 0.63 in the full sample (or 0.69 when 2024 is excluded) to approximately

¹⁸This persistent deviation is suggestive of structural factors such as limited bank competition, legacy mortgage products, or regulatory differences, which are emphasized in theoretical and empirical work on market structure and monetary transmission (Byrne and Foster 2023; Kashyap and Stein 2000).

1.01 once Ireland is omitted (cols. 5–6). Country-level heterogeneity remains stable across specifications, indicating that the relative difference with respect to the pass-through in Spain is robust (although Latvia's coefficient rises significantly to 1.016 when 2024 is excluded). These cross-country patterns reinforce that structural market and institutional factors underpin national differences in consumer credit pass-through.

Age effects continue to be confined to the oldest borrowers (55–64 and ≥ 65), whose pass-through interactions remain in the 0.14–0.19 range across specifications. Meanwhile, short-term loans (under 12 months) consistently exhibit lower pass-through, with the coefficient growing more negative—from -0.27 to -0.68 —as the sample is trimmed, underscoring the reliability of the short-maturity discount.

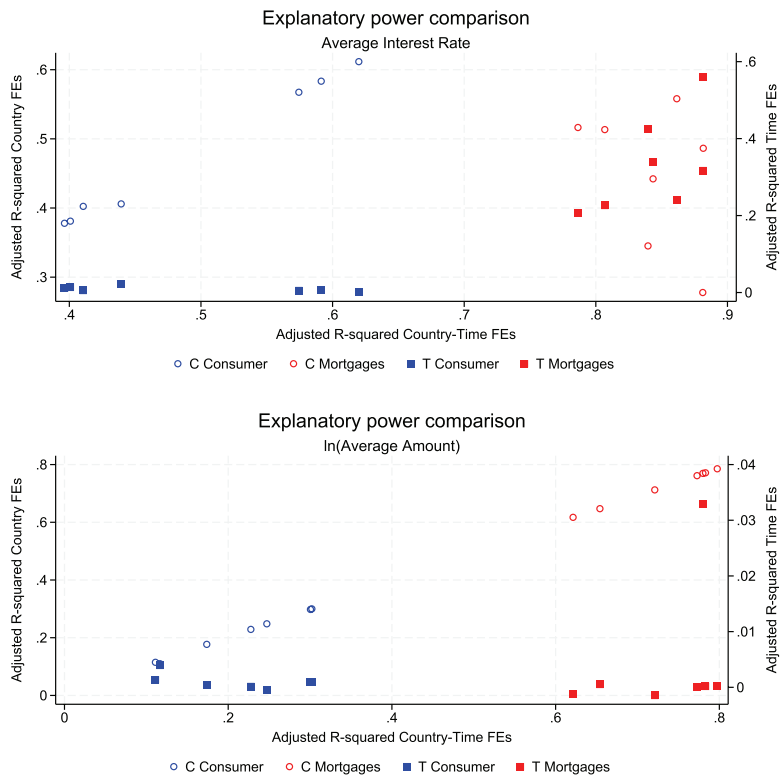
Finally, in the last column, we additionally add an interaction term between the reference rate and a secured-loan dummy. This interaction yields a significant negative coefficient (-0.265), indicating that collateralized consumer contracts experience systematically lower pass-through even after controlling for all other heterogeneity. Importantly, adding this additional dimension does not alter any of the other insights.

6.2 *Dimensions of Variation: Country versus Time*

The graphs presented in Figure 3 illustrate the explanatory power of different fixed-effects structures in explaining variations in key loan characteristics: the average interest rate (upper panel) and the logarithm of the average loan amount (lower panel). Specifically, we are interested in uncovering whether country features and/or time variation have explanatory power across products and product characteristics. This information allows us to assess the degree of heterogeneity in the drivers of loan dynamics in different markets.

The two panels follow a similar structure, and we explain their design and information content based on the upper panel. Using the average interest rate as a dependent variable (of which summary statistics can be found in Table 1), we run 42 regressions which are combinations of two samples, three sets of fixed effects, and seven weights used in the regression. The two samples correspond to the two credit product categories (distinguishing between consumer loans and mortgages). The three sets of fixed effects include

Figure 3. Sources of Variation in Loan Characteristics



Note: The figure illustrates the explanatory power of different fixed-effects structures in explaining variations in the average interest rate (upper panel) and average committed amount (lower panel) across credit product categories. The x-axis represents the adjusted R-squared values from regressions with country-time fixed effects, while the y-axes show the adjusted R-squared values from regressions with country fixed effects (left) and time fixed effects (right). The scatter-plots compare the explanatory power of country and time fixed effects against country-time fixed effects. Different colors and marker shapes indicate loan product types and fixed-effects structures. Seven weighting schemes are applied to account for differences in economic significance across credit segments.

(i) time fixed effects, (ii) country fixed effects, and (iii) time-varying country fixed effects. Broadly speaking, time fixed effects control for macroeconomic conditions and common shocks affecting all countries similarly at a given point in time. Country fixed effects could account for the country-specific structural differences in the banking

markets (like competition). Finally, country-time fixed effects capture time-varying factors that differ across countries or common shocks with heterogeneous impacts, such as monetary policy transmission, financial regulations, or market conditions.

The seven different weighting schemes allow us to view the analysis from different perspectives. The first approach is unweighted, thus giving equal weight to any combination of characteristics. However, not every combination of characteristics is equally present or economically important. Therefore, we consider two weighting schemes that give more weight to combinations that are more likely or more sizable in the overall credit market (segment). The four other weighting schemes are modifications such that the count- or value-weights either sum to one in each country-year, or sum to one in each country-year-product. The former treats each country as equally important in determining the fit; the latter treats each product market in each country as equally important (but allows for variation in importance within a country-time-product triple).

For each dependent variable, the adjusted R^2 of each of these 42 regressions is extracted and plotted to illustrate how much of the variation can be explained by each specification of fixed effects. The x-axis represents the adjusted R^2 values of the specifications with country-time fixed effects. The left y-axis corresponds to the adjusted R^2 values of regressions with country fixed effects, while the right y-axis corresponds to the adjusted R^2 values of time fixed effects regressions. We overlay two scatterplots. The first is a scatterplot that compares adjusted R^2 values of country fixed effects with country-time fixed effects (circles). The second is a scatterplot that compares adjusted R^2 values of time fixed effects with country-time fixed effects (squared). Different colors and shades are used for the product categories. Each specific label appears seven times on the scatterplot (one for each weighting approach).

In fact, the figures confirm the relative importance of different fixed effects in explaining variations in loan pricing and contract size between countries and over time. Higher adjusted R^2 values indicate a greater ability of the corresponding fixed-effects specification to account for observed heterogeneity in loan characteristics. In turn, these provide guidance for which dimensions to analyze and on which credit markets to focus.

For example, interest rates can be very well explained by time fixed effects, but only for mortgage loans. This suggests that in the

period of analysis, the pass-through to mortgage rates was fairly homogeneous across countries, being explained for the largest part by a common time trend (the variation in reference rates). For consumer credit, the variation in interest rates is primarily country-specific, without significant differential effects over time. When looking at the average loan amount, we observe that it is primarily driven by cross-country differences rather than time variation (the values on the right-hand-side y-axis are very small). Consequently, if one wants to focus on the impact of monetary policy, the best starting point would be to focus on rate setting in the mortgage market, as this is where time fixed effects have substantial explanatory power.

In sum, these graphs provide a systematic way to assess the explanatory power of fixed effects in credit market data. They help in identifying the appropriate level of analysis and thus help researchers in providing focus on the relevant drivers. They should not be limited to these sets of fixed effects. Alternatively, one could, for instance, replace the country or time fixed effects with age group (or loan maturity bin) fixed effects, to see whether heterogeneity in credit market outcomes is structurally related to differences in borrower age and whether these age-group effects are fixed, time-varying, or country-specific.

7. Policy Implications

The heterogeneous transmission of monetary policy across European household credit markets documented in this study provides valuable guidance for both national and European policymakers. Our results indicate that while mortgage markets are relatively well integrated, consumer credit markets remain fragmented, and these differences matter for the design of monetary, macroprudential, and consumer-protection policies.

For mortgages, during the latest tightening episode, the contractual pass-through from reference rates to mortgage markets was nearly complete and, once borrower and contract characteristics are controlled for, cross-country differences mostly vanish. This highlights that, at this stage of the transmission mechanism, mortgage pricing adjusts almost one-for-one to benchmark rates in a broadly uniform manner. At the same time, the disproportionate sensitivity of younger borrowers underscores the distributional consequences of policy, as first-time buyers face higher costs during tightening cycles.

This highlights the potential complementarity with fiscal measures in support of more exposed borrowers.

Contract design also shapes the trade-off between transmission speed and financial stability. In countries dominated by short fixation periods, interest rate changes feed through quickly, but households are more exposed to interest rate risk, whereas in systems with long fixation horizons, transmission is slower but borrowers are more insulated. These findings point to the importance of calibrating borrower-based measures and stress-test assumptions to reflect the rapid pass-through observed in mortgage markets, while ensuring that national frameworks account for the risks embedded in local market structures.

In contrast to mortgage markets, consumer credit markets remain highly fragmented. Even after controlling for loan and borrower characteristics, pass-through differs markedly across countries, reflecting persistent institutional heterogeneity in regulation, enforcement, and competitive conditions. Moreover, monetary tightening often leads to a widening of rate dispersion and may disproportionately affect less sophisticated or financially constrained borrowers. This suggests that fragmented consumer credit markets may exacerbate both geographic and socioeconomic inequality in the transmission of policy. Regulatory harmonization, enhanced transparency, policies that strengthen financial literacy, and monitoring of pricing practices would help to reduce these divergences and ensure that consumer credit does not remain a source of weakness in the monetary transmission chain.

Finally, the persistence of outliers such as Ireland, where mortgage pass-through is consistently weaker, illustrates that country-specific institutional and competitive factors can override broader integration trends. Identifying the underlying causes of such deviations is essential both for national supervisors and for assessing the risks of transmission fragmentation at the euro-area level.

8. Conclusion

This paper has adopted a unique, cross-country, loan-level perspective on European mortgage and consumer credit markets to document the heterogeneity in household borrowing conditions and to analyze the transmission of monetary policy during the recent

tightening cycle. Leveraging granular data from nine credit registers, we have shown that substantial differences persist across products, borrower groups, and national contexts, and that these differences shape how policy shocks are passed through to household borrowing costs. This heterogeneity in household credit conditions both across and within countries was previously hard to detect using aggregate data.

Our findings underscore the value of harmonized credit register data and uncover a high degree of heterogeneity, the explanations and drivers of which we discuss only briefly and non-exhaustively here, leaving open several promising avenues for further research. Future work could disentangle more systematically the role of institutional frameworks, labor market dynamics, competitive conditions, and borrower behavior in driving cross-country differences. Furthermore, understanding how rate dispersion arises within similar product categories would also help illuminate the mechanisms behind unequal pass-through. In addition, joint analyses across credit products—including revolving credit and overdrafts—could shed light on substitution patterns that shape household responses to monetary policy.

Additional work linking borrower-level outcomes to household and bank balance sheets, and distinguishing between supply- and demand-side drivers, would help clarify the channels through which monetary policy affects households and financial stability. While our study focused on new loan originations, another important margin of adjustment is the refinancing or renegotiation of existing contracts. Our binning framework could in principle be extended to this dimension, allowing a cross-country analysis of refinancing activity and its sensitivity to monetary policy. This would help assess how existing borrowers, not just new ones, transmit and absorb policy shocks.

Finally, this paper has focused solely on a tightening episode, which calls for further evidence on easing phases and on potential asymmetries. Such research would not only complement the findings reported here but also provide a stronger basis for evaluating the broader economic and distributional consequences of monetary policy in Europe.

Appendix A. Country-Specific Information

A.1 BE—Belgium

The National Bank of Belgium has been collecting granular data on credit to individual persons since 2006. In May 2024, a new data model and platform (BECRIS-ICR) was introduced, replacing the previous credit register (CCR/KCP). As the coverage of the two systems differs, there is a break in the time series as of May 2024. The credit register collects loan-level information on new loans, overdue debt, and payment arrears. Information on the Individual Credit Register is available at <https://www.nbb.be/en/central-credit-registers/individual-credit-register-icr>.

Importantly, *interest rates* are not recorded. For mortgages, they can be estimated (at origination) from data on the principal, the first monthly payment, the frequency of payments, and the number of payments. Assuming annuity amortization of mortgages, if F is the amount of the loan at the time of issuance (face value) and C is the monthly payment that the borrower must make until maturity (T periods later), then the interest rate r on the loan can be computed from the following formula:

$$F = \frac{C}{r} \left(1 - \frac{1}{(1+r)^t} \right). \quad (\text{A.1})$$

A.2 ES—Spain

The Spanish Credit Register (Central de Información de Riesgos del Banco de España, or CIRBE) provides monthly information on all loans, credits, bank endorsements, and general risks that financial institutions have with their customers. This detailed information is reported at the contract level and includes both the contract and borrower characteristics at the time of origination and throughout the life of the contract. Specifically, contract characteristics include the loan amount at origination, maturity, interest rate modality (adjustable versus fixed), interest rate, collateral requirements, purpose of the loan, or the bank granting the loan. For more information, see <https://www.bde.es/wbe/en/publicaciones/informes-memorias-anuales/memoria-cir/>.

A.3 HU—Hungary

The Hungarian credit register has continuously evolved: it started as a list of delinquent borrowers in 1995, and was extended to include descriptive information on all outstanding loan contracts granted by financial institutions to households in 2012. The data used in this paper come from a new administrative data source (HITREG) that financial institutions (FIs) have to provide to the Central Bank of Hungary established by MNB Directive 35/2018 (XI. 13.). FIs have to submit information on all outstanding loans in their portfolio, including information on the loans' attributes both at origination and at the time of reporting. Attributes include loan amount, origination date, duration, loan type, interest rate, formula for calculating the interest rate (base rate + premium), macroprudential measures (loan-to-value and debt service-to-income ratios), information on collateral values, and information on the borrowers (date of birth, gender) and guarantors of the loan contract. Of course, not all of these variables are applicable for all loans, and some of these variables could be missing for contracts that were originated before the establishment of the current reporting framework. For more information, see <https://aszp.mnb.hu/mnb-data-reporting/technical-assistance>.

The Hungarian database was constructed based on the June 30 and December 31 screenshots of the HITREG for the analyzed period of 2021–24, which means that the new loans with less than six months maturity are not included in the final database (less than 0.3 percent for 2022 first half-year) and neither are new loans which were terminated before the end of the half-year (less than 1.7 percent for 2022 first half-year). For the analyzed period of 2021–24, most new loans are denominated in HUF; less than 0.1 percent are denominated in EUR or CHF. We converted non-EUR loans to EUR amount on the half-year end exchange rate. Loans with zero outstanding capital credit were dropped from the sample.

A.4 IE—Ireland

Information on Ireland's Central Credit Register (CCR) is available at <https://www.centralcreditregister.ie/about-us/what-is-included-on-the-central-credit-register/>. Some data points were excluded

from the analysis due to confidentiality rules governing the use of CCR data.

A.5 IT—Italy

The main source used for data on Italian mortgages is the Bank of Italy's TAXIA (quarterly analytical sample survey of lending rates), which collects information on interest rates from a sample of banks on household clients. The sample of banks sends information on mortgage loans both at origination (among others, interest rate, TAEG, and outstanding amount) and on outstanding loans (*numeri computistici*, interest paid, charges). This data set covers the period from 2004 onward, on a quarterly basis. For the needs of this paper, from the section covering only new loans, we took the so-called TAEG (annual percentage rate of charge, i.e., the interest rate with all charges) at origination, the loan amount issued at origination, and the interest rate fixation period (IRF).

From the section covering outstanding loans, we took interest paid quarterly, the product of days and capital outstanding (*numeri computistici*, which highlights from quarter to quarter how much the debtor has to repay in capital line), as a proxy to validate the consistency of estimated interest rates and maturities.¹⁹

Importantly, TAXIA only covers exposures above EUR 75,000 per borrower–bank relationship, which limits its coverage to larger credit exposures. We use the more comprehensive Italian Central Credit Register (Centrale dei Rischi, or CR) with a threshold of EUR 30,000, managed by Banca d'Italia, only as a complementary source for mortgage loans, as it does not contain information on interest rates or interest rate fixation periods. The CR includes data from all financial institutions, covering, for the scope of this paper, monthly outstanding value (used to estimate the monthly capital repayment) and information on the presence of collateral (including type).

Maturity at origination is provided only in classes in both sources, and thus needs to be estimated. We assume annuity amortization and use the following formula, where the installment C is

¹⁹By dividing the *numeri computistici* by a number hovering near 90, we get a proxy for the outstanding capital quarterly.

interpreted as the annual payment (including both capital and interest). This value is projected from the quarterly change in outstanding amounts:

$$A = \frac{\ln(-P R / (C T - P R))}{P \ln(1 + \frac{T}{P})}, \quad (\text{A.2})$$

where

A = number of years to repay the loan,
 C = principal (loan amount at origination from TAXIA),
 T = annual interest rate (TAEG, from TAXIA),
 P = number of payment periods per year (12, as we took the delta in capital from CR from a month to another),
 R = periodic payment amount (sum of $12 \times$ capital monthly repayment from CR and $4 \times$ interest paid quarterly from TAXIA).

We used different estimates, over different periods, and cross-validated using the maturity (and IRF for fixed rate) class information provided in TAXIA and CR, which serves as a benchmark for plausibility. When values are implausible, we input the minimum value to satisfy the class of belonging (e.g., above 5 but below 10, we input 5.1). We drop all observations which do not satisfy any condition based on the default information given by the two sources. For more information, see <https://www.bancaditalia.it/statistiche/raccolta-dati/centrale-rischi/index.html>.

For consumer credit and revolving credit, we rely on data from the Consorzio Tutela del Credito (CTC), a privately managed credit register. CTC collects data from a sample of banks and other financial companies that participate, with a focus on exposures below EUR 30,000. The data set is available from December 2021 onward on a quarterly frequency and includes information on collateral and maturity. The latter is thus reported directly and does not require any estimation. Although interest rates are not available, the outstanding amount typically includes the bulk of interest paid over the life of the loan, thus we cannot get the actual capital taken as loan by the debtor. IRFs are not available. However, since we know from

other sources that consumer credit in Italy is almost always fixed rate, we assume the IRF equals the maturity.

Loan classification is based on self-explanatory variables available in each data set. In TAXIA, mortgage loans are explicitly labeled as such and identified as loans for house purchase. In the CTC data set, consumer credit is identified as loans with a declared purpose, or those labeled as personal loans or income-backed loans. Revolving credit includes credit cards and overdraft-like instruments, distinguished by their structure and repayment flexibility. No additional assumptions are required for classification, as the data sets provide direct identifiers for loan purpose and structure. As a final remark, the TAXIA-CR database managed by Bank of Italy and the CTC cannot be connected in any case, as they must be considered two distinct databases with no key in common.

A.6 *LT—Lithuania*

The Lithuanian Household Credit Register (Namų Ūkių Finansinės Stebėsenos Informacinė Sistema or NŪFSIS) is managed by the Bank of Lithuania (BoL) and provides quarterly data on credit obligations of resident natural persons (debtors) and their collateralization, information on the household composition of the debtor, income received and taxes paid, and additional details on real estate used as collateral. Data are collected at the most granular level, so that person-household-loan-creditor-collateral combinations are observed.

As there are several variables related to the purpose of the loan and the type of instrument in the register, the following assumptions are made to distinguish between credit lines, consumer credit, and housing loans:

- **Credit Lines:** We consider a loan to be a credit line if its instrument type is any of the following: “overdrafts,” “credit card debt,” “revolving credit other than overdrafts and credit card debt,” or “credit lines other than revolving credit.”
- **Consumer Credit:** We categorize a loan as consumer credit if the purpose of the loan to the household is “credit for consumption,” “credit for house purchase,” or “other credit” and its purpose is none of the following: “investment in first

residential property for owner-occupation,” “investment in residential property for rental,” “other investment in residential property,” “purchase of first residential property for owner-occupation,” “purchase of residential property for rental,” “other purchase of residential property,” and the credit is issued according to the consumer credit Directive 2008/48/EC.

- **Housing Loan:** We consider a loan to be a housing loan if the purpose of the loan to the household is “lending for house purchase”; the loan is related to real estate in accordance with the Responsible Lending Regulations (RLR), including cases where the application status of the RLR has not been reported. Also, if the purpose of the loan to the household is either “other lending” or not reported; the purpose of the loan is either “construction investment in first residential real estate for own use,” “construction investment in residential real estate for renting,” “other construction investment in residential real estate,” “purchase of first residential real estate purchase for own use,” “purchase of residential real estate for renting,” “other purchase of residential real estate”; the loan is related to real estate in accordance with Responsible Lending Regulations (RLR) including cases where the application status of RLR was not reported.

Loans that do not fall into any of these groups are categorized as “other loans” and are therefore excluded from the analysis.

A.7 LV—Latvia

The Latvian Credit Register is a national information system established and managed by Latvijas Banka, in which information is collected regarding credit obligations of individuals and legal persons, the guarantees received and provided, and late payments. The Credit Register data is provided by credit institutions, subsidiaries of credit institutions (leasing companies), other commercial companies, insurers, credit unions, JSC, Development Finance Institution Altum, and the Treasury. Information on the Credit Register is available at <https://www.bank.lv/en/operational-areas/credit-register>. See Latvijas Banka (2024) for an example of the use of this Credit

Register in policy analysis for Latvia, in particular focusing on housing lending across Latvia's regions (pp. 29–36) and refinancing costs of housing loans (box 1, pp. 11–12).

A.8 *PT—Portugal*

Information on the household credit register is available at <https://www.bportugal.pt/en/perguntas-frequentes/crc>. The Portuguese household credit register includes sole proprietorships. We exclude these borrowers from our analysis, as their loans can be used for both personal and professional use, having different risk profiles.

A.9 *SK—Slovakia*

The data on loans to households are compiled by the National Bank of Slovakia (NBS) primarily for supervisory purposes and are confidential. The NBS has the mandate to impose the reporting obligation on any entity that provides loans to the retail sector. A loan is understood as a loan provided based on a loan agreement between the client and the bank or a branch of a foreign bank, including a refinancing loan and a renegotiated loan. Only loans provided to individuals (Sector S.143, 144) are reported, excluding loans granted to individuals who are entrepreneurs. All loans that are in the bank's portfolio as of the reporting date are reported, i.e., loans for which the loan agreement has been signed by all contracting parties as of the reporting date, regardless of the actual loan disbursement. Approved loans for which the loan agreement has not been signed by all contracting parties are not reported. The data are reported on an individual basis, that is, the bank does not report retail loans provided by its subsidiary companies (e.g., installment sales and leasing companies).

Definitions of Loan Types. Mortgage loans represent (housing) loans that are secured by residential real estate, flats, or non-residential spaces. These loans are intended to acquire ownership rights to real estate intended for housing. Collateral does not necessarily have to be the subject of purchase (it might be existing land, other real estate intended for housing, construction of real estate intended for housing, etc.). Consumer loans are noncollateralized loans with amounts between EUR 100 and EUR 75,000. These loans

are not intended for the acquisition of ownership rights in real estate intended for housing. Revolving loans include consumer loans in the form of credit cards and overdrafts. New loans refer to “pure new loans.” Refinancing and renegotiated loans are excluded, as they are granted under very different circumstances than “pure new loans.”

Data Adjustments and Assumptions. Loan maturities are calculated as a difference (in months) between the date of repayment (the date of full repayment) and the drawn date (the date of the loan withdrawal) minus one month (to take into account the possibility of withdrawing the loan one month before the start of the repayment schedule). Information about the interest rate on the date of granting of revolving loans is not available. Therefore, it is replaced by the interest rate at the reporting date (at the end of June or December, respectively). Loans that were already repaid before the reporting, but for some reason were not excluded from the reports, were excluded from the stock analysis.

Appendix B. Literature Using the Underlying Microdata

The literature on the transmission of monetary policy to household credit markets is extensive, though it typically relies on aggregate or country-specific data.²⁰ Our study, by contrast, seeks to integrate insights from a diverse set of countries within a harmonized methodological framework, complementing the work of Badarınza, Campbell, and Ramadorai (2018) and De Stefani and Mano (2025), who analyze credit market dynamics using aggregated cross-country microdata.²¹ What follows is a selective overview of recent household credit-register-based studies from our sample of countries. It highlights the range of questions that household credit registers can address—but it is by no means exhaustive.

Granular data from national credit registers provide valuable insights into the distribution of household debt and lender–borrower

²⁰For instance, see Benetton, Gavazza, and Surico (2025), Beraja et al. (2018), Berger et al. (2021), Bosshardt et al. (2024), Drechsler, Savov, and Schnabl (2022), Emiriz and Koulischer (2023), Flodén et al. (2020), Indarte (2023), Lieberohn and Rothstein (2025).

²¹When it comes to corporate credit, an expanding literature exploits contract-level data across countries—much of it enabled by AnaCredit in the euro area (see, e.g., Vilerts et al. 2026 and references therein).

interactions. Using data from the Belgian household credit register, Emiris, Koulischer, and Spaenjers (2025) show that local bank competition and borrower characteristics (creditworthiness, impatience, and switching costs) significantly influence refinancing outcomes. Emiris and Koulischer (2023) use the same data set to examine the impact of interest rate changes on debt distribution and find that older households with preexisting housing wealth increased their borrowing as interest rates declined.

The Spanish Credit Register has been extensively used for research, particularly concerning nonfinancial corporations (NFCs).²² In the household sector, Mayordomo, Rachedi, and Rodríguez Moreno (2024) identify the presence of over-appraisals of mortgage collateral values aimed at reducing bank capital requirements. Bover, Crespo, and García-Urbe (2022) compare the Spanish Survey of Household Finances with the Spanish Credit Register, finding no significant differences in the levels of debt calculated from both data sets. Furthermore, this data set is frequently used in policy reports, for instance, to discuss the transmission of monetary policy to interest payments on bank debt for households and firms.

Using data from the Hungarian credit register, Verner and Gyöngyösi (2020) show the detrimental effects of the revaluation of foreign-currency-denominated mortgages (following the depreciation of the local currency in the wake of the 2008–09 financial crisis) on economic activity. They use geographic variation in the prevalence of foreign-currency-denominated mortgages to show that the revaluation has led to a worse local recession, driven by a decline in local demand, and negative spillover effects on nearby borrowers without foreign currency debt. Briglevics et al. (2024) use data from the Hungarian credit register to show that the introduction of positive information into the credit register (as opposed to the earlier practice of reporting only delinquencies) has led to an overall increase in credit access measured by both the probability of loan application acceptance and the credit amount. While credit access increased,

²²For instance, Jiménez et al. (2012, 2014) investigate the unique role of bank lending to firms in the transmission of monetary policy to the real economy. Furthermore, various aspects of NFC access to credit have been examined, such as the effects of housing booms (Martín, Moral-Benito, and Schmitz 2021) or the impact of unconventional monetary policies of the European Central Bank (Arce, Mayordomo, and Gimeno 2020).

default rates decreased after the introduction of more information in the credit register.

Dirma and Karmelavičius (2023) extensively use household-level credit data in Lithuania to assess the adequacy of borrower-based measures (BBMs). They build a novel lifetime expected credit loss framework that is founded on actual loan-level default and household income data. They show that the BBM package effectively contains mortgage credit risk and that housing loans are more resilient to stress than in the pre-regulatory era in Lithuania.

Using Portuguese Credit Register data, Farinha and Lacerda (2010) explore household indebtedness profiles, revealing significant differences in delinquency across credit products. Farinha and Costa (2011) further assess the role of foreign banks in housing loans and their impact on deleveraging during financial instability. Oliveira and Queiro (2022) analyze macroprudential policies on mortgage contracts using an overlapping generations model, showing that while loan-to-value (LTV) caps reduce mortgage debt and defaults, they also lower household welfare, particularly for low-income and low-wealth households. Bonfim and Zhao (2024) document a climate risk premium on mortgages exposed to wildfire risk and find that tighter monetary policy reduces this premium. Their results suggest that climate risk is managed primarily through pricing rather than quantities, with reduced risk sensitivity in more profitable environments.

Granular household credit register data of Slovakia, originally compiled for supervisory purposes, is increasingly utilized in macroprudential analyses and enhances macro stress testing. Micro-level data on individual retail loans improve estimates of nonperforming loans, particularly during periods of economic stress, such as the COVID-19 pandemic (Klacso 2024). Moreover, detailed retail credit data facilitate a more precise cost-benefit analysis of borrower-based measures (Cesnak, Klacso, and Vasil' 2021) as well as analyzing how the presence and role of financial advisors shape the impact of macroprudential policies on loan characteristics (Cesnak et al. 2025).

Appendix C. Summary Statistics by Product and Country

This appendix provides descriptive evidence on the structure of the granular loan-level data used in the analysis. The tables document key characteristics of newly originated household loans across nine

European countries, distinguishing between two different loan product types: *mortgages* and *consumer credit*. All statistics are derived from narrowly defined bins constructed over combinations of country, time, lender, borrower characteristics (including age), and loan features such as collateral and maturity.

Table C.1 presents the distribution of the number of new loan contracts per bin, reported separately by loan type. Each panel of the table corresponds to a distinct loan product category. For each country and product type, the table reports the total number of observations (i.e., bins with at least five contracts), along with the mean and selected percentiles of the within-bin number of new loan contracts. This serves as a measure of the representativeness and density of the underlying microdata across different country-product combinations.

Tables C.2 and C.3 offer a more detailed breakdown of within-bin summary statistics for each of the two loan product types, separately. Each table focuses on a single product: mortgages (Table C.2) or consumer credit (Table C.3). These tables present statistics across six dimensions:

- Panel A reports the average loan amount per bin.
- Panel B shows the standard deviation of loan amounts.
- Panel C provides the coefficient of variation for loan amounts, offering a scale-free measure of dispersion.
- Panel D presents the average interest rate within each bin.
- Panel E gives the standard deviation of interest rates.
- Panel F reports the coefficient of variation for interest rates.

All monetary values are expressed in euros, while interest rates are shown in percentage terms. For each panel, the number of bins, as well as measures of central tendency and dispersion (mean and percentiles), are reported at the country level. The breakdown by product type and by panel enables comparisons in loan size and pricing heterogeneity across products and countries.

Table C.1. Number of Observations: By Product and Country

	Count	Mean	p5	p10	p25	p50	p75	p90	p95
	A. Number of Observations—Mortgages								
BE	126	4,432	9	22	208	1,416	6,078	11,835	19,387
ES	508	2,360	9	12	55	379	1,193	6,837	12,288
HU	392	431	6	11	30	177	638	1,245	1,541
IE	249	430	10	16	44	137	498	1,181	1,947
IT	726	1,084	6	8	16	80	360	1,793	5,386
LT	94	534	12	14	36	89	474	1,926	3,824
LV	174	149	5	6	11	46	114	470	792
PT	423	799	9	11	30	89	471	2,002	4,498
SK	308	779	6	9	15	58	458	1,688	2,871
	B. Number of Observations—Consumer Credit								
BE	235	7,265	65	163	1,092	3,736	10,351	20,383	24,136
ES	906	18,158	9	15	78	786	15,665	72,918	95,554
HU	844	2,177	8	12	32	368	1,190	7,441	13,739
IE	398	5,764	19	32	144	1,179	8,608	19,094	29,123
IT	643	21,164	20	33	120	4,930	25,340	64,450	101,314
LT	606	1,717	11	14	29	146	2,354	5,207	7,155
LV	626	1,138	6	10	34	506	1,615	2,998	3,659
PT	1,209	2,076	8	12	39	239	1,025	6,207	11,505
SK	261	4,018	7	11	268	1,811	4,923	9,166	23,967

Table C.2. Mortgages: Summary Statistics by Country

	Count	Mean	p5	p10	p25	p50	p75	p90	p95
A. Average Amount									
BE	126	172,429	117,087	118,302	133,845	169,069	194,047	252,048	262,869
ES	508	114,114	46,313	54,595	76,244	115,689	151,880	167,623	179,748
HU	392	34,769	8,628	10,351	14,304	32,533	47,228	62,026	70,693
IE	249	186,817	80,303	98,080	137,896	185,998	236,787	283,527	300,847
IT	726	156,183	109,180	116,180	128,434	145,514	169,116	210,469	251,524
LT	94	73,369	30,149	31,932	47,577	74,693	100,177	116,318	118,986
LV	174	75,483	31,163	35,110	44,778	69,738	99,366	127,048	141,133
PT	423	74,913	33,388	40,045	58,005	77,283	91,988	103,543	108,806
SK	308	71,200	30,409	34,275	43,986	67,695	94,854	113,339	121,812
B. SD Amount									
BE	126	132,409	83,153	93,462	109,801	127,258	159,803	176,381	191,351
ES	508	79,938	36,693	43,803	59,026	81,397	98,603	113,027	121,141
HU	392	22,931	5,988	7,067	10,012	22,004	31,252	41,953	49,639
IE	249	120,525	50,881	59,949	82,337	116,698	150,622	183,376	200,583
IT	726	93,772	40,127	47,408	63,311	84,714	113,281	150,039	182,191
LT	94	49,580	15,183	25,810	33,957	48,429	61,189	76,040	82,343
LV	174	42,369	13,839	15,875	27,668	41,614	56,400	68,307	75,779
PT	423	53,853	28,929	36,842	46,727	53,795	61,068	69,032	73,728
SK	308	49,869	18,745	22,788	36,733	48,470	62,241	74,958	84,213
C. Coefficient of Variation—Amount									
BE	126	0.78	0.52	0.56	0.65	0.76	0.93	1.05	1.10
ES	508	0.80	0.37	0.42	0.54	0.67	0.89	1.53	1.82
HU	392	0.69	0.35	0.43	0.53	0.67	0.80	0.98	1.13
IE	249	0.68	0.40	0.44	0.52	0.63	0.77	0.95	1.12
IT	726	0.59	0.33	0.37	0.45	0.58	0.69	0.82	0.93
LT	94	0.70	0.48	0.50	0.55	0.68	0.78	0.92	1.09
LV	174	0.58	0.31	0.37	0.45	0.57	0.69	0.82	0.89
PT	423	0.76	0.51	0.53	0.61	0.72	0.87	1.05	1.25
SK	308	0.73	0.41	0.46	0.57	0.71	0.85	1.02	1.13

(continued)

Table C.2. (Continued)

	Count	Mean	p5	p10	p25	p50	p75	p90	p95
	<i>D. Average Interest Rate</i>								
BE	99	3.43	1.67	1.83	2.89	3.46	3.90	4.69	5.56
ES	508	3.11	1.41	1.63	2.50	3.15	3.79	4.35	4.75
HU	392	7.65	4.66	5.11	6.10	7.13	8.37	10.87	13.49
IE	249	3.64	2.42	2.50	2.89	3.81	4.22	4.44	4.53
IT	726	3.93	1.74	2.06	3.02	4.20	4.84	5.43	5.66
LT	94	4.78	2.38	2.67	3.73	5.41	5.88	6.23	6.53
LV	173	4.74	2.02	2.52	3.97	5.11	5.70	5.94	6.19
PT	423	3.47	1.13	2.13	3.08	3.73	4.12	4.41	4.59
SK	308	3.64	1.29	1.44	2.95	4.07	4.59	4.91	5.10
	<i>E. SD Interest Rate</i>								
BE	99	1.43	0.51	0.55	0.62	1.09	1.78	2.37	3.10
ES	508	0.97	0.40	0.51	0.74	0.96	1.21	1.38	1.54
HU	392	1.46	0.42	0.51	0.74	1.31	2.05	2.57	3.04
IE	249	0.61	0.29	0.33	0.43	0.53	0.71	1.00	1.23
IT	726	0.94	0.52	0.61	0.74	0.91	1.11	1.33	1.46
LT	94	1.08	0.57	0.66	0.80	0.98	1.28	1.53	1.81
LV	172	0.58	0.23	0.30	0.44	0.56	0.70	0.90	0.95
PT	423	0.84	0.26	0.29	0.46	0.73	1.07	1.68	1.85
SK	308	0.68	0.27	0.31	0.44	0.60	0.81	1.21	1.44
	<i>F. Coefficient of Variation—Interest Rate</i>								
BE	99	0.41	0.15	0.16	0.22	0.35	0.53	0.73	0.87
ES	508	0.34	0.12	0.17	0.24	0.31	0.42	0.55	0.62
HU	392	0.21	0.06	0.07	0.09	0.17	0.28	0.41	0.49
IE	249	0.17	0.09	0.10	0.12	0.15	0.20	0.26	0.31
IT	726	0.26	0.13	0.14	0.19	0.24	0.34	0.40	0.44
LT	94	0.25	0.10	0.12	0.15	0.23	0.35	0.41	0.48
LV	172	0.14	0.04	0.06	0.08	0.12	0.17	0.25	0.27
PT	423	0.27	0.07	0.08	0.13	0.22	0.38	0.53	0.63
SK	308	0.23	0.06	0.07	0.11	0.18	0.29	0.42	0.61

Table C.3. Consumer Credit: Summary Statistics by Country

	Count	Mean	p5	p10	p25	p50	p75	p90	p95
A. Average Amount									
BE	235	23,709	1,484	2,047	4,715	19,554	26,277	62,820	76,832
ES	906	19,883	742	1,407	7,113	16,534	24,142	36,143	55,428
HU	844	8,290	404	626	1,013	3,827	10,904	25,963	27,492
IE	398	21,264	2,307	4,449	10,681	21,236	25,859	29,205	42,278
IT	643	17,015	724	1,020	3,626	11,416	26,032	40,313	50,128
LT	606	9,035	602	755	1,377	7,241	15,485	21,073	22,109
LV	626	4,849	489	564	970	3,802	6,837	10,083	14,220
PT	1,209	15,358	1,991	2,545	6,933	12,449	16,065	29,061	45,423
SK	261	8,975	754	816	1,234	5,436	17,380	21,282	22,380
B. SD Amount									
BE	235	14,908	2,329	3,740	6,013	14,336	22,299	29,089	32,746
ES	906	15,796	917	2,201	6,604	10,563	19,506	38,400	49,513
HU	844	3,818	216	315	616	2,465	5,495	10,295	11,764
IE	398	14,488	3,144	3,844	6,413	12,173	14,609	20,074	55,612
IT	643	10,229	602	1,276	3,359	9,458	15,800	21,029	22,449
LT	606	5,185	867	1,038	1,576	4,796	8,069	9,862	10,673
LV	626	2,883	402	464	1,015	2,788	4,509	5,757	6,322
PT	1,209	13,809	1,647	2,387	5,729	8,216	14,817	34,035	48,847
SK	261	6,081	862	998	1,408	5,959	9,858	11,475	13,122
C. Coefficient of Variation—Amount									
BE	235	1.12	0.29	0.37	0.55	0.83	1.35	2.36	2.81
ES	906	1.07	0.35	0.39	0.56	0.83	1.29	1.90	2.69
HU	844	0.66	0.10	0.24	0.44	0.65	0.84	1.09	1.27
IE	398	0.75	0.41	0.46	0.53	0.62	0.81	1.09	1.38
IT	643	0.86	0.39	0.44	0.57	0.71	0.92	1.29	1.45
LT	606	0.84	0.39	0.43	0.50	0.73	1.03	1.34	1.85
LV	626	0.82	0.33	0.43	0.62	0.82	1.01	1.22	1.35
PT	1,209	0.99	0.42	0.47	0.60	0.84	1.04	1.43	1.91
SK	261	0.96	0.47	0.49	0.63	0.92	1.25	1.46	1.56

(continued)

Table C.3. (Continued)

	Count	Mean	p5	p10	p25	p50	p75	p90	p95
<i>D. Average Interest Rate</i>									
BE	884	7.32	2.53	3.60	Not Recorded	7.80	8.93	10.06	10.95
ES	843	17.22	8.02	8.78	12.16	15.94	22.29	25.60	28.97
HU	398	7.52	4.97	5.55	6.51	7.78	8.52	8.97	9.30
IE					Not Recorded				
IT									
LT	543	13.51	3.24	4.38	6.68	10.21	17.61	25.83	37.72
LV	625	20.11	1.44	4.89	13.40	20.40	26.66	34.80	40.77
PT	1,209	6.77	1.56	3.30	5.33	7.33	8.48	9.42	9.98
SK	261	10.66	3.26	3.92	6.41	10.45	14.49	17.03	19.40
<i>E. SD Interest Rate</i>									
BE	883	2.85	0.52	0.94	Not Recorded	2.83	3.90	4.96	5.77
ES	839	4.15	0.57	0.86	1.68	3.59	6.03	8.61	10.09
HU	398	2.60	1.43	1.69	1.98	2.41	3.01	3.78	4.43
IE					Not Recorded				
IT									
LT	537	6.25	0.60	0.75	1.43	3.21	8.94	18.08	22.93
LV	625	8.05	0.26	0.43	3.02	7.48	11.94	16.45	17.63
PT	1,209	2.39	0.94	1.32	1.68	2.12	3.08	4.04	4.40
SK	261	5.53	0.36	0.58	0.97	3.95	8.67	13.32	15.75
<i>F. Coefficient of Variation—Interest Rate</i>									
BE	883	0.46	0.07	0.15	Not Recorded	0.38	0.48	0.87	1.35
ES	839	0.23	0.06	0.08	0.13	0.21	0.30	0.40	0.48
HU	398	0.37	0.18	0.20	0.24	0.31	0.47	0.57	0.66
IE					Not Recorded				
IT									
LT	537	0.39	0.10	0.12	0.19	0.39	0.53	0.68	0.78
LV	625	0.40	0.05	0.10	0.23	0.35	0.55	0.71	0.90
PT	1,208	0.55	0.14	0.17	0.21	0.30	0.51	1.03	2.00
SK	261	0.45	0.07	0.10	0.19	0.38	0.65	0.88	0.96

Appendix D. Aggregate Household Credit Developments

This appendix presents graphical evidence on the evolution of household credit across nine European countries during the period 2022–24. The figures offer a visual summary of key features of new loan origination, disaggregated by product type and country.

Figures D.1–D.9 display developments in new mortgage and consumer loan contracts for each country separately. For each country, four panels are shown:

- The top-left panel reports the time series of the weighted average amount and interest rate for newly originated mortgage loans.
- The top-right panel presents the same two variables for new consumer credit.
- The bottom-left panel shows the total volume (in million euros) and the number of new mortgage contracts over time.
- The bottom-right panel provides the corresponding totals for consumer credit.

Each panel covers semiannual periods from the first half of 2022 through the first half of 2024 (with data for the second half of 2024 included where available). This layout facilitates both cross-country and within-country comparisons of household borrowing trends over time.

All figures are based on aggregated data computed from highly disaggregated bins defined by time, country, loan type, creditor, borrower age, initial maturity, and interest rate fixation period. The plotted statistics correspond to either weighted averages or totals, as noted in the figure labels and axis annotations.

The sequence of figures is Belgium (D.1), Spain (D.2), Hungary (D.3), Ireland (D.4), Italy (D.5), Lithuania (D.6), Latvia (D.7), Portugal (D.8), and Slovakia (D.9).

Figure D.1. Household Credit in Belgium during 2022–24

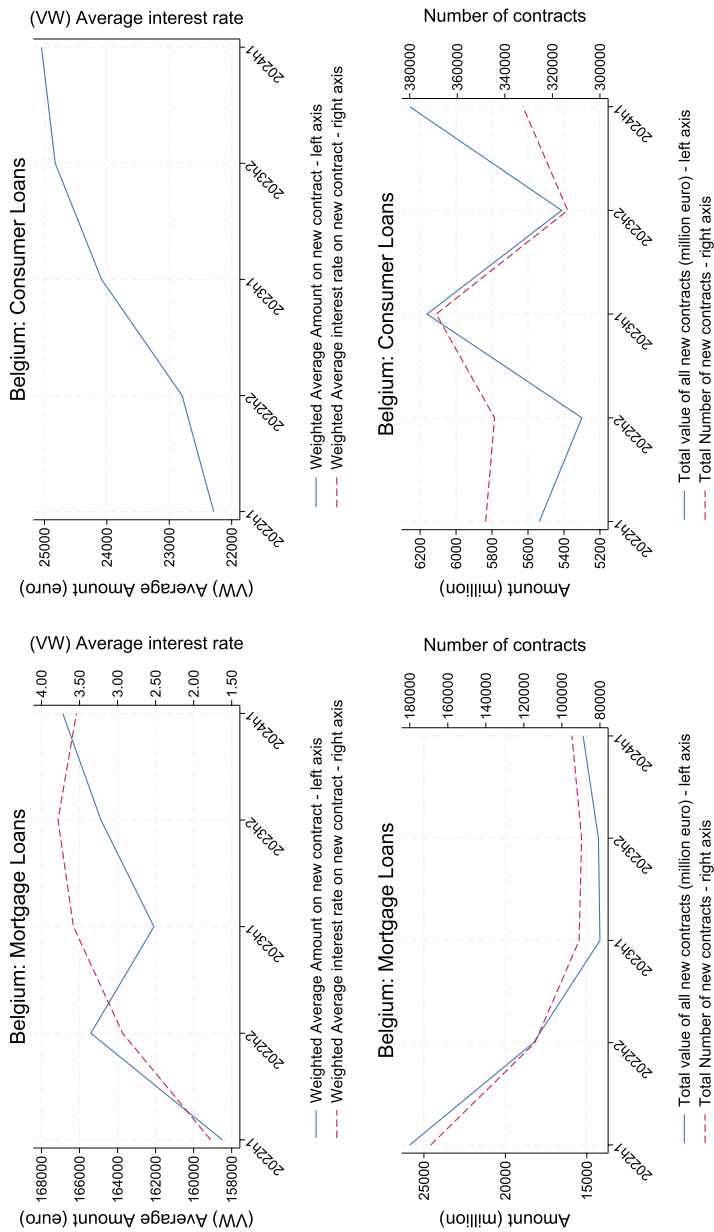


Figure D.2. Household Credit in Spain during 2022–24

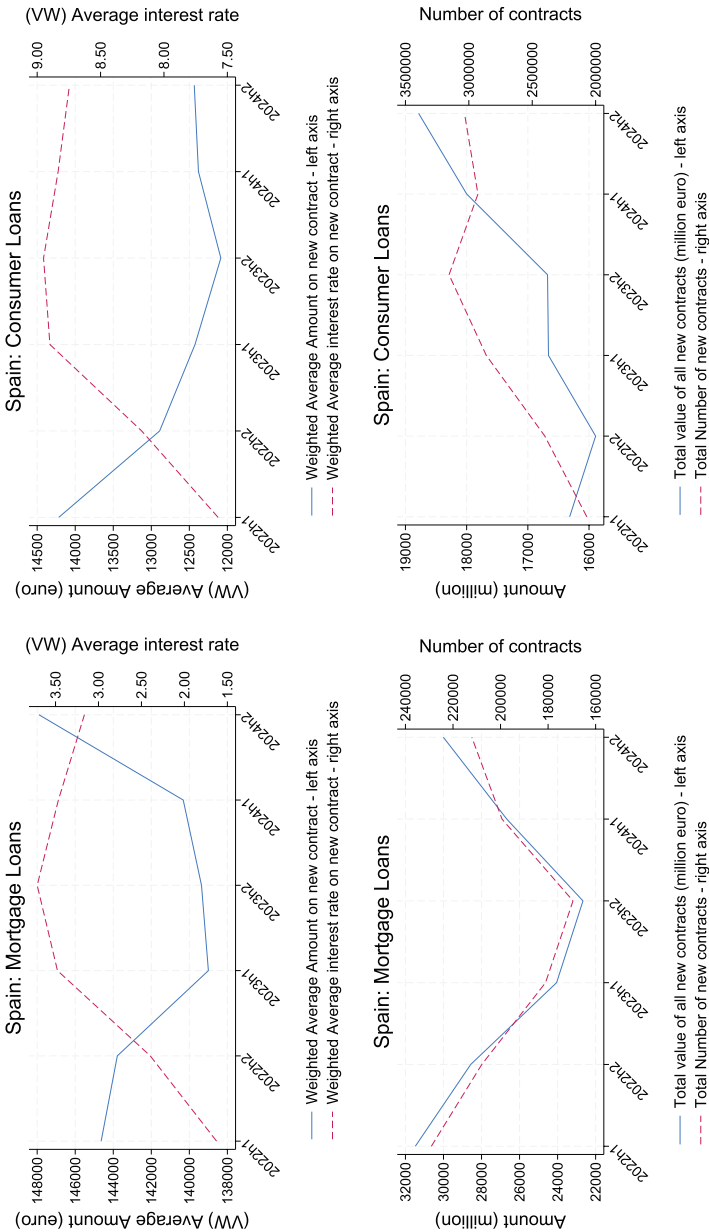


Figure D.3. Household Credit in Hungary during 2022–24

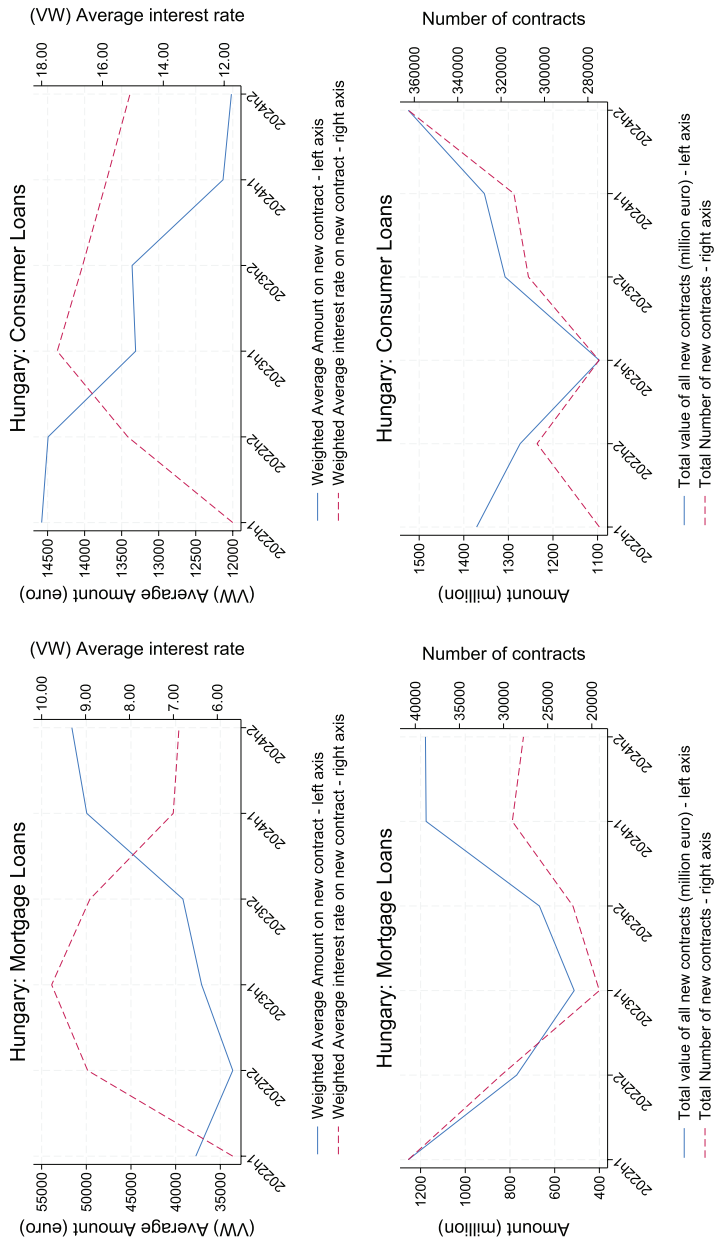


Figure D.4. Household Credit in Ireland during 2022–24

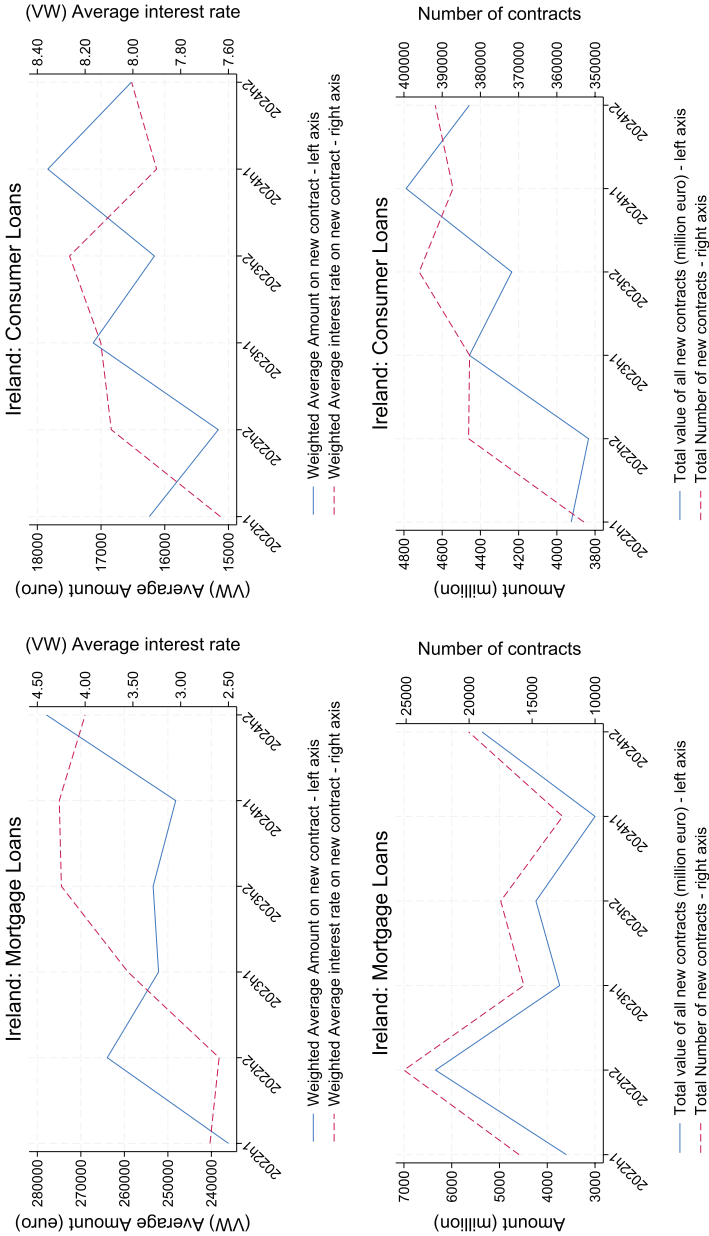


Figure D.5. Household Credit in Italy during 2022–24

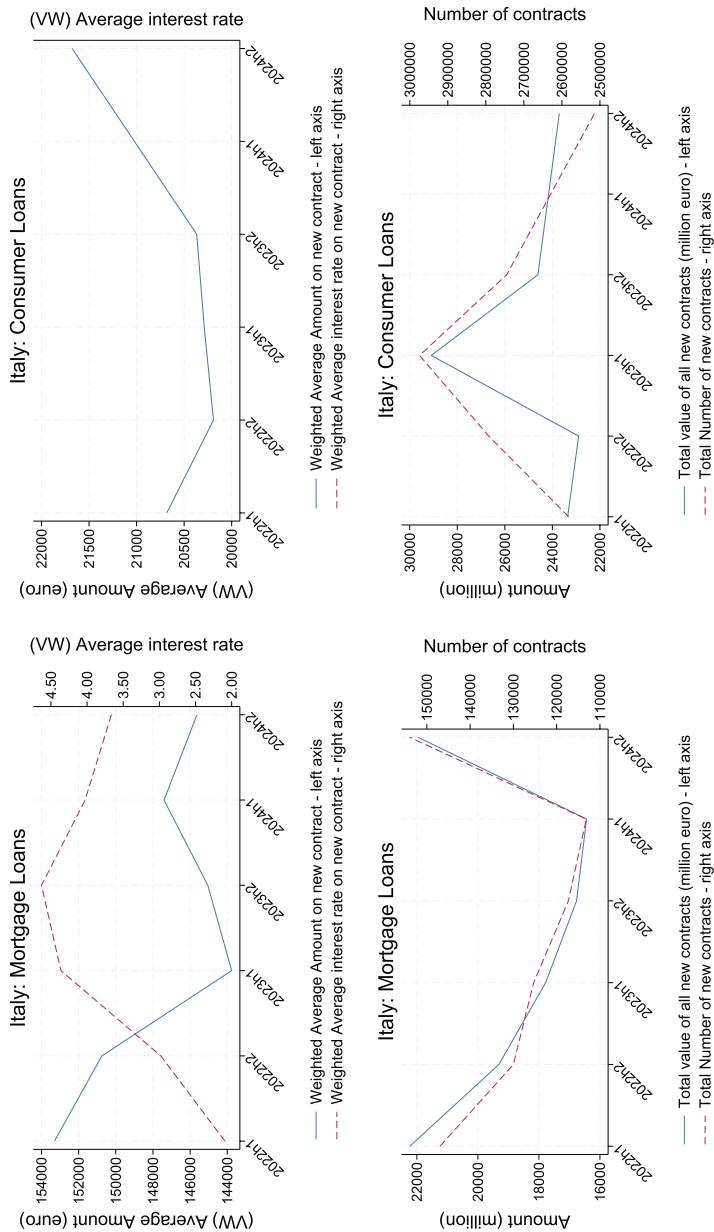


Figure D.6. Household Credit in Lithuania during 2022–24

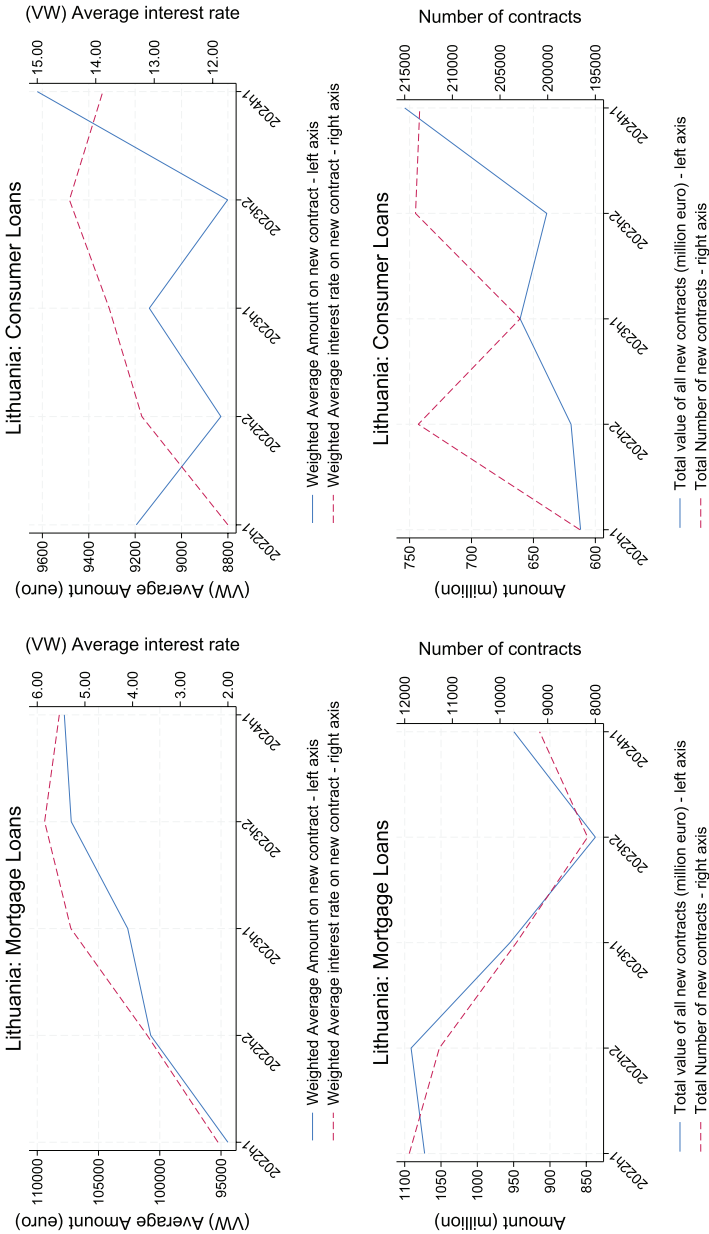


Figure D.7. Household Credit in Latvia during 2022–24

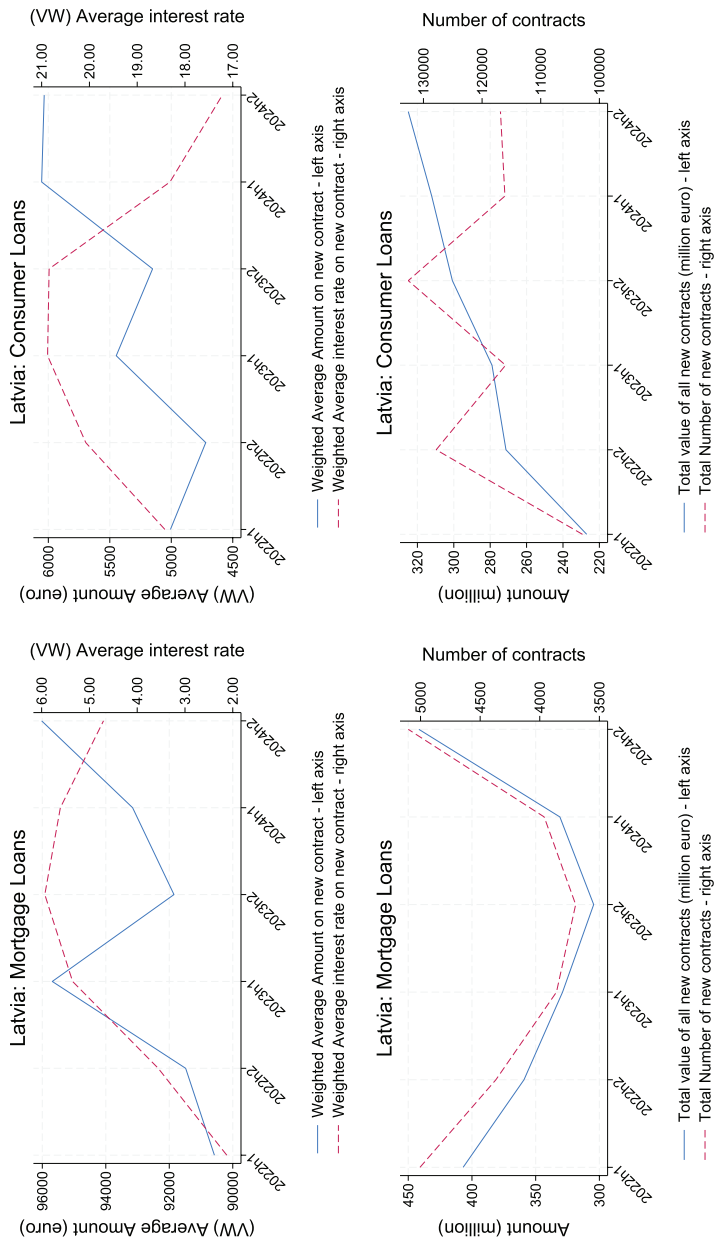


Figure D.8. Household Credit in Portugal during 2022–24

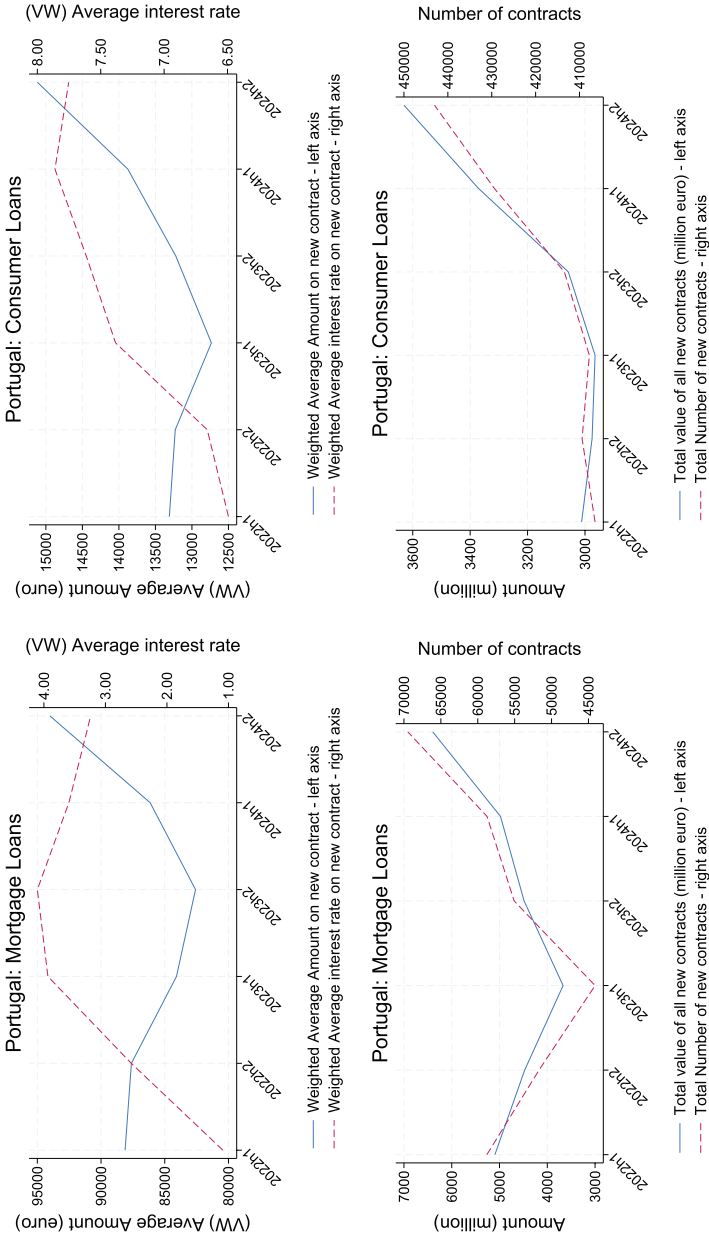
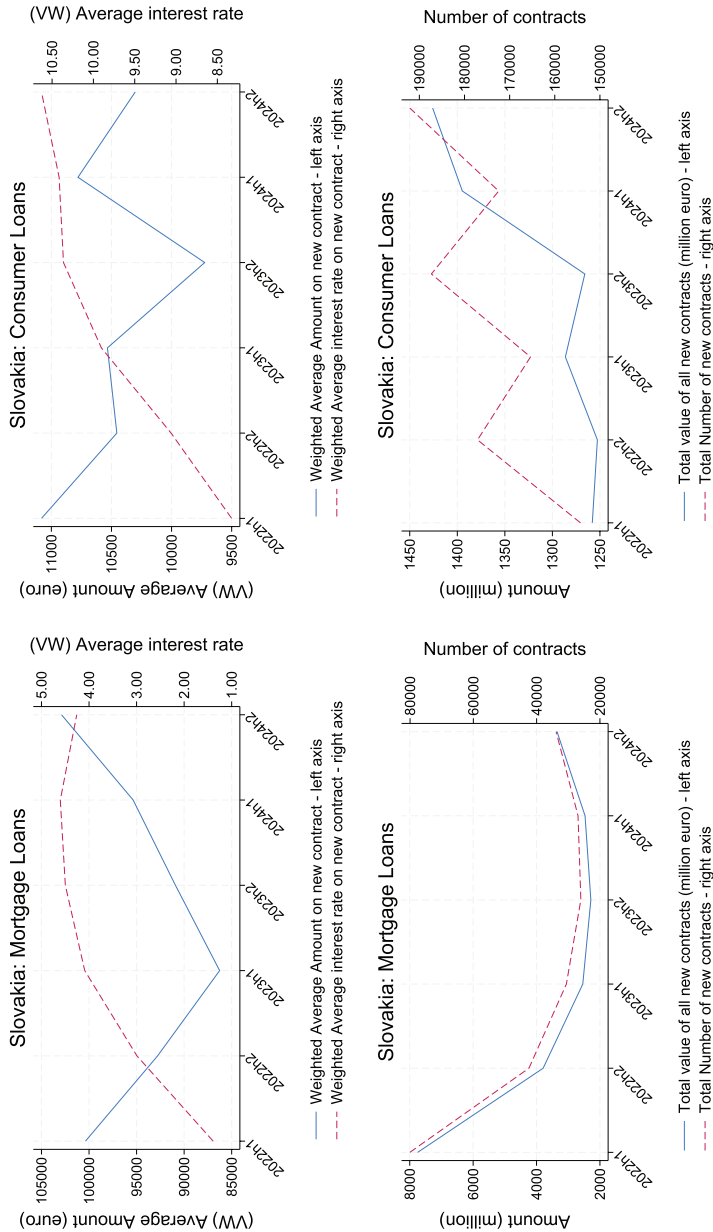


Figure D.9. Household Credit in Slovakia during 2022–24



Appendix E. Household Credit: Split by Initial Maturity

Figures E.1–E.9 display the evolution of household loan characteristics, disaggregated by the initial maturity of the loan. Each figure corresponds to a specific country and covers the semiannual periods from 2022:H1 through 2024:H2.

For each country, the figure is structured into four panels:

- The top-left panel shows the average interest rate on newly originated *mortgage loans*, split by maturity group.
- The top-right panel presents the same statistic for new *consumer loans*.
- The bottom-left panel reports the average amount of new *mortgage loans*, again disaggregated by maturity group.
- The bottom-right panel shows the average amount of new *consumer loans* by maturity.

The maturity groups are defined as follows:

- For mortgage loans: *Short* (≤ 10 years), *Medium* (10–20 years), *Long* (20–30 years), and *Very Long* (> 30 years);
- For consumer loans: *Short* (≤ 12 months), *Medium* (1–3 years), *Long* (3–10 years), and *Very Long* (> 10 years).

The figures provide a dynamic view of how the pricing (interest rates) and scale (loan amounts) of new household borrowing vary over time and across maturity segments. This breakdown allows for the inspection of potential shifts in the distribution of new lending, as well as the differential response of short- and long-term credit to changes in economic or financial conditions.

Countries included in this series are Belgium (E.1), Spain (E.2), Hungary (E.3), Ireland (E.4), Italy (E.5), Lithuania (E.6), Latvia (E.7), Portugal (E.8), and Slovakia (E.9).

Figure E.1. Maturity Breakdown: Belgium

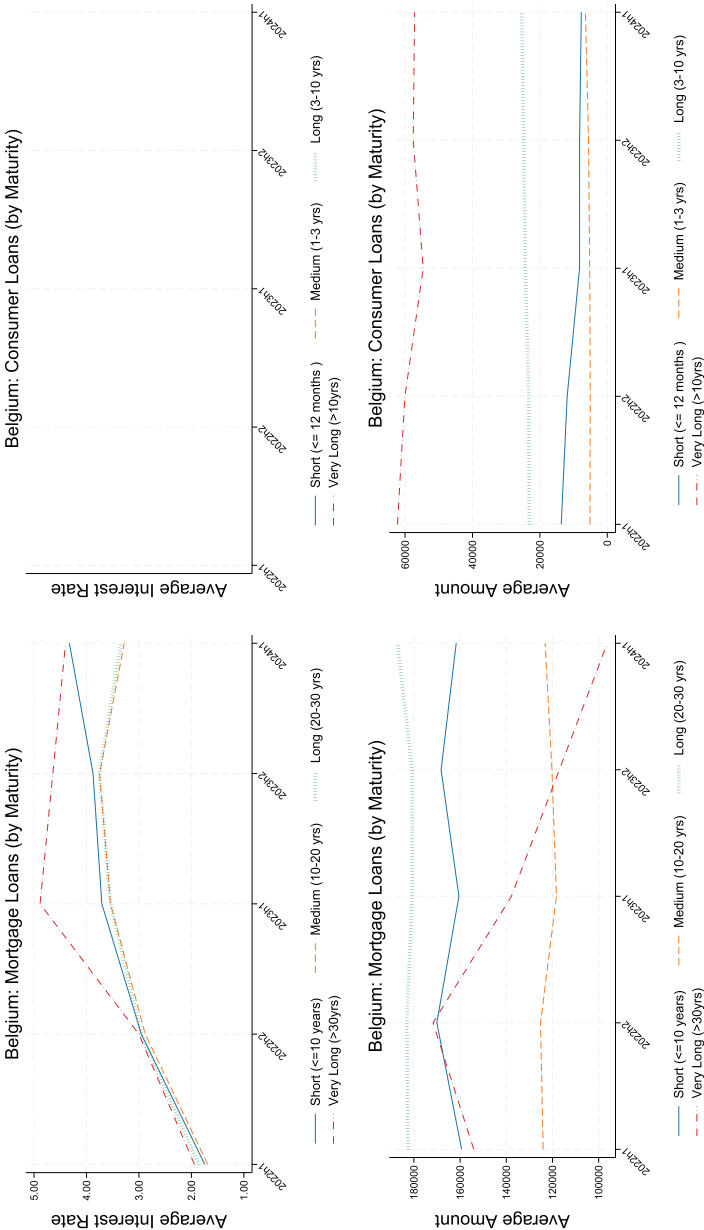


Figure E.2. Maturity Breakdown: Spain

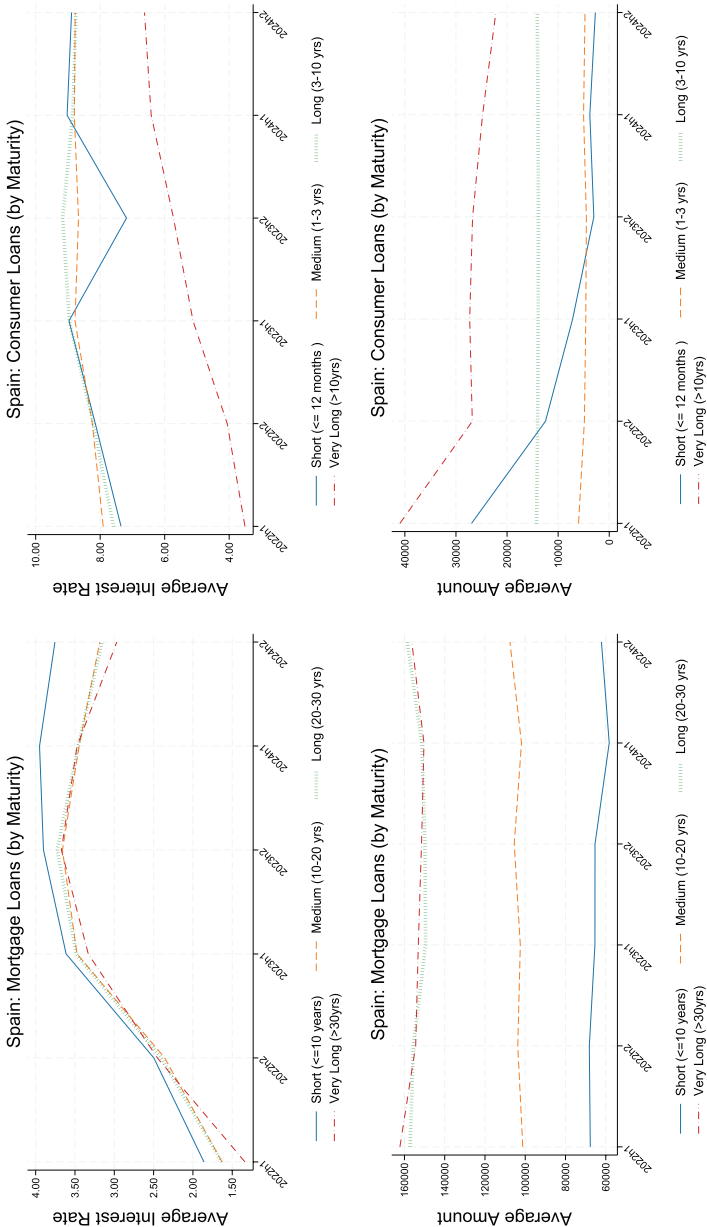


Figure E.3. Maturity Breakdown: Hungary

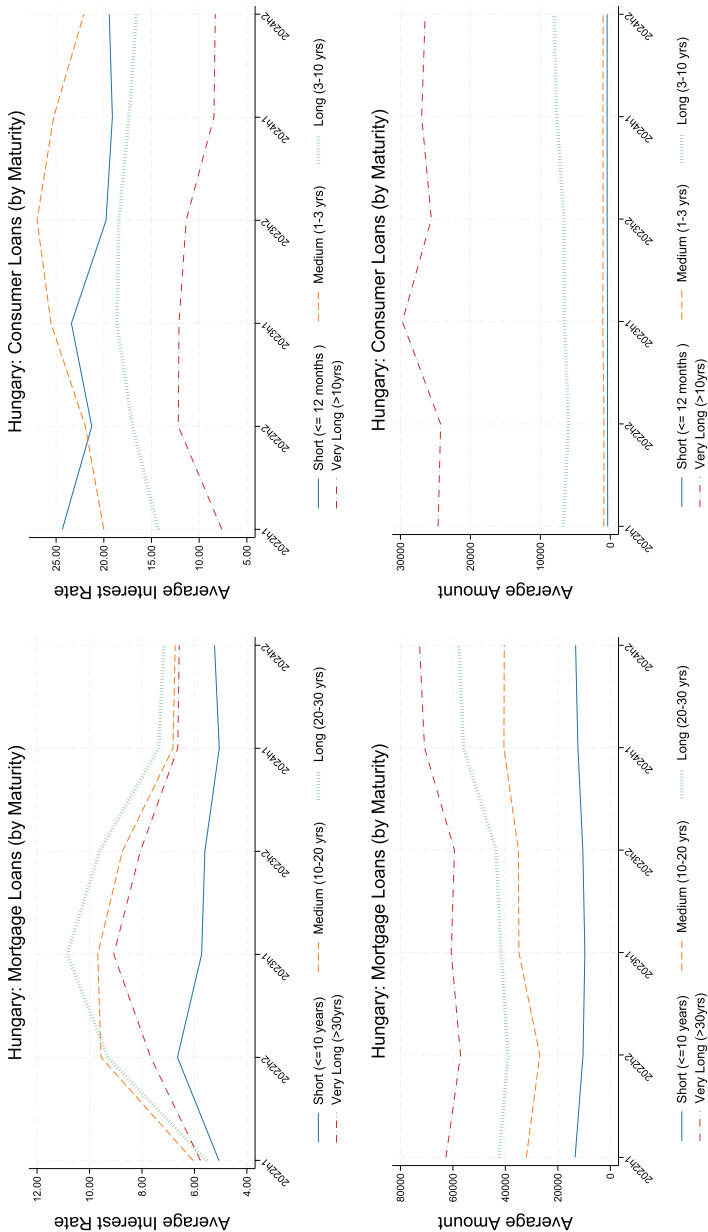


Figure E.4. Maturity Breakdown: Ireland

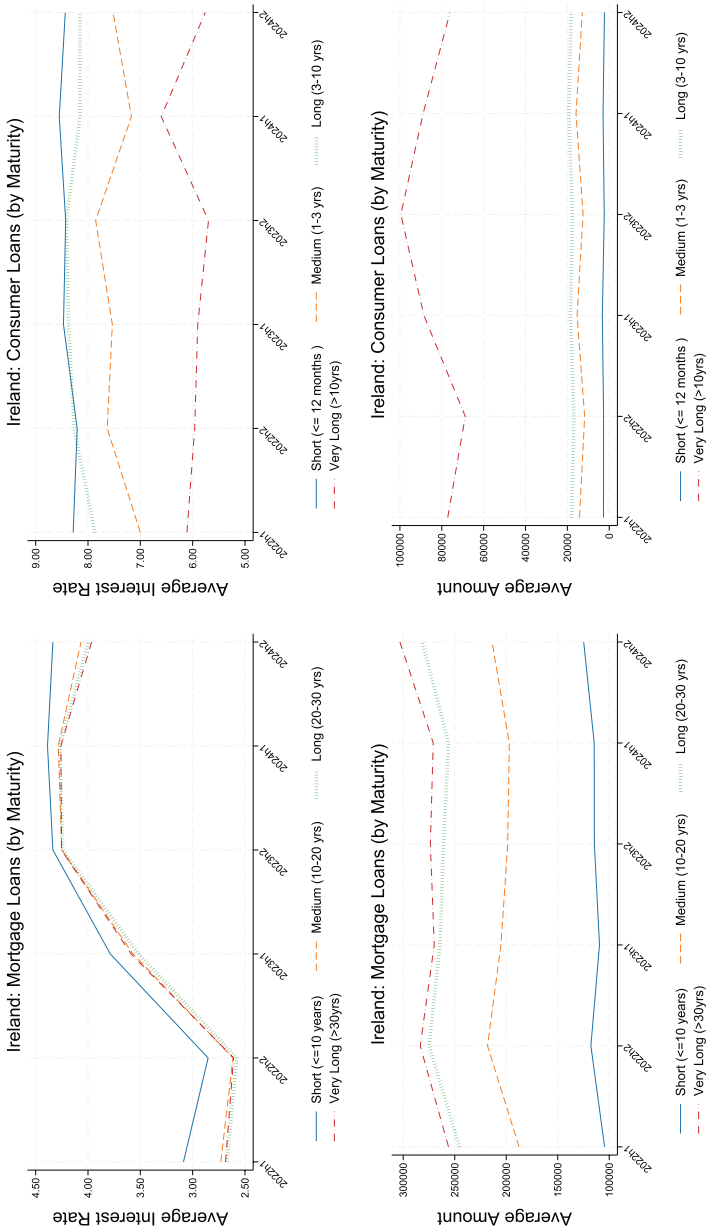


Figure E.5. Maturity Breakdown: Italy

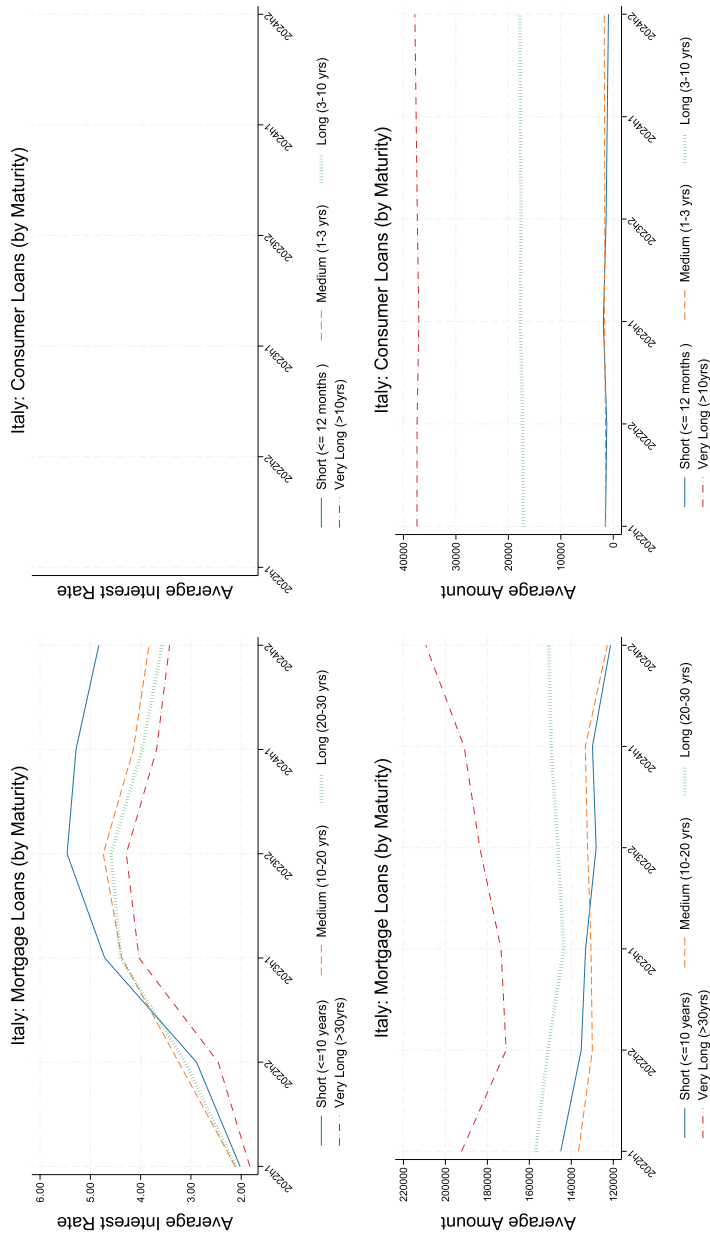


Figure E.6. Maturity Breakdown: Lithuania

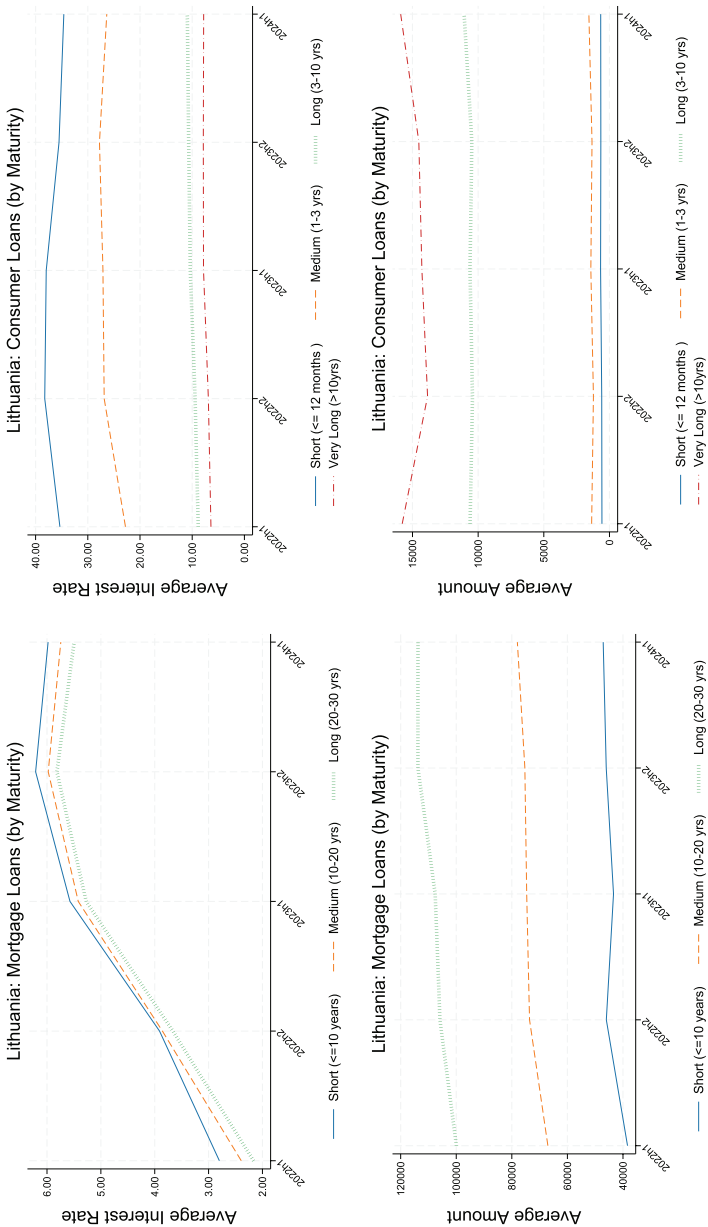


Figure E.7. Maturity Breakdown: Latvia

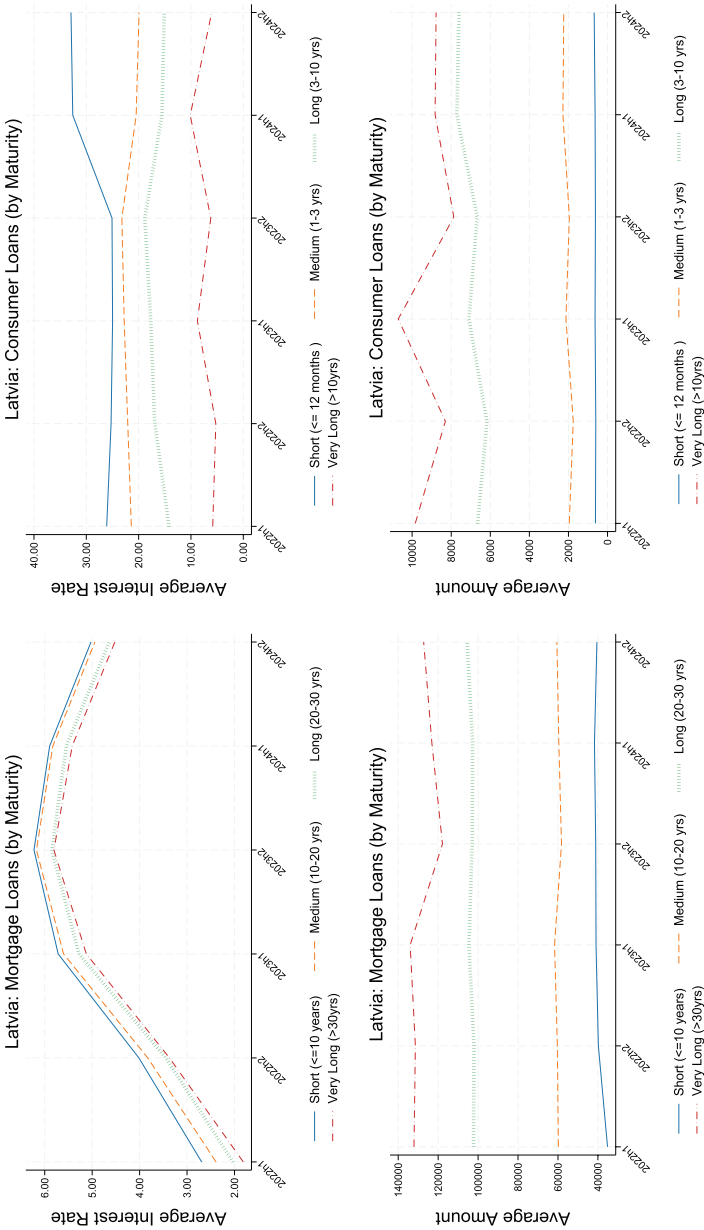


Figure E.8. Maturity Breakdown: Portugal

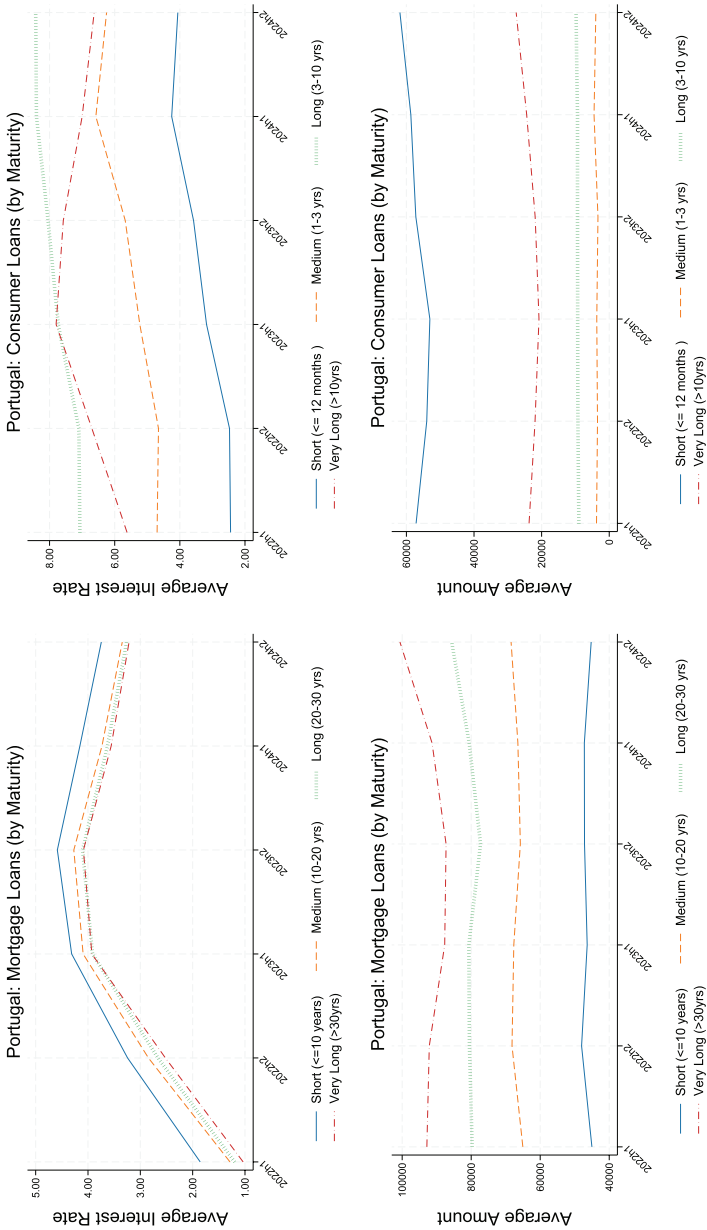
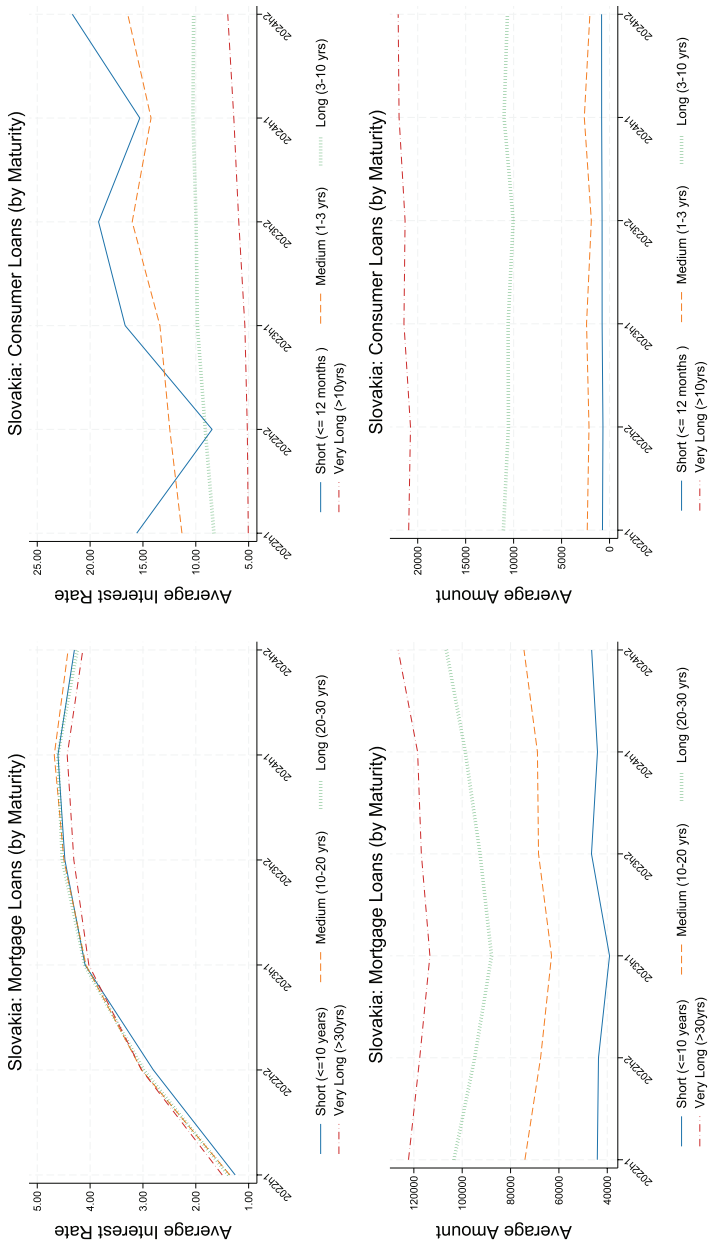


Figure E.9. Maturity Breakdown: Slovakia



Appendix F. Household Credit: Split by Borrower Age

Figures F.1–F.9 present a breakdown of new household loans by the age group of the borrower. Each figure corresponds to a single country and covers the period from 2022:H1 to 2024:H2.

For each country, the figure is structured into four panels:

- The top-left panel shows the average interest rate on newly originated *mortgage loans*, disaggregated by borrower age.
- The top-right panel presents the same statistic for new *consumer loans*.
- The bottom-left panel reports the average loan amount for new *mortgage loans* by borrower age.
- The bottom-right panel shows the average amount of new *consumer loans* by age group.

Borrowers are grouped into six age categories: *Under 25*, *25–34*, *35–44*, *45–54*, *55–64*, and *65 and older*. The graphical layout enables an assessment of how borrowing terms and loan sizes differ across age cohorts and evolve over time.

These figures facilitate comparisons of both price and volume of household credit across age groups, helping to uncover demographic patterns in credit market access, demand, and pricing. The countries included are Belgium (F.1), Spain (F.2), Ireland (F.3), Italy (F.4), Hungary (F.5), Lithuania (F.6), Latvia (F.7), Portugal (F.8), and Slovakia (F.9).

Figure F.1. Age Breakdown: Belgium

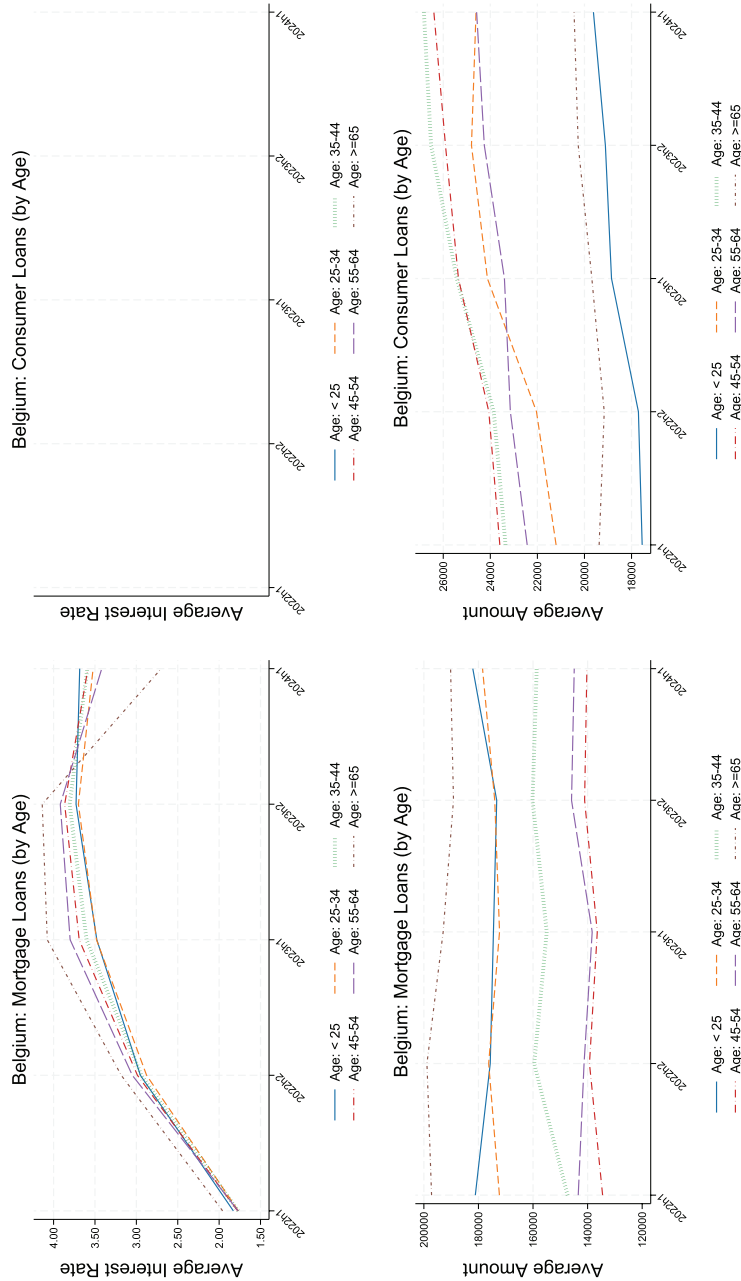


Figure F.2. Age Breakdown: Spain

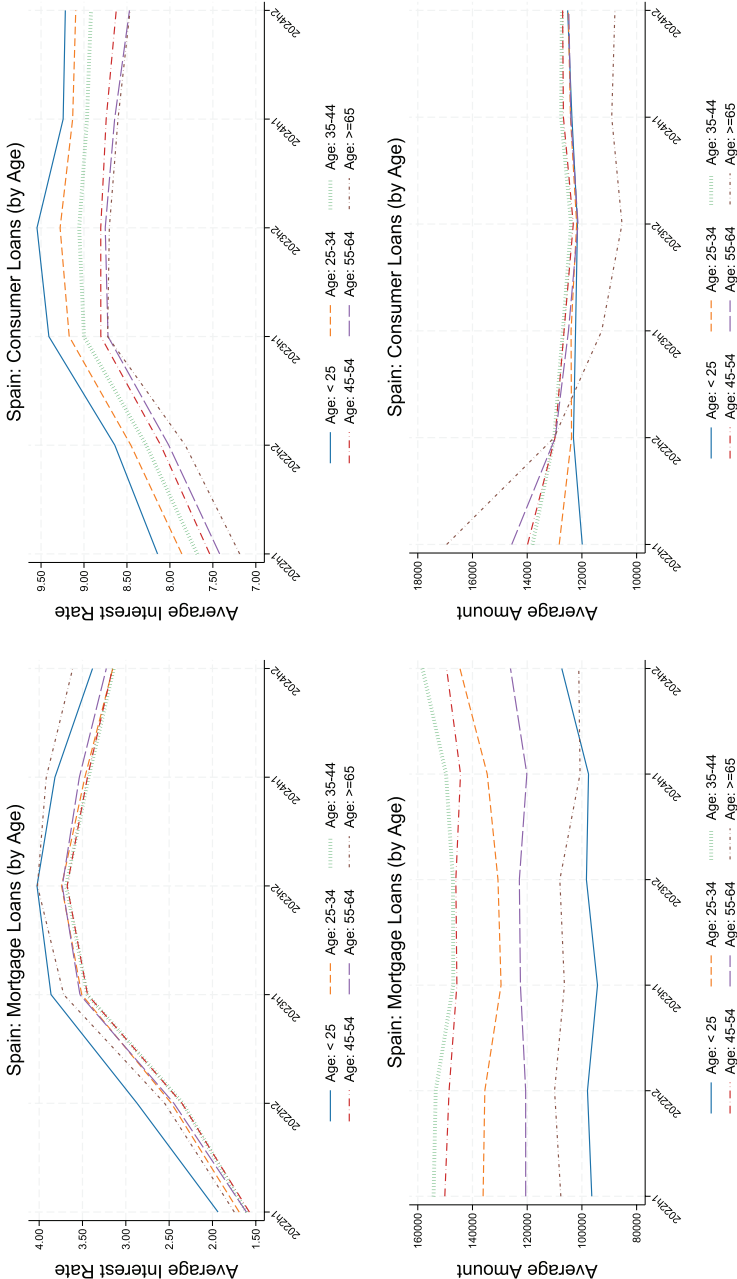


Figure F.3. Age Breakdown: Ireland

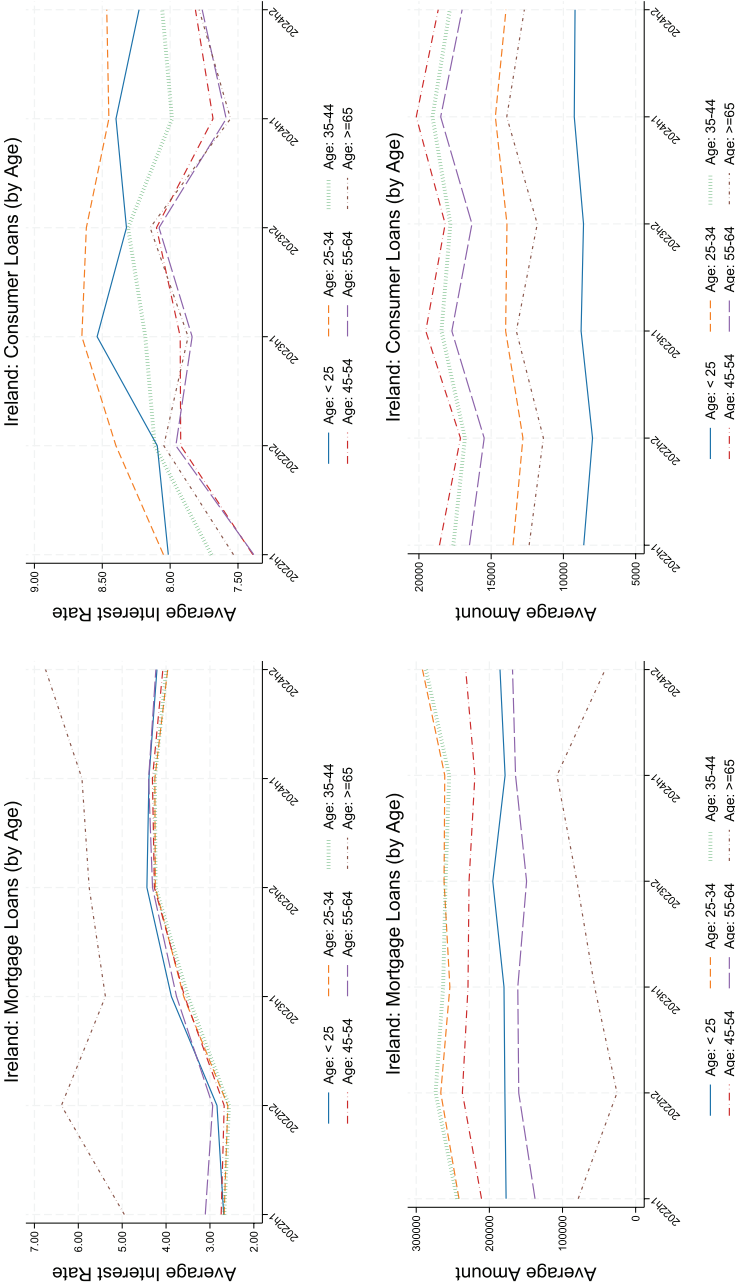


Figure F.4. Age Breakdown: Italy

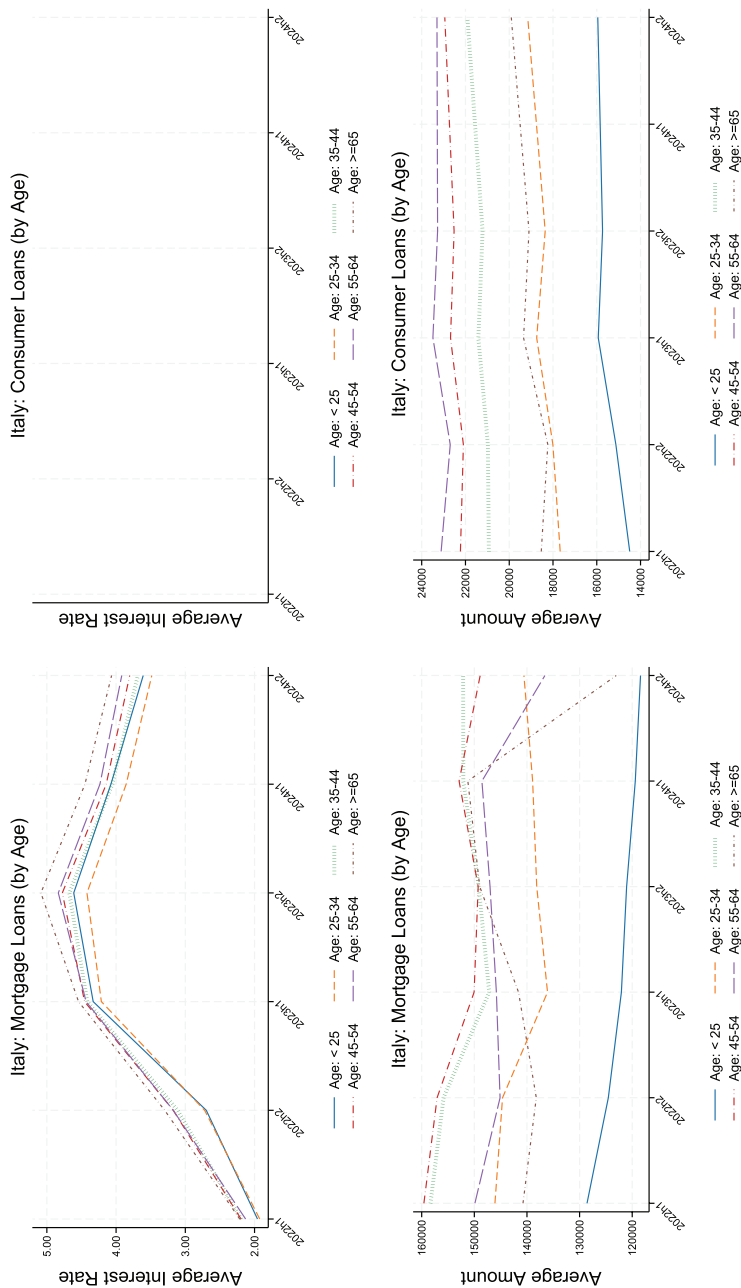


Figure F.5. Age Breakdown: Hungary

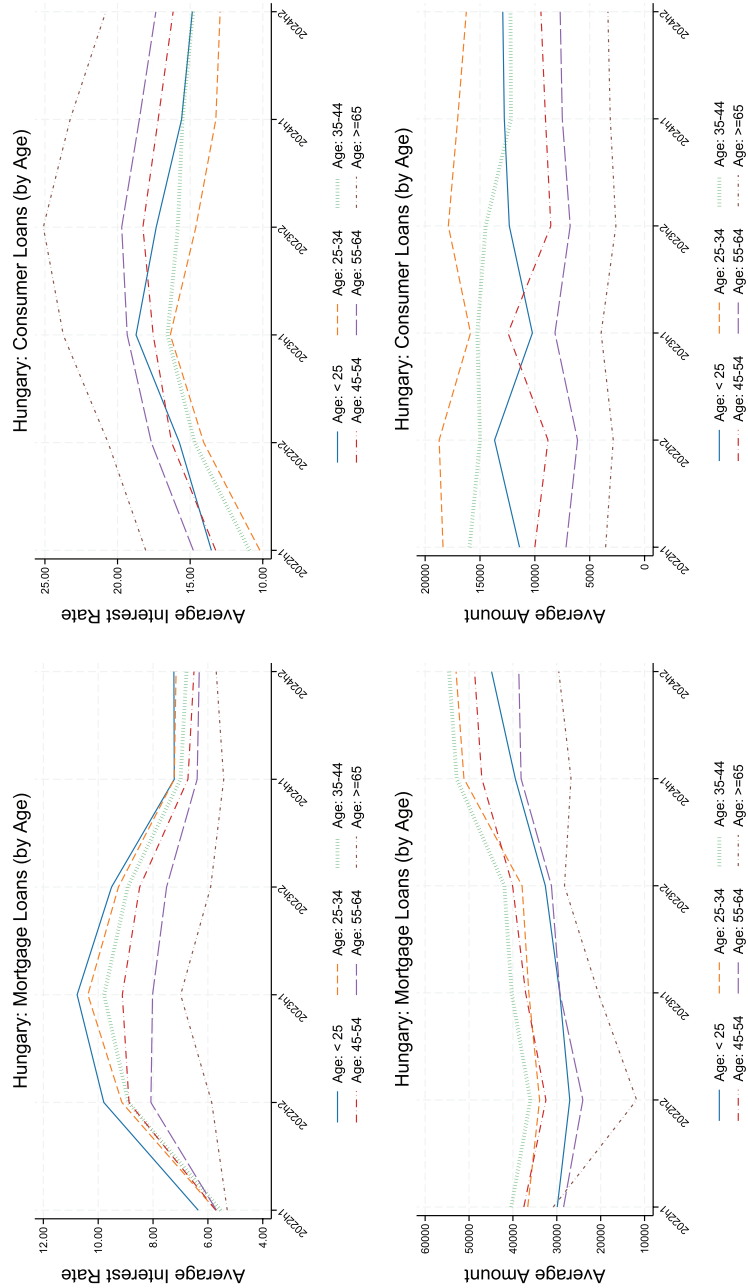


Figure F.6. Age Breakdown: Lithuania

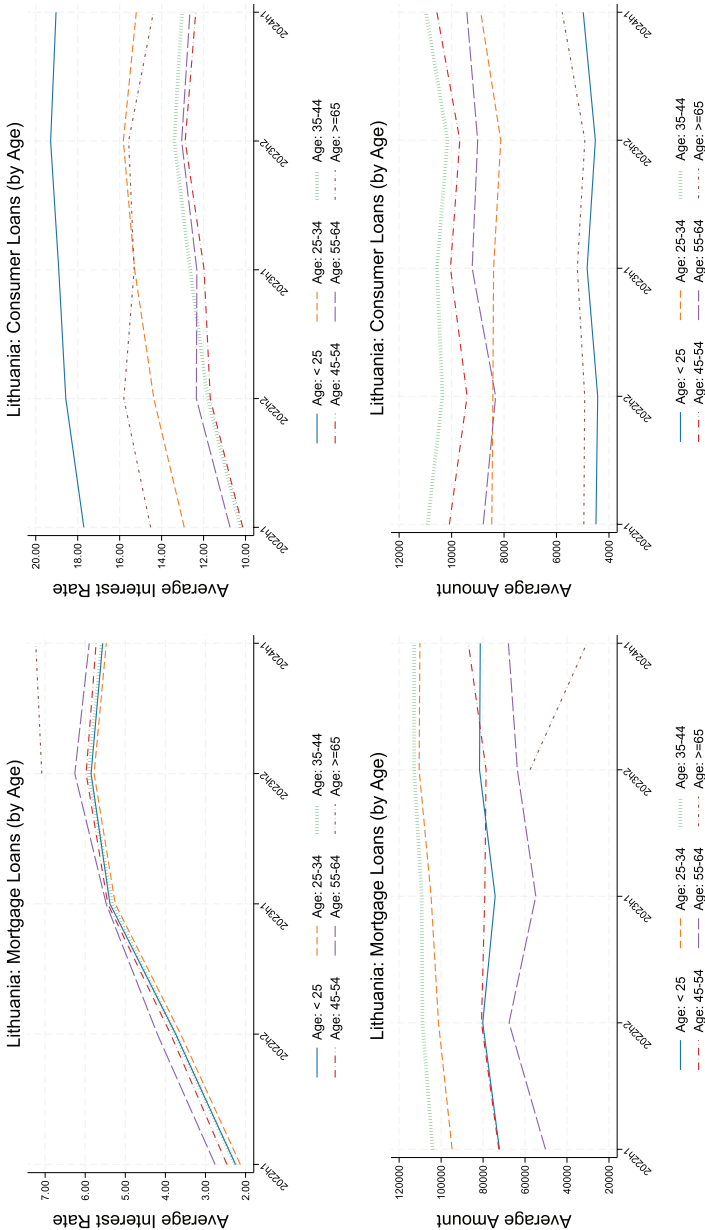


Figure F.7. Age Breakdown: Latvia

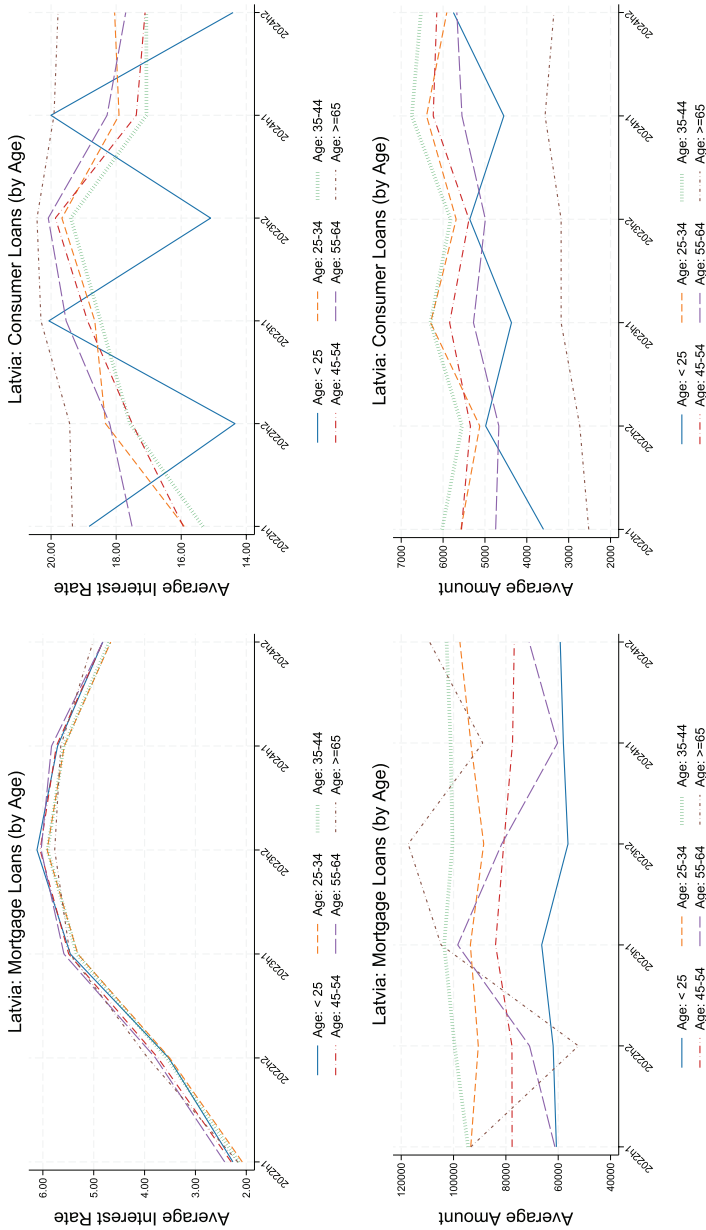


Figure F.8. Age Breakdown: Portugal

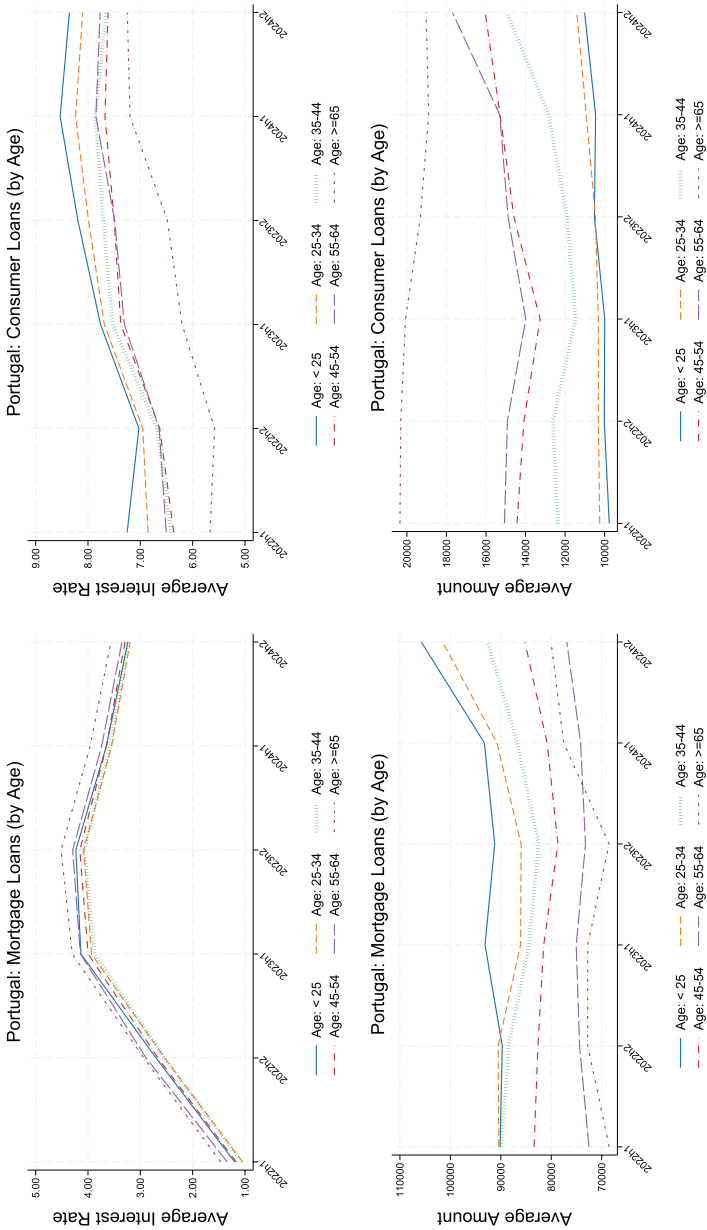
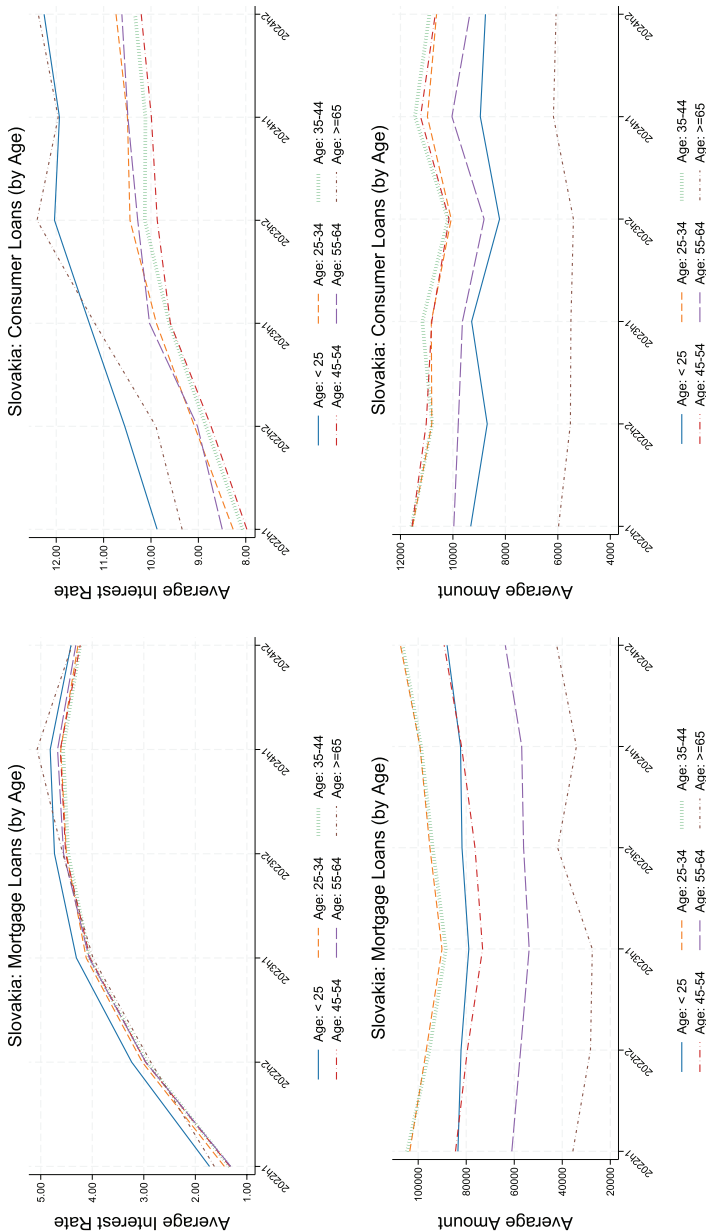
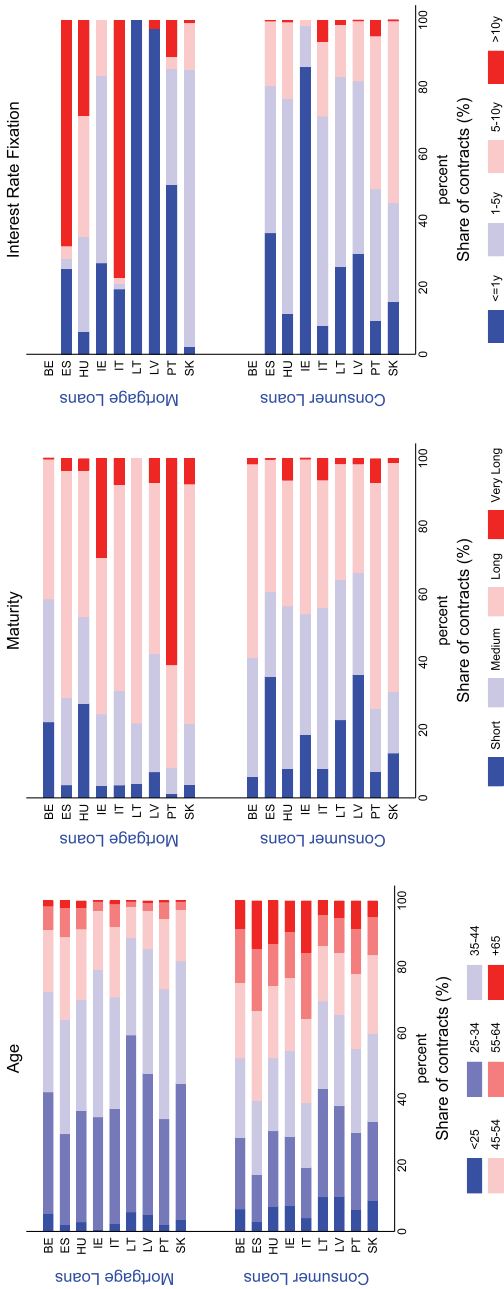


Figure F.9. Age Breakdown: Slovakia



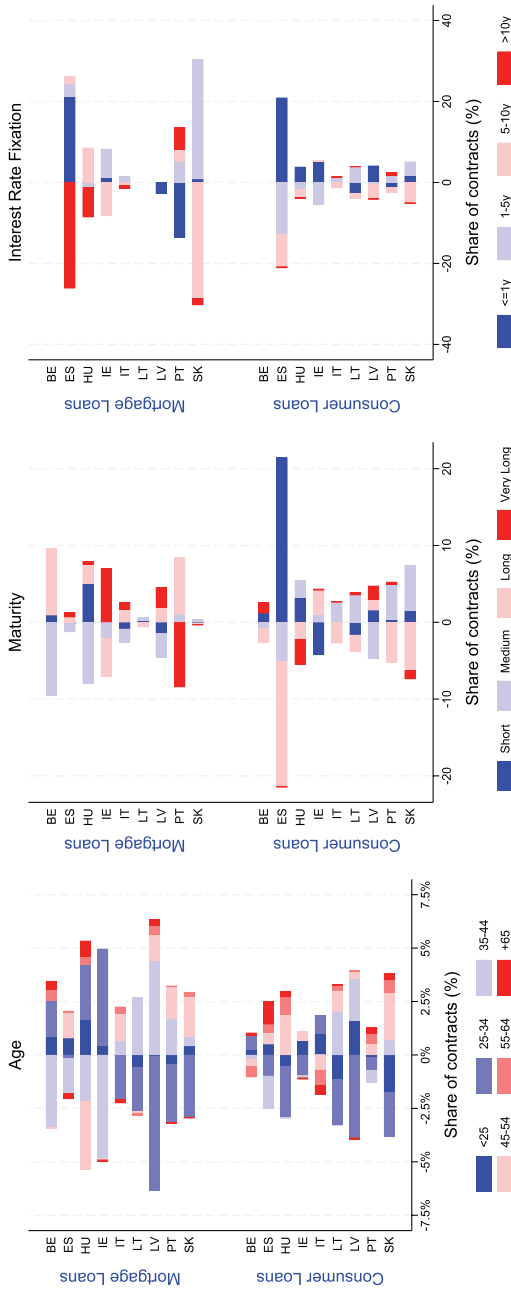
Appendix G. Stylized Facts: Number of Contracts (like Figures 1 and 2)

Figure G.1. Breakdown of New Loan Contracts (number) by Age, Maturity, and Interest Rate Fixation Period



Note: The figure presents the composition of the *number* of new loan contracts with respect to borrower age (left), loan maturity (middle), and interest rate fixation (right) across two different credit products—mortgage loans (top) and consumer loans (bottom)—for selected European countries. Each bar represents the percentage distribution of the value of new loan contracts by age/maturity/IRF group. Maturity bins are different for mortgage and consumer credit. For *mortgages*, we distinguish between short-term (≤ 10 years), medium-term (10–20 years), long-term (20–30 years), and very long-term (> 30 years) maturities. For *consumer loans*, the four bins are short-term (≤ 1 year), medium-term (1–3 years), long-term (3–10 years), and very long-term (> 10 years). The graph is based on new credit contracts initiated between January 2022 and December 2024. The countries included in the analysis are Belgium (BE), Spain (ES), Hungary (HU), Ireland (IE), Italy (IT), Lithuania (LT), Latvia (LV), Portugal (PT), and Slovakia (SK). The graph is based on loan-level data, aggregated by country and credit product.

Figure G.2. Share Changes over a Tightening Episode (number): Breakdown by Age, Maturity, and IRF



Note: The figure presents the change in the composition of the number of new loan contracts with respect to borrower age (left), loan maturity (middle), and interest rate fixation (right) across two different credit products—mortgage loans (top) and consumer loans (bottom)—for selected European countries. Each bar represents the percentage change in the share of new loan contracts by age/maturity/IRF group between two time periods (2022:H1 and 2023:H2). Maturity bins are different for mortgage and consumer credit. For *mortgages*, we distinguish between short-term (≤ 10 years), medium-term (10–20 years), long-term (20–30 years), and very long-term (> 30 years) maturities. For *consumer loans*, the four bins are short-term (≤ 1 year), medium-term (1–3 years), long-term (3–10 years), and very long-term (> 10 years). The countries included in the analysis are Belgium (BE), Spain (ES), Hungary (HU), Ireland (IE), Italy (IT), Lithuania (LT), Latvia (LV), Portugal (PT), and Slovakia (SK). Positive values indicate an increase in the share of new loan contracts for a given product, while negative values indicate a decline. The graph is based on loan-level data, aggregated by country and credit product, with percentage point changes computed relative to the total number of new loan contracts in each country and product category.

Table H.1. Heterogeneity in the Pass-Through: Robustness

Variation	Mortgages			Consumer Credit		
	(1)	(2)	(3)	(4)	(5)	(6)
	No 2024	No BE/IT	No IE	No 2024	No IE	No IE + Collateral
Reference Rate	0.874*** (0.071)	0.950*** (0.056)	0.904*** (0.053)	0.686*** (0.147)	0.966*** (0.165)	1.009*** (0.166)
BE × Reference Rate	0.183** (0.069)		0.181*** (0.055)			
ES × Reference Rate	—	—	—	—	—	—
HU × Reference Rate	0.023 (0.094)	0.063 (0.071)	0.065 (0.077)	−0.295*** (0.074)	−0.260*** (0.073)	−0.255*** (0.074)
IE × Reference Rate	−0.620*** (0.031)	−0.628*** (0.035)		−0.515*** (0.105)		
IT × Reference Rate	0.211*** (0.044)		0.176*** (0.037)			
LT × Reference Rate	0.008 (0.071)	−0.036 (0.054)	0.005 (0.051)	−0.430*** (0.104)	−0.453*** (0.110)	−0.437*** (0.110)
LV × Reference Rate	0.030 (0.065)	−0.049 (0.051)	−0.011 (0.047)	1.016*** (0.213)	0.554*** (0.193)	0.549*** (0.192)
PT × Reference Rate	0.055 (0.058)	−0.037 (0.051)	0.015 (0.048)	−0.094 (0.078)	−0.066 (0.066)	−0.047 (0.067)
SK × Reference Rate	0.024 (0.073)	0.085 (0.059)	0.141** (0.065)	0.382*** (0.100)	0.451*** (0.098)	0.438*** (0.098)

(continued)

Table H.1. (Continued)

	Mortgages			Consumer Credit		
	(1)	(2)	(3)	(4)	(5)	(6)
Variation	No 2024	No BE/IT	No IE	No 2024	No IE	No IE + Collateral
Age: <25 × Ref. Rate	0.074*** (0.022)	0.063*** (0.015)	0.064*** (0.020)	0.026 (0.064)	-0.009 (0.076)	-0.014 (0.076)
Age: 25-34 × Ref. Rate	0.032*** (0.011)	0.026*** (0.006)	0.027*** (0.010)	0.028 (0.017)	0.028 (0.022)	0.026 (0.022)
Age: 35-44 × Ref. Rate	—	—	—	—	—	—
Age: 45-54 × Ref. Rate	-0.002 (0.008)	0.002 (0.006)	0.000 (0.008)	0.008 (0.021)	0.009 (0.025)	0.010 (0.025)
Age: 55-64 × Ref. Rate	-0.024 (0.025)	-0.017 (0.029)	-0.017 (0.028)	0.038 (0.026)	0.060* (0.032)	0.061* (0.032)
Age: ≥65 × Ref. Rate	-0.165*** (0.041)	-0.176*** (0.050)	-0.146*** (0.046)	0.138*** (0.034)	0.187*** (0.044)	0.184*** (0.044)
Short × Ref. Rate	-0.273** (0.109)	-0.431*** (0.139)	-0.343** (0.132)	-0.459*** (0.153)	-0.632*** (0.173)	-0.676*** (0.179)
Medium × Ref. Rate	0.012 (0.010)	-0.003 (0.012)	0.003 (0.012)	-0.144 (0.095)	-0.271** (0.123)	-0.293** (0.125)
Long × Ref. Rate	—	—	—	—	—	—
Very Long × Ref. Rate	-0.027 (0.021)	-0.028 (0.022)	-0.037 (0.025)	0.352* (0.199)	0.344 (0.229)	0.570** (0.276)

(continued)

Table H.1. (Continued)

Variation	Mortgages		Consumer Credit			
	(1)	(2)	(3)	(4)	(5)	(6)
	No 2024	No BE/IT	No IE	No 2024	No IE	No IE + Collateral
IRF: ≤ 1 Year $\times$ Ref. Rate	—	—	—	—	—	—
IRF: 1–5 Years $\times$ Ref. Rate	0.472*** (0.128)	0.402*** (0.083)	0.416*** (0.092)	−0.085 (0.125)	−0.378*** (0.125)	−0.401*** (0.119)
IRF: 5–10 Years $\times$ Ref. Rate	−0.127 (0.139)	−0.168* (0.085)	−0.201** (0.092)	−0.047 (0.122)	−0.356*** (0.117)	−0.383*** (0.123)
IRF: >10 Years $\times$ Ref. Rate	−0.207 (0.147)	−0.284*** (0.098)	−0.209** (0.099)	−0.427*** (0.109)	−0.671*** (0.098)	−0.763*** (0.104)
Collateralized $\times$ Ref. Rate						−0.265*** (0.088)
Observations	1,949	2,071	2,623	1,775	2,380	2,380
Adjusted R-squared	0.91	0.91	0.92	0.89	0.87	0.87
Note: This table presents heterogeneity in the pass-through on new household loans, distinguishing between mortgages (columns 1–3) and consumer credit (columns 4–6). The top row indicates how the specification differs from the benchmark. The main independent variable is the reference interest rate, interacted with four sets of characteristics. Variables, weighting, clustering, and fixed effects are as in 2 and 3. Statistical significance is denoted as follows: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.						

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Has the Transmission of U.S. Monetary Policy Changed Since 2022?*

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Activity and inflation responded slowly to the Federal Reserve's rate hikes in 2022. Was this because the transmission of monetary policy had changed? Or did other shocks offset tighter policy? We use pre-pandemic data to estimate a VAR with monetary policy shocks identified from high-frequency data, and use it as a filter to back out the sequence of structural monetary policy shocks consistent with data since 2022. We compare these implied shocks with the actual shocks and find the difference statistically significant during February–July 2022. These differences imply that monetary transmission had roughly 75 percent of its usual pre-COVID effect. We provide suggestive evidence that weaker monetary policy transmission was the result of a steeper Phillips curve.

JEL Codes: C32, E43, E52.

1. Introduction

In March 2022, as inflation reached its highest level since the mid-1980s, the Federal Reserve made the first of a series of interest rate hikes. In the next 19 months, the federal funds rate increased from zero to over 5 percent. By any standards, this was a large and fast monetary tightening. In the four preceding tightening cycles, increases in the federal funds rate had been much smaller and slower,

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taking an average of 21 months to reach peaks typically 2.5 percentage points above their starting levels.¹

By July 2022, it was clear that this tightening cycle would last some time. By then, many forecasters were predicting that interest rates would increase further and remain elevated until at least late 2023. This policy stance was widely expected to reduce output below its long-run trend and pull inflation back down toward target, in line with standard views of the transmission of monetary policy (see Figure 1).

However, outturns largely confounded these predictions. Output grew rapidly throughout 2022 and 2023, at more than double the rate of even the most optimistic forecasters. This growth surprise occurred even though interest rates ended up being much higher than expected, something which would normally be expected to have dampened real activity further. And despite the surge in output, inflation has since returned to levels close to the Federal Reserve's 2 percent target.

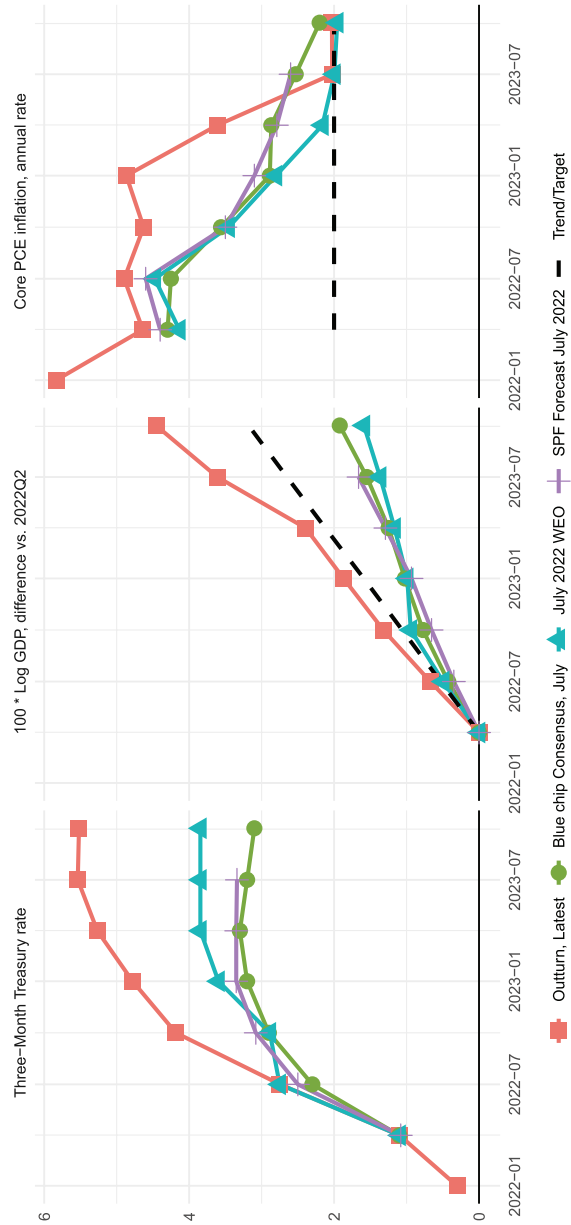
The exceedingly strong performance of the real economy and the apparently unconnected disinflation during such an aggressive tightening cycle raises an obvious question: was the transmission of monetary policy somehow different during this episode? And if so, how? This paper seeks to answer these questions.

The key challenge in answering these questions is in disentangling the impact of other macroeconomic shocks (or combinations of them) from any potential change in the transmission of monetary policy. For example, the mix of an unanticipated fiscal expansion and a positive supply shock could produce higher-than-expected output and interest rates and falling inflation, even if monetary policy had been transmitted exactly as expected.

Given this challenge, we develop a method to answer this question which accounts for the impact of other shocks but without having to identify them explicitly. Our basic approach proceeds in three steps. We first construct a series of monetary policy shocks from high-frequency intraday financial market data in the spirit of Jarociński and Karadi (2020) and use a pre-COVID sample to estimate a factor-augmented vector autoregression (FAVAR) which

¹Cycles starting in February 1994, June 1999, June 2004, and December 2015.

Figure 1. Outturns versus Forecasts Made in July 2022



Note: The figure shows outturns for interest rates, output, and the price level versus forecasts made in July 2022. Forecasts for change in log GDP abstract from revisions by calculating relative to contemporaneous data for forecasts. The trend for log GDP is at annualized rate of 2.1, the 2000–19 average. For core personal consumption expenditures (PCE) the target is 2 percent, the Federal Reserve’s inflation target. Abbreviations: WEO, IMF’s World Economic Outlook; SPF, Federal Reserve Bank of Philadelphia’s Survey of Professional Forecasters.

includes the monetary policy shocks as data.² The Cholesky decomposition of the FAVAR's reduced-form shock with the monetary shocks ordered first then has a semi-structural interpretation where the monetary policy shocks are identified but non-monetary ones are not. The purpose of this step is to produce a data-generating process for macroeconomic time series which embodies relatively standard views about the transmission of monetary policy in the pre-COVID period.³

In the second step, we invert the FAVAR to infer the most likely sequence of monetary shocks given the macro data alone. This can be thought of as a thought experiment where we pretend that we do not see the monetary shocks starting in 2021.⁴ We then generate the distribution of the high-frequency shocks since 2021 under the null hypothesis that the mapping from monetary policy to the macro data has not changed. A strength of our method is that it explicitly takes account of the changing composition of the other shocks. Although the inferred monetary shocks depend directly on the estimated transmission of monetary policy, they depend on other shocks only indirectly via the covariance of the reduced-form residuals. Put differently, even if some unusual combination of other shocks were driving outcomes in the post-tightening period, this would not affect the inferred monetary shocks at all, so long as those other shocks were consistent with the estimated covariance of the data. This feature suggests a natural extension: to relax this assumption simply by replacing the estimated reduced-form covariance matrix with another one, such as one estimated on the post-2021 data. This allows a calculation of the distribution of monetary shocks consistent with the data and unchanged transmission, even if the scale of other shocks has also changed. Moreover, this can be done without

²Aside from the monetary policy shocks, the baseline VAR also includes data on the federal funds rate, industrial production, private consumption, inflation, and the excess bond premium as well as the first five principal components of a large set of other macroeconomic time series.

³As Paul (2020) shows, including the instrument directly in the VAR is identical to the more standard external instruments approach pioneered by Stock and Watson (2012) and Mertens and Ravn (2013), up to rescaling by a constant. We discuss the relationship of our method to using external instruments further in Section 2.

⁴Despite the title of our paper, we start in 2021 to give something of a lead-in to the 2022 tightening cycle.

specifying exactly how other shocks have changed, just their impact on the covariance of the aggregate data.

In the third step, we compare these distributions with the observed high-frequency monetary policy shocks since 2021. If the null hypothesis holds—that monetary policy transmission has not changed—then the realized and inferred shocks should line up. We propose likelihood-ratio tests to assess the strength of the evidence that the transmission of monetary policy has changed, as well as modifications to these tests which permit a quantitative assessment of how much weaker monetary policy might have been.

We present results for two types of monetary shocks within the same estimation framework, one a “pure” shock to the stance of monetary policy and the other a central bank information shock. The former corresponds to truly tighter monetary policy whereas the latter occurs when the Federal Reserve (the Fed) communicates its insights about other future shocks to other agents in the economy. We find that between February and July 2022, the measured high-frequency monetary stance shocks were persistently and statistically significantly higher than those implied by the filter, both on average and in individual months. That is, although the Federal Reserve was repeatedly surprising the markets with unexpectedly tight monetary policy, the macroeconomic data—output, inflation, interest rates—were responding as if policy were considerably looser. This pattern provides strong statistical evidence that the transmission of monetary policy to the macroeconomy was materially weaker during this period than in the pre-COVID era. We also show that the realized central bank information shocks were larger than those implied by the filter throughout 2021; the data responded as if the Fed was communicating much more negatively than it did.

We investigate the results for the pure monetary shock further, quantifying the magnitude of this change. We show that in our baseline specification, the data from February to July 2022 are statistically inconsistent with monetary policy having its normal pre-COVID effectiveness. Specifically, we can reject at the 5 percent confidence level scenarios where interest rate changes had any more than about 75 percent of their usual impact on output, inflation, and other macrovariables. Put differently, for every three interest rate hikes that the Fed did during this period, they would have needed to do approximately one more to deliver the same degree of tightening as one would have expected pre-COVID. When we allow for the

variance of other shocks to change to replicate the covariance of the 2021–23 data, we find that this conclusion is a little weaker—holding with the same confidence only in March and April 2022. Conversely, including policymakers’ speeches in our shock series strengthens this finding, with the same effect holding at the 1 percent level.

We also analyze the source of the uncertainty arising in our approach, since the quantification of this uncertainty is essential to formulating the hypothesis tests we apply. We find that uncertainty over the model parameters accounts for a narrow minority of the total. Instead, uncertainty due to the colinearity of shocks—that is, the extent to which monetary policy and other shocks have similar impact on the data and are thus hard to disentangle—are slightly more important.

To try to understand why transmission might have changed, we calibrate a well-known New Keynesian macro model (Smets and Wouters 2007) to match our impulse responses. We then replace key structural parameters with alternate values to produce a series of alternate data-generating processes which we can use as candidate null hypotheses in our test of the post-2021 data. This allows us to answer the question: if one had a structural model which captured the pre-COVID transmission of monetary policy, what parameter would need to be changed (and by how much) in order to give monetary transmission consistent with the data when monetary policy was tightening? Of course, this is not equivalent to full reestimation of a structural model, so we consider this evidence to be suggestive rather than conclusive. Nevertheless, we find that a steeper price Phillips curve and, to a lesser extent, a steeper wage Phillips curve are able to explain the post-2022 data.

Related Literature. Our work contributes to three prior areas of research.

First, there is an emerging literature on the monetary policy response to the COVID-19 pandemic and its aftermath (Ball, Leigh, and Mishra 2022; English, Forbes, and Ubide 2024). Much of this work focuses on key ingredients of the monetary transmission mechanism, rather than the more general notion of transmission that we look at. For example, Stedman and Pollard (2023) highlight that services-sector labor markets have been particularly tight since the pandemic. These sectors are less sensitive to changes in interest rates, potentially dampening the monetary policy impact. Similarly, Cohen (2023) argues that resilience in the labor market is connected

to severe labor shortages in the post-pandemic era, leading employers to hold on to workers and hire less-skilled workers. D'Amico and King (2023) argue that the lags to monetary policy might have been shorter in the post-pandemic tightening cycle because of an increased role of the expectations channel of monetary policy. Glover and Oliide (2024) note that private-lending spreads have been unusually low in this period, implying less restrictive monetary policy. Bauer, Pflueger, and Sunderam (2024) find that public perception of the Fed's monetary policy response to inflation has shifted over the post-pandemic period. Such a shift can explain disinflation with relatively low output and unemployment costs. Fosso, Robays, and Goosen (2023) ask a similar question as our paper, but employ a different methodology. They use a structural VAR, allowing for time-varying parameters, and don't find evidence for a change in monetary policy transmission, but that offsetting shocks can explain why unemployment has stayed low in the post-pandemic period, despite rapid policy tightening. These studies of the post-COVID period also tie in to a broader literature on the state-dependent effects of monetary policy. Although there is considerable evidence in favor of impact of the state-dependent impact of monetary policy, there is not clear agreement on what the states are and why they matter.⁵ Analyses which specify a priori that monetary transmission is affected by a particular state risk ignoring other sources of variation in transmission. By investigating whether monetary policy has operated differently at specific periods, rather than the more general issue of in specific states, we sidestep the preliminary question of what that state should be.

Second, our work relates to a methodological literature on using high-frequency monetary policy announcements as identified shocks in vector autoregressive (VAR) models (Stock and Watson 2012; Mertens and Ravn 2013; Gertler and Karadi 2015). These papers study the dynamic impact of identified monetary policy shocks on

⁵For example, Tenreyro and Thwaites (2016) show that monetary policy is more powerful in expansions than recessions, something that Bernstein (2021) rationalizes with a model with occasionally binding liquidity constraints. Ascari and Haber (2022) connect to the literature on endogenous price setting and show that monetary policy is more effective when either shocks are large or trend inflation is low. And Berger et al. (2021) and Eichenbaum, Rebelo, and Wong (2022) highlight that the presence of substantial debt in fixed-rate, prepayable mortgages can lead to state-dependent effects of monetary policy.

the economy. While we use a similar approach to estimate a VAR, our ultimate application is different. Once we have estimated the VAR, we invert it and use it as a filter to back out the most likely set of shocks over a test period. We then construct a statistical test on whether the mapping of monetary policy to macro data has changed or not. Similar to Paul (2020), our pursuit of an extra step not easily represented in the external instruments framework motivates our direct inclusion of shocks in the VAR.

Third, we contribute to the literature that uses high-frequency changes in asset prices around specific events to construct identified monetary policy shocks (Kuttner 2001; Gürkaynak, Sack, and Swanson 2005; and many others). Initially the literature was centered on Federal Open Market Committee (FOMC) announcements. However, there are only a limited number of scheduled FOMC meetings (typically eight per year), and the size of the asset price changes around the events is small. And so a recent literature (Swanson 2023; Swanson and Jayawickrema 2023) extends the approach to include post-FOMC-meeting press conferences, releases of FOMC meeting minutes, and speeches by the Chair and Vice Chair of the Federal Reserve. They find that these events are important sources of variation for asset prices. In an extension, we expand the list of events further by including speeches by all members of the Federal Reserve Board of Governors.

The paper is organized as follows: in Section 2 we describe our method; in Section 3 we discuss the construction of our monetary policy shocks; in Section 4 we describe the estimated FAVAR and validate our method on the pre-COVID sample; in Section 5 we present our main results, comparing the filter-implied shocks with those measured in the high-frequency data; in Section 6 we discuss alternative interpretations of our results; and in Section 7 we conclude.

2. Methodology

2.1 Estimating a Semi-Structural Vector Autoregression

Let y_t be a vector of N macrovariables and v_t an i.i.d. vector of proxies for K different monetary policy shocks identified from other data. We describe how to create such a series in the next section, but here we take this as given. We collect monthly data on y_t and v_t

since 1990 (more detail on the data we use in Section 4.1). We call the period from 1990 to 2019 the *estimation period* and 2021–23 the *testing period*, with lengths $N_{est} = 336$ and $N_{test} = 36$, respectively.

We assume that the data are generated by a VAR of the form

$$\begin{bmatrix} v_t \\ y_t \end{bmatrix} = \sum_{j=1}^J A_j \begin{bmatrix} v_{t-j} \\ y_{t-j} \end{bmatrix} + \epsilon_t, \quad \epsilon_t \sim N(0, \Omega) \quad (1)$$

and impose that

$$A_j = \begin{bmatrix} 0_{K \times K} & 0_{K \times N} \\ 0_{N \times K} & B_j \end{bmatrix}.$$

The top line of zeros follows from the assumption that v_t is i.i.d. The zeros in the other rows simply impose that y_t depend only on lagged shocks through lagged y_t , not directly—i.e., that y_t has a reduced-form VAR, not VARMA (vector autoregressive moving average), representation. We estimate this specification on the estimation period.

As is usual, we assume that the reduced-form errors are a linear function of the true, structural shocks, so that

$$\epsilon_t = D\delta_t \quad \text{Cov } \delta_t \sim N(0, I_{N+K}), \quad (2)$$

where $DD' = \Omega$. Because v_t are already externally identified monetary shocks, any decomposition of Ω where the nondiagonal elements of the top K rows are all zero will have monetary shocks in the first two columns.⁶ If the shocks are orthogonal, a Cholesky decomposition will satisfy this condition.

Then we write

$$D = \begin{bmatrix} I_K & 0 \\ D_m & D_x \end{bmatrix}.$$

Then, the D_m columns thus describe the direct impact of monetary policy shocks on the macroeconomy in the estimation period. We also will find it helpful to use similar notation to divide ϵ_t :

$$\epsilon_t = \begin{bmatrix} \epsilon_t^v \\ \epsilon_t^y \end{bmatrix}.$$

⁶Strictly speaking, the first K entries in δ_t are the *innovations* to v_t . But since the shocks are i.i.d. this distinction is not meaningful.

And for δ_t , we use the superscripts m and x to denote the monetary and non-monetary shocks:

$$\delta_t = \begin{bmatrix} \delta_t^m \\ \delta_t^x \end{bmatrix}.$$

Later, we will consider the case where we pretend that we only see the reduced-form errors for the macrovariables, ϵ_t^y . Then, we can rewrite Equation (2) as

$$D_y \delta_t = \epsilon_t^y, \quad (3)$$

where $D_y = [D_m \ D_x]$. Note that because ϵ_t^y is of length N but δ_t is of length $N + K$, Equation (3) does not have a unique solution for δ_t . Instead, a set of structural shocks are consistent with any given observation for ϵ_t^y . In the next section we characterize the distribution of δ_t within that set.

The Relationship to External Instruments. A common alternative approach is to use v_t as an external instrument (Stock and Watson 2012; Mertens and Ravn 2013; and Olea, Stock, and Watson 2021). That is, one could rewrite Equation (1) purely in terms of the y_t ,

$$y_t = \sum_{j=1}^J B_j y_{t-j} + \nu_t, \quad \nu_t \sim N(0, \Xi). \quad (4)$$

And if the elements of v_t are strongly correlated with the monetary shocks (instrument strength) but plausibly uncorrelated with the other structural shocks (the exclusion restriction), one can use the v_t as an external instrument for the monetary shocks. The result is a decomposition of Ξ where the first two columns can be interpreted as the responses to a policy shocks.

Paul (2020) shows—subject to some technical conditions satisfied in our application—that the resulting impulse responses are identical to directly including the v_t series in the VAR, up to a scalar constant. That is, the two approaches differ only in their unit of measurement rather than anything more fundamental.⁷ Given that

⁷A yet further equivalent formulation is one where the data-generating process for y_t includes an explicit contemporaneous relationship, in which case

the approaches are in this sense equivalent (something which we check in our estimation later), we prefer the direct inclusion of the shock in the VAR because it makes our next step—recasting the VAR as a filter—much easier. A similar extension motivated the derivation of the results in Paul (2020). He estimates a VAR with time-varying parameters, which is much easier when the identified shocks are directly included rather than used as instruments.

2.2 Recasting the VAR as a Filter

We now pretend that we do not observe the monetary policy shock on the testing period. The purpose of this assumption is to use the macro data y_t alone to infer what the monetary shocks would have been under the null hypothesis that the transmission of monetary policy was unchanged.

Conditional on the estimates $\hat{\theta} = \{\hat{B}_1, \dots, \hat{B}_J, \hat{\Omega}\}$, we can compute the fitted reduced-form errors $\hat{\epsilon}_t^y$ associated with the macro data y_t alone by

$$\hat{\epsilon}_t^y = y_t - \sum_{j=1}^J \hat{B}_j y_{t-j}.$$

As noted above, we cannot solve for a unique δ_t given $\hat{\epsilon}_t^y$. Instead we calculate the most likely structural shock given $\hat{\theta}$, via maximum likelihood.

Because $\delta_t \sim N(0, I_{N+K})$, the maximum likelihood function just reduces to least squares. Thus, we can compute the most likely structural shocks by solving the following linear-quadratic program period-by-period:

$$\hat{\delta}_{t|\hat{\theta}} = \arg \min_{\delta \in \mathbb{R}^{N+K}} \delta' \delta \quad \text{s.t.} \quad \hat{D}_y \delta = \hat{\epsilon}_t^y,$$

where $\hat{D}_y$ is the sample equivalent of D_y , i.e., the bottom N rows of the Cholesky decomposition of $\hat{\Omega}$. This has the following solution:

$$\hat{\delta}_{t|\hat{\theta}} = \hat{D}_y' (\hat{\Omega}_y)^{-1} \hat{\epsilon}_t^y, \tag{5}$$

$\tilde{B}_0 y_t = \sum_{j=1}^J \tilde{B}_j y_{t-j} + \tilde{\nu}_t$. This is identical to Equation (4) if $\tilde{B}_j = \tilde{B}_0 B_j$, $\tilde{\nu}_t \sim N(0, \tilde{B}_0 \Xi \tilde{B}_0')$ and so the same equivalence between external instruments and inclusion of the external shocks in the VAR still holds.

where $\hat{\Omega}_y = \mathbb{E} \hat{\epsilon}_t^y \hat{\epsilon}_t^{y'} = \hat{D}_y \hat{D}_y'$ is the estimated variance covariance matrix of the reduced-form residuals for the data, $\hat{\epsilon}_t^y$. Because we are only interested in the monetary shock, we will find it helpful to focus only on the top K rows of this estimator:

$$\hat{\delta}_{t|\hat{\theta}}^m = \hat{D}_m' (\hat{\Omega}_y)^{-1} \hat{\epsilon}_t^y.$$

It is straightforward to show that this is unbiased, conditional on unbiasedness of $\hat{\theta}$. That is,

$$\mathbb{E} \hat{\theta} = \theta \Rightarrow \mathbb{E} \hat{\delta}_{t|\hat{\theta}}^m = \delta_t^m.$$

We can also compute the mean square error of the estimated structural shocks:

$$\mathbb{E}_t \left(\hat{\delta}_{t|\hat{\theta}}^m - \delta_t^m \right) \left(\hat{\delta}_{t|\hat{\theta}}^m - \delta_t^m \right)' = I_K - \hat{D}_m' \hat{\Omega}_y^{-1} \hat{D}_m. \quad (6)$$

This is just a Kalman filter with the unobserved structural shocks as a hidden state and the reduced-form shocks as observed signals. The only differences from the usual setup are that there is no persistence in the state variable and that uncertainty over the state comes not from noise but from incomplete observation (in that the dimension of $\hat{\epsilon}_t^y$ is less than $\hat{\delta}_t$).⁸

Given that δ_t itself is normal and that $\hat{\delta}_{t|\hat{\theta}}^m$ is an unbiased estimator for δ_t^m conditional on the data and the model, then the distribution of the true shocks conditional on the data and the model parameters, which we denote $\delta_{t|t,\hat{\theta}}^m$, is given by a multivariate normal:

$$\delta_{t|t,\hat{\theta}}^m \sim \mathcal{N} \left(\hat{D}_m' (\hat{\Omega}_y)^{-1} \hat{\epsilon}_t^y, I_K - \hat{D}_m' \hat{\Omega}_y^{-1} \hat{D}_m \right). \quad (7)$$

We will find it helpful to denote the corresponding normal density by $f_{t,|\hat{\theta}}(\delta)$.

⁸A related literature is that on conditional forecasting in VARs, initiated by Waggoner and Zha (1999), where one seeks to forecast a subset of variables in a VAR by imposing conditioning assumptions on the other variables. Our approach maps to this framework if one interprets the inferred shocks as the subset of variables to be forecast and the observed macro data as the conditioning assumptions.

An important property of this filter is that the distribution for the inferred monetary shocks is a function only of D_m and the reduced-form error covariance $\hat{\Omega}_y$. One would get exactly the same results for *any* other $\tilde{D}_x$, satisfying

$$\tilde{D}_x \tilde{D}_x' = D_x D_x'.$$

In other words, the filtered monetary shocks are wholly invariant to any other interpretation of the non-monetary shocks, so long as they are consistent with the data covariance. Moreover, if we want to know how any other candidate *distribution* of the non-monetary shocks affects our results, we need only specify its impact on the data via the reduced-form residual covariance matrix, $\hat{\Omega}_y$. We will use this fact later when investigating the robustness of our results to the changing variance of non-monetary shocks.

2.3 Parameter Uncertainty

The preceding section derives the maximum likelihood estimator and the mean square error for the structural shocks given the data and a model. However, the true model is unknown. The distribution in Equation (7) does not account for this. And so to account for model uncertainty, we integrate over the distribution of the parameters via bootstrap.

Specifically, we use the point estimates $\hat{\theta} = \{\hat{B}_1, \dots, \hat{B}_J, \hat{\Omega}\}$ to simulate N_{sim} draws of the estimation sample $\{(v_1^i, y_1^i), \dots, (v_{N_{est}}^i, y_{N_{est}}^i)\}_{i=1}^{N_{sim}}$ from the data-generating process in Equation (1). On each sample, we estimate the corresponding parameters $\hat{\theta}^i = \{\hat{B}_1^i, \dots, \hat{B}_J^i, \hat{\Omega}^i\}$. We then compute the distribution for $\delta_{t|t}$, the structural shocks conditional only on the data. This is a mixture of normal distributions with density given by

$$f_t(\delta^m) = \frac{1}{N_{sim}} \sum_{i=1}^{N_{sim}} f_{t|\hat{\theta}^i}(\delta^m). \quad (8)$$

This density represents the distribution of shocks under the null that the relationship between the data and the shocks remains unchanged, taking into account uncertainty around that relationship

(i.e., model uncertainty). As such, we can use it to assess hypothesis tests of this null.

This approach has two further benefits. First, we can use the simulated parameter draws to compute bootstrapped confidence intervals for impulse responses. Second, in each testing period t we can compare the distributions $f_{t,\hat{\theta}}(\cdot)$ and $f_t(\cdot)$ to understand the sources of uncertainty over our estimates of the structural shocks. The former captures the irreducible uncertainty coming from the colinearity of the structural shocks. The latter adds the effect of model uncertainty, which is mitigated in longer samples. We return to this point later in Section 5.4.

3. Constructing Monetary Policy Shocks from High-Frequency Financial Data

3.1 Approach

We identify monetary shocks using the high-frequency identification approach pioneered by papers including Kuttner (2001) and Gürkaynak, Sack, and Swanson (2005). The idea is to look at the change in the price of securities around a narrow time window of relevant monetary policy events, such as FOMC meetings. The change in price should capture the pure surprise component of the news announcement uncorrelated with other shocks, and therefore is a good candidate for an identified monetary policy shock. The choice of a narrow window aims to eliminate the possibility that other news drives the effect. The literature has traditionally focused on FOMC announcements to construct the shocks. But more recently, researchers (Swanson 2023; Swanson and Jayawickrema 2023) have extended the approach to include other events, like speeches by Federal Reserve leadership, FOMC press conferences, or Fed meeting minutes releases. These papers find that other events can have at least as much effect on financial markets as FOMC announcements. We conduct our baseline analysis using monetary policy shocks based on FOMC announcements, but use a shock series including speeches by all members of the Federal Reserve Board of Governors as robustness exercise.

We follow Jarociński and Karadi (2020) and disentangle the pure monetary policy component of a monetary policy announcement

from an “information” component. The former can be thought of as a traditional monetary policy shock, where monetary policy itself is unexpectedly and exogenously tighter. The information component captures the situation when a policy announcement provides new information on (the central bank’s view of) fundamentals of the economy to the market. Jarociński and Karadi (2020) propose disentangling the two effects by their differential impacts on interest rates and equity prices, and present evidence that doing so improves the quality of the monetary policy shocks.

3.2 Data

Our approach uses high-frequency data on interest rates and the stock market. For interest rates we use the price of “fourth,” or three-month-ahead, federal funds futures (FF4), as in Gertler and Karadi (2015); for the stock market we use the S&P 500 index, as in Jarociński and Karadi (2020). For January 2008 to December 2023, we use tick-level data purchased from Bloomberg. We compute “raw” changes in these series over windows which start 10 minutes before FOMC policy announcements and end 20 minutes after the announcement, as is common in the literature. To extend our sample further back, we use data from Gürkaynak, Karasoy-Can, and Lee (2022) which contains the same changes for these two securities, going back to the late 1980s. The data end in June 2019, and we use the overlapping sample period to confirm that the raw changes agree. Appendix A.1 presents full details of how we compute the relevant changes using the high-frequency financial data.

Following Swanson (2023) and Swanson and Jayawickrema (2023), we also develop a secondary shock series, which adds changes in market prices in windows around speeches by members of the Board of Governors of the Federal Reserve System.⁹ However, data limitations mean that this secondary series is only available back to 2010. We discuss this series more in Appendix A.2.

⁹The Board of Governors of the Federal Reserve System, or Federal Reserve Board of Governors, consists of seven members who serve staggered 14-year terms. Among these seven members are the Chair and Vice Chair of the Federal Reserve. All seven Board members also serve on the FOMC, which additionally includes five regional Reserve Bank presidents.

3.3 A Rotational Decomposition

To decompose high-frequency movements in interest rates and equity prices into a pure monetary shock and a central bank information shock, we use a rotational decomposition. The advantage of this approach is that it relies only on the high-frequency financial market data and not on other macro time series. This is important for us since we want to use macro data to formulate a null distribution for the shocks in the test period using the filter. The test statistics for the hypothesis tests will use the true high-frequency shocks. If they also depend on the macro data, then they are not independent of the relationship between the shocks and the data and so the tests will be invalid. In contrast, Jarociński and Karadi (2020) identify their shock as a sign restriction in a VAR which includes macro data, which would be problematic given the exercise we have in mind. In practice, though, our shock series and theirs are very similar.

We let the vector m_t be the changes in interest rates and the S&P 500 in the window around FOMC announcements in each month. We write

$$m_t = \begin{bmatrix} \Delta i_t \\ \Delta q_t \end{bmatrix}.$$

We assume that the data have already been transformed to be mean zero and denote the variance of m_t by Γ .

We want to give the high-frequency data m_t a structural interpretation, converting it into structural shocks v_t , which are related by $m_t = H v_t$ where v_t is mean zero and $\mathbb{E} v_t v_t' = I_2$. In particular, if the first element of v_t exhibits a negative correlation of interest rates and equity prices and the second a positive correlation, then we can interpret the elements of v_t as a pure monetary shock and a Fed information shock, respectively. This imposes a sign restriction on H :

$$H = \begin{bmatrix} + & + \\ - & + \end{bmatrix}.$$

We satisfy these restrictions with an orthogonal decomposition, imposing a parametric structure on the H matrix:

$$H = \begin{bmatrix} h_1 \cos \psi & h_2 \sin \psi \\ -h_1 \sin \psi & h_2 \cos \psi \end{bmatrix}, \quad h_1, h_2 > 0 \quad \psi \in (0, \pi/2).$$

The intuition for this setup is that ψ is the angle of a line through $(\Delta i_t, \Delta q_t)$ below the x axis. This is the prime axis of rotation. It is close to, but not necessarily identical to, the ordinary least squares (OLS) line of best fit. The two components of v_t then contain instructions for getting to the corresponding point m_t , expressed as a movement along the line of best fit (the first component) and perpendicular to it (the second component).

Solving for h_1, h_2, ψ such that $HH' = \Gamma$, we get that

$$\begin{aligned}\tan 2\psi &= \frac{2\gamma_{12}}{\gamma_{22} - \gamma_{11}} \\ h_2^2 &= \frac{1}{2} \left(\gamma_{11} + \gamma_{22} + \frac{\gamma_{12}}{\sin \psi \cos \psi} \right) \\ h_1^2 &= \gamma_{11} + \gamma_{22} - (h_2)^2,\end{aligned}$$

where γ_{ij} is the (i, j) th entry of Γ .¹⁰ We then create the structural monetary policy shocks from $v_t = H^{-1}m_t$.¹¹

3.4 The Shocks

Appendix Figure A.2 shows the time series for our two monetary shocks, together with the respective shock series from Jarociński and Karadi (2020).¹² The latter shocks end in June 2019, while our shocks include the pandemic period and end in January 2024.¹³ The shocks from both approaches overlap very well. The correlation coefficient on the overlapping sample is 87 percent for the monetary policy shock and 77 percent for the central bank information shock, respectively. Note that some discrepancy is not surprising due to the different methodology: Jarociński and Karadi (2020) include macroeconomic variables when constructing the shocks, while our approach—intentionally—does not. The shocks series from both

¹⁰This is similar to the decomposition in Jarociński (2022). However, there the decomposition does not impose unit covariance on the v_t shocks and so cannot solve uniquely for ψ .

¹¹Since $|H| = h_1 h_2 > 0$, H is always invertible.

¹²See Appendix A.1.1, Table A.1 for summary statistics and Figure A.1 for a scatterplot of the raw shocks.

¹³Data limitations prevent us from extending the shock series in Jarociński and Karadi (2020) exactly.

approaches show a higher variance around “crisis” periods like around 2001 after the bursting of the dot-com bubble, as well as the Great Financial Crisis of 2008 and 2009. A period of some divergence between the two approaches is before 1994, where our monetary policy shocks tend to be smaller, while the central bank information shocks tend to be bigger. This is also the time period where the FOMC did not regularly issue a press release.

4. Estimation and Validation

4.1 Data

We aim to incorporate information from a broad set of macro time series in our estimation, while still retaining interpretable impulse responses for comparison with those in the literature. To this end, we include both commonly used macro time series in the vector y_t , as well as factors extracted from a wider set of variables. That is, we estimate a FAVAR. In our baseline specification, y_t includes five variables directly—the federal funds rate, log industrial production, the log of the consumer price index (CPI), the employment-population ratio rate, and the excess bond premium—as well as five factors summarizing further 18 variables covering financial markets, prices, real activity, and labor markets. These variables are listed in Appendix Table A.3, and we use a principal components decomposition to create the factors. These five factors explain 70 percent of the variation in these covariates. Appendix Figure A.3 plots the data for the five main series after detrending and deseasonalizing, together with the factors. The full data sample runs from January 1990 to December 2023, with data before December 2019 used for estimation and after January 2021 for out-of-sample testing. Figure A.4 in Appendix A.5 plots the wider set of data series used to extract the factors.

4.2 A Baseline View of the Transmission of Monetary Policy

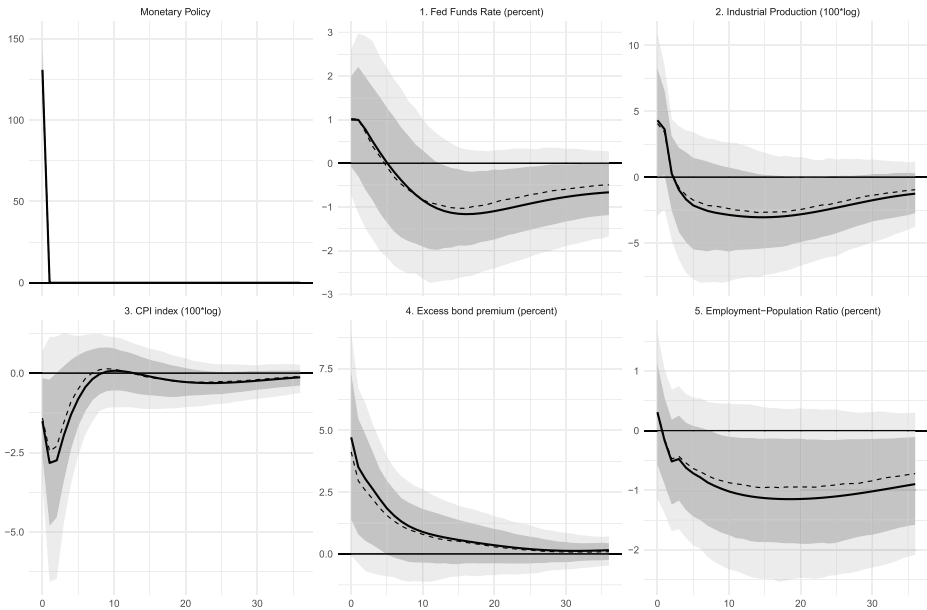
We estimate the reduced-form VAR in Equation (1) on the 1990–2019 period, and interpret the first two rows of the Cholesky decomposition as the impact of shocks to monetary policy and central bank information. Although our focus is not on estimating these

responses, the impulse responses are an important step in our analysis since they embody the view of the transmission of monetary policy with which we want to compare more recent data. We allow for $J = 2$ lags in our baseline specification, selected by the Akaike information criterion (AIC). A short lag length is not entirely surprising since the inclusion of the factors means we have many variables in the VAR—extra lags are more heavily penalized and there is less marginal information from extra lags. Longer lag lengths produce similar results.

Figure 2 shows the response of the five headline variables to the monetary policy shock, normalized to a 1 percentage point increase in the federal funds rate. Factors are included, but not shown; see Figures B.1 and B.2 in Appendix B.1 for the responses of the factors. The results are similar to those in the literature. Industrial production falls sharply in the months after the monetary shock, with a maximum contraction of around 3 percentage points. The real impact reaches its maximum around eight months after the shock, although the effect persists for rather longer. We also estimate a decline in prices on impact, although this is a little less persistent than estimates elsewhere. The decline in real activity and prices is also associated with a subsequent reduction in interest rates, consistent with an endogenous response to lower output and inflation.

Our estimated central bank information shock (see Figure 3) is also broadly consistent with other estimates, both qualitatively and quantitatively. Positive information about the future state of the economy leads to an increase in real activity and prices with interest rates rising in response and employment increasing.

In Figures 2 and 3 we scale the shocks such that the initial impulse for the federal funds rate is 1 percentage point. Typically, this requires a relatively large high-frequency shock, around 120 standard deviations of the monetary policy shock and 35 of the central bank information shock. This occurs because the identified shock captures only a small part of the variation of interest rates. As mentioned above, an alternative approach is to treat v_t as imperfectly correlated instruments for a broader set of monetary shocks. As Paul (2020) shows, this produces identical impulse responses, just rescaled—effectively a change of units to account for the fact that not all the monetary policy shocks are captured by

Figure 2. Impulse Responses: Monetary Policy Shock

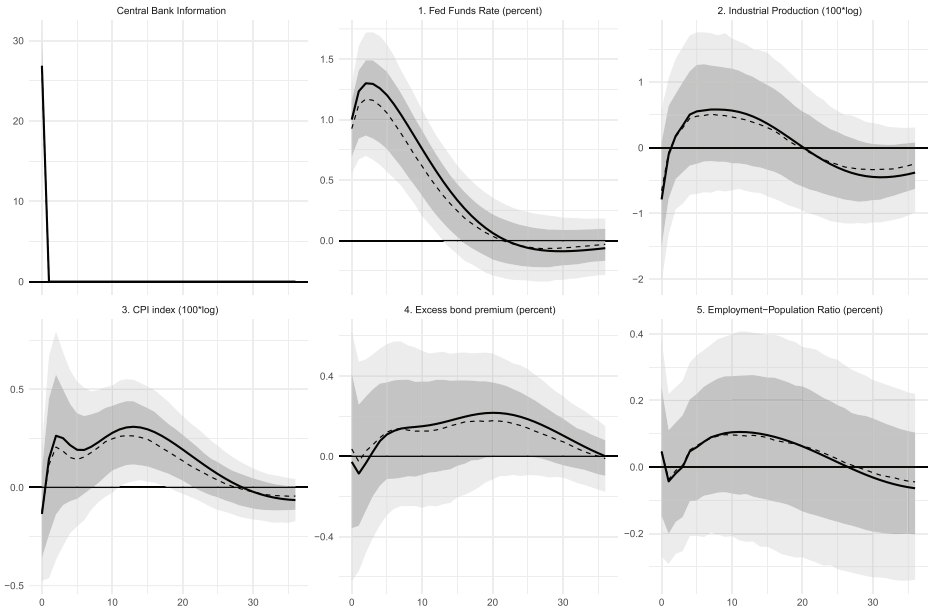
Note: The figure shows the response of the headline variables to a monetary policy shock, scaled to a 1 percentage point increase in the federal funds rate. The estimated VAR includes the five main variables, plus both shocks and five factors. Solid lines are point estimates and shaded regions are the 68 and 90 percent confidence intervals from a bootstrap with $K = 1000$ replications. Dashed lines show median responses from the bootstrap. Data are monthly and the estimation sample is January 1990–December 2019.

the high-frequency series. We verify this in Figures B.3 and B.4 in Appendix B.2 where we present the point estimate for the impulse responses using the shocks as external instruments. As they should, they match the impulse responses in Figures 2 and 3 identically up to a scalar.

4.3 Robustness

In Appendix B.3 we investigate the sensitivity of the estimated structural VAR model to a variety of alternatives. We show there that longer lag lengths make little difference to the impulse responses (Figures B.5 and B.6). Changing the interest series has little impact

**Figure 3. Impulse Responses:
Central Bank Information Shock**



Note: The figure shows the response of the headline variables to a monetary policy shock, scaled to a 1 percentage point increase in the federal funds rate. The estimated VAR includes the five main variables, plus both shocks and five factors. Solid lines are point estimates and shaded regions are the 68 and 90 percent confidence intervals from a bootstrap with $K = 1000$ replications. Dashed lines show median responses from the bootstrap. Data are monthly and the estimation sample is January 1990–December 2019.

either—the responses using a one-year government bond rate in place of the federal funds rate are identical, and when using a three-month rate, only the interest rate response changes slightly (Figures B.7 and B.8). A similar lack of difference holds when variously replacing our headline series with other measures of inflation, activity, financial market variables, labor market activity, and residential investment (see Figures B.9–B.14). We suspect that this stability across alternative specifications is likely a result of the factors that we use to augment our VAR. Since they soak up much of the correlated variation in other series, replacing one or the other

of the headline measures makes little difference to the estimated impulse responses.

We verify the importance of the factors in Appendix Figures B.15 and B.16 where we show that removing the factors has a relatively large impact, with interest rates staying persistently higher and output and employment slower to respond. The inclusion of a relatively rich set of factors also largely explains why our impulse responses differ somewhat from canonical estimates such as those of Jarociński and Karadi (2020). There, the peak decline in industrial policy per unit increase in peak short rates for a monetary policy shock is about three, and is reached at a horizon of around 10–20 months. In contrast, our baseline results have a similar peak response of industrial production, but reach it much faster, at around seven to eight months. They also produce a hump-shaped response for short-term interest rates, which we do not. When we exclude the factors from our VAR, though, these differences largely go away. This is consistent with the notion that we are capturing similar shocks to Jarociński and Karadi (2020) (as Figure A.2 suggests) but the inclusion of a wide set of factors means that we control for other sources of correlated variation. Figure B.17 compares our results directly to the corresponding monthly impulse responses from the online appendix of Jarociński and Karadi (2020), showing that when we remove factors, match variables, and estimate on the same sample we recover very similar impulses for the monetary shock.

We also check that our results are robust to the extension of our shock series to include speeches by policymakers since 2010, as suggested by Swanson (2023). Appendix Figures B.24 and B.25 present the corresponding impulse responses using this shock series and find that they are broadly similar, albeit with a slightly more delayed response of the real activity variables, industrial production and employment.

To be clear, the broader point of this section and the previous one is not to claim that we have the definitive measure of how monetary policy affects the macroeconomy. Rather, it is to convince the reader that the view of the monetary transmission mechanism against which we test the 2022 data is broadly sensible. And although reasonable people may differ in the details of exactly how policy transmits, the baseline view that our impulse responses represent is consistent with mainstream views.

4.4 *Validation*

To check that our method works, we run the filter on the estimation period (1990–2019), backing out the two implied policy shocks from the data alone. This, of course, should work—that is the point of a check—but it is also a chance for our method to fail in case there were something wrong with it.

Appendix Table B.1 assesses the point estimates from our filter, regressing the actual shocks on our inferred shocks. If our filter is truly producing a conditionally unbiased estimate of the shocks, then this regression should have a slope of one—a unit change in the estimated shock predicts a unit change in the actual shock—and have an intercept of zero—no bias. This holds true for both shocks. One less obvious point is that the R^2 is low for both shocks, especially the pure monetary policy shock. This says that much of the variation in the realized shocks cannot be captured by our VAR. One possibility is that our model is misspecified, but the other is simply that this might be unavoidably difficult. In particular, if, as seems likely, other shocks are driving much of the variation in macro outcomes, it will be hard to infer the monetary shocks from macro data.

Whatever the degree of difficulty in recovering shocks from macrovariables, our measure of uncertainty should always reflect this. To check this, Appendix Table B.2 reports the coverage ratios of the realized shocks computing percentile ranges at each point in the estimation sample using the mixture of normals in Equation (8). If our assumptions on the distribution of δ_t and subsequent calculations are correct, then the true shocks should be inside the $x\%$ confidence interval x percent of the time. The column labeled “Data” computes the coverage for the actual data, and suggests that the filter characterizes the 90 and 95 percent intervals fairly well, but perhaps a little wide for the middle of the distribution. This is consistent with the identified shocks being highly leptokurtic, with excess kurtoses of around 13. These findings are important context for the hypothesis tests we apply later, since these tests are based on the distribution of shocks inferred from the filter.

To understand how this result might be affected by the particular realization of the data, we supplement this calculation with two simulation exercises. We first use the estimated data-generating

process to simulate 500 samples of the same length as the true data, and compute coverage ratios for each. These make precise the idea that the filter does a good job of measuring the tails of the distribution of shocks but, in the middle of the distribution, produces confidence intervals which are a little too wide. Specifically, the 95 percent coverage ratios seem well-centered (medians are close to 95 percent) and informative (90 percent of simulated ratios are within 2 percentage points of the truth) for both shocks. For the 90 percent confidence intervals, performance is only fractionally worse and still quite good for the monetary policy shock. But the 68 percent confidence interval seems to be systematically too wide. This means that any tests using this part of the distribution will under-reject the data-generating process different to the null. We return to this point later.

We also generate a single very long simulation of 12,000 observations (i.e., 1,000 years worth of data) and compute the corresponding in-sample coverage ratios. This should give a sense of whether the confidence intervals are consistent, and the extent to which any biases might ease in large samples. In the end, the corresponding coverage ratios look very similar to the data, implying that we have sufficient data to rely on our conclusions.

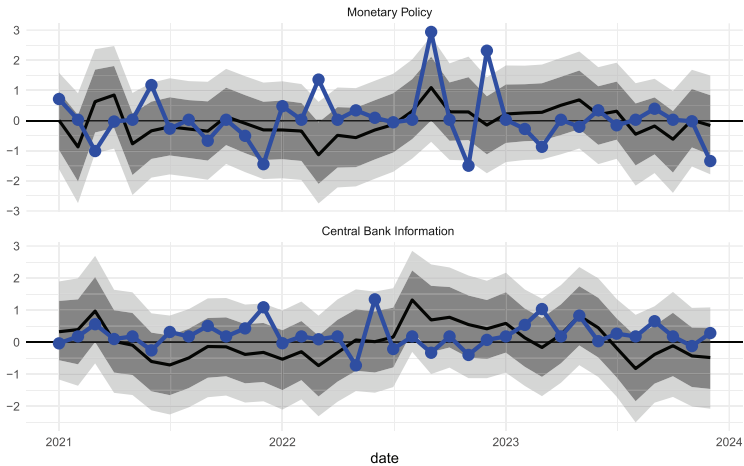
5. Results from the Filter

5.1 *True versus Filter-Implied Shocks*

We apply the VAR-based filter to the post-2021 data. This calculates in each period the distribution of monetary (and other) shocks under the null hypothesis that the data-generating process has remained unchanged from the pre-COVID period. By then comparing the realized monetary shocks from the high-frequency data to this sequence of distributions, we perform an out-of-sample test that the data-generating process (and thus the monetary transmission process) remains unchanged. If the data-generating process is unchanged, the realized shocks should be statistically indistinguishable from the inferred ones.

Figure 4 presents the headline results from this experiment. The shaded regions show the confidence intervals for shocks conditional on the data under the null that the data-generating process

Figure 4. Inferred and Realized Shocks: 2021–23



Note: Solid lines are point estimates of the inferred shocks under the null hypothesis that the data-generating process remains unchanged. The shaded regions are the 68 and 90 percent confidence intervals from a bootstrap with $K = 1000$ replications. The blue line with large dots is the actual shock, computed from high-frequency data.

is unchanged, computed using the distribution in Equation (8). The blue line shows the shocks measured from the high-frequency financial market data. Both series show notable and persistent departures of the realized shocks from the inferred ones. In particular, the measured monetary policy shock exceeds the point estimate from the filter throughout January to July 2022. In other words, the FOMC was repeatedly moving financial markets in ways consistent with tightening, but the macro data were behaving as if they were loosening. That is, the transmission of monetary policy was looser. Likewise, the Fed information shock exceeds the point estimate from April 2021 until April 2022. This means that markets were interpreting Fed announcements at the time as if they were revealing positive information about the state of the economy, but the monthly macro data did not subsequently confirm that.

How sure can we be of these conclusions? The shaded regions in Figure 4 provide a metric to answer this question period-by-period, as they represent the distribution for the data-consistent

shocks, under the null that the transmission of monetary policy is unchanged. As should be apparent from the figure, the evidence in any given month for either shock transmitting differently from usual is not strong—with a few exceptions, the blue dots are typically inside the center of the distribution. Bear in mind, though, the evidence of the previous section that the central confidence intervals are too wide. And so true statistical power is likely understated here. And so the statistical confidence that monetary policy transmission is weaker in any given period is typically low. However, the measured monetary policy shock is not in the upper part of the filter distribution in just one period, but rather for a sequence of periods. Even if we cannot be very confident that the transmission of monetary policy is weaker in any *given* period, perhaps we can pool evidence across these periods to conclude that monetary policy transmission was weaker on average, or at least some of the time. Much of the rest of this section attempts to tackle this issue.

5.2 *Informal Assessment*

We first start with an informal test of this idea, asking: how rare is it that the observed shocks are persistently above or below the filter-implied estimates? It is very rare. For both shocks, the periods are the (sometimes joint) longest streaks where the observed data exceed the point estimate from the filter in the entire sample, starting in 1990. Appendix Table B.4 puts these streaks in context, reporting the longest streaks where the observed high-frequency shocks are consistently above and below the filter-implied point estimates, across the whole sample (1990–2023). This shows that only in times of extreme stress—the Global Financial Crisis and the onset of the COVID-19 pandemic—have there been even a comparable number of consecutive observations of the monetary policy shock above or below the filter point estimates.

5.3 *Formal Assessment: Hypothesis Testing*

Having observed that the measured high-frequency monetary shocks persistently exceeded those implied by our filter during 2022, we now ask: could this pattern have arisen by chance under normal

monetary transmission? Or does it provide statistical evidence that transmission changed?

Defining Unchanged Transmission. To answer this question, we must first be precise about what we mean by “normal” or “unchanged” monetary transmission. In our framework, normal transmission means that the relationship between monetary policy shocks and subsequent macroeconomic outcomes remains the same as in the pre-COVID period (1990–2019). Under this null hypothesis of unchanged transmission, if we observe the macroeconomic data alone—output, inflation, interest rates, and other variables—and use our estimated VAR to infer what monetary shocks must have occurred, these inferred shocks should match the shocks we actually measured in financial markets.

Three Complementary Tests. We formalize this intuition with three statistical tests, each asking a slightly different question about when exactly the null might hold in the post-tightening period.

Test 1 asks: Is there statistical evidence that monetary policy transmission was weaker in *every single* month during this period? This is an extremely stringent test—it requires that in each and every month, the measured shock significantly exceeded what the macro data would suggest under normal transmission.

Test 2 asks: Is there statistical evidence that monetary policy transmission was weaker in *at least some* months during this period? This is a more lenient test that simply asks whether we can detect any change in at least one month.

Test 3 asks: Is there statistical evidence that monetary policy transmission was weaker *on average* across this period? This test pools information across months, asking whether the average discrepancy is statistically significant.

Each test has its own advantages. Test 1 provides the strongest evidence if it rejects the null, but is very hard to satisfy. Test 3 has the most statistical power by averaging across months, but may miss short-lived changes. Test 2 falls between these extremes, asking only whether transmission changed detectably in at least some periods.

Formal Setup. We now state these tests formally. Let $\eta_t = v_t - \bar{\delta}_t$ denote the difference between the measured shock v_t and the mean of the filter-implied distribution $\bar{\delta}_t$ in month t . Under unchanged transmission, the expected value of this difference is zero

in every period: $\mu_t = \mathbb{E}[\eta_t] = 0$ for all t . A positive value of μ_t means that measured monetary tightening exceeded what the macro data would imply under normal transmission—consistent with weaker policy transmission. For any particular period $\mathcal{T}$ our three tests can be written as follows:

Test 1 $H_0 : \mu_t^i \leq 0$ for some $t \in \mathcal{T}$ vs. $H_1 : \mu_t^i > 0$ for all $t \in \mathcal{T}$

Test 2 $H_0 : \mu_t^i \leq 0$ for all $t \in \mathcal{T}$ vs. $H_1 : \mu_t^i > 0$ for some $t \in \mathcal{T}$

Test 3 $H_0 : \bar{\mu}^i \leq 0$ vs. $H_1 : \bar{\mu}^i > 0$, where $\bar{\mu}^i = \frac{1}{|\mathcal{T}|} \sum_{t \in \mathcal{T}} \mu_t^i$.

All three tests are explicitly one-sided, which is important in our application since we have a priori evidence from Figure 4 that μ_t is positive for multiple consecutive periods. Note also that only in Test 3, where we compute a sample mean, does a central limit theorem apply directly. In Tests 1 and 2, we effectively add an extra parameter with each additional observation, and so the power of the test does not grow with the number of periods in $\mathcal{T}$ in the usual way. Indeed, Test 1 is extremely stringent, and the null will be rejected only if we have strong evidence that transmission changed in *all* periods. This stringency is also reflected in how the null is evaluated. The null is satisfied if monetary policy transmission is unchanged in *some* time during $\mathcal{T}$ —which point is not specified. The likelihood-ratio test we employ therefore effectively computes the most likely point within the null set as the comparison point for the test.¹⁴

Results. Table 1 presents p-values for these three tests applied to both the pure monetary policy shock and the central bank information shock. Lower p-values indicate that the data are inconsistent with normal transmission—specifically, that measured monetary shocks were too large relative to observed macro outcomes to

¹⁴This is also why the null hypothesis is formulated as a set rather than a single point. Although it may seem unfamiliar, this formulation is essential for one-sided likelihood-ratio tests when the most likely parameter value under the null depends on the data, as here. We provide complete technical details on the implementation of these likelihood-ratio tests, including computation of rejection regions and p-values, in Appendix C. The interested reader is also referred to Casella and Berger (2002), chapter 8, for the general theory of hypothesis testing with set-valued nulls.

Table 1. P-Values for Joint Hypothesis Tests

Shock	$\mathcal{T}$ Start	$\mathcal{T}$ End	$ \mathcal{T} $	Test 1	Test 2	Test 3
Monetary Policy	2022-02-01	2022-02-01	1	0.349	0.346	0.349
	2022-02-01	2022-03-01	2	0.349	0.012**	0.017**
	2022-02-01	2022-04-01	3	0.349	0.021**	0.021**
	2022-02-01	2022-05-01	4	0.349	0.025**	0.013**
	2022-02-01	2022-06-01	5	0.349	0.036**	0.014**
	2022-02-01	2022-07-01	6	0.468	0.054*	0.021**
Central Bank Information	2021-05-01	2021-05-01	1	0.383	0.382	0.383
	2021-05-01	2021-06-01	2	0.383	0.537	0.316
	2021-05-01	2021-07-01	3	0.383	0.346	0.149
	2021-05-01	2021-08-01	4	0.383	0.363	0.103
	2021-05-01	2021-09-01	5	0.383	0.379	0.074*
	2021-05-01	2021-10-01	6	0.383	0.442	0.072*
	2021-05-01	2021-11-01	7	0.383	0.407	0.046**
	2021-05-01	2021-12-01	8	0.383	0.233	0.017**
	2021-05-01	2022-01-01	9	0.383	0.260	0.014**
	2021-05-01	2022-02-01	10	0.383	0.289	0.013**
	2021-05-01	2022-03-01	11	0.383	0.270	0.008***
	2021-05-01	2022-04-01	12	0.383	0.300	0.007***

Note: The table shows p-values for the joint hypothesis that consecutive observations are drawn from the null. Each line considers three tests that the observed high-frequency shocks are drawn from a distribution with a higher mean than would be expected based on pre-COVID transmission of monetary policy, over a period of consecutive months. Test 1 assesses whether the impact of monetary policy is different at all points during the window of consecutive observations. Test 2 assesses whether it is different at some point. And Test 3 assesses whether it is different on average. The tests are likelihood-ratio tests; see Appendix C for implementation details. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

be explained by chance alone under the pre-COVID relationship between policy and the economy.¹⁵

Several patterns emerge from Table 1. First, as expected, Test 1 never rejects the null for any window—its extreme stringency means we cannot conclude that transmission changed in *every single month*. Second, for the pure monetary policy shock (the upper part of the table), both Tests 2 and 3 show strong evidence of changed transmission, with p-values below 0.05 for most windows starting in February 2022 and extending through mid-year. The evidence is particularly strong for windows ending in March through July 2022. Third, for the central bank information shock (panel B), the evidence is weaker and less consistent across windows, though there is some indication of changed transmission in early 2022.

These results suggest that during the critical early months of the 2022 tightening cycle, monetary policy transmission was detectably weaker than normal. The measured high-frequency shocks indicating tighter policy were persistently larger than what the subsequent macro outcomes would imply under the pre-COVID transmission mechanism. The p-values tell us that this pattern is unlikely to have occurred by chance.

5.4 *The Role of Uncertainty*

The measured uncertainty in our estimates is essential to computing the test statistics for the hypothesis tests above. So what drives them? In Appendix Figure B.29 we decompose the 90 percent confidence intervals from Figure 4 into their contributions from model uncertainty (i.e., that coming from the fact that the true model parameters are unknown) and from the inference uncertainty (i.e., that coming from the filter). The latter is constant, since it reflects the lack of knowledge over the shocks due to missing information, which does not change. Most of the time, this is the main source of uncertainty, and arises because the shocks are fundamentally hard to disentangle. In some periods, however, the contribution from the model uncertainty is substantial. This is notably true in early 2021,

¹⁵Appendix C describes in detail how these are computed. Note that the excess width of the central confidence intervals shown in Table B.2 means that the computed p-values are likely upper bounds on the true values, i.e., these are conservative tests.

and occurs when the reduced-form shocks are largest, since uncertainty over the parameters is amplified by the magnitude of the shocks. This also explains why the contribution from model uncertainty is low in other periods, when smaller shocks imply less of an impact from model uncertainty.

5.5 *Robustness*

In Appendix B.5 we repeat these hypothesis tests for the monetary policy shock using the same set of alternative specifications as for the impulse responses. In Figures B.18–B.23 we find that the results are minimally affected by changes in lag length, changing the short-term interest rate series, or replacing the excess bond premium with the exchange rate or long-term interest rates. Changing the number of factors has some effect, generally strengthening our key result that the transmission of policy was lessened on average. Likewise, replacing the inflation measure with core PCE or including residential investment tends to (weakly) strengthen this result. The only alternative specification which weakens this result at all is where we change the labor variable, to either the unemployment rate or aggregate hours.

In Appendix B.6 we repeat the hypothesis tests using the shock series which includes policymakers' speeches and show that they strengthen our results. In Appendix Figure B.26 we show the post-2020 outturns compared with the filter-inferred shocks. The differences are larger than in the baseline, a fact confirmed by Appendix Table B.3, which rejects unchanged monetary transmission even at the 1 percent level. Similarly, Figures B.27 and B.28 imply that monetary policy is statistically significantly weaker even if the covariance of other shocks is different.

6. Interpretation

In the preceding section, we evaluated the strength of the evidence that the link between the data and high-frequency identified shocks had changed sometime in 2022. But what does it really mean to reject any of the tests laid out above? One obvious interpretation is that it means that the transmission of monetary policy is somehow different. But it does not tell us how much it differs, or in

what ways, nor if other interpretations might be equally valid. This section thus attempts to answer three related questions. Quantitatively, how much weaker would the transmission of monetary policy have to be to reconcile the observed high-frequency shocks with the data? What other factors, such as the changing impact of other shocks, could explain the results of the previous section? And what economic forces might have caused any such weakness?

6.1 Setup

The key ingredients in our filter which maps data into implied structural shocks are the matrices $\hat{D}_y$ and $\hat{\Omega}_y$, which together define the mean and covariance of the filter estimate conditional on the parameters, $\delta_{t|t,\hat{\theta}}$.

We can use modified $\hat{D}_y$ matrices to consider what would happen if the null transmission of the shocks were different by multiplying by a diagonal matrix of positive numbers, Λ , which we partition as

$$\Lambda = \begin{bmatrix} \Lambda_m & 0_{K \times N} \\ 0_{N \times K} & \Lambda_x \end{bmatrix}.$$

Then the modified causal impact matrix is

$$\tilde{D}_y = D_y \Lambda = [D_m \Lambda_m D_x \Lambda_x].$$

This represents the statistical model where the impact of each of the shocks has been scaled by the corresponding entry of Λ .

For example, if the top-left first entry of Λ were 0.9 and all other entries were 1, this would capture the scenario where the transmission of the pure monetary shock was uniformly 10 percent less effective, in a specific and well-defined sense: a given monetary shock moves all outcomes by 10 percent less than in the baseline model. Running the filter on the same data but replacing D_y with $\tilde{D}_y$ would then produce a distribution for the high-frequency indicators under the new null, one where the impact of monetary shocks was 10 percent weaker than usual. Performing our regular hypothesis tests using this modified model would then permit a test of the hypothesis that monetary policy was at least 10 percent weaker on any given sample.

A natural question to ask is: how should one pick the entries of such a modifying Λ ? After all, if we alter some column of the impact matrix D , then without an offsetting modification of the other shocks the data-generating process will no longer have the same covariance as the data. In the example above, proposing that the monetary shocks have 10 percent less impact than usual will mean that some of the variation in the data otherwise explained by the monetary shock is now unexplained by any shock in the model. However, because the other shocks matter only to the extent that they affect the covariance of the data, $\hat{\Omega}_y$,¹⁶ one need only be explicit about the assumptions on these other shocks *up to the extent that they affect this covariance*. This allows for something of an end run around the problem of missing variation. For example, if one posits a modified shock matrix $\bar{D}_y$ where only the monetary shock is affected, then computing the filter with $\hat{\Omega}_y$ unchanged will recover the distribution of monetary shocks under the assumption that the unidentified shocks change *in whatever ways are needed* to guarantee that the data-generating process still matches that estimated from the sample. The cost of this shortcut is that the time-series distributions for the other, unidentified, shocks will not be correct. But we are not interested in those. Of course, if one thinks that the variance of the data has changed on the test period, one can replace $\hat{\Omega}_y$ with something different. We return to this point later, in Section 6.3.

6.2 The Changing Impact of Monetary Policy

Having established statistical evidence that monetary transmission changed during 2022, we now quantify the magnitude of this change. How much weaker was monetary policy during this period?

To answer this question, we conduct a series of hypothesis tests under different counterfactual scenarios. Specifically, we ask: if monetary policy had been only λ times as effective as normal (for λ between 0 and 1), would the 2022 data still be statistically inconsistent with this modified transmission mechanism? This exercise allows us to identify the range of effectiveness levels that are consistent with the observed data.

¹⁶See Equation (7) and subsequent discussion.

Methodology. We implement this by considering a series of modified structural impact matrices:

$$\tilde{D}(\lambda_1) = D_y \times \begin{bmatrix} \lambda_1 & 0 & \cdots & 0 \\ 0 & 1 & & 0 \\ \vdots & & \ddots & \vdots \\ 0 & 0 & \cdots & 1 \end{bmatrix}$$

for $\lambda_1 \in (0, 1)$. When $\lambda_1 = 1$, we have our baseline case of unchanged transmission. When $\lambda_1 < 1$, monetary policy is less effective than normal. For instance, when $\lambda_1 = 0.75$, monetary policy is only 75 percent as effective—meaning the contemporaneous impact of a monetary shock on all macrovariables is scaled down by 25 percent.¹⁷

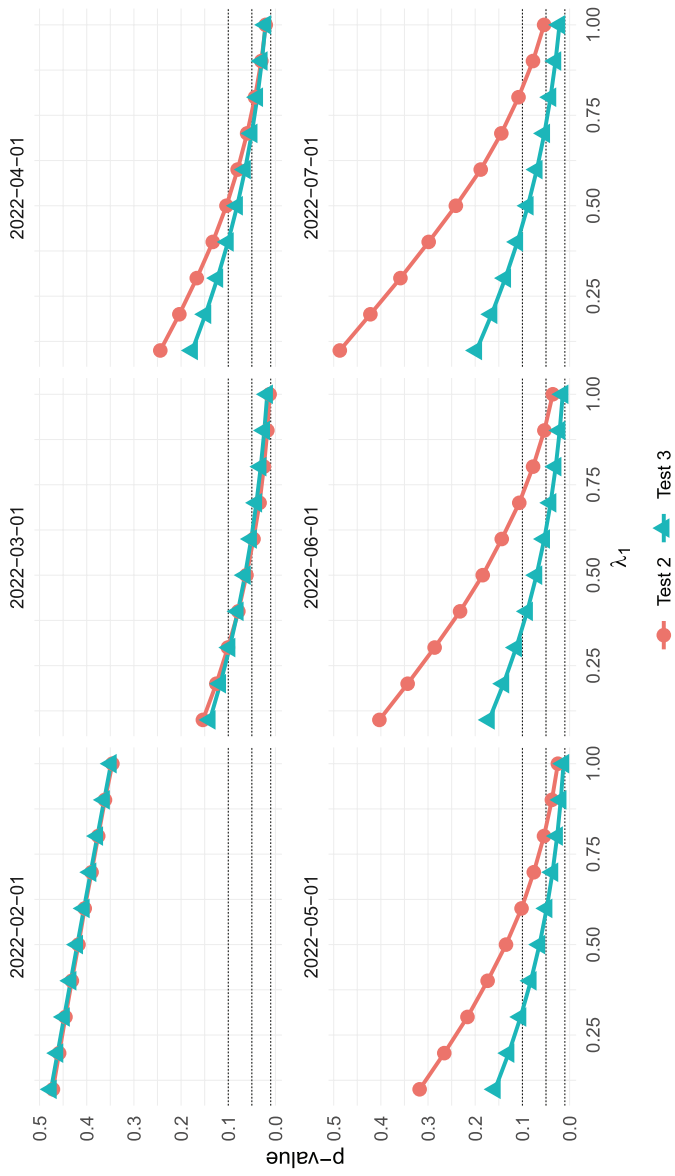
For each value of λ_1 , we rerun our hypothesis tests from the preceding section, replacing D_y with $\tilde{D}(\lambda_1)$. Given the excessive stringency of Test 1, which never rejects any null, we focus on Tests 2 and 3 in this and subsequent exercises. Figure 5 plots the resulting p-values for six monthly windows $\mathcal{T}$ starting in February 2022 and ending in the month indicated in each panel. The connection to Table 1 is that the rightmost values (i.e., at $\lambda_1 = 1$) match the numbers in the corresponding columns of that table.

Results and Interpretation. Figure 5 shows that the results of this exercise are clearest in March and April 2022. During this time, p-values for any test with $\lambda_1 > 0.75$ are less than 5 percent. This implies that we can reject *any* scenario where monetary policy was more than 75 percent as effective as its pre-COVID norm during this time.

The figure also reveals important variation in the strength of our findings over time and across tests. The evidence for reduced effectiveness is strongest for windows ending in March and April 2022. For the March window, we can even reject effectiveness above approximately 50 percent for both tests at the 5 percent significance level. As the window extends later into 2022, the evidence weakens

¹⁷This assumes that the residual covariance in the post-2022 period, $\hat{\Omega}_y$, is unchanged. Because other shocks only matter to the extent that they affect the covariance of the reduced-form residuals, these results hold identically no matter how other non-monetary shocks might have changed, so long as the *covariance* of the data is unaffected. We relax this assumption in Section 6.3.

Figure 5. P-Values for Hypothesis Tests as Strength of Monetary Policy Shock Varies



Note: The figure shows p-values for hypothesis tests described in Section 5.3, under a series of nulls, indexed by λ_1 . Horizontal lines show 1, 5, and 10 percent confidence levels. The interpretation of λ_1 is that it represents a null where monetary policy is only λ_1 as effective as pre-COVID. Each panel computes the corresponding hypothesis tests on a window starting in February 2022 and ending in the titular month. The values for both lines when $\lambda_1 = 1$ thus match the corresponding columns in Table 1.

somewhat: for the July window, we can still reject $\lambda_1 > 0.75$ at the 5 percent level on average. This pattern suggests that the weakening of monetary transmission was most pronounced in the very early stages of the tightening cycle.

Test 2, which asks whether transmission was weaker in at least some periods, generally produces similar conclusions but with somewhat weaker statistical evidence (higher p-values for any given λ_1). This is consistent with our earlier finding that pooling information across periods (Test 3) provides more statistical power than looking for changes in individual periods (Test 2).

Figure 5 also makes clear some of the limitations of the evidence that we present. For example, at a 2 percent significance level, we would conclude that monetary policy was weaker only in March, and even then only incrementally.

Economic Impact. In economic terms, this finding implies that during the critical early months of the tightening cycle, interest rate changes had substantially muted effects on output, inflation, and other macrovariables. To achieve the same degree of macroeconomic tightening that a 0.75 percentage point rate increase would have delivered pre-COVID, the Fed would have needed to raise rates by approximately 1 percent. Alternatively, one could say that each of the Fed's actual rate increases during this period had roughly three-quarters of its normal impact.

6.3 *Allowing for Changing Variances*

It is, of course, possible that the nature of other shocks changed during 2022. After all, the post-COVID period was a highly unusual one with many of the after-effects of a once-in-a-lifetime global pandemic still being worked out. Such changes in other shocks could affect our interpretation of the results.

One of the advantages of our approach is that we can capture the impact of such changes on our filter-implied monetary shocks without specifying exactly how these other shocks changed. This is fortunate, since we do not identify them. Instead, it is enough to describe solely how any such changes would have affected the reduced-form residual covariance matrix, $\hat{\Omega}_y$. Indeed, if the reduced-form residual covariance matrix had changed during the test period,

this would undermine that interpretation of our results as capturing a change in the efficacy of monetary policy.¹⁸

For example, because the filter expects that tighter monetary policy will, at the margin, produce lower inflation—see Figure 2—it will interpret the persistence of inflation in spite of high interest rates as a sign of weaker monetary policy. Of course, other offsetting shocks could be driving the slow decline in inflation—for example, continuing supply disruptions in the post-COVID recovery. But the filter adjusts for this based on the usual correlation of the other macrovariables. However, if the variance of those offsetting shocks has changed, then the filter will account for them incorrectly.

Appendix Figure B.30 presents preliminary evidence that the reduced-form covariance has indeed changed. It shows the standard deviations of the reduced-form residuals for the data series in the VAR. Every series shows an increase in the standard deviation, in some cases more than doubling. And so in Appendix Figure B.31, we repeat the exercise in Figure 5, but replacing $\hat{\Omega}_y$ with the covariance matrix of the VAR residuals from 2021 onward. Given that we use the same D_m matrix, the interpretation is that we are testing whether the impact of monetary policy has changed, allowing for any excess volatility in the data to be explained by changing variances of the other structural shocks. The results do somewhat mitigate our earlier findings on the transmission of monetary policy. Nevertheless, the implication that policy was weaker than expected in at least March and April of 2022 remains robust. Moreover, this experiment sets a relatively high threshold for concluding that the impact of monetary policy has changed, since the residual shock covariance itself will be imprecisely estimated and including the full post-2021 period may not be representative of the population covariance during 2022.

6.4 *Implications from a Medium-Scale Macro Model*

In Section 6.2 we attempted to quantify how much monetary policy transmission might have weakened, considering the case where

¹⁸A further possibility that we do not investigate is that the autoregressive coefficients of the VAR also changed. The brevity of the post-tightening period means that such an exercise will likely be hopelessly imprecise.

weaker monetary policy affects all variables equally. Although a reasonable starting point, this is somewhat restrictive. Even the very simplest macroeconomic models contradict this assumption.

For example, in the well-known standard New Keynesian model, both a steeper Phillips curve or a flatter intertemporal IS curve would weaken monetary transmission, in the sense of dampening the response of output to a monetary policy shock. However, the change in the impact of monetary policy on output varies across these two cases, and in neither scenario is it the same as the marginal impact on inflation. We show this explicitly for the textbook model of Galí (2015) in Appendix D. The intuition is that a change in the slope of the IS curve affects how a given change in the real rate affects growth of the output gap, leaving the relationship between the output gap and inflation unaffected. However, a change in the slope of the Phillips curve affects the relationship between the output gap and inflation. The result is that although a flatter intertemporal IS curve will always reduce the impact of a given monetary tightening on inflation and output, a steeper Phillips curve may result in a smaller output response but a *larger* inflation response to monetary tightening.¹⁹

Given this concern, not only do we extend our framework to test whether the data are consistent with weaker transmission in general, but we also try to identify the underlying economic source of that weakness. Specifically, we make the following assumptions:

- A structural model is a mapping from a history of shocks $\epsilon_0, \dots, \epsilon_t$ to data y_t parameterized by a vector θ .
- Shocks are normal, independent of each other and their lags, and have unit variance.
- The model is invertible, in that the solution can be approximated arbitrarily precisely by sufficiently long finite-order VAR:

$$y_t = \sum_{j=1}^J B_j(\theta)y_{t-j} + D(\theta)\epsilon_t.$$

¹⁹Under some conditions—most notably, if monetary shocks are sufficiently persistent.

- The first entry of the shock vector is a monetary shock ϵ_t^m , and the first column of $D(\theta)$ is the contemporaneous response of the model to this shock, which we write as $D_m(\theta)$.
- For some baseline value of the parameters θ_0 , the model produces the same monetary impulse responses as our empirical estimates. That is,

$$D_m(\theta_0) = \hat{D}_m \quad B_j(\theta_0) = \hat{B}_j \quad \forall j.$$

- For the same θ_0 , the model produces the same residual covariance as our empirical estimates:

$$D(\theta_0)D(\theta_0)' = \hat{\Omega}_y.$$

If the preceding assumptions hold true, then when $\theta = \theta_0$ the model represents exactly the same view of monetary transmission (since the impulse responses are identical). Moreover, for any given observation y_t , the filter distribution in Equation (7) is exactly the same whether one uses the empirical estimates or their model equivalents.²⁰ In other words, the structural model is an equivalent to our estimated statistical model in that it will rationalize a given set of observations as being consistent by exactly the same distribution set of monetary shocks.

The advantage of using a structural model to rationalize our empirical results is that we can run more informative counterfactual tests rather than just uniformly weaker monetary transmission. In particular, for any alternative parameter value θ_1 we can rerun our hypothesis tests replacing $\hat{D}_M$ with $D_m(\theta_1)$, $\hat{B}_j$ with $B_j(\theta_1)$, and $\hat{\Omega}_y$ with $D(\theta_1)D(\theta_1)'$. This then reformulates the null as a data-generating process consistent with a structural model with the new parameters. The alternate parameter values required to fail to reject the null can then offer some insight into what might have changed to make the transmission of monetary policy weaker in 2022.

To implement this in practice, we take the well-known model of Smets and Wouters (2007) as a baseline for the U.S. economy. In our main exercise, we parameterize the model to fit the estimated

²⁰Why? Because $B_j = B_j(\theta_0)$ means that the reduced-form residual $\hat{\epsilon}_t^y$ is the same, and because $D_m(\theta_0) = \hat{D}_m$ and $D(\theta_0)D(\theta_0)' = \hat{\Omega}_y$ mean that the other ingredients of this formula are identical.

impulse response. This determines the values of the vector of parameters θ_0 (see Appendix D.2 for details).²¹ We then consider alternative values for three elements of the parameter vector: the Calvo reset probability for prices, the Calvo reset probability for wages, and the intertemporal elasticity of substitution (EoS). These have different economic impacts, affecting the wage Phillips curve, the price Phillips curve, and the IS curve, respectively. For each alternate parameter vector θ_1 , we re-solve the model and compute the response to a monetary shock. This gives values for $D_m(\theta_1)$, $B_j(\theta_1)$, and $D(\theta_1)$, which we can use to calculate the distribution of the test statistic under the alternate null.

Appendix Figure B.32 reports the results of this exercise for alternate values of the two Calvo reset parameters. This shows that perturbing the Calvo probabilities so that the slope of either the wage or price Phillips curves increase relative to the baseline raises the p-value of the test. That is, the data in 2022 cannot reject the possibility that the wage or price Phillips curves steepened. Of course, this increase is not limitless; for sufficiently steep Phillips curves the p-value falls again; very steep Phillips curves are also inconsistent with the 2022 data.

To capture the range of structural interpretations of our model consistent with the data, Table 2 reports the range of slope coefficient with p-values in excess of 5 percent, in the columns labeled “Acceptance Region.” These correspond to the parts of Appendix Figure B.32 above the 5 percent horizontal line. In relative terms, the acceptance regions for the two Phillips curves are remarkably similar. The data are consistent with both price and wage Phillips curves, which are as much as 25 to 50 percent larger than the baseline under Tests 2 and 3, respectively.²² However, the results from altering the IS curve are somewhat counterintuitive, implying that the data would be consistent with a steeper IS curve. One important difference here is that the relative change in the IS curve is much smaller, implying that the data would only admit a fractionally steeper IS curve before rejecting the model once more.

²¹The model is not able to match our impulse responses very closely. So to check the extent to which this might affect our results, we also apply our method using the original model calibration of Smets and Wouters (2007). The results are similar; see Appendix B.10.

²²Because $0.042/0.033 \simeq 0.62/0.51 \simeq 1.25$ and $0.051/0.033 \simeq 0.75/0.51 \simeq 1.5$.

Table 2. Summary Results: Changing IS and Phillips Curve Slopes
in a Structural Model, Impulse Response Matched Parameter Values

Slope of	Baseline	Parameter Changed	Test 2		Test 3	
			Acceptance Region	Max p-value	Acceptance Region	Max p-value
Phillips Curve	0.033	ξ_p , Price Calvo Probability	(0.033, 0.043)	0.21	(0.032, 0.051)	0.2
Wage Phillips Curve	0.51	ξ_w , Wage Calvo Probability	(0.5, 0.63)	0.17	(0.49, 0.76)	0.16
IS Curve	1.7	σ_c , Intertemporal EoS	(1.7, 1.8)	0.16	(1.7, 2)	0.15

Note: The table reports the results of perturbing the Smets and Wouters (2007) model of the U.S. economy by various parameters and using the corresponding impulse response functions in our tests. The model is estimated using the impulse response matching approach; see Appendix D.2.

Moreover, to the extent that p-values can be treated as a metric of how consistent data are with a given hypothesis, the higher peak p-values of the alternate price Phillips curve models suggest that this is a more plausible interpretation of our results.²³

Overall, we consider the results of this section as being broadly indicative of the idea that the weaker transmission of monetary policy was due to a steepening of the price (or wage) Phillips curve. A steeper IS curve could be an alternate explanation, but the magnitude would be much smaller and the result less statistically plausible. However, one should not over-interpret these results. We think of them as offering a plausible approximate structural interpretation for our empirical findings, rather than conclusive evidence.

7. Conclusions

In this paper we propose a general method for assessing whether a dynamic data-generating process has changed when one has externally identified shocks. We applied it to the question of whether the transmission of monetary policy in the U.S. was different during the 2022 tightening cycle.

We conclude that during the early months of 2022 (particularly February through July), the transmission of monetary policy to the macroeconomy was materially weaker than in the pre-COVID period. The data are statistically inconsistent with unchanged transmission and point to monetary policy having roughly 75 percent of its normal effectiveness during this period—meaning the Fed would have needed to add one extra interest rate hike for every three of a given size to achieve comparable macroeconomic effects. This finding is particularly robust for March and April 2022. When we allow for changing variance in other shocks (by using post-2021 covariance), this conclusion weakens somewhat but remains statistically significant for the key early months. When we include speeches by all Federal Reserve Board members in our shock series, the evidence of reduced transmission strengthens considerably.

The core of this paper is in the method for inverting the VAR to back out specific shocks. The strength of our approach is twofold.

²³Rerunning this exercise using the Smets and Wouters (2007) original calibration produces very similar results; see Appendix Table B.5.

First, it takes seriously the concern that other shocks may be acting to mask the impact of monetary policy, and thus giving the appearance of an altered transmission mechanism. Second, by maintaining a fixed pre-test null and by focusing on the changing response of just monetary shocks, we can get reasonable statistical power even when looking at relatively short periods. This contrasts to other approaches, such as time-varying parameter methods or subsample reestimation, where the quantity of changing parameters impedes inference.

This generality comes at a price, though. Although we can robustly test whether monetary transmission changed, it is rather harder to say why. We attempt to offer some explanation for our results in Section 6 by coercing a standard macro model to match our pre-COVID impulse responses and seeing how much it must be changed to fail to reject the 2022 data. And although this gives us some evidence that a steeper Phillips curve may be to blame, we do not see this as conclusive.

More generally, though, our method has broad scope for wider application, to almost any setting where one has a well-identified shock and wishes to assess whether the propagation of that shock has changed. For example, using narrative identification of fiscal shocks, one might be able to more precisely examine the extent to which public spending multipliers are state-dependent. Similarly, an identified set of exchange rate shocks could allow one to assess whether the strength of the pass-through to inflation has changed in response to a new policy environment. We leave these applications for future research.

Appendix A. Data

A.1 Details on the Construction of the “Raw” Shock Series

We use the price of “fourth,” or three-month-ahead, federal funds futures (FF4), as high-frequency data for interest rates.²⁴ For the stock market, we use the S&P 500 index. We use tick-level data purchased from Bloomberg ranging from January 2008 to December

²⁴We transform the price p of the federal funds future to an interest rate by setting $p = 100 - R$, where R is the arithmetic average of the daily effective federal funds rates during the contract month.

2023. The window to compute “raw” changes in the variables starts 10 minutes before FOMC policy announcements and ends 20 minutes after the announcement, as is common in the literature. We use data from Gürkaynak, Karasoy-Can, and Lee (2022) to extend the “raw” shock series back to the late 1980s.

More concretely, let $p_{t_1}^{FF4}$ be the last traded price of the fourth federal funds future (FF4) before the event window starts, and $p_{t_2}^{FF4}$ be the first traded price of FF4 once the event window ends. The raw shock for FF4 ξ_t^{FF4} is calculated as

$$\xi_t^{FF4} = p_{t_2}^{FF4} - p_{t_1}^{FF4}.$$

Let $p_{t_1}^{SP500}$ be the last traded value of the S&P 500 index before the event window starts, and $p_{t_2}^{SP500}$ be the first value of the S&P 500 index once the event window ends. The raw shock for S&P 500 ξ_t^{SP500} is calculated as

$$\xi_t^{SP500} = \ln p_{t_2}^{SP500} - \ln p_{t_1}^{SP500}.$$

A.1.1 Summary Statistics and Plot for Raw Shocks for FF4 and S&P 500

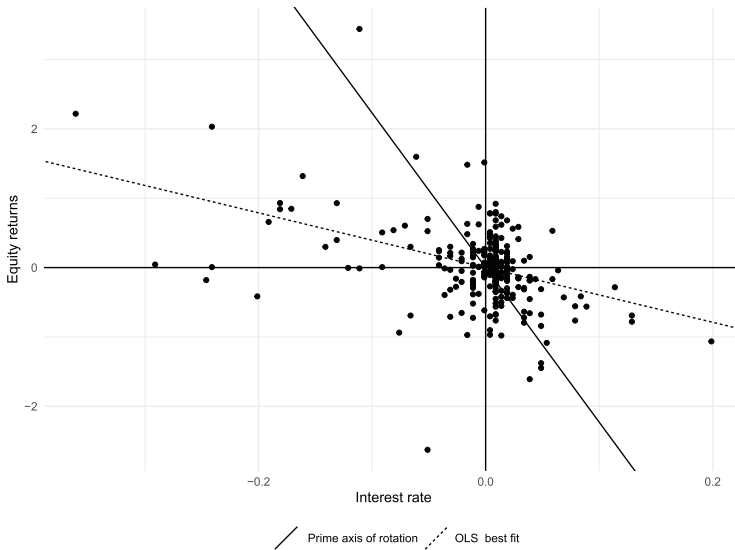
Summary statistics for the changes in the fourth federal funds future and the S&P 500 stock market index over the 30-minute event window are displayed in Table A.1. The total number of observations for FF4 is 298, and for S&P 500 is 297. The shocks are small: the mean absolute change is 2.77 basis points for FF4 (36.1 basis points for S&P 500) over the full sample, with a standard deviation of 5.48 for FF4 (55.8 for S&P 500). The mean absolute change is slightly smaller since the pandemic, with 1.55 basis points for FF4 and 34.3 basis points for S&P 500. Our sample includes 31 FOMC meetings since March 2020, which we consider to be the point in time in which the COVID-19 pandemic started in the U.S.

Figure A.1 shows the cross-sectional plot of the raw shocks, m_t , over the estimation period. The solid line shows the first column of H . Each point can be uniquely decomposed into a distance along this line and a distance perpendicular to it. These distances are the entries in v_t and represent our pure monetary policy shock and central bank information shocks, respectively.

Table A.1. Summary Statistics for Raw Shocks for Federal Funds Future (FF4) and S&P 500 Stock Index (SP500), Both in Basis Points

Statistic	FF4		SP500	
	All	< 03/2020	≥ 03/2020	All
Mean	-1.21	-1.39	0.35	1.51
Mean (Absolute)	2.77	2.91	1.55	36.06
Standard Deviation	5.48	5.71	2.31	55.76
Minimum	-37.00	-37.00	-6.00	-188.03
Maximum	12.00	12.00	5.50	407.58
Observations	298	267	31	297
Obs > Zero	94	81	13	149
Obs = Zero	77	67	10	3
Obs < Zero	127	119	8	145
Note: The raw shocks are the change in the price of FF4 and the basis point change in the S&P 500 stock index over the 30-minute event windows. “All” refers to full sample ranging from January 1990 to January 2024, “< 03/2020” to before March 2020, and “≥ 03/2020” to since March 2020.				

**Figure A.1. High-Frequency Data:
Rotational Decomposition**



Note: The figure shows changes in the interest rate and equity returns in 30-minute windows around FOMC meetings, aggregated monthly during 1992–2019. The prime axis of rotation corresponds to the vector in the first column of H , so ψ is the angle between it and the x-axis.

A.2 Details on the Construction of Shock Series for Speeches

The fundamental idea to construct high-frequency monetary shocks from speeches by policymakers is the same as for FOMC meetings. We take the change in asset prices around the event dates, as described in Sections 3.1 and 3.2, and Appendix A.1. To follow this procedure, we have to collect exact dates and times of the respective speeches, and choose an appropriate event time window. We collect dates of speeches of all seven members on the Federal Reserve Board of Governors from the homepage of the Board of Governors of the Federal Reserve System.²⁵ Starting from year 2010, this page also links the speech as a pdf document, including the time the

²⁵See <https://www.federalreserve.gov/newsevents/speeches.htm>.

**Table A.2. Data on Federal Reserve Board
of Governors Speeches: Summary**

Board Member	Number of Speeches	Earliest Speech	Latest Speech
Michael Barr	16	Sep. 2022	Dec. 2023
Ben Bernanke	68	Feb. 2010	Jan. 2014
Michelle Bowman	65	Feb. 2019	Dec. 2023
Lael Brainard	98	Dec. 2014	Jan. 2023
Richard Clarida	42	Oct. 2018	Nov. 2021
Lisa Cook	16	Oct. 2022	Nov. 2023
Elizabeth Duke	26	Jan. 2010	May 2013
Stanley Fischer	38	Jul. 2014	Sep. 2017
Philip Jefferson	12	Oct. 2022	Nov. 2023
Donald Kohn	4	Jan. 2010	May 2010
Jerome Powell	102	Feb. 2013	Dec. 2023
Randal Quarles	54	Nov. 2017	Dec. 2021
Sarah Bloom Raskin	13	Nov. 2010	Jul. 2013
Jeremy Stein	15	Oct. 2012	May 2014
Daniel Tarullo	44	Feb. 2010	Apr. 2017
Christopher Waller	32	Mar. 2021	Nov. 2023
Kevin Warsh	4	Feb. 2010	Nov. 2010
Janet Yellen	60	Oct. 2010	Nov. 2017

Note: The table shows summary information on the speeches by members of the Board of Governors of the Federal Reserve System used to construct monetary policy shocks. The table shows the speakers in the sample, the number of speeches in the sample, the month and year of the earliest speech in the sample, and the month and year for the latest speech in the sample.

speech was released or delivered (including the time zone).²⁶ Table A.2 shows the list of speakers in the sample. We follow Swanson and Jayawickrema (2023) and set the event window to 120 minutes. Using this procedure, we are able to use 709 speeches ranging from 2010 to 2023 to construct raw shocks from both the fourth federal funds futures (FF4) and the S&P 500 index.

As with regular meetings, we aggregate to monthly frequency by taking the sum of the individual shocks within a given month.

²⁶Speeches by regional Reserve Bank presidents could be used as additional monetary policy events, especially for the members who serve on the FOMC. Since we generally couldn't locate exact times when these speeches were delivered, or they were not available in a format to process them easily, we focus here on speeches by members of the Federal Reserve Board of Governors.

A.3 High-Frequency Shocks: Comparison with Jarociński and Karadi (2020)

Figure A.2. High-Frequency Shocks: Comparison with Jarociński and Karadi (2020)

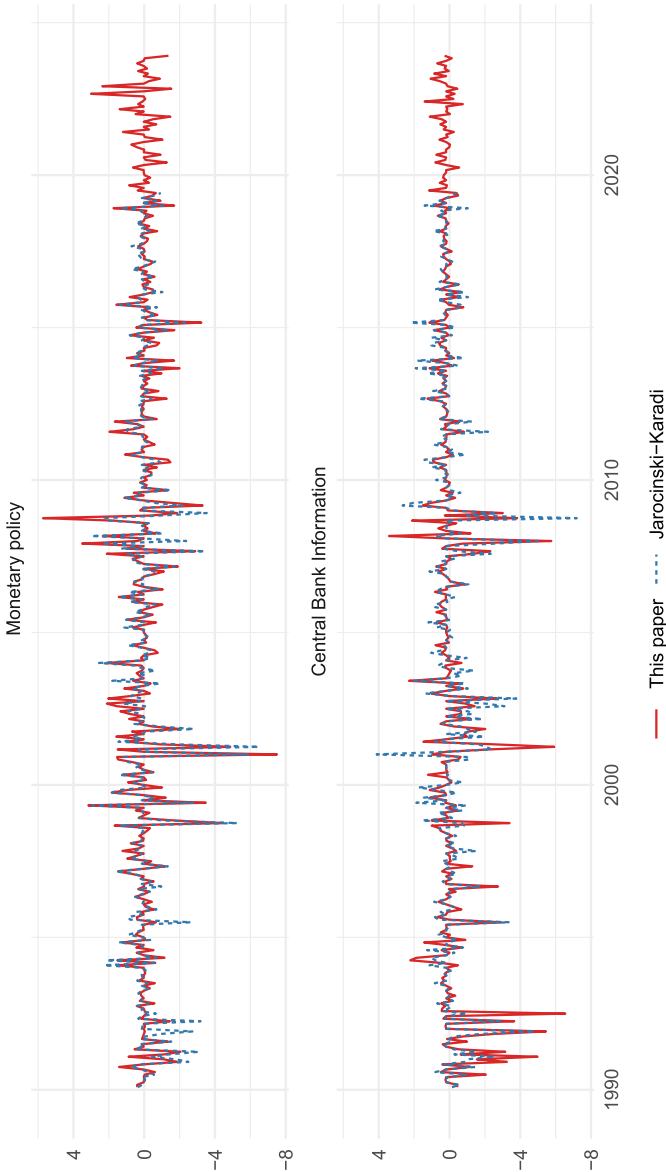
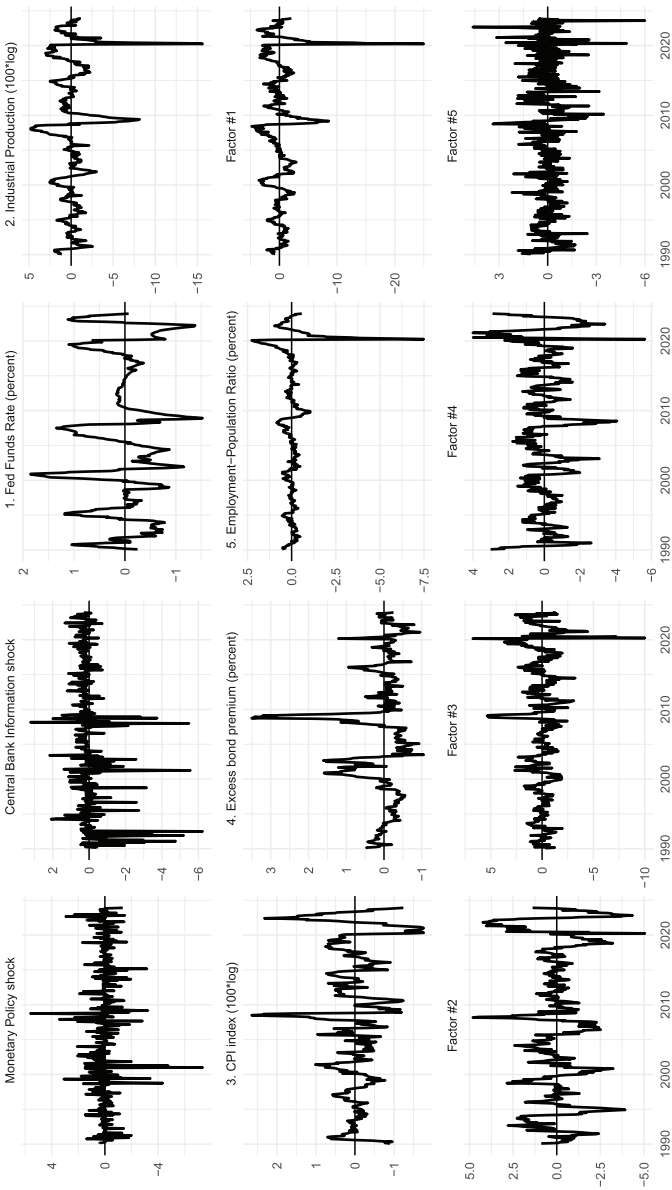


Table A.3. Data and Sources

Variable	Source	Transformation
Included Directly: Federal Funds Rate Industrial Production Consumer Price Index Employment-Population Ratio Excess Bond Premium For Inclusion as Factors: 3-Month Treasury Rate 1-Year Treasury Rate 10-Year Treasury Rate U.S. Dollar Broad Effective Exchange Rate S&P 500 Index CPI Inflation PCE Deflator Real Oil Price, CPI Deflated Commodity Price Index Federal Government Outlays Federal Government Receipts Unemployment Rate Average Weekly Hours Average Hourly Earnings Residential Investment Manufacturing Orders Consumer Confidence Real Personal Consumption	Federal Reserve Board Federal Reserve Board Bureau of Labor Statistics Bureau of Labor Statistics Federal Reserve Board Federal Reserve Board Federal Reserve Board Federal Reserve Board Bank for International Settlements Standard & Poors/Dow Jones Bureau of Labor Statistics Bureau of Economic Analysis Energy Information Administration Commodity Research Bureau U.S. Treasury U.S. Treasury Bureau of Labor Statistics Bureau of Labor Statistics Bureau of Labor Statistics Bureau of Economic Analysis U.S. Census Bureau Conference Board Bureau of Economic Analysis	Level Log Level Log Level Level, Percent Level Level Level Level Log Level Log Level Percent Log Level Log Level Log Level Log Level Log Level Log Level Level, Percent Log Level Log Level Log Level Log Level Log Level Level Log Level
Note: All variables are monthly and deseasonalized, and detrended using a Hodrick-Prescott filter. The division of series into those included directly and those which are included as factors follows the baseline specification.		

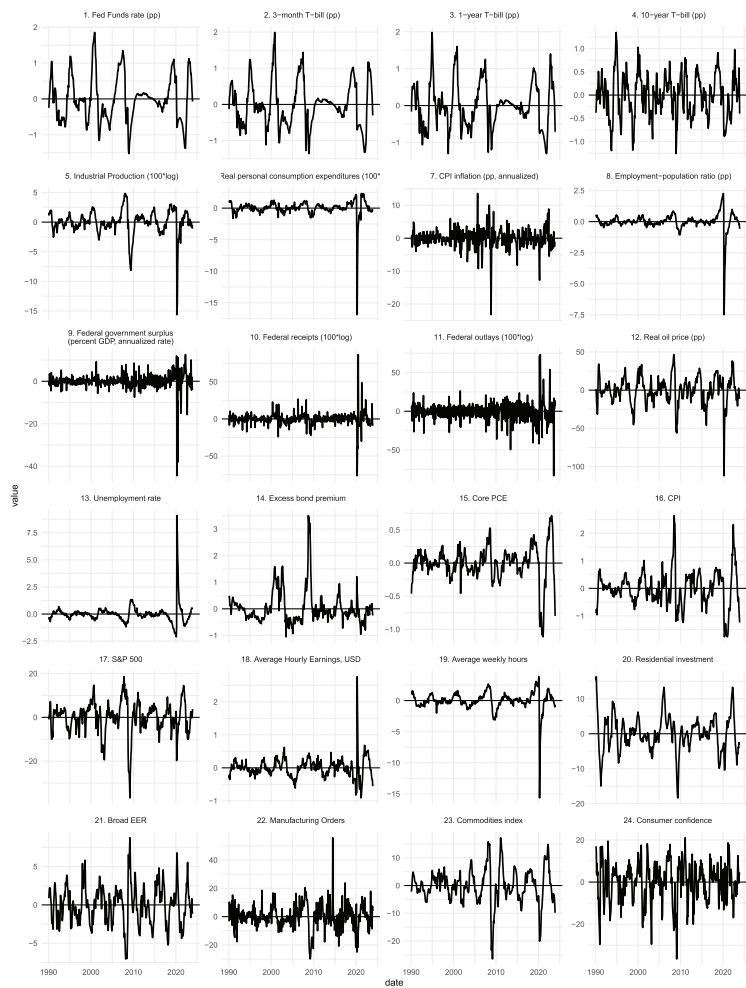
A.5 Time-Series Plots

Figure A.3. Data in the Baseline VAR



Note: The figure shows the data series in the baseline vector autoregression including factors, after transformations.

Figure A.4. All Time-Series Data

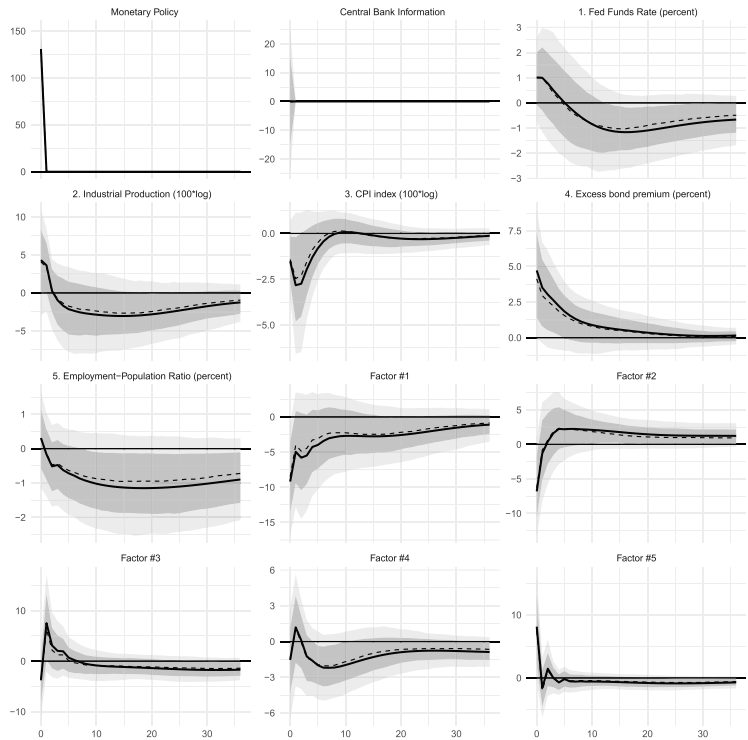


Note: The figure shows the full set of data in Appendix Table A.3, after transformations.

Appendix B. Additional Results

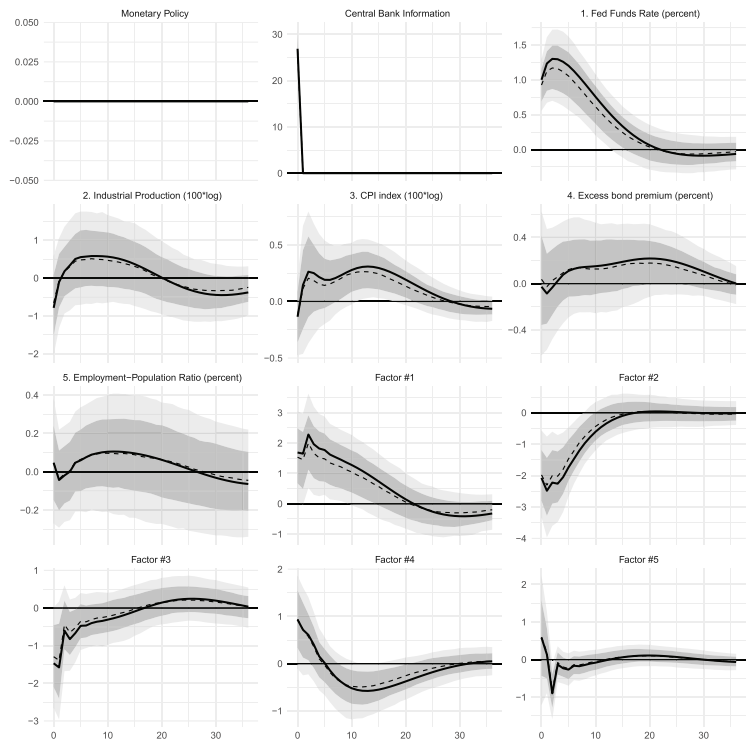
B.1 Additional Impulse Response Functions

Figure B.1. Impulse Responses: Monetary Policy Shock



Note: The figure shows the response of the headline variables to a monetary policy shock, scaled to a 1 percentage point increase in the federal funds rate. Solid lines are point estimates and shaded regions are the 68 and 90 percent confidence intervals from a bootstrap with $K = 1000$ replications. Dashed lines show median responses from the bootstrap.

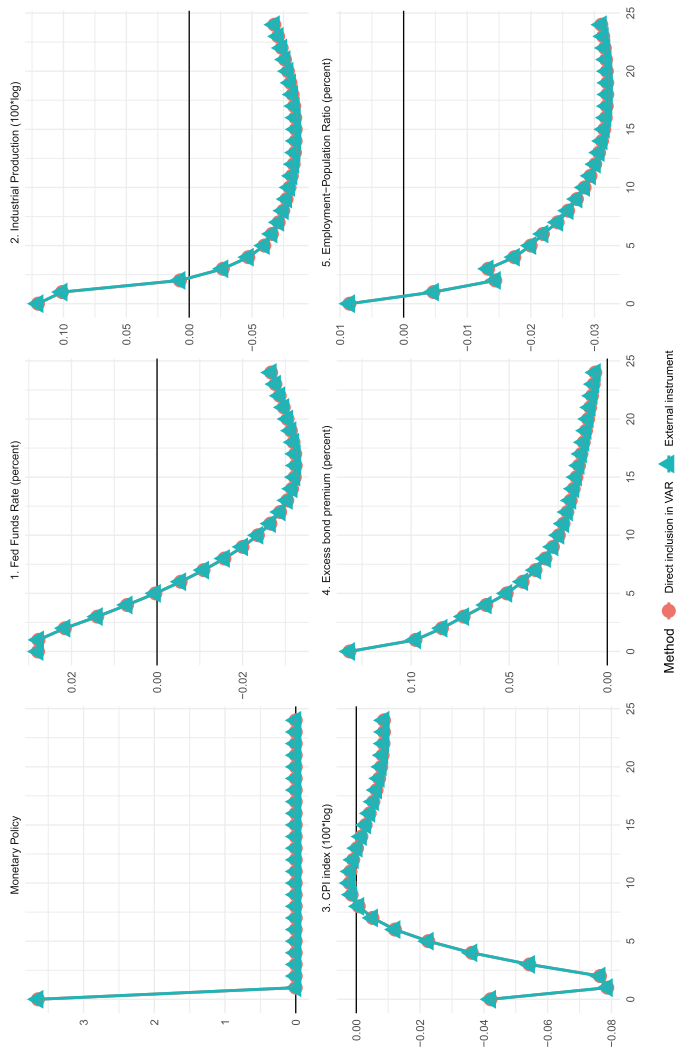
Figure B.2. Impulse Responses: Central Bank Information Shock



Note: The figure shows the response of the headline variables to a monetary policy shock, scaled to a 1 percentage point increase in the federal funds rate. Solid lines are point estimates and shaded regions are the 68 and 90 percent confidence intervals from a bootstrap with $K = 1000$ replications. Dashed lines show median responses from the bootstrap.

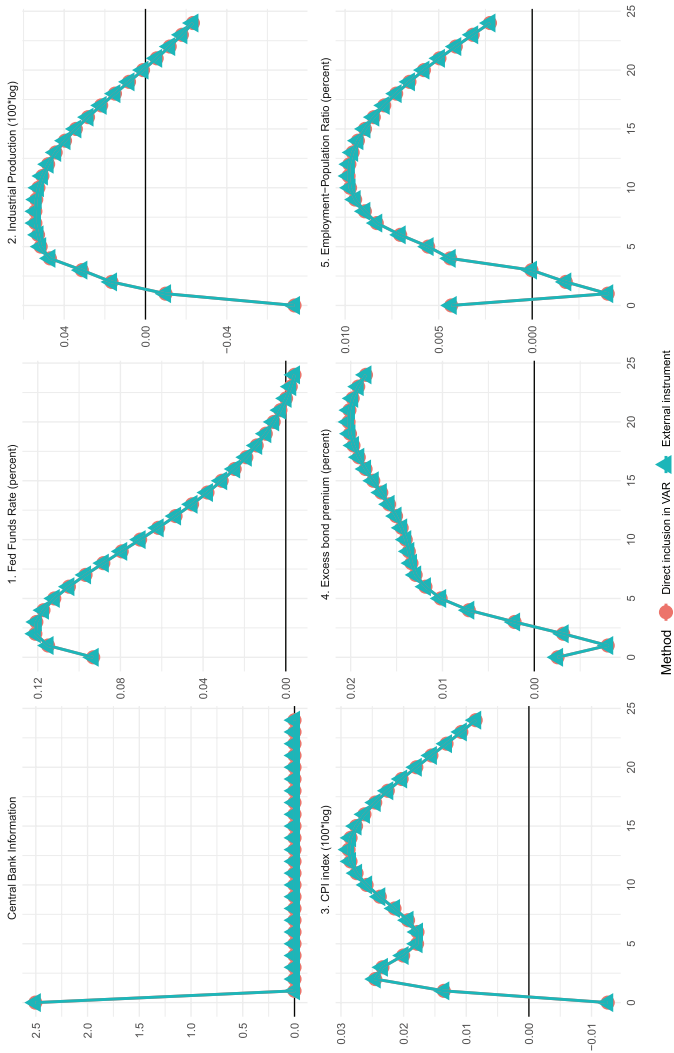
B.2 External Instruments

Figure B.3. Direct Inclusion versus External Instruments: Monetary Policy Shock



Note: The line labeled “External instrument” shows the point estimate of the response of the headline variables to a one-standard-deviation shock computed using the high-frequency monetary policy series as an external instrument. The line labeled “Direct inclusion in VAR” shows our baseline impulse response functions (IRFs), rescaled to match the same initial impulse as for the external instrument. The five factors are included in the estimation but, for clarity, their responses are omitted.

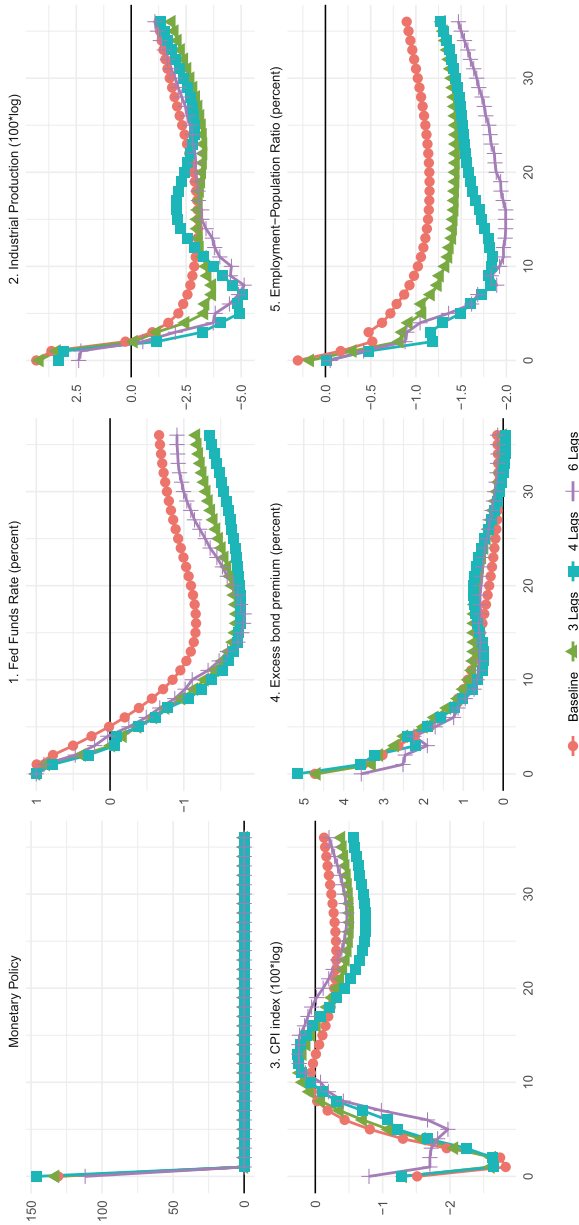
Figure B.4. Direct Inclusion versus External Instruments: Central Bank information Shock



Note: The line labeled “External instrument” shows the point estimate of the response of the headline variables to a one-standard-deviation shock computed using the high-frequency monetary policy series as an external instrument. The line labeled “Direct inclusion in VAR” shows our baseline IRFs, rescaled to match the same initial impulse as for the external instrument. The five factors are included in the estimation but, for clarity, their responses are omitted.

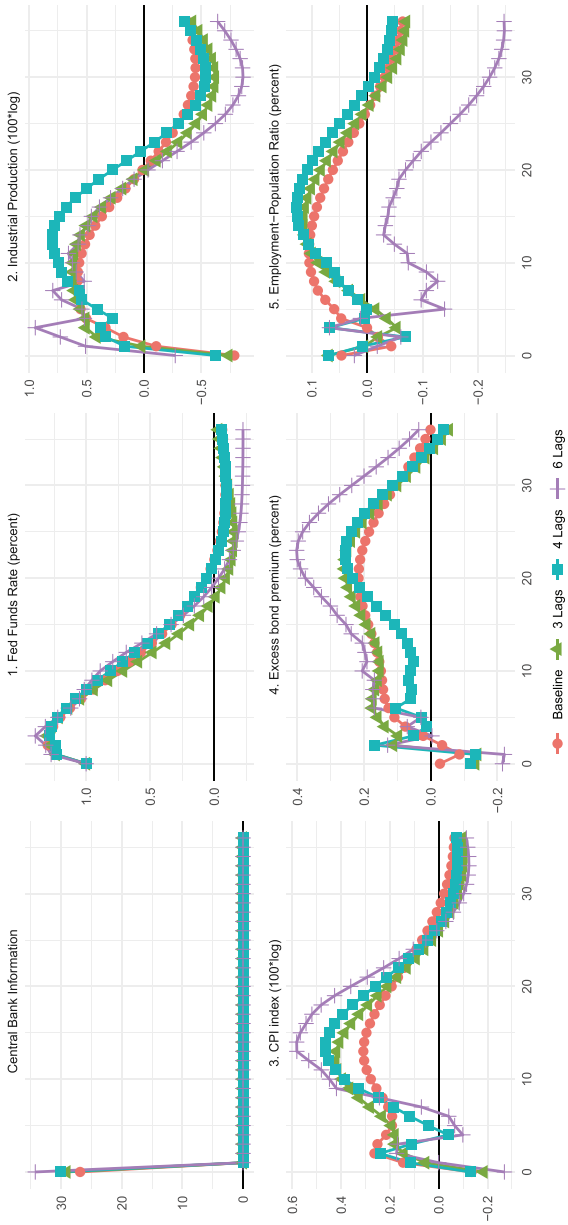
B.3 *Alternative Specifications*

Figure B.5. Impulse Responses: Monetary Policy Shock, Different Lag Structures



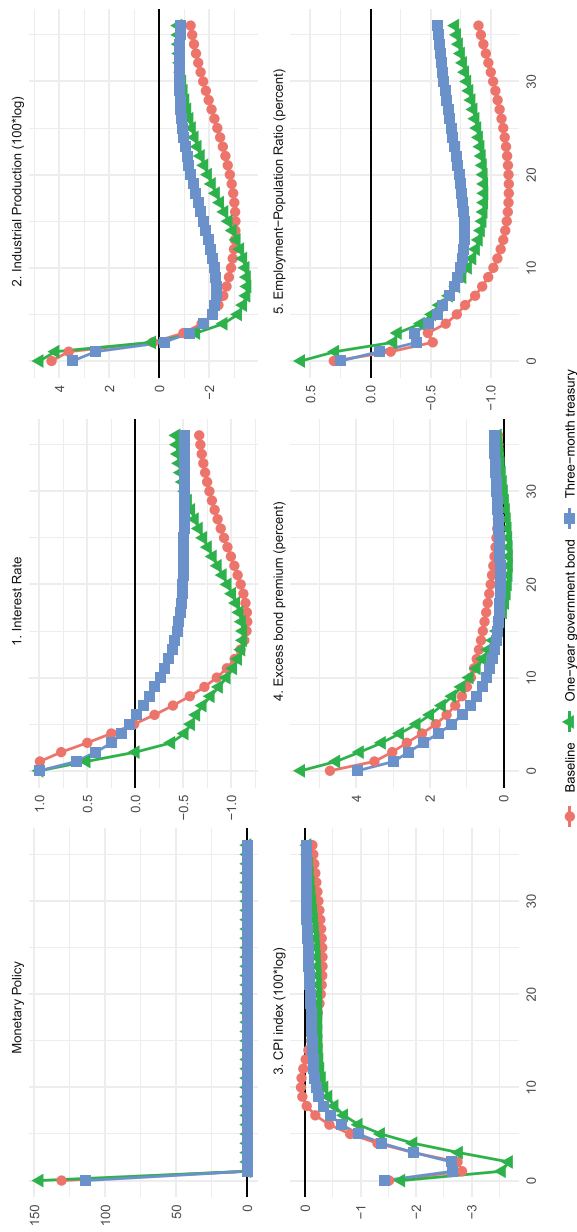
Note: The figure shows the point estimates of the response of a factor-augmented VAR to a monetary policy shock, scaled to a 1 percentage point increase in the interest rate. Lines differ in their lag length, which in the baseline is two periods, chosen by AIC. All specifications include five factors, responses of which are not shown here.

Figure B.6. Impulse Responses: Central Bank Information Shock, Different Lag Structures



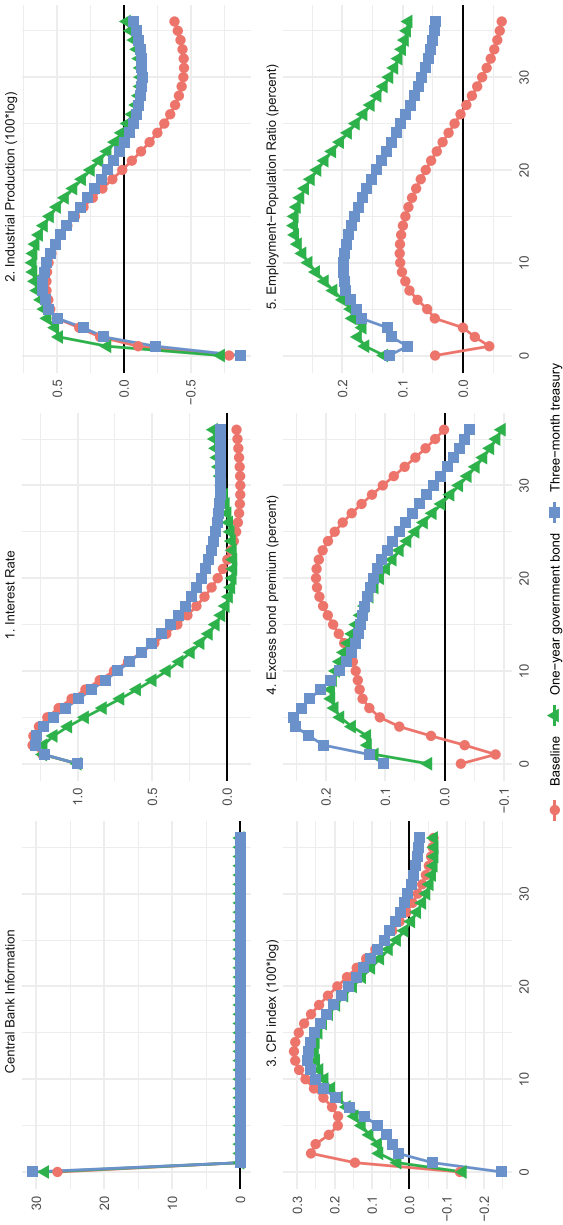
Note: The figure shows the point estimates of the response of a factor-augmented VAR to a central bank information shock, scaled to a 1 percentage point increase in the interest rate. Lines differ in their lag length, which in the baseline is two periods, chosen by AIC. All specifications include five factors, responses of which are not shown here.

Figure B.7. Impulse Responses: Monetary Policy Shock, Different Interest Rate Series



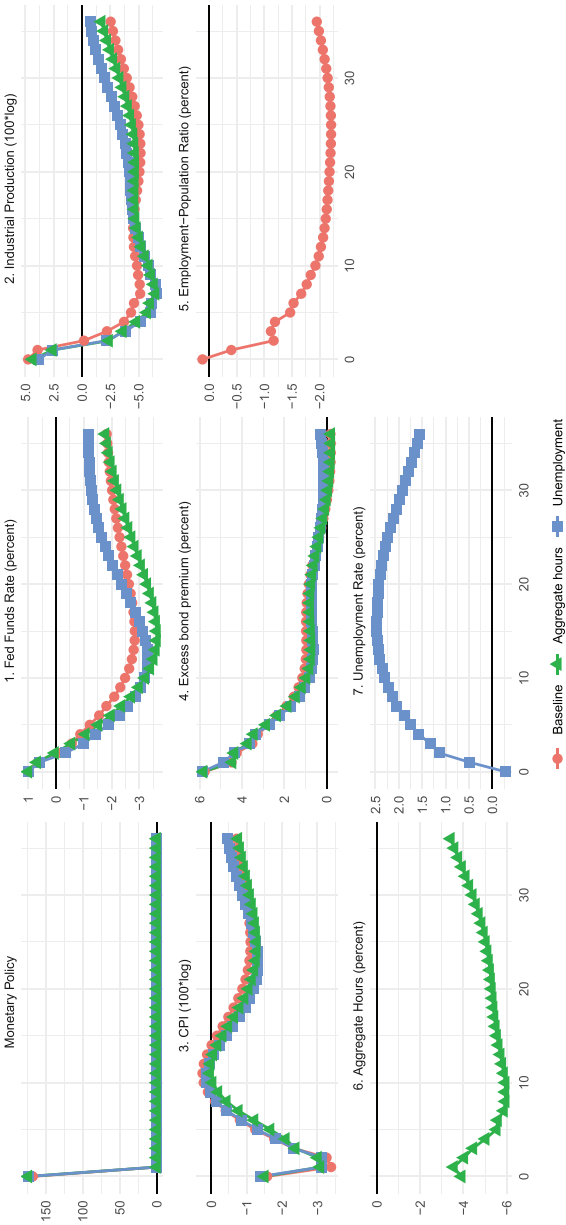
Note: The figure shows the point estimates of the response of a factor-augmented VAR to a monetary policy shock, scaled to a 1 percentage point increase in the interest rate. Lines differ in their measure of the interest rate, which in the baseline is the federal funds rate. All specifications include five factors, responses of which are not shown here.

Figure B.8. Impulse Responses: Central Bank Information Shock, Different Interest Rate Series



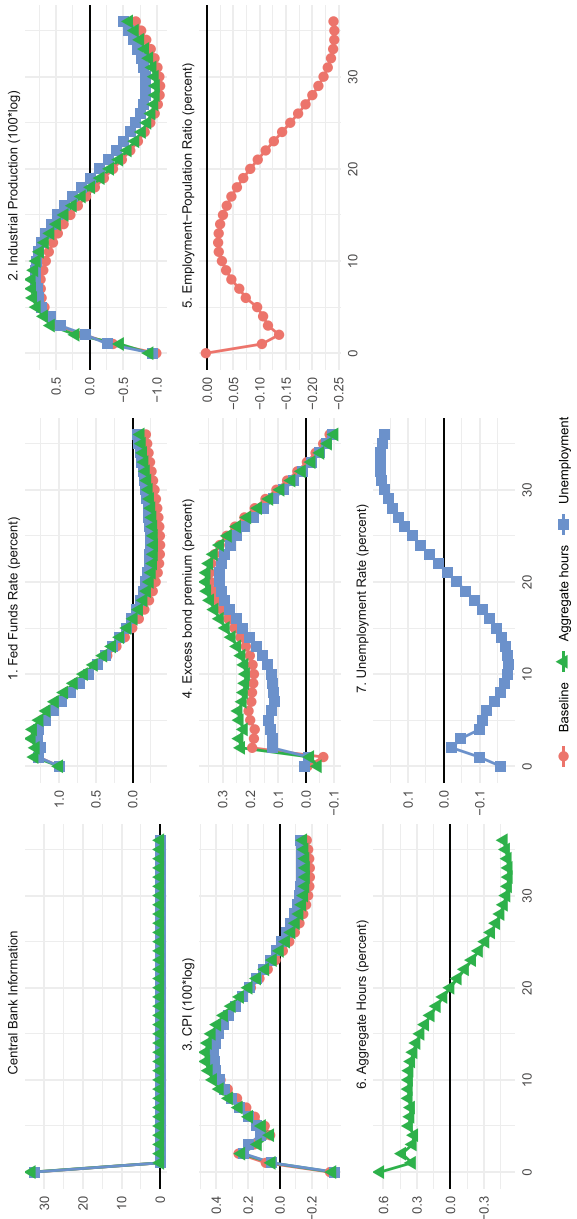
Note: The figure shows the point estimates of the response of a factor-augmented VAR to a central bank information shock, scaled to a 1 percentage point increase in the interest rate. Lines differ in their measure of the interest rate, which in the baseline is the federal funds rate. All specifications include five factors, responses of which are not shown here.

Figure B.9. Impulse Responses: Monetary Policy Shock, Different Labor Series



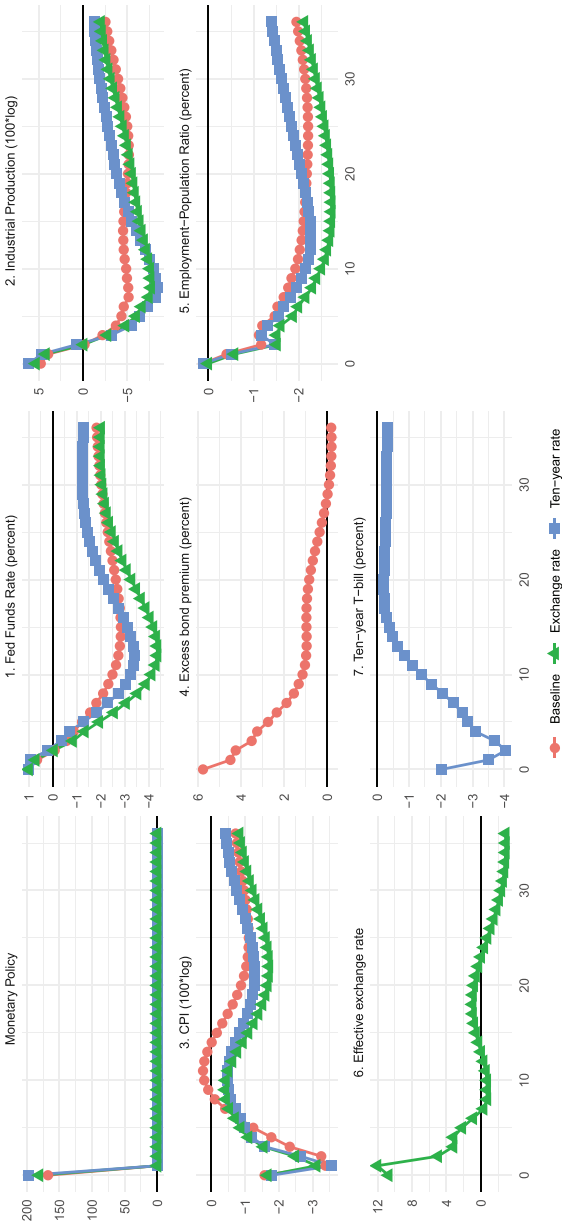
Note: The figure shows the point estimates of the response of a factor-augmented VAR to a monetary policy shock, scaled to a 1 percentage point increase in the interest rate. Lines differ in their labor market variable, which in the baseline is the employment-population ratio. All specifications include five factors, responses of which are not shown here.

Figure B.10. Impulse Responses: Central Bank Information Shock, Different Labor Series



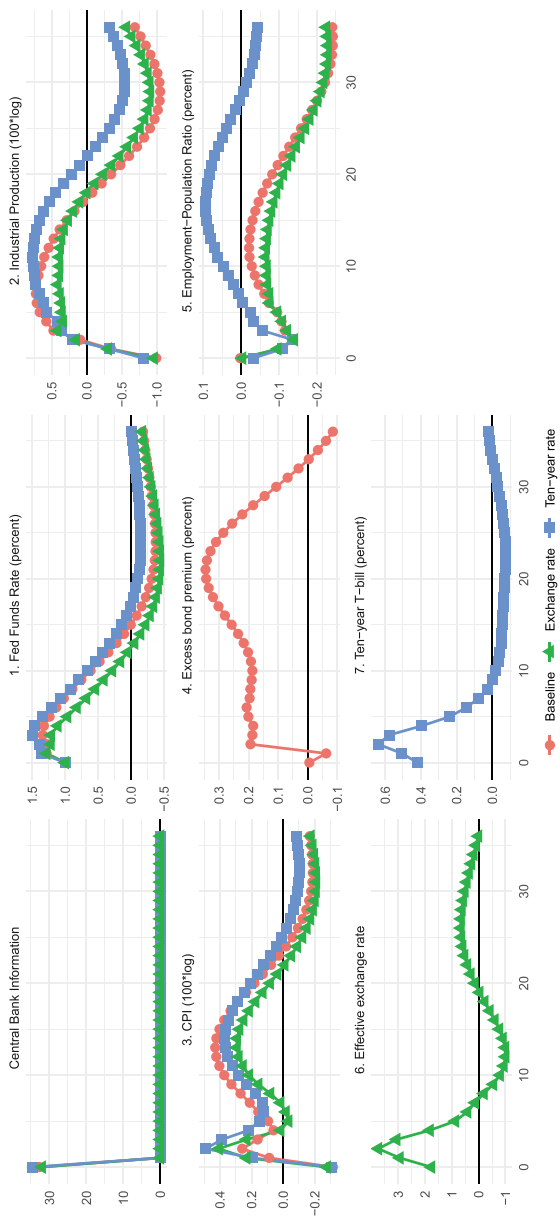
Note: The figure shows the point estimates of the response of a factor-augmented VAR to a monetary policy shock, scaled to a 1 percentage point increase in the interest rate. Lines differ in their labor market variable, which in the baseline is the employment-population ratio. All specifications include five factors, responses of which are not shown here.

Figure B.11. Impulse Responses: Monetary Policy Shock, Different Financial Series



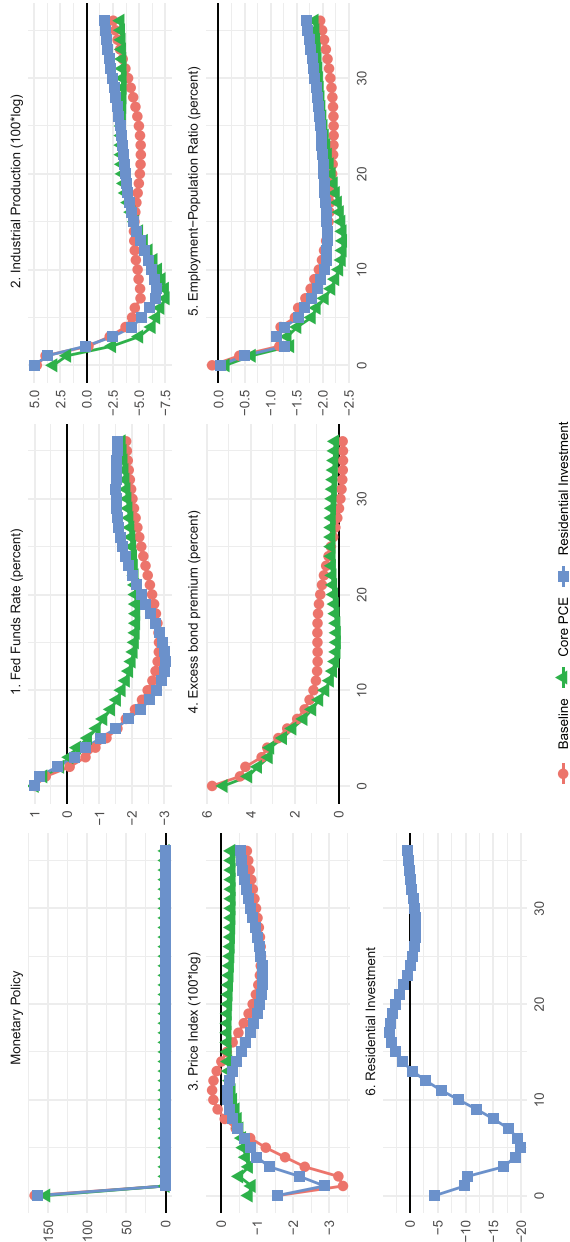
Note: The figure shows the point estimates of the response of a factor-augmented VAR to a monetary policy shock, scaled to a 1 percentage point increase in the interest rate. Lines differ in their financial variable, which in the baseline is the excess bond premium. All specifications include five factors, responses of which are not shown here.

Figure B.12. Impulse Responses: Central Bank Information Shock, Different Financial Series



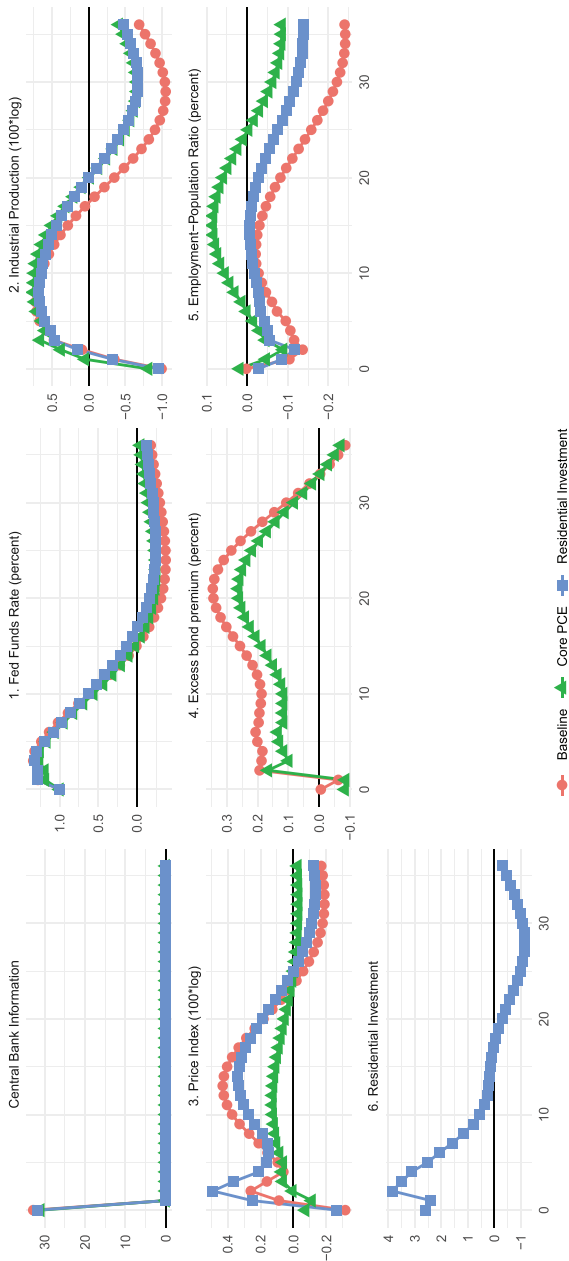
Note: The figure shows the point estimates of the response of a factor-augmented VAR to a monetary policy shock, scaled to a 1 percentage point increase in the interest rate. Lines differ in their financial variable, which in the baseline is the excess bond premium. All specifications include five factors, responses of which are not shown here.

Figure B.13. Impulse Responses: Monetary Policy Shock, Different Other Variables



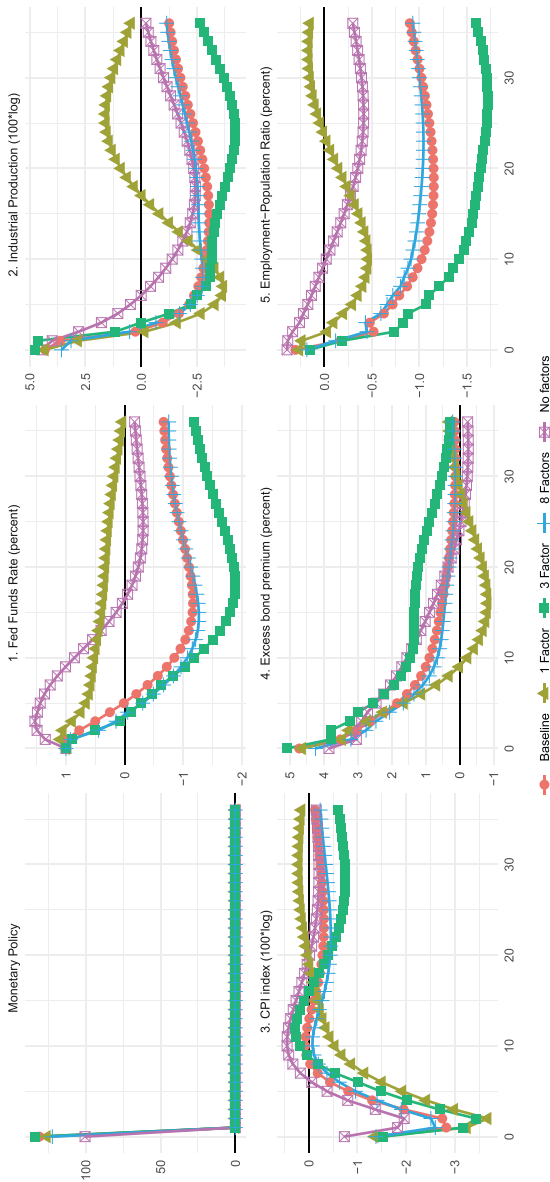
Note: The figure shows the point estimates of the response of a factor-augmented VAR to a monetary policy shock, scaled to a 1 percentage point increase in the interest rate. Lines differ in the variables that they include. The “Core PCE” specification replaces the CPI with core PCE and is shown in graph 3. The “Residential Investment” specification replaces the employment-population ratio with log residential investment. All specifications include five factors, responses of which are not shown here.

Figure B.14. Impulse Responses: Central Bank Information Shock, Different Other Variables



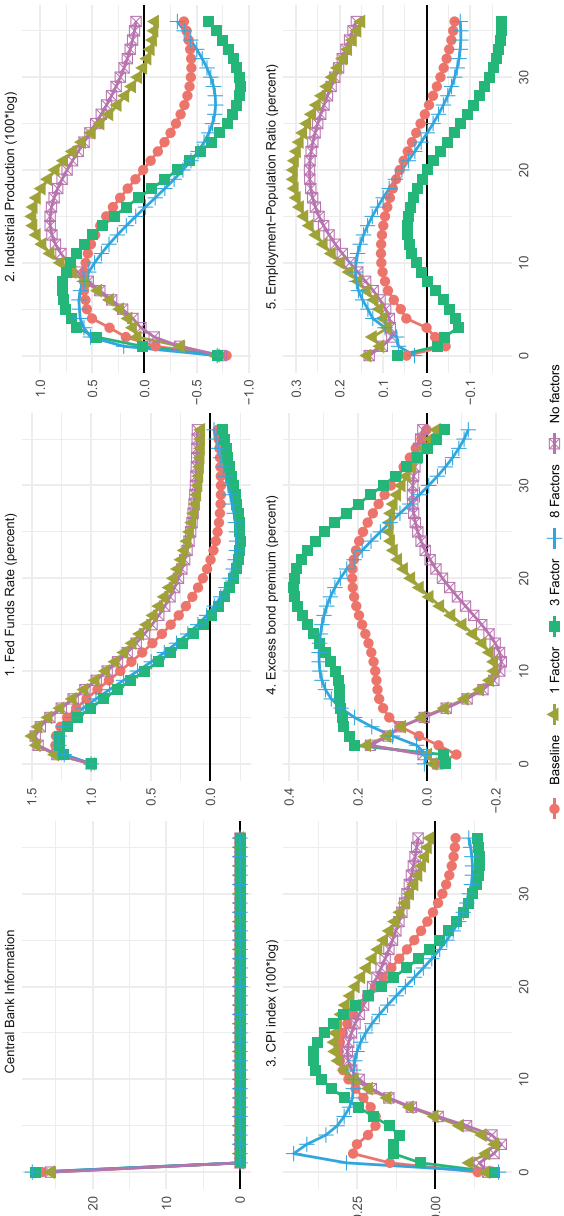
Note: The figure shows the point estimates of the response of a factor-augmented VAR to a monetary policy shock, scaled to a 1 percentage point increase in the interest rate. Lines differ in the variables that they include. The “Core PCE” specification replaces the CPI with core PCE and is shown in graph 3. The “Residential Investment” specification replaces the employment-population ratio with log residential investment. All specifications include five factors, responses of which are not shown here.

Figure B.15. Impulse Responses: Monetary Policy Shock, Different Number of Factors



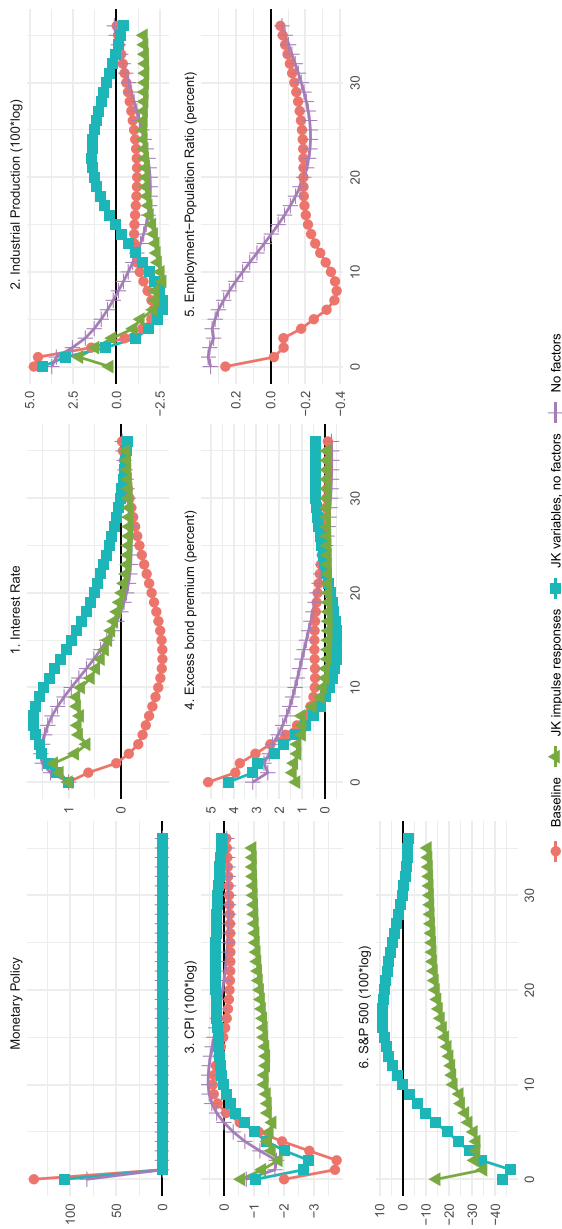
Note: The figure shows the point estimates of the response of a factor-augmented VAR to a monetary policy shock, scaled to a 1 percentage point increase in the interest rate. Lines differ in the number of factors, which in the baseline is five. Responses of factors are not shown here.

Figure B.16. Impulse Responses: Central Bank Information Shock, Different Number of Factors



Note: The figure shows the point estimates of the response of a factor-augmented VAR to a central bank information shock, scaled to a 1 percentage point increase in the interest rate. Lines differ in the number of factors, which in the baseline is five. Responses of factors are not shown here.

Figure B.17. Impulse Responses: Monetary Policy Shock, February 1992–December 2016



Note: The figure shows the point estimates of the response of a factor-augmented VAR to a monetary policy shock, scaled to a 1 percentage point increase in the interest rate. The “JK impulse responses” are the scaled impulse responses in Figure C3 of the Online Appendix of Jarocinski and Karadi (2020) (which is monthly and so we use instead of their quarterly headline results). The “Baseline” line corresponds to our baseline specification, reestimated on the matching sample: February 1992–December 2016. This specification retains factors, the responses of which are not shown here. The “No factors” line repeats our baseline estimation on this sample, just excluding factors. And “JK variables, no factors” matches the Jarocinski and Karadi specification, excluding factors and replacing the federal funds rate and employment-population ratio with the three-month Treasury-bill rate and S&P 500 index.

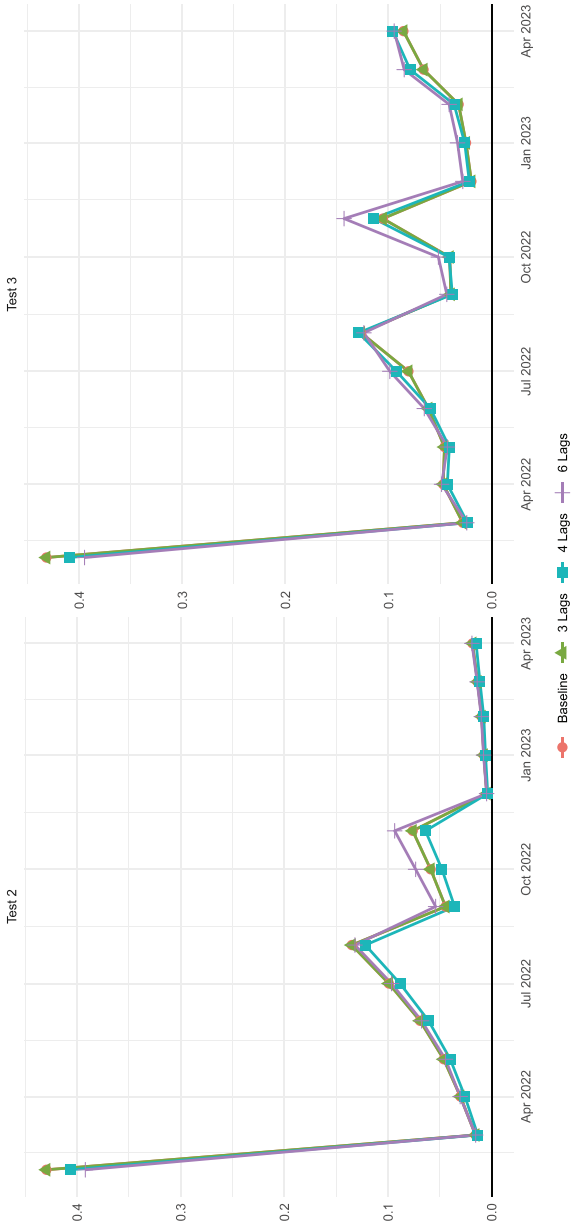
B.4 *Validation*

**Table B.1. Model Validation: Regressing
Actual on Estimated Shocks**

	Monetary Policy (1)	Central Bank Information (2)
Estimated Shock	1.011*** (0.187)	1.005*** (0.122)
Constant	0.005 (0.051)	−0.025 (0.049)
Observations	356	356
R ²	0.076	0.161
Adjusted R ²	0.074	0.158
Residual Std. Error (df = 354)	0.966	0.921
Note: Regression of actual shocks on the shocks estimated via the filter, monthly data 1992–2019. Spherical standard errors shown in parentheses. This is a test of our method and, if it works, should return coefficients of one and zero on the estimated shock and the constant, respectively. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.		

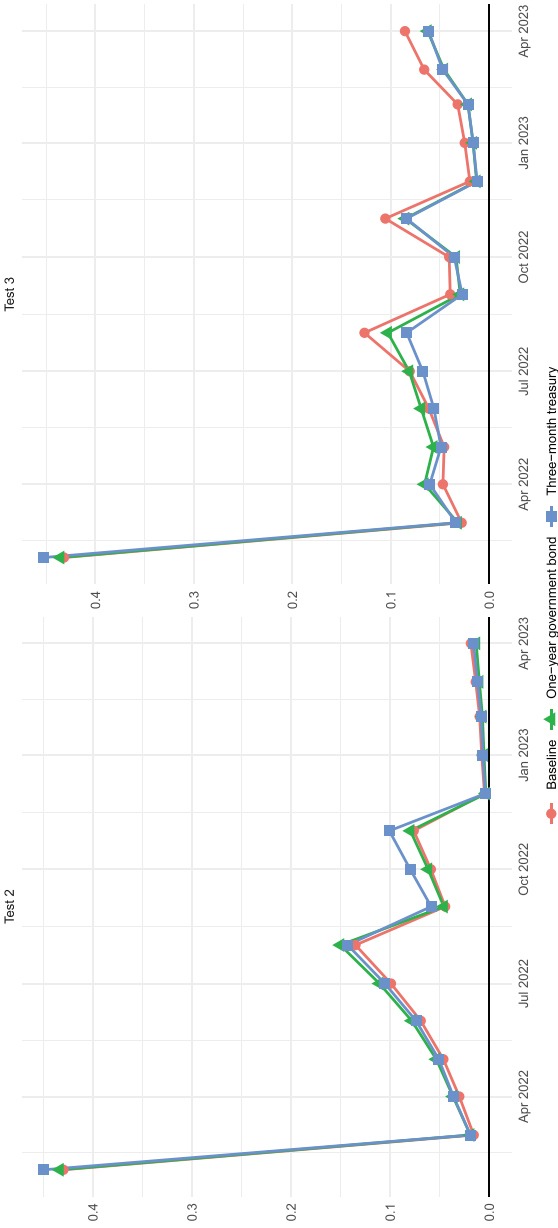
B.5 Hypothesis Tests under Alternative Specifications

Figure B.18. Hypothesis Tests: Monetary Policy Shock, Different Lag Structures



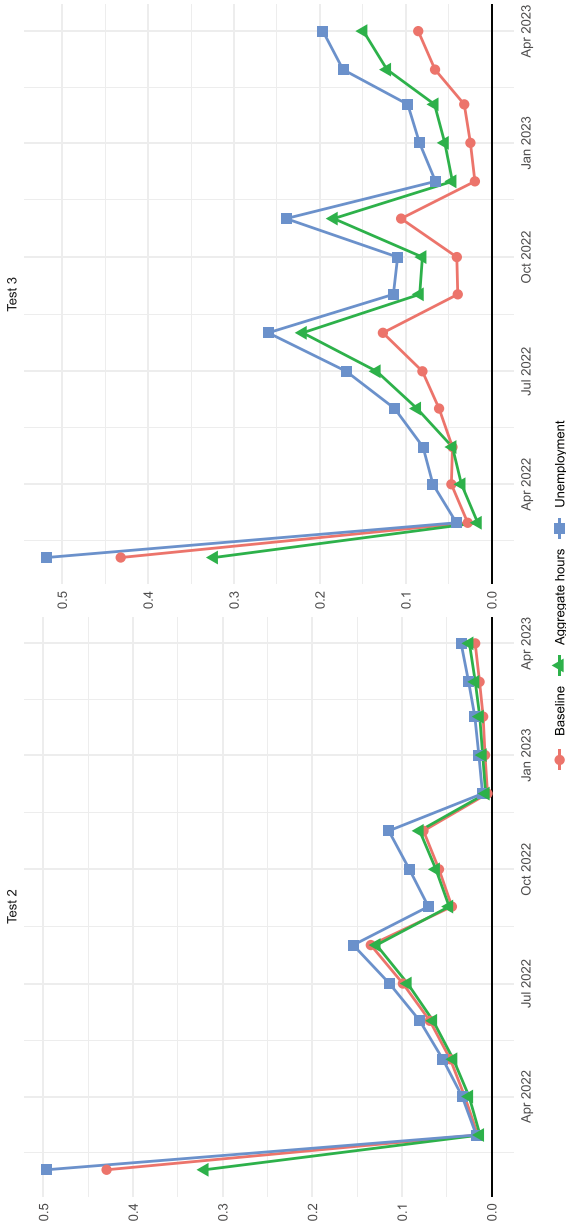
Note: The figure shows the p-values of the rolling hypothesis tests that the impact of the monetary policy shock is unchanged in an expanding window starting in February 2022. Tests 2 and 3 test, respectively, whether the transmission of monetary policy has changed at some point and on average in the test window. Lines differ in their lag length, which in the baseline is two periods, chosen by AIC. All specifications also include five factors.

Figure B.19. Hypothesis Tests: Monetary Policy Shock, Different Interest Rate Series



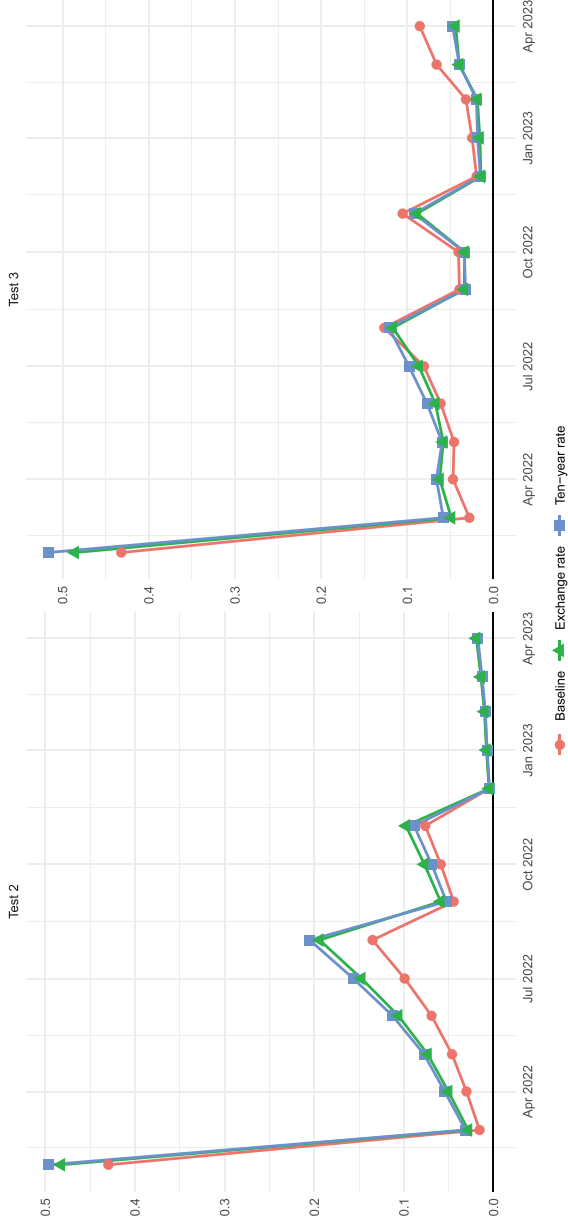
Note: The figure shows the p-values of the rolling hypothesis tests that the impact of the monetary policy shock is unchanged in an expanding window starting in February 2022. Tests 2 and 3 test, respectively, whether the transmission of monetary policy has changed at some point and on average in the test window. Lines differ in their measure of the interest rate, which in the baseline is the federal funds rate. All specifications also include five factors.

Figure B.20. Hypothesis Tests: Monetary Policy Shock, Different Labor Series



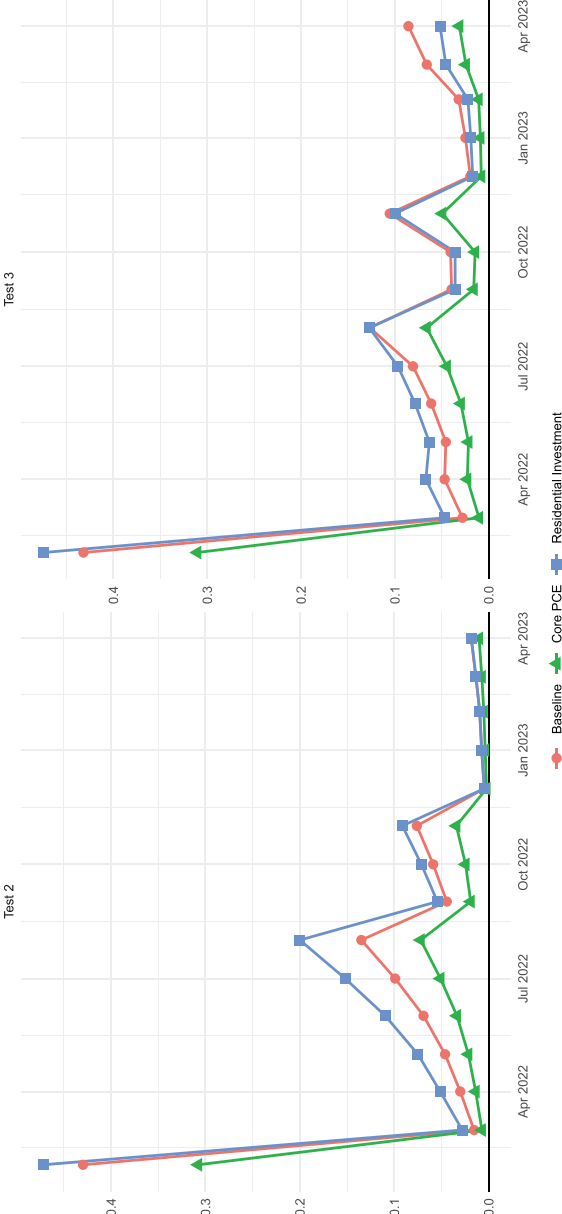
Note: The figure shows the p-values of the rolling hypothesis tests that the impact of the monetary policy shock is unchanged in an expanding window starting in February 2022. Tests 2 and 3 test, respectively, whether the transmission of monetary policy has changed at some point and on average in the test window. Lines differ in their labor market variable, which in the baseline is the employment-population ratio. All specifications also include five factors.

Figure B.21. Hypothesis Tests: Monetary Policy Shock, Different Financial Series



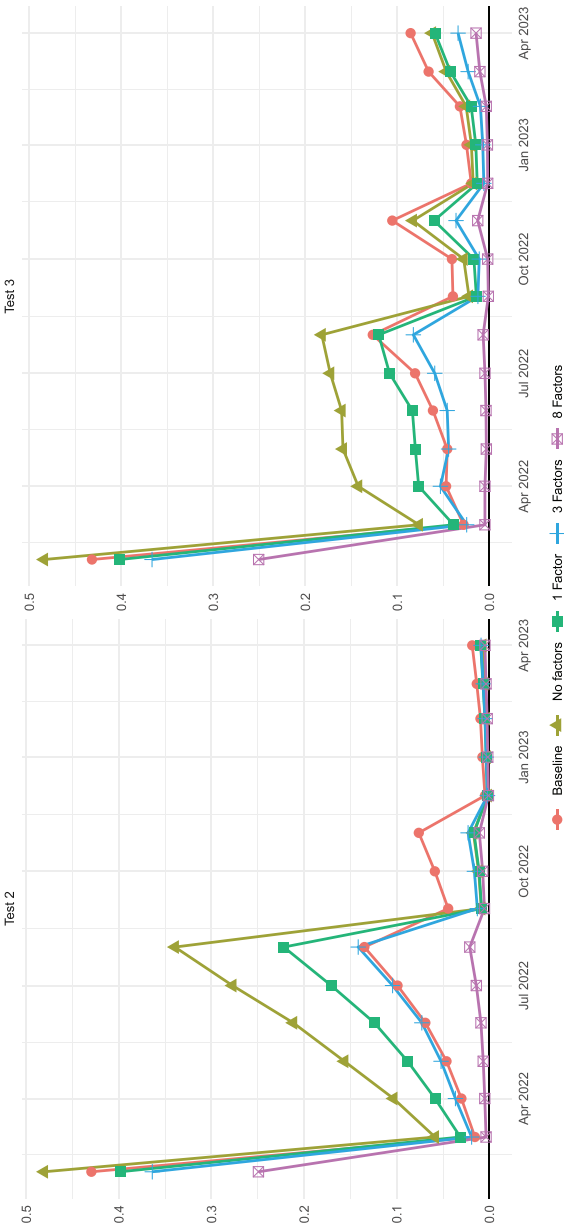
Note: The figure shows the p-values of the rolling hypothesis tests that the impact of the monetary policy shock is unchanged in an expanding window starting in February 2022. Tests 2 and 3 test, respectively, whether the transmission of monetary policy has changed at some point and on average in the test window. Lines differ in their financial variable, which in the baseline is the excess bond premium. All specifications also include five factors.

Figure B.22. Hypothesis Tests: Monetary Policy Shock, Different Other Variables



Note: The figure shows the p-values of the rolling hypothesis tests that the impact of the monetary policy shock is unchanged in an expanding window starting in February 2022. Tests 2 and 3 test, respectively, whether the transmission of monetary policy has changed at some point and on average in the test window. The “Core PCE” specification replaces the CPI with core PCE. The “Residential Investment” specification replaces the employment-population ratio with log residential investment. All specifications also include five factors.

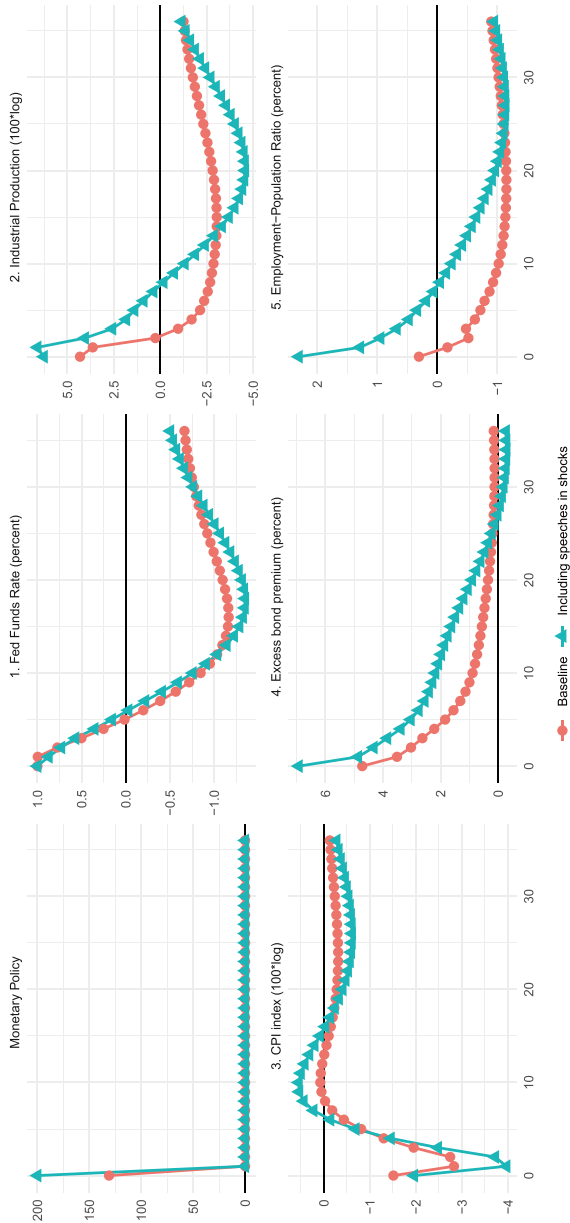
Figure B.23. Hypothesis Tests: Monetary Policy Shock, Different Number of Factors



Note: The figure shows the p-values of the rolling hypothesis tests that the impact of the monetary policy shock is unchanged in an expanding window starting in February 2022. Tests 2 and 3 test, respectively, whether the transmission of monetary policy has changed at some point and on average in the test window. Lines differ in the number of factors, which in the baseline is five.

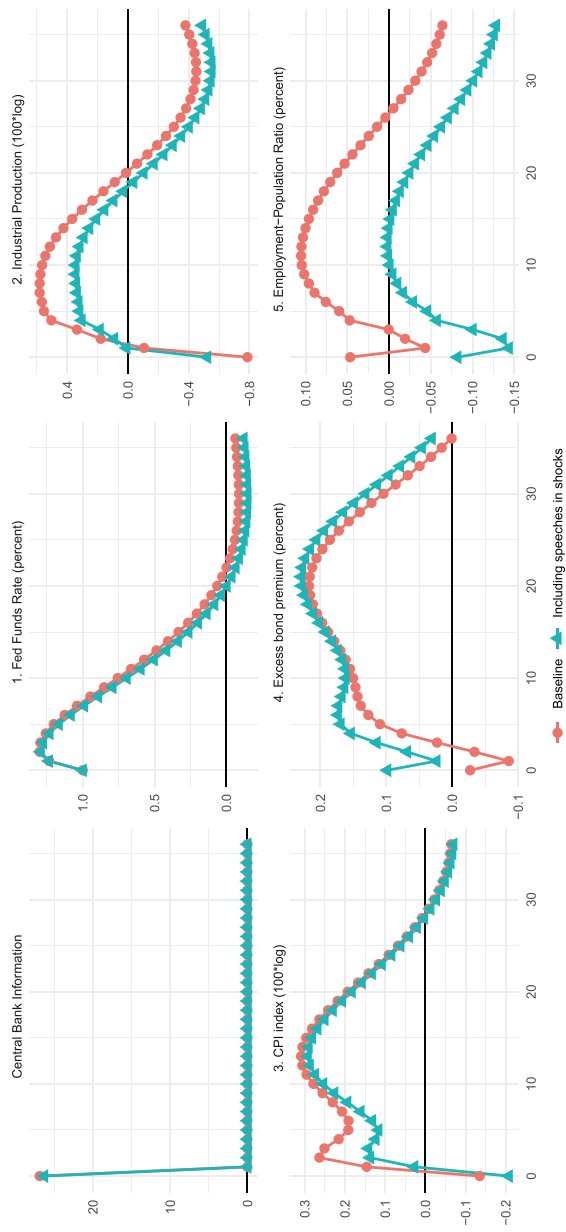
B.6 *Including Speeches*

Figure B.24. Impulse Responses: Monetary Policy Shock, Including Speeches



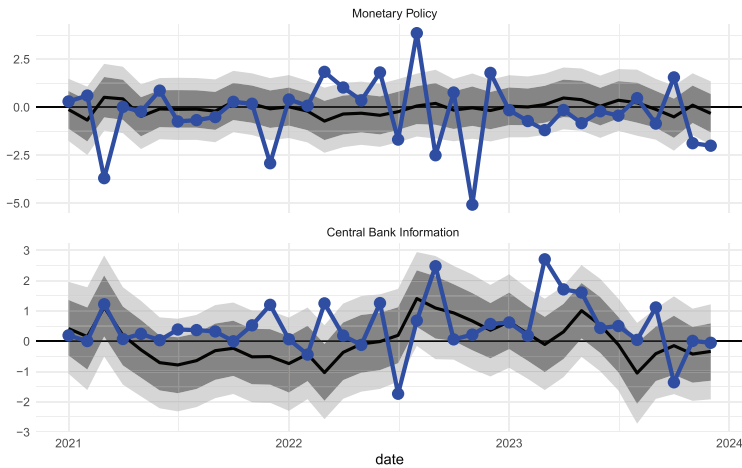
Note: The figure shows the point estimates of the response of a factor-augmented VAR to a monetary policy shock, scaled to a 1 percentage point increase in the interest rate. Green lines use shocks including policymakers' speeches, while the baseline only includes shocks based on FOMC meetings. All specifications include five factors, responses of which are not shown here.

Figure B.25. Impulse Responses: Central Bank Information Shock, Including Speeches



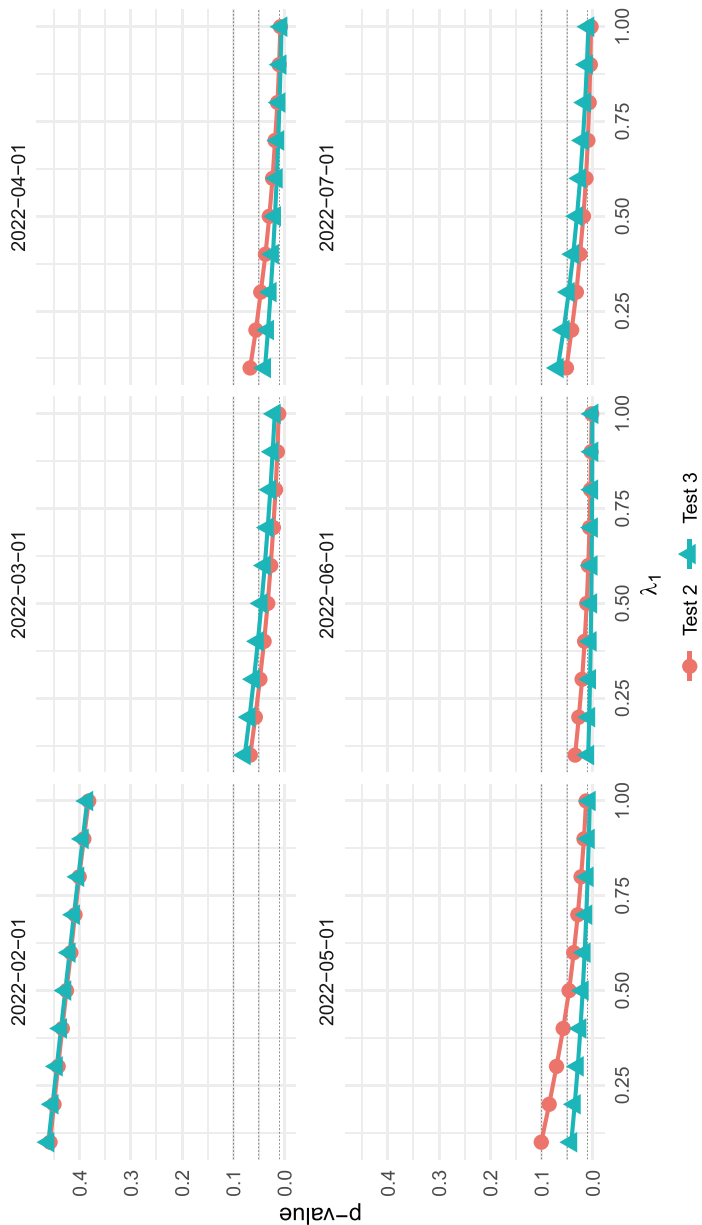
Note: The figure shows the point estimates of the response of a factor-augmented VAR to a central bank information shock, scaled to a 1 percentage point increase in the interest rate. Green lines use shocks including policymakers' speeches, while the baseline only includes shocks based on FOMC meetings. All specifications include five factors, responses of which are not shown here.

**Figure B.26. Inferred and Realized Shocks: 2021–23,
Including Speeches in Shocks**



Note: Solid lines are point estimates of the inferred shocks under the null hypothesis that the data-generating process remains unchanged. The shaded regions are the 68 and 90 percent confidence intervals from a bootstrap with $K = 1000$ replications. The blue line with large dots is the actual shock, computed from high-frequency data. This version uses shocks including policymakers' speeches.

Figure B.27. P-Values for Hypothesis Tests as Strength of Monetary Policy Shock Varies



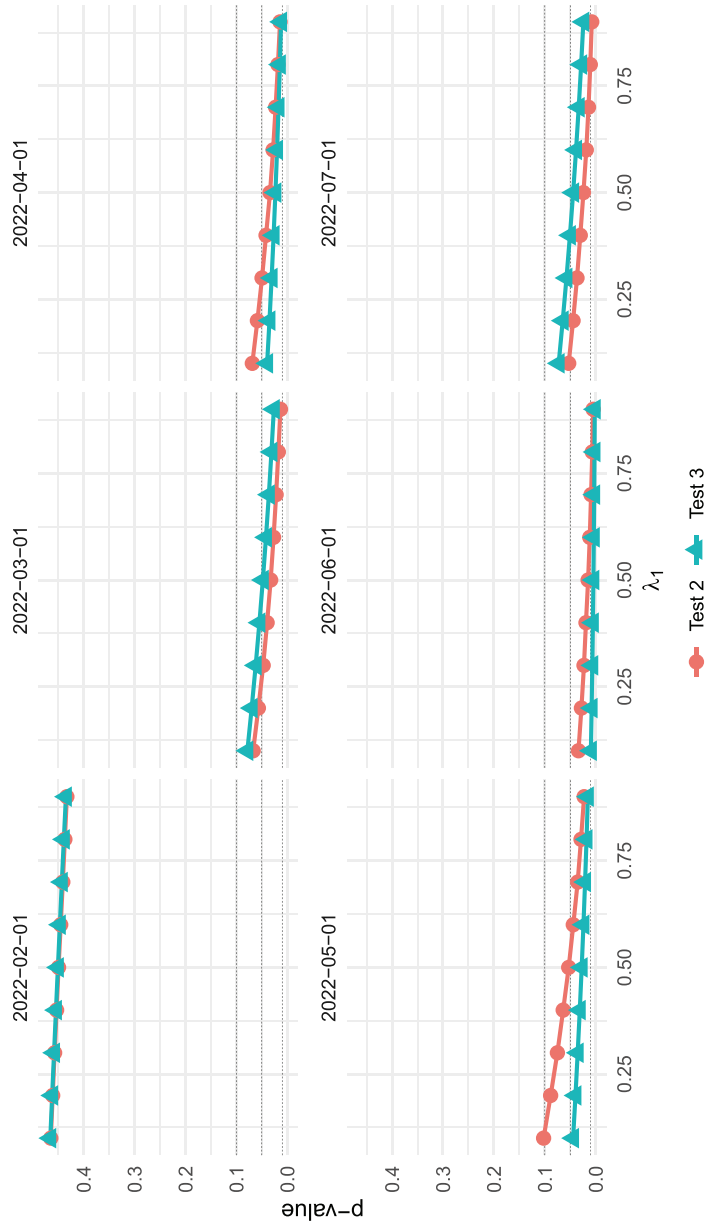
Note: The figure shows p-values for hypothesis tests described in Section 5.3, under a series of nulls, indexed by λ_1 . Horizontal lines show 1, 5, and 10 percent confidence levels. The interpretation of λ_1 is that it represents a null where monetary policy is only λ_1 as effective as pre-COVID. Each panel computes the corresponding hypothesis tests on a window starting in February 2022 and ending in the titular month. The values for both lines when $\lambda_1 = 1$ thus match the corresponding columns in Table 1. This version uses shocks including ‘policymakers’ speeches.

Table B.3. P-Values for Joint Hypothesis Tests, Including Speeches in Shocks

Shock	$\mathcal{T}$ Start	$\mathcal{T}$ End	$ \mathcal{T} $	Test 1	Test 2	Test 3
Monetary Policy	2022-02-01	2022-02-01	1	0.385	0.382	0.385
	2022-02-01	2022-03-01	2	0.385	0.011**	0.019**
	2022-02-01	2022-04-01	3	0.385	0.008***	0.006***
	2022-02-01	2022-05-01	4	0.385	0.013**	0.006***
	2022-02-01	2022-06-01	5	0.385	0.002***	0.001***
	2022-02-01	2022-07-01	6	0.933	0.003***	0.008***
Central Bank Information	2021-05-01	2021-05-01	1	0.281	0.281	0.281
	2021-05-01	2021-06-01	2	0.281	0.311	0.162
	2021-05-01	2021-07-01	3	0.281	0.192	0.061*
	2021-05-01	2021-08-01	4	0.281	0.162	0.029**
	2021-05-01	2021-09-01	5	0.281	0.182	0.022**
	2021-05-01	2021-10-01	6	0.399	0.234	0.026**
	2021-05-01	2021-11-01	7	0.399	0.185	0.013**
	2021-05-01	2021-12-01	8	0.399	0.070*	0.003***
	2021-05-01	2022-01-01	9	0.399	0.070*	0.002***
	2021-05-01	2022-02-01	10	0.507	0.091*	0.003***
	2021-05-01	2022-03-01	11	0.507	0.012**	0.000***
	2021-05-01	2022-04-01	12	0.507	0.016**	0.000***

Note: The table shows p-values for the joint hypothesis that consecutive observations are drawn from the null. Each line considers three tests that the observed high-frequency shocks are drawn from a distribution with a higher mean than would be expected based on pre-COVID transmission of monetary policy, over a period of consecutive months. Test 1 assesses whether the impact of monetary policy is different at all points during the window of consecutive observations. Test 2 assesses whether it is different at some point. And Test 3 assesses whether it is different on average. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. This version uses shocks including policymakers' speeches.

Figure B.28. P-Values for Hypothesis Tests as Strength of Monetary Policy Shock Varies: Post-2021 Residual Covariance



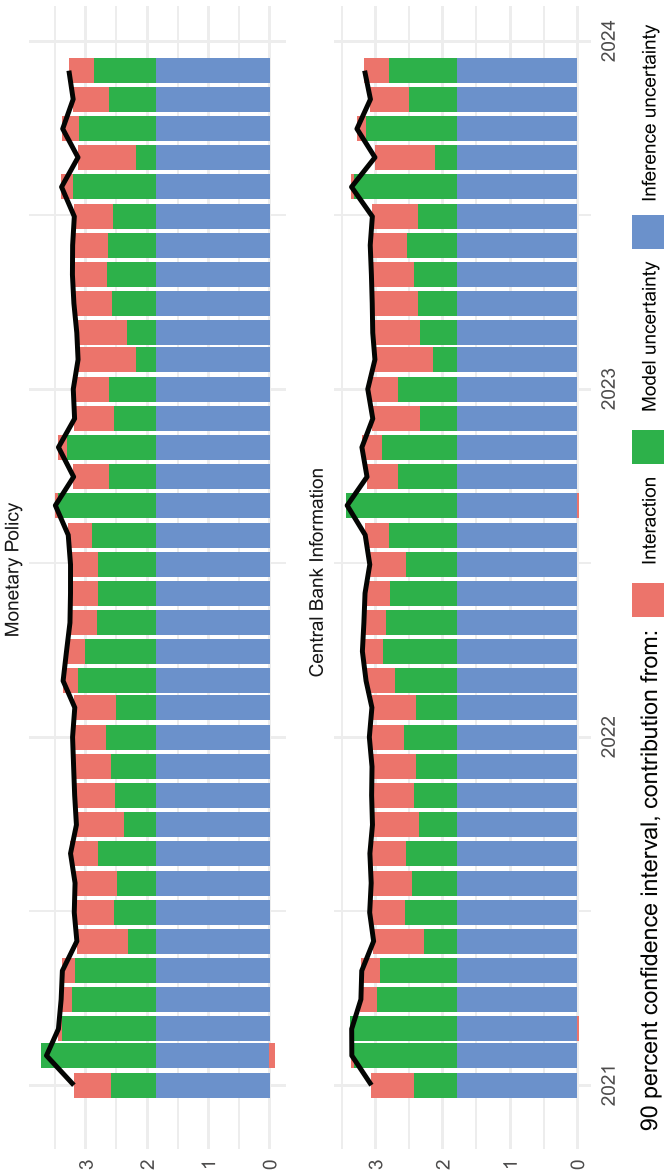
Note: The figure shows p-values for hypothesis tests described in Section 5.3, under a series of nulls, indexed by λ_1 . Horizontal lines show 1, 5, and 10 percent confidence levels. The interpretation of λ_1 is that it represents a null where monetary policy is only λ_1 as effective as pre-COVID. Each panel computes the corresponding hypothesis tests on a window starting in February 2022 and ending in the titular month. The values for both lines when $\lambda_1 = 1$ thus match the corresponding columns in Table 1. This version uses the post-2021 residual covariance for $\hat{\Omega}_q$, and shocks including policymakers' speeches.

B.7 Informal Assessment

Table B.4. Historic Consecutive Periods of Unusual Shocks: Full Sample, 1990–2023

Shock	Month	Consecutive Periods Above	Consecutive Periods Below
Monetary Policy	2007-03-01	0	6
Monetary Policy	2008-04-01	7	0
Monetary Policy	2020-06-01	0	9
Monetary Policy	2022-07-01	7	0
CB Information	2010-09-01	9	0
CB Information	2014-12-01	9	0
CB Information	2019-04-01	10	0
CB Information	2022-04-01	13	0
Note: The table shows terminal months of the longest streaks of high-frequency observed shocks above or below the point estimate. Episodes included in the table are the four longest consecutive sequences above or below the point estimate for each shock. For example, all other observations have six or fewer consecutive observations above or below the inferred monetary policy shock.			

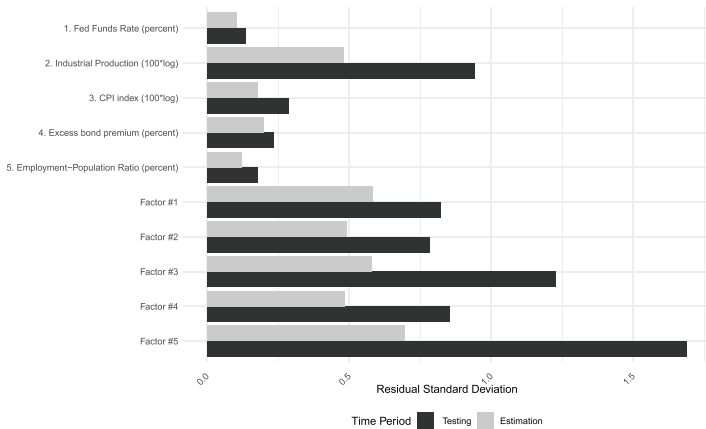
Figure B.29. Uncertainty Decomposition: 2021–23



Note: The figure shows decomposition of 90 percent confidence intervals into their contributions from model uncertainty, inference uncertainty, and their interaction.

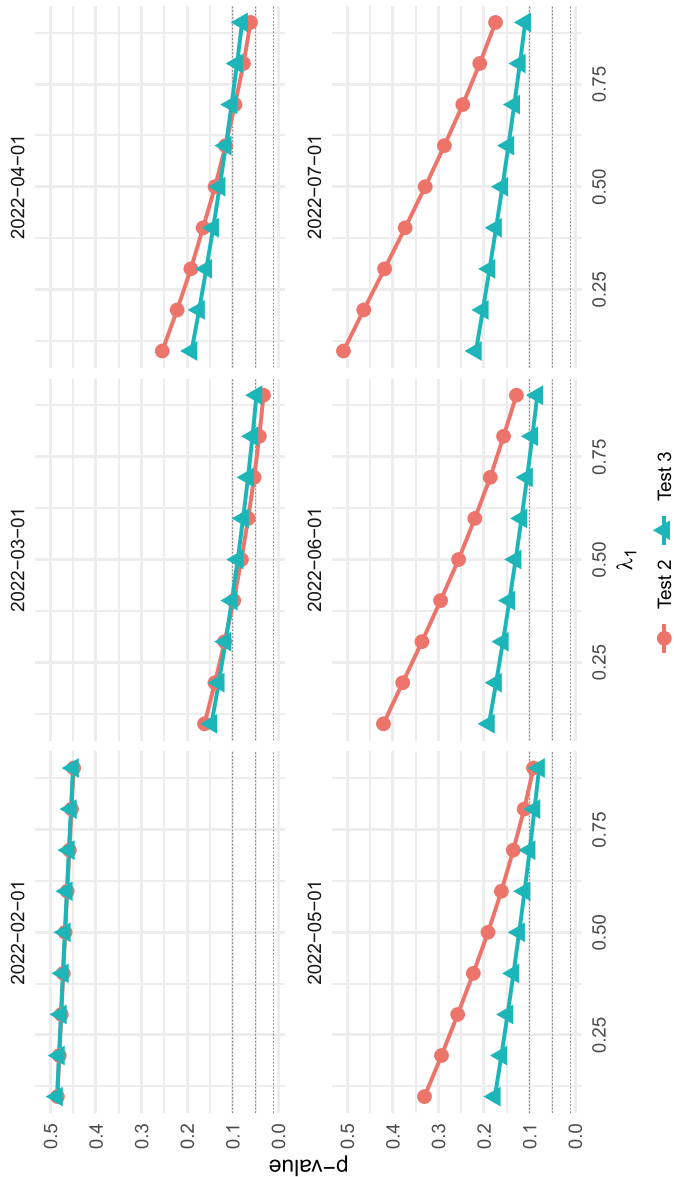
B.9 Results When Allowing for Changing Variances

Figure B.30. Reduced-Form Residual Standard Deviations



Note: Bars show the standard deviation of the reduced-form shocks on the estimation and testing periods. In both cases, the autoregressive parameters, B_j , are assumed to be those estimated on the pre-COVID data.

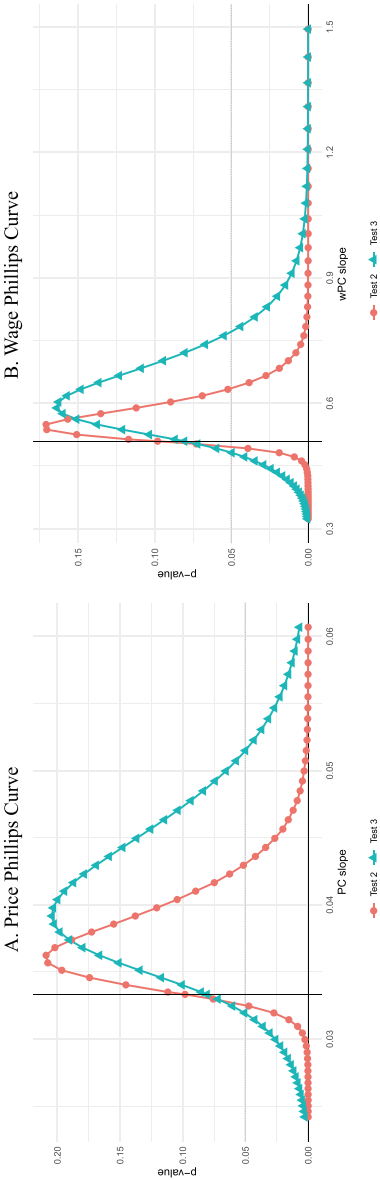
Figure B.31. P-Values for Hypothesis Tests as Strength of Monetary Policy Shock Varies: Post-2021 Residual Covariance



Note: The figure shows p-values for hypothesis tests described in Section 5.3, under a series of nulls, indexed by λ_1 . This version uses the post-2021 residual covariance for $\hat{\Omega}_y$. Horizontal lines show 1, 5, and 10 percent confidence levels. The interpretation of λ_1 is that it represents a null where monetary policy is only λ_1 as effective as pre-COVID. Each panel computes the corresponding hypothesis tests on a window starting in February 2022 and ending in the titular month. The values for both lines when $\lambda_1 = 1$ thus match the corresponding columns in Table 1.

B.10 *Results from the Structural Model*

Figure B.32. P-Values for Alternate Structural Parameters



Note: The figure shows p-values for tests of alternate nulls using the data-generating process from the Smets and Wouters (2007) model. For each point in panel A we recompute the p-value of the tests on the February–July 2022 data using the values of $D_m(\theta_1)$, $B_z(\theta_1)$, and $D(\theta_1)$ implied by the model solution when the price Calvo reset probability is varied. We then plot this (on the y-axis) against the Phillips curve (on the x-axis). Panel B does the same but for the wage Calvo reset probability. The horizontal dashed line shows the 5 percent critical value and the vertical line the baseline value, matching our empirical exercise.

Appendix C. Deriving the Hypothesis Tests

Here we derive the hypothesis tests we apply in Section 5.3.

C.1 Setup

Recall that η_t is the difference between the observed high-frequency shocks and their filter-inferred equivalent, given by $\eta = v_t - \bar{\delta}_t$, where $\bar{\delta}_t = \int_{\delta} \delta f_t(\delta) d\delta$ and $(\sigma_t^i)^2 = \int_{\delta} (\delta - \bar{\delta}_t)^2 f_t(\delta) d\delta$ are the mean and variance of the filter-implied distribution for the i th structural shock.

We define the parameter $\mu_t = \mathbb{E}_t \eta_t$ as the mean of η_t and want to test jointly whether some or all of these parameters are positive on a sample $t = 1, \dots, T$. Specifically, we want to test the following:

Test 1 $H_0 : \mu_t^i \leq 0$ for some $t \in \mathcal{T}$ vs. $H_1 : \mu_t^i > 0$ for all $t \in \mathcal{T}$

Test 2 $H_0 : \mu_t^i \leq 0$ for all $t \in \mathcal{T}$ vs. $H_1 : \mu_t^i > 0$ for some $t \in \mathcal{T}$

Test 3 $H_0 : \bar{\mu}^i \leq 0$ vs. $H_1 : \bar{\mu}^i > 0$, where $\bar{\mu}^i = \frac{1}{|\mathcal{T}|} \sum_{t \in \mathcal{T}} \mu_t^i$.

It is convenient to fix i and work with the normalized shock $x_t = \eta_t^i / \sigma_t^i$, where σ_t^i is the root mean square error of the filter-inferred shock. As is common, we assume that this is known. Normality of the filter-inferred estimator implies that

$$x_t \sim_{i,i,d} N(\mu_t, 1) \quad \forall t \in \mathcal{T}.$$

Given this, we can deal with Test 3 straightforwardly, since it becomes a standard univariate one-sided test of the sample mean. That is, we can construct the test statistic

$$\bar{x}_{\mathcal{T}} = \frac{1}{|\mathcal{T}|} \sum_{t \in \mathcal{T}} x_t.$$

Then

$$\mathbb{E} \bar{x}_{\mathcal{T}} = \bar{\mu}^i = \frac{1}{T} \sum_{t \in \mathcal{T}} \mu_t^i,$$

where $T = |\mathcal{T}|$. And so

$$\bar{x}_{\mathcal{T}} \sim N(\bar{\mu}^i, 1/\sqrt{T}).$$

Under the null for Test 3, the mean of this distribution is zero, so we can compute one-sided p-values for the data from

$$p_3(\bar{x}_T) = 1 - \Phi(\bar{x}_T/\sqrt{T}).$$

However, for Tests 1 and 2, things are less straightforward. We derive likelihood-ratio tests more formally in the following sections.

C.2 Rejection Regions

Tests 1 and 2 both consider the tests of the form $H_0 : \theta \in \Theta_0$ versus $H_1 : \theta \in \Theta_0^c$, where Θ_0 is a restricted subset of the parameter space Θ and Θ_0^c is the complement of Θ_0 .

The likelihood-ratio test statistic is then

$$\lambda(\mathbf{x}) = \frac{\sup_{\Theta_0} L(\theta|\mathbf{x})}{\sup_{\Theta} L(\theta|\mathbf{x})}, \quad (\text{C.1})$$

where $\mathbf{x} = (x_1, \dots, x_T)$ is the data and $L(\cdot)$ is the likelihood function.²⁷

In our Test 1, the parameter restriction is that $\mu_t^i \leq 0$ for some $t \in \mathcal{T}$. This is a fairly loose restriction, and to reject corresponds to the idea that one is certain that monetary policy is less effective than usual in all the periods in $\mathcal{T}$. We illustrate this for the simple case where $\mathcal{T} = \{1, 2\}$ in Figure C.1, panel A. In contrast, the parameter restriction for Test 2 is much tighter, requiring that $\mu_t^i \leq 0$ for all $t \in \mathcal{T}$. This corresponds to the idea that monetary policy was less effective than usual at least some of the time during $\mathcal{T}$. In the two-period case, this is shown by the smaller shaded region in Figure C.1, panel B.

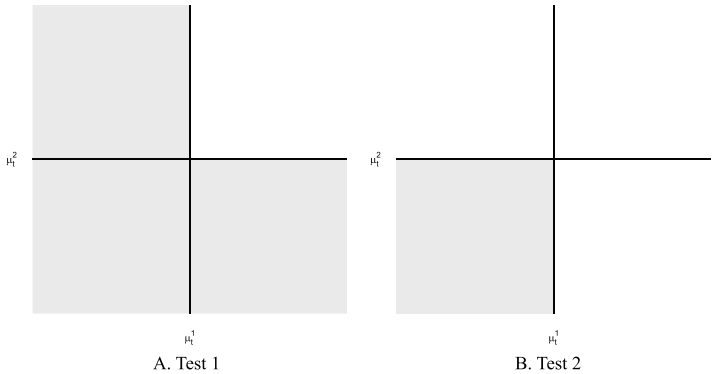
Formally, we define the parameter restriction sets for the two tests:

$$\begin{aligned} \Theta_0^1 &= \{\boldsymbol{\mu} \in \mathbb{R}^T \mid \mu_t \leq 0 \quad \forall t \in \mathcal{T}\} \\ \Theta_0^2 &= \{\boldsymbol{\mu} \in \mathbb{R}^T \mid \mu_t \leq 0 \text{ for some } t \in \mathcal{T}\}. \end{aligned}$$

The likelihood-ratio test rejects H_0 in favor of H_1 when $\lambda(\mathbf{x})$ falls below some critical value. For any given critical value, the set

²⁷See chapter 8 of Casella and Berger (2002) for a textbook treatment of this topic, which we follow closely here.

Figure C.1. Parameter Restrictions for Tests 1 and 2



Note: Shaded regions show the restricted parameter under the null, Θ_c , for Tests 1 and 2 for the case where $\mathcal{T} = \{1, 2\}$.

of possible observations of the data which would cause the test to reject the null is known as the rejection region. Because the data are drawn from independent normal distributions with unit variance, the likelihood ratio is simply a function of the minimum Euclidian distance from the observed data to the nearest corresponding point inside Θ_c .

For each test $j = 1, 2$, we can write down the general formula for a rejection regions with a given point $\mathbf{x}$ on the boundary as

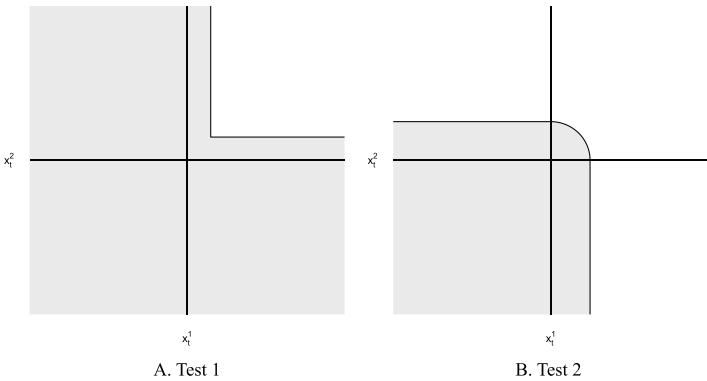
$$R_j(\mathbf{x}) = \{\mathbf{X}' \in \mathbb{R}^t \mid d_j(\mathbf{x}') \geq d_j(\mathbf{x})\},$$

where $d_j(\mathbf{x})$ is the Euclidian distance between $\mathbf{x}$ and the nearest point inside the constraint set:

$$d_j(\mathbf{x}) = \inf_{\boldsymbol{\mu} \in \Theta_0^j} \|\mathbf{x} - \boldsymbol{\mu}\|.$$

This produces rejection regions which look like the parameter restriction sets plus a buffer (see Figure C.2 for the two-period example once more). This buffer is larger for smaller critical values of the likelihood ratio. More intuitively, the critical value for the likelihood-ratio test sets the bar for how much the data have to disagree with the null sufficiently in order to reject it. Of course, likelihood-ratio

Figure C.2. Rejection Regions for Tests 1 and 2



Note: Unshaded regions show rejection regions for Tests 1 and 2 for the case where $\mathcal{T} = \{1, 2\}$.

critical values are not easy to interpret. In the next section, we derive the more familiar p-values for these tests.

C.3 Computing p-Values

The p-value is a function of the data, $p(\mathbf{x})$. This is usually described as the probability of rejecting the observed data under the null. However, with multivariate set nulls (as we have here) this definition is insufficient. Instead, a valid p-value is the maximum probability of rejection over the null set, akin to a worst-case scenario (again see Casella and Berger 2002 for details).

$$p_j(\mathbf{x}) = \sup_{\boldsymbol{\mu} \in \Theta_0^j} \mathbb{P}(\mathbf{x}' \in R_j(\mathbf{x}))$$

For each test, this means we can compute the p-value in two steps. First, we calculate $\boldsymbol{\mu}_j^* \in \Theta_0^j$, the parameter value which maximizes this probability over the null set. Second, we then integrate over draws of the data conditional on $\boldsymbol{\mu}^*$.

For Test 1, we can compute a closed-form solution for this test:

$$p_1(\mathbf{x}) = 1 - \Phi(\min_{t \in \mathcal{T}} x_t).$$

However, for Test 2, we can only complete the first step analytically, since $\boldsymbol{\mu}_j^* = \mathbf{0}$, the origin. To compute the p-values, we integrate numerically, drawing 100,000 points from the mean-zero unit-variance multivariate normal and computing the fraction of observations in $R_2(\mathbf{x})$.

Appendix D. Structural Models

In this section we describe two different structural models and discuss how they can be mapped into our empirical exercises. The first is intentionally simple, and we use it only to illustrate explicitly the point that weaker transmission of monetary policy shocks can have different impacts on different variables depending on how that weaker transmission is implemented (i.e., which structural parameter is changed to deliver it). In the second, we outline a more complicated structural economic model which we actually apply to the data.

D.1 A Simple New Keynesian Model

Consider the standard linear New Keynesian model where inflation π_t , the output gap y_t , the nominal interest rate i_t , and the natural interest rate r_t^n are related by

$$\begin{array}{ll} \pi_t = \beta \mathbb{E}_t \pi_{t+1} + \kappa y_t & \text{Phillips curve} \\ y_t = -\frac{1}{\sigma} (i_t - \mathbb{E}_t \pi_{t+1} - r_t^n) + \mathbb{E}_t y_{t+1} & \text{Intertemporal IS curve} \\ i_t = \rho + \phi_\pi \pi_t + \phi_y y_t + v_t & \text{Taylor rule} \\ r_t^n = \rho + \sigma \psi_{ya} \rho_a a_t & \text{Natural rate of interest.} \end{array}$$

The model is driven by exogenous shocks to monetary policy v_t and technology a_t . These are assumed to follow first-order autoregressive processes:

$$v_t = \rho_v v_{t-1} + \epsilon_t^v \quad a_t = \rho_a a_{t-1} + \epsilon_t^a.$$

Uniqueness of the saddle path solution is guaranteed if $\kappa(\phi_\pi - 1) + (1 - \beta)\phi_y > 0$. For further details and derivations, the reader is

directed to chapter 3 of Galí (2015), which also solves explicitly for the contemporaneous response to a monetary policy shock. That is,

$$D_m(\theta) = \begin{bmatrix} \frac{\partial y_t}{\partial v_t} \\ \frac{\partial \pi_t}{\partial v_t} \end{bmatrix} = \begin{bmatrix} -(1 - \beta\rho_v)\Lambda_v \\ -\kappa\Lambda_v \end{bmatrix},$$

where

$$\Lambda_v = ((1 - \beta\rho_v)(\sigma(1 - \rho_v) + \phi_y) + \kappa(\phi_\pi - \rho_v))^{-1}.$$

These derivatives are the initial points of the impulse response to a monetary policy shock. Under the parameter restrictions required for uniqueness, both entries are negative; a positive monetary shock reduces both inflation and the output gap on impact.

We consider two ways in which monetary transmission may be weakened: either a flatter Phillip curve (κ falls), or a flatter intertemporal IS curve (σ rises). The relevant derivatives are

$$\frac{\partial D_m(\theta)}{\partial \kappa} = \begin{bmatrix} (1 - \beta\rho_v)\Lambda_v^2\phi_\pi \\ \Lambda_v(1 - \kappa\phi_\pi\Lambda_v) \end{bmatrix} \quad \frac{\partial D_m(\theta)}{\partial \sigma} = \begin{bmatrix} (1 - \beta\rho_v)^2\Lambda_v^2(1 - \rho_v) \\ \kappa(1 - \beta\rho_v)\Lambda_v^2(1 - \rho_v) \end{bmatrix}.$$

The derivatives with respect to σ are both positive, so the initial impulse response is attenuated when the IS curve flattens (σ rises). Yet the impact on inflation and interest rates differs by a ratio of $\kappa(1 - \beta\rho_v)$. This difference arises because the Phillips curve induces a positive correlation between output and inflation. Thus, the simple assumption that weaker monetary transmission affects all variables equally is clearly violated if implemented by a flattening of the IS curve. Likewise, when the slope of the Phillips flattens (i.e., κ declines), the marginal effect on the impulse responses of inflation and the output gap differ. However, this case is even more extreme in that if ρ_v and $\kappa\phi_\pi$ are sufficiently large, the signs of the derivatives may differ. That is, a flatter Phillips curve can lead to an attenuation of the inflation response to a monetary tightening but an amplification of the output decline.

In sum, the preceding example shows how even in a simple model the impact of weaker monetary transmission can have different effects on different variables. This motivates the more general case discussed in Section 6.4.

D.2 A More Complicated Model

We use the canonical medium-scale dynamic stochastic general equilibrium (DSGE) model as in Smets and Wouters (2007). The model doesn't allow for an analytical solution. We can write the solution of a linearized DSGE model in the following state-space representation:

$$X_t = B(\theta)X_{t-1} + \Gamma(\theta)\eta_t \quad (\text{D.1})$$

$$Y_t = A(\theta)X_t. \quad (\text{D.2})$$

Here X_t are predetermined, or state, variables; Y_t are observable variables, including non-predetermined, or jump, variables; A , B , and Γ are coefficient matrices that depend on deep parameters θ ; and η_t is a vector of shocks, with $E(\eta_t) = 0$ and $E(\eta_t\eta_t') = I$. Under certain conditions, the state-space representation of the DSGE model has an infinite-order VAR process representation:

$$Y_t = \sum_{j=1}^{\infty} \Delta_j Y_{t-j} + A\Gamma\eta_t \quad (\text{D.3})$$

$$\Delta_j = ABM^{j-1}\Gamma(A\Gamma)^{-1} \quad (\text{D.4})$$

$$M = [I_n - \Gamma(A\Gamma)^{-1}A]B. \quad (\text{D.5})$$

Equations (D.3)–(D.5) show how a solution to a DSGE model in state-space representation relates to a VAR model as we use in the main body of the text.

To estimate the DSGE model, we use an impulse response matching approach as in Christiano, Eichenbaum, and Evans (2005). For this approach, we distinguish between two sets of parameters $\theta = \{\theta^1, \theta^2\}$. We set θ^1 equal to values taken from the literature. θ^2 are set to match the impulse response for a monetary policy shock from our VAR as closely as possible. Formally, our estimator of θ^2 is

$$J = \min_{\theta^2} [\hat{\Psi} - \Psi(\theta^2)]' V^{-1} [\hat{\Psi} - \Psi(\theta^2)], \quad (\text{D.6})$$

where $\Psi(\theta^2)$ is the mapping from θ^2 to the structural model's impulse response function, $\hat{\Psi}$ is the impulse response function estimated using the VAR, and V is a weighting matrix.

The set of parameters that enter the impulse response matching algorithm are $\theta^2 \in \{\sigma_c, \xi_p, \xi_w, \phi_p, \varphi, \lambda, \psi, \sigma_L, \rho, \iota_p, \iota_w\}$. Here, σ_c is the inverse intertemporal elasticity of substitution, ξ_p is the degree of price stickiness, ξ_w is the degree of wage stickiness, ϕ_p is one plus the share of fixed costs in production, φ is the elasticity of the capital adjustment cost function, λ is the habit parameter in consumption, ψ is a parameter related to the capital utilization adjustment cost function, σ_L is the elasticity of labor supply with respect to the real wage, ρ captures the degree of interest rate smoothing in the monetary policy reaction function, ι_p is the degree of price indexation, and ι_w is the degree of wage indexation. This set of parameters is mostly the same as in Christiano, Eichenbaum, and Evans (2005), except that we add ι_p and ι_w , as these parameters are not featured in the model of Christiano, Eichenbaum, and Evans (2005), and we add ρ , as it better helps to match the response of the nominal interest rate.

The remaining parameters, θ^1 , are set to the posterior means in Smets and Wouters (2007), Table 1a. Exceptions are discount factor β and depreciation rate δ , which are set to imply a steady-state annualized nominal interest rate of 3 percent and annualized depreciation rate of 10 percent, respectively. Furthermore, since our model is at monthly frequency, some additional parameters (γ, ρ_R) are transformed accordingly from the value in Smets and Wouters (2007).²⁸ Table D.1 shows the structural parameters both for the version of the model where we use the impulse response matching to estimate selected parameters (θ^2), and the parameterization of the original Smets and Wouters (2007) model.²⁹

²⁸The Smets and Wouters (2007) model is estimated at quarterly frequency, while the data used to generate our VAR impulse responses are at monthly frequency. Therefore, for the original Smets and Wouters (2007) parameterization, we change parameters β , δ , γ , and ρ_R accordingly.

²⁹We use Dynare to solve the DSGE model and perform the impulse response matching.

Table D.1. Parameterization of Structural Model

Parameter	Symbol	Value Est.	Value S&W
Elasticity of the Capital Adjustment Cost Function	φ	3.97	5.74
Coefficient of Relative Risk Aversion	σ_c	0.28	1.38
Habit Persistence Parameter	λ	0.34	0.71
Degree of Wage Stickiness	ξ_w	0.13	0.70
Elasticity of Labor Supply with Respect to the Real Wage	σ_L	2.78	1.83
Degree of Price Stickiness	ξ_p	0.54	0.66
Degree of Wage Indexation	ι_w	0.78	0.58
Degree of Price Indexation	ι_p	0.38	0.24
Parameter Related to Capital Utilization Adjustment Cost Function	ψ	0.36	0.54
1 + Share of Fixed Cost in Production	ϕ_p	1.73	1.60
Inflation Parameter in Monetary Policy (MP) Reaction Function	r_π	2.04	2.04
Interest Rate Smoothing Parameter in MP Reaction Function	ρ	0.64	0.81
Output Gap Parameter in MP Reaction Function	r_y	0.08	0.08
Change in Output Gap Parameter in MP Reaction Function	$r_{\Delta y}$	0.22	0.22
Discount Factor	β	0.9975	0.9975
Steady-State Growth Rate	γ	0.14	0.14
Output Elasticity of Capital	α	0.19	0.19
Depreciation Rate	δ	0.0083	0.0083
Kimball Goods Market Aggregator	ϵ_p	10	10
Kimball Labor Market Aggregator	ϵ_w	10	10
1 + Stady-State Labor Market Markup	ϕ_w	1.5	1.5
Persistence of Monetary Shock	ρ_R	0.53	0.53

Note: The table reports parameterization of Smets and Wouters (2007) model of the U.S. economy. Column “Value Est.” refers to version where selected parameters are estimated using impulse response matching, “Value S&W” to parameterization in Smets and Wouters (2007, Table 1a).

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Capital Requirements in Light of Monetary Tightening*

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In 2021, euro-area inflation surged, prompting the European Central Bank to reverse its monetary stance. This paper argues that this shift did not generate significant financial stress, partly because of the bank capital requirements introduced in the 2010s. We develop a framework to assess their effects over the business cycle. Although capital requirements remained broadly stable, they shaped the transmission of the structural shocks underlying this episode. We find that while these requirements modestly constrained post-COVID growth, they successfully prevented the materialization of risks. Overall, capital requirements strengthened the economy's resilience to adverse shocks at a relatively low macroeconomic cost.

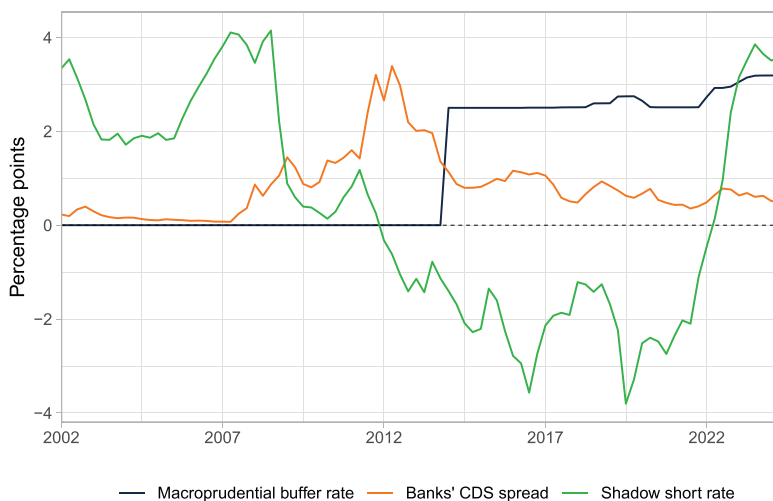
JEL Codes: E44, E52, E58, G28.

1. Introduction

Starting in 2021, euro-area inflation surged, prompting the European Central Bank (ECB) to sharply tighten its monetary stance. The rapid increase in interest rates could have generated financial stress, for instance if lenders had imperfectly hedged fixed-income positions or if borrowers had been unable to absorb the rise in financing

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Figure 1. Monetary Policy, Macroprudential Buffer, and Financial Risks



Source: ESRB, Bloomberg, Krippner (2013).

Note: The macroprudential buffer rate is the simple average of the capital conservation buffer (CCoB), systemic risk buffer (SyRB), and countercyclical capital buffer (CCyB) rates, measured at their announcement rather than implementation date. Banks' CDS spread corresponds to the average spread of a sample of euro-area banks, weighted by their market capitalization. The shadow short rate is an estimate of the implicit short-term interest rate derived from the yield curve, accounting for the unconventional monetary policies implemented over the period.

costs. Such risks were particularly salient given prevailing financial and macroeconomic conditions. First, Figure 1 shows a U-shaped ECB's stance over the past 15 years, a pattern historically associated with financial crises (Jiménez et al. 2022). Second, part of the surge in inflation stemmed from supply-side forces, another well-documented predictor of financial stress (Boissay et al. 2025). Yet, despite these regularities, the euro area did not experience a major financial disruption, as evidenced by the only modest increase in banks' credit default swap (CDS) spreads after 2022 (see Figure 1). This paper argues that a key reason lies in the bank capital requirements introduced during the 2010s.

Indeed, the prudential environment faced by banks today differs substantially from that prevailing during past monetary tightening

episodes. Following the 2008 Global Financial Crisis (GFC) and the 2010 euro-area sovereign debt crisis, European authorities introduced a range of prudential instruments that raised banks' capital requirements from 8 percent to at least 10.5 percent of risk-weighted assets (see Figure 1).¹ Although these tools were initially conceived as countercyclical instruments, consistent with early academic contributions on the topic (Mendoza 2010; Bianchi 2011), authorities have since reassessed this view. During the monetary tightening of 2021–23, despite a marked slowdown in credit growth, these capital buffers were not released. In some jurisdictions, they were even tightened. Overall, capital requirements have been used primarily to bolster banks' resilience rather than to smooth the financial cycle (Hempell, Scalone, and Silva 2024; ECB-ESRB 2025).

In this paper, we assess the implications of this new prudential paradigm in a context of monetary tightening. Although capital requirements have remained relatively stable over time, they can nonetheless shape the transmission of the structural shocks underlying macroeconomic fluctuations. We develop a framework to quantify the costs and benefits of time-invariant capital requirements over the business cycle. Our analysis focuses on the post-COVID period and the subsequent monetary tightening episode, which represents the most volatile macroeconomic environment in the euro area since the post-GFC regulatory reforms. We find that while capital requirements modestly constrained post-COVID growth, they also prevented the materialization of financial risks as the ECB raised policy rates. Put differently, capital requirements acted as automatic stabilizers, complementing monetary policy. Overall, they have contributed to safeguarding macrofinancial resilience at a relatively low macroeconomic cost.

¹The first measure strengthened time-invariant requirements through the capital conservation buffer (CCoB). The second introduced potentially time-varying and sector-specific buffers, namely the systemic risk buffer (SyRB) and the countercyclical capital buffer (CCyB). A third measure established additional requirements for systemically important institutions. This last dimension lies beyond the scope of the present paper, as it would require explicitly modeling heterogeneous banks. While this framework is defined at the European level, its specific design and activation are determined nationally. See the European Systemic Risk Board (ESRB) website for details.

To evaluate the contribution of capital requirements to business cycle dynamics, we employ a medium-scale New Keynesian dynamic stochastic general equilibrium (DSGE) model with financial frictions, building on Clerc et al. (2015) and Mendicino et al. (2018). The model features two dynasties of households: patient households, who own banks and nonfinancial corporations, and impatient households, who borrow from banks. Both impatient households and nonfinancial corporations can default on their loans, which entails costly bankruptcies. Banks, in turn, borrow from patient households, who are assumed to be myopic with respect to banks' risk profile. A prudential authority therefore imposes a limit on banks' leverage through capital requirements. We estimate the model using euro-area data from 2002:Q1 to 2024:Q2 and recover the structural shocks over this period. Given that the model includes a broad set of standard structural disturbances, we can jointly fit a large number of macro-financial series and identify the main drivers of the business cycle, particularly during the post-COVID monetary tightening. We then construct counterfactual scenarios altering the level and design of capital requirements to assess the effects of the current prudential framework.

We find that the post-COVID increase in interest rates was driven by a combination of consumption catch-up and cost-push shocks. Importantly, while standard markup shocks account for a non-negligible share of these cost-push shocks, the model attributes a substantial role to shocks to the marginal efficiency of investment (MEI) for nonfinancial corporations. This reflects the coexistence of high inflation and policy rates with resilient investment activity, a pattern that MEI shocks can naturally rationalize. By contrast, risk shocks in the spirit of Christiano, Motto, and Rostagno (2014) play only a limited role, consistent with the subdued volatility of bank credit. The MEI shock is typically interpreted as capturing variations in the effectiveness of financial intermediation (Justiniano, Primiceri, and Tambalotti 2011). In the present context, it may reflect, among other factors, the widening of banks' net interest margins and the growing role of nonbank intermediaries during the monetary tightening, consistent with the deposit channel of monetary policy (Drechsler, Savov, and Schnabl 2017).

Against this backdrop, our counterfactual exercise indicates that time-invariant capital requirements act as effective automatic

stabilizers. While they slightly dampened gross domestic product (GDP) growth during the 2022 expansion, they also reduced banks' probability of default at the start of 2023, thereby supporting credit and activity during the subsequent slowdown. These results stem from the specific combination of shocks characterizing this episode: although capital requirements somewhat attenuated the transmission of positive MEI shocks to GDP, they also limited the macroeconomic cost of banks' and firms' risk shocks. Even though risk shocks played a modest role during the period under study, this finding underscores the effectiveness of capital requirements in mitigating financial risks at a low macroeconomic cost. Moreover, we find that capital requirements generate heterogeneous effects across savers and borrowers, emphasizing their role in intertemporal risk sharing. Finally, we show that sectoral capital requirements on housing loans (such as those implemented in several European jurisdictions) are particularly effective: during the episode we analyze, they (would have) alleviated the dampening impact of broad-based capital requirements on the propagation of positive MEI shocks. Overall, our results highlight the role of capital requirements in ensuring macroeconomic resilience over the business cycle.

Literature Review. We contribute to a growing literature examining the link between monetary policy tightening and financial stress by highlighting the mitigating role of capital requirements. Boissay et al. (2021) and Jiménez et al. (2022) show that abrupt increases in interest rates following prolonged monetary accommodation tend to trigger financial stress. Boissay et al. (2025) further emphasize that the nature of inflationary shocks matters: supply-driven inflation raises financial stress, whereas demand-driven inflation does not. In contrast, we stress the role of a stabilizing factor in this relationship, namely, the level of bank capital requirements.

Our analysis also relates to the literature on the interaction between monetary and macroprudential policies, with a particular focus on a high-interest-rate environment. Much of the existing work studies *countercyclical* capital requirements in low-rate environments (Rubio and Carrasco-Gallego 2016; Rubio and Yao 2020). We instead focus on the dynamic properties of a *time-invariant* capital requirement in a context of high interest rates, when policy-makers have reconsidered the macroprudential stance. Revelo and Leveuge (2022) show that monetary and macroprudential policies

may conflict in the presence of supply-side or bank-capital shocks. By contrast, our framework emphasizes how banks' resilience shapes macroeconomic dynamics, leading us to conclude that these policies can be complementary in such cases. Consistent with Boissay et al. (2023), we find that tighter capital requirements provide greater room for monetary policy to combat inflation.

We further contribute to the literature studying capital requirements in structural general equilibrium models by identifying which shocks are most affected by these prudential instruments. While these models necessarily simplify banks' balance sheets compared with stress-testing frameworks, they allow for the analysis of second-round effects of financial shocks on macroeconomic stability and enable normative evaluation of prudential tools (Jondeau and Sahuc 2022). Most existing studies focus on the effects of *changes* in capital requirements, either in the long run or in response to specific shocks (Clerc et al. 2015; Poutineau and Vermandel 2017; Mendicino et al. 2018, 2020; de Bandt et al. 2022; Bratsiotis and Pathirage 2023; Gasparini et al. 2024). In contrast, we examine the role of a given, *time-invariant level* of capital requirements in the propagation of a broad set of real and financial shocks, thereby identifying which shocks are most sensitive to prudential constraints.

Finally, we also contribute to the literature on the drivers of post-COVID inflation, though our focus is primarily on interest rate dynamics. Recent empirical work highlights the roles of price and wage rigidities (Arce et al. 2024; Giannone and Primiceri 2024; Bernanke and Blanchard 2025; Eickmeier and Hofmann 2025). We complement this evidence by estimating a New Keynesian DSGE model for the euro area that embeds financial frictions. In line with the literature, we find that the surge in inflation and interest rates resulted from a complex mix of supply and demand shocks. However, our results underscore the importance of positive MEI shocks, which help rationalize the resilience of investment and growth in the euro area despite the sharp monetary tightening.

The remainder of the paper is organized as follows. Section 2 presents the model. Section 3 describes the data and estimation strategy. Section 4 discusses the estimation results and the identified structural shocks, with a focus on the 2021–24 period. Section 5 quantifies the role of capital requirements in the transmission of these shocks. Section 6 concludes.

2. Model

This section presents an overview of the model, which extends Clerc et al. (2015) to analyze the transmission mechanisms of capital requirements under various shocks. We describe the optimization problems faced by each type of agent. The complete set of equilibrium conditions, including optimality and market-clearing relations, is provided in the appendix.

The economy is populated by two dynasties of infinitely lived households (HH), patient (p) and impatient (i), which differ in their discount factors ($\beta^i < \beta^p$). Within each dynasty, members perfectly share risk. Both types of households consume, supply labor, and accumulate housing. Patient households additionally accumulate productive capital and save through bank deposits. They own all firms in the economy and pay a lump-sum tax to finance the partial deposit insurance provided by the government. Impatient households, by contrast, borrow from banks and are subject to idiosyncratic housing-quality shocks that can lead to default, thereby giving rise to a borrowing constraint.

A continuum of monopolistic intermediate goods firms produce differentiated goods using rented physical capital and labor. Prices are sticky due to Calvo-type rigidity. Perfectly competitive final-good producers combine these intermediate goods into a homogeneous final output, which is either consumed or used by capital and housing producers who face dynamic adjustment costs. Productive capital is owned by both capital management firms and nonfinancial corporations (NFCs). NFCs, which are owned by entrepreneurs belonging to patient households, rely on bank loans and are exposed to idiosyncratic capital-quality shocks that can lead to default, thereby inducing a borrowing constraint.

There are two types of competitive banks. Both collect deposits from patient households and raise equity from bankers. One type extends loans to impatient households, while the other lends to NFCs.² Each bank faces portfolio management costs that may result

²This specialization simplifies the pricing problem of each bank type and allows for a heterogeneous transmission of risks across sectors within a period, while still preserving intertemporal transmission through bankers' portfolio reallocation between the two bank types.

in default. Since savers are myopic to banks' individual risk profiles, they do not impose participation constraints. Consequently, banks have an incentive to overleverage, which calls for policy intervention through a capital requirement, limiting loans to a fixed fraction of equity. Banks are owned by bankers, who are members of the patient household and allocate resources so as to equalize expected returns across the two bank types. Banks' balance sheets affect the economy through two main channels: the *net worth channel*, whereby bank profitability influences the income accruing to patient households, and the *credit supply channel*, whereby bank profitability determines the tightness of borrowing constraints faced by debtors.

Finally, the public sector comprises three authorities. The government finances a stochastic stream of expenditures and the deposit insurance scheme through lump-sum taxation. The deposit insurance agency guarantees a fixed share of deposits. The monetary authority sets the short-term nominal interest rate according to a standard Taylor rule.

The model is subject to 11 structural shocks. Following Smets and Wouters (2007), six are standard real shocks: total factor productivity, labor productivity, price markup, monetary policy, time preference, and government spending shocks. In addition, five financial shocks are introduced: two shocks to the marginal efficiency of investment in housing and productive capital, and three risk shocks affecting housing, productive capital, and banks' portfolios. MEI shocks represent exogenous changes in the efficiency with which investment goods are transformed into productive capital, thereby capturing the overall effectiveness of the financial system's intermediation function, as in Justiniano, Primiceri, and Tambalotti (2011). Risk shocks correspond to shocks to the cross-sectional distribution of capital quality and are best interpreted as volatility shocks (Christiano, Motto, and Rostagno 2014).

2.1 Households

Patient Households. The economy features a mass $m^p \in (0, 1)$ of infinitely lived patient households. Each household comprises a mass m^e of entrepreneurs, a mass m^b of bankers, and a mass $m^w = m^p - m^e - m^b$ of workers. In every period, an entrepreneur becomes a worker with probability $1 - \theta^e$, and a banker becomes

a worker with probability $1 - \theta^b$. A corresponding mass of workers is drawn to replace the exiting entrepreneurs and bankers, ensuring that the relative shares of each household member type remain constant over time. The household collects income from all of its members and provides perfect risk sharing among them.

The representative patient households has utility given by

$$\mathbb{E}_t \left[\sum_{s=0}^{\infty} (\beta^p)^s e^{\zeta_{c,t+s}} \left(\log(c_{t+s}^p - \psi \bar{c}_{t+s-1}^p) + v^p \log(h_{t+s}^p) - \frac{\varphi^p}{1+\eta} \Theta_{t+s}^p (\ell_{t+s}^p)^{1+\eta} \right) \right],$$

where c_t^p denotes consumption of nondurable goods at time t , Ψ is the consumption-smoothing parameter, $\bar{c}_t^p$ is the aggregate counterpart of c_t^p , h_t^p is the total stock of housing held by the household's members, and ℓ_t^p denotes labor supply. The parameter $\varphi > 0$ is a scale factor, and $\eta > 0$ is the inverse of the Frisch elasticity of labor supply. The consumption preference parameter $\zeta_{c,t}$ is an exogenous taste shifter following an AR(1) process. Finally, we introduce an endogenous taste shifter Θ_t^p , which is taken as given by patient households and evolves according to

$$\Theta_t^p = \frac{J_t^p}{\bar{c}_t^p - \psi \bar{c}_{t-1}^p}, \quad (1)$$

where

$$J_t^p = (J_{t-1}^p)^{1-\zeta_J} [(\bar{c}_t^p - \psi \bar{c}_{t-1}^p)]^{\zeta_J}. \quad (2)$$

The specification of the endogenous taste shifter follows Galí (2011) and Galí, Smets, and Wouters (2012). Compared with Jaimovich-Rebelo preferences (Jaimovich and Rebelo 2009), it introduces a distinction between the short-run wealth effect on labor supply and its long-run counterpart, through the endogenous taste shifter parameter ζ_J .

The household maximizes the above objective subject to the sequence of budget constraints:

$$P_t c_t^p + D_t^p + Q_t^H h_t^p + (Q_t^K + P_t s_t^K) k_t^p + T_t^p \leq W_t \ell_t^p + \tilde{R}_t D_{t-1}^p + Q_t^H (1 - \delta^H) h_{t-1}^p + (P_t r_t^K + (1 - \delta^K) Q_t^K) k_{t-1}^p + \frac{1}{m^p} P_t \text{Div}_t, \quad (3)$$

where P_t denotes the price of nondurable goods; Q_t^H and Q_t^K the prices of housing and capital goods, respectively; and W_t the nominal wage rate. T_t^p represents a lump-sum tax, and s_t^K is a per-unit real management cost, taken as given by the household and paid to capital management firms. Div_t denotes the total real dividends received by patient households from capital good producers, monopolistic firms, entrepreneurs, financial intermediaries, and capital management firms.

D_t denotes the stock of nominal deposits held at time t , which pay a gross nominal interest rate $\tilde{R}_t$. This return has two components. A fraction $\kappa \in [0, 1]$ represents insured deposits, remunerated at the contractual nominal rate R_{t-1} . The remaining fraction corresponds to uninsured debt, which pays (i) R_{t-1} in the absence of default, and (ii) the net recovery value of bank assets in the event of default. Because savers cannot observe individual banks' risk profiles, they value bank debt according to the expected credit risk of an average unit of bank liabilities. It follows that

$$\tilde{R}_t = R_{t-1} - (1 - \kappa)\Omega_t, \quad (4)$$

where Ω_t is the average default loss per unit of bank debt.

Impatient Households. There is a mass $m^i = 1 - m^p$ of infinitely lived, identical, impatient households. The representative impatient household has utility given by

$$\mathbb{E}_t \left[\sum_{s=0}^{\infty} (\beta^i)^s e^{\zeta_{c,t+s}} \left(\log(c_{t+s}^i - \psi \bar{c}_{t+s-1}^i) + v^i \log(h_{t+s}^i) - \frac{\varphi^i}{1 + \eta} \Theta_{t+s}^i (\ell_{t+s}^i)^{1+\eta} \right) \right],$$

where the utility function and notations are identical to those introduced for patient households. Impatient households differ, however, in their budget constraint. At time t , the representative impatient household borrows a nominal amount B_t^i from banks, which

is evenly distributed across its members. Each household member purchases h_t^i units of housing at the nominal price Q_t^H . At the beginning of period $t + 1$, the housing stock is subject to an idiosyncratic shock ω_{t+1}^i , drawn from a log-normal distribution with mean $-\frac{1}{2} \exp(\zeta_{i,t}) \bar{\sigma}_i^2$ and standard deviation $\exp(\zeta_{i,t}) \bar{\sigma}_i$, where $\zeta_{i,t}$ follows an AR(1) process. These shocks are i.i.d. across time and household members. At time $t + 1$, the household member sells the undepreciated portion of the housing stock, earning the nominal amount $(1 - \delta^H) Q_{t+1}^H h_t^i$, and repays debt at the noncontingent gross nominal interest rate R_t^i . The household member retains the option to default on debt. The impatient household therefore maximizes expected lifetime utility subject to the following resource constraint:

$$P_t c_t^i + Q_t^H h_t^i \leq P_t w_t \ell_t^i + B_t^i + \int_0^\infty \max\{\omega^i (1 - \delta^H) Q_{t+1}^H h_t^i - R_t^i B_t^i; 0\} f_{t+1}^i(\omega^i) d\omega^i$$

and banks' participation constraint.

2.2 Production

Final Good Production. The final good is produced by perfectly competitive firms by combining a continuum of intermediate goods according to the constant-returns-to-scale (CES) production technology

$$y_t = \left(\int_0^1 y_t(f)^{\frac{1}{\mu_t}} df \right)^{\mu_t}, \quad (5)$$

where $\mu_t = \mu e^{\zeta_{\mu,t}}$ is the markup of intermediary good producers with $\zeta_{\mu,t} \sim ARMA(1, 1)$. Let P_t denote the nominal price of the final good and let $P_t(f)$ denote the nominal price of good f . Firms are price takers and seek to maximize nominal profits

$$P_t y_t - \int_0^1 P_t(f) y_t(f) df.$$

Intermediary Goods Production. Intermediate good f is produced by monopolist f by combining labor and capital according to

$$y_t(f) = e^{\zeta_{a,t}} (k_t(f))^\alpha (e^{\zeta_{z,t}} \ell_t(f))^{1-\alpha}, \quad (6)$$

where $\alpha \in (0, 1)$ is the elasticity of gross production with respect to capital, $\zeta_{a,t}$ a stochastic total factor productivity, and $\zeta_{z,t}$ a stochastic labor productivity. Both these variables follow an $AR(1)$ process. The rental rate of capital r_t^K and the wage rate w_t are taken as given by firm f . In a first step, firm f seeks to minimize production costs, given a production level $y_t(f)$:

$$\begin{aligned} & \min_{k_t(f), \ell_t(f)} \{r_t^K k_t(f) + w_t \ell_t(f)\} \\ \text{s.t.} \quad & e^{\zeta_{a,t}} (k_t(f))^\alpha (e^{\zeta_{z,t}} \ell_t(f))^{1-\alpha} = y_t(f), \\ & k_t(f) \geq 0, \quad \ell_t(f) \geq 0. \end{aligned}$$

In a second step, firm f chooses its nominal price $P_t(f)$ to maximize the value to its shareholders (the patient households), taking into account the demand schedule of final good producers. At time t , firms discount payoffs at $t+s$ by $(\beta^p)^s \lambda_{t+s}^p$, where λ_{t+s}^p denotes the marginal utility of consumption of patient households at $t+s$. Firm f faces nominal rigidities à la Calvo: in each period, it can reset its price with probability $1 - \xi$, where $\xi \in (0, 1)$. If not allowed to reset its price at t , firm f mechanically updates its price according to

$$P_t(f) = (\Pi_*)^{1-\iota} (\Pi_{t-1})^\iota P_{t-1}(f),$$

where $\iota \in (0, 1)$, $\Pi_t \equiv P_t/P_{t-1}$, and Π_* denotes the steady-state inflation rate. Accordingly, firm f selects $P_t^*(f)$ to maximize. Firm f thus selects $P_t^*(f)$ so as to maximize

$$\begin{aligned} \mathbb{E}_t \sum_{s=0}^{\infty} (\beta^p \xi)^s \frac{\lambda_{t+s}^p}{\lambda_t^p} y_{t+s} & \left[\left(\frac{\Delta_{t,t+s} P_t^*(f)}{P_{t+s}} \right)^{\frac{1}{1-\mu_t}} \right. \\ & \left. - mc_{t+s} \left(\frac{\Delta_{t,t+s} P_t^*(f)}{P_{t+s}} \right)^{\frac{\mu_t}{1-\mu_t}} \right], \end{aligned}$$

where

$$\Delta_{t,t+s} = \prod_{j=t}^{t+s-1} (\Pi_*)^{1-\iota} (\Pi_j)^\iota,$$

and where mc_t is the real marginal cost solution to the cost minimization problem.

Housing Good and Capital Good Production. Capital and housing goods producers face a similar problem. Let $J \in \{K, H\}$ denote the type of durable good produced, H standing for housing and K for capital. These firms produce i_t^J new units sold at nominal price Q_t^J and are owned by the patient households. The firm's technology is characterized by adjustment costs. In order to produce i_t^J units of new durable goods, the firm must spend

$$\left(1 + S_J \left(\frac{i_t^J}{i_{t-1}^J}\right)\right) i_t^J e^{\zeta_{i,J,t}}$$

units of final good, where

$$S_J(X) = \frac{\psi_J}{2} (X - 1)^2, \quad \psi_J > 0, \quad \zeta_{i,J,t} \sim AR(1).$$

Letting $q_t^J \equiv Q_t^J/P_t$, the typical capital producer seeks to maximize the value to their shareholders

$$\mathbb{E}_t \left\{ \sum_{t=0}^{\infty} (\beta^p)^t \lambda_t^p \left[q_t^J i_t^J - \left(1 + S_J \left(\frac{i_t^J}{i_{t-1}^J}\right)\right) i_t^J e^{\zeta_{i,J,t}} \right] \right\}.$$

The marginal efficiency of investment shock $\zeta_{i,J,t}$ reflects an exogenous change in the efficiency with which investment goods are transformed into capital ready for use in production as in Justiniano, Primiceri, and Tambalotti (2011).

Capital Management Firms. Households acquire units of physical capital through financial intermediaries that charge a management fee, thereby capturing the role of nonbank financial intermediation. The capital management cost s_t associated with households' direct holdings of capital k_t^p is a fee collected by a continuum of measure-one firms operating under decreasing returns to scale. Each firm faces a convex cost function $z(m^p k_t^p)$, where $z(0) = 0$,

$z'(\cdot) > 0$, and $z''(\cdot) > 0$. Capital management firms maximize profits according to

$$\text{Div}_t^c = s_t^K m^p k_t^p - z(m^p k_t^p).$$

We assume a quadratic cost function

$$z(x) = \frac{\xi_s}{2}(x)^2 \quad (7)$$

with $\xi_s > 0$.

Entrepreneurs. At the beginning of period t , entrepreneur j has net worth $N_t^e(j)$. The period $t + 1$ gross nominal return on investment projects is Z_{t+1}^e . The individual entrepreneur seeks to solve the program

$$V_t^e = \max_{\widetilde{\text{Div}}_t^e, E_t^e} \left\{ \widetilde{\text{Div}}_t^e + \mathbb{E}_t \left[\beta^p \frac{\Lambda_{t+1}^p}{\Lambda_t^p} [(1 - \theta^e)N_{t+1}^e + \theta^e V_{t+1}^e] \right] \right\}$$

$$\text{s.t. } \widetilde{\text{Div}}_t^e + E_t^e \leq N_t^e,$$

$$N_{t+1}^e = Z_{t+1}^e E_t^e,$$

$$\widetilde{\text{Div}}_t^e \geq 0,$$

where Λ_t^p is the Lagrange multiplier associated with patient households' budget constraint.

Nonfinancial Corporations. Investment project j receives equity $E_t^e(j)$ from entrepreneurs, together with debt $B_t^e(j)$ from banks. These funds are used to acquire $k_t^e(j)$ units of capital at price Q_t^K . The balance sheet of investment project j is thus

$$E_t^e(j) + B_t^e(j) = Q_t^K k_t^e(j).$$

The capital stock is then subject to a quality shock ω_{t+1}^e at time $t+1$, drawn from a log-normal distribution with mean $-\frac{1}{2} \exp(\zeta_{e,t}) \bar{\sigma}_e^2$ and standard deviation $\exp(\zeta_{e,t}) \bar{\sigma}_e$, where $\zeta_{e,t}$ follows an AR(1) process. After the realization of the quality shock, the effective capital stock is rented to intermediate goods producers at the nominal rental rate $P_{t+1} r_{t+1}^K$. The capital depreciates at rate δ^K , and the undepreciated portion is sold back to capital producers at the price Q_{t+1}^K . At the

end of period $t + 1$, the entrepreneurial firm repays its gross debt obligations at rate R_t^e . Hence, the entrepreneurial firm maximizes the expected discounted value of net profits:

$$\mathbb{E}_t \left[\beta^p \frac{\Lambda_{t+1}^p}{\Lambda_t^p} (1 - \theta^e + \theta^e v_{t+1}^e) \max \{ \omega_{t+1}^e R_{t+1}^K Q_t^K k_t^e(j) - R_t^e B_t^e(j); 0 \} \right] - v_t^e E_t^e(j)$$

subject to banks' participation constraint, denoting

$$R_{t+1}^K = \frac{P_{t+1} r_{t+1}^K + (1 - \delta) Q_{t+1}^K}{Q_t^K},$$

and where v_t^e is the Lagrange multiplier associated with entrepreneurs' balance sheet constraint.

2.3 Bankers and Banks

Bankers. An individual banker starts period t with net worth N_t^b , which is invested as equity (i) in a continuum of investment projects E_t^F and (ii) a continuum of housing projects E_t^M . The period $t + 1$ aggregate gross nominal return on these projects is Z_{t+1}^b . The individual banker seeks to solve the program

$$V_t^b = \max_{\widetilde{\text{Div}}_t^b, E_t^M, E_t^F} \left\{ \widetilde{\text{Div}}_t^b + \mathbb{E}_t \left[\beta^p \frac{\Lambda_{t+1}^p}{\Lambda_t^p} [(1 - \theta^b) N_{t+1}^b + \theta^b V_{t+1}^b] \right] \right\}$$

$$\text{s.t.} \quad \widetilde{\text{Div}}_t^b + E_t^M + E_t^F \leq N_t^b,$$

$$N_{t+1}^b = Z_{t+1}^M E_t^M + Z_{t+1}^F E_t^F,$$

$$\widetilde{\text{Div}}_t^b \geq 0,$$

where Λ_t^p is the Lagrange multiplier associated with patient households' budget constraint.

Banks. At time t , a bank of type $j \in \{M, F\}$ takes equity E_t^j from bankers and borrows D_t^j at gross rate R_t from households to extend loans B_t^j . Hence the balance sheet constraint:

$$E_t^j + D_t^j = B_t^j.$$

The return at time $t + 1$ on a well-diversified portfolio of loans is denoted R_{t+1}^j . This portfolio is subject to a performance shock ω_{t+1}^j at $t + 1$, drawn from a log-normal distribution with mean $-\frac{1}{2} \exp(\zeta_{B,t}) \bar{\sigma}_j^2$ and standard deviation $\exp(\zeta_{B,t}) \bar{\sigma}_j$,³ where $\zeta_{B,t}$ is common to both bank types and follows an AR(1) process. At the end of period $t + 1$, the bank pays the gross interest rate on deposits. A bank of type j thus maximizes

$$\mathbb{E}_t \left[\beta^p \frac{\Lambda_{t+1}^p}{\Lambda_t^p} (1 - \theta^b + \theta^b v_{t+1}^b) \max \left\{ \omega_{t+1}^j R_{t+1}^j B_t^j - R_t D_t^j; 0 \right\} \right] - v_t^b E_t^j,$$

where v_t^b is the Lagrange multiplier associated with bankers' balance sheet constraint.

Because of limited liability, shareholders' payoffs cannot be negative. Whenever $\omega_{t+1}^j R_{t+1}^j B_t^j < R_t D_t$, the bank defaults. In this event, its equity is written down to zero, and deposits are taken over by the deposit insurance agency (DIA), which reimburses an exogenous fraction κ of deposits. The DIA partially recovers these payments by seizing the failed bank's loan portfolio net of resolution costs, assumed to amount to a fraction μ^j of recovered assets.

Finally, the bank faces a regulatory capital constraint

$$E_t^j \geq \phi_t \gamma_t^j B_t^j, \quad (8)$$

which states that the capital-to-asset ratio must exceed a (possibly) time-varying level set exogenously by the prudential authority. This requirement is decomposed into two components: a broad-based risk-weighted capital requirement ϕ_t , common to all banks, and a risk weight γ_t^j , specific to each bank type. Both are assumed to be set exogenously by the prudential authority, consistent with the Basel

³This parameterization ensures that $\mathbb{E}_t[\omega_{t+1}^j] = 1$.

standardized approach to risk.⁴ In equilibrium, this constraint binds with equality since deposits, being partially insured, are cheaper than equity (Mendicino et al. 2020). Hence, loans can be written as

$$B_t^j = \frac{E_t^j}{\phi_t \gamma_t^j}.$$

Macroeconomic Shocks, Banks Balance Sheets, and Capital Requirements. Capital requirements affect the transmission of macroeconomic shocks to credit through two channels. A first channel goes through the denominator of the above ratio: the higher $\phi_t \gamma_t^j$, the lower the share of loans in the balance sheet, and the lower the financial accelerator. This suggests that capital requirements may act as automatic stabilizers across the business cycle. The bank net worth is a second channel. It follows the aggregate law of motion:

$$N_t^b = [(\theta^b + \chi_b(1 - \theta^b))(Z_t^M E_{t-1}^M + Z_t^F E_{t-1}^F),$$

where θ_b is the proportion of net worth left by retiring bankers to new bankers. Z_t^i is the aggregate return on equity:

$$Z_t^i = \frac{1}{\phi_t} R_t^i \Upsilon(\bar{\omega}_t^i),$$

where $\Upsilon(\cdot)$ denotes the aggregate value of banks' idiosyncratic shocks. This function depends on $\bar{\omega}_t^i$, the threshold below which banks default. A higher capital requirement lowers this threshold, thereby increasing banks' net worth. In addition, for a given reduction in bank risk, capital requirements alter the transmission of macroeconomic shocks to banks' portfolio returns R_t^i . For instance, holding risk constant, consumption-preference or marginal-efficiency shocks affect the return on bank portfolios R_t^i more strongly than risk shocks that directly affect the default threshold $\bar{\omega}_t^i$. Changes in net worth thus result from the interplay between two opposing effects: risk mitigation and reduced loan profitability. When net

⁴In practice, many banks use internal models where risk weights depend on estimated default probabilities. This simplification can still be justified: (i) the mapping between default probabilities and risk weights is nonlinear and would complicate the model; (ii) empirical default probabilities differ from banks' internal estimates due to portfolio selection and model optimization; and (iii) capital regulation allows supervisors to impose additional buffers or risk weights by asset class, which can be interpreted as an exogenous process.

worth remains positive, bankers reinvest it as equity to finance new loans. The relative strength of these two effects depends on the configuration of shocks hitting the economy. The remainder of the paper investigates this mechanism over the 2021–24 episode by solving and estimating the model.

2.4 Public Authorities

Government. The government levies lump-sum taxes $T_t = m^p T_t^p$ to finance the DIA and a stochastic flow of expenditures g_t . The budget is assumed to be balanced so that $T_t = T_t^{DIA} + P_t g_t$. Government expenditures follow the process $g_t = g e^{\zeta_{g,t}}$, with $\zeta_{g,t}$ following an $AR(1)$ process. We impose that in steady state $g/y = s_g$.

Deposit Insurance Agency. The DIA collects all payments from banks on the deposit market. The gross return on deposits from nondefaulting banks is recovered in full by the DIA. There is a fraction $1 - F_t^j(\bar{\omega}_t^j)$ of such deposits in the banking sector j . The remaining fraction is subject to default. In case of default, the DIA recovers the assets of the defaulting bank, net of a fraction μ^j due to recovery costs. The average default loss per unit of bank debt in sector j is thus

$$\begin{aligned} \Omega_t^j = & \left(\int_0^{\bar{\omega}_t^j} f_{t+1}^j(\omega^j) d\omega^j \right) R_{t-1} \\ & - (1 - \mu^j) \left(\int_0^{\bar{\omega}_t^j} \omega^j f_t^j(\omega^j) d\omega^j \right) R_t^j \frac{B_{t-1}^j}{D_{t-1}^j}. \end{aligned}$$

Let us define the aggregate average default loss per unit of bank debt:

$$\Omega_t = \frac{d_{t-1}^M}{d_{t-1}} \Omega_t^M + \frac{d_{t-1}^F}{d_{t-1}} \Omega_t^F. \quad (9)$$

The DIA insures a fraction κ of deposits and then redistributes the recovered net assets to the depositors, so that

$$\tilde{R}_t = \kappa R_{t-1} + (1 - \kappa)(R_{t-1} - \Omega_t) = R_{t-1} - (1 - \kappa)\Omega_t.$$

It follows that the total cost for the DIA of insuring deposits, and hence the total amount of lump-sum taxes, is

$$T_t^{DIA} = \kappa \Omega_t d_{t-1}.$$

Monetary Policy. The central bank sets the (gross) short-term nominal interest rate R_t according to the following monetary policy rule:

$$\begin{aligned} \log \left(\frac{R_t}{R_*} \right) = & \rho_R \log \left(\frac{R_{t-1}}{R_*} \right) + (1 - \rho_R) \left[a_\Pi \log \left(\frac{\Pi_t}{\Pi_*} \right) \right. \\ & \left. + a_y \log \left(\frac{GDP_t}{GDP_{t-1}} \right) \right] + \zeta_{R,t}, \end{aligned} \quad (10)$$

where star values denote steady-state counterparts, ρ_R measures the degree of interest rate smoothing, a_Π measures the reaction to inflation, a_y measures the reaction to GDP, and $\zeta_{R,t}$ a white noise shock.

3. Data and Estimation

A key contribution of this paper is to bring a medium-scale DSGE model with a rich set of financial frictions and shocks to euro-area (EA) data. Identifying which shocks are most affected by capital requirements is essential, since their impact depends on the underlying source of macroeconomic fluctuations. Estimating the model allows us to disentangle these channels and to assess the macroeconomic consequences of the prevailing prudential stance, conditional on the nature of the business cycle.

The model is estimated using quarterly euro-area data spanning 2002:Q1–2024:Q2. Although this sample is relatively short and constrained by data availability, it covers nearly the entire lifetime of the euro area.⁵ Parameters determining the steady state are calibrated independently of the model's dynamic properties, while the remaining parameters are estimated. Data sources are detailed in Table 1.

⁵Calibration targets are computed from the inception of the euro area whenever possible.

Table 1. Data Sources

Series	Source
Shadow Short-Term Interest Rate	Krippner (2013, 2015)
3-Month EURIBOR	ECB, FM, Q.U2.EUR.RT.MM.EURIBOR3MD.HSTA
Implicit GDP Price Index	Eurostat, MNA, Q.PDI5.EUR.SCA.BIGQ.EA
Real GDP	Eurostat, MNA, Q.CIV15.MEUR.SCA.BIGQ.EA
Real Household Consumption	Eurostat, MNA, Q.CIV15.MEUR.SCA.P31.S14.S15.EA
Nominal Households' Investment	ECB, QSA, Q.Y.I9.W0.S11.N.D.P51G..Z.Z.Z.XDC..T.S.V.N..T
Nominal Firms' Investment	ECB, QSA, Q.Y.I9.W0.S11.N.D.P51G..Z.Z.Z.XDC..T.S.V.N..T
Hours Worked	ECB, ENA, Q.Y.U2.W2.S1.S1..Z.EMP..Z..T..Z.HW..Z.N
EA Population (Changing Composition)	ECB, ENA, Q.N.U2.W0.S1.S1..Z.POP..Z..Z.Z.PS..Z.N
EA 20 Population (Fixed Composition)	ECB, ENA, Q.N.U2.W0.S1.S1..Z.POP..Z..Z.Z.PS..Z.N
Nominal Credit to Households	ECB, BSI, Q.U2.N.A.A20.A.1.U2.2250.Z01.E
Nominal Credit to Firms	ECB, BSI, Q.U2.N.A.A20.A.1.U2.2250.Z01.E
Real House Prices	OECD, House Prices, Q.EA.RHP
Banks' 5-Year CDS Premium	Bloomberg, Conversion into Default Probabilities (40% Recovery Rate)
Return on Equity, Banks	ECB, CBD2, Q.U2.W0.57..Z..Z.A.A.I2003..Z.Z.Z..Z.Z.PC
Price-to-Book Ratio of Banks	Datastream
General Government Final Consumption Expenditure (% GDP)	Eurostat, Government Revenue, Expenditure and Main Aggregates, A.PC.GDP.S13.P3.EA19
Nominal GDP	ECB, MNA, Q.N.U2.W2.S1.S1.B.BIGQ..Z.Z..Z.EUR.V.N
Household Interest Rate	ECB, MIR, M.U2.B.A2C.A.R.A.2250.EUR.N
NFC Interest Rate	ECB, MIR, M.U2.B.A2A.A.R.A.2240.EUR.N
Lending for House Purchases: Up to 1 Year	ECB, BSI, M.U2.N.A.A22.F.1.U2.2250.Z01.E
Lending for House Purchases: Over 1 Year and Up to 5 Years	ECB, BSI, M.U2.N.A.A22.I.1.U2.2250.Z01.E
Lending for House Purchases: Over 5 Years	ECB, BSI, M.U2.N.A.A22.J.1.U2.2250.Z01.E
Loans to NFC: Up to 1 Year	ECB, BSI, M.U2.N.A.A20.F.1.U2.2240.Z01.E
Loans to NFC: Over 1 Year and Up to 5 Years	ECB, BSI, M.U2.N.A.A20.I.1.U2.2240.Z01.E
Loans to NFC: Over 5 Years	ECB, BSI, M.U2.N.A.A20.J.1.U2.2240.Z01.E
EURIBOR 3-Month	ECB, FM.M.U2.EUR.RT.MM.EURIBOR3MD..HSTA
German Bond 3 Years	ECB, FM.B.DE.EUR.RT.BB.DE3YT..RR.YLD
German Bond 7 Years	ECB, FM.B.DE.EUR.RT.BB.DE7YT..RR.YLD
German Bond 20 Years	ECB, FM.B.DE.EUR.RT.BB.DE20YT..RR.YLD
Bank Loans in NFC Total Debt	BDF, CFT, Q.S.I8.W0.S11.S1.N.L.E.F401.T..Z.XDC_R.DEBT..T.S.V.N..T

We implicitly assume that the structural features of the economy remain stable throughout the two decades of data used for calibration and estimation, a strong assumption made for tractability. Moreover, since the next section focuses on a counterfactual analysis of the post-COVID episode, our main interest lies in obtaining reasonable end-of-sample estimates for the parameters and shocks. Potential misspecifications at the beginning of the sample are therefore not excessively concerning. Finally, we assume that prudential authorities do not follow any preannounced rule known to all agents, as there was no mechanical adjustment of capital requirements across euro-area countries over the sample period.

3.1 *Calibrated Parameters*

A first subset of parameters is set prior to estimation. Some are commonly used in the literature, while others are chosen to simultaneously match a series of steady-state moments. These parameters are presented in Table 2. Targeted moments and their theoretical counterparts are presented in Table 3.

Demographics, Preferences, and Government. The share of impatient households m_i corresponds to the share of households with debt (Finance and Network 2013). The inverse Frisch elasticity is set to 4, following Galí (2010) and Chetty et al. (2011). The labor disutility parameters φ_p and φ_i are normalized to 1, as they only affect the scale of the economy. The discount factor of patient households is set to $\beta_p = 0.997$, targeting the risk-free rate.⁶ The share of final government expenditure s_g is directly set from the data to 21 percent.

Production. The markup rate is set to 20 percent, implying $\mu = 1.2$. The depreciation rate of capital is set to $\delta_K = 0.03$. The capital share in production is $\alpha = 0.3$. The survival rate of entrepreneurs θ_e is set to 0.975, as in Gertler and Kiyotaki (2010). Transfers from households to entrepreneurs are calibrated to match the NFC debt-to-GDP ratio. The standard deviation of idiosyncratic shocks affecting entrepreneurs, $\bar{\sigma}_e$, is chosen to match the observed default probability of NFCs.

⁶See Iacoviello and Neri (2010) for a discussion of patient households' discount factor.

Table 2. Preset and Calibrated Parameters

	Parameter	Value
<i>A. Preset Parameters</i>		
Inverse Frisch Elasticity	η	4
Patient Disutility of Labor	φ^p	1
Impatient Disutility of Labor	φ^i	1
Bank M Bankruptcy Cost	μ_M	0.3
Bank F Bankruptcy Cost	μ_F	0.3
NFC Bankruptcy Cost	μ_e	0.3
HH Bankruptcy Cost	μ_i	0.3
Share of Insured Deposits in Bank Debt	κ	0.54
Consumption Smoothing	ψ	0.14
Productivity	A	1
Steady-State Markup	$1 - \mu$	0.2
Capital Share in Production	α	0.3
Depreciation Rate of Capital	δ_K	0.03
Survival Rate of Entrepreneurs	θ_e	0.975
Capital Requirements for Bank F	ϕ_F	0.105
<i>B. Calibrated Parameters</i>		
Impatient Household Discount Rate	β_i	0.982
Patient Household Discount Rate	β_p	0.997
Housing Depreciation Rate	δ_h	0.009
Patient Housing Scale Factor	ν_p	0.064
Impatient Housing Scale Factor	ν_i	0.642
Management Cost	ξ_s	0.005
Survival Rate of Bankers	θ_B	0.873
Std. Idiosyncratic Shocks, Bankers M	$\bar{\sigma}_M$	0.013
Std. Idiosyncratic Shocks, Bankers F	$\bar{\sigma}_F$	0.043
Std. Idiosyncratic Shocks, Entrepreneurs	$\bar{\sigma}_e$	0.397
Std. Idiosyncratic Shocks, HH	$\bar{\sigma}_i$	0.361
Banker's Endowment	χ_b	0.869
Entrepreneur's Endowment	χ_e	0.329
Capital Requirements for Bank M	ϕ_M	0.037

Banks. The share of insured deposits in bank debt κ is set to 0.54, following Demirgüç-Kunt, Kane, and Laeven (2015). The survival rate of bankers θ_b is used to match the price-to-book ratio of

Table 3. Calibration Targets

	Target	Model
Indebted Households Share m_i	0.44	0.44
Final Gov. Consumption Exp. s_g	0.21	0.21
Risk-Free Rate $\bar{r}$	1.34%	1.34%
Yearly Inflation Rate	1.99%	1.99%
Return on Asset Equity	8.78%	8.78%
Housing Investment as a Share of GDP	0.06	0.06
HH Loans to (Quarterly) GDP	1.98	1.97
Housing among Households Capital	0.61	0.58
NFC Loans to (Quarterly) GDP	1.67	1.66
Banks Default Rate	1.22%	1.22%
Price-to-Book Ratio μ_b	1.15	1.12
Loan to Value	37.3%	37.7%
Capital Share of Households	0.16	0.16
Spread NFC Loans	1.38	1.36
Spread Households Loans	1.05	1.05
NFC Default Rate (Untargeted)	2.5%	2.16%
HH Default Rate (Untargeted)	1%	2.28%

euro-area banks, denoted μ_b . Transfers from bankers χ_b are chosen to reproduce banks' return on assets and equity. Capital requirements are assumed to remain constant throughout the calibration period, in line with Basel III standards. Since our primary interest lies in reproducing end-of-period conditions, we set the broad-based capital requirement to $\phi = 10.5\%$. Risk weights γ^F and γ^H are set to 0.35 and 1, respectively. The volatility parameter σ_F is used to match banks' probability of default. The ratio between σ_F and σ_M , which is not observed, is set such that $\frac{PD_F}{PD_M} = \frac{\gamma^F}{\gamma^M}$, ensuring an internally consistent sectoral capital structure. Although banks granting mortgages are therefore less risky than those lending to firms, household loans are not necessarily less risky than NFC loans.

Nonfinancial Corporations and Households. As it is impossible to target both the spread and the default probability of households and NFCs at the steady state (as they depend on the same parameter), we target the spread. The resulting nontargeted default

probability remains consistent with the data (see Table 3).⁷ The depreciation rate of housing capital δ_h , the discount factor of impatient households β_i , the housing utility scale factors v_i and v_p , the standard deviation of idiosyncratic shocks affecting households $\bar{\sigma}_i$, and the management cost ξ_s are set to jointly match: (i) housing investment as a share of GDP, (ii) the spread on household loans, (iii) housing as a share of capital held by patient households, (iv) household credit as a share of GDP, (v) the average loan-to-value ratio, and (vi) the share of capital held by households in total capital.

3.2 *Estimated Parameters*

The remaining parameters are estimated using 10 quarterly, calendar, and seasonally adjusted series: the GDP implicit price index, real GDP, real household consumption, the short-term interest rate, hours worked, real household investment, real nonfinancial corporations' investment, real credit to households, real credit to NFCs, and banks' default probabilities. Most of these variables are standard observables in the literature. A distinctive feature of the period under study, however, is the historically low level of nominal interest rates, suggesting that the monetary authority may have been constrained by an effective lower bound (ELB). While we do not explicitly model the ELB or unconventional monetary policy tools designed to circumvent it, we use as observable the shadow rate of Krippner (2013, 2015), i.e., the hypothetical short-term interest rate that would prevail if the ELB were not binding. This proxy enables us to analyze the role of capital requirements during the exceptional monetary tightening experienced in the euro area since 2020. When available, series correspond to a changing euro-area composition. In addition, because banks' risk shocks play a central role in assessing the macroeconomic implications of capital requirements,

⁷We compute targeted spreads between a composite interest rate on loans and the composite risk-free rate. The composite loan rate is the weighted average of interest rates by maturity (up to one year, one to five years, and over five years). The composite risk-free rate is the weighted average of the following benchmarks: the three-month euro-area interbank offered rate, or EURIBOR (up to 1 year); the three-year German bond yield (1 to 5 years); the 7-year German bond yield (over 5 years for NFC loans); and a weighted average of the 7-year and 20-year bond yields (over 5 years for housing loans).

we construct a series of banks' expected default probabilities from CDS spreads of a sample of large euro-area banks. All quantities are normalized by total population. With the exception of the short-term interest rate and banks' probability of default, all variables are expressed in demeaned log-differences to focus on cyclical dynamics.⁸ Both the short-term interest rate and banks' probability of default are expressed as deviations from their steady-state values.

The estimation follows a Bayesian approach applied to a first-order log-linearized version of the model, using the Kalman filter. Computations are performed with the Dynare toolbox (Adjemian et al. 2024). Prior and posterior distributions are reported in Table 4. As the model expresses shocks in percent of their standard deviation, the estimated standard deviations are scaled accordingly. Priors are set consistently with the literature on New Keynesian models estimated on euro-area data. For parameters related to consumption habits, adjustment costs, price indexation, and the monetary policy rule, we follow Jondeau and Sahuc (2022), whose framework similarly focuses on macrofinancial interactions in the euro area. The prior for the taste-shifter trend follows Galí, Smets, and Wouters (2012). The prior mean for price rigidity is aligned with Smets and Wouters (2003), but we impose a relatively small standard deviation to avoid overestimating rigidity during the COVID episode, which featured a sharp drop in consumption but only a mild fall in inflation. This restriction is consistent with recent empirical evidence on pre-COVID euro-area price rigidity (Gautier et al. 2024). Finally, we adopt an agnostic stance regarding shocks by assigning identical priors to all of them.

One distinctive outcome of our estimation is the relatively low degree of habit formation compared with the literature (around 0.7), implying less consumption inertia. This is partly offset by a lower estimated trend in the endogenous taste shifter relative to the variant without unemployment as an observable variable studied in Galí, Smets, and Wouters (2012): in our case, the marginal rate of substitution between labor and consumption places greater weight on

⁸The series of investment by nonfinancial corporations display irregularities between 2015 and 2019 unrelated to macroeconomic conditions, primarily due to data for the Netherlands and Ireland. Their respective series are therefore excluded from the euro-area aggregate.

Table 4. Estimated Parameters

	Prior Distribution			Posterior Distribution		
	Dist.	Mean	Std.	Mean	Std.	
A. Structural Parameters						
Endogenous Taste Shifter	ζ_J	Beta	0.5	0.2	0.0805	0.1205
Habits	ψ	Beta	0.4	0.1	0.1319	0.0423
Housing Adjustment Cost	ψ_H	Gamma	4	1	4.2464	0.8716
Capital Adjustment Cost	ψ_K	Normal	4	1	2.7013	0.5124
Price Rigidity	ξ	Beta	0.75	0.025	0.8681	0.0099
Price Indexation	ι	Beta	0.4	0.1	0.2917	0.0870
Monetary Policy Smoothing	ρ_R	Beta	0.8	0.1	0.8364	0.0146
MP Reaction to Inflation	α_π	Normal	1.7	0.1	2.0256	0.0954
MP Reaction to GDP Growth	α_y	Normal	0.125	0.05	0.1206	0.0362
B. Shocks Standard Deviation						
Total Productivity	σ_a	Inv. Gam.	0.5	2	3.6483	0.8429
Labor Productivity	σ_z	Inv. Gam.	0.5	2	0.7786	0.0596
Markup	σ_μ	Inv. Gam.	0.5	2	23.1760	3.0251
Housing Adjustment	σ_{iH}	Inv. Gam.	0.5	2	2.7875	0.2161
Capital Adjustment	σ_{iK}	Inv. Gam.	0.5	2	5.0465	0.4658
Monetary Policy	σ_R	Inv. Gam.	0.5	2	0.1460	0.0129
Government Spending	σ_g	Inv. Gam.	0.5	2	2.1734	0.1672
Preference	σ_c	Inv. Gam.	0.5	2	2.4774	0.2556
NFC Risk	σ_e	Inv. Gam.	0.5	2	2.0718	0.2267
HH Risk	σ_i	Inv. Gam.	0.5	2	1.3335	0.1409
Bank Risk	σ_B	Inv. Gam.	0.5	2	4.1870	0.3169
C. Shocks Autocorrelation						
Total Productivity	ρ_a	Beta	0.5	0.2	0.8972	0.0317
Labor Productivity	ρ_z	Beta	0.5	0.2	0.9307	0.0195
Markup Shock	ρ_μ	Beta	0.5	0.2	0.0961	0.0519
Housing Adjustment Shock	ρ_{iH}	Beta	0.5	0.2	0.6017	0.0547
Capital Adjustment Shock	ρ_{iK}	Beta	0.5	0.2	0.7510	0.0354
Government Spending Shock	ρ_g	Beta	0.5	0.2	0.5761	0.0841
Time Preference Shock	ρ_c	Beta	0.5	0.2	0.4044	0.0930
NFC Risk Shock	ρ_e	Beta	0.5	0.2	0.9580	0.0209
HH Risk Shock	ρ_i	Beta	0.5	0.2	0.9685	0.0177
Bank Risk Shock	ρ_B	Beta	0.5	0.2	0.8970	0.0344

past consumption. Both features appear to reflect the inclusion of the COVID period. A variant estimated over the subsample ending in 2019:Q4 (although not perfectly comparable due to the nonstationarity of the short-term interest rate) yields a more conventional combination of these parameters. Adjustment costs are also somewhat lower for similar reasons but remain consistent with previous studies. Other estimated parameters are broadly standard. The monetary policy coefficients suggest that the shadow interest rate successfully captures the stance of unconventional policy at the ELB. As in Christiano, Motto, and Rostagno (2014), risk shocks—whether for households, nonfinancial corporations, or banks—are among the most persistent, together with total factor productivity and labor productivity shocks. The price markup shock displays high variance and short persistence, capturing high-frequency movements in inflation and the euro area’s exposure to import price fluctuations. Other shocks, such as preference, government spending, and adjustment cost shocks, fall within intermediate ranges of persistence and volatility.

3.3 Model Evaluation

The resulting theoretical variance decomposition is reported in Table 5. A notable finding is the limited contribution of risk shocks, consistent with Pfeifer (2016). Most of the variation in macroeconomic aggregates is driven by standard sources of fluctuations—in particular, preference shocks. This partly reflects the fact that we use these shocks to match credit dynamics rather than credit spreads, in order to replicate the empirical behavior of the credit-to-GDP ratio, a key variable for macroprudential policymakers. It also reflects the estimation window, during which macroeconomic disturbances played a particularly prominent role. One might nevertheless question the relatively small contribution of bank risk shocks, despite the inclusion of the 2008–09 period in the estimation sample and the explicit use of banks’ probability of default as an observable. This pattern stems from the fact that euro-area banks’ default probabilities did not rise markedly in 2008, but rather in 2011, during the sovereign debt crisis. Although that episode was associated with below-trend growth, it did not exhibit the same crisis dynamics as 2008–09. In the model, therefore, risk shocks primarily capture the medium-run

Table 5. Variance Decomposition, in Percent

	σ_a	σ_z	σ_μ	σ_{i_K}	σ_{i_H}	σ_R	σ_g	σ_c	σ_e	σ_i	σ_B
GDP	4.38	3.66	6.54	15.31	1.05	4.42	8.49	56.01	0.06	0.04	0.04
Consumption	0.39	2.47	4.88	1.45	0.07	4.08	0.25	86.28	0.05	0.06	0.02
Hours Worked	4.07	4.74	6.56	13.58	1.03	3.81	8.69	56.88	0.14	0.02	0.49
Policy Rate	25.58	5.58	9.42	31.59	0.26	6.95	2.19	15.25	1.08	0.4	1.72
Inflation Rate	7.87	4.21	36.48	17.37	0.17	12.27	2.01	18.36	0.35	0.12	0.79
NFC Investment	15.14	1.36	2.82	77.96	0.04	1.27	0.05	0.47	0.86	0.02	0.01
HH Investment	5.28	6.57	1.46	14.07	68.68	0.66	0.19	1.58	0.85	0.48	0.17
NFC Credit	9.63	0.42	5.48	11.69	0.13	2.26	0.2	6.08	58.29	4.6	1.33
HH Credit	5.58	0.73	13.27	3.13	0.68	7.89	0.06	2.57	11.22	54.36	0.5
PD Banks	0.24	0.01	0.05	0.06	0	0.17	0	0.05	0.95	0.13	98.34

consequences of crises rather than their initial triggers. Overall, standard macroeconomic shocks remain the main drivers of both business and financial cycles in the estimated model.

Table 6 reports selected empirical and theoretical moments. The model replicates reasonably well the high variance observed in the data for most variables. While empirical GDP volatility exceeds model-implied volatility, the opposite holds for credit, partly because the model only features one-period debt. Empirical covariances and autocorrelations, however, deviate more substantially from their theoretical counterparts, reflecting the extraordinary nature of the COVID episode. This also suggests that the structural parameters were not excessively distorted by this period. Finally, empirical autocorrelations are qualitatively consistent with model-implied ones, except for household credit, which exhibits substantially greater persistence, partly because house prices are not included as an observable in the estimation.

4. The Anatomy of Monetary Tightening

To assess the effects of capital requirements on the macroeconomic impact of the 2021–23 monetary tightening, we first identify the structural shocks that triggered this unprecedented episode. The 11 shocks included in the model exert distinct influences on inflation, interest rates, and default probabilities. Their joint dynamics therefore shape how capital requirements propagate through the economy, underscoring that the origin of the shock is critical.

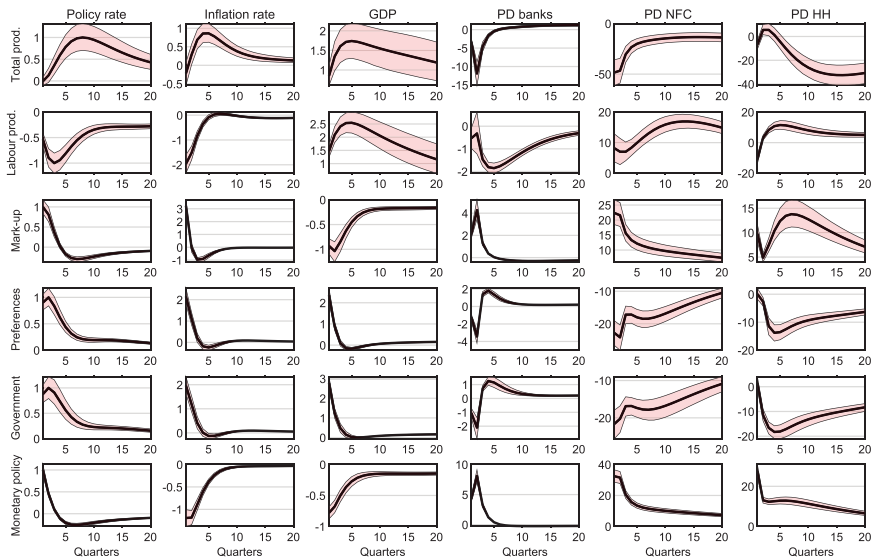
4.1 Impulse Response Functions

Figure 2 displays the impulse response functions of the policy rate, inflation, GDP, and the three probabilities of default following macroeconomic shocks. We consider three supply shocks (total factor productivity, labor productivity, and the markup of intermediate goods producers), two demand shocks (households' preference and government spending), and one monetary policy shock. Each shock is calibrated to generate a 1 percent increase in the policy rate, in absolute value. Productivity shocks lead to a more gradual adjustment in the policy rate compared with other disturbances. Policy rate, firms' markup, and labor productivity shocks all depress

Table 6. Data and Model Moments

	Data	Model		
		Mean	90%	CI
A. Variance				
GDP	3.53	2.61	2.07	3.09
Consumption	4.75	5.65	4.01	7.24
Hours Worked	4.93	4.95	3.92	5.89
MP Rate	5.41	4.43	2.97	5.72
Inflation	0.16	0.36	0.28	0.46
NFC Investment	12.34	20.31	14.63	26.63
HH Investment	8.21	9.43	7.6	11.58
NFC Credit	1.7	2.72	2.2	3.19
HH Credit	0.91	3.49	2.84	4.17
PD Banks	1.33	1.1	0.45	1.6
B. Covariance with GDP				
GDP	3.53	2.61	2.07	3.09
Consumption	3.87	3.11	2.14	3.92
Hours Worked	4.04	3.35	2.58	4
MP Rate	−0.14	−0.17	−0.37	0.03
Inflation	−0.29	0.08	−0.04	0.2
NFC Investment	4.87	3.47	2.55	4.44
HH Investment	4.62	0.73	0.57	0.9
NFC Credit	0.01	−0.3	−0.51	−0.09
HH Credit	0.67	0.92	0.71	1.16
PD Banks	−0.24	−0.03	−0.04	−0.02
C. First-Order Autocorrelation				
GDP	−0.2	−0.13	−0.19	−0.06
Consumption	−0.29	−0.19	−0.25	−0.12
Hours Worked	−0.28	−0.17	−0.23	−0.11
MP Rate	0.98	0.91	0.88	0.94
Inflation	0.49	0.51	0.42	0.6
NFC Investment	−0.11	0.22	0.08	0.34
HH Investment	−0.02	0.21	0.07	0.36
NFC Credit	0.67	0.49	0.45	0.53
HH Credit	0.56	0	−0.03	0.03
PD Banks	0.93	0.9	0.84	0.95

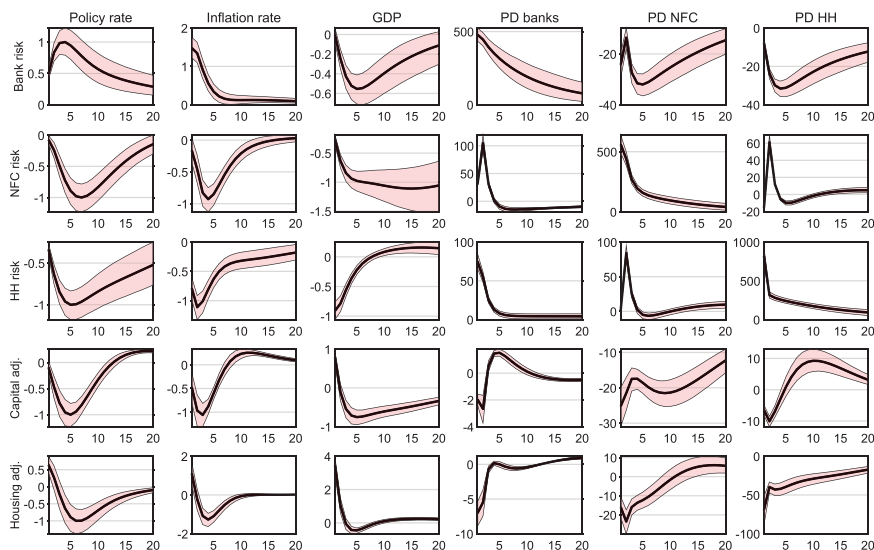
Figure 2. Impulse Response to Macroeconomic Shocks



Note: All variables are expressed as deviations from their steady state: in percentage points for the policy and inflation rates, in percent for GDP, and in basis points for default probabilities. All shocks are calibrated to generate a 1 percent increase in the policy rate (in absolute value). Ninety-percent confidence intervals are based on 2,000 simulated draws from the parameter distributions.

GDP, while inflationary government spending, preference, and total productivity shocks raise it. Across all cases, banks’ default probabilities respond less strongly than those of nonfinancial actors, a direct consequence of the assumption of perfect portfolio diversification. Among macroeconomic shocks, markup and monetary policy shocks are the most prone to generate financial stress. By contrast, productivity shocks display the greatest uncertainty regarding their macrofinancial impact, while uncertainty remains limited for the others.

Figure 3 presents the corresponding impulse response functions for sectoral shocks. These include three risk shocks—affecting non-financial corporations’ capital, impatient households’ housing, and banks’ portfolios—along with two MEI shocks for productive capital and housing. An increase in NFC and household risk induces

Figure 3. Impulse Response to Sectoral Shocks

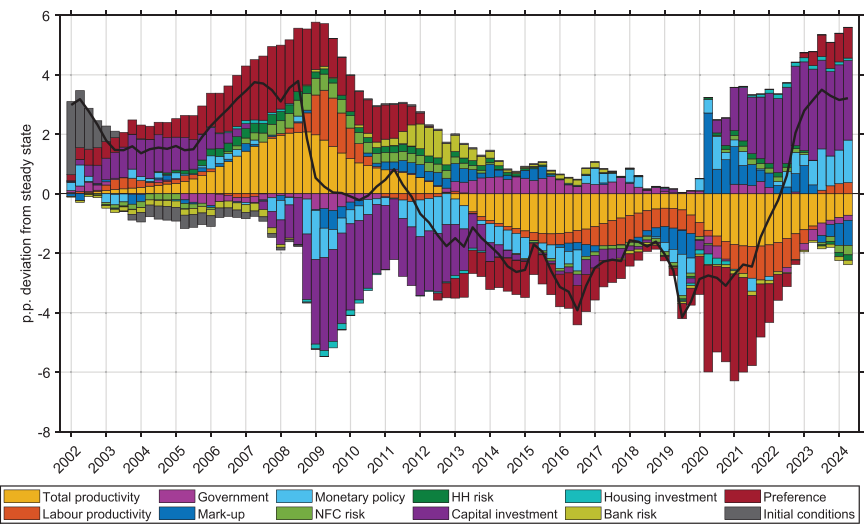
Note: All variables are expressed as deviations from their steady state: in percentage points for the policy and inflation rates, in percent for GDP, and in basis points for default probabilities. All shocks are calibrated to generate a 1 percent increase in the policy rate (in absolute value). Ninety-percent confidence intervals are based on 2,000 simulated draws from the parameter distributions.

deflationary pressures, whereas an increase in banks' risk produces inflationary pressures by fostering temporary overproduction. A reduction in capital or housing adjustment costs initially lowers GDP, as firms face greater demand, but subsequently raises it as investment adjusts more easily. This accelerates inflation and, in turn, prompts a tightening of the policy rate.

4.2 Historical Decomposition

These shocks enable one to capture the origins of the empirical variations in inflation, interest rate, and GDP. Figure 4 plots the decomposition of the policy rate, in deviation from its steady-state value. The rise of interest rates of 2006-07 is thus mainly explained by positive supply (total productivity shocks) and demand (positive preference shocks) factors. The sudden decrease of 2008 is mostly explained by a negative investment (MEI) shock, supplemented by

Figure 4. Decomposition of Short-Term Interest Rate

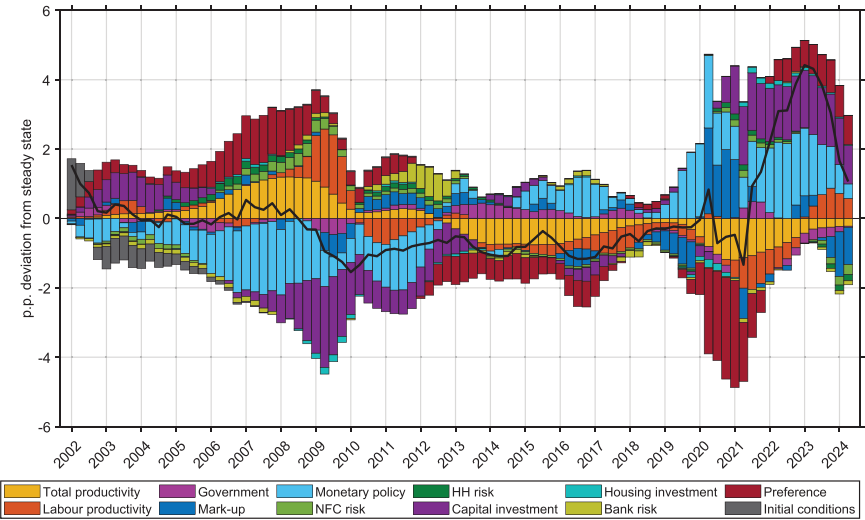


Note: Deviations from steady state in percentage points.

a particularly aggressive monetary policy. The low-rate environment that followed is then explained by a reversal in supply-side cycles, with strong and persistent negative productivity shocks, supplemented by negative preference shocks. At the beginning of this subperiod, decrease in firms' markup as well as government policy shock are the only inflationary pressures that contribute positively to GDP. Starting from 2016, total productivity begins to recover, but labor productivity and government spending firms' markup decrease, maintaining interest rates at a low level.

The COVID period set the seed of inflationary pressures, with sizable cost-push shocks countered by negative demand shocks. However, once the latter receded, inflationary pressures did not disappear. The markup shock left the stage for an increase in the marginal efficiency of investment, which contributed positively to GDP and inflation. This shock stands for sharp decrease in the relative price of tangible assets in 2021–23, as consumption prices rose more than investment prices, thus giving firms an incentive to invest and leading to overdemand in the final good market. This may capture, among others, the increase in banks' net interest rate margin and the increasing role of nonbank intermediaries during the rise

Figure 5. Decomposition of Year-on-Year Inflation Rate



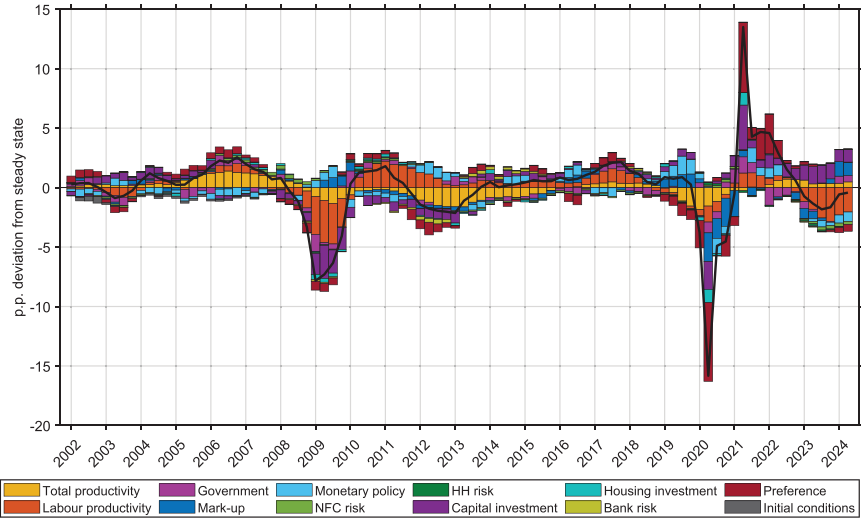
Note: Deviations from steady state in percentage points.

in interest rates, consistently with the deposit channel of monetary policy (Drechsler, Savov, and Schnabl 2017). As this shock is persistent and expansionary, it warrants a stronger reaction of monetary policy compared with a cost-push shock, which is short-lived and recessionary.

As mentioned by Licchetta and Meyermans (2022), gross fixed capital has rebounded at a significantly faster rate after COVID compared with the aftermath of the financial crisis. Public investment was more important in the post-COVID period than in the post-GFC time to support the recovery. Investment also benefited from a real cost of financing which remains relatively low at the end of our estimation period. The important contribution of the shock to the cost of investment reflects this relatively strong recovery of real investment for the period.

The decomposition of year-on-year inflation is plotted in Figure 5 and shows notably the action of monetary policy on inflation. While monetary policy appears relatively tight until 2012, expansionary shocks are the norm between 2014 and 2019 to fight below average inflation. However, once deflationary pressures disappeared after COVID, past accommodative monetary policy continued to

Figure 6. Decomposition of Year-on-Year GDP Growth Rate



Note: Deviations from steady state in percentage points.

push inflation up, despite the exogenous increase in the policy stance exemplified in Figure 4. Figure 6 plots the decomposition of year-on-year GDP growth, and shows that these shocks brought down GDP growth below its average in 2023, starting from the strong post-COVID context driven by positive demand shocks.

This decomposition thus indicates that the strong rise in interest rates of 2021–23 finds its roots in mostly supply-side shocks, complemented by a positive demand shock and the lagged effect of accommodative monetary policy, with an overall ambiguous effect on GDP. We recover this combination of shocks to assess how the level of capital requirements affected their transmission in the euro area.

5. Capital Requirements and the Transmission of Monetary Tightening

5.1 *Basel III and Monetary Tightening*

To what extent do capital requirements affect the transmission of monetary tightening? European prudential authorities strengthened

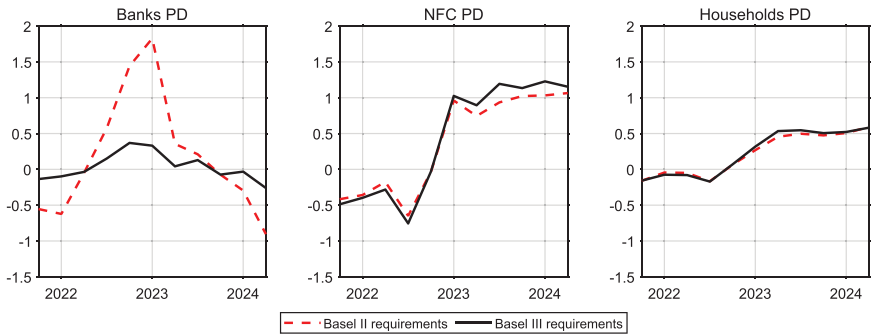
capital regulation during the 2010s, transitioning from Basel II to Basel III. In particular, the minimum 8 percent risk-weighted assets (RWA) capital requirement under Basel II was supplemented by a 2.5 percentage point capital conservation buffer (CCoB). This new prudential environment calls for a reassessment of how monetary tightening interacts with financial stability. On the one hand, higher buffers increase banks' resilience to adverse shocks. On the other hand, by constraining credit supply in times of crisis, they may amplify those shocks. This section quantifies the impact of higher capital requirements, by 2.5 percentage points, on the transmission of the 2021–23 inflation surge and monetary tightening in the euro area, by constructing a counterfactual scenario in which capital requirements remained at 8 percent of RWA.

To assess the state of the economy, we define both an *observed* and a *counterfactual* scenario over the simulation period (2021:Q3–2024:Q2):

- The *observed scenario* is simulated using the calibrated and estimated parameters for the full sample, together with the shocks recovered for the 2021:Q3–2024:Q2 period. In this case, the capital requirement ratio is set at 10.5 percent. We assume that, in the initial steady state, the structural increase in capital requirements from Basel II (8 percent) to Basel III (10.5 percent) has been fully transmitted. Given that the reform was announced in 2012 and completed by 2019, this assumption appears reasonable.
- The *counterfactual scenario* differs only by the level of the capital requirement ratio, which is set at 8 percent, consistent with Basel II. Using the structural model, we isolate the effect of this single parameter change while holding constant the shocks estimated during the post-COVID monetary tightening episode.

Figure 7 reports the probability of default of banks under two scenarios: (i) the sum of all estimated shocks between 2021:Q3 and 2024:Q2 under Basel III regulation (black line), and (ii) the same shocks under Basel II regulation (dashed red line). Higher capital requirements under Basel III dampen the volatility of banks' probability of default. Under Basel II, the inflationary surge and

**Figure 7. Probabilities of Default:
Basel III vs. Basel II Capital Requirements**



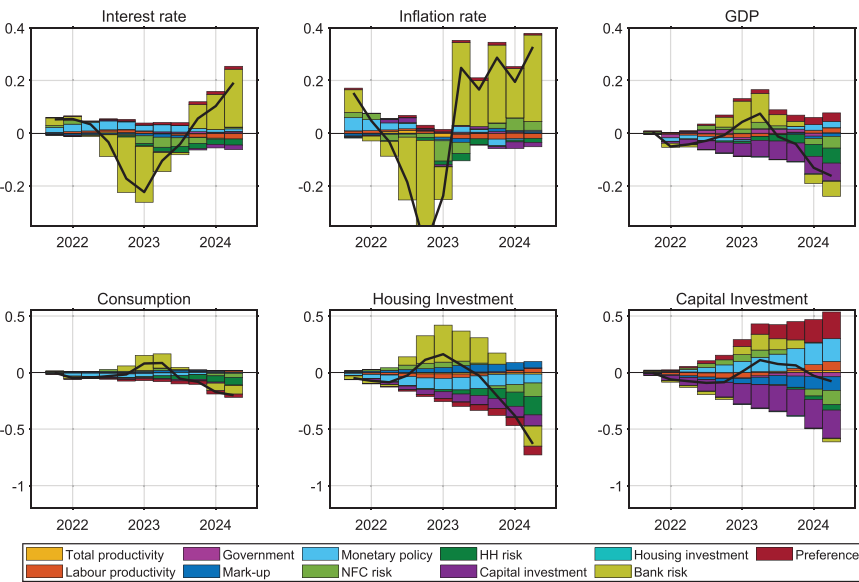
Note: The y-axis represents deviations from steady state in percentage point. The x-axis represents quarters, the first one being 2021:Q4.

subsequent monetary tightening would have raised the deviation of banks’ probability of default from its long-term trend by about 100 basis points.

These results indicate that capital requirements do not need to be explicitly countercyclical to be effective. In an inflationary environment, they act as automatic stabilizers by curbing the amplitude of both expansionary and contractionary shocks. In Figure 8 and Figure 9, the black line shows the difference in macroeconomic and financial variables under Basel III relative to Basel II, while the colored bars display the contribution of each structural shock to this difference. The contribution of the bank risk shock dominates the divergence between the two regimes. As shown in Figure 4, this shock exerts a negative contribution to the evolution of the policy rate over the period considered.

Figure 8 further shows that, with Basel III, the policy rate was higher during the immediate post-COVID expansion and lower in early 2023 than it would have been under Basel II. During the post-COVID phase, higher capital requirements increased inflation, as aggregate supply was restrained by tighter credit supply and lower net worth for savers, weighing on GDP. Conversely, at the beginning of 2023, aggregate supply contracted less because credit supply remained resilient, implying higher supply relative to demand, thereby reducing inflation and the policy rate. Hence, higher

Figure 8. Impact of Basel III from 2021:Q3 to 2024:Q2—Macroeconomic Variables

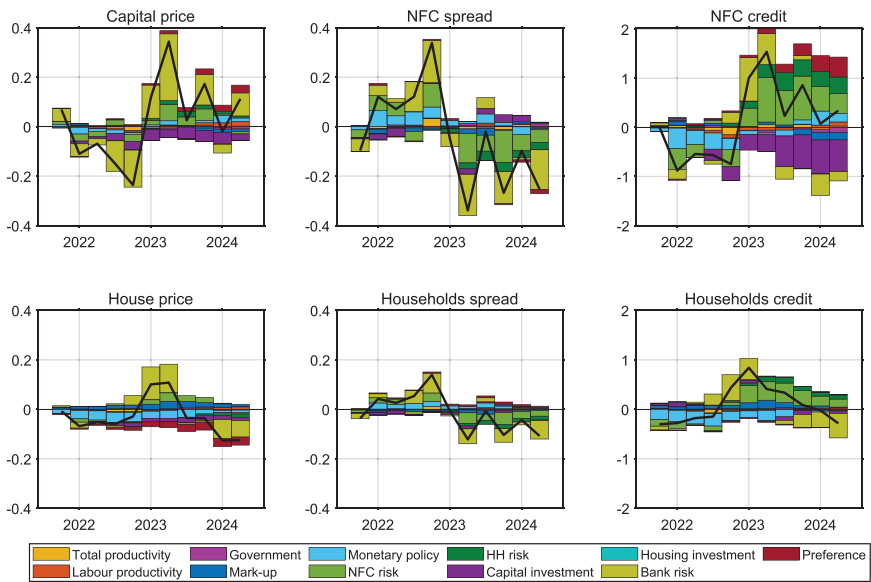


Note: The black line shows the difference between the actual and the counterfactual paths. The colored bars indicate the contribution of each shock to this overall effect. The x-axis reports quarters, with the first corresponding to 2021:Q4. All variables are expressed as deviations from their steady state in percent, except for the interest rate and inflation rate, which are in percentage points.

capital requirements smooth the cycle by moderating expansions and supporting credit during downturns.

The bank risk shock is the main contributor to the difference between the Basel II and Basel III scenarios, even though this shock remains relatively small during the simulation period. Indeed, ECB Financial Stability Reviews (European Central Bank 2023a, 2023b) describe banking risk in the euro area as contained, especially when compared with other advanced economies. This simulation suggests that the automatic stabilization role of capital requirements could be even stronger in a counterfactual scenario with larger bank risk shocks, underscoring the importance of precisely identifying the composition of shocks. For instance, the bank risk shock is almost negligible in the post-COVID period in Figure 4, while it was relatively

Figure 9. Impact of Basel III from 2021:Q3 to 2024:Q2—Financial Variables



Note: The black line shows the difference between the actual and the counterfactual paths. The colored bars indicate the contribution of each shock to this overall effect. The x-axis reports quarters, with the first corresponding to 2021:Q4. All variables are expressed as deviations from their steady state in percent, except for the interest rate and inflation rate, which are in percentage points.

large between 2010 and 2014. While the impact on inflation and the policy rate is mainly driven by the bank risk shock, capital requirements contributed negatively to the transmission of the MEI shock by constraining credit supply and dampening its expansionary effects. Toward the end of the period, higher capital requirements shielded the economy from the adverse effects of bank risk shocks on consumption and investment. They also appear to mitigate the negative impact of the markup shock on consumption and to stimulate capital accumulation by moderating the rise in the policy rate.

The same automatic-stabilizer mechanism applies to asset prices. Under Basel III, banks curtailed lending during the expansion but expanded it during the downturn. During the recovery phase, to meet capital requirements, banks adjusted their lending behavior

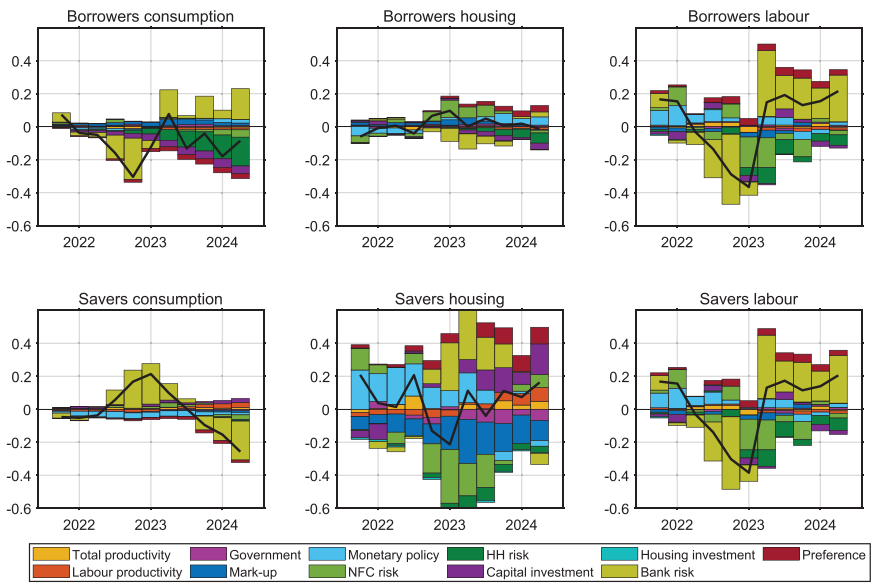
by reducing loan volumes and raising lending spreads to rebuild retained earnings, thereby sharing the cost of regulation with borrowers. Figure 9 shows that higher capital requirements implied lower capital and housing prices during the post-COVID expansion, but relatively higher asset prices when bank risk increased toward the end of the period, reflecting tighter credit supply during the recovery and looser supply during the riskier phase.

Household and NFC risk was only slightly affected by higher capital requirements. While the tighter Basel III requirements proved costly during the post-COVID recovery, they reduced households' and NFCs' leverage, thereby limiting their risk exposure over this period. Conversely, by supporting credit supply in subsequent phases of expansion, higher capital requirements implied a slightly higher probability of default for borrowers.

The quantitative effects were larger for firms than for households. During the 2022 expansion, the impact on credit supply and lending spreads was stronger for NFCs than for households because risk weights were higher for NFC exposures. In addition, the investment and firm risk shocks, which played a key role in the unfolding of the monetary tightening, directly affected firms rather than households. Yet, housing investment declined more than business investment. Investment was influenced not only by credit supply but also by demand: as patient households bore the cost of bank losses, they reduced their demand for housing. When borrowing costs rose, households cut housing investment relatively more than firms reduced their NFC investment. This effect was further amplified by another channel: capital requirements also influenced savers' demand for assets through their effect on banks' profitability.

In conclusion, the abrupt rise in interest rates could have had large macroeconomic effects through risk shocks. As discussed by Hoffmann et al. (2019), the transmission of a monetary tightening may operate through the balance sheet channel, either via banks' net worth or borrowers' leverage, with implications for consumption and investment. The potential materialization of these risks depends on which agents bear the interest rate risk. In particular, leveraged actors could have faced tighter borrowing constraints if they were imperfectly hedged against interest rate risk or held concentrated portfolios. However, our historical decompositions attribute a relatively small role to such shocks in 2023, as capital requirements

Figure 10. Impact of Basel III from 2021:Q3 to 2024:Q2—Distributive Effects



Note: The black line shows the difference between the actual and the counterfactual paths. The colored bars indicate the contribution of each shock to this overall effect. The x-axis reports quarters, with the first corresponding to 2021:Q4. All variables are expressed as deviations from their steady state in percent.

were sufficiently high to absorb a moderate risk shock. The benefits of higher capital requirements would have been even larger in the case of a stronger bank risk shock, similar to that estimated for 2011, for instance.

5.2 *Redistributive Effects of Capital Requirements*

As suggested in the previous subsection, the mitigating impact of capital requirements operated through both savers’ net worth and borrowers’ credit constraints. This implies that capital requirements had pronounced heterogeneous effects across households. Figure 10 displays the differential evolution of each household’s choice variables under Basel III and Basel II. For instance, under Basel III, savers’ consumption was 0.2 percentage points higher in 2023:Q1. More generally, higher capital requirements smoothed

savers' consumption, a result largely driven by the bank risk shock. When bank risk was low, capital requirements weighed on banks' profitability, reducing savers' income flows. Conversely, when bank risk materialized, the opposite occurred, boosting savers' consumption. In contrast, borrowers' consumption was temporarily constrained by capital requirements when the bank risk shock hit: higher capital buffers protected banks' owners rather than their borrowers. However, the contribution of capital requirements turned positive by the end of the period, once bank risk subsided. By strengthening banks' balance sheets, capital requirements ensured a faster recovery. Deposit insurance, financed by patient households, already helped smooth the impact of shocks, and was complemented by capital requirements, which reduced households' overall losses.

Regarding housing, capital requirements helped preserve borrowers' access to credit, with housing investment growth 0.1 percentage points higher in 2023:Q1. Interestingly, the contribution of nearly every shock was of opposite sign for savers and borrowers. For example, while capital requirements amplified the negative impact of monetary policy shocks on savers' housing, they mitigated it for borrowers. However, the quantitative effects of capital requirements on housing investment were roughly five times larger for savers than for borrowers, highlighting that their macroeconomic influence mainly operated through savers' net worth.

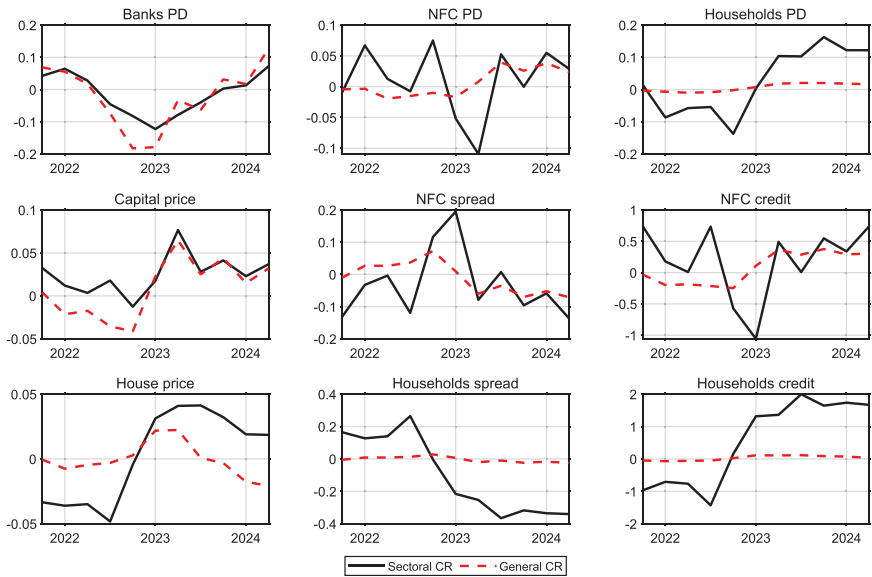
Finally, capital requirements had no economically significant heterogeneous effects on hours worked across households. This reflects the low short-run wealth effects in the model, implying that labor supply plays a negligible role in the adjustment process. As both households face identical labor demand, capital requirements generate no heterogeneity in employment outcomes.

These heterogeneous and redistributive effects of capital requirements help explain cross-country differences in macroprudential stances within the euro area. Countries with a larger share of borrowers have less incentive to raise capital requirements above the Basel III minimum.

5.3 Macprudential Policies and Monetary Tightening

In recent years, several European macroprudential authorities have increased risk weights on mortgage exposures or introduced sectoral

Figure 11. Impact of Macroprudential Policies—Financial Variables



Note: All lines show the difference between the actual and the counterfactual paths. The x-axis reports quarters, with the first corresponding to 2021:Q4. All variables are expressed as deviations from their steady state in percent, except for the interest rate and the inflation rate, which are in percentage points.

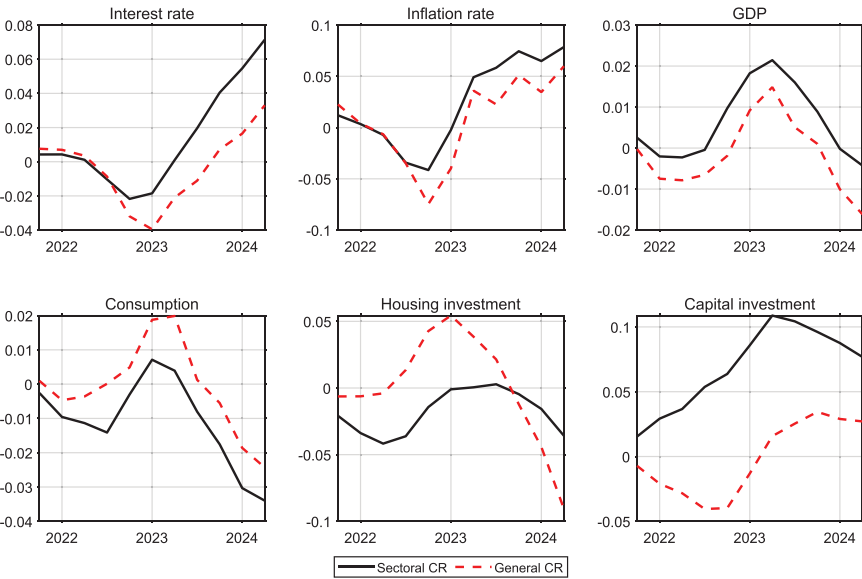
buffers on housing loans.⁹ In this section, we analyze the macroeconomic and financial effects of such policies. We adopt a conservative scenario in which banks' equity-to-risk-weight ratios are kept constant at their pre-crisis level throughout the entire episode. While banks are formally allowed to draw down these buffers in times of stress, past experience suggests they rarely do so, due to market stigma and the expectation that buffers will need to be rebuilt later.¹⁰

Figure 11 displays the response of banks' probabilities of default under two alternative policy scenarios: (i) *sectoral capital requirements*, where total capital requirements remain at their Basel III level (10.5 percent) and the authority raises the mortgage risk weight

⁹See ESRB risk-weight measures and ESRB systemic risk buffer measures

¹⁰See ECB Macroprudential Bulletin.

Figure 12. Impact of Macroprudential Policies—Macroeconomic Variables



Note: All lines show the difference between the actual and the counterfactual paths. The x-axis reports quarters, with the first corresponding to 2021:Q4. All variables are expressed as deviations from their steady state in percent, except for spreads and probabilities of default, which are in percentage points.

from 35 percent to 100 percent; and (ii) *general capital requirements*, where the authority instead increases the overall capital ratio from 10.5 percent to 11.5 percent. Both policies entail short-run costs but generate medium-term gains in terms of bank stability and GDP.

Sectoral capital requirements turn out to be more efficient at stabilizing GDP (see Figure 12). By offsetting distortions introduced by differential risk weights, they induce a portfolio reallocation toward NFC lending. In particular, the higher mortgage risk weights generate a substitution effect whereby banks redirect part of their lending from households to NFCs, supporting productive investment. Overall, this policy reduces the GDP cost of tighter capital requirements during the post-COVID investment catch-up.

Under the alternative scenario (ii) with higher general capital requirements (red dashed line), the cost–benefit balance is less

favorable. This outcome reflects the contribution of capital requirements to capital investment dynamics in the wake of the post-COVID recovery. Compared with a sectoral tightening, the general capital requirement has a smaller negative effect on consumption and housing investment but dampens NFC investment more severely. This result illustrates how higher capital requirements can affect housing and productive investment asymmetrically, given the composition of estimated shocks. Nonetheless, this option remains attractive in terms of financial stability, as it delivers lower volatility in financial variables and could therefore be preferred by households, whose consumption and housing investment are less affected.

6. Conclusion

This paper develops a New Keynesian model with a rich set of financial frictions and structural shocks to assess the role of capital requirements in business cycle dynamics, specifically during the 2021–24 monetary tightening episode. We show that while banks' capital requirements limited post-COVID growth, they successfully prevented the materialization of risks when the ECB raised short-term interest rates. By smoothing the response of banks' net worth to macroeconomic conditions, capital requirements acted as automatic stabilizers and reduced the likelihood of a hard landing during the tightening cycle. Overall, they proved complementary to monetary policy. Hence, even in a period of unprecedented monetary tightening, capital requirements do not need to be countercyclical to be effective. Their impact, however, is heterogeneous across savers and borrowers, and thus across euro-area member states, leaving room for cross-country heterogeneity in the design of macroprudential policies.

That said, while capital requirements helped prevent a hard landing by shifting interest rate risk toward banks, they are not sufficient on their own to ensure financial stability. By enhancing banks' resilience, such policies may, up to a point, encourage greater indebtedness among private agents. Other macroprudential tools, such as borrower-based measures, can therefore play a complementary role by ensuring sound financing conditions and appropriate leverage levels before the onset of a monetary tightening episode.

Appendix. Equilibrium Conditions

To simplify the exposition, we redefine a number of variables:

$$\begin{aligned} b_t^e &\leftarrow m^e b_t^e, \\ k_t^e &\leftarrow m^e k_t^e, \\ n_t^e &\leftarrow m^e n_t^e, \\ n_t^b &\leftarrow m^b n_t^b. \end{aligned}$$

We let Φ denote the cumulative distribution function (CDF) of the $\mathcal{N}(0, 1)$ distribution. The threshold value of idiosyncratic shock above which an entity defaults is denoted $\bar{\omega}_t^j$, where $j \in \{e, i, F, M\}$.

We also express capital requirements as a leverage ratio combining the broad base capital requirement as well as sector-specific risk weights:

$$f_t^F = \phi_t \gamma_t^F$$

$$f_t^M = \phi_t \gamma_t^M.$$

Intermediary and final good producer:

$$w_t = s_t e^{\zeta_{a,t}} (1 - \alpha) e^{\zeta_{z,t}} \left(\frac{k_{t-1}}{e^{\zeta_{z,t}} \ell_t} \right)^\alpha \quad (\text{A.1})$$

$$r_t^K = s_t e^{\zeta_{a,t}} \alpha \left(\frac{k_{t-1}}{e^{\zeta_{z,t}} \ell_t} \right)^{\alpha-1} \quad (\text{A.2})$$

$$\bar{P}_t^* = \frac{K_{1,t}}{K_{2,t}} \quad (\text{A.3})$$

$$K_{1,t} = \mu_t y_t s_t + (\beta^p \xi) \mathbb{E}_t \left[\frac{\lambda_{t+1}^p}{\lambda_t^p} \left(\frac{(\Pi_*)^{1-\iota} (\Pi_t)^\iota}{\Pi_{t+1}} \right)^{\frac{\mu_t}{1-\mu_t}} K_{1,t+1} \right] \quad (\text{A.4})$$

$$K_{2,t} = y_t + (\beta^p \xi) \mathbb{E}_t \left[\frac{\lambda_{t+1}^p}{\lambda_t^p} \left(\frac{(\Pi_*)^{1-\iota} (\Pi_t)^\iota}{\Pi_{t+1}} \right)^{\frac{1}{1-\mu_t}} K_{2,t+1} \right] \quad (\text{A.5})$$

$$1 = (1 - \xi) (\bar{P}_t^*)^{\frac{1}{1-\mu_t}} + \xi \left[\frac{(\Pi_*)^{1-\iota} (\Pi_{t-1})^\iota}{\Pi_t} \right]^{\frac{1}{1-\mu_t}} \quad (\text{A.6})$$

$$\Upsilon_t^{\frac{\mu_t}{1-\mu_t}} y_t = e^{\zeta_{a,t}} (k_{t-1})^\alpha (e^{\zeta_{z,t}} \ell_t)^{1-\alpha} \quad (\text{A.7})$$

$$\Upsilon_t^{\frac{\mu_t}{1-\mu_t}} = (1-\xi)(\bar{P}_t^*)^{\frac{\mu_t}{1-\mu_t}} + \xi \left(\frac{(\Pi_*)^{1-\iota} (\Pi_{t-1})^\iota}{\Pi_t} \right)^{\frac{\mu_t}{1-\mu_t}} \Upsilon_{t-1}^{\frac{\mu_t}{1-\mu_t}} \quad (\text{A.8})$$

Capital production:

$$\begin{aligned} q_t^K &= e^{\zeta_{i_K,t}} \left(1 + \frac{\psi_K}{2} \left(\frac{i_t^K}{i_{t-1}^K} - 1 \right)^2 + \psi_K \left(\frac{i_t^K}{i_{t-1}^K} - 1 \right) \frac{i_t^K}{i_{t-1}^K} \right) \\ &\quad - \beta^p \mathbb{E}_t \left[e^{\zeta_{i_K,t+1}} \frac{\lambda_{t+1}^p}{\lambda_t^p} \psi_K \left(\frac{i_{t+1}^K}{i_t^K} - 1 \right) \left(\frac{i_{t+1}^K}{i_t^K} \right)^2 \right] \end{aligned} \quad (\text{A.9})$$

$$k_t = (1 - \delta^K) k_{t-1} + i_t^K \quad (\text{A.10})$$

Housing production:

$$\begin{aligned} q_t^H &= e^{\zeta_{i_H,t}} \left(1 + \frac{\psi_H}{2} \left(\frac{i_t^H}{i_{t-1}^H} - 1 \right)^2 + \psi_H \left(\frac{i_t^H}{i_{t-1}^H} - 1 \right) \frac{i_t^H}{i_{t-1}^H} \right) \\ &\quad - \beta^p \mathbb{E}_t \left[e^{\zeta_{i_H,t+1}} \frac{\lambda_{t+1}^p}{\lambda_t^p} \psi_H \left(\frac{i_{t+1}^H}{i_t^H} - 1 \right) \left(\frac{i_{t+1}^H}{i_t^H} \right)^2 \right] \end{aligned} \quad (\text{A.11})$$

$$h_t = (1 - \delta^H) h_{t-1} + i_t^H \quad (\text{A.12})$$

Patient households:

$$\lambda_t^p = \frac{e^{\zeta_{c,t}}}{c_t^p - \psi c_{t-1}^p} \quad (\text{A.13})$$

$$e^{\zeta_{c,t}} \varphi^p J_t^p (\ell_t^p)^\eta = w_t \quad (\text{A.14})$$

$$J_t^p = (J_{t-1}^p)^{1-\zeta_J} [(c_t^p - \psi c_{t-1}^p)]^{\zeta_J} \quad (\text{A.15})$$

$$\lambda_t^p = \beta^p \mathbb{E}_t \left[\lambda_{t+1}^p \frac{\tilde{R}_{t+1}}{\Pi_{t+1}} \right] \quad (\text{A.16})$$

$$\lambda_t^p (q_t^K + \xi_s m^p k_t^p) = \beta^p \mathbb{E}_t [\lambda_{t+1}^p [r_{t+1}^K + (1 - \delta^K) q_{t+1}^K]] \quad (\text{A.17})$$

$$\lambda_t^p q_t^H = e^{\zeta_{c,t}} v^p \frac{1}{h_t^p} + \beta^p (1 - \delta^H) \lambda_{t+1}^p q_{t+1}^H \quad (\text{A.18})$$

Bankers and banks:

$$v_t^b = \beta^p \mathbb{E}_t \left[\frac{\lambda_{t+1}^p}{\lambda_t^p} (1 - \theta^b + \theta^b v_{t+1}^b) \frac{Z_{t+1}^M}{\Pi_{t+1}} \right] \quad (\text{A.19})$$

$$v_t^b = \beta^p \mathbb{E}_t \left[\frac{\lambda_{t+1}^p}{\lambda_t^p} (1 - \theta^b + \theta^b v_{t+1}^b) \frac{Z_{t+1}^F}{\Pi_{t+1}} \right] \quad (\text{A.20})$$

$$\bar{\omega}_t^M = (1 - f_{t-1}^M) \frac{R_{t-1}}{R_t^M} \quad (\text{A.21})$$

$$\bar{\omega}_t^F = (1 - f_{t-1}^F) \frac{R_{t-1}}{R_t^F} \quad (\text{A.22})$$

$$Z_t^M = \frac{(1 - \Gamma_t^M) R_t^M}{f_{t-1}^M} \quad (\text{A.23})$$

$$Z_t^F = \frac{(1 - \Gamma_t^F) R_t^F}{f_{t-1}^F} \quad (\text{A.24})$$

$$n_t^b = [\theta^b + \chi^b (1 - \theta^b)] \left(\frac{Z_t^M}{\Pi_t} f_{t-1}^M m^i b_{t-1}^i + \frac{Z_t^F}{\Pi_t} f_{t-1}^F b_{t-1}^e \right) \quad (\text{A.25})$$

$$n_t^b = f_t^M m^i b_t^i + f_t^F b_t^e \quad (\text{A.26})$$

Entrepreneurs and investment firms:

$$v_t^e = \mathbb{E}_t \left[\beta^p \frac{\lambda_{t+1}^p}{\lambda_t^p} (1 - \theta^e + \theta^e v_{t+1}^e) \frac{Z_{t+1}^e}{\Pi_{t+1}} \right] \quad (\text{A.27})$$

$$x_t^e = \frac{R_t^e b_t^e}{q_t^K k_t^e} \quad (\text{A.28})$$

$$\bar{\omega}_t^e = \frac{x_{t-1}^e}{R_t^K} \quad (\text{A.29})$$

$$R_t^K = \Pi_t \frac{r_t^K + (1 - \delta)q_t^K}{q_{t-1}^K} \quad (\text{A.30})$$

$$R_t^F = (\Gamma_t^e - \mu^e G_t^e) R_t^K \frac{q_{t-1}^K k_{t-1}^e}{b_{t-1}^e} \quad (\text{A.31})$$

$$Z_t^e = (1 - \Gamma_t^e) R_t^K \frac{q_{t-1}^K k_{t-1}^e}{n_{t-1}^e} \quad (\text{A.32})$$

$$\mathbb{E}_t \left[\beta^p \frac{\lambda_{t+1}^p}{\lambda_t^p} \frac{1}{\Pi_{t+1}} \left((1 - \theta^e + \theta^e v_{t+1}^e) \Gamma_{t+1}^{e'} \right. \right. \\ \left. \left. - \xi_t^e (1 - \theta^b + \theta^b v_{t+1}^b) (1 - \Gamma_{t+1}^F) (\Gamma_{t+1}^{e'} - \mu^e G_{t+1}^{e'}) \right) \right] = 0 \quad (\text{A.33})$$

$$\mathbb{E}_t \left[\beta^p \frac{\lambda_{t+1}^p}{\lambda_t^p} \left((1 - \theta^e + \theta^e v_{t+1}^e) (1 - \Gamma_{t+1}^e) \right. \right. \\ \left. \left. + \xi_t^e (1 - \theta^b + \theta^b v_{t+1}^b) (1 - \Gamma_{t+1}^F) (\Gamma_{t+1}^e - \mu^e G_{t+1}^e) \right) \frac{R_{t+1}^K}{\Pi_{t+1}} \right] - \xi_t^e f_t^F v_t^b = 0 \quad (\text{A.34})$$

$$n_t^e = [\theta^e + \chi^e (1 - \theta^e)] (1 - \Gamma_t^e) (r_t^K + (1 - \delta)q_t^K) k_{t-1}^e, \quad (\text{A.35})$$

$$n_t^e + b_t^e = q_t^K k_t^e \quad (\text{A.36})$$

Impatient households:

$$R_t^M = (\Gamma_t^i - \mu^i G_t^i) \frac{R_t^H q_{t-1}^H h_{t-1}^i}{b_{t-1}^i} \quad (\text{A.37})$$

$$c_t^i + q_t^H h_t^i = w_t \ell_t^i + b_t^i + (1 - \Gamma_t^i) (1 - \delta^H) q_t^H h_{t-1}^i \quad (\text{A.38})$$

$$R_t^H = \Pi_t \frac{(1 - \delta^H) q_t^H}{q_{t-1}^H} \quad (\text{A.39})$$

$$x_t^i = \frac{R_t^i b_t^i}{q_t^H h_t^i} \quad (\text{A.40})$$

$$\bar{\omega}_t^i = \frac{x_{t-1}^i}{R_t^H} \quad (\text{A.41})$$

$$\lambda_t^i = \frac{e^{\zeta_{c,t}}}{c_t^i - \psi c_{t-1}^i} \quad (\text{A.42})$$

$$w_t = e^{\zeta_{c,t}} \varphi^i J_t^i (\ell_t^i)^\eta \quad (\text{A.43})$$

$$J_t^i = (J_{t-1}^i)^{1-\zeta_J} [(c_t^i - \psi c_{t-1}^i)]^{\zeta_J} \quad (\text{A.44})$$

$$\begin{aligned} \beta^i \mathbb{E}_t \left[\lambda_{t+1}^i \frac{\Gamma_{t+1}^{i'}}{\Pi_{t+1}} \right] &= \xi_t^i \beta^p \mathbb{E}_t \left[\frac{\lambda_{t+1}^p}{\lambda_t^p} (1 - \theta^b + \theta^b v_{t+s+1}^b) \right. \\ &\quad \left. \times (1 - \Gamma_{t+1}^M) \frac{\Gamma_{t+1}^{i'} - \mu^i G_{t+1}^{i'}}{\Pi_{t+1}} \right] \end{aligned} \quad (\text{A.45})$$

$$\lambda_t^i = \xi_t^i f_t^M v_t^b \quad (\text{A.46})$$

$$\begin{aligned} \lambda_t^i q_t^H &= e^{\zeta_{c,t}} v^i \frac{1}{h_t^i} + \beta^i \mathbb{E}_t [\lambda_{t+1}^i (1 - \Gamma_{t+1}^i) (1 - \delta^H) q_{t+1}^H] \\ &\quad + \beta^p \xi_t^i \mathbb{E}_t \left[\frac{\lambda_{t+1}^p}{\lambda_t^p} (1 - \theta^b + \theta^b v_{t+s+1}^b) (1 - \Gamma_{t+1}^M) \right. \\ &\quad \left. \times (\Gamma_{t+1}^i - \mu^i G_{t+1}^i) (1 - \delta^H) q_{t+1}^H \right] \end{aligned} \quad (\text{A.47})$$

Market clearing:

$$k_t = k_t^e + m^p k_t^p \quad (\text{A.48})$$

$$h_t = m^p h_t^p + m^i h_t^i \quad (\text{A.49})$$

$$c_t = m^p c_t^p + m^i c_t^i \quad (\text{A.50})$$

$$\ell_t = m^p \ell_t^p + m^i \ell_t^i \quad (\text{A.51})$$

$$\begin{aligned} y_t &= c_t + e^{\zeta_{i_K,t}} \left(1 + \frac{\psi_K}{2} \left(\frac{i_t^K}{i_{t-1}^K} - 1 \right)^2 \right) i_t^K \\ &\quad + e^{\zeta_{i_H,t}} \left(1 + \frac{\psi_H}{2} \left(\frac{i_t^H}{i_{t-1}^H} - 1 \right)^2 \right) i_t^H + s_g y_* e^{\zeta_{g,t}} \\ &\quad + \mu^i G_t^i (1 - \delta^H) q_t^H m^i h_{t-1}^i + \mu^e G_t^e (r_t^K + (1 - \delta) q_t^K) k_{t-1}^e \\ &\quad + \mu^M G_t^M \frac{R_t^M}{\Pi_t} m^i b_{t-1}^i + \mu^F G_t^F \frac{R_t^F}{\Pi_t} b_{t-1}^e + \frac{\xi_s}{2} (m^p k_t^p)^2 \end{aligned} \quad (\text{A.52})$$

Deposit insurance:

$$\tilde{R}_t = R_{t-1} - (1 - \kappa)\Omega_t \quad (\text{A.53})$$

$$\begin{aligned} \Omega_t d_{t-1} = & (\bar{\omega}_t^M - \Gamma_t^M + \mu^M G_t^M) R_t^M m^i b_{t-1}^i \\ & + (\bar{\omega}_t^F - \Gamma_t^F + \mu^F G_t^F) R_t^F b_{t-1}^e \end{aligned} \quad (\text{A.54})$$

$$n_t^b + d_t = b_t^e + m^i b_t^i \quad (\text{A.55})$$

Monetary authority:

$$\begin{aligned} \log \left(\frac{R_t}{R_*} \right) = & \rho_R \log \left(\frac{R_{t-1}}{R_*} \right) \\ & + (1 - \rho_R) \left[a_\Pi \log \left(\frac{\Pi_t}{\Pi_*} \right) + a_y \log \left(\frac{GDP_t}{GDP_{t-1}} \right) \right] + \zeta_{R,t} \end{aligned} \quad (\text{A.56})$$

Exogenous processes:

$$\zeta_{a,t} = \rho_a \zeta_{a,t-1} + \frac{\sigma_a}{100} \epsilon_{a,t}, \quad (\text{A.57})$$

$$\zeta_{z,t} = \rho_z \zeta_{z,t-1} + \frac{\sigma_z}{100} \epsilon_{z,t}, \quad (\text{A.58})$$

$$\zeta_{i_K,t} = \rho_{i_K} \zeta_{i_K,t-1} + \frac{\sigma_{i_K}}{100} \epsilon_{i_K,t}, \quad (\text{A.59})$$

$$\zeta_{i_H,t} = \rho_{i_H} \zeta_{i_H,t-1} + \frac{\sigma_{i_H}}{100} \epsilon_{i_H,t}, \quad (\text{A.60})$$

$$\zeta_{e,t} = \rho_e \zeta_{e,t-1} + \frac{\sigma_e}{100} \epsilon_{e,t}, \quad (\text{A.61})$$

$$\zeta_{i,t} = \rho_i \zeta_{i,t-1} + \frac{\sigma_i}{100} \epsilon_{i,t}, \quad (\text{A.62})$$

$$\zeta_{B,t} = \rho_B \zeta_{B,t-1} + \frac{\sigma_B}{100} \epsilon_{B,t}, \quad (\text{A.63})$$

$$\zeta_{c,t} = \rho_c \zeta_{c,t-1} + \frac{\sigma_c}{100} \epsilon_{c,t}, \quad (\text{A.64})$$

$$\zeta_{\mu,t} = \rho_\mu \zeta_{\mu,t-1} + \frac{\sigma_\mu}{100} \epsilon_{\mu,t}, \quad (\text{A.65})$$

$$\zeta_{g,t} = \rho_g \zeta_{g,t-1} + \frac{\sigma_g}{100} \epsilon_{g,t}, \quad (\text{A.66})$$

$$\zeta_{R,t} = \frac{\sigma_R}{100} \epsilon_{R,t}, \quad (\text{A.67})$$

Auxiliary variables:

$$\Gamma_t^F = G_t^F + \bar{\omega}_t^F \left[1 - \Phi \left(\frac{\log(\bar{\omega}_t^F) + \frac{1}{2}(\sigma_F e^{\zeta_{B,t}})^2}{\sigma_F e^{\zeta_{B,t}}} \right) \right] \quad (\text{A.68})$$

$$G_t^F = \Phi \left(\frac{\log(\bar{\omega}_t^F) - \frac{1}{2}(\sigma_F e^{\zeta_{B,t}})^2}{\sigma_F e^{\zeta_{B,t}}} \right) \quad (\text{A.69})$$

$$\Gamma_t^M = G_t^M + \bar{\omega}_t^M \left[1 - \Phi \left(\frac{\log(\bar{\omega}_t^M) + \frac{1}{2}(\sigma_M e^{\zeta_{B,t}})^2}{\sigma_M e^{\zeta_{B,t}}} \right) \right] \quad (\text{A.70})$$

$$G_t^M = \Phi \left(\frac{\log(\bar{\omega}_t^M) - \frac{1}{2}(\sigma_M e^{\zeta_{B,t}})^2}{\sigma_M e^{\zeta_{B,t}}} \right) \quad (\text{A.71})$$

$$\Gamma_t^e = G_t^e + \bar{\omega}_t^e \left[1 - \Phi \left(\frac{\log(\bar{\omega}_t^e) + \frac{1}{2}(\sigma_e e^{\zeta_{e,t}})^2}{\sigma_e e^{\zeta_{e,t}}} \right) \right] \quad (\text{A.72})$$

$$G_t^e = \Phi \left(\frac{\log(\bar{\omega}_t^e) - \frac{1}{2}(\sigma_e e^{\zeta_{e,t}})^2}{\sigma_e e^{\zeta_{e,t}}} \right) \quad (\text{A.73})$$

$$\Gamma_t^{e'} = 1 - \Phi \left(\frac{\log(\bar{\omega}_t^e) + \frac{1}{2}(\sigma_e e^{\zeta_{e,t}})^2}{\sigma_e e^{\zeta_{e,t}}} \right) \quad (\text{A.74})$$

$$G_t^{e'} = \frac{1}{\sigma_e e^{\zeta_{e,t}}} \varphi \left(\frac{\log(\bar{\omega}_t^e) + \frac{1}{2}(\sigma_e e^{\zeta_{e,t}})^2}{\sigma_e e^{\zeta_{e,t}}} \right) \quad (\text{A.75})$$

$$\Gamma_t^i = G_t^i + \bar{\omega}_t^i \left[1 - \Phi \left(\frac{\log(\bar{\omega}_t^i) + \frac{1}{2}(\sigma_i e^{\zeta_{i,t}})^2}{\sigma_i e^{\zeta_{i,t}}} \right) \right], \quad (\text{A.76})$$

$$G_t^i = \Phi \left(\frac{\log(\bar{\omega}_t^i) - \frac{1}{2}(\sigma_i e^{\zeta_{i,t}})^2}{\sigma_i e^{\zeta_{i,t}}} \right) \quad (\text{A.77})$$

$$\Gamma_t^{i'} = 1 - \Phi \left(\frac{\log(\bar{\omega}_t^i) + \frac{1}{2}(\sigma_i e^{\zeta_{i,t}})^2}{\sigma_i e^{\zeta_{i,t}}} \right) \quad (\text{A.78})$$

$$G_t^{i'} = \frac{1}{\sigma_i e^{\zeta_{i,t}}} \varphi \left(\frac{\log(\bar{\omega}_t^i) + \frac{1}{2}(\sigma_i e^{\zeta_{i,t}})^2}{\sigma_i e^{\zeta_{i,t}}} \right) \quad (\text{A.79})$$

$$S_t^b = \beta^p \frac{\lambda_t^p}{\lambda_{t-1}^p} (1 - \theta^b + \theta^b v_t^b) \quad (\text{A.80})$$

$$S_t^e = \beta^p \frac{\lambda_t^p}{\lambda_{t-1}^p} (1 - \theta^e + \theta^e v_t^e). \quad (\text{A.81})$$

$$\begin{aligned} GDP_t = & m^i c_t^i + m^p c_t^p + e^{\zeta_{i_K,t}} \left(1 + \frac{\psi_K}{2} \left(\frac{i_t^K}{i_{t-1}^K} - 1 \right)^2 \right) i_t^K \\ & + e^{\zeta_{i_H,t}} \left(1 + \frac{\psi_H}{2} \left(\frac{i_t^H}{i_{t-1}^H} - 1 \right)^2 \right) i_t^H + s_g y_* e^{\zeta_{g,t}} \end{aligned} \quad (\text{A.82})$$

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Rightsizing Bank Capital: The Role of Macrofinancial Structure*

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We employ a macroeconomic cost-benefit approach to determine the appropriate tier 1 capital ratio for the banking systems of advanced economies. The macroeconomic costs of higher capital ratios are quantified using structural and semi-structural models of the Irish economy, while the macroeconomic benefits are derived using probabilistic models of financial crises and assumptions about the persistence of resulting output losses. We then examine the role of certain macrofinancial characteristics in determining the optimal level of bank capital. Incorporating these characteristics, the “rightsized” level of capital lies in a range of 12.5 to 19 percent.

JEL Codes: E5, G01, G17, G28, R39.

1. Introduction

Given their importance in the economy, banks face a number of requirements on the quantity and quality of their funding structure. Large losses in the banking system can have considerable spillover consequences for the real economy, which cannot be fully internalized by individual banks when mitigating their own risk profile. In addition, banks benefit from both explicit and implicit deposit insurance. Since capital represents the first line of defense in terms of loss

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absorption, banks are required to hold minimum levels of capital in line with existing regulations, regardless of the contribution of debt to their funding structure. In addition, they must fund themselves via tranches of capital to cater to specifically targeted balance-sheet-related risks, as well as systemic risk related to the broader environment or the systemic importance of a particular institution.

O'Brien and Wosser (2022) find that economies that are smaller, more open to trade and financial flows, more dependent on foreign direct investment (FDI), and that have more concentrated banking systems tend to have relatively higher systemic banking crisis probabilities, suffer larger systemic banking crisis-related costs, and exhibit relatively more adverse gross domestic product (GDP) growth tail risk through the financial cycle. In this paper, we examine the implications for appropriate levels of capital in the banking system of countries which display these structural characteristics. Specifically, we examine the influence of macrofinancial structural characteristics on the appropriate range of tier 1 capital¹ during periods when cyclical systemic risks are neither elevated nor subdued, including as necessary any additional bank capital needed to mitigate the effect of such structure-related systemic risks. This issue has been largely neglected in the existing literature.

Several studies have attempted to establish an appropriate capital range for particular banking systems based on the macroeconomic cost-benefit methodology that we follow in this paper (see Table 1). In the post-Global Financial Crisis (GFC) era, the Basel Committee on Banking Supervision (BCBS) study finds that the “optimal” tier 1 capital ratio range lies in the interval of 10 to 15 percent for advanced economies (see BCBS 2010).² However, as the approach has been refined and applied in other settings, the range found in subsequent studies is often somewhat higher.

From a methodological perspective, our approach is most similar to that of Brooke et al. (2015), who in turn follow Miles, Yang, and

¹ The tier 1 capital ratio is the ratio of a bank's core tier 1 capital, that is, its equity capital and disclosed reserves, to its total risk-weighted assets. Risk-weighted assets comprise approximately 60 to 70 percent of total assets (see Firestone, Lorenc, and Ranish 2019, p. 218).

² In an update to their paper (Birn et al. 2020), the BCBS acknowledges subsequent studies and concludes that optimal capital levels are likely to be higher than originally estimated.

Table 1. Estimates of Optimal Capital in Previous Studies

Study	Country	MM Offset	Optimal Bank Capital
Admati and Hellwig (2014)	— U.S.	Holds	20–30% tier 1 capital ratio
Barth and Miller (2018)		Does not fully hold	Leverage ratio of 19% (equates to a tier 1 capital ratio of 25%)
BCBS (2010)	OECD U.K.	Holds	10–15% tier 1 capital ratio
Brooke et al. (2015)		Does not fully hold	10–14% tier 1 capital ratio, 15–19% if resolution ineffective
Clerc et al. (2015)	Euro Area Ind. Countries	No reference (does not hold)	Circa 10.5% tier 1 capital ratio
Cline (2016)		Does not fully hold	12–14% CET1 to RWA
Collard et al. (2017)	U.S. Ind. Countries	Does not hold	10–14% tier 1 capital ratio
Dagher et al. (2016) (IMF)		0–75%	15–23% tier 1 capital ratio
Firestone, Lorenc, and Ranish (2019)	U.S. Switzerland	Does not fully hold	13–26% tier 1 capital ratio
Junge and Kugler (2013)		Does not fully hold	13–17% tier 1 ratio for global systemically important banks (G-SIBs)
Miles, Yang, and Marcheggiano (2013)	U.K. None Norway Sweden	Does not fully hold (45% offset used)	16–20% CET1 to RWA
Schroth and Møen (2017)		No reference	12.75–15.75% tier 1 ratio
Kragh-Sørensen (2012)		Does not fully hold	13–23% CET1 to RWA
Almenberg et al. (2017)		Holds	10–17% CET1 to RWA
Note: The MM offset captures the extent to which a bank's weighted average cost of capital is invariant to changes in its capital structure in accordance with Modigliani and Miller (1958). RWA refers to risk-weighted assets.			

Marcheggiano (2013), although we depart from their paper in some aspects of our modeling framework and in our focus on the role of macrofinancial structural characteristics. In addition, we employ two structural macroeconomic models of a small open economy (Ireland) to estimate the output-related costs of higher capital. Depending on the assumed effectiveness of the resolution regime for banks that are failing or likely to fail, Brooke et al. (2015) establish an optimal tier 1 capital range of 10–19 percent for the U.K. Abstracting from the potential benefits of the resolution regime, Miles, Yang, and Marcheggiano (2013) similarly find that much higher levels of capital than those deemed optimal by the original BCBS (2010) study were appropriate for the U.K., up to as much as 20 percent.

An important determinant of the macroeconomic cost of higher capital requirements in the literature is the extent to which the Modigliani and Miller (1958) theorem regarding the irrelevance of the capital structure to the weighted average cost of capital is expected to hold empirically for banks. Higher capital requirements can reduce economic activity if they raise banks' funding costs and, consequently, lead to higher interest rates for firms and households. Studies that have examined the macroeconomic impact of raising capital requirements typically assume that equity is more costly than debt financing, and that the increase in funding costs associated with higher capital requirements is partially or fully passed through to borrowers (BCBS 2010). However, the change in banks' weighted average cost of capital due to higher capital targets hinges on the value of the Modigliani-Miller "offset" that is applied. This offset determines the degree to which lower levels of leverage, by reducing the risk of bank failure, will also reduce banks' cost of equity and debt (Birn et al. 2020). Higher values of the offset will accordingly reduce the sensitivity of banks' funding costs to changes in the debt-equity composition of their liabilities.

Firestone, Lorenc, and Ranish (2019) consider the U.S. economy and estimate the optimal tier 1 capital ratio to be between 13 and 25 percent. In their analysis, the Modigliani-Miller offset does not fully hold, which has the effect of raising the optimal capital levels somewhat and goes some way toward explaining their relatively higher upper bound. In one of the few papers to specifically consider a smaller economy, Almenberg et al. (2017) determine the appropriate capital range for Sweden. They estimate that the optimal tier 1

capital ratio falls within a slightly different range (10–24 percent with Modigliani-Miller offset set to zero). However, this paper does not consider the specific contribution that the macrofinancial structural characteristics of Sweden contributes to their findings.

Barth and Miller (2018) examine the costs and benefits from raising the leverage ratio from 4 to 15 percent.³ They find that, based on their benchmark specification, the optimal leverage ratio is 19 percent (equating to a tier 1 capital ratio of circa 25 percent). At those levels of capital, banks are able to absorb high losses and almost completely shield the sovereign from having to recapitalize the banking system following crisis onset.

Of particular relevance is the study conducted by Cline (2016) who, similar to this paper, examines optimal capital across a range of countries, including Ireland. Our optimal capital range (12.5 to 19 percent) is aligned with his (12 to 14 percent).⁴ While the range for optimal capital ratios that we estimate is wider, this is likely attributable to our more structural approach, which tries to identify the independent and joint role of individual macrofinancial characteristics in determining these optimal levels of capital.

In line with the previous literature, we examine the resulting macroeconomic costs and benefits associated with requiring banks to raise additional capital. Following a similar methodology to the papers discussed above, we find that during periods when systemic risk is at median levels, the net marginal benefit of higher capital is zero when the tier 1 capital ratio is 16 percent. Below this point, the benefits of higher capital still outweigh the costs, whereas above this point the opposite holds. The results require a range of assumptions on key modeling parameters, such as the value of the Modigliani-Miller offset applied, the duration of crises, and the discount factor applied to future GDP losses related to systemic banking crises. Based on these assumptions, we derive a range of appropriate tier

³ Defined as total capital divided by total exposures (i.e., inclusive of off-balance-sheet items). The leverage ratio can be converted to a tier 1 capital ratio via the application of a risk-weighted asset scaling factor. Our sample's median risk-weighted assets to total assets ratio is 60 percent. Assuming this condition holds in our generalized "benchmark" country, a tier 1 ratio of 16 percent then equates to a leverage ratio of 9.7 percent.

⁴ Under the same assumptions of median risk-weighted assets to total assets of 60 percent, this equates to an optimal leverage ratio range of 7.2 to 12 percent.

1 capital ratios for normal risk environments between 12.5 to 19 percent with a benchmark level that is centered around 16 percent.

It is important to note, however, that our analysis does not address ancillary factors such as changes in the resolution framework or in approaches to risk weighting, which may influence both the steady-state and transition costs or benefits of particular capital levels. In terms of transition costs, the short-run impact of shifting to stronger capital regulations could also be significant. For example, the Macroeconomic Assessment Group (MAG 2010) finds that a 1 percentage point (pp) increase in capital ratios leads to a temporary negative deviation of GDP from its baseline of between 0.1 and 0.26 percent, with a concomitant 15 to 17 basis point increase in lending spreads. Similarly, Dorich and Zhang (2010) find that a 1 percentage point increase in capital ratios, implemented over four years, generates a fall in Canadian GDP of 0.26 percentage points eight years after implementation. In the case of Spain, Gerba and Mencia (2017) find that a 1 percentage point increase in capital leads to a drop in bank credit to firms and households by 1.1 and 1.4 percentage points, respectively, after four quarters, lowering GDP by close to 0.3 percentage points. More recently, Mendicino et al. (2020) find, using the three-dimensional model of the euro area, that higher capital requirements can carry considerable short-term costs, leading to a decline of GDP by up to 0.34 percent. Consequently, they argue that changes in required capital requirements should be implemented gradually across the euro area.

In terms of one of the main focal points of this paper, we find that net benefits of up to 3.5 percentage points of additional tier 1 capital accrue when macrofinancial structural characteristics are taken into account. This additional capital holds over the range derived from various modeling parameter assumptions mentioned above. While we consider the independent and joint impact of a number of structural macrofinancial characteristics, our results highlight the particularly important role played by country size, trade and financial openness, and FDI exposure in determining optimal capital buffers for a given country. Accordingly, countries that are smaller, more open to trade and financial flows, or have a high dependency on FDI should maintain slightly higher levels of tier 1 capital to mitigate the concomitant higher levels of systemic risk within their financial systems.

Finally, a key advantage of our analytical approach is that our results for the optimal capital ratio are readily generalizable to other countries. In particular, as we use a panel of OECD (Organisation for Economic Co-operation and Development) countries to estimate the marginal benefits of higher capital for given macrofinancial characteristics, these estimates provide an international benchmark which can be used to inform the rightsizing of capital levels across countries. On the marginal cost side, while our estimates are Irish-specific, they are consistent with the literature on the impact of higher capital ratios on steady-state output levels.

Moreover, there are aspects of our empirical approach that can provide insights for policymakers in other countries in terms of the strength of the different mechanisms through which capital-based macroprudential policy is transmitted to the real economy. We also highlight how using two macroeconomic models that feature detailed banking sectors, but that are methodologically quite different, can be used to enhance the overall robustness of the cost estimates. Alternatively, given the assumption that the marginal macroeconomic cost of higher capital is constant, policymakers could also combine local estimates of these costs with our results for marginal benefits to derive an optimal capital range for their banking system.

The remainder of the paper is structured as follows. Section 2 outlines our methodology for estimating the macroeconomic costs and benefits of additional capital. Section 3 presents our benchmark estimate of optimal capital and then quantifies the role of macrofinancial structural characteristics in shifting this optimal level. Section 4 addresses the robustness of our results and Section 5 concludes.

2. Estimating the Costs and Benefits of Higher Capital

We adopt and modify the “top-down” optimal capital approach set out in the existing literature. This methodology determines the marginal macroeconomic costs and benefits, in GDP terms, that are associated with additional bank capital. In this framework the marginal benefit of higher capital relates to the reduction in the probability of a systemic banking crisis as capital levels increase. The lower probability of a crisis in turn reduces the potential GDP losses that arise when systemic banking crises emerge. The marginal costs of

additional capital relate to the potential for higher capital requirements to increase bank lending rates, and consequently to damp consumption, investment, and aggregate output growth.

Combining the marginal benefit with the marginal cost yields the net marginal benefit of additional bank capital:

$$NetMargBen = (\Delta CrisisProb * CrisisCost) - \Delta CapitalCost, \quad (1)$$

which remains positive up until the point at which the costs associated with a given level of capital exceed the benefits. The level of capital achieved at that point is the estimate of the appropriate level of capital for a given banking system.

In order to address our main research question—the role of macrofinancial structural characteristics in determining appropriate levels of bank capital—we focus primarily on how these factors can influence the marginal benefit component of Equation (1). Specifically, we examine how these characteristics can influence the probability of systemic banking crises and thus their expected cost in output terms. Our approach to estimating marginal costs employs structural and semi-structural models of the Irish economy, which could be considered the prototypical small, open advanced economy. Accordingly, the key aspects of our modeling framework can be adapted to examine optimal capital regulation in other small, open economies.

2.1 Marginal Benefits of Higher Capital

The first step in our analytical approach is to estimate systemic banking crisis probabilities for each country over time. Following O'Brien and Wosser (2018), we use a pooled logit model that estimates systemic banking crisis probabilities for 27 OECD countries.⁵ Crisis probabilities are determined over a forecasting horizon of up to two years, conditional on a set of contemporaneous indicators found in the literature to have robust predictive properties, which we augment with the level of tier 1 bank capital in each country. The

⁵ The logit model has become the de facto standard in the literature. See Demirgüç-Kunt and Detragiache (1997), Davis and Karim (2008), Eichler and Sobański (2012), and Lo Duca and Peltonen (2013).

model estimates the following regression specification using biannual data from 1980 to 2021:

$$\text{Log} \left(\frac{\text{Pr}(\text{Crisis}_{it} | Z_{it})}{\text{Pr}(\text{NoCrisis}_{it} | Z_{it})} \right) = \alpha + \beta Z_{it} + \epsilon_{it}. \quad (2)$$

The dependent variable is a binary variable that takes a value of one in crisis periods and zero otherwise. To determine crisis and non-crisis periods in our sample we use data from Lo Duca et al. (2017) for European countries and Laeven and Valencia (2013) for non-European countries. Based on the definitions in Laeven and Valencia (2013) and Lo Duca et al. (2017), systemic banking crises are those that result in the failure of one or more banks, exhibit some degree of bank-to-bank contagion, require particular government-backed support programs for the banking sector, and result in spillovers from banking system losses to the real economy. In the benchmark O'Brien and Wosser (2018), vector Z_{it} comprises eight leading indicator variables: (i) the first-difference of the short-term real interest rate, (ii) the credit-to-GDP ratio, (iii) a house price index and (iv) its deviation from its long-run trend, (v) losses on equity markets, (vi) unemployment rate, (vii) a financial conditions index, and (viii) the extent of household leverage relative to GDP. To this specification we add the tier 1 capital ratio in the banking system for each country in our sample. All variables are lagged by one period. The list of our sample countries is provided in Table 2, while variable definitions, data sources, and summary statistics are shown in Table 3.

Our data are pooled across all countries, with standard errors clustered by country. We do not use a fixed-effects model, so that countries which never experienced a banking crisis may be included during model estimation. Following this, we add dummy variables capturing structural characteristics to the model, allowing for the potential role of macrofinancial structural characteristics to be identified.

To estimate the marginal effect of higher capital on crisis probability, we hold the remaining explanatory variables at their sample median levels to reflect time periods when systemic risk levels are neither elevated nor subdued. We then estimate crisis probabilities for each percentage point increment of the tier 1 capital ratio over a range from 5 to 30 percent. The fitted crisis probabilities across this

Table 2. Countries, Crises, and Macrofinancial Structure

Country	Crisis Years		Source	Small Country	Fin. Open	Trade Open	CBS (R.E.)	CBS (WB)	CBS (Mkt. Sect.)	FDI Dep. Country
	Start	End								
Argentina	1980:Q1	1980:Q4	LV (2013)							
	1989:Q1	1989:Q4	LV (2013)							
Australia	—	—	LV (2013)		X	X				
Austria	2007:Q4	2014:Q1	ECB (2016)					X		
Belgium	2007:Q4	Ongoing	ECB (2016)		X	X		X		
Brazil	1990:Q1	1990:Q4	LV (2013)							X
	1994:Q1	1994:Q4	LV (2013)							
Canada	—	—	LV (2013)				X			
China	1998:Q1	1998:Q4	LV (2013)							
	1987:Q1	1995:Q1	ECB (2016)	X	X	X				
Denmark	2008:Q1	2013:Q4	ECB (2016)							
Finland	1991:Q3	1996:Q4	ECB (2016)	X	X			X		
France	1991:Q2	1995:Q1	ECB (2016)		X			X		
	2008:Q2	2009:Q4	ECB (2016)							
Germany	2001:Q1	2003:Q4	ECB (2016)		X					
	2007:Q3	2013:Q2	ECB (2016)							
Greece	2010:Q2	Ongoing	ECB (2016)	X						
Hungary	1991:Q1	1995:Q4	ECB (2016)	X		X				X
	2008:Q3	2010:Q3	ECB (2016)							
Ireland	2008:Q3	2013:Q4	ECB (2016)	X		X		X		
Italy	1991:Q3	1997:Q4	ECB (2016)		X	X				X
	2011:Q3	2013:Q4	ECB (2016)							

(continued)

Table 2. (Continued)

Table 3. Variable Description and Summary Statistics

Variable Name	Summary Statistics				Obs.	Countries	Coverage	Source
	Mean	S.D.	Min.	Max				
Real Short-Term Interest Rates	0.053	0.058	-0.01	0.53	3,560	27	1980:Q1-2021:Q2	OECD
Losses Only S&P 500 Index	-0.014	0.04	-0.26	0.00	4,676	27	1980:Q1-2021:Q2	Yahoo Finance Historical Data
% Deviation of House Price Index from Trend	-0.91	0.11	-0.48	2.38	3,550	17	1981:Q4-2021:Q2	BIS—Long Property Series
% Deviation of Household Credit Growth from Trend	0.036	5.63	-100.45	30.22	2,780	27	1981:Q2-2021:Q2	BIS—Total Credit Statistics
% Deviation of Unemployment Rate from Trend	-0.06	1.4	-10.00	6.73	3,228	25	1980:Q4-2021:Q2	OECD
Credit-to-GDP Ratio	135.48	64.63	9.4	455.3	4,249	27	1980:Q1-2018:Q3	BIS—Total Credit Statistics
Financial Stability Index	0.12	0.10	0.00	0.84	2,750	17	2000:Q1-2021:Q2	ECB—Statistical Data Warehouse
House Price Index	155.75	101.52	21.29	564.96	3,727	17	1980:Q1-2021:Q2	BIS—Long Property Series

(continued)

Table 3. (Continued)

Variable Name	Summary Statistics				Description	Obs.	Countries	Coverage	Source
	Mean	S.D.	Min.	Max					
GDP World Share	2.40	4.07	0.42	22.27	Contribution to world GDP share	4,202	27	1980:Q1–2021:Q2	IMF—IFS Database
Trade Openness	75.14	54.35	11.54	423.99	Ratio of exports plus imports as a percentage of GDP	3,816	27	1980:H1–2021:H1	IMF—IFS Database
Financial Openness	10.65	47.17	0.01	466.33	External claims inward and outward as a percentage of GDP	2,348	27	1980:H1–2021:H1	IMF—IFS Database
FDI Dependency	49.58	62.27	0.58	391.11	Stock of FDI to GDP ratio	1,225	27	1980:H1–2021:H1	IMF—IFS Database
Tier 1 Capital Ratio	0.11	.04	.01	.25	Tier 1 capital as a proportion of risk-weighted assets	1,450	15	2000:H1–2020:H2	Bloomberg
Bank Concentration I	66.47	20.02	20.19	100.00	Share of total banking assets held by three largest banks	2,252	26	2000:H1–2020:H2	World Bank
Bank Concentration II	2,318.01	520.43	1,365.41	3,588.03	HHI Index of concentration	682	11	2000:H1–2020:H2	BIS
Bank Concentration III	53.39	11.28	21.87	74.27	Share of R/E exposure of banking sys.	744	11	2000:H1–2020:H2	Bloomberg

Note: This table presents information on the variables in the panel. The EWS panel is held at quarterly frequency whereas bank balance sheet data are only available. EWS data are collapsed according to the frequency available for analysis. The bank crisis data are taken from the ECB's crisis database (Lo Duca et al. 2017) for EU countries and from Laeven and Valencia (2013) for non-EU countries.

Table 4. Crisis Probability and Tier 1 Capital

	(1)
Constant	0.489 (3.473)
Real Short-Term Int. Rates	-141.558*** (35.074)
Losses Only S&P 500	-8.352** (3.960)
House Prices (%d)	0.846 (4.429)
Unemployment (%d)	-0.121 (0.139)
Household Credit Growth (%d)	0.159 (0.136)
Credit-to-GDP Ratio	0.005 (0.005)
Fin. Stability Index	-13.821** (5.483)
House Price Index	0.001 (0.016)
Tier 1 Capital Ratio	-54.561*** (9.051)
Observations	1,021
Pseudo R ²	0.348
Note: This table shows the effects of including tier 1 capital in the baseline EWS panel logit model with random effects. The dependent variable is a crisis dummy variable that takes a value of one in the quarter in which a systemic banking crisis begins, and is zero otherwise. Variables denoted by (%d) represent the deviation of the variable from its underlying trend. ***, **, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively.	

range allow the marginal contribution of each additional 1 percentage point of tier 1 capital to be estimated. Fitted systemic banking crisis probabilities are determined according to the following equation:

$$Pr\left(Crisis_{it} = \frac{e^{Z_{it}\beta}}{1 + e^{Z_{it}\beta}}\right).$$

(3)

An example of the regression output from one of the underlying logit models is presented in Table 4. In this model, tier 1 capital is negative and statistically significant at the 1 percent level, indicating that the given level of tier 1 capital reduces the likelihood

of a systemic banking crisis emerging in the subsequent two years when “normal” or median risk conditions prevail. However, there are diminishing marginal benefits as tier 1 capital increases, which suggests that there is an optimal level beyond which the costs of additional capital requirements outweigh the benefits.

This finding that tier 1 capital has a negative and statistically significant effect on crisis likelihood is robust to alternative model specifications. Table 5 presents the results of a robustness test for the inclusion of tier 1 capital in the logit model using the algorithm outlined in Young and Holsteen (2017). This procedure examines all possible combinations of the variables in the logit model together with the tier 1 ratio. In total, 254 models are considered. In all cases, the coefficient on tier 1 capital is negative and significant. The lower panel of the table shows the effect that the exclusion of each of the control variables has on the coefficient on tier 1 capital. For example, in all models that exclude real short-term interest rates, the coefficient on tier 1 capital is 5.4 percent larger than its mean value when all variables are included. Overall, we interpret these results as providing strong evidence of a systematic negative correlation between bank capitalization and crisis likelihood.

From Equation (1), there are two components to the calculation of the marginal benefits of capital comprising the probability of systemic crises and the expected cost of crises. In our analysis we use a common measure of the cost of crises derived from the crisis episodes in our sample. In particular, we estimate costs as the difference between actual GDP in the five years following the onset of the crisis and a pre-crisis linear projection of GDP over the same period, and express this difference as a percentage of the projected values.⁶ The results suggest that, on average, crises reduce GDP by 7.17 percent relative to pre-crisis trend. This is in line with prior studies, as shown in Table 6.

The final elements to consider when estimating the marginal benefit of higher bank capital are (i) the permanence or transience of the shock to GDP arising from crises, (ii) if the crisis costs are transitory, how long do they persist, and (iii) the appropriate discount rate to be used to evaluate the present value of losses.

⁶ Calculated using an exponentially weighted moving average of the GDP series up to the quarter where the crisis is deemed to have commenced.

Table 5. Logit Model Robustness and Tier 1 Capital

Model Robustness: Tier 1 Capital			
Dependent Var. Robust Var. Control Variables No. Models No. Observations	Prob. Sys. Crisis Tier 1 Capital 8 254 1,023		
Model Robustness Statistics		Significance Testing (%)	
Mean Coefficient Sampling Std. Error Modeling Std. Error Total Std. Error	-0.346 0.113 0.024 0.116	Sign Stability Significance Rate Positive Positive and Significant Negative Negative and Significant	100 100 0 0 100 100
Model Influence			
Marg. Effect of Variable		%Δ from Mean Coeff.	
Unemployment (%d) Real Short-Term Rate Credit-to-GDP Ratio House Price Index Losses Only S&P 500 Fin. Stability Index HH Credit Growth (%d) House Prices (%d) Constant	0.025 -0.019 -0.016 -0.010 -0.008 -0.007 -0.006 0.001 -0.326	7.3 5.4 4.7 2.9 2.3 2.0 1.6 -0.1	
R-Squared	0.643		
Note: This table presents robustness results related to the inclusion of tier 1 capital in the benchmark EWS logit model using the Young and Holsteen (2017) algorithm. The dependent variable is a crisis dummy variable that takes a value of one in the quarter in which a systemic crisis begins, and is zero otherwise. The remaining variables are as defined in Table 2. All possible combinations of these variables that include tier 1 capital are examined (254 models).			

If the GDP loss from a systemic banking crisis is permanent, the present value (PV) of the loss can be calculated using the formula for a perpetuity, with the PV of the loss ($C = 7.17$ percent) determined via the discount factor r according to

$$PV(Crisis_{it}) = \frac{C}{r}.$$

(4)

Table 6. Prior Studies of Crises and Output Loss

Study	Peak Loss (% GDP)	Long-Run Impact (% GDP)
Barrell et al. (2010)	6	3
Cecchetti, Kohler, and Upper (2009)	9	—
Cerra and Saxena (2008)	8	7
IMF (2009)	10	10
BCBS (2010)	9	6
Romer and Romer (2015)	4	3
Brooke et al. (2015)	5	4

In contrast, if the costs of crises are assumed to be transitory, then the duration over which they persist t will also be incorporated in the computation of losses (again with $C = 7.17$ percent):

$$PV(Crisis_{it}) = C \left(\frac{1 - (1 + r)^{-t}}{r} \right). \tag{5}$$

Overall, our estimate of the marginal benefit of higher bank capital is the reduction in crisis probability at each level of the tier 1 capital ratio multiplied by the expected crisis cost as calculated above.

2.2 Marginal Costs of Higher Capital

Imposing additional capital requirements on the banking sector may raise banks’ funding costs and ultimately lead to higher interest rates on lending to firms and households. The macroeconomic cost of higher capital is the lower growth in consumption, investment, and GDP that results from the increase in interest rates relative to the scenario in which capital requirements remain unchanged. To illustrate how such capital shocks are transmitted to the economy and to quantify their macrofinancial impact, we use two detailed macroeconomic models of the Irish economy—COSMO and Eire-Mod.

The transmission mechanism of an increase in capital requirements to the real economy is broadly similar in both models. There are several channels through which banks can raise their risk-weighted capital ratios: through retained earnings, via deleveraging,

by shifting the risk profile of their lending portfolios, or by issuing new equity. Both COSMO and EireMod assume that banks generate higher capital ratios through raising retained earnings. This assumption is consistent with the “pecking order theory” (see Myers and Majluf 1984) according to which banks prefer to exhaust internal funds first due to having to pay a premium on external finance. It is also consistent with other studies that use macroeconomic models to assess the costs of higher capital requirements (BCBS 2010) and with the empirical evidence on the behavior of banks’ capital ratios since the Global Financial Crisis (Cohen and Scatigna 2016). Deleveraging does occur endogenously in COSMO and EireMod as households and firms demand less credit in response to higher lending rates.

To quantify the impact of higher capital requirements, we simulate a scenario in both COSMO and EireMod in which banks are required to increase their capital ratios by 1 percentage point. Following the literature on optimal levels of capital, our analysis focuses on the long-run or “steady-state” impact of higher requirements and abstracts from transition costs.⁷

2.3 COSMO

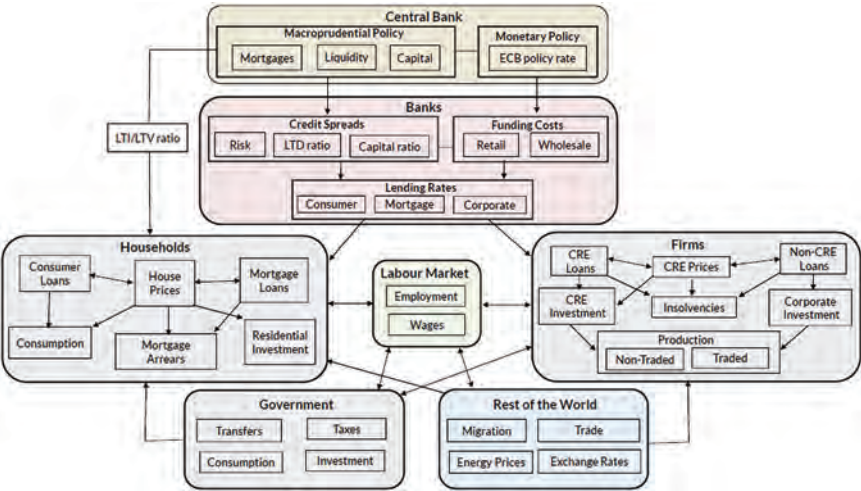
COSMO is a medium-scale estimated semi-structural model of the Irish economy. At its core it comprises three sectors: a traded sector that depends on world demand and Ireland’s export prices relative to competitors; a nontraded sector that is primarily driven by domestic economic conditions; and a government sector that grows in line with the rest of the economy in the absence of exogenous policy changes.⁸

The model incorporates a wide range of linkages between the central bank, retail banks, households, and firms. A key feature of the model is that it includes several borrower- and lender-based macroprudential instruments. The central bank can set limits on loan-to-income (LTI) and loan-to-value (LTV) ratios on household

⁷ If banks are required to increase capital ratios relatively quickly, they may choose to adjust loan volumes rather than interest rates. This may generate substantial short-run economic costs (Bridges et al. 2014; Aiyar, Calomiris, and Wieladek 2016).

⁸ See Bergin et al. (2017), Conefrey, O’Reilly, and Walsh (2018), and McInerney (2020) for details on different aspects of COSMO.

Figure 1. Macrofinancial and Macroprudential Linkages in COSMO



Source: Authors’ own schematic representation of the COSMO model.

mortgage borrowing. Given the focus of our study, it can also impose additional capital requirements or buffers on banks by raising their target capital ratio. These macroprudential and macrofinancial linkages are illustrated in Figure 1, where the transmission paths of macroprudential and monetary shocks are represented by the solid arrows.

COSMO assumes a monopolistically competitive banking sector in which banks set lending rates as a variable spread over deposit and wholesale funding costs. These lending rates form supply curves for the respective types of credit in the model: mortgages, consumer credit, commercial real estate (CRE) lending, and other (non-CRE) corporate credit. Lending spreads are a function of policy and risk factors, and it is through the spread that lender-based macroprudential instruments operate. When the central bank raises capital requirements, banks respond by raising lending spreads so as to generate the necessary increase in retained earnings to meet the new target capital ratio.

Higher lending rates increase the user cost of capital for firms and households and reduce the demand for all types of credit. The

Table 7. Impact of a 1 pp Increase in Banks’ Capital Ratio in COSMO

Variable	Long-Run Impact (%)
Lending Rates (bps)	+11
Credit	−0.61
Total Investment	−0.28
Residential Investment	−0.64
CRE Investment	−0.51
House Prices	−0.42
Consumption	−0.06
GDP	−0.05
“Nontraded” Output	−0.12
Total Employment	−0.06
Note: Estimates are percent deviations from baseline except for lending rates, which are basis point deviations.	

contraction in credit depresses house prices. This reduces household consumption through the housing wealth effect and residential investment due to the fall in the profitability of housing construction. The combination of higher mortgage rates and lower house prices pushes up the rate of household mortgage arrears. As lending to households is now riskier, banks raise lending spreads on loans to this sector.

Similarly, higher borrowing costs reduce corporate investment while the fall in CRE prices reduces investment in CRE. The fall in CRE prices reduces the value of collateral used for corporate borrowing, leading to a fall in both CRE and non-CRE corporate lending. Lower collateral values also affect firms’ ability to roll over existing credit lines and obtain working capital. This increases the rate of corporate insolvency, which—as a driver of the risk component of the corporate lending rate—further raises firms’ user cost of capital. The cumulative fall in investment leads to lower capital stocks and accordingly reduces the productive capacity of each sector in the economy in the long run.

To assess the macroeconomic cost of higher capital requirements, we simulate a 1 percentage point increase in banks’ capital ratios in COSMO. The impact on key variables is reported in Table 7. To generate the retained earnings necessary to meet the new target for the capital ratio, banks raise the weighted average lending rate by 11

basis points (bps) relative to the baseline scenario in which capital requirements do not change.⁹ The increase in the cost of borrowing reduces the demand for credit, with the weighted average stock of credit falling by 0.6 percent.¹⁰

The increase in lending rates raises the user cost of capital for both firms and households. In response, total investment falls by approximately 0.3 percent. CRE and residential investment, which are more interest rate-sensitive than other types of investment, fall by 0.5 percent and 0.6 percent, respectively. The increase in the user cost of housing capital reduces the demand for housing, leading to a fall in house prices relative to baseline of 0.4 percent. Household consumption falls by 0.06 percent due to the negative wealth effect arising from lower house prices.

Overall, the long-run macroeconomic impact of a 1 percentage point increase in capital requirements in COSMO is to reduce GDP by 0.05 percent and employment by 0.06 percent. However, the impact on the more bank-dependent, domestically oriented sectors of the economy is larger, with the output of the nontraded sector falling by 0.12 percent relative to the baseline.¹¹

2.4 *EireMod*

EireMod is a dynamic stochastic general equilibrium (DSGE) model of the Irish economy. At its core it comprises two sectors: a tradable-good sector, which consists of firms producing consumption and investment goods for the domestic market and a tradable goods sector producing export goods. The tradable goods sector uses intermediate imported goods as input and employs foreign capital as a factor of production. The core structure of the model has been extended by Lozej, Onorante, and Rannenberg (2023) to include a realistic

⁹ The weighted average rise in lending rates comprises an increase of 10, 11, and 19 bps in corporate, mortgage, and consumer lending rates, respectively.

¹⁰ This comprises a fall of 0.5, 0.4, 0.7, and 0.8 percent in the stock of mortgage, (non-CRE) corporate, CRE, and consumer credit, respectively.

¹¹ The traded, nontraded, and government sectors in COSMO are defined using the Irish Central Statistics Office's (CSO) Supply and Use Input-Output tables. The nontraded sector includes those sectors in which less than 50 percent of total final uses is exported and those sectors in which less than 50 percent of total final uses is used as government consumption.

financial sector, so that credit is intermediated by banks subject to a minimum capital requirement.

In the model, an implicit government guarantee on deposits creates an incentive for banks to increase leverage. As discussed below, this creates a break with the Modigliani-Miller proposition. As banks face a trade-off between increasing their leverage and incurring regulatory penalties if a shock hits their balance sheet, they optimally set their capital ratio above the regulatory minimum. All else equal, a marginal increase in the capital requirement forces the bank to deleverage, decreasing the bank's profitability. Monopolistically competitive banks respond to the decline in profitability by increasing the lending rate. In the long run, the lending rate will increase enough to keep the return on equity unaltered.

The model features a central role of the banking system in credit intermediation, by assuming that at each point in time, a constant share of both productive capital and the housing stock is intermediated through bank loans. An increase in the lending rate will therefore directly affect investment in productive capital and the demand for housing, which, in turn, affect investment in the construction sector. In the long run, capital will be lower, limiting production possibilities in the economy. Overall, the long-run macroeconomic impact of a 1 percentage point increase in capital requirements in EireMod is to increase average lending rates by 10 basis points and to reduce GDP by about 0.04 percent.¹²

2.5 *The Modigliani-Miller Offset*

The Modigliani-Miller theorem (Modigliani and Miller 1958) comprises two propositions. The first states that in a perfectly competitive and frictionless economy the value of a firm is independent of how it is financed, while the second postulates that, given that the first proposition holds, the cost of equity for a leveraged firm increases linearly with the debt-to-equity ratio. The implication of the theorem is that the relative returns on debt and equity adjust in response to a change in the capital structure so that the weighted average cost of capital (WACC) remains constant. For example, an

¹² A more detailed decomposition of these results is available from the authors.

Table 8. Estimates of the Modigliani-Miller Offset

Study	Sample	Offset (%)
Toader (2015)	European Banks	42
ECB (2011)	International Banks	41–73
Yang and Tsatsaronis (2012)	International Banks	10
Junge and Kugler (2013)	Swiss Banks	36
Miles, Yang, and Marcheggiano (2013)	U.K. Banks	45–90
Brooke et al. (2015)	U.K. Banks	53
Kashyap, Stein, and Hanson (2010)	U.S. Banks	36–64
Cline (2015)	U.S. Banks	45
Clark, Jones, and Malmquist (2018)	U.S. Banks	25–100
Barth and Miller (2018)	U.S. Banks	0

increase in capital (reduction in leverage) reduces the volatility of the return on equity and therefore lowers its required return.

However, there are two main distortions that prevent the Modigliani-Miller theorem from holding empirically in relation to banks. The first is the tax treatment of debt—banks’ interest payments on debt can be used to reduce their tax liabilities. The second relates to the implicit or explicit public guarantees on bank debt, mainly deposits. This acts as a subsidy to banks that reduces the cost of debt relative to equity, which does not enjoy such guarantees.

Accordingly, the key question from an empirical perspective is whether the required return on equity falls as the volatility of the return falls due to higher capital ratios (or lower leverage). The most common approach to testing the Modigliani-Miller theorem uses the Hamada (1972) framework that combines the theorem with the capital asset pricing model (CAPM). Essentially, this involves estimating the relationship between a bank’s leverage and its equity beta.¹³ Assuming that the beta of a bank’s debt is zero, this gives a relationship between the beta (or riskiness) of a bank’s assets and its leverage (Miles, Yang, and Marcheggiano 2013).¹⁴

As Table 8 shows, the empirical evidence on the Modigliani-Miller theorem is mixed. The estimated values for the offset from the

¹³ A bank’s equity beta measures the covariance between the return on the bank’s equity and that of the overall market.

¹⁴ Assuming a debt beta of zero implies that debt has bank-specific default risk but no systematic market risk.

Table 9. Impact of a 1 pp Increase in Banks’ Capital Ratio

Study	Country	Lending Rates (bps)	GDP (%)	Offset (%)
COSMO	Ireland	11	−0.05	0
EireMod	Ireland	10	−0.04	0
Mikkelsen and Pedersen (2017)	Denmark	10	−0.2	0
BCBS (2010)	OECD	9 to 19	−0.02 to −0.35	0
Almenberg et al. (2017)	Sweden	16	−0.09	0
Brooke et al. (2015)	U.K.	5 to 10	−0.01 to −0.05	50 to 0
Cline (2016)	U.S.	6	−0.08	50
Federal Reserve Bank of Minneapolis (2017)	U.S.	5.7	−0.06	50
Firestone, Lorenc, and Ranish (2019)	U.S.	3.4 to 6.9	−0.04 to −0.08	50 to 0

literature range from zero to a full offset, with a median value of close to 50 percent. There is some evidence that the offset is close to full for large or too-big-to-fail banks but significantly lower for small- and medium-sized banks (Clark, Jones, and Malmquist 2018) and some evidence that it is time-varying (Kashyap, Stein, and Hanson 2010).

Given the considerable heterogeneity in estimates of the offset, together with the absence of specific estimates for Irish banks, it is not clear which value for the offset should be applied to the increase in lending rates. The median estimate for the offset from Table 8 is close to 50 percent. However, in many studies that examine the macroeconomic impact of higher capital requirements, the rate at which the increase in lending rate is “offset” by a fall in the required return on equity is simply assumed. As our baseline, we assume that the offset is zero, but we consider alternative values in Section 3.1. Moreover, it is important to note that, as both COSMO and EireMod are linear models, any value for the offset can be applied ex post by scaling the fall in output due to higher capital requirements by that value.

2.6 Comparing Cost Estimates with Other Studies

Table 9 compares the impact of a 1 percentage point increase on lending rates and GDP in COSMO and EireMod with estimates for

other countries. Similar to our approach, some of these studies such as BCBS (2010) and Mikkelsen and Pedersen (2017) use macroeconomic models that include a banking sector to quantify the impact of higher capital requirements on both lending rates and GDP and which allow for feedback from the real economy to the banking sector. The remaining studies broadly adopt a two-step approach. They first use a basic loan pricing model and information on the relative cost of debt and equity to calculate how lending spreads may increase in response to higher capital requirements. They then feed this information into either a simple production function as an increase in the cost of capital (Brooke et al. 2015; Cline 2016) or as an increase in bond spreads facing nonfinancial firms in a macro model (Almenberg et al. 2017; Federal Reserve Bank of Minneapolis 2017; Firestone, Lorenc, and Ranish 2019).

All of the approaches yield broadly similar results to those from COSMO and EireMod in terms of the long-run increase in lending spreads and fall in output relative to a baseline. The study by the Basel Committee on Banking Supervision (BCBS 2010) is perhaps the most comprehensive, as it covers 13 countries and uses a variety of models. It finds that a 1 percentage point increase in capital requirements raises lending rates by between 9 and 19 basis points and reduces output relative to baseline between 0.02 and 0.35 percent of GDP. However, our results are closest to those of the Bank of England (Brooke et al. 2015). They find that a similar increase in capital requirements raises lending rates by between 5 and 10 basis points and reduces output relative to baseline by between 0.01 and 0.05 percent, depending on the value of the Modigliani-Miller offset applied. As the results from COSMO and EireMod assume the Modigliani-Miller offset is zero, they closely match the upper bound in Brooke et al. (2015).

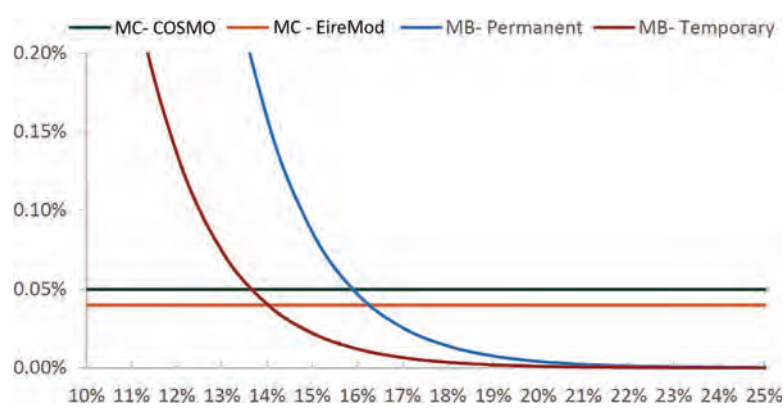
The similarity of our cost estimates to those from other studies reflects a consistency in the approach to modeling these costs across countries. The models employed by these studies have similar transmission mechanisms from the shock to capital to the increase in lending rates and through to the reduction in the interest rate-sensitive components of aggregate demand. In particular, in the case of those models that explicitly incorporate a banking sector, the primary channel through which banks achieve the higher capital target is by accumulated retained earnings from raising lending rates. In

this respect, our framework for quantifying the macroeconomic cost of increasing capital requirements can be readily applied to other countries.

While our cost estimates are consistent with the literature, there are two potential caveats to these estimates that should be mentioned relating to the use of macroeconomic models of the Irish economy to generate these estimates. First, the models' equations are estimated or calibrated over a sample period that includes episodes of severe macrofinancial and macrofiscal volatility in the aftermath of the 2007–08 financial crisis. Isolating and quantifying the economic impact of higher capital requirements is therefore complicated by the concurrent shocks to banks' balance sheets, financial market liquidity, and sovereign creditworthiness that buffeted the real economy and banking system over that period. In addition, the introduction of comprehensive macroprudential regulations over the last decade in Ireland has significantly changed how banks respond to real and financial shocks. These factors can therefore complicate the identification of capital shocks when using Irish data.

Second, the outsized presence of multinational enterprises (MNEs) in the Irish economy distorts the relationship between conventionally used measures of aggregate output, such as GDP, and macroeconomic conditions in the domestic economy. The activities of MNEs in Ireland are outward-focused and have limited spillovers to the local economy. Moreover, as they are not typically dependent on domestic banks for financing, investment by MNEs in their Irish operations is generally insensitive to any increase in retail lending rates arising from higher capital requirements for Irish banks (Desai, Foley, and Hines 2004; Lawless, O'Connell, and O'Toole 2014). These factors result in a relatively low (semi-) elasticity of aggregate output (GDP) with respect to interest rates in the macroeconomic models we use, and explain why our estimates of the macroeconomic impact of capital shocks are at the lower bound of the literature. However, our analysis does also highlight how when we focus on the impact of shocks to bank capital on the output of domestically focused sectors, our results are close to the mean found in the literature. These additional results can therefore provide useful information for interpreting our estimates relative to those found in studies of similar sized economies.

Figure 2. Benchmark Rightsized Tier 1 Capital Ratio



Note: The figure plots the marginal benefits of additional tier 1 capital when systemic banking crises have temporary (MB-Temporary) or permanent (MB-Permanent) effects, and the marginal costs of additional capital as simulated by a DSGE (MC-EireMod) and semi-structural (MC-COSMO) model. The marginal benefits relate to a risk environment in which EWS variables are at their median.

3. Rightsizing Tier 1 Capital Ratios

3.1 Benchmark Model

Using the estimates discussed above, we plot the resulting marginal benefit and marginal cost curves consistent with given levels of tier 1 capital in Figure 2. We show the marginal benefits curves for the cases in which systemic crises are assumed to have either permanent or temporary effects. In the latter scenario, the economic impact of a crisis is assumed to dissipate after five years. The slope of these curves capture the extent to which the marginal gain in terms of lower crisis likelihood from additional capital diminishes as the tier 1 capital ratio increases. The point at which the marginal cost and benefit curves intersect corresponds to the level of capital beyond which net benefits of higher capital become negative, and thus this level can be interpreted as the “optimal” or “rightsized” level for the economy.

Figure 2 clearly illustrates how assumptions regarding the nature of economic scarring effects from financial crises can significantly

influence the optimal capital ratio. If the policymaker assumes that crises have a transitory effect on the economy only, the appropriate level of capital for an economy is approximately 14 percent. In contrast, if these effects are assumed to be permanent, the appropriate level of capital rises to 16 percent. From a macroprudential and risk management perspective, policymakers would likely adopt a conservative approach to the calibration of capital buffers and thus we adopt the latter as our “benchmark” model of optimal capital. This represents the baseline case against which alternative parameterizations of the marginal benefit and marginal cost curves can be compared. This benchmark model includes all eight early warning system (EWS) variables, set to their median values, and the tier 1 capital ratio. It assumes that crises reduce GDP by 7.17 percent relative to the pre-crisis trend and these output losses are discounted using a discount rate of 3 percent. Finally, it assumes that banks’ weighted average cost of capital rises in proportion to the increase in capital so that the Modigliani-Miller offset is zero.

To highlight how the appropriate tier 1 capital ratio varies according to particular calibrations of the model, we consider alternative values for each of the key input parameters. A summary of alternative model calibrations is presented in panel A of Table 12. For example, assuming crises negatively affect GDP growth over a 10-year horizon results in an optimal tier 1 capital ratio of 13.5 percent (Scenario 2). Table 12 also shows the impact that different values of the Modigliani-Miller offset has on the optimal level. Setting the offset to a value of 50 percent, as some studies have assumed, results in a 1 percentage point increase in the appropriate capital level (Scenario 5). While the relatively small incremental increase in optimal capital that results from a halving of the macroeconomic costs of capital may seem surprising, it is important to note that the slope of the marginal benefit at that point mainly determines this change. As discussed above, the change in the slope of the marginal benefit curve is primarily a function of how the probability of experiencing a systemic crisis falls as the tier 1 capital ratio of the banking system increases. Thus, if the marginal benefit curve is relatively steep at the benchmark, this indicates that small changes in capital levels have a relatively large effect on mitigating the likelihood of lost output, or the expected output loss, due to a crisis. Accordingly, a given change in the marginal cost of higher capital will

result in a smaller increase in optimal capital than if the marginal benefit curve were flatter.

Overall, this sensitivity analysis suggests that, based on plausible alternative parameterizations of the marginal cost and marginal benefit curves, optimal tier 1 capital levels can range between 12.5 and 19 percent, with our benchmark or baseline calibration indicating a level of 16 percent. Having established this benchmark estimate for an appropriate tier 1 capital ratio for advanced economies, we now focus on the role that macrofinancial structural characteristics may play in influencing that ratio.

3.2 The Role of Macrofinancial Characteristics

Our choice of structural characteristics is informed by O'Brien and Wosser (2022), who find that economic size, the extent of trade and financial openness, the dependency on inward foreign direct investment (FDI), and the degree of market and sectoral exposure concentration in the banking system are important structural factors determining systemic risk. We now employ the same set of variables in our framework to assess how they affect the optimal or rightsized level of capital for the economy. The definitions of these variables, along with data sources and summary statistics, are reported in Table 3.

We first consider the role played by macrofinancial structural variables in amplifying or mitigating financial crisis likelihood. To classify an economy as having a particular macrofinancial structure, we construct dummy variables that indicate whether the economy is the top or bottom tertile of the cross-country distribution of the variable. For example, a country is categorized as being “small” if its contribution to global GDP is in the bottom tertile of countries in our sample. In the case of trade openness, we classify a country as being “trade open” if its ratio of exports and imports to GDP is in the top tertile across countries. In terms of financial openness, we focus on a de facto measure of financial globalization as captured by the ratio of inward and outward external claims to the total assets of the country's banking system. A country is thus defined as being “financially open” if this ratio is in the top tertile in our sample.

We measure bank concentration using three variants: (i) a market exposures concentration index (based on a Herfindahl-Hirschmann

Index (HHI) of market-sector exposures), (ii) the share of total banking system assets held by the three largest banks in a country, and (iii) the extent to which a banking system has exposures to real estate in particular. Our final structural variable measures the extent to which the country may be FDI dependent as indicated by the ratio of the stock of FDI to GDP.

The results of including these macrofinancial variables in our benchmark model with tier 1 capital are presented in columns 1–7 in Table 10, with the results relating to the structural dummy variables shown in the bottom panel. We find that only the small (at the 10 percent level) trade and financial openness variables have an independent effect on crisis likelihood for a given level of tier 1 capital. While not shown here, it is worth noting that the FDI dependency variable and two of the banking concentration measures are statistically significant if the capital variable is excluded or if these variables are included in continuous rather than discrete (dummy variable) form.¹⁵ Thus, these variables may be systematically associated with higher probabilities of crises, but either these risks are offset by higher levels of capital or the direct relationship is essentially linear.

We now examine the interactions between the dummy variables given that countries in our sample may exhibit multiple macrofinancial structural characteristics concurrently. For example, countries may be small and highly open to trade, or they may be financially open and also have concentrated banking systems. To assess how combinations of different characteristics may amplify or mitigate the effect of each characteristic on systemic risk, we include the interaction of different combinations of macrofinancial variables in the benchmark logit model. The results of these regressions are reported in columns 8–10 in Table 10, where only statistically significant interactions are reported for expository reasons. We find that being a small country tends to amplify the impact of other macrofinancial characteristics on systemic risk. In particular, systemic risk tends to be higher in countries that are both small and also open to trade, financially open, and dependent on FDI.

¹⁵ These results are available on request from the authors.

Table 10. Crisis Probability and Macrofinancial Structure

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Constant	-0.635 (1.987)	0.622 (1.987)	-1.195 (1.748)	2.023 (4.003)	1.438 (2.774)	1.724 (3.900)	1.784 (3.790)	-1.780 (2.562)	-2.542 (2.818)	1.447 (4.063)
Real Short-Term Int. Rates	303.889*** (116.285)	226.663*** (75.315)	277.919*** (94.199)	270.206*** (98.968)	258.736*** (85.719)	271.202*** (96.705)	279.548*** (83.620)	277.553*** (92.790)	282.989*** (99.256)	276.429*** (105.146)
Losses Only	-30.031** (11.976)	-26.460*** (10.005)	-25.493*** (9.100)	-26.979** (10.757)	-25.468*** (9.191)	-25.919** (10.204)	-27.576*** (10.161)	-28.456*** (10.395)	-26.357*** (9.560)	-28.586*** (11.000)
S&P 500	9.947** (4.333)	12.838*** (4.295)	12.333*** (4.106)	10.659*** (3.258)	8.931* (4.765)	9.701*** (3.102)	8.143* (4.664)	13.935*** (4.515)	12.463*** (4.359)	11.459*** (2.998)
House Prices (%d)	-0.374* (0.205)	-0.521** (0.217)	-0.586* (0.312)	-0.391* (0.231)	-0.302 (0.214)	-0.325* (0.179)	-0.272 (0.182)	-0.430*** (0.130)	-0.424*** (0.164)	-0.319** (0.135)
Unemployment (%d)	0.272** (0.133)	0.234** (0.096)	0.270** (0.117)	0.229** (0.090)	0.221*** (0.086)	0.234** (0.097)	0.229** (0.102)	0.248** (0.118)	0.281** (0.139)	0.234** (0.106)
HH Credit Growth (%d)	0.004 (0.007)	-0.009 (0.009)	-0.009 (0.010)	-0.007 (0.012)	-0.001 (0.010)	-0.002 (0.009)	0.008 (0.022)	-0.001 (0.010)	0.003 (0.010)	-0.002 (0.009)
Credit-to-GDP Ratio	-19.943** (8.852)	-19.146** (7.577)	-17.468** (7.033)	-16.426*** (6.140)	-14.970** (6.567)	-15.444*** (5.586)	-15.356*** (5.325)	-16.596*** (5.781)	-15.595*** (5.532)	-16.269*** (5.959)
Fin. Stability Index	-0.002 (0.015)	-0.001 (0.013)	0.016 (0.015)	-0.013 (0.033)	-0.015 (0.022)	-0.013 (0.031)	-0.025 (0.024)	0.009 (0.018)	0.017 (0.021)	-0.013 (0.033)
House Price Index	-55.914*** (17.973)	-55.315*** (12.227)	-61.195*** (16.540)	-50.983*** (12.106)	-49.889*** (11.909)	-54.288*** (12.660)	-51.821*** (11.204)	-59.001*** (16.685)	-67.710*** (21.902)	-53.461*** (12.774)
Tier 1 Capital Ratio										

(continued)

The marginal effects of these macrofinancial characteristics on the probability of experiencing a systemic financial crisis are presented in Table 11. We find that, conditional on the early warning indicators in the benchmark model, the marginal effect of being a small country is statistically significant (at the 10 percent level) and increases the probability of experiencing a crisis by 0.7 percentage points. Countries that are classified as “trade open” or “financially open” are over 1 percentage point more likely to experience crises. Focusing on the statistically significant interaction terms, crisis probabilities are 1.2 percentage points higher in countries that are both small and trade open, and 1.1 percentage points higher in countries that are both small and financially open. Finally, we find that the marginal effect of being small and FDI dependent has a similar impact on crisis likelihood (1.1 percentage point increase) as being small and either trade or financially open.

To quantify how optimal capital varies in countries with these types of macrofinancial structure, we first compute the marginal benefits of additional tier 1 capital incorporating the impact of these macrofinancial characteristics on the likelihood of financial crises. Figure 3 illustrates how the optimal level of capital changes for countries that are small and those that are trade open when crises are assumed to have either permanent or temporary effects on output. The marginal benefit curves from the benchmark models that exclude structural characteristics are shown for comparison. The top panel of Figure 3 shows that for small countries, the optimal level of capital increases by 1 percentage point relative to the benchmark in both scenarios for crisis-related output losses. The bottom panel of Figure 3 shows the impact of being “trade open” on the optimal level of capital. While small countries are typically also those that are most open to trade, we find that the impact of the latter on the appropriate level of capital is 50 basis points higher than the former in our baseline case that assumes that crises have permanent effects on output. However, in the case that the effects of financial crises are transitory, the small country and trade open characteristics have a similar impact on optimal levels of capital.¹⁶

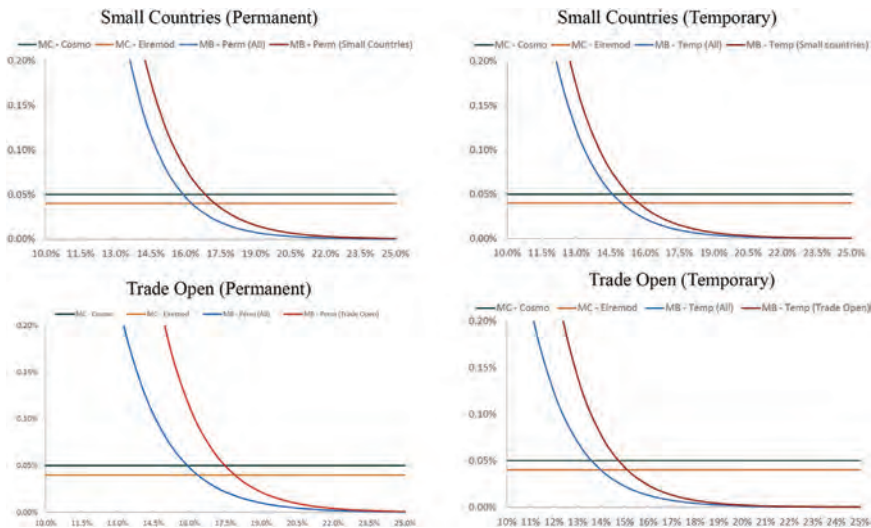
¹⁶ It is important to note that the relative scale of the marginal effects associated with structural variables, vis-à-vis each other, does not necessarily translate

Table 11. Marginal Effects of Macrofinancial Characteristics on Crisis Probabilities

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Small Country	0.007* (0.004)									
Trade Openness		0.010*** (0.004)								
Fin. Openness			0.011** (0.005)							
CBS I: HHI				0.003 (0.004)						
CBS II: Three-Bank Share					0.005 (0.007)					
CBS III: Real Estate						0.002 (0.004)				
FDI Dependent							0.008 (0.006)			
Small*TradeOpen								0.012*** (0.003)		
Small*Fin.Open									0.011*** (0.004)	
Small*FDI										0.011*** (0.002)
Observations	1,021	1,021	1,021	1,021	1,021	1,021	1,021	1,021	1,021	1,021

Note: This table shows the marginal effects of macrofinancial structural characteristics variables on crisis likelihood. ***, **, and * denote statistical significance at the 1, 5, and 10 percent levels, respectively.

Figure 3. Impact of Structural Characteristics on Rightsized Capital Ratios



Note: Impact of the “small country” and “trade open” characteristics on optimal capital ratios when systemic crises are assumed to have either permanent or temporary effects. The marginal benefit curves for all countries are plotted for comparison.

The loadings on the benchmark capital ratio that are attributable to each of the other macrofinancial characteristics are shown in panel B of Table 12. To keep the presentation tractable, we focus on the case where financial crises have permanent effects on output. For completeness, we also include the loadings based on individual characteristics, including those for FDI dependency and banking concentrations. As these coefficients are not statistically significant, any inference based on their values is subject to the associated caveats.

into a similar scale of optimal capital adjustments. The inclusion of various structural variables in regressions is associated with variation in the coefficients of the other EWS variables, depending upon which structural variable (or combination thereof) is included. Thus the trajectory of the marginal benefit curve, and its intersection point with the marginal cost curve, is altered, depending upon the structural variable involved. Whereas the optimal capital point is influenced by the marginal effect of the structural variable upon crisis probability, fitted values for the latter are also affected by the variation in the other coefficients.

However, the point estimates may be indicative of the magnitude of capital loadings that could be appropriate in countries that exhibit these characteristics. In these cases, the additional capital that is appropriate given higher levels of systemic risk ranges from 1 percentage point in countries with banking systems that are highly concentrated in either the share of assets held by the largest three or in their exposure to the real estate sector, to 2 percentage points in countries that are FDI dependent.

Table 12 shows that the largest increase in optimal capital relative to the benchmark comes from being both small and dependent on FDI, with an additional 3.5 percentage points of tier 1 capital required to mitigate the systemic risks associated with these characteristics. In countries that are small and open to trade, or small and financially open, additional capital of approximately 1.25 to 1.5 percentage points above the benchmark level may be appropriate. To our knowledge, these are the first estimates of how macrofinancial characteristics can independently or jointly raise the level of tier 1 capital that is optimal for a country's banking system.

In summary, the results in Table 12 show that estimates of appropriate levels of the tier 1 capital ratio in the banking system of advanced economies when cyclical systemic risk is at its median level can, depending on the particular calibration, range from 12.5 to 19 percent with a benchmark estimate of 16 percent. However, when various structural macrofinancial characteristics are considered, that range and benchmark could be up to 3.5 percentage points higher. From a financial stability perspective, therefore, our analysis suggests that it may be prudent for macroprudential authorities in countries that aim to incorporate systemic risk factors in their calibration of required capital buffers to also consider the structural macrofinancial dimension to these risks.

3.3 Capital Ratios, Risk Weights, and Cyclical Risks

While our framework is primarily designed to assess appropriate capital ratios in the steady state, policymakers would also likely require an analytical assessment of how rightsized levels of capital vary due to risk weights and to cyclical risks. In terms of the former, we can derive the implicit average risk weights applied to bank

Table 12. Model Calibration and Effect of Economic Structure on Optimal Capital

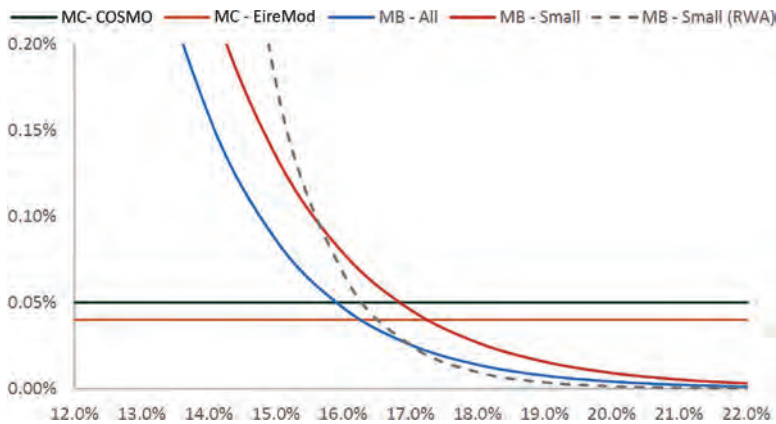
A. Sensitivity of Benchmark Tier 1 Ratio of 15% to Alternative Parameterization						
Parameterization Scenario	MM Offset (%)	Discount Factor	Perm. Crisis Effects	Temp. Crisis Effects	Duration (Years)	Optimal T1 Capital (%)
Benchmark	0	3	Y	N	—	16.00
Scenario 1	0	3	N	Y	5	12.50
Scenario 2	0	3	N	Y	10	13.50
Scenario 3	25	3	Y	N	—	16.50
Scenario 4	75	3	Y	N	—	18.00
Scenario 5	50	3	Y	N	—	17.00
Scenario 6	50	3	N	Y	5	13.75
Scenario 7	50	3	N	Y	10	14.75
Scenario 8	50	5	Y	N	—	16.25
Scenario 9	50	5	N	Y	5	13.75
Scenario 10	50	5	N	Y	10	14.50
Scenario 11	50	1	Y	N	—	19.00
Scenario 12	50	1	N	Y	5	14.00
Scenario 13	50	1	N	Y	10	15.00

(continued)

Table 12. (Continued)

B. Impact of Structural Characteristics Loadings on Benchmark Tier 1 Ratio of 16%		
Structural Variable	Definition	ΔOptimal Ratio (pp)
Small Economy	Based on contribution of bottom tertile of countries in sample to world GDP Countries in top tertile of share of exports plus imports in GDP Countries with ratio of banking system external claims to GDP in top tertile Countries with ratio of FDI to GDP in top tertile Countries in top tertile of shares of total banking system assets held by three largest retail banks	+1.00
Trade Openness		+1.50
Financial Openness		+1.00
FDI Dependent [†]		+2.00
CBS I [†]		+1.00
CBS II [†]		+1.25
CBS III [†]	Countries in top tertile of concentrated market-sector exposures measured by HHI Countries in top tertile of concentrated real estate market exposures Based on cohort of countries that are both small and trade open	+1.00
Small and Trade Open		+1.25
Small and Fin. Open		+1.50
Small and FDI Dependent	Based on cohort of countries that are both small and FDI dependent	+3.50
Note: EWS variables in the benchmark scenario are set at their sample median. “CBS” refers to the different measures of a concentrated banking system. [†] denotes variables with coefficient point estimates that are not statistically significant in the structural dummy variable regressions.		

Figure 4. Impact of Risk-Weighted Assets on Structural Capital Delta



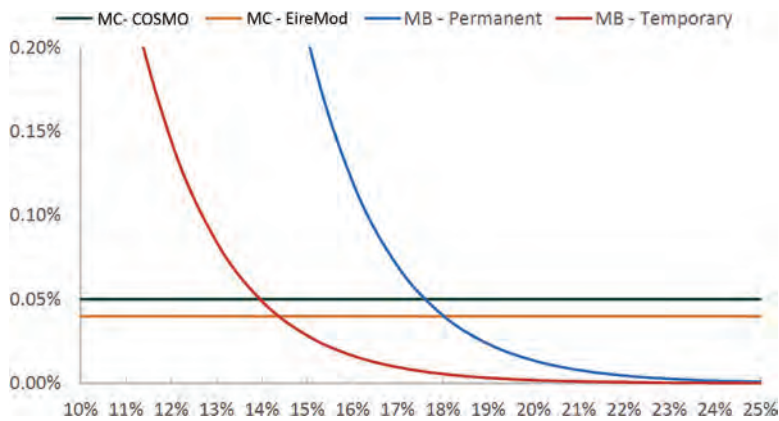
Note: The figure plots the marginal benefits of additional tier 1 capital for all countries, for small countries, and for small countries incorporating risk weights. The marginal benefit curves assume that systemic banking crises have permanent effects on output.

assets by country and over time.¹⁷ This allows us to examine the capital implications implied by higher risk weights, in addition to the capital implications of different structural macrofinancial characteristics considered above. For exposition purposes, we focus only on the “small” structural characteristic in the case that crises have permanent effects on output. Figure 4 illustrates how the incorporation of risk weights changes both the slope and position of the marginal benefit curve. Importantly, it shows that, although higher risk weights reduce the structure-related capital gap, they do not eliminate it.

In terms of cyclical risks, our analysis has focused on appropriate levels of capital when risks are held at median levels. To address the issue of how rightsized capital ratios change when risks are elevated, we evaluate the marginal benefits of additional capital when each leading crisis indicator is at a “stressed” level. Note that as the macroeconomic models we use to estimate the marginal costs of

¹⁷ For example, where tier 1 capital to total assets are reported alongside tier 1 capital to risk-weighted asset ratios such as in BCBS (2010), the risk-weighted assets and the average risk weights may be determined. See also footnote 1.

Figure 5. Rightsized Capital in an Elevated Risk Environment



Note: The figure plots the marginal benefits of additional tier 1 capital when EWS variables are at their 75th percentile for cases in which banking crises have temporary (MB-Temporary) and permanent (MB-Permanent) effects. The marginal costs of additional capital are simulated using a DSGE (MC-EireMod) and semi-structural (MC-COSMO) model and relate to average macroeconomic costs over the cycle.

additional capital are linear, these costs do not vary over the cycle. To keep the discussion concise, we focus on the benchmark capital models and thus abstract from the impact of macrofinancial structure. Figure 5 illustrates how the rightsized level of capital changes when all early warning indicators reflect a stressed value represented by the 75th percentile. We find that the rightsized levels increase by approximately 2 percentage points to 18 percent when systemic crises have permanently harmful effects, and by 1 percentage point to circa 14.5 when crises are assumed to have temporary effects on output. With the caveat that our framework is more suitable for assessing long-run capital requirements, these results suggest that accounting for an elevated risk environment can materially affect the calibration of appropriate macroprudential buffers.

4. Robustness Checks

We assess the robustness of our results across several dimensions. To ensure that our results are not driven by a single crisis episode or

country, we drop each crisis observation sequentially and reestimate the logistic model. These changes in the composition of our sample do not affect our results. We also estimate the model over different time periods and similarly find that our primary findings are broadly unchanged.

More fundamentally, we adjust the classification scheme for defining dummy variable according to each macrofinancial structural characteristic. Instead of assigning a particular characteristic to a country depending on whether they are above or below the top or bottom tertile value of the macrofinancial variable, we consider different thresholds such as quartiles and quintiles. We do find that some results are sensitive to these changes. The reclassification mainly affects bank concentration indicators with the coefficients on the HHI and market-sector exposure variables changing sign when the thresholds change.

As O'Brien and Wosser (2022) show however, when these structural characteristics are included in the estimation in continuous instead of discrete form, they are shown to be positively and significantly correlated with higher crisis likelihood. Moreover, when the dummy variables are more narrowly defined based on quartiles or quintiles, it becomes more difficult to distinguish between country cohorts sharing multiple characteristics, and thus the underlying structural characteristic become more difficult to identify. Accordingly, we argue that, while a more granular classification would be preferable *a priori*, our approach based on classifying countries according to whether they are above or below the top or bottom tertile of a particular variable is apposite given the relatively small number of country-financial crisis episodes that is available.

5. Conclusions

The analysis in this paper suggests that when the prevailing systemic risk environment is neither elevated nor subdued, the appropriate tier 1 capital range for the banking systems of advanced economies can plausibly lie in a range between 12.5 and 19 percent, with our benchmark specification indicating an optimal value of approximately 16 percent.

Additionally, we show that a country's macrofinancial structural characteristics can materially influence the appropriate level of capital by an additional 3.5 percentage points. The characteristics we find to be most important in determining steady-state capital ratios include whether a country is "small" in terms of its contribution to global GDP, the extent to which it is "open" to trade and capital flows, and the degree to which it is FDI dependent.

The range we identify is relatively broad but is consistent with prior literature, especially with those studies that use panel data similar to that used here. Our results, particularly in terms of the marginal benefits of higher capital for given macrofinancial characteristics, could therefore help inform the calibration of optimal capital ratios across different banking systems. Further, the analytical framework we outline in this paper is sufficiently flexible to allow policymakers to derive a country-specific range for appropriate levels of bank capital by combining local estimates of the marginal macroeconomic costs of higher capital requirements, which are assumed to be constant, with our results for the marginal benefits (which are nonlinear).

To further narrow the optimal capital range, additional analysis is warranted. On a country-by-country basis, a bottom-up approach toward "rightsizing" banking system capital should also be considered given that prior research has shown that the results achieved in top-down studies, such as we have adopted, can yield results indicating a higher capital range than suggested by a bottom-up approach. However, our results do point to the relevant additional capital that macroprudential policymakers could consider when framing an approach to capital buffers in small open advanced economies.

While our finding that the assessment of appropriate levels of capital should incorporate structure-related factors is novel, it should be noted that these recommendations relate to a "long-run" perspective and refer specifically to normal or median systemic risk levels. In particular, the transitional costs associated with shifting to higher levels of capital could be relatively large depending on how the introduction of additional capital requirements is sequenced. When making specific policy decisions regarding the calibration of these ratios, macroprudential authorities would also need to evaluate the

impact of prevailing economic and systemic risk conditions, as well as any associated transition costs to the steady state (or normal times), on their rightsized level.

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Reassessing International Effects of U.S. Monetary Policy Shocks*

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In light of recent evidence on the significant contribution of persistent monetary shocks to inflation dynamics in the U.S., we study their international transmission. In contrast to standard temporary nominal interest rate shocks, persistent shocks increase long-run inflation and the nominal rate while decreasing the real rate. We find that this leads to nonnegligible international spillovers and dollar depreciation. We further show that when it comes to understanding the international spillover effects of U.S. monetary policy, persistent monetary policy shocks rather than temporary nominal interest rate shocks have the potential to explain long-run comovements of macroeconomic variables across advanced countries.

JEL Codes: E12, F31, E52, E58.

1. Introduction

This paper studies the international transmission of U.S. monetary policy shocks, by considering two types of shocks—a standard temporary interest rate shock and a persistent inflation target shock. Traditionally, the effects of monetary policy are studied through temporary nominal interest rate shocks. Several key contributions, however, highlight the importance of persistent monetary policy shocks in understanding large and persistent swings in inflation in postwar data; that is, the low-frequency behavior of inflation or trend inflation (see Ireland 2007; Stock and Watson 2007; Cogley, Primiceri, and Sargent 2010; Del Negro, Giannoni, and Schorfheide 2015,

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among others). In closed-economy settings of the U.S. economy, a few recent contributions provide empirical evidence on the effects of an inflation target shock, which is one way to model persistent shifts in monetary policy (Mumtaz and Theodoridis 2018; Uribe 2022; Lukmanova and Rabitsch 2023).¹ A common conclusion in this literature is that a positive target shock tends to raise nominal interest rates together with inflation (and possibly output), inducing a positive comovement already in the short run or in the close aftermath of the shock.² There is also a vast empirical literature on the international transmission of U.S. monetary policy shocks (see, for example, Kim 2001; Canova 2005; Jacques and Rogers 2007; Bluedorn and Bowdler 2011; Dedola, Rivolta, and Stracca 2017, among many others). The general conclusion (across various time and country samples) is that there exist sizable international spillovers from the U.S. to other countries. However, most studies typically focus only on temporary monetary shocks, with the international transmission of persistent shocks remaining an underinvestigated topic.

Our paper contributes in this respect, proposing an empirical vector autoregression (VAR) modeling approach that explicitly distinguishes a standard temporary shock from a persistent monetary policy shock, studying their international transmission. In this, we build upon recent theoretical (Schmitt-Grohé and Uribe 2014, 2022; Cochrane 2016; DeMichelis and Iacoviello 2016) and empirical work (Mumtaz and Theodoridis 2018; Uribe 2022; Lukmanova and Rabitsch 2023) that argues that central banks affect inflation dynamics and the macroeconomy in these two ways. In addition to the conventional temporary shock to the nominal interest rate, we model the persistent monetary policy shock as a shock that shifts the inflation target. Adapting this setting to an international setup allows us to extend the analysis of the spillover effects of U.S. monetary shocks to long-natured policy shocks, and to disentangle the two types of monetary shocks. Our precise empirical setting is a simple extension of a baseline open-economy VAR model often used in

¹Uribe (2022) studies an explicitly permanent interest rate shock, not an inflation target shock. However, as documented in Lukmanova and Rabitsch (2023), permanent interest rate shocks and highly persistent inflation target shocks affect the economy in qualitatively similar ways.

²This positive comovement is also at the heart of a recent discussion on the existence of a Neo-Fisher effect.

the literature. In particular, in our baseline setting we include major macroeconomic variables typically used in the literature (i.e., output growth, inflation, nominal interest rates) of the domestic economy (U.S.) and a foreign composite economy, consisting of eight major advanced economies,³ and the (change in the) nominal exchange rate. In addition, to be able to identify U.S. inflation target shocks, we include a time series that serves as a proxy for the inflation target in addition to these conventional macroeconomic variables. In particular, we adopt the inflation target series (denoted “PTR”) from the Federal Reserve Board’s FRB/US model (cf. Brayton, Laubach, and Reifschneider 2014).^{4,5}

For identification, we adopt the novel methodology of Baumeister and Hamilton (2015, 2018, 2019), following our related research work on inflation target shocks in a closed-economy setting (Lukmanova and Rabitsch 2023). Baumeister and Hamilton (2015, 2018, 2019) show how explicit prior information can be used about both contemporaneous structural coefficients and the impacts of shocks, proposing to incorporate prior beliefs about the magnitude and signs of equilibrium impacts in a nondogmatic way. Our goal of adopting this methodology is that it allows us to derive guidance about the implied structural VAR parameters from theoretical models; for parameters about which there is consensus, we can specify priors with higher prior precision (for example, effects of the temporary interest rate shock), but the framework also explicitly allows us to account for the uncertainty of our structural parameters where there is less consensus from theoretical models (such as the effects of persistent inflation target shocks).

Our results are as follows. Domestically, the two types of shock have been shown to lead to opposite effects. Our paper’s

³We include Australia, Canada, Japan, New Zealand, Norway, Sweden, Switzerland, and the U.K.

⁴We do so because the PTR has the longest data availability. Alternatively, long-run inflation expectations of the Survey of Professional Forecasters are available from 1979 onward. Lukmanova and Rabitsch (2023) discuss a number of possible proxy measures for the U.S. inflation target.

⁵For other countries, we collect data on long-run inflation expectations from various national sources. Yet, because availability of these estimates for a number of countries is limited—the shortest sample starts in 1990:Q2—a foreign inflation target measure is not included in the VAR.

contribution is to document that the international spillovers also differ markedly. The conventional temporary monetary policy shock causes an increase in the nominal interest rate and, due to nominal rigidities, an increase in the real rate, and tends to lead to a contraction, decreasing inflation and output. We find that the contraction spills over to the foreign economy, where foreign output and inflation also decrease. The exchange rate appreciates. Yet, quantitatively, international spillovers are mild. In contrast, the transmission mechanism of a persistent inflation target shock is markedly different. Domestically, it implies an increase in inflation expectations. When households expect higher prices, the real rate drops, they start increasing current consumption, and output grows. To accommodate increased demand, producers increase prices, so that inflation goes up. As the economy is expanding, the central bank raises the nominal interest rate and we therefore observe a comovement between the nominal rate and inflation. Internationally, the U.S. inflation target increase leads to a U.S. depreciation, in line with related studies (cf. Mumtaz and Theodoridis 2018; Schmitt-Grohé and Uribe 2022, further discussed below), and raises not only domestic but also foreign inflation, as foreign countries import part of the higher inflation from the U.S. Given an insufficient response of foreign central banks, this leads to a rise in foreign countries' inflation expectations as well, also decreasing the foreign real rate. As a result, we observe expansionary effects abroad as well, and a dollar depreciation from U.S. inflation target shocks. After a few quarters, the foreign nominal interest rate responds to the changed macroeconomic environment, starting to rise in a similar fashion to that in the U.S. The behavior of real rates has also been identified as a main channel of international transmission of U.S. monetary policy in previous work (Kim 2001; Faust et al. 2003; Canova 2005; Ehrmann and Fratzscher 2009). As the U.S. real rate moves in opposite directions after each shock, disentangling the two shocks helps to uncover unbiased international effects of U.S. monetary policy.

In an extension to our baseline empirical setup, we consider a VAR model augmented with additional time series on long-term interest rates, to investigate if highly persistent monetary policy shocks contribute to the comovement of long-term rates across countries. In the model version with long-term rates, a positive U.S. inflation target shock continues to lead to expansionary effects in

the foreign economy and to higher foreign inflation expectations, but moreover leads to higher long-term interest rates. In contrast, there is no significant response of long-term interest rates to the nominal interest rate shock. This suggests that inflation target shocks have the potential to explain international comovement of long-term interest rates, as previously suggested by Chin et al. (2018), and, more generally, may have important implications for our understanding of bond term premia.^{6,7}

In related work, Schmitt-Grohé and Uribe (2022) study the effects of U.S. permanent monetary policy shocks on the exchange rate of the dollar versus the pound or yen. They find a persistent dollar depreciation after a permanent monetary policy shock. They also discover that once the effects of permanent and transitory shocks are separated, there appears to be no exchange rate overshooting. We similarly do not find evidence of exchange rate overshooting in our work. Using a factor-augmented vector autoregression (FAVAR) model augmented with international data, Mumtaz and Theodoridis (2018) similarly suggest that persistent monetary shocks, such as inflation target shocks, may play a significant role in the international transmission of U.S. monetary policy, leading to an increase in foreign output, inflation, and short- and long-run interest rates, as well as a nominal depreciation of the U.S. dollar. Kim, Moon, and Velasco (2017) relate the exchange rate overshooting result to the monetary regime, documenting that the overshooting behavior is a consequence of a lack of monetary credibility during Volcker's chairmanship of the Federal Reserve: instead of treating monetary shocks toward the end of the 1970s to the beginning of the 1980s as nearly permanent—the Fed was committed to combating high inflation—these shocks were perceived as temporary, creating wrong expectations. In other words, Kim, Moon, and Velasco (2017) point to the fact that assessing the right magnitude of the persistence of monetary shocks is consequential for the response of exchange rates.

⁶Using an open-economy estimated DSGE model, Chin et al. (2018) show that an inflation target shock in the U.S. results in a shift in long-run inflation of its foreign partner, and may be responsible for international comovement in long-term interest rates. Our empirical VAR model with observable time series for long-run inflation expectations and long-term interest rates produces findings that are consistent with Chin et al. (2018).

⁷Bauer and Rudebusch (2019) also emphasize the low-frequency behavior of inflation as a key determinant in understanding the yield curve.

Inoue and Rossi (2019) emphasize that the effects of monetary policy depend on how it affects expectations. They show that when interest rates are constrained at zero (which applies to about one-third of observations), fluctuations in exchange rates are driven not only by short-term expectations (as is the case when interest rates are not constrained) but also by longer-term expectations (5, 10, and even 20 years ahead). Our methodological approach deviates from these references by directly looking at the observable time series on long-run inflation expectations and adopting a set of identifying restrictions that ensures the inflation target shock absorbs efforts of the Fed to change the long-run inflation expectations.

The remainder of the paper is organized as follows: in Section 2 we summarize the channels of international monetary transmission discussed in the related literature. Section 3 presents the baseline VAR model of the international economy, describes the data used in our estimations, and discusses the methodological approach. Section 4 lays out the results of the baseline model, while in Section 5 we consider an extension of our model adding the long-term interest rates. Finally, Section 6 concludes.

2. Hypotheses about International Monetary Transmission Channels and Related Literature

Our profession's understanding of the international transmission channels of monetary shocks starts with textbook macroeconomic models such as the Mundell-Fleming model, where the behavior of exchange rates plays a crucial role. Much of the theoretical and empirical literature focuses exclusively on short-natured monetary shocks, such as (temporary) shocks to the nominal interest rate. According to the existing empirical literature, a contractionary monetary policy shock in the U.S. leads to contractionary effects in foreign economies and an appreciation of the U.S. dollar (Eichenbaum and Evans 1995; Bluedorn and Bowdler 2011; Georgiadis and Mehl 2016; Dedola, Rivolta, and Stracca 2017; Inoue and Rossi 2019; Kose et al. 2019).⁸

⁸We provide a description of some closely related literature on international effects of the U.S. monetary policy, as well as exchange rate behavior, in Table A.1 (see Appendix A).

In such a setting, several possible transmission channels are at work. One, an increase in the U.S. nominal rate leads to contractionary effects on the U.S. economy, lowering demand domestically as well as U.S. import demand, thus potentially contracting exports to the U.S. economy in the foreign economy via a trade channel. Two, the exchange rate appreciates in response to the U.S. monetary contraction, making home goods more expensive relative to foreign goods. In fact, in his seminal work, Dornbusch (1976) suggests that, in response to a monetary contraction, the level of appreciation in the short run is more pronounced than in the long run, known as exchange rate overshooting behavior. This behavior is consistent with a drop in the domestic price level that occurs in the long run to restore (long-run) equilibrium in goods markets—implied by purchasing power parity—in conjunction with an expected depreciation dictated by equilibrium in asset markets when the domestic over foreign interest rate differential is positive—implied by the uncovered interest rate parity condition. As a consequence of the dollar depreciation, which makes U.S. goods relatively cheaper, the foreign economy may thus benefit via an exchange rate channel, increasing its exports and contributing to an increase in foreign output. On the other hand, when imports from the U.S. prevail, U.S. goods become relatively more expensive due to dollar appreciation, which might lead to a decrease in purchasing power and a contraction of foreign demand. Three, a number of recent studies (Kim 2001; Faust et al. 2003; Canova 2005; Ehrmann and Fratzscher 2009) have also emphasized that spillovers to the foreign economy take place through shifts in the real interest rate, not exchange rates, and that the real rate channel constitutes an important international transmission channel.

Concerning the empirical evidence on the behavior of exchange rates, there is consensus in the empirical literature that contractionary monetary policy shocks lead to an appreciation of the country's nominal exchange rate (Clarida and Gali 1994; Eichenbaum and Evans 1995; Faust and Rogers 2003). However, a lot of the discussion on international effects of monetary policy shocks centers around the precise pattern of exchange rate responses, some confirming Dornbusch's result on exchange rate overshooting on impact (Kim 2001; Faust and Rogers 2003; Bjørnland 2008; Kim,

Moon, and Velasco 2017), while others argue it might occur in a delayed fashion (Scholl and Uhlig 2008; Steinsson 2008). Inoue and Rossi (2019) show that the reaction of exchange rates depends on how the monetary shock is perceived: depending on whether the shock affects short-run or long-run inflation expectations, the effects on exchange rates might vary. Moreover, there is a large literature pointing toward persistent deviations from uncovered interest rate parity (UIP) in the data (Eichenbaum and Evans 1995; Faust and Rogers 2003; Faust et al. 2003). Other recent studies (Hettig, Müller, and Wolf 2019; Huber and Rabitsch 2019; Schmitt-Grohé and Uribe 2022) find no clear evidence for overshooting behavior. Most closely related, Schmitt-Grohé and Uribe (2022) study international effects of two types of monetary shocks, one permanent, one transitory, and conclude that no overshooting occurs.

The focus of our empirical analysis is on understanding the spillover effects on macroeconomic variables in the foreign country. As such, we study not only the effects on exchange rates, but track all key foreign macroeconomic variables, including economic activity in the foreign economy. In addition, our model allows for a distinction between two types of monetary shocks, an inflation target shock that affects long-run inflation expectations, and a nominal interest rate shock.⁹ While there is little theoretical and empirical work on spillover effects in response to long-term natured monetary policy shocks, we expect similar mechanisms of a trade channel, an exchange rate channel, and a real rate channel to be relevant also for our shock to long-run inflation expectations. We expect the following effects of the inflation target shock and the nominal rate shock. The inflation target shock in the U.S. transmits via an increase in inflation expectations, which pushes down the real interest rate. As a result, agents increase consumption demand, causing an increase in

⁹In similar spirit, Schmitt-Grohé and Uribe (2022) contrast permanent and temporary interest rate shocks; however, their focus is mostly on implications for exchange rate behavior, not the size of spillovers on foreign activity. They estimate a five-variable VAR model, including U.S. output growth, U.S. inflation, U.S. nominal interest rate, exchange rate, and foreign nominal interest rate. The analysis is done for the U.K. and Japan.

output. With a demand increase, inflation jumps up. The simultaneous reaction of the nominal rate is small. A contractionary nominal interest rate shock leads to an increase in the nominal interest rate in the U.S. and to a decrease in U.S. output growth and inflation. The nature of this shock is well-known: in the U.S., a positive nominal interest rate shock leads to an increase in the nominal rate and, due to delayed response of prices, an increase in the real rate. This leads to a contraction of consumption and demand, leading to a decrease in output and inflation. As our types of monetary shocks affect the real interest rate in opposite ways and to a differing degree of persistence, we expect to also observe differences in international spillovers: the inflation target shock is very persistent, and it affects long-run inflation expectations and the real rate in the U.S. In fact, Inoue and Rossi (2019) show that monetary policy transmits to exchange rates via both the inflation expectations channel and the real rate channel, and the inflation target shock affects both.

3. Econometric Framework

This section introduces a structural vector autoregressive (VAR) model for our empirical analysis of the international transmission of U.S. monetary shocks. Our aim is to study the transmission mechanism and comovement properties of macroeconomic variables in response to a persistent monetary shock, the inflation target shock, in addition to the more widely studied temporary nominal interest rate shock. We are interested in the transmission of U.S. persistent monetary shocks to other advanced economies. We adopt the recent approach of Baumeister and Hamilton (2015, 2018, 2019) to obtain inference in structural VARs, applied on a VAR including main U.S. and foreign variables.

3.1 Data

We use data on the U.S. and eight foreign advanced economies: Australia, Canada, Japan, New Zealand, Norway, Sweden, Switzerland, and the U.K. Our main data sources are the OECD, the Federal Reserve Bank of St. Louis FRED II database, and the BIS. For the baseline specification, our estimations start in 1974:Q1 (this is

dictated by data availability for all countries) and end in 2019:Q1. We consider a baseline setting of a bilateral VAR model of U.S. and foreign macroeconomic variables (output growth, inflation, and nominal interest rates), the nominal exchange rate, and a proxy variable for the U.S. inflation target. Foreign macroeconomic variables are constructed as trade-weighted averages (using trade weights of the BIS) over the eight advanced economies. Table 1 provides further details on data sources and transformations, and on time samples. A potential problem arises from the fact that the inflation target is unobserved. As a proxy for the implicit U.S. inflation target, we use time series on U.S. long-run inflation expectations that come from the Fed's FRB/US model, described in Brayton, Laubach, and Reifschneider (2014), the PTR (*perceived inflation target rate*) measure. This time series is available on a quarterly basis from 1962:Q1.¹⁰ Thus, our vector of observed data includes the following eight variables:

$$\mathbf{y}_t = [\bar{\pi}_t, \Delta y_t, \pi_t, i_t, \Delta ex_t, \Delta y_t^*, \pi_t^*, i_t^*],$$

where $\bar{\pi}_t$ is the proxy for the domestic (U.S.) inflation target, $\Delta y_t^{(*)}$ is domestic (foreign) real output growth, $\pi_t^{(*)}$ is domestic (foreign) inflation, $i_t^{(*)}$ is the domestic (foreign) interest rate, and Δex_t is the change of the nominal effective exchange rate. U.S. variables are referred to as domestic variables; variables with an asterisk belong to foreign economies. The exchange rate is defined as the U.S. dollar price of a unit of foreign currency ($EX_t = \text{dollar/foreign currency}$), such that an increase in EX_t reflects a domestic (dollar) depreciation, and $\Delta ex_t := \log(EX_t) - \log(EX_{t-1})$.

¹⁰For comparison, we show foreign inflation expectations in Figure C.1. A few observations stand out. First, foreign and U.S. inflation expectations are highly correlated, very persistent, and less volatile compared to actual inflation time series. Second, in the past 10 years, inflation expectations of all countries converge to values close to 2 percent. As we have many variables in our VAR but not a lot of data points, we have decided not to incorporate foreign long-run inflation expectations as an additional time series, as it would considerably shrink our sample size, being available only from 1990:Q1. Also, there is very little variation across all countries (U.S. included), thus the inclusion of foreign inflation expectations in addition to domestic brings little additional information.

Table 1. Data Description

Time Series	Database	Country	Time Span Available
U.S. Inflation Expectations, PTR	FRB/US model, Federal Reserve Board	US	1962:Q1–2019:Q1
Real Output Growth	OECD	AU, CA, JP, NZ, NO, SE, CH, UK, US	1962:Q1–2019:Q1
CPI-Based Inflation	OECD	AU, CA, JP, NZ, NO, SE, CH, UK, US	1962:Q1–2019:Q1
Short-Term Rate: - Three-Month Yield	FRED II, Federal Reserve Bank of St. Louis	NZ	1974:Q1–2019:Q1
- Interbank Rate		NO, US CH	1962:Q1–2019:Q1 1972:Q1–2019:Q1
- Three-Month Treasury Bill Rate		JP, SE, UK US	1960:Q2–2019:Q1 1947:Q2–2019:Q1
Long-Term Rate: - 10-Year Government Bond Yield	FRED II, Federal Reserve Bank of St. Louis	CA, CH, UK, US	1960:Q1–2019:Q1
		NZ AU NO SE JP	1970:Q1–2019:Q1 1969:Q3–2019:Q1 1985:Q1–2019:Q1 1987:Q1–2019:Q1 1989:Q1–2019:Q1
Trade Weights: - Weighting Matrix for Narrow Indices	BIS	AU, CA, JP, NZ, NO, SE, CH, UK, US	2017–19
Exchange Rate: - Nominal Effective Exchange Rate (NEER), Narrow Indices	BIS	AU, CA, JP, NZ, NO, SE, CH, UK, US	1964:Q1–2019:Q1
Note: This table contains the description of time series used for the analysis, their sources and time span.			

3.2 The Model

Our baseline structural VAR model takes the following form:

$$\mathbf{A}\mathbf{y}_t = \mathbf{B}\mathbf{x}_{t-1} + \mathbf{u}_t, \mathbf{u}_t \sim \mathcal{N}(0, \mathbf{D}), \quad (1)$$

where $\mathbf{y}_t$ is the vector of observed variables at time t , vector $\mathbf{x}'_{t-1} = (\mathbf{y}'_{t-1}, \mathbf{y}'_{t-2}, \dots, \mathbf{y}'_{t-p}, 1)'$ contains p lags of $\mathbf{y}$ and an intercept, $\mathbf{u}_t$ is the vector of structural shocks with covariance matrix $\mathbf{D}$. We specify our baseline model with two lags, i.e., $p = 2$.

A crucial challenge when working with VAR models arises from the need to identify structural shocks from the data. In particular, the data are loaded into the reduced-form VAR model:

$$\mathbf{y}_t = \mathbf{\Psi}\mathbf{x}_{t-1} + \boldsymbol{\epsilon}_t, \boldsymbol{\epsilon}_t \sim \mathcal{N}(0, \mathbf{\Sigma}), \quad (2)$$

where $\mathbf{\Psi} = \mathbf{A}^{-1}\mathbf{B}$ and $\boldsymbol{\epsilon}_t = \mathbf{A}^{-1}\mathbf{u}_t$ is the vector of reduced-form innovations, which are some linear combination of the structural shocks, $\mathbf{u}_t$, with covariance matrix $\mathbf{\Sigma}$. In order to draw conclusions on the causal effects of the monetary shocks on other variables in the system, we need to identify structural shocks, $\mathbf{u}_t$; thus, we need to know $\mathbf{A}$ and $\mathbf{D}$. The problem is that estimating the reduced-form VAR gives us $\mathbf{\Psi}$ and $\mathbf{\Sigma}$, which is not enough to know $\mathbf{A}$ and $\mathbf{D}$. The identification of the recent methodology of Baumeister and Hamilton (2018, 2019), which we follow, entails the prior choice on both contemporaneous structural coefficients (the coefficients of the $\mathbf{A}$ -matrix) and the impacts of shocks (prior information on elements of the $\mathbf{H}$ -matrix, which is defined below as an adjugate matrix of $\mathbf{A}$), proposing to incorporate prior beliefs about the magnitude and signs of equilibrium impacts in a nondogmatic way. Unlike the sign restrictions literature (cf. Uhlig 2005; Scholl and Uhlig 2008), which aims to identify structural shocks by relying on theory predictions about the signs of the responses of identified shocks, the identification of Baumeister and Hamilton (2018) (BH2018 henceforth) entails specific prior distribution choices, making explicit the identification assumptions and allowing for uncertainty over them. Since the related theoretical literature does not provide us with consistent predictions about the expected effects of the inflation target shock, especially not in the international environment, we believe incorporating uncertainty over the identifying assumptions is crucial for our analysis to yield reliable results.

All elements of the $\mathbf{A}$ -matrix are distributed according to a t-distribution with some prior parameters, where we specify prior means according to predictions from economic theory. In particular, in our model the $\mathbf{A}$ -matrix takes the following general form:

$$\mathbf{A} = \begin{bmatrix} a_{11} & a_{12} & a_{13} & a_{14} & a_{15} & a_{16} & a_{17} & a_{18} \\ a_{21} & a_{22} & a_{23} & a_{24} & a_{25} & a_{26} & a_{27} & a_{28} \\ a_{31} & a_{32} & a_{33} & a_{34} & a_{35} & a_{36} & a_{37} & a_{38} \\ a_{41} & a_{42} & a_{43} & a_{44} & a_{45} & a_{46} & a_{47} & a_{48} \\ a_{51} & a_{52} & a_{53} & a_{54} & a_{55} & a_{56} & a_{57} & a_{58} \\ a_{61} & a_{62} & a_{63} & a_{64} & a_{65} & a_{66} & a_{67} & a_{68} \\ a_{71} & a_{72} & a_{73} & a_{74} & a_{75} & a_{76} & a_{77} & a_{78} \\ a_{81} & a_{82} & a_{83} & a_{84} & a_{85} & a_{86} & a_{87} & a_{88} \end{bmatrix}$$

and with $\mathbf{A}^{-1} = \frac{1}{\det(\mathbf{A})}\mathbf{H}$, where $\mathbf{H}$ is the adjugate matrix of $\mathbf{A}$. We then check the signs of the resulting impulse responses by computing the prior and posterior probabilities that the response of a specified structural shock on the indicated variable is positive after the shock. If necessary, we impose additional restrictions on the sign of impulse responses by imposing structure on the elements of the $\mathbf{H}$ -matrix using an asymmetric t-distribution as proposed in BH2018.

We begin by discussing our prior choices on the elements of the $\mathbf{A}$ -matrix. For the prior means of the a coefficients highlighted in blue, which correspond to the contemporaneous relations between $\Delta y_t, \pi_t, i_t$ (or $\Delta y_t^*, \pi_t^*, i_t^*$, respectively), we adopt the values used by BH2018 based on a simple three-equation (closed-economy) New Keynesian (NK) model to pin them down (for both the domestic and the foreign economy). These capture the supply relation (based on a Phillips curve), the demand relation (IS curve), and an interest rate rule of a small-scale theoretical model. This model is summarized by Equations (4)–(6) below. For coefficients a_{11} to a_{14} , a_{21} , a_{31} , and a_{41} , highlighted above in green, we follow our closed-economy paper on inflation target shocks, Lukmanova and Rabitsch (2023), where we adapted the three-equation NK model of BH2018 to a four-equation model accounting for the presence of a time-varying inflation target, $\hat{\pi}_t$, given by Equation (3) of the system below:

$$\hat{\pi}_t = \rho_{\hat{\pi}} \hat{\pi}_{t-1} + \varepsilon_{\hat{\pi},t} \tag{3}$$

$$\hat{Y}_t = \alpha^S \hat{\pi}_t + u_t^S \quad (4)$$

$$\hat{Y}_t = \beta^D \hat{\pi}_t + \gamma^D \hat{R}_t + u_t^D \quad (5)$$

$$\hat{R}_t = \rho_R \hat{R}_{t-1} + (1 - \rho_R) \left[\rho_\pi (\hat{\pi}_t - \hat{\pi}_t^*) + \rho_Y (\hat{Y}_t) \right] + \varepsilon_{R,t}. \quad (6)$$

To summarize, we thus adopt the choice for the prior parameters for the t-distribution of the elements of the **A**-matrix pertaining to the standard three-equation NK model from BH2018. We use them not only for the choice of prior means for *a*-elements of the home (U.S.) economy, but also for the foreign economy (lower right block of blue-highlighted coefficients (a_{66} to a_{68} , a_{76} to a_{78} , and a_{86} to a_{88})). Additionally, we augment the domestic country's setting with a time-varying inflation target, following Lukmanova and Rabitsch (2023). The prior means of all remaining elements of the **A**-matrix in these equations equal to zero. Finally, this leaves specifying the coefficients of the **A**-matrix of the equations pertaining to the exchange rate, coefficients a_{51} to a_{58} . Our choice to follow closely the prior choices of BH2018 implies that we only obtain guidance for the choices of prior means of closed-economy relations. To establish prior choices for the coefficients of the exchange rate equations, we continue to let theoretical relationships guide us. In particular, suppose, as would be obtained by many open-economy macroeconomics models, that the exchange rate is determined by an uncovered interest rate parity (UIP) equation that relates the expected change in the exchange rate to the domestic to foreign interest rate differential:

$$E_t (ex_{t+1} - ex_t) = i_t - i_t^*. \quad (7)$$

Assuming, similar to how BH2018 do for a number of macro variables, that the law of motion for the exchange rate is governed by a simply (persistent) autoregressive process, e.g. $ex_{t+1} = \rho_{ex} ex_t + \varepsilon_{ex,t}$, we can derive the future expected change in the exchange rate, given by $E_t (ex_{t+1} - ex_t) = (\rho_{ex} - 1) ex_t$. Using this assumption together with the above UIP relation then allows us to choose coefficients $a_{54} = 1$, $a_{55} = 1 - \rho_{ex}$, and $a_{58} = -1$. We set $\rho_{ex} = 0.95$. Finally, we set the prior means of all other *a*-coefficients

equal to zero. The resulting matrix containing prior means of the elements of the $\mathbf{A}$ -matrix is the following:

$$\mathbf{A} = \begin{bmatrix} a_{11} & a_{12} & a_{13} & a_{14} & a_{15} & a_{16} & a_{17} & a_{18} \\ a_{21} & a_{22} & a_{23} & a_{24} & a_{25} & a_{26} & a_{27} & a_{28} \\ a_{31} & a_{32} & a_{33} & a_{34} & a_{35} & a_{36} & a_{37} & a_{38} \\ a_{41} & a_{42} & a_{43} & a_{44} & a_{45} & a_{46} & a_{47} & a_{48} \\ a_{51} & a_{52} & a_{53} & a_{54} & a_{55} & a_{56} & a_{57} & a_{58} \\ a_{61} & a_{62} & a_{63} & a_{64} & a_{65} & a_{66} & a_{67} & a_{68} \\ a_{71} & a_{72} & a_{73} & a_{74} & a_{75} & a_{76} & a_{77} & a_{78} \\ a_{81} & a_{82} & a_{83} & a_{84} & a_{85} & a_{86} & a_{87} & a_{88} \end{bmatrix}$$
$$= \begin{bmatrix} 1 & 0 & 0 & 0 & 0 & 0 & 0 & 0 \\ 0 & 1 & -2 & 0 & 0 & 0 & 0 & 0 \\ 0 & 1 & -0.75 & 1 & 0 & 0 & 0 & 0 \\ 0.75 & -0.25 & -0.75 & 1 & 0 & 0 & 0 & 0 \\ 0 & 0 & 0 & 1 & 0.05 & 0 & 0 & -1 \\ 0 & 0 & 0 & 0 & 0 & 1 & -2 & 0 \\ 0 & 0 & 0 & 0 & 0 & 1 & -0.75 & 1 \\ 0 & 0 & 0 & 0 & 0 & -0.25 & -0.75 & 1 \end{bmatrix}$$

Together with prior means, we need to specify the standard deviation of the t-distributions of the a_{ij} elements, σ_{ij} , reflecting how confident or uncertain we are about the above specified prior means. We specify prior scales that reflect somewhat more confidence on the a -coefficients highlighted in blue, setting them equal to 0.3. With respect to the inflation target process, we remain quite uninformative about the way it interacts with the other variables in the VAR system, following the logic outlined in the closed-economy setting in Lukmanova and Rabitsch (2023). This is because the inflation target shock is our object of interest, and also because the inflation target measure used in the VAR should be viewed as reflecting the variation in the (true) target itself, but potentially also other variations (due to observation, measurement or estimation errors of our target proxy). We thus set prior scales of a -coefficients highlighted in green, as well as for all cross-border relations and coefficients of the exchange rate relation, equal to 0.6, reflecting our decision to be more uninformative there. Table 2 below summarizes all prior choices on the elements of the $\mathbf{A}$.

Table 2. Prior Parameters of the T-Distribution on the Elements of the *A*-Matrix

Parameter	Prior Mode	Prior Scale	Truncation
<i>Inflation Target Relation</i>			
a_{11}	1	0.3	>0
a_{12}	0	0.6	
a_{13}	0	0.6	
a_{14}	0	0.6	
<i>Supply Relation</i>			
a_{21}	0	0.6	
$\{a_{22}, a_{66}\}$	1	0.3	≥ 0
$\{a_{23}, a_{67}\} = -\alpha^S$	-2	0.3	≤ 0
$\{a_{24}, a_{68}\}$	0	0.3	
<i>Demand Relation</i>			
a_{31}	01	0.6	
$\{a_{32}, a_{76}\}$	1	0.3	≥ 0
$\{a_{33}, a_{77}\} = -\beta^D$	-0.75	0.3	
$\{a_{34}, a_{78}\} = -\gamma^D$	1	0.3	≥ 0
<i>Nominal Interest Rate Relation</i>			
$a_{41} = (1 - \rho_R)\rho_\pi$	0.75	0.6	
$\{a_{42}, a_{86}\} = -(1 - \rho_R)\rho_Y$	-0.25	0.3	≤ 0
$\{a_{43}, a_{87}\} = -(1 - \rho_R)\rho_\pi$	-0.75	0.3	≤ 0
$\{a_{44}, a_{88}\}$	1	0.3	≥ 0
<i>Exchange Rate Relation</i>			
a_{54}	1	0.6	
a_{58}	-1	0.3	
$a_{55} = \rho_{ex}$	0.05	0.3	

Next, let us turn to discussing additional prior information we include on the sign of impulse responses by imposing structure on the elements of the *H*-matrix using an asymmetric t-distribution as proposed in BH2018. We center prior means of the responses of domestic (foreign) output and inflation responses such that they are negative in response to a domestic (foreign) nominal interest rate increase,

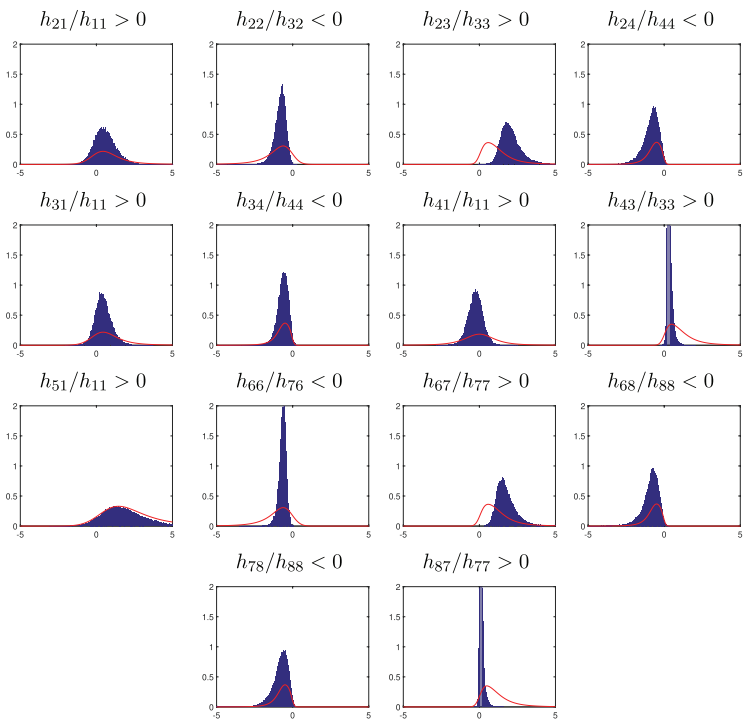
i.e., $h_{24}/h_{44} < 0$, $h_{34}/h_{44} < 0$ ($h_{68}/h_{88} < 0$, $h_{78}/h_{88} < 0$). We also suggest that domestic (foreign) output growth and the nominal interest rate respond positively to a domestic (foreign) demand shock, i.e., $h_{23}/h_{33} > 0$, $h_{43}/h_{33} > 0$ ($h_{67}/h_{77} > 0$, $h_{87}/h_{77} > 0$), and that domestic (foreign) inflation responds negatively to a domestic (foreign) supply shock, i.e., $h_{22}/h_{32} < 0$ ($h_{66}/h_{76} < 0$). Additionally, we center the output growth, inflation, and nominal interest rate responses to a domestic inflation target shock around zero, but with positive skew. Similarly, we include prior information on the exchange rate, with positive prior mean (appreciation) to a domestic nominal interest rate shock and negative prior mean (depreciation) to a domestic inflation target. It is important to emphasize that the prior information on the impacts of shocks (prior information on elements of the $\mathbf{H}$ -matrix) does not dogmatically impose a sign on the equilibrium impacts, but instead allows us to provide additional structure, which is useful in identifying the structural shocks, especially because many of the coefficients of the $\mathbf{A}$ -matrix have not been precisely specified. That is, further information on the $\mathbf{H}$ -matrix allows us to incorporate prior beliefs about the magnitude and signs of equilibrium impacts in a nondogmatic way. Figure 1 below summarizes the imposed prior information on the $\mathbf{H}$ -matrix graphically.

Table 3 summarizes the prior and posterior probabilities that the response of a specified structural shock on the indicated variable is positive at horizons $h = 0, 1$, and 2 . To save space, and because our focus is on the international spillovers of U.S. shocks, we do this for the domestic inflation, supply, demand, and nominal interest rate shocks only, but we look at the signs of domestic and foreign variables. Comparing prior and posterior probabilities of variables' responses being positive, we conclude that most of the implications of our choices for the impact responses, or for the $\mathbf{A}$ -matrix, are in line with the economic intuition discussed above.

4. Results: Baseline Model

This section presents results of our baseline empirical model, described in the previous section, by comparing impulse responses to the two types of monetary shocks. Throughout this section, we report mean impulse responses and 68 percent confidence bands for

Figure 1. Distribution of the Coefficients of the H -matrix



Note: Asymmetric t-distributions representing priors for the coefficients of impact response. Red line: prior distribution, blue histogram: posterior distribution.

the foreign economy (black solid and black dashed lines) alongside U.S. responses (blue lines and blue shaded areas). In our baseline model we use data from 1974:Q1 to 2019:Q1.

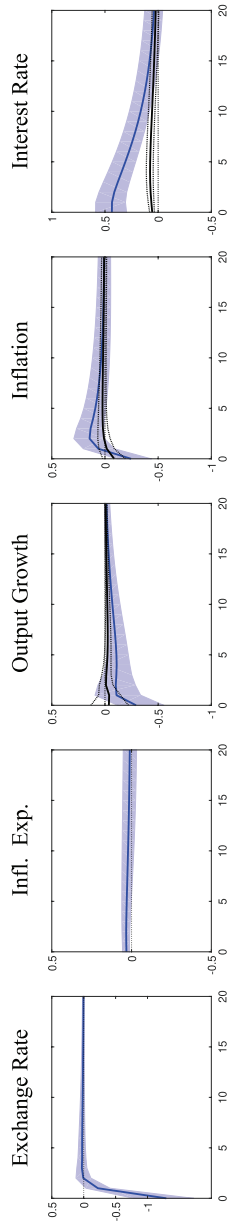
4.1 International Effects of a Shock to the U.S. Nominal Interest Rate

We start with the standard nominal interest rate shock, as its effects are well-documented in the literature (see Section 2). Figure 2 displays aggregate impulse responses to a nominal interest rate shock of one standard deviation in the U.S. Domestically, the effects are standard: a contractionary nominal interest rate shock translates into a

Table 3. Probabilities of Impulse Response Being Positive

	U.S Inflation Target Shock		U.S. Supply Shock		U.S. Demand Shock		U.S. Nominal Rate Shock	
Variable	Prior	Posterior	Prior	Posterior	Prior	Posterior	Prior	Posterior
$h = 0$								
π_t^*	0.99	1.00	0.51	0.10	0.66	0.95	0.29	0.95
Δy	0.77	0.76	0.71	1.00	0.98	1.00	0.02	0.01
π	0.78	0.80	0.02	0	0.99	1.00	0.05	0.01
i	0.22	0.28	0.14	0.22	0.93	1.00	0.99	1.00
Δex_t	0.87	0.94	0.58	0.69	0.59	0.61	0.04	0.01
Δy^*	0.49	0.60	0.52	0.71	0.52	0.81	0.46	0.42
π^*	0.52	0.63	0.53	0.03	0.51	0.91	0.37	0.21
i^*	0.54	0.56	0.52	0.01	0.50	1.00	0.44	1.00
$h = 1$								
π_t^*	0.56	1.00	0.50	0.02	0.51	0.98	0.48	0.95
Δy	0.52	0.52	0.53	1.00	0.59	0.97	0.42	0.30
π	0.52	0.85	0.40	0	0.58	1.00	0.46	0.67
i	0.48	0.62	0.43	0.47	0.56	1.00	0.60	1.00
Δex_t	0.52	0.62	0.51	0.83	0.51	0.50	0.43	0.12
Δy^*	0.50	0.73	0.51	0.88	0.50	0.87	0.49	0.34
π^*	0.50	0.76	0.51	0.00	0.50	0.85	0.48	0.41
i^*	0.50	0.64	0.50	0.04	0.50	1.00	0.51	1.00
$h = 2$								
π_t^*	0.82	1.00	0.50	0.01	0.60	0.97	0.35	0.93
Δy	0.61	0.74	0.51	1.00	0.61	0.76	0.41	0.27
π	0.60	0.89	0.35	0	0.71	0.97	0.43	0.85
i	0.39	0.74	0.37	0.46	0.56	1.00	0.67	1.00
Δex_t	0.67	0.43	0.53	0.58	0.56	0.53	0.31	0.52
Δy^*	0.49	0.82	0.50	0.93	0.50	0.76	0.49	0.44
π^*	0.52	0.89	0.51	0.00	0.50	0.80	0.45	0.56
i^*	0.53	0.75	0.51	0.10	0.50	1.00	0.47	1.00
Note: Prior and posterior probabilities that the response of a specified structural shock on the indicated variable is positive at horizons $h = 0, 1$, and 2 .								

Figure 2. Impulse Responses to a Nominal Interest Rate Shock: Baseline Model



Note: Impulse responses to a U.S. nominal interest rate shock. Displayed are mean responses and 68 percent confidence intervals. Blue solid lines and blue shaded areas show U.S. responses, black solid and dashed lines show responses of foreign variables. Baseline model with perceived inflation target rate (PTR) measure from the FRB/US model (see Brayton, Laubach, and Reifschneider 2014). Sample: 1974:Q1 to 2019:Q1. Horizontal axis: periods after the shock. Vertical axis: percentage change.

corresponding increase in the real rate, which leads to a decline in output growth and a drop in inflation. Internationally, the U.S. positive nominal interest rate shock leads to no significant change in foreign output growth, a mild decrease in foreign inflation, and an increase in the foreign nominal interest rate, together with a dollar appreciation.

Several aspects of these results deserve further mentioning. One, our results suggest that in response to the shock foreign variables tend to move in the same direction as U.S. variables. This is consistent with much of the literature, which finds that foreign variables react to U.S. monetary policy shocks with the same sign as U.S. variables.

Two, we find that the spillover effects of nominal interest rate shocks in Figure 2 tend to be somewhat smaller in magnitude compared to some earlier studies, even though a direct comparison is difficult, as the size of the international spillover of nominal interest rate shocks may be affected by our time samples and precise model specification.¹¹ Table A.1 in Appendix A provides a more detailed discussion of results on international spillovers from the literature and the various modeling frameworks used. Our sample period of 1974–2019 differs substantially from the existing literature’s earlier starting and ending dates, before the crisis. Moreover, our model specification is often different from the existing studies, in that we include foreign macroeconomic variables explicitly in our model—as, on the one hand, our interest is in the size of international macroeconomic spillovers and not exchange rate behavior only and, on the other hand, in understanding the international transmission channel of U.S. inflation target shocks and inflation spillovers. Finally, our identification strategy is different, as we aim to account for the effects of both temporary and persistent U.S. monetary shocks, which can

¹¹Studies using sign restrictions to identify the nominal interest rate shock, for example Scholl and Uhlig (2008), report insignificant international spillovers (see the working paper version). Kim, Moon, and Velasco (2017) point to a structural break in international spillovers after the Volcker era: the exchange rate reaction is smaller and has no delayed overshooting behavior. In a global VAR model, Georgiadis (2016) reports a maximum response of real GDP of -0.15bp after a 100bp contractionary monetary policy shock. As Georgiadis (2016) uses sign restrictions to identify the shock, the trough response should lie on the border of the confidence interval, making the finding consistent with this paper.

change the results of the nominal interest rate shock as compared to the specification with the temporary monetary shocks only.

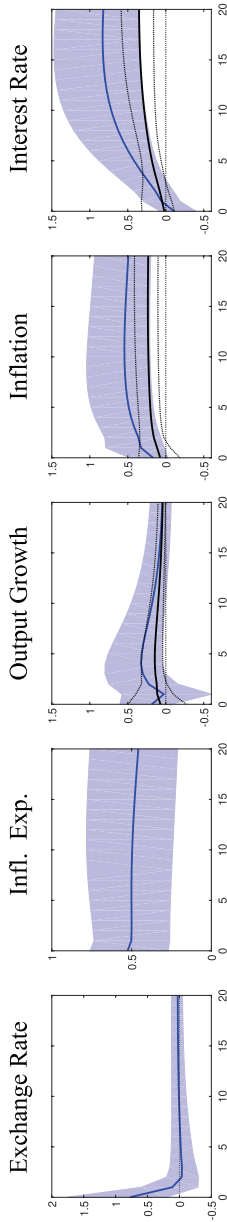
Three, in response to the contractionary U.S. nominal interest rate shock, the nominal exchange rate tends to appreciate on impact. The peak response appears to be on impact of the shock, i.e., the data do not support the overshooting result. This finding is consistent with Inoue and Rossi (2019). Our measure of the inflation target is unaffected by the nominal interest rate shock. Given the estimation sample, during which central banks in advanced countries implemented consistent policies toward inflation anchoring, we do not expect the long-run inflation expectations to react to short-lived monetary policy shocks, like a shock to the nominal interest rate.

4.2 International Effects of the U.S. Inflation Target Shock

Now we move on to the persistent monetary policy shock. To give some intuition, this shock can be thought of as a shock to the perceived inflation target that increases the inflation goal of the Federal Reserve in the medium to long run. It propagates into the economy by moving long-run inflation expectations in the U.S. upward. This, in turn, pushes the real rate down, stimulating aggregate demand, creating upward pressure on prices, and thus driving up actual inflation. The central bank responds in a Taylor rule logic by increasing the policy rate to cool down prices and demand. Figure 3 displays impulse responses to this shock in the baseline model. At home (responses in blue on Figure 3), the inflation target shock leads to persistent positive effects on long-run inflation expectations. In response, domestic demand increases and we observe increases in output growth and actual inflation.¹² The Fed gradually increases the policy rate to prevent the economy from overheating and to

¹²The response of U.S. output growth on impact of a positive inflation target shock is insignificant and it turns significantly positive several periods after the shock. This is in line with the empirical evidence on the effects of inflation target shocks in a closed-economy VAR setup estimated on U.S. data in Lukmanova and Rabitsch (2023). The authors show that the delay in output effects is due to the fact that economic agents need time to adjust their expectations and, as a result, their economic behavior to increase consumption demand. Gradually, this leads to higher economic growth.

Figure 3. Impulse Response to an Inflation Target Shock: Baseline Model



Note: Impulse response to an inflation target shock. Displayed are mean responses and 68 percent confidence intervals. Blue solid lines and blue shaded areas show U.S. responses, black solid and dashed lines show responses of foreign variables. Baseline model with perceived inflation target rate (PTR) measure from the FRB/US model (see Brayton, Laubach, and Reifschneider 2014). Sample: 1974:Q1 to 2019:Q1. Horizontal axis: periods after the shock. Vertical axis: percentage change.

control inflation. Overall, the shock leads to expansionary effects in the U.S., which is consistent with the closed-economy literature on the effects of U.S. inflation target shocks (DeMichelis and Iacoviello 2016; Mumtaz and Theodoridis 2018; Uribe 2022; Lukmanova and Rabitsch 2023).

Internationally, we find that the shock spills over abroad. On average, foreign countries experience an increase in inflation and nominal rates, following a similar pattern to U.S. variables. In the aftermath of the inflation target shock in the U.S., foreign countries are confronted with permanently higher inflation from abroad (U.S.), thus facing a threat of importing this higher inflation. However, on impact of the shock, foreign central banks do not raise interest rates to counteract U.S. inflation (black line on the last subfigure of Figure 3 is insignificant from zero). In other words, even when U.S. inflation is already significantly above zero, foreign central banks do not increase policy rates in time to prevent importing higher inflation from the U.S. This leads to an increase in inflation expectations outside the U.S. and, due to an insufficient monetary policy response abroad, foreign real rates drop. This, in turn, leads to expansion-like effects abroad: foreign inflation goes up, as does foreign output growth (although, just as in the case of the U.S. economy, we do not observe the output increase on impact of the shock, but rather with some delay).

An additional amplification channel comes via exchange rates. We observe a strong dollar depreciation resulting from a shift away from domestic assets as the uncovered interest rate differential, $i_t - i_t^* - E_t(ex_{t+1} - ex_t)$, declines.¹³ Note that the variable included in the VAR is the change in the nominal exchange rate, so that even though the change quickly converges back, the nominal exchange rate depreciates to a higher level. The initial depreciation can be understood as the result of stronger inflation reactions in the U.S. compared to abroad, and a negative on-impact response of the nominal rate, in contrast to the near zero-impact response of the foreign nominal rate. A few periods after the shock, it appears that the downward pressure caused by the higher inflation rate in the U.S. compared to other countries is more or less balanced by the upward

¹³Schmitt-Grohé and Uribe (2022) report a similar exchange rate response to their permanent nominal interest rate shock.

pressure from the higher interest rates in the U.S. as opposed to those abroad.

Our findings are consistent with the related literature. Using a structural open-economy DSGE model, Chin et al. (2018) also show that an inflation target shock originating in the U.S. leads to expansionary effects domestically as well as abroad. They show that an important element is an insufficient reaction of the foreign central bank to permanently higher inflation in the U.S. We confirm this using a purely empirical setup using data for the U.S. and other advanced economies (in contrast, Chin et al (2018) focus on U.S.–U.K. transmission only). Inoue and Rossi (2019) also emphasize a real rate and an inflation expectations channel as important for the international transmission of monetary policy shocks. Our results emphasize that inflation expectations abroad go up due to a weak response of the policy rate. This, using the logic of the Fisher equation, leads to a decline in the real rate abroad, triggering further real and nominal effects in foreign economies.

Trend inflation and interest rates (long- and short-term) have gone down more or less at the same time in advanced economies since the mid-1980s, as central banks switched toward (implicit or explicit) inflation targeting regimes. One interpretation could be that the global reorientation of monetary policy was a reaction to the Federal Reserve leading the way. At the same time, there are global common trends in low-frequency inflation dynamics, and it is possible to speculate about the degree to which these are the result of this global reorientation toward inflation targeting regimes or a reaction to the Fed leading the way. Our structural inflation target shock can be interpreted as a shock to global long-run inflation, proxied by the U.S. (leading the way), providing more structural and theory-based evidence behind the comovement in interest rates as we attempt to rationalize the common trends in inflation. With this, we do not exclude the possibility that there are other factors driving the inflation comovement across countries. However, we show that U.S. inflation target shocks are one of the reasons behind it.

Finally, compared to the effects of the U.S. nominal interest rate shock, international spillovers stemming from the inflation target shock are more persistent: inflation expectations, inflation, and nominal rates are away from the steady state even 20 quarters after the inflation target shock. The nature of the two shocks is different too:

while both types of shocks lead to an increase in U.S. interest rates, only the inflation target shock leads to an expansion in the U.S. These differences are the result of the relative movements in inflation expectations and real rates: while the inflation target shock increases inflation expectations, leading to a decrease in the real rate, given an insufficient reaction from the central bank, the nominal interest rate shock increases the real rate due to nominal rigidities pushing inflation expectations down. Internationally, these dynamics are largely mirrored as foreign economies tend to move in tandem with the U.S. Thus, our results suggest that U.S. inflation target shocks are important for understanding dynamics of economic variables not only in the U.S., but also abroad.

4.3 *Robustness*

We provide impulse responses to U.S. monetary policy shocks generated from two variations of our estimated model as robustness exercises. In the first variation, we evaluate the effect of a change in the time sample used. The choice of sample period might be particularly relevant for the size of international spillovers, as many of the earlier studies employ data that end before the Global Financial Crisis (GFC) of 2007–08. Until after the GFC, the time series of inflation or nominal interest rates are not subject to a lot of fluctuations. On the one hand, central banks were focused on stabilizing and anchoring inflation. On the other hand, the aftermath of the GFC was associated with the period of a binding zero lower bound on the nominal interest rate. In particular, the federal funds rate was pushed to nearly zero for about eight years, a substantial fraction of our sample, during which the Federal Reserve was conducting unconventional monetary policies, mainly aiming at decreasing interest rates at longer maturities, targeting term spreads. The specification of our baseline model does not provide a channel to account for the effects of unconventional monetary policy, again leading to a potential downplaying of the size of international spillovers in our sample. As a consequence, we report impulse responses from a model estimated over the time sample 1974:Q1–2008:Q2, summarized by Figures B.1 and B.2 in Appendix B.1. The results are robust to the estimation sample: excluding data after the GFC still leads to similar

effects of the U.S. nominal interest rate shock (Figure B.1) and inflation target shock (Figure B.2). We interpret this as having enough information in the precrisis sample to inform our model on the effects of U.S. monetary policy shocks and their international transmission. In the next section, we extend the model to include long-term rates to bring in additional information on financial markets.

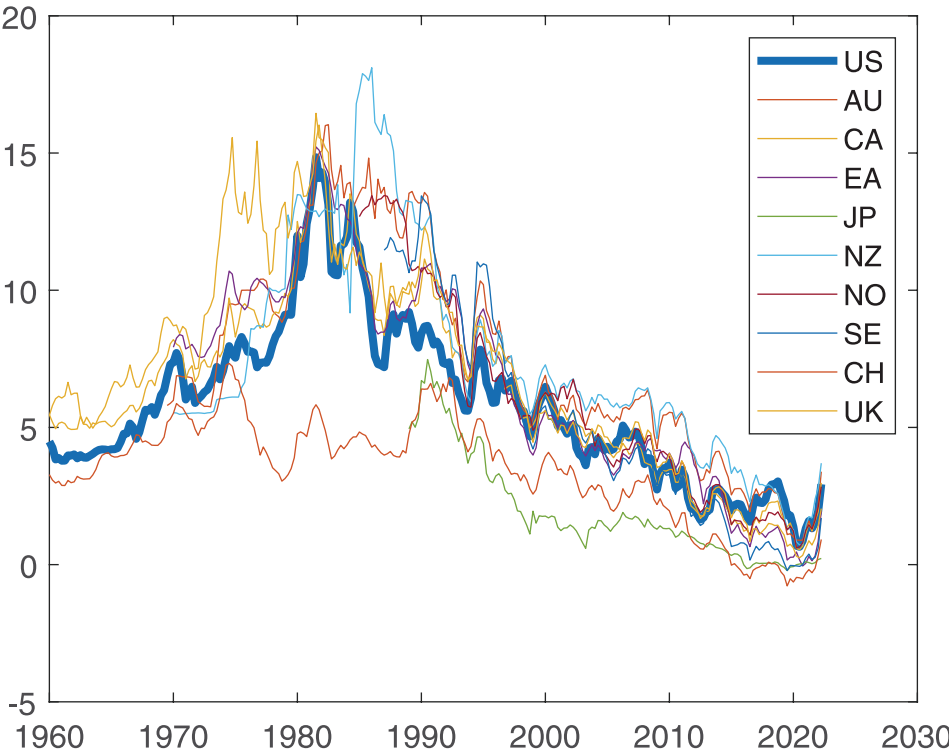
In the second variation, we present impulse responses to a U.S. nominal interest rate shock when the variable capturing the U.S. inflation target is dropped, and only the other seven macro time series are used, reported in Figure B.3 in Appendix B.2. The impulse responses to the nominal interest rate shock are similar to the baseline model, yet the foreign output growth response is positive, while the foreign inflation response is insignificant. We read this as evidence that (i) long-run inflation expectations bring important additional information into the system, and (ii) separating the effects of temporary and persistent monetary policy shocks helps to identify the effects of the former.

5. Results: Extended Model with Long-Term Rates

In this section we extend our model to include long-term interest rates. As is evident from Figure 4, and as discussed in the literature (e.g., Wright 2011), long-term interest rates strongly comove across advanced countries. Our baseline model results indicate that U.S. inflation target shocks lead to comovement in short-term rates. Here, we investigate if U.S. inflation target shocks also lead to comovement in long-term rates. Using an open-economy DSGE model, Chin et al. (2018) show that inflation target shocks originated in the U.S. can cause U.S.–U.K. long-term rates to comove. We tackle this question using an empirical VAR model including not only U.S.–U.K. but other advanced countries as well. With our identification strategy, which takes into account uncertainty about the effects of the inflation target shock at home and abroad, we do not need to impose a structure on the effects of the inflation target shock. Rather, we let the data speak. To do so, we extend the vector of observed data to include the long-term rates such that:

$$\mathbf{x}_t = [\hat{\pi}_t, \Delta y_t, \pi_t, i_t, i_t^L, \Delta ex_t, \Delta y_t^*, \pi_t^*, i_t^*, i_t^{L,*}],$$

Figure 4. Long-Term Rates in Advanced Countries



Note: This figure plots 10-year government bond yields across advanced countries. The data start in 1960:Q1 (subject to availability for a particular country) and continue until 2022:Q2.

where i_t^L is the long-term rate that we measure as the return on 10-year government bonds. We use the same set of identifying restrictions (no additional restrictions are placed on long-term rates, as we are interested in their responses). In particular, we impose uninformative t-distributed priors with zero mean on the coefficients of the **A**-matrix governing responses of long-term rates. We do not impose any additional restrictions on the **H**-matrix that would guide the responses of long-term rates. At the same time, all previous prior restrictions remain. That is, with respect to the inflation target shock we expect that it causes an increase in our measure of long-term

inflation expectations and we let the data determine corresponding effects on other variables in the model, both domestically and abroad.

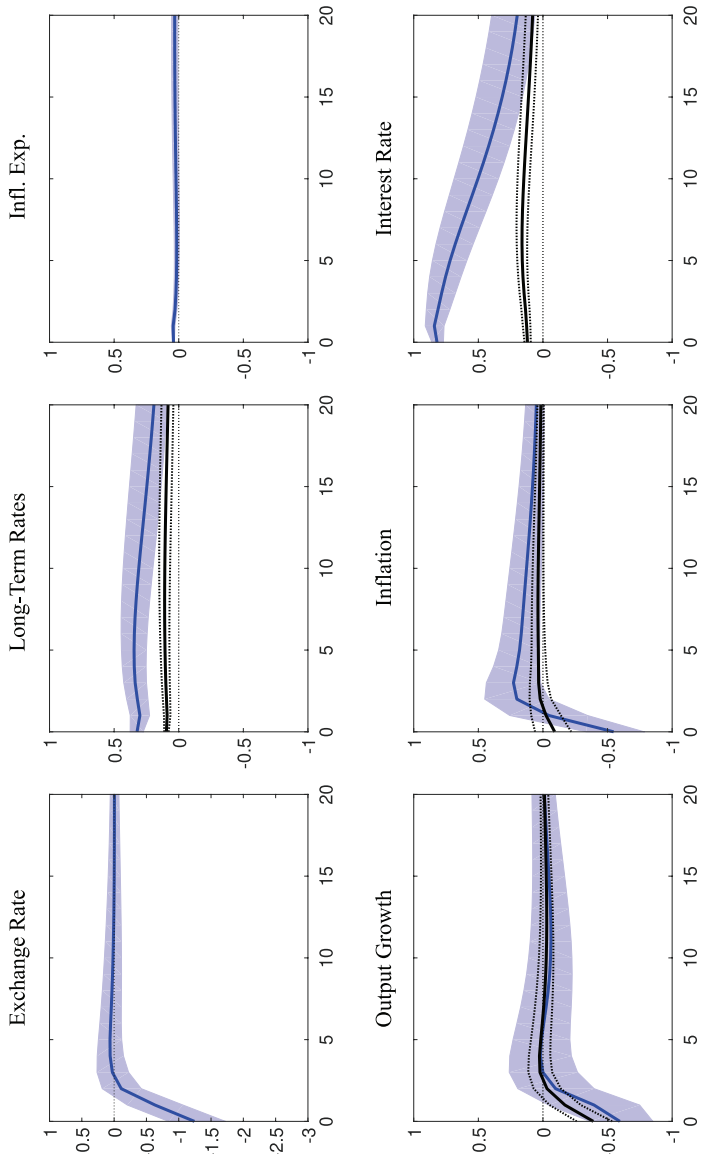
Results of the estimation of the extended model are reported in Figures 5 and 6. The inclusion of long-term rates helps to identify the effects of both temporary and persistent monetary shocks: the estimated impulse responses from the extended model have narrower confidence bands. A number of studies (Krishnamurthy and Vissing-Jorgensen 2012; Passari and Rey 2015; Rey 2016; Chin et al. 2018) point to an important role of adjustments in the term premium in determining the international transmission of monetary policy. The term premium is the difference in returns between the long-term bonds and average short-term bonds across the maturity of the long-term bond. Information from long-term rates gives us exactly this: it allows us to take into account the relative dynamics of short-term and long-term interest rates determined in financial markets. This is especially important with respect to the past decade, when short-term rates were bound at zero.¹⁴ The unconventional monetary policies, such as quantitative easing, employed by major central banks during this period targeted the spread between short- and long-term rates. Including long-term rates helps us to account for this information.

For the nominal interest rate shock (Figure 5), information from long-term rates helps to pin down the response of foreign output, making it significantly negative, all else equal. While both domestic and foreign long-term rates increase in response to the shock, quantitatively the response of foreign long-term rates is very small.

Instead, in response to the U.S. inflation target shock, we also observe that both domestic and foreign output react positively, with responses significantly above zero. This serves as a further proof of the international transmission channel of the U.S. inflation target shock: the shock lifts up inflation expectations in the U.S., creating persistent domestic inflation. Foreign central banks do not react sufficiently to persistently higher U.S. inflation, as is evident from the response of the foreign short-term rate: it goes down on impact.

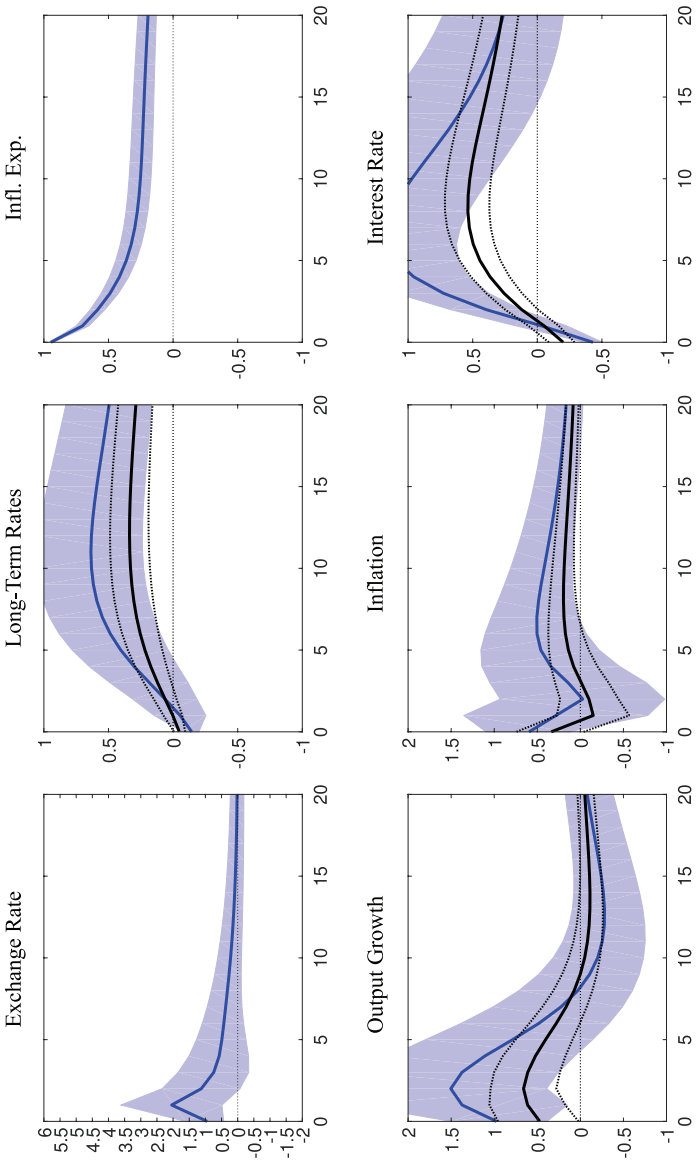
¹⁴For Japan, the binding zero lower bound period includes almost the whole estimation sample.

Figure 5. Impulse Response to a Nominal Interest Rate Shock: Extended Model



Note: Impulse response to a U.S. nominal interest rate shock. Displayed are mean responses and 68 percent confidence intervals. Blue solid lines and blue shaded areas show U.S. responses, black solid and dashed lines show responses of foreign variables. Baseline model with perceived inflation target rate (PTR) measure from the FRB/US model (see Brayton, Laubach, and Reifschneider 2014). Sample: 1989:Q1 to 2019:Q1. Horizontal axis: periods after the shock. Vertical axis: percentage change.

Figure 6. Impulse Response to an Inflation Target Shock: Extended Model



Note: Impulse response to an inflation target shock. Displayed are mean responses and 68 percent confidence intervals. Blue solid lines and blue shaded areas show U.S. responses, black solid and dashed lines show responses of foreign variables. Baseline model with perceived inflation target rate (PTR) measure from the FRB/US model (see Brayton, Laubach, and Reifschneider 2014). Sample: 1989:Q1 to 2019:Q1. Horizontal axis: periods after the shock. Vertical axis: percentage change.

This creates enough inflationary pressure from abroad to push foreign real rates down so that we observe an increase in foreign demand and foreign output going up. Eventually, foreign inflation also picks up, prompting central banks to increase short-term rates. Importantly, we observe a strong comovement in long-term rates across the U.S. and other advanced economies: they move nearly one-to-one in response to the U.S. inflation target shock. Thus, we conclude that the comovement of long-term rates across advanced economies can be explained by a rise in inflation expectations caused by a positive inflation target shock originated in the U.S.

Finally, once we include long-term rates, the response of exchange rates changes, but only in response to the inflation target shock. We still observe a dollar depreciation, but it peaks not on impact but rather one to two periods after the shock. In the extended model, domestic and foreign inflation reactions are almost the same. As time passes, U.S. inflation increases faster than foreign inflation and the Federal Reserve increases the interest rate more strongly compared to foreign central banks, all of which culminates in the peak reaction of the exchange rate about two quarters after the shock itself. Soon after, as foreign interest rates rise and U.S. inflation stabilizes persistently above foreign inflation, the change in the exchange rate goes back to the steady-state value.

6. Conclusions

This paper proposes a simple empirical model to estimate international effects of U.S. monetary policy shocks, allowing for both a nominal interest rate shock and an inflation target shock. We employ a novel identification methodology from Baumeister and Hamilton (2018) that allows us to distinguish between the two shocks. Our sample covers eight advanced economies and the U.S., with a baseline sample period of 1974:Q1 to 2019:Q1.

We find sizable international spillovers from the U.S. inflation target shock. At home the shock has persistent expansionary effects, through an increase in inflation expectations and a decrease in the real rate, followed by an increase in domestic consumption demand and inflation. Foreign countries import persistently higher inflation from the U.S. and, given an insufficient response of foreign central banks, inflation expectations abroad rise as well, also decreasing

the foreign real rate. As a result, we observe expansionary effects abroad as well as at home, and a strong dollar depreciation from U.S. inflation target shocks.

We use the empirical evidence on the effects of U.S. inflation target shocks to take a step toward explaining the comovement in long-term rates across advanced countries that we observe in the data. In particular, as we separate the effects of temporary and persistent monetary policy shocks, we show that it is the persistent inflation target shock that generates long-lasting comovement in long-term rates between the U.S. and foreign countries.

Appendix A. Selected Literature on International Effects of U.S. Nominal Interest Rate Shocks

Table A.1. Selected Literature on International Spillovers of U.S. Monetary Policy Shocks

Paper	Time and Country Sample	Identification	Model Specification	Spillover Effects
Kim (2001)	1974–96, U.S. + G6	Cholesky decomposition. Monetary policy shock – shock to federal funds rate or to ratio of nonborrowed reserves to total reserves.	U.S. model: Real GDP, inflation, commodity price (PC), federal funds rate (and ratio of nonborrowed reserves to total reserves). International variables: trade balance, exports, imports, nominal exchange rate, terms of trade, real GDP, industrial production, inflation.	Expansionary MPS: nominal exchange rate depreciates, boom in foreign economies with an increase in real GDP by 0.05–0.2 percent (compared to 0.2–0.4 percent in the U.S.) explaining about 4–4.3 percent of foreign GDP.
Scholl and Uhlig (2008)	1990–2002, U.S. + Germany, Japan, U.K., and G7 aggregate	Sign restrictions for contractionary MPS (horizon of one year): U.S. interest rates do not fall, prices and the ratio of nonborrowed to total reserves do not rise.	U.S. and foreign industrial production, U.S. and foreign three-month interest rates, the U.S. ratio of nonborrowed to total reserves, the U.S. consumer price index and the nominal exchange rate.	Contractionary MPS: WP version reports small negligible effects on foreign output, not significantly different from zero.
Dedola, Rivolta, and Stracca (2017)	1981–2013, 36 countries	Estimate shocks from large-scale Bayesian VAR with U.S. data, sign restrictions: interest rate positive, output and inflation negative.	Quarterly data: foreign real GDP, GDP deflator, unemployment rate, real house prices, real domestic credit, total portfolio, total bank inflows.	Report aggregated IRFs. Foreign inflation's reactions are of little significance.

(continued)

Table A.1. (Continued)

Paper	Time and Country Sample	Identification	Model Specification	Spillover Effects
Bluedorn and Bowdler (2011)	1974–2001, U.S. + G7	Identification as in Romer and Romer (2004): In the first step, determine intended changes in the U.S. target federal funds rate announced during the FOMC meetings. In the second step, these federal funds rate changes are decomposed into two components, one that can be explained by the Federal Reserve’s Greenbook forecasts, and one that cannot be explained by those forecasts. The second component defines the monetary policy shock.	U.S. output, prices and bank reserves, exchange rate, foreign interest rates, and foreign output.	Contractionary MPS: Foreign output shows a mixed initial response which becomes negative after four years. The U.S. price responses show a price puzzle. The price response depends critically on the number of lags of the policy shock included in the model. As the number of policy shock lags is increased, the price puzzle disappears while preserving many of the other impulse responses.
Kim, Moon, and Velasco (2017)	1974–2006, 12 U.S. trading partners from Europe, Canada, and Japan	Sign restrictions: prices and nonborrowed reserve ratio are required not to rise and interest rates are required not to fall for one year in response to a U.S. contractionary monetary shock. They find that identification via Cholesky leads to a persistent price puzzle, which is the reason why they do not study results obtained via this identification.	U.S. and foreign industrial production, U.S. and foreign three-month interest rates, the ratio of U.S. nonborrowed to total reserves, the U.S. consumer price index, and the real exchange rate.	Delayed overshooting is a phenomenon of the 1980s. UIP fails to hold during the Volcker era and tends to hold in the other periods considered. U.S. monetary policy shocks have substantial impacts on exchange rate variations but misleadingly appear to have small impacts when monetary policy regimes are pooled.
Georgiadis (2016)	1999–2009, 61 countries	Sign restrictions: short-term interest rates rise on impact, inflation turns negative after four quarters.	GVAR model, for each economy include: output, prices, bilateral nominal exchange rates, oil prices.	Figure 2 presents maximum response of real GDP. For countries included in our paper, the maximum response reported by Georgiadis (2016) is a 0.15 bp decrease after a contractionary MPS of 100 bp.

(continued)

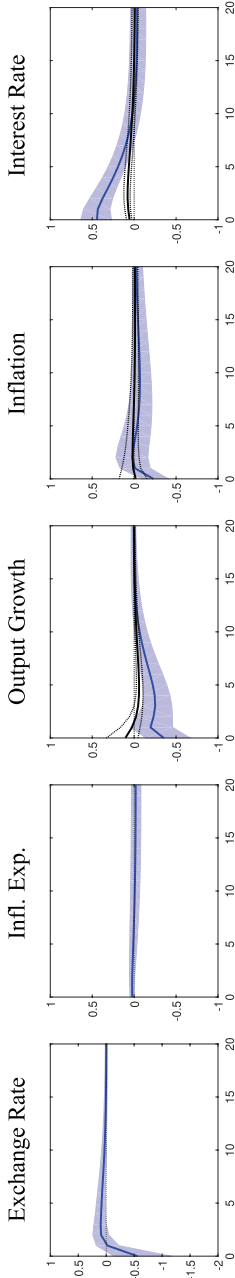
Table A.1. (Continued)

Paper	Time and Country Sample	Identification	Model Specification	Spillover Effects
Faust et al. (2003)	1974–2001, U.S.-U.K., U.S.-Germany	Identification sign restrictions from high-frequency data: a surprise loosening of monetary policy cannot lower output (foreign or domestic), prices, or NBRX contemporaneously, it cannot cause the dollar to appreciate contemporaneously and it cannot cause foreign interest rates to rise contemporaneously.	Domestic and foreign output, U.S. prices (measured as the CPI), the three-month U.S. and foreign interest rates, the ratio of nonborrowed reserves to total reserves in the U.S., and the exchange rate.	Expansionary MPS: foreign output contemporaneous reaction is weak around zero, increases and is positive 20 months after the shock, between 0.05 and 0.4 bp. Foreign nominal interest rate is between -0.2 and 0 bp on impacts, decreasing and is negative after the first year, between -0.3 and -0.1 bp.
Faust and Rogers (2003)	1974–97, U.S.-U.K., U.S.-Germany	Sign restrictions.	Standard seven-variable model (U.S. and foreign output, the U.S. consumer price index, U.S. and foreign short-term interest rates, the ratio of U.S. nonborrowed reserves to total reserves, and the exchange rate), and a new 14-variable model.	Expansionary MPS: foreign output increases by 0.2 and 0.1 bp depending on identification, foreign prices drop by 0.1 and 0 on impact, positive (about 0.1 bp) after four years.

Appendix B. Robustness Checks

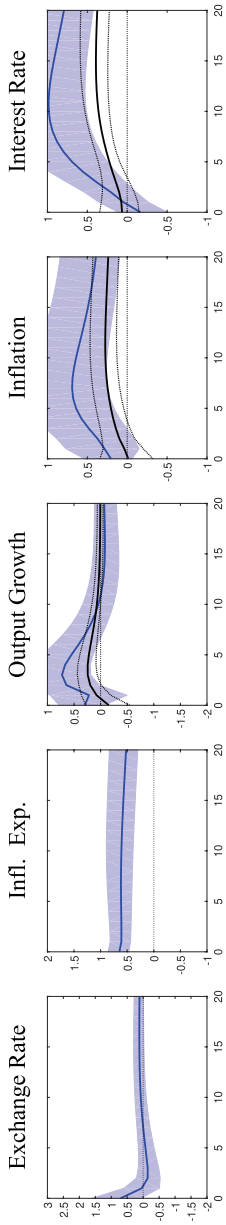
B.1 *Excluding the Binding Zero Lower Bound Period*

Figure B.1. Impulse Response to a Nominal Interest Rate Shock:
Excluding Zero Lower Bound Period



Note: Impulse responses to a U.S. nominal interest rate shock. The blue shaded area is the U.S. response; the black line shows the response of foreign variables. Baseline model with perceived inflation target rate (PTR) measure from the FRB/US model (see Brayton, Laubach, and Reifschneider 2014) when prior mode for the $\mathbf{A}$ -matrix is fully consistent with a four-equation DSGE model. The shaded area shows the 68 percent confidence interval and the blue dotted line the 90 percent confidence interval. Sample: 1974:Q1 to 2008:Q2. Horizontal axis: periods after the shock. Vertical axis: percentage change.

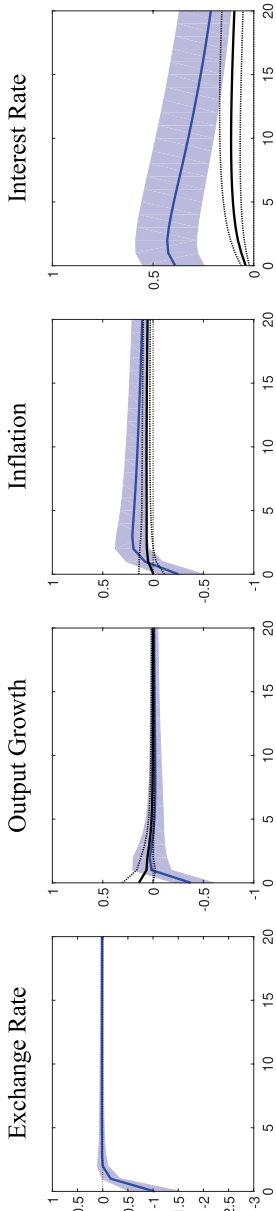
Figure B.2. Impulse Response to an Inflation Target Shock:
Excluding Zero Lower Bound Period



Note: Impulse responses to a U.S. inflation target shock. The blue shaded area is the U.S. response; the black line shows the response of foreign variables. Baseline model with perceived inflation target rate (PTR) measure from the FRB/US model (see Brayton, Laubach, and Reifschneider 2014) when prior mode for the $\mathbf{A}$ -matrix is fully consistent with a four-equation DSGE model. The shaded area shows the 68 percent confidence interval and the blue dotted line the 90 percent confidence interval. Sample: 1974:Q1 to 2008:Q2. Horizontal axis: periods after the shock. Vertical axis: percentage change.

B.2 *Seven-Variable Model Without Inflation Target Variable*

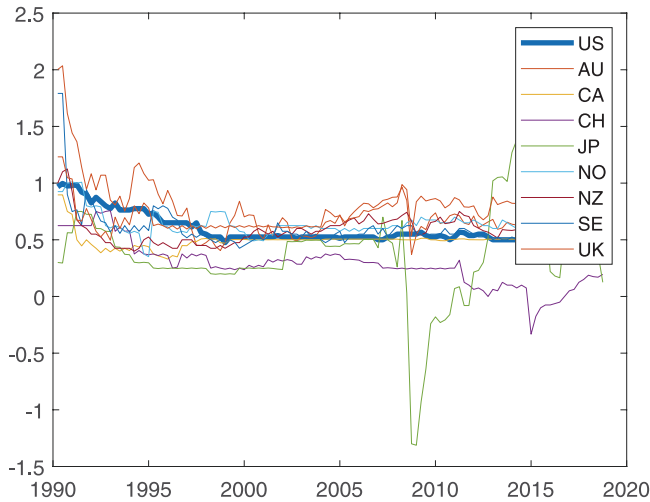
**Figure B.3. Impulse Responses to a Nominal Interest Rate Shock:
Seven-Variable Model without Inflation Target Variable**



Note: Impulse responses to a U.S. nominal interest rate shock. Displayed are mean responses and 68 percent confidence intervals. Blue solid lines and blue shaded areas are U.S. responses; black solid and dashed lines show responses of foreign variables. Reduced model with only seven variables, without inflation target variable. Sample: 1974:Q1 to 2019:Q1. Horizontal axis: periods after the shock. Vertical axis: percentage change.

Appendix C. Inflation Expectations in Advanced Countries

Figure C.1. Inflation Expectations in Advanced Countries



Note: This figure plots inflation expectations across countries from our sample. The data start in 1990:Q2 and continue until 2018:Q4. For the U.S., we use the PTR measure of long-run inflation (see Brayton, Laubach, and Reifschneider 2014). For Australia, we use 10-year inflation expectations. For Canada, we use 6–10 year inflation expectations. For Japan, Norway, Sweden, Switzerland, and the U.K., we use five-year inflation expectations (and 10-year forecasts from 2004 onward for the U.K.). For New Zealand, we use two-year inflation expectations. All data are from national sources.

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The Housing Supply Channel of Monetary Policy*

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We study the role of regional housing markets in the transmission of U.S. monetary policy. Using a FAVAR model over 1999:Q1–2019:Q4, we find that differences in housing supply constraints explain part of the regional heterogeneity in the responses of U.S. states to a contractionary monetary policy shock. Specifically, house prices and consumption respond more in supply-constrained states. Financial stability risks also increase more sharply in these areas as mortgage delinquencies and foreclosures surge, worsening banks' balance sheets. Our findings stress the importance of regional housing supply conditions in assessing the macrofinancial effects of rising interest rates.

JEL Codes: C23, E32, E52, R31.

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1. Introduction

Housing plays a central role for macroeconomic fluctuations: two-thirds of U.S. households own a home, housing consumption accounts for around one-third of total private consumption expenditures,¹ and mortgagors display a higher marginal propensity to consume (MPC) compared with renters or outright owners (Cloyne, Ferreira, and Surico 2020). In addition, housing is highly sensitive to interest rate changes, with an extensive literature indicating that monetary policy affects housing demand.² Recent studies have begun to explore how differences in housing supply elasticities—the supply response to demand-driven house price changes—affect the monetary policy transmission (Fischer et al. 2021; Aastveit and Anundsen 2022; Cooper, Luengo-Prado, and Olivei 2022; Aastveit, Albuquerque, and Anundsen 2023).³ We extend this emerging literature by highlighting the prominent role of housing supply in the transmission of monetary policy to the real economy.

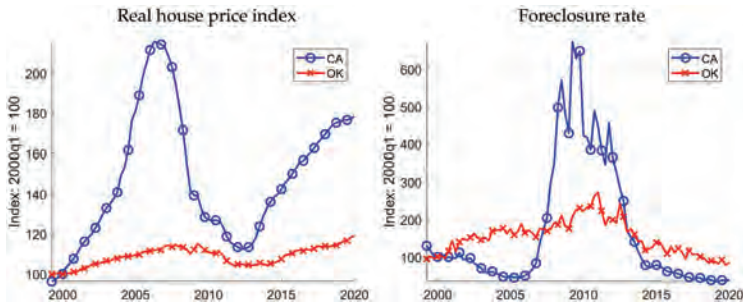
Understanding how housing supply affects the transmission of monetary policy shocks is particularly important in a context of rapidly rising interest rates. Higher interest rates may lead to a sharper decline in house prices in inelastic markets, resulting in greater consumption cuts and overall reduced economic activity. In addition, the decline in mortgagors' housing equity could heighten

¹According to the 2022 Survey of Consumer Finances and the 2023 Bureau of Labor Statistics (BLS) Consumer Expenditures Survey.

²First, monetary policy stimulates borrowing and consumption through the credit supply channel, where lower borrowing costs and higher inflation reduce the real value of debt (Jordà, Schularick, and Taylor 2015; Bhutta and Ringo 2021; Wong 2021). Second, the household balance sheet channel, or home equity loan/collateral channel, posits that monetary policy has important housing wealth effects, encouraging mortgagors to extract equity for consumption and investment (Iacoviello 2005; Del Negro and Otrok 2007; Jarociński and Smets 2008; Bhutta and Keys 2016; Aladangady 2017; Beraja et al. 2019; Cloyne, Ferreira, and Surico 2020; Garriga and Hedlund 2020; Andersen and Leth-Petersen 2021). Third, changes in mortgage-related costs can influence demand for real estate services, affecting economic activity (Best and Kleven 2018; Bhutta and Ringo 2021; Anenberg and Ringo 2022; Benmelech, Guren, and Melzer 2023).

³Other structural features of the housing market, such as the share of adjustable-rate mortgages (ARMs) and the homeownership rate, may also influence the regional effects of monetary policy on house prices (Calza, Monacelli, and Stracca 2013; Corsetti, Duarte, and Mann 2022; Pica 2023).

Figure 1. Real House Prices and Foreclosure Rate in California and Oklahoma



Note: Real house prices and the foreclosure rate are rescaled to an index equal to 100 in 2000:Q1.

financial stability risks in these low-supply-elasticity areas due to increased mortgage delinquencies and foreclosures.

Figure 1 illustrates this point. California—an area with inelastic housing supply—has experienced more pronounced boom-bust housing cycles over the last two decades compared with Oklahoma, where housing supply is more elastic. House prices in California surged in the run-up to the Global Financial Crisis (GFC), coinciding with a fast rise in mortgage debt. This was followed by a sharper decline as the credit crunch intensified, leading to a notable increase in foreclosure rates that exacerbated the initial drop in house prices (Huang and Tang 2012; Anundsen and Heebøll 2016; Chodorow-Reich, Guren, and McQuade 2024).

Against this background, we examine the regional heterogeneity in the transmission of contractionary monetary policy to the real economy, focusing on differences in housing markets across U.S. states, particularly housing supply elasticities—the *housing supply channel of monetary policy*. Our main contributions are three-fold. First, we trace out the effects of monetary policy not only on regional house prices—as explored in several studies (Fratantoni and Schuh 2003; Fischer et al. 2021; Aastveit and Anundsen 2022; Cooper, Luengo-Prado, and Olivei 2022; Aastveit, Albuquerque, and Anundsen 2023)—but also on the real economy and on financial variables. Second, we focus on how housing supply constraints across states influence the impact of contractionary monetary policy,

allowing us to assess regional macrofinancial effects based on housing supply differences. Third, we incorporate a rich set of housing market variables, including both owner-occupied and rental segments. In contrast to most of the related literature, this allows us to study how monetary policy affects households' housing tenure decisions, which may amplify or dampen the effects of monetary policy (Dias and Duarte 2019, 2022; Koeniger, Lennartz, and Ramelet 2022).

We estimate a factor-augmented vector autoregression (FAVAR) model (Bernanke, Boivin, and Elias 2005), using a comprehensive quarterly data set covering the 50 U.S. states over 1999:Q1–2019:Q4. The model captures dynamic relationships in the economy and includes several state-level, and U.S. aggregate, indicators related to the real economy, labor market, financial sector, public finances, and housing market. The housing market block has information on prices and quantities, including rental and house prices, building permits, housing starts, homeownership rates, and vacancy rates. We identify monetary policy shocks using high-frequency changes in the Federal Reserve's policy rate around Federal Open Market Committee (FOMC) announcements (Gürkaynak, Sack, and Swanson 2005; Gertler and Karadi 2015; Nakamura and Steinsson 2018). We then apply the exogenous variable approach (Paul 2020) to assess the effects of monetary policy shocks within the FAVAR model.

Our main findings are as follows. First, we find significant heterogeneity in the transmission of an aggregate U.S. contractionary monetary policy shock to the U.S. states. The heterogeneity is particularly pronounced among housing market variables, which aligns with recent findings in the literature (Fischer et al. 2021; Aastveit and Anundsen 2022; Cooper, Luengo-Prado, and Olivei 2022; Corsetti, Duarte, and Mann 2022; Koeniger, Lennartz, and Ramelet 2022; Aastveit, Albuquerque, and Anundsen 2023). As homeownership costs rise due to tighter monetary policy, house prices fall while rent prices increase in most states, indicating a shift in demand from the owner-occupied market to renting (Dias and Duarte 2019, 2022). Our contribution is to show that monetary policy can differentially affect housing tenure choices across states. For instance, declines in the house price-to-rent ratio vary widely, and some states experience minimal changes in homeownership rates while others see sharper declines. The response of housing supply may help

explain the differences across states in the shift from the owner-occupied to the rental market: home vacancy rates decline and rental vacancy rates increase in some states, diverging from aggregate U.S. trends.

Second, we find that differences in housing supply restrictions across states help explain the heterogeneity in responses to monetary policy. Using the state-level land-use restriction index (LRI) from Herkenhoff, Ohanian, and Prescott (2018) as our baseline measure of housing supply constraints, we compare how high- and low-regulation states respond to monetary policy. We find that house prices are more responsive in areas with tighter housing supply constraints, in line with recent evidence (Gyourko, Saiz, and Summers 2008; Saiz 2010; Glaeser et al. 2014; Herkenhoff, Ohanian, and Prescott 2018; Aastveit and Anundsen 2022; Cooper, Luengo-Prado, and Olivei 2022; Aastveit, Albuquerque, and Anundsen 2023; Albuquerque et al. 2026). Our contribution is to show that tighter housing supply constraints, which imply lower supply elasticities, not only affect house price responses to monetary policy but also have wider macrofinancial consequences. Specifically, states with more inelastic housing supply experience sharper declines in private consumption following a contractionary monetary policy shock, driven by a larger drop in housing wealth. In addition, financial stability risks rise more sharply in these areas, with higher rates of mortgage delinquencies, foreclosures, and a broader deterioration of the banking sector's health. Overall, our results suggest that the greater decline in consumption in states with restricted housing supply stems from collateral, credit supply, and local general equilibrium effects. This aligns with the transmission channels explaining the decline in U.S. household consumption due to falling house prices during the GFC (Aruoba, Elul, and Kalemli-Özcan 2026). Relatedly, our findings are consistent with those of Albuquerque et al. (2026), who show that countries with more restrictive housing supply constraints tend to experience stronger economic adjustments during housing contractions.

Third, household debt overhang also influences the heterogeneous effects of monetary policy: states with greater household debt imbalances are more sensitive to contractionary monetary policy shocks. Debt overhang can thus amplify business cycles (Schularick and Taylor 2012; Jordà, Schularick, and Taylor 2013, 2015; Dell'Ariccia

et al. 2016; Mian, Sufi, and Verner 2017; Albuquerque and Krustev 2018; Albuquerque 2019; Greenwood et al. 2022; Jordà et al. 2022). States with greater debt gaps often have less elastic housing supply, which may explain why our results depart from the conventional view that the response of house prices to contractionary demand shocks is independent of supply restrictions (Glaeser and Gyourko 2005; Glaeser, Gyourko, and Saiz 2008; Aastveit and Anundsen 2022). In fact, areas experiencing stronger credit booms during economic expansions and credit busts during downturns also tend to have lower supply elasticities. This makes house prices more responsive to changes in housing demand, both increases and decreases. Our finding is in line with recent research showing that financial-accelerator effects (Huang and Tang 2012; Anundsen and Heebøll 2016), households' overoptimism during housing booms (Chodorow-Reich, Guren, and McQuade 2024), and a price-foreclosure spiral during housing busts (Guren and McQuade 2020; Chodorow-Reich, Guren, and McQuade 2024) help explain why supply-inelastic U.S. areas had more pronounced housing cycles around the GFC. Our findings highlight how household debt overhang and housing supply constraints amplify the macrofinancial impact of contractionary monetary policy, suggesting that house prices are not independent of supply constraints amid negative demand shocks, financial-accelerator effects, and misaligned household expectations.

Fourth, differences in housing wealth across states—both in the level and volatility—may partly explain the heterogeneity in the transmission of monetary policy to the housing market. While prior studies have documented the impact of housing wealth and the ability of homeowners to tap into home equity for consumption (e.g., Bhutta and Keys 2016; Aladangady 2017; Beraja et al. 2019; Andersen and Leth-Petersen 2021), the role of housing wealth volatility is less understood. Presumably, higher volatility could lead households to reduce demand for real estate services, such as realtors, loan officers, and mortgage brokers, lowering economic activity (Best and Kleven 2018; Bhutta and Ringo 2021; Anenberg and Ringo 2022; Benmelech, Guran, and Melzer 2023). Furthermore, uncertainty about future house prices can influence household tenure choices (Henderson and Ioannides 1983; Rosen, Rosen, and Holtz-Eakin 1984; Fu 1995). We find that differences in housing wealth

(and its volatility) partially account for states' differential responses to monetary policy shocks.

Finally, we find that monetary policy may have a stronger influence on housing tenure decisions in low-supply-elasticity areas. Differences in market segmentation between states, such as the costs of converting owner-occupied housing to rental units, may help explain this (Greenwald and Guren 2025). In inelastic states, house prices and homeownership rates decline more sharply, indicating a greater contraction in demand for owner-occupied housing. At the same time, the rental vacancy rate remains stable, and rent prices increase less compared with more elastic areas, which challenges the assumption that housing demand shifts to rentals. However, we interpret the modest rise in rent prices as evidence that the rental market in inelastic areas can absorb demand more readily, likely due to lower adjustment costs. In addition, strict regulations on new housing construction may incentivize homeowners or investors to lower adjustment costs of converting owner-occupied units to rental units in response to declining demand.

Our main finding—states with more inelastic housing supply are more adversely affected by contractionary monetary policy shocks—does not imply that housing supply constraints are the only factor contributing to this heterogeneity, nor does it establish a causal link. We acknowledge that other factors, such as state-level differences in industry composition, demographics, income levels, and institutional quality, may also influence regional responses to monetary policy. While establishing causality is arguably challenging, we conduct cross-sectional regressions to control for several state-specific characteristics that might explain these differences. Even in this setting, housing supply restrictions are important in explaining the regional heterogeneity in the transmission of monetary policy.

We check the robustness of our results along several dimensions, including (i) using alternative monetary policy surprises (forward guidance) that affect the long end of the yield curve (Swanson 2021); (ii) controlling for central bank information effects in monetary policy (Jarociński and Karadi 2020); (iii) considering alternative measures of state-specific housing supply restrictions (Saiz 2010; Aastveit, Albuquerque, and Anundsen 2023); and (iv) using

an alternative measure of state-level inflation (Hazell et al. 2022). Our main findings are overall robust.

In summary, our paper contributes to the growing literature on how housing supply constraints affect the transmission of demand shocks—such as monetary policy shocks—to housing markets (Gyourko, Saiz, and Summers 2008; Saiz 2010; Glaeser et al. 2014; Herkenhoff, Ohanian, and Prescott 2018; Albuquerque, Iseringhausen, and Opitz 2020; Fischer et al. 2021; Aastveit and Anundsen 2022; Cooper, Luengo-Prado, and Olivei 2022; Aastveit, Albuquerque, and Anundsen 2023). We add to this research by examining the differential macrofinancial effects of monetary policy across U.S. states, going beyond only analyzing house price dynamics. Our paper also relates to recent studies on how monetary policy affects households' housing tenure decisions, particularly showing that contractionary policy may disadvantage renters and mortgagors (Dias and Duarte 2019, 2022; Koeniger, Lennartz, and Ramelet 2022). We empirically document that monetary policy may influence housing tenure decisions differently across states, depending on differences in supply constraints, housing market segmentation, and household debt imbalances. Finally, our paper can be placed within the broader literature on regional and cross-country differences in the responses of macroeconomic variables to monetary policy (Carlino and DeFina 1998, 1999; Fratantoni and Schuh 2003; Francis, Owyang, and Sekhposyan 2012; Calza, Monacelli, and Stracca 2013; Albuquerque 2019; Fischer et al. 2021; Aastveit and Anundsen 2022; Corsetti, Duarte, and Mann 2022; Aastveit, Albuquerque, and Anundsen 2023; Pica 2023).

2. Data

We use state-level data to explore the regional heterogeneity in the U.S. economy. Although we would prefer to use more granular data, such as metropolitan statistical areas (MSAs) or city-level data, in this case data availability issues on a set of economic and financial indicators become more severe. U.S. states are characterized by significant variation across key macroeconomic variables (Figure B.1 in Appendix B). In particular, the heterogeneity in the dynamics of housing market variables is consistent with the notion that housing markets are local and exhibit their own cyclical movements, possibly

decoupled from the national cycle (Ghent and Owyang 2010; Ferreira and Gyourko 2012; Hernandez-Murillo, Owyang, and Rubio 2017). We also note the substantial variation over time in house prices relative to rents, indicating that house prices may not move in line with rents over the long term—the no-arbitrage condition—as predicted by theory (Poterba 1984).⁴

For the FAVAR analysis, we use a quarterly data set of 26 state-specific series for 50 U.S. states over 1999:Q1–2019:Q4. We complement the data set with 47 aggregate national variables, including standard macroeconomic, financial, and housing market variables, to control for the state of the U.S. business cycle, for a total of over 1,200 time series. When extracting the factors, we exclude those U.S. variables that are already included at the state level (e.g., gross domestic product, GDP) or whose subcomponents are included, resulting in 1,221 series from which the factors are extracted (Table B.1 in Appendix B). At the state level, we include variables related to the real economy, the labor market, the financial sector, public finances, and the housing market. The rich information on state-specific housing market conditions includes house prices and rental prices, supply-side variables, such as permits and housing starts, and other important characteristics, e.g., the homeownership rate and vacancy rates. All nominal series are deflated using the U.S. consumer price index. If necessary, we take first (log-)differences of the variables to guarantee stationarity. Table B.1 in Appendix B contains detailed information on the variables' definitions, transformations, and data sources. Table B.2 provides summary statistics for the state-level variables.

To measure price developments in state-level rental markets, we use a new rent price index developed by Howard and Liebersohn (2021). The authors build annual real rent price indices for a large

⁴Standard theory predicts that the price of a house should be determined by the present value of cumulated future rents. The run-up to the GFC, however, showed that house prices can deviate from rents for an extended period of time. Glaeser and Gyourko (2007) show empirically that the no-arbitrage condition does not hold given the substantial differences between owning and renting, while renters and owners also differ dramatically from each other. More recently, Amaral et al. (2024) find that house prices inequality has increased more than rent prices inequality across (U.S. and other international) cities, as house prices rose more than rents, especially in areas with high house price-to-rent ratios.

panel of U.S. MSAs by resorting to data on rental incomes of multi-family residential properties taken from mortgage-backed securities data from Trepp. Their repeat-rent index (quality-adjusted) is conceptually similar to the consumer price index (CPI), which makes it comparable to the rent series in the CPI, but with the advantage of a much wider geographical coverage. We aggregate the original MSA rent index at the state level using population weights for each MSA.⁵

Our measure of monetary policy surprises follows the recent literature relying on high-frequency identification (Gürkaynak, Sack, and Swanson 2005; Gertler and Karadi 2015; Nakamura and Steinsson 2018). Specifically, we take the surprises in interest rates for three-month-ahead contracts on federal funds futures in a 30-minute window surrounding FOMC meetings. We then sum up all daily surprises within the respective quarter. Since the surprises may not capture the “true” structural monetary policy shock—for instance, due to monetary policy relevant news outside the FOMC announcement window—we use the one-year Treasury rate as the monetary policy indicator, which also captures forward-guidance effects about the future path of interest rates (Gertler and Karadi 2015). Several recent papers have emphasized the importance of further purging the high-frequency surprises from the so-called central bank information effect (e.g., Jarociński and Karadi 2020; Miranda-Agrippino and Ricco 2021; Bauer and Swanson 2023). In Section 6 we test the robustness of our baseline results by controlling for possible information effects.

3. Factor-Augmented VAR(X) Model

The FAVAR has several advantages compared with alternative models. For instance, traditional small and medium-scale VAR models can suffer from the problem of information deficiency, while the

⁵We interpolate the annual series to obtain quarterly data with the Denton (1971) method, using the real rent of primary residence series from the U.S. CPI as the auxiliary indicator. We decided not to use rents data from Zillow given the shorter time dimension (data starting only in 2015). The real rent index from Howard and Liebersohn (2021) is available for 217 MSAs (not available for four states, namely Alaska, Hawaii, New Hampshire, and Vermont), which contrasts with only 25 MSAs published by the Bureau of Economic Analysis.

FAVAR processes a much larger information set, providing a more complete overview of the economy (Bernanke, Boivin, and Elias 2005). Specifically, the FAVAR extracts a small number of common factors from the full data set. Similarly, and in contrast to individual VARs estimated for each state, the FAVAR provides a parsimonious framework to jointly analyze heterogeneous responses to a common monetary policy shock. We also prefer the FAVAR model over a panel VAR due to the lower estimation costs and fewer required specification assumptions.

The FAVAR model has a simple state-space representation. The observation equation reflects the assumption that the dynamics of a large set of N observed time series depend linearly on a smaller number of common (un)observed factors. In particular, the observation equation is given by

$$X_t = \Lambda H_t + \nu_t, \quad \nu_t \sim \mathcal{N}(0, \Omega), \quad (1)$$

where $X_t = (X_{1t}, \dots, X_{Nt})'$ is a vector of data observations, $H_t = (F_{1t}, \dots, F_{qt}, R_{1t}, \dots, R_{kt})'$ is a vector of q unobserved factors (F) and k observed factors (R), and Λ is a $N \times (q + k)$ matrix of factor loadings. In our case, there is only one observed factor ($k = 1$), which is the one-year Treasury rate used to scale the monetary policy shock. Finally, ν_t is a vector of normally distributed and uncorrelated error terms with diagonal covariance matrix Ω . The transition equation of the FAVAR model assumes that the factors follow a VAR(X) process given by

$$H_t = \Phi_1 H_{t-1} + \dots + \Phi_p H_{t-p} + A z_t + u_t, \quad u_t \sim \mathcal{N}(0, \Sigma), \quad (2)$$

where $\Phi = (\Phi_1, \dots, \Phi_p)$ is the $(q + 1) \times p$ matrix containing the VAR coefficients and z_t is the “pre-identified” monetary policy surprise computed from intradaily financial market data. A is a vector of coefficients and u_t are the “non-monetary policy” disturbances assumed to be normally distributed with full covariance matrix Σ . We include $p = 2$ lags in the VAR, a fairly common choice for data at the quarterly frequency (see, for example, Baumeister, Liu, and Mumtaz 2013; Mumtaz and Theodoridis 2017).

To identify monetary policy shocks, we use the exogenous variable approach of Paul (2020). This identification strategy relies on including the set of “pre-identified” monetary policy

surprises z_t (see Section 2) as an exogenous variable in the VAR Equation (2). Under the assumption that z_t is a noisy measure of the true monetary policy shock $\epsilon_{mp,t}$ —i.e., $z_t = \alpha\epsilon_{mp,t} + \eta_t$, with η_t orthogonal to all other variables—Paul (2020) shows that this approach consistently estimates the relevant impulse response functions. Specifically, the contemporaneous reaction of factor j to a one-unit increase in the policy rate R_t is given by A_j/A_R . For the remaining horizons, the relative impulse responses are derived by tracing the shock in the policy rate through the system described by Equation (2). As the surprise series z_t is only identified up to sign and scale, we normalize the coefficient vector A such that a contractionary monetary policy shock corresponds to a median increase in the policy rate of 25 basis points (bps). On a methodological note, the *exogenous variable* approach of Paul (2020) shares similarities with the *external instrument* approach of Stock and Watson (2018). Paul (2020) shows analytically that the contemporaneous impulse responses obtained from both approaches are identical. For further details on the exogenous variable approach, we refer to Paul (2020).

As outlined in Bernanke, Boivin, and Elias (2005), the FAVAR model can be estimated in two ways: (i) using a fully likelihood-based (Bayesian) approach, in which the unobserved factors are sampled alongside the other model parameters; or (ii) by a two-step approach that first estimates the unobserved factors and then the remaining parameters conditional on these factors. In this paper, we follow Stock and Watson (2005), Korobilis (2013), and Corsetti, Duarte, and Mann (2022) and use the two-step approach that is based on principal component analysis, which is easier to implement and computationally less demanding.

In the first step, we extract, based on the Bayesian information criterion (BIC) of Bai and Ng (2002), five principal components from the set of state-level and aggregate U.S. variables.⁶ Following common practice, the principal components are obtained from the standardized data after subtracting the mean of each series and dividing by the respective standard deviation. We address the issue that the principal component representation of the data is only identified up

⁶The maximum number of factors for the Bai and Ng (2002) test is set to $k_{max} = 10$.

to rotation by imposing the standard normalization $\Lambda'_F \Lambda_F = I_q$. The five factors explain around 56 percent of the variation in the data. In the second step, conditional on the estimated principal components to proxy F , the observation Equation (1) collapses to N univariate regressions (Korobilis 2013). Moreover, the transition Equation (2) constitutes a standard VAR(X) model. For the estimation of the model parameters and the impulse response functions, we use the demeaned, but not fully standardized, data. This allows us to interpret the impulse response functions directly and not just in relation to the variables' standard deviations. We estimate all parameters using Bayesian Markov chain Monte Carlo (MCMC) methods. In particular, we rely on an established Gibbs sampling algorithm, using a total of 10,000 draws and dropping the first 5,000 draws as “burn-in.” We refer to Appendix A for details on the estimation procedure and prior choices, which are overall standard with the exception of a somewhat tighter prior for the covariance matrix of the VAR innovations, Σ , given the relatively short time dimension of our sample. The latter also contributes to the fairly wide posterior density intervals of many of the impulse response functions presented in the following sections. However, these intervals often still exclude the zero.

Figure C.1 in Appendix C shows the first five principal components together with the monetary policy indicator, which is the observed factor.⁷ To better understand the explanatory power of the factors, we regress each variable in our data set on the six factors—both one factor at a time and all factors jointly—and report the respective R-squared values. Table C.1 lists the top five variables that are best explained by each factor. Although it is generally not possible to assign a structural interpretation to the unobserved factors, these results provide some evidence of what the factors may capture. While the first factor appears to relate closely to (mortgage) loan performance, the second, third, fourth, and fifth factors seem to contain information about the homeownership rate, rent

⁷While the second step of our model estimation does not account for uncertainty in the estimation of the factors, Figure C.1 also shows the (bootstrapped) confidence bands around the principal components (Gonçalves and Perron 2020). Estimation uncertainty of the factors appears overall limited, and virtually absent for the first factor.

developments, aggregate inflation and (state-level) personal expenditures, as well as (state-level) personal bankruptcies, respectively.

4. The Transmission of Monetary Policy to the Housing Market

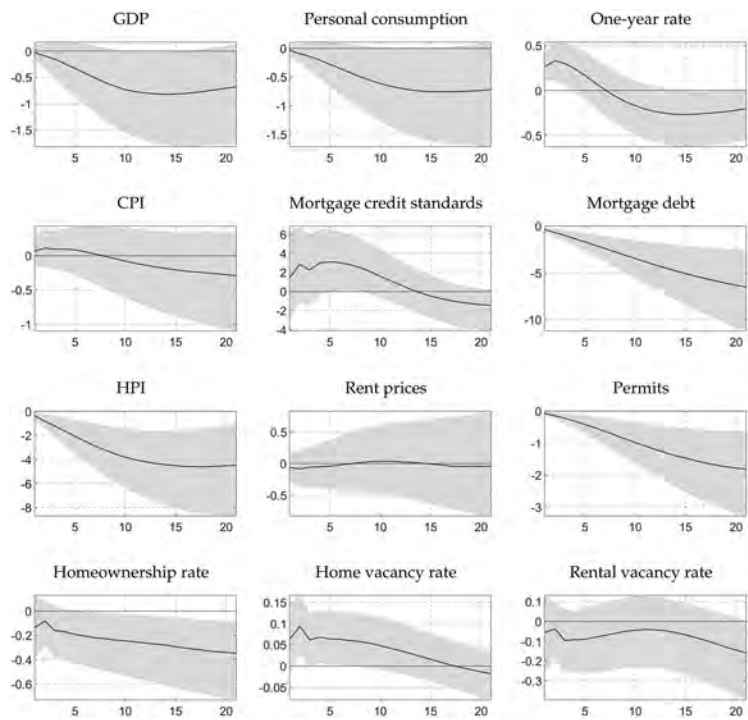
In this section we start by presenting the responses of aggregate U.S. variables to a monetary policy tightening with a particular focus on the housing market. We then move to the core of our analysis centered around the heterogeneity of the state-level responses.

4.1 U.S. Aggregate Evidence

The aggregate responses of U.S. variables allow us to check if our model is able to replicate a set of stylized facts on the effects of monetary policy. All variables are expressed in real terms, with the exception of permits, interest rates, and ratios. Figure 2 presents the cumulative impulse response functions (IRFs) of selected U.S. variables. Overall, we find that our results are in line with standard economic theory (Christiano, Eichenbaum, and Evans 1996, 1999). Following a monetary policy tightening calibrated to increase the one-year Treasury rate by 25 bps on impact, we find that economic activity falls quickly, with real GDP decreasing by around 0.6 percent after two years and reaching a trough of around 0.8 percent after three to four years. The temporary rise in the one-year Treasury rate—which lasts for about seven quarters—leads to a decline in inflation over the medium term, as measured by the consumer price index.

Turning to housing market variables, a tightening of monetary policy decreases both house prices and housing supply, measured by the number of building permit authorizations. Specifically, we find that house prices fall on impact, declining by roughly 3 percent after two years and 4.5 percent after five years, while building permits contract by almost 1 percent after two years and by 1.5–2 percent after five years. These relatively large estimates, including the impact on real activity, fall on the high side of those reported in a meta-analysis by Williams (2015), who reviews 11 papers on the effects of monetary policy shocks on house prices and activity. Williams (2015) finds that a 100

**Figure 2. Impulse Response Functions
of Selected U.S. Variables**



Note: Cumulative IRFs of selected U.S. variables after a monetary policy tightening that increase the one-year Treasury rate by 25 bps. The solid black line is the median response (over all MCMC draws) and the gray shaded areas represent the 68 percent highest density interval (HDI).

bps monetary policy shock leads average house prices to decline between 1.7 percent and 10.8 percent after two years, which compares with 12 percent in our estimates. In turn, Williams (2015) finds that real activity falls between 0.3 percent and 9.3 percent after two years, which compares with 2.4 percent in our analysis.

The large drop of house prices is consistent with recent evidence that house prices may have become more responsive to monetary policy since the GFC due to a decline in housing supply elasticities (Albuquerque, Iseringhausen, and Opitz 2020; Aastveit, Albuquerque, and Anundsen 2023), a long-term increase in the

investor share of home purchases, or a prolonged period of ultra-low interest rates (Chudik and Kumar 2023).⁸

We find suggestive evidence that monetary policy may affect both the intensive and extensive margins of housing. On the intensive margin, the literature has documented how homeowners' housing wealth effects may transmit to consumption (Iacoviello 2005; Bhutta and Keys 2016; Aladangady 2017; Beraja et al. 2019; Cloyne, Ferreira, and Surico 2020; Garriga and Hedlund 2020). In turn, the extensive margin can be seen in the fall in housing demand from prospective homeowners due to tighter credit conditions—measured with the net percentage share of banks reporting tightening standards for mortgage loans from the Senior Loan Officer Opinion Survey (SLOOS). As house purchases are typically debt-financed, tighter credit conditions lead to a decline in the homeownership rate (Bhutta and Ringo 2021). In this context, some housing demand presumably shifts to the rental market, as also evidenced by an increase in the home vacancy rate and a fall in the rental vacancy rate. We further explore this reallocation of housing demand in the following section.

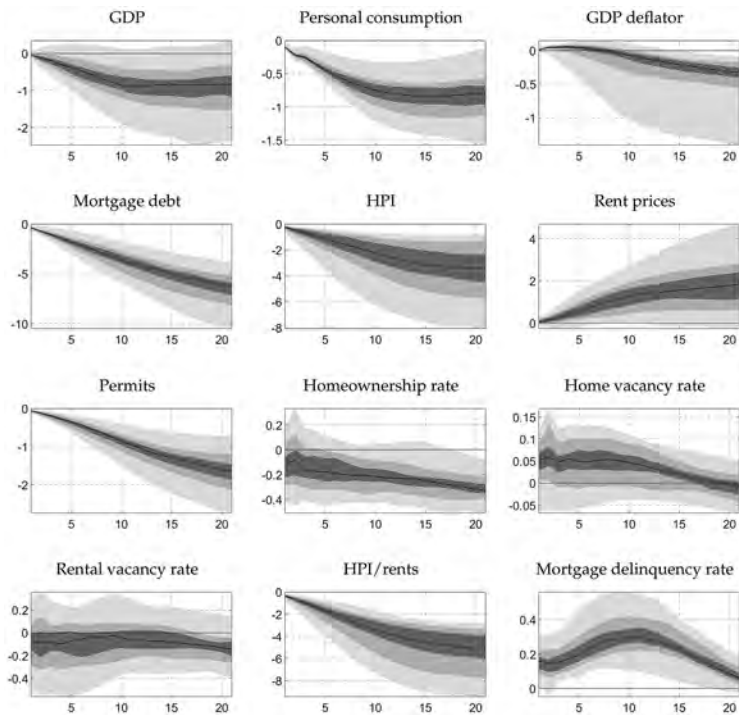
4.2 Transmission of Monetary Policy to State-Level Housing Markets

We take a closer look at the heterogeneity of housing market responses to a contractionary monetary policy shock across U.S. states.⁹ Figure 3 summarizes the posterior median responses of

⁸Moreover, there is an ongoing debate about whether house prices may have become less responsive to contractionary monetary policy during the pandemic due to the increase in the share of fixed-rate mortgages. To be sure, the U.S. mortgage market is increasingly dominated by mortgagors locked in (fixed) low-interest-rate mortgages that originated before the 2022 tightening cycle started: the share of fixed-rate mortgages rose to 96.4 percent in 2023:Q2, up from 94.5 percent in 2019:Q4. Higher interest rates therefore only affect directly new mortgages. We remain silent on this issue since we are mostly interested in the cross-sectional variation of housing supply constraints. In addition, there is little variation in the share of fixed-rate mortgages across states.

⁹Our FAVAR model assumes symmetry regarding the monetary policy effects, i.e., contractionary and expansionary monetary policy shocks give rise to effects of the same magnitude, but with different signs. Recent literature, however, finds that there may be important asymmetries in the regional responses to monetary

Figure 3. Dispersion of State-Level Impulse Response Functions



Note: Distribution of the median (cumulative) IRFs across U.S. states after a monetary policy tightening that increase the one-year Treasury rate by 25 bps. The black line is the median response of all state-level (median) responses. The gray shaded areas include 30 percent (35–65 percentile), 60 percent (20–80 percentile), and 90 percent (5–95 percentile) of the median responses, respectively, going from dark to lighter gray.

all U.S. states. The solid black line is the response of the median state, while the gray areas refer to different percentiles of the states’

policy shocks: areas with more inelastic housing supply are more responsive to expansionary monetary policy shocks than to contractionary shocks (Aastveit and Anundsen 2022). These findings, however, are based on a sample that starts in the early 1980s and stops in 2007. During this period, real house prices were mostly on an upward trend. We leave the study of such asymmetric effects of shocks for future work.

median responses. Two main findings emerge. First, there is sizable heterogeneity across U.S. states in the responses to a contractionary monetary policy shock; while economic activity, house prices, housing supply, and the homeownership rate all fall across the board, the magnitude of those declines varies widely.¹⁰ Differences in the housing market structure, which we explore later, may explain part of this heterogeneity. Overall, our results confirm a large degree of heterogeneity in the transmission of monetary policy or demand shocks to the housing market (Paciorek 2013; Fischer et al. 2021; Aastveit and Anundsen 2022; Corsetti, Duarte, and Mann 2022; Cooper, Luengo-Prado, and Olivei 2022; Koeniger, Lennartz, and Ramelet 2022; Aastveit, Albuquerque, and Anundsen 2023).

Second, we find that real rents increase over the horizon, which contrasts with the house price dynamics. This suggests that housing demand shifts from the owner-occupied segment to the rental market as the cost of homeownership goes up following a contractionary monetary policy shock. This is in line with recent research arguing that monetary policy influences the housing tenure decisions of households (Dias and Duarte 2019, 2022; Koeniger, Lennartz, and Ramelet 2022). We contribute to this literature by showing that, despite the synchronized fall in house prices and the increase in rent prices, monetary policy seems to exert a differential impact on households' housing tenure decisions across states. For instance, the magnitude of the fall in the house price-to-rent ratio (HPI/rents)—typically used to summarize how expensive the owner-occupied segment is relative to renting—varies markedly across states, presumably influencing differently households' decision to buy or rent.¹¹

Other factors that should influence households' housing tenure decisions are the cost of debt or credit conditions more generally. We indeed find that the tightening in credit conditions leads to a contraction in mortgage debt and an increase in mortgage delinquencies. Consistent with this, the reallocation of demand from the

¹⁰Considerable dispersion is also present in the state-level responses for unemployment, employment, and nominal rents (Figure C.2 in Appendix C).

¹¹We are simplifying the discussion, as regional heterogeneity in house price/rent ratios should reflect differences in housing risk, and in expectations on house and rent prices. But expectations about future house prices can be reasonably captured by current house prices in a context of extrapolative expectations (Glaeser, Gyourko, and Saiz 2008).

owner-occupied market to the rental market does not seem to evolve at the same pace across states. In fact, we find that the homeownership rate declines only marginally for some states, while falling more markedly for others. In addition, housing market differences across states can also be seen in the responses of housing supply: home (rental) vacancy rates actually decline (increase) for some states, which contrasts with the median state response and the U.S. aggregate evidence in Figure 2. The persistence in the decline of the rental vacancy rate indicates a high degree of segmentation in the U.S. housing market, as increasing demand for rental units may not be fully met by supply, thus explaining the increase in rent prices (Greenwald and Guren 2025).¹²

The shift from the owner-occupied to the rental market is consistent with recent evidence that contractionary monetary policy may disadvantage renters and homeowners with a mortgage (Dias and Duarte 2022). Our results indeed suggest that the combination of tighter credit conditions with lower house prices and higher rent prices could have disproportionate effects on these households. This questions the view that monetary policy may have little impact on renters (Aladangady 2017; Wong 2021) and bears relevance as mortgagors, and to a lesser extent renters, tend to be associated with the largest MPC (Cloyne, Ferreira, and Surico 2020). Moreover, by affecting house prices differently, and thus housing wealth and consumption, monetary policy may have important distributional effects (Coibion et al. 2017; Holm, Paul, and Tischbirek 2021; Amberg et al. 2022; Bonifacio et al. 2022; Amaral et al. 2024). Overall, our results suggest that the monetary policy transmission depends on state-specific characteristics. As we will see, differences in housing supply elasticities, and in credit and housing wealth conditions, may account for a portion of this regional variation.

¹²The theoretical and empirical predictions in Greenwald and Guren (2025) suggest that rental and owner-occupied housing in the U.S. are highly segmented. Their model generates house price dynamics that are close to those under perfectly segmented markets, reflecting large frictions in rental markets. The segmentation between owner-occupied and rental markets implies that credit supply shocks that shift the housing demand curve lead to higher house price-to-rent ratios, while the homeownership rate remains relatively unchanged.

5. Regional Characteristics and the Transmission of Monetary Policy

In this section we investigate the relevance of possible channels and state-specific characteristics in explaining the heterogeneous transmission of monetary policy. We follow the spirit of Corsetti, Duarte, and Mann (2022), who look at institutional characteristics of selected euro-area member states and their correlations with the strength of euro-area monetary policy transmission. Given our larger cross-section of U.S. states, we focus on the average responses of states, grouping them by state-specific characteristics. Specifically, we compare the posterior distributions of the average response between states belonging to the top and bottom deciles of selected characteristics (Figures 5–8). We also show in Appendix C the posterior distributions of the differences between the groups' average IRFs (Figures C.3–C.5), as well as the responses of the individual IRFs (Figures C.6–C.8) and the average quintiles (Figures C.9–C.11). We then complement this analysis with a conditional (regression-based) correlation analysis. Table B.3 in Appendix B contains the (average) values of selected characteristics for each state, highlighting those states that belong to the top/bottom deciles.

5.1 *Transmission Channels*

5.1.1 *Housing Supply Channel*

Recent research has shown that housing supply elasticities can play an important role in the transmission of demand shocks (e.g., from expansionary monetary policy) to the housing market (Gyourko, Saiz, and Summers 2008; Saiz 2010; Glaeser et al. 2014; Albuquerque, Iseringhausen, and Opitz 2020; Fischer et al. 2021; Aastveit and Anundsen 2022; Aastveit, Albuquerque, and Anundsen 2023). According to this strand of research, house prices in areas with lower housing supply elasticities are more responsive to an expansion in demand than those in high-supply elasticity areas. This finding is predicated on tighter land-use regulation and geographical restrictions that characterize a typical U.S. low-supply-elasticity area—found predominantly in coastal areas and in high-productivity and high-income places. These supply constraints make

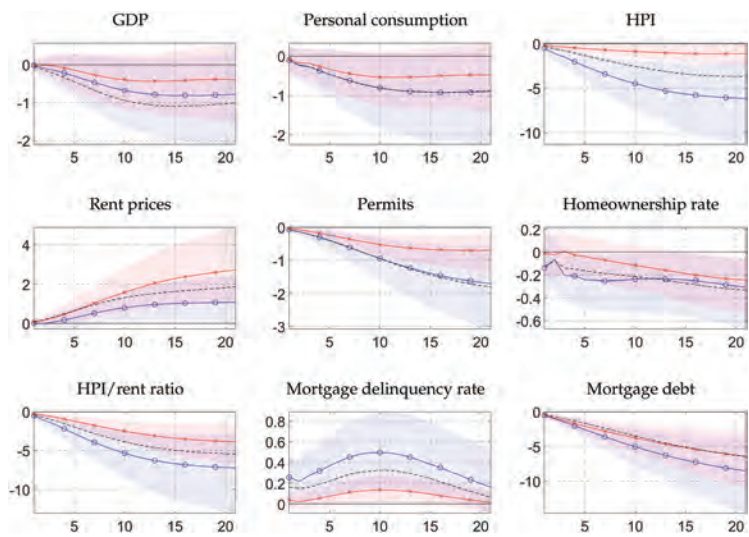
it more difficult and expensive to expand supply in the face of rising demand, resulting in a stronger increase of house prices to absorb demand.

While there is growing evidence on the aforementioned link between monetary policy, supply elasticities, and house prices, the literature has not yet assessed, to the best of our knowledge, the wider macrofinancial implications of monetary policy conditional on regional differences in housing supply—the *housing supply channel* of monetary policy. This is particularly important given the recent rise of interest rates, where an expected larger fall in house prices in inelastic areas may amplify the contraction in economic activity, while raising financial stability risks.

We use the land-use restriction index (LRI) constructed by Herkenhoff, Ohanian, and Prescott (2018) as our measure of housing supply constraints. This indicator is based on a general equilibrium spatial model of the United States augmented with state-level data on employment, workers' output, house prices, and the amount of usable land. It is available for 48 states, excluding Alaska and Hawaii, and for each decade since 1950: 1950, 1960, ..., 2000, and 2014. We split the states based on their indicator value in 2014, which should capture more accurately the prevailing stringency of land-use regulation during our estimation sample. Moreover, we divide the indicator by its standard deviation and change its sign so that higher values reflect states with tighter land-use regulation. There is a strong link between stringent land-use regulation and low housing supply elasticities, so we use these two terms interchangeably to refer to states with inelastic housing supply (Figure 4). We opted for the LRI of Herkenhoff, Ohanian, and Prescott (2018) as our baseline measure of supply constraints since the other measures available in the literature (Saiz 2010; Aastveit, Albuquerque, and Anundsen 2023) are computed at the MSA level, and thus need to be aggregated at the state level using population weights. The LRI is also available for an additional state (New Hampshire). In addition, the Saiz (2010) elasticities are estimated over 1970–2000, so they do not take into account the changes in supply elasticities over the last two decades.

We find evidence of an important role played by differences in housing supply restrictions in the transmission of monetary

Figure 5. Impulse Responses across Herkenhoff, Ohanian, and Prescott (2018) LRI (decile averages)



Note: Posterior distributions of the average (cumulative) IRFs across U.S. states after a monetary policy tightening that increases the one-year Treasury rate by 25 bps. The blue (red) line with circles (crosses) indicates the median (over all MCMC draws) of the average responses of the states belonging to the top (bottom) decile of the respective state characteristic. Shaded areas reflect the 68 percent HDI. The dashed black line is the median of the average IRFs across all the other states.

the collapse in house prices led to a large decline in U.S. household consumption between 2006 and 2009.¹⁵ Aruoba, Elul, and Kalemli-Özcan (2026) find that the decline in U.S. consumption around the GFC was mostly driven by financial constraints (56 percent), followed by local general equilibrium effects (30 percent), and by credit supply effects (15 percent). Pure housing wealth effects did not seem

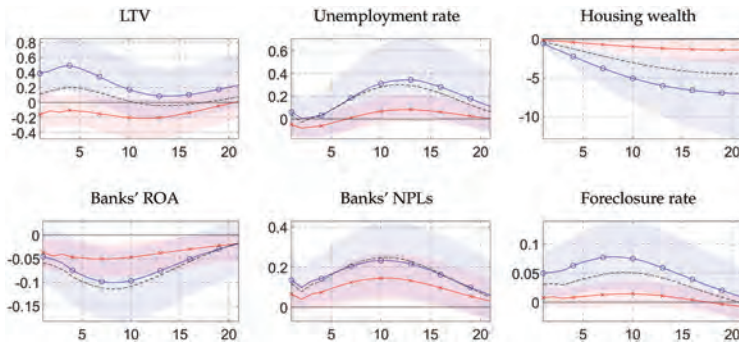
¹⁵ Aruoba, Elul, and Kalemli-Özcan (2026) explore the following channels: (i) financial constraints, measured with the ex ante loan-to-value (LTV) ratio and with ex post delinquency rates; (ii) credit supply effects at the county level, computed as cumulative changes in credit supply driven by banks' fundamentals; (iii) pure housing wealth effects, measured with the change in consumption from prime mortgagors with very low LTVs; and (iv) local general equilibrium effects, measured with the change in the unemployment rate.

to explain the fall in consumption around the GFC. This is consistent with estimates in the literature showing that households with large mortgages or high LTVs are considerably more responsive to house price changes (Aladangady 2017; Garriga and Hedlund 2020; Guren et al. 2021).

These studies typically use rich granular data at the household level that allow for a better understanding of the channels through which house price changes may affect household consumption expenditures. In this context, our aggregated state-level data prevent us from properly investigating the contribution of each channel for the response of consumption to house price changes. Bearing in mind the data and model limitations of our setting, we further explore the role of some of these channels. In particular, we investigate the role of households' financial constraints in driving the regional heterogeneity in the consumption responses to monetary policy shocks. Financial constraints have been found to be the single most important driver in explaining the link between lower house prices and a greater fall in consumption. More specifically, we add the LTV ratio to our FAVAR model to understand whether there is any differential dynamics between inelastic and elastic areas. The assumption is that the fall we observe in house prices (the collateral) would tighten households' credit constraints: an increase in the LTV would thus make households more vulnerable to shocks, increasing the probability that high-LTV households would make a larger adjustment to their consumption. A scenario of a greater increase in the LTV could also translate into negative equity for some households—becoming “underwater”—encouraging further cuts in consumption. Figure 6 confirms our prior that the LTV ratio would increase by substantially more in inelastic states, while remaining unchanged (or even declining slightly) in more elastic states. This is consistent with the notion that the collateral channel or financial constraints channel plays an important role in driving the fall in private consumption in inelastic states in the face of contractionary monetary policy shocks. Our findings also align with Albuquerque et al. (2026), who document stronger economic adjustments in countries with tighter housing supply constraints during housing contractions.

At the same time, financial stability risks also seem to increase more sharply in inelastic states. Mortgage delinquencies rise more sharply, and foreclosures and other indicators proxying the health of

Figure 6. Impulse Responses across Herkenhoff, Ohanian, and Prescott (2018) LRI (decile averages)



Note: See Figure 5.

the banking sector also deteriorate more considerably (bottom panel of Figure 6). In addition, mortgage debt falls more strongly, arguably reflecting not only lower demand but also banks' tighter credit conditions. These results thus provide support for important credit supply effects driving down consumption in these inelastic areas. Finally, the decline in housing demand (collateral channel) coupled with the aforementioned tightening in credit conditions may feed to local general equilibrium effects—in the words of Aruoba, Elul, and Kalemli-Özcan (2026). This is reflected in a greater increase in the unemployment rate in inelastic states (Figure 6). Lower employment would exacerbate the decline in consumption.

Overall, our results suggest that the larger fall in consumption in states with tighter housing supply constraints seems to reflect the combination of collateral effects, local general equilibrium effects, and credit supply effects. Although we remain silent on pure housing wealth effects, we believe they should play a negligible role in explain the regional variation in consumption responses given that outright owners or borrowers with small mortgages are typically not responsive to house prices changes (Aruoba, Elul, and Kalemli-Özcan 2026).

Our results should not be driven by differences in the share of adjustable-rate mortgages across states. ARMs are fairly uncommon in the U.S. economy, which contrasts with Europe, where the share

of ARMs varies considerably across countries, and therefore the pass-through of monetary policy to mortgage markets also varies widely (Calza, Monacelli, and Stracca 2013; Corsetti, Duarte, and Mann 2022; Pica 2023). In addition, according to data from the Federal Housing Finance Agency, there is very little variation in the ARM share across U.S. states: for instance, the ARM share stood at 5.5 percent of all outstanding mortgages nationally in 2019:Q4, with a standard deviation of 1.8 percentage points across all states.

Our finding that house prices decline more in inelastic states may, at face value, be surprising. Housing supply is rigid downwards, given the durability of housing, thus the house price response to negative demand shocks should be independent of supply restrictions (Glaeser and Gyourko 2005; Glaeser, Gyourko, and Saiz 2008; Aastveit and Anundsen 2022). We rationalize this result with findings on (i) the role of credit boom-busts in driving housing market cycles in more inelastic areas during the 2000s U.S. housing boom-bust (Huang and Tang 2012; Anundsen and Heebøll 2016), and on (ii) household overoptimism during the housing boom phase due to diagnostic expectations (Chodorow-Reich, Guren, and McQuade 2024). This strand of research shows that more inelastic U.S. areas had a stronger housing boom-bust during the 2000s, followed by a sharper post-GFC rebound (Chodorow-Reich, Guren, and McQuade 2024).

On (i) above, Huang and Tang (2012) find that households in cities with tighter supply constraints also relied more on credit, particularly subprime mortgages, which may explain why these cities recorded both a larger boom in house prices in the run-up to the GFC and a subsequent larger bust in house prices during the crisis as the credit crunch started to bite. Anundsen and Heebøll (2016) also document that both financial-accelerator effects and price-to-price feedback loops may explain why low-supply-elasticity areas recorded a stronger contraction in house prices in the aftermath of the GFC.

Finally, on (ii) above, Chodorow-Reich, Guren, and McQuade (2024) show empirically and theoretically that high-growth price areas (low-supply-elasticity areas) experienced a larger boom in house prices in the run-up to the GFC due to households' overoptimism, giving rise to a larger bust when beliefs started to correct. The fall in house prices in these low-supply-elasticity areas was also

amplified by excessive borrowing and a price-foreclosure spiral as foreclosures increased the stock of housing available for sale, further depressing prices (also in line with the theoretical predictions in Guren and McQuade 2020). House prices may thus not be independent of the supply elasticity in the presence of negative demand shocks when important financial-accelerator effects and misaligned household expectations are at play. This can then rationalize our finding of a stronger credit crunch and rising financial stability risks in inelastic states, amplifying the house price responses after a tightening in monetary policy.¹⁶

Our findings also speak to research documenting the impact of monetary policy on households' housing tenure decisions (Dias and Duarte 2019, 2022; Koeniger, Lennartz, and Ramelet 2022). Our contribution is to show that monetary policy may have a stronger influence on housing tenure decisions in states where supply is more constrained. In particular, we draw this implication from the larger fall in house prices, coupled with signs of a larger decline in homeownership rates, and a stronger increase in home vacancy rates for inelastic states (Figure C.15 in Appendix C). These dynamics point to a larger contraction in housing demand for the owner-occupied segment in inelastic states.¹⁷ But rent prices seem to increase by less in inelastic areas, which would, at face value, be at odds with the assumption of housing demand shifting to the rental market.

¹⁶A complementary explanation relies on the relationship between house prices and minimum profitable construction costs (MPPC). Glaeser and Gyourko (2018) argue that the shape of the housing supply curve depends on house prices relative to MPPC. Building on this notion, Aastveit and Anundsen (2022) find that house prices are typically above the MPPC in supply-inelastic areas, which theoretically makes it possible for house prices to decline by more than in (elastic) areas where house prices are close to the MPPC. This is consistent with our results: low-supply-elasticity areas, typically found in coastal areas (e.g., California), characterized by high house prices relative to MPPC, experience a stronger fall in house prices after a contractionary monetary policy shock.

¹⁷Our results are not restricted to a few states with low or high supply constraints. Figure C.16 in Appendix C shows that U.S. states whose housing supply constraints are less severe (red lines) experience a smaller house price fall relative to states with low supply elasticities, or tight supply constraints (blue lines). Rent prices increase by more in most of elastic areas (with some exceptions), also validating our baseline results. Results for homeownership rates are more mixed, since they also seem to fall by less in some inelastic areas.

The smaller increase in rent prices in inelastic states, however, may be related to differences in market segmentation across states, i.e., the frictions and adjustment costs in converting owner-occupied housing units to rental properties (Greenwald and Guren 2024). While we cannot observe the degree of segmentation across markets, the rental market in these states seems to have more capacity to absorb demand than in elastic states, further illustrated by a relatively unchanged rental vacancy rate for inelastic states (Figure C.15). Overall, we speculate that more stringent regulation to build new housing units in some areas may indirectly create incentives for homeowners to reduce adjustment costs in converting owner-occupied units to rental units in the face of shocks. An alternative explanation for the weaker response of rent prices in inelastic areas could be the initially already high rent levels in these areas (according to Zillow data), which potentially limit the scope for further increases. Stringent rent controls in several cities in two inelastic states, California and Maryland (according to the National Multifamily Housing Council and RentPrep), may also explain the more muted response of rent prices. Overall, we acknowledge that our finding that monetary policy may have stronger effects on households' housing tenure decisions in areas with low supply elasticity is not conclusive.

Finally, the larger decline in house prices in low-supply-elasticity states, which leads to negative housing wealth effects and lower housing equity of existing homeowners, suggests important distributional effects *across* states. These are high-income states where households tend to have larger housing wealth and higher consumption/income per capita (Figures C.17–C.19). The larger decline in housing wealth in inelastic states can be mapped to a larger fall in overall consumption expenditures, and therefore in economic activity in these states. Although it is outside of the scope of our paper, we find that contractionary monetary policy shocks may have a non-negligible role in reducing regional consumption and housing wealth inequality. This chimes with Amaral et al. (2024), who suggest that higher interest rates can decrease the dispersion and thus inequality in house prices across U.S. cities.

Let us conclude this section by stressing that the presented exercise neither implies that housing supply constraints are the only driver of regional heterogeneity in the responses to monetary

policy shocks nor proves a causal relationship. For instance, differences across states in the industry composition, demography, income levels, and in the quality of institutions may also explain the documented heterogeneity. Despite the limitations of our empirical approach that prevent us from fully exploring a causal link from housing supply constraints, we find reassurance in established literature highlighting the significant role these constraints play in shaping regional house price responses to demand shocks (Saks 2008; Saiz 2010; Aastveit and Anundsen 2022; Aastveit, Albuquerque, and Anundsen 2023). Moreover, Section 5.2 analyzes the combined effect of several state-specific characteristics, including housing supply constraints, on the differential responses to monetary policy shocks across states. Overall, we believe that the interaction of housing demand changes with housing supply frictions plays an important role in driving the differential behavior of the real economy across U.S. states.

5.1.2 Debt Overhang Channel

Another possible source of cross-sectional heterogeneity in the response to monetary policy shocks is differences in households' debt imbalances, or debt overhang in the spirit of (Eggertsson and Krugman 2012). In theory, as the cost of borrowing goes up, borrowing constraints become more binding for households with larger debt imbalances, i.e., whose debt deviates more from fundamentals, leading to a larger contraction in housing demand and house prices, consumption, and economic activity (Iacoviello 2005; Calza, Monacelli, and Stracca 2013; Hedlund, Karahan, and Ozkan 2016; Bhutta and Ringo 2021; Pica 2023; Bosshardt et al. 2024). We proxy household debt overhang for each state with the concept of a debt gap (Albuquerque 2019; Alpanda and Zubairy 2019). We compute the debt gap with the Hamilton (2018) filter on the state-level mortgage debt-to-income ratio, which involves regressing a given nonstationary variable at $t+h$ on a constant and on the four most recent values of the dependent variable available at time t . We set the forecast horizon to $h = 20$, the suggested horizon for financial variables at the quarterly frequency. Specifically, we compare states that fall in the top and bottom deciles of the debt gap

distribution, using the maximum value of the debt gap over time for each state.¹⁸

We find that contractionary monetary policy shocks transmit more strongly to states with larger debt gaps: house prices, housing supply, mortgage debt, and consumption fall by considerably more than in states with smaller debt imbalances (Figure 7). Financial stability risk may also increase more prominently in areas with larger household debt imbalances (Figure C.21 in Appendix C). These findings are in line with research that finds that changes in interest rates affect more households and regions closer to the borrowing constraint (Hedlund, Karahan, and Ozkan 2016; Bhutta and Ringo 2021; Bosshardt et al. 2024). Furthermore, we find that the responses of states with high debt gaps are qualitatively similar to the previous results we saw for areas with low supply elasticity. This is not a surprise given the positive correlation between supply constraints and household debt gaps (Figure C.20 in Appendix C).¹⁹ This reinforces the view that the interconnectedness between household debt overhang and housing supply constraints may amplify the macrofinancial effects of contractionary monetary policy shocks.

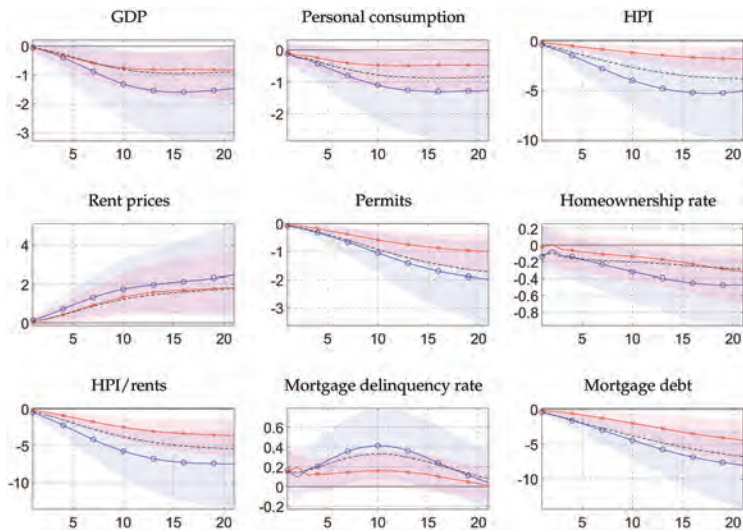
5.1.3 Housing Wealth Volatility and Household Balance Sheet Channel

The ability of mortgagors to extract home equity to finance consumption should also be relevant to explain the differential effects of monetary policy (Bhutta and Keys 2016; Aladangady 2017; Beraja et al. 2019; Andersen and Leth-Petersen 2021). According to this strand of the literature, the fall in house prices and in housing equity reduces housing equity extraction for existing homeowners, which leads to a contraction in housing demand that reinforces the decline in house prices and in economic activity. As a corollary, households

¹⁸Our results remain qualitatively similar when proxying debt imbalances with the state-level debt-to-income ratio.

¹⁹Despite the positive correlation (around 0.45), we generally capture different states in the high/low bins of the LRI and the debt gap. The only overlap refers to Iowa and North Dakota, which appear in both the bottom bins of the LRI and debt gap (see also Table B.3). This suggests that our results capture more the differential effects of groups of states with low/high supply elasticities and household debt imbalances, rather than being driven by particular states.

**Figure 7. Impulse Responses across
Debt Gap (decile averages)**

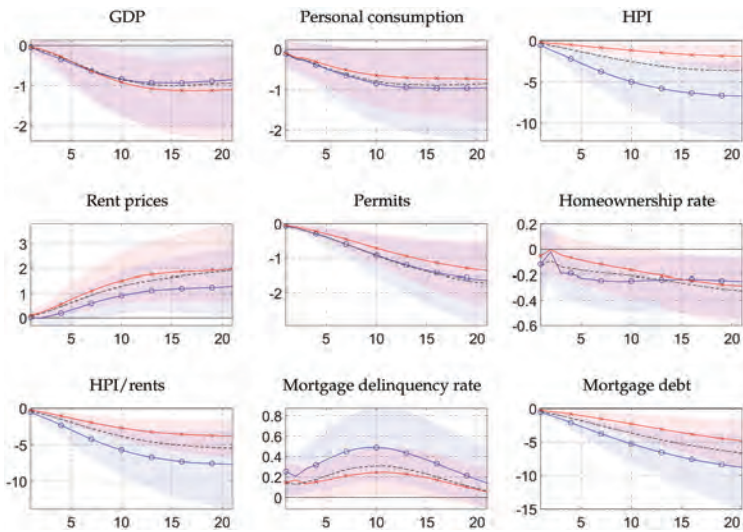


Note: See Figure 5.

with large housing wealth are in a better position to smooth their consumption expenditures in the face of interest rate shocks, as the probability of going *underwater*, i.e., the value of their houses falling below their mortgage commitments, is lower than for low-housing-wealth households. This is indeed what we see in Figure C.13 in Appendix C.

What is less studied is how the volatility of housing wealth—irrespective of its level—may affect households’ housing demand and consumption decisions in the face of a monetary policy tightening. We posit that higher uncertainty or volatility of housing wealth may lead households to reduce more their demand for real estate services or postpone their house purchase, leading to an overall stronger reduction in economic activity. This conjecture relates to research that has found a link between uncertainty about future house prices and households’ housing tenure decisions (Henderson and Ioannides 1983; Rosen, Rosen, and Holtz-Eakin 1984; Fu 1995). We use the standard deviation of housing wealth over the sample

Figure 8. Impulse Responses across Housing Wealth Volatility (decile averages)



Note: See Figure 5.

period as a proxy for housing wealth volatility. We follow Albuquerque and Krustev (2018) and compute housing wealth for each state j and quarter t as $(\text{homeownership rate}_{j,t} \times \text{total occupied housing units}_{j,t}) \times \text{house price index}_{j,t} \times \text{median house price in } 2000_j$.

Figure 8 suggests that states with larger fluctuations in housing wealth may indeed react more strongly to monetary policy: house prices fall by more than in states with lower housing wealth volatility, which goes hand-in-hand with a larger fall in consumption expenditures. Interestingly, while we find a significant and positive correlation between housing wealth volatility and the consumption reaction, this is not the case when considering average housing wealth (see the unconditional pairwise correlations in Table C.2), despite the housing wealth level and its volatility being highly correlated (around 0.9). In addition, for most of the remaining variables, the correlation between the strength of the response and the volatility of housing wealth appears to be stronger compared with

the housing wealth level. Overall, these findings suggest that larger uncertainty about housing wealth could strengthen the transmission of monetary policy.

5.2 Cross-Sectional Multivariate Analysis

In this section we use cross-sectional regressions to simultaneously analyze the drivers of heterogeneity in the responses of economic activity and housing markets to monetary policy. Specifically, we follow Mumtaz, Sunder-Plassmann, and Theophilopoulou (2018) and regress the IRFs of key variables on a set of state-level characteristics:

$$IRF_{h,i}^y = \alpha + X_i\beta + regions_i + \eta_i, \quad \eta_i \sim \mathcal{N}(0, \sigma_\eta^2), \quad (3)$$

where $IRF_{h,i}^y$ denotes the cumulative response of variable y in state i after h quarters. The $1 \times K$ vector X_i includes state-specific variables that potentially explain the heterogeneity in the responses, β is a $K \times 1$ vector of regression coefficients, α is the regression constant, and $regions_i$ is a vector of dummies for the eight divisions defined by the Bureau of Economic Analysis. While this exercise allows us to jointly analyze the importance of several variables, we stress that the results should be taken with a pinch of salt given the relatively small cross-section of U.S. states. In addition, and differently from Section 5.1 that explored the differential responses between groups of states, the cross-sectional regressions focus on the *average* relationship between monetary policy and selected state characteristics.

On top of the variables discussed in Section 5.1, we also control for other state-level characteristics that may play a role in explaining the heterogeneity in the regional responses to a tightening in monetary policy. First, the so-called sand states (Arizona, California, Florida, and Nevada) are known to have a more sensitive housing cycle (Ben-David, Towbin, and Weber 2024). Second, we explore the role of *labor market rigidities*. A more rigid labor market may cushion the effects of an adverse monetary policy shock, leading to a smaller decline in income and potentially reducing the transmission to the housing market. We follow Mumtaz, Sunder-Plassmann, and Theophilopoulou (2018) and use the existence of the right-to-work

legislation as a proxy for such rigidities. States which have implemented this legislation are considered to have a more flexible labor market. Third, we consider the share of manufacturing in state-level GDP and the share of small firms in total employment. States with more manufacturing-intensive sectors and areas with a larger share of small firms have been found to be more sensitive to interest rates (Carlino and DeFina 1998, 1999). Finally, we also control for the overall house price dynamics in a state by including the cumulative house price growth over the sample.

Table 1 presents our main results. While we focus on the variables' (medium-term) cumulative response after $h = 12$ quarters, our results remain qualitatively similar when using $h = 8$ and $h = 20$. We highlight several key results. For instance, the degree of land-use regulation in a state stands out as an important driver of the transmission of monetary policy to the housing market and the broader economy. A state in which land-use regulation is one standard deviation higher exhibits a 0.9 percentage point larger house price drop following a surprise tightening of 25 bps. Our results also indicate that this translates into lower consumption.

When inspecting the evidence of a potential debt overhang channel, measured by the debt gap, we do not find any of the coefficients to be significant. This suggests that household debt imbalances have little extra explanatory power for the differential effects of monetary policy on the housing market once we account for differences in housing supply constraints. Regarding a possible housing wealth channel, differences in the first moment of housing wealth over the sample can explain some degree of heterogeneity in the GDP and rents reactions, while a larger second moment, i.e., higher housing wealth volatility, is linked to a stronger drop in house prices.

For other state characteristics—whether a state is a so-called sand state or has the right-to-work legislation in place—the results are broadly consistent with our prior assumptions. The same holds for the small firms share, where the signs of the estimated coefficients, while not statistically different from zero, suggest that the larger the proportion of small firms in total employment, the stronger the fall in economic activity and house prices (Carlino and DeFina 1998; Furceri, Mazzola, and Pizzuto 2019). Regarding the role of a higher manufacturing share in an area, the results are somewhat mixed, with GDP reacting stronger but house prices weaker.

Table 1. Cross-Sectional Regression Results ($h = 12$)

Dependent Variables	(1) GDP	(2) PCE	(3) HPI	(4) Rents	(5) Permits	(6) HOR
LRI	-0.349 (0.213)	-0.358*** (0.101)	-0.932** (0.361)	-0.918* (0.468)	-0.010 (0.099)	-0.028 (0.074)
Debt Gap	-0.012 (0.024)	-0.004 (0.012)	0.010 (0.047)	0.025 (0.050)	0.002 (0.012)	0.003 (0.006)
HW (Avg.)	0.012** (0.006)	-0.000 (0.003)	0.021 (0.013)	0.020** (0.009)	0.001 (0.003)	0.001 (0.002)
HW (Std.)	-0.006 (0.004)	0.001 (0.002)	-0.018* (0.009)	-0.001 (0.007)	-0.003 (0.003)	-0.001 (0.001)
Sand State	-1.419** (0.541)	-0.576** (0.254)	-3.048*** (0.862)	-0.776 (0.896)	-0.670*** (0.228)	-0.083 (0.079)
RTW	-0.447** (0.198)	-0.625*** (0.136)	-0.890* (0.516)	-0.614 (0.646)	0.326** (0.124)	-0.040 (0.083)
Manufacturing	-0.039* (0.022)	0.018 (0.011)	0.100** (0.041)	0.073 (0.045)	0.002 (0.012)	0.004 (0.007)
Small Firms	-0.029 (0.019)	-0.010 (0.010)	-0.043 (0.035)	-0.001 (0.051)	0.024** (0.010)	0.005 (0.006)
HPI Growth	0.073*** (0.020)	0.037*** (0.011)	0.066* (0.035)	0.010 (0.030)	0.024** (0.012)	0.011** (0.004)
Observations	48	48	48	46	48	48
Adjusted R^2	0.652	0.673	0.857	0.222	0.673	0.392

Note: The table shows the point estimates and standard errors for the regression shown in Equation (3). GDP = real GDP. PCE = real personal consumption expenditure. HPI = real house price index. HOR = homeownership rate. LRI = land-use restriction index of Herkenhoff, Ohanian, and Prescott (2018) for the year 2014. Debt gap = max. value of Hamilton-filtered mortgage debt-to-income ratio over 1999–2019. HW (Avg.) = 100*log(average housing wealth over 1999–2019). HW (Std.) = 100*log(standard deviation of housing wealth over 1999–2019). Sand state = dummy variable for Arizona, California, Florida, and Nevada. RTW = dummy variable for right-to-work states. Manufacturing = share of manufacturing in state-level GDP (average over 1999–2019). Small firms = share of small firms employment in total employment (average over 1999–2019). HPI growth = growth rate of house prices over 1999–2019. All regressions include regional dummies. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

6. Robustness Checks

In this section, we carry out three main exercises to check the robustness of our baseline results. First, we use alternative monetary policy surprise series from the literature. We first depart from the conventional monetary policy shocks by also using shocks that capture more the long end of the yield curve. Specifically, we use shocks on forward guidance (FG) from Swanson (2021). Inspecting the cross-state dispersion of the IRFs of consumption, house prices, and permits, as well as the reactions sorted by the land-use restriction index of Herkenhoff, Ohanian, and Prescott (2018), we find that the results remain qualitatively similar (Figure C.22 in Appendix C). We also replace our baseline series of monetary policy surprises with the one from Jarociński and Karadi (2020) that controls for central bank information effects. While the new results remain qualitatively similar for house prices and permits, in this case the differential effect of monetary policy on consumption across states, grouped by LRI, disappears (Figure C.23).

Second, we take alternative measures of the elasticity of housing supply from the existing literature. In particular, we replace the land-use restriction index of Herkenhoff, Ohanian, and Prescott (2018) with the housing supply elasticity series from Saiz (2010) and Aastveit, Albuquerque, and Anundsen (2023). These supply elasticity series are available for a large set of U.S. MSAs, so we aggregate them at the state level using population weights (results are very similar when using income weights). When using the housing supply elasticities computed by Aastveit, Albuquerque, and Anundsen (2023), available as average elasticities over 1996–2006 (Figure C.24) or over 2012–19 (Figure C.25), we find that—with some exceptions—the results remain broadly comparable to our baseline findings in Figure 5. The same holds true when using Saiz (2010) housing supply elasticities (Figure C.26).

Third, we replace the state-specific GDP deflator, used throughout the paper, with an alternative state-level inflation series computed by Hazell et al. (2022). We find that our results for the cross-state dispersion of the IRFs of consumption and house prices remain comparable, while the price reactions differ, showing an initial increase (Figure C.27). Nevertheless, the differences in the consumption and house price reactions across states, grouped by the

LRI, are very similar to the baseline results. While the state-level inflation data of Hazell et al. (2022) end in 2017:Q4, this series is smoother for a few states compared with the growth rate of the GDP deflator series.

7. Conclusion

We use a FAVAR model and a large set of state-level (and aggregate U.S.) variables over 1999:Q1–2019:Q4 to trace out the role of regional housing markets in the transmission of monetary policy. Our main findings point to significant heterogeneity in the transmission of an aggregate contractionary monetary policy shock to the U.S. states. We find that the regional variation in responses to monetary policy can be partly accounted for by state-specific characteristics, most prominently by differences in housing supply elasticities, but also household debt overhang and housing wealth (volatility). In particular, we show that low-supply-elasticity areas, where land-use regulation is more stringent, record a larger fall in house prices and in economic activity more generally. In addition, financial stability risks appear to increase more sharply in these areas, as evidenced by a surge in mortgage delinquencies and foreclosures, while indicators proxying bank health also deteriorate faster. We find similar results for areas with higher imbalances in household debt—presumably where household borrowing constraints become more binding. Our findings thus show that the interaction between household debt overhang and housing supply constraints can amplify the macrofinancial effects of monetary policy.

Our results shed more light on the possible macrofinancial effects of monetary policy in the context of recently rapidly rising interest rates to tame inflationary pressures. It is well-established that housing is highly sensitive to interest rates, but we have shown that its sensitivity varies across U.S. states, particularly resulting from differences in supply conditions in the housing market, and in household indebtedness. Although the Federal Reserve conducts monetary policy with a dual mandate of price stability and full employment for the U.S. economy as a whole, our paper shows that monetary policy can nonetheless have differential macrofinancial effects across states within the country.

Our results have policy implications. If housing markets, and the wider macroeconomy, are more sensitive to monetary policy in areas with more inelastic housing supply, it could be advisable for financial supervision to tighten more in these areas to limit excessive house price volatility (Glaeser 2019). Since house purchases are mostly financed by mortgage debt, there is a case for strengthening macroprudential measures aimed at taming borrowing, such as limits on loan-to-income and/or debt-service ratios, before debt imbalances start to emerge. In addition, the relaxation of land-use restrictions should make an area less prone to boom-bust cycles in house prices, as builders are less constrained to expand supply during an expansion. By smoothing the cycle during an expansion, less stringent regulation should help an economy be more resilient to contractionary demand shocks.

Although we have carried out a battery of tests to validate our main findings, our results on the regional heterogeneity in the responses to monetary policy cannot necessarily be interpreted as causal: the identification of the effect of monetary policy on economic activity through housing supply constraints is not fully immune to endogeneity issues. For instance, we acknowledge that other state characteristics beyond housing supply constraints may also play an important role in explaining the regional heterogeneity we find in the responses to monetary policy. Against this background, our work opens up new research avenues. Further research can build on our work by extending the analysis to more granular data, and to a longer time series. For example, focusing on city-level data over a long period could help identify the response of macroeconomic variables, including house prices, to monetary policy shocks around changes in land-use regulation. A difference-in-differences framework, where land-use regulation changes in one city but not in another with similar levels of housing supply constraints, provides an ideal setting to study the role of housing supply constraints on economic activity after contractionary monetary shocks.

Appendix A. Estimation Procedure

This appendix contains details on the MCMC algorithm used to estimate the FAVAR model.

Block 1: Sample the factor loadings Λ from $p(\Lambda|X, H, \Omega)$ and the error covariance matrix Ω from $p(\Omega|X, H, \Lambda)$.

Conditional on the estimated factors (principal components), sampling the elements of the matrix of factor loadings in Equation (1) and the error covariance matrix Ω reduces to N standard linear Bayesian regression problems (see, e.g., Koop 2003). The conditional posterior of the loadings in each of the N rows of Λ , denoted by $\Lambda_i = (\lambda_{1i}^F, \dots, \lambda_{qi}^F, \lambda_i^R)$ for $i = 1, \dots, N$, under the normal prior $\Lambda_i \sim \mathcal{N}(\Lambda_{i,0}, \Sigma_{\Lambda_i,0})$, is

$$\Lambda_i|X_i, H, \omega_i^2 \sim \mathcal{N}(\hat{\Lambda}_i, \Sigma_{\Lambda_i}), \quad \text{with} \quad (\text{A.1})$$

$$\hat{\Lambda}_i = \left(\Sigma_{\Lambda_i,0}^{-1} + \frac{1}{\omega_i^2} H' H \right)^{-1} \left(\Sigma_{\Lambda_i,0}^{-1} \Lambda_{i,0} + \frac{1}{\omega_i^2} H' H \right)^{-1}, \quad (\text{A.2})$$

$$\Sigma_{\Lambda_i} = \left(\Sigma_{\Lambda_i,0}^{-1} + \frac{1}{\omega_i^2} H' X_i \right)^{-1}, \quad (\text{A.3})$$

where $H = (F_1, \dots, F_q, R)$, $X_i = (X_{i1}, \dots, X_{iT})'$, and ω_i^2 is the i th diagonal element of Ω . After sampling Λ_i , the corresponding element of Ω , ω_i^2 , can be sampled, under the inverse-gamma prior $\omega_i^2 \sim \mathcal{IG}(c_0, C_0)$, from

$$\omega_i^2|X_i, H, \Lambda_i \sim \mathcal{IG}(c_0 + (T-1)/2, C), \quad (\text{A.4})$$

where notation follows Chan and Hsiao (2014) and where C is defined as

$$C = C_0 + \left[\sum_{t=1}^T (X_{it} - \Lambda_i H_t')^2 \right] / 2. \quad (\text{A.5})$$

Finally, the prior values are set to $\Lambda_{i,0} = 0_{(q+1) \times 1}$, $\Sigma_{\Lambda_i,0} = 4 \times I_{(q+1)}$, $c_0 = 0.2$, and $C_0 = 0.2$.

Block 2: Sample the VAR(X) coefficients Φ and A from $p(\Phi, A|H, z, \Sigma)$.

The VAR(X) coefficients $\theta = \text{vec}((\Phi, A))$, assuming a normal prior distribution $\theta \sim \mathcal{N}(\theta_0, V_{\theta,0})$, have the following conditional posterior distribution (see, for example, Blake and Mumtaz 2012):

$$\theta \sim \mathcal{N}(\bar{\theta}, V_{\theta}), \quad \text{with} \quad (\text{A.6})$$

$$\bar{\theta} = (V_{\theta,0}^{-1} + \Sigma^{-1} \otimes \tilde{H}'\tilde{H})^{-1}(V_{\theta,0}^{-1}\theta_0 + \Sigma^{-1} \otimes \tilde{H}'\tilde{H}\bar{\theta}_{\text{OLS}}), \quad (\text{A.7})$$

$$\Sigma_{\theta} = (V_{\theta,0}^{-1} + \Sigma^{-1} \otimes \tilde{H}'\tilde{H})^{-1}, \quad (\text{A.8})$$

and where $\tilde{H} = (H_{t-1}, \dots, H_{t-p}, z)$ and $\bar{\theta}_{\text{OLS}}$ is the ordinary least squares (OLS) estimate of the VAR(X) coefficients. The prior configuration is relatively uninformative, i.e., $\theta_0 = 0_{[(q+1) \times (1+p \times (q+1))] \times 1}$ and $V_{\theta,0} = I_{(q+1) \times (1+p \times (q+1))}$.

Block 3: Sample the VAR(X) innovation covariance matrix Σ from $p(\Sigma|H, z, \Phi, A)$.

The covariance matrix of the VAR innovations, assuming an inverse-Wishart prior distribution $\Sigma \sim \mathcal{IW}(\Sigma_0, \nu_{\Sigma})$, has the following conditional posterior distribution (see, for example, Blake and Mumtaz 2012):

$$\Sigma \sim \mathcal{IW}(\Sigma_0 + u'u, \nu_{\Sigma} + T - p), \quad (\text{A.9})$$

where $u = (u_1, \dots, u_T)'$ are the “non-monetary policy” VAR residuals with each $u_t = H_t - \Phi_1 H_{t-1} - \dots - \Phi_p H_{t-p} - A z_t$, and the prior scale matrix and prior degrees of freedom are set to $\Sigma_0 = \lambda_0 I_{q+1}$ (with $\lambda_0 = 3^{-1}$) and $\nu_{\Sigma} = q + 1$, respectively.

Appendix B. Data Description

We use seasonally adjusted quarterly data from 1999:Q1 to 2019:Q4 and deflate all nominal data using the U.S. consumer price index. To avoid double-counting in the factor estimation (principal component analysis, or PCA), we do not include some of the aggregate variables such as GDP, since all its components are already included. We also do not include U.S. aggregate variables if the same variable is included at the state level (e.g., the homeownership rate). The last column of Table B.1 indicates which variables are included in the PCA.

Transformations:

- 1 – no transformation
- 2 – first (log-)differences
- 3 – logarithm

Table B.1. Data and Transformations

Code	Description	Transform.	Source	PCA
U.S. Variables				
<i>Rates and Spreads</i>				
BaaAaa Spread	Difference between Moody's seasoned Baa corporate bond yield (DBAA) and Moody's seasoned Aaa corporate bond yield (DAAA)	1	FRED	Y
FEDFUNDS	Federal funds rate	1	FRED	
GS1	One-year Treasury rate	1	FRED	
GZ Spread	Excess bond premium	1	Gilchrist and Zakrajšek (2012)	Y
Mortgage Spread	Difference between 30-year mortgage rate and market yield on U.S. Treasury securities at 30-year constant maturity, quoted on an investment basis (DGS30)	1	FRED	Y
Prime Spread	Difference between bank prime loan rate (MPRIME) and federal funds rate	1	FRED	Y
Term Spread	Difference between GS10 and GS1	1	FRED	Y
<i>Housing Variables</i>				
CUSR0000SEHA	Rents: CPI for all urban consumers: Rent of primary residence in U.S. city average	2	FRED	
CSUSHPISA	S&P/Case-Shiller U.S. National Home Price Index	2	FRED	
HOUST	New privately owned housing units started	3	FRED	
PERMIT	New privately owned housing units authorized in permit-issuing places	3	FRED	
RHVRUSQ156N	Home vacancy rate	1	FRED	
RRVRUSQ156N	Rental vacancy rate	1	FRED	
RSAHORUSQ156S	Homeownership rate	1	FRED	

(continued)

Table B.1. (Continued)

Code	Description	Transform.	Source	PCA
<i>Financial Market Variables</i>				
BUSLOANS	Commercial and industrial loans, all commercial banks	2	FRED	Y
CMDEBT	Households and nonprofit organizations; Debt securities and loans; Liability, level	2	FRED	Y
DRSFRMACBS	Delinquency rate on single-family residential mortgages, booked in domestic offices, all commercial banks	1	FRED	
HHMSDODNS	Households and nonprofit organizations; One-to-four family residential mortgages; Liability, level	2	FRED	Y
M2SL	M2 money stock	2	FRED	Y
NPTLTL	Nonperforming total loans (past due 90+ days plus nonaccrual) to total loans	1	FRED	
REALLN	Real estate loans, all commercial banks	2	FRED	Y
SLOOS	Net percentage share of banks reporting tightening standards for mortgage loans (Senior Loan Officer Opinion Survey on Bank Lending)	1	Haver	
SP500	S&P 500	2	FRED	Y
TLAACBW027SBOG	Total assets, all commercial banks	2	FRED	
TOTBKCR	Bank credit, all commercial banks	2	FRED	Y
TWEXMMTH	Trade-weighted U.S. dollar index: Major currencies, goods	1	FRED	Y
USROA	Return on average assets for all U.S. banks	1	FRED	
VIX	CBOE volatility index	1	FRED	Y

(continued)

Table B.1. (Continued)

Code	Description	Transform.	Source	PCA
<i>Real Variables</i>				
CBI	Change in private inventories	1	FRED	Y
DSPI	Disposable personal income	2	FRED	
EXPGS	Exports of goods and services	2	FRED	Y
FDEFX	Federal government: National defense consumption expenditures and gross investment	2	FRED	Y
FNDEFX	Federal government: Nondefense consumption expenditures and gross investment	2	FRED	Y
GDPC1	Real gross domestic product	2	FRED	
IMPGS	Imports of goods and services	2	FRED	Y
INDPRO	Industrial production: Total index	2	FRED	Y
PCE	Personal consumption expenditures	2	FRED	
PCEDG	Personal consumption expenditures: Durable goods	2	FRED	Y
PCENDG	Personal consumption expenditures: Nondurable goods	2	FRED	Y
PCES	Personal consumption expenditures: Services	2	FRED	Y
PINCOME	Personal income	2	FRED	
PNFI	Private nonresidential fixed investment	2	FRED	Y
PRFI	Private residential fixed investment	2	FRED	Y
SLCE	State and local consumption expenditures and gross investment	2	FRED	
UNRATE	Unemployment rate	1	FRED	

(continued)

Table B.1. (Continued)

Code	Description	Transform.	Source	PCA
Other Variables				
CPI	Consumer price index for all urban consumers: All items in U.S. city average (CPIAUCSL)	2	FRED	Y
SENTIMENT	University of Michigan: Consumer sentiment	2	FRED	Y
State-Level Variables				
Housing Variables				
HOMEOWN _{<i>i</i>}	Homeownership rate	1	Haver	Y
HOMEVAC _{<i>i</i>}	Home vacancy rate	1	Haver	Y
HPI _{<i>i</i>}	All-transactions house price index (ALSTHPI)	2	FRED	Y
HPIINC _{<i>i</i>}	House price-to-income ratio calculated by dividing HPI _{<i>i</i>} by INC _{<i>i</i>}	2	Own calculations	
HPIRENT _{<i>i</i>}	House price-to-rent ratio calculated by dividing HPI _{<i>i</i>} by RENT _{<i>i</i>}	2	Own calculations	
HSTARTS _{<i>i</i>}	New privately owned housing units started	3	Haver	Y
PERMITS _{<i>i</i>}	New private housing units authorized by building permit (ALBPPRIVSA)	3	FRED	Y
RENT _{<i>i</i>}	State-level aggregation of MSA-level real rent index (interpolated with Denton 1971 method using rent of primary residence from U.S. CPI: CUSR0000SEHA, adj. for overall CPI)	2	Howard and Liebersohn (2021)	Y
RENTVAC _{<i>i</i>}	Rental vacancy rate	1	Haver	Y

(continued)

Table B.1. (Continued)

Code	Description	Transform.	Source	PCA
<i>Financial Variables</i>				
BA _i	Total assets for commercial banks (ALTASt)	2	FRED	Y
FORECL _i	All foreclosures started (in %)	1	Haver	Y
LTV _i	Loan-to-value ratio (in %)	1	Haver	
MDR _i	All mortgages past due (in %)	1	Haver	Y
MORTDEBT _i	State-level mortgage debt per capita (interpolated with Denton 1971 method using U.S. data)	2	Haver	Y
NBBANK _i	Nonbusiness bankruptcy filings	2	Haver	Y
NPL _i	Nonperforming loans (past due 90+ days plus nonaccrual) to total loans for banks: (ALNPTL)	1	FRED	Y
ROA _i	Return on average assets for banks (ALROA)	1	FRED	Y
<i>Real Variables</i>				
GOVEXP _i	Total governmental state expenditure taken from the Annual Survey of State Government Finances (interpolated with Denton 1971 method using U.S. government total expenditures: W068RCQ027SBEA)	2	U.S. Census Bureau	Y
GOVREV _i	State tax collections: Total taxes (QTAXTOTALQTAX-CAT3ALNO)	2	FRED	Y
GDP _i	Real GDP. Starting 2005:Q1 ALRQGSP for Alabama. Before 2005:Q1 interpolated with Denton (1971) method using U.S. real GDP (GDPC1)	2	FRED	Y
INC _i	Personal income (ALOTOT)	2	FRED	Y
NFC _i	All employees: construction (ALCONS)	2	FRED	Y
NFP _i	All employees: Total nonfarm (ALNA)	2	FRED	Y
STATEDEFL _i	State-level GDP deflator (before 2005:Q1 calculated based on interpolated nominal and real GDP)	2	FRED	Y
STATEINFL _i	State-level inflation	1	Hazell et al. (2022)	
STATEPCE _i	State-level personal consumption expenditure: ALPCE (interpolated with Denton 1971 method using U.S. data)	2	FRED	Y
UEB _i	State-level unemployment benefits (ALOBEN)	2	FRED	Y
UR _i	State-level unemployment rate (ALUR)	1	FRED	Y

(continued)

Table B.1. (Continued)

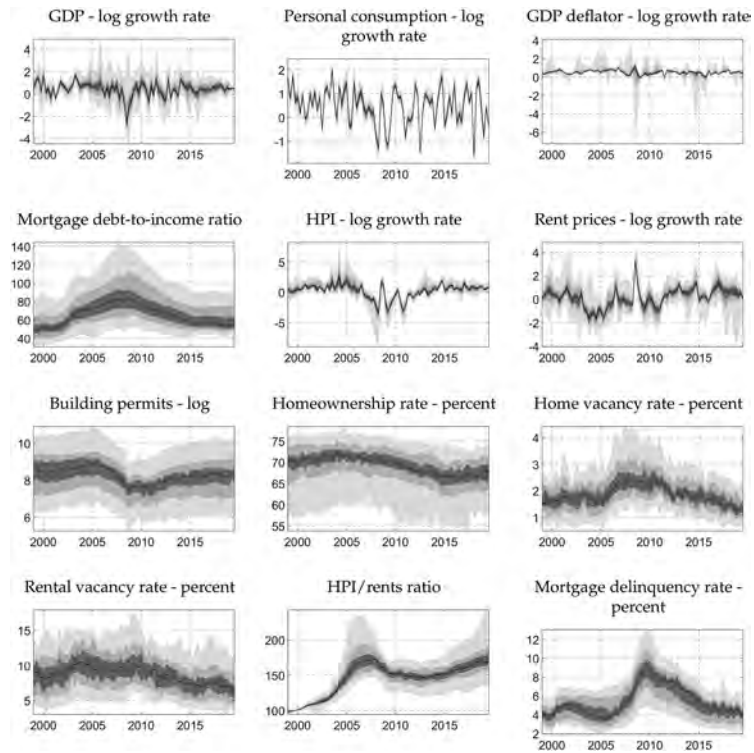
Code	Description	Transform.	Source	PCA
State-Level Variables for Cross-Sectional Regression				
Debt Gap	Calculated by applying the Hamilton (2018) filter to the state-level mortgage debt-to-income series (forecast horizon $h = 20$), max. value over 1999–2019	1	Own calculations	
HPI Growth	Cumulative growth of the real house price index over 1999–2019	1	Own calculations	
HW (Avg.)	Average housing wealth over 1999–2019. HW = (Homeownership rate $\times$ Housing units) $\times$ HPI $\times$ Median house price in 2000	1	Own calculations	
HW (Std.)	Standard deviation of housing wealth over 1999–2019	1	Own calculations	
LRI	Land-use restriction index (in 2014)	1	Herkenhoff, Ohanian, and Prescott (2018)	
Manufacturing	Share of manufacturing in state-level GDP (average over 1999–2019)	1	Own calculations, U.S. Bureau of Economic Analysis	
RTW	Dummy variable for states with right-to-work legislation	1	National Right to Work Legal Defense Foundation	
Sand State	Dummy variable for Arizona, California, Florida, and Nevada	1	Own calculations	
Small Firms	Share of employees in firms with less than 250 employees divided by total number of employees in that state (average over 1999–2019)	1	Own calculations, U.S. Bureau of Labor Statistics	

Table B.3. Values of Selected State Characteristics (sample averages)

State	LRI	Debt Gap	HW (Avg.)	HW (Std.)	Sand State	RTW	Manufacturing	Small Firms	HPI Growth
AL	-1.6	13.1	34,575.3	2,680.8	0	1	16.9	77.2	3.0
AK	—	9.7	46,293.9	3,970.5	0	0	2.9	77.2	10.7
AZ	-0.9	32.7	48,327.7	11,721.2	1	1	10.0	66.1	17.7
AR	-2.6	7.7	23,412.1	1,671.7	0	1	16.8	73.6	3.6
CA	-0.3	21.6	95,388.2	23,708.3	1	0	12.1	75.3	26.8
CO	-0.7	25.3	83,949.5	10,840.5	0	0	7.5	78.8	22.1
CT	-0.3	12.4	88,095.2	13,926.8	0	0	13.5	75.1	3.5
DE	-0.7	17.3	82,772.9	12,971.6	0	0	7.7	69.5	9.5
FL	-1.0	20.6	53,897.7	13,862.7	1	1	5.4	74.2	21.4
GA	-1.4	14.8	43,434.8	6,194.0	0	1	11.7	73.5	6.7
HI	—	16.6	123,974.9	27,701.1	0	0	2.0	75.9	26.7
ID	-1.3	19.5	41,584.8	6,466.8	0	1	12.5	80.9	18.7
IL	-1.6	15.3	56,062.8	7,630.4	0	0	13.4	68.3	0.4
IN	-2.6	12.3	35,715.6	2,911.5	0	1	28.1	70.6	1.0
IA	-3.7	7.1	30,652.7	1,624.2	0	1	19.4	75.0	4.7
KS	-2.8	9.5	27,845.3	2,285.8	0	1	15.7	74.7	6.1
KY	-2.5	8.9	34,795.7	2,447.1	0	1	18.9	74.6	4.9
LA	-1.6	6.3	27,668.1	3,156.1	0	1	19.9	79.0	9.1
ME	-1.1	7.2	77,794.9	9,091.6	0	0	10.7	80.1	15.8
MD	-0.4	12.0	81,152.2	15,671.4	0	0	6.1	77.6	14.5
MA	-0.2	11.4	146,481.5	20,720.6	0	0	11.2	71.8	18.3
MI	-2.4	18.5	53,730.8	8,098.9	0	0	19.9	68.3	-0.9
MN	-1.4	18.5	61,391.1	8,284.7	0	0	14.4	70.7	12.3
MS	-2.3	7.2	23,941.7	1,641.5	0	1	16.0	74.9	1.6
MO	-2.1	11.5	36,887.9	3,089.4	0	0	13.4	73.6	5.9

(continued)

Figure B.1. Dispersion of Variables across U.S. States



Note: Distribution of selected variables across U.S. states. The black line is the median realization, and the gray areas include 30 percent (35–65 percentile), 60 percent (20–80 percentile), and 90 percent (5–95 percentile) of the realizations, going from dark to lighter gray.

Appendix C. Additional Tables and Figures

Table C.1. Explanatory Power of Factors for Variables

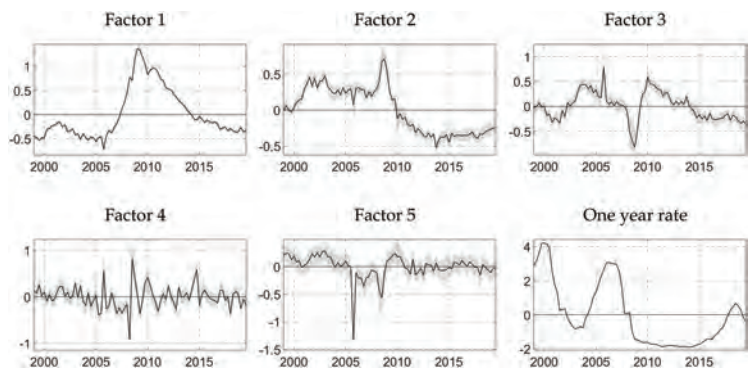
	Series	R^2
All Factors	New Housing Units Started: U.S.	0.9809
	Nonperforming Loan Share: Michigan	0.9784
	Nonperforming Loan Share: U.S.	0.9783
	New Permits: U.S.	0.9777
	Nonperforming Loan Share: Tennessee	0.9769
Factor 1	Nonperforming Loan Share: Minnesota	0.9440
	Nonperforming Loan Share: Florida	0.9365
	Mortgage Delinquency Ratio: California	0.9365
	Nonperforming Loan Share: Georgia	0.9345
	Nonperforming Loan Share: Illinois	0.9320
Factor 2	Homeownership Rate: Nevada	0.7793
	Homeownership Rate: U.S.	0.7401
	Homeownership Rate: California	0.6758
	Homeownership Rate: Colorado	0.6482
	Homeownership Rate: Georgia	0.6473
Factor 3	Rents: Washington	0.6137
	Rents: North Carolina	0.5180
	Rents: Oregon	0.5116
	Rents: Utah	0.4834
	Rents: Georgia	0.4742
Factor 4	Consumer Price Index: U.S.	0.5531
	Personal Expenditures: Ohio	0.5459
	Personal Expenditures: Louisiana	0.5268
	Personal Expenditures: Michigan	0.5254
	Personal Expenditures: Kentucky	0.5236
Factor 5	Nonbusiness Bankruptcies: Pennsylvania	0.6268
	Nonbusiness Bankruptcies: Texas	0.6253
	Nonbusiness Bankruptcies: Oklahoma	0.6114
	Nonbusiness Bankruptcies: Kansas	0.6034
	Nonbusiness Bankruptcies: Ohio	0.6021
One-Year Rate	Federal Funds Rate: U.S.	0.9728
	Return on Bank Assets: Wyoming	0.7094
	Mortgage Spread: U.S.	0.6819
	Unemployment Rate: Connecticut	0.6051
	Unemployment Rate: New Mexico	0.6049
Note: List of series that are best explained by the extracted factors, according to the R-squared of a linear regression of the (transformed) series on the respective factor.		

Table C.2. Pairwise Correlations with State-Level Responses after Three Years

	GDP	PCE	HPI	Rents	Permits	HOR	HPI/Rents	MDR	Mortg. Debt
LRI	-0.12 (0.43)	-0.43*** (0.00)	-0.64*** (0.00)	-0.17 (0.27)	-0.33** (0.02)	-0.01 (0.95)	-0.61*** (0.00)	0.60*** (0.00)	-0.35** (0.01)
Debt Gap	-0.45*** (0.00)	-0.55*** (0.00)	-0.53*** (0.00)	0.08 (0.61)	-0.50*** (0.00)	-0.43*** (0.00)	-0.62*** (0.00)	0.46*** (0.00)	-0.44*** (0.00)
HW (Avg.)	0.15 (0.30)	-0.14 (0.35)	-0.54*** (0.00)	-0.08 (0.61)	-0.19 (0.18)	0.14 (0.34)	-0.50*** (0.00)	0.53*** (0.00)	-0.51*** (0.00)
HW (Std.)	-0.12 (0.40)	-0.34** (0.01)	-0.78*** (0.00)	-0.21 (0.16)	-0.32** (0.02)	-0.06 (0.69)	-0.73*** (0.00)	0.66*** (0.00)	-0.68*** (0.00)
Sand State	-0.55*** (0.00)	-0.56*** (0.00)	-0.66*** (0.00)	-0.27* (0.07)	-0.53*** (0.00)	-0.42*** (0.00)	-0.60*** (0.00)	0.51*** (0.00)	-0.59*** (0.00)
RTW	-0.32** (0.02)	-0.15 (0.29)	0.32** (0.02)	0.01 (0.94)	0.28* (0.05)	-0.14 (0.34)	0.33** (0.03)	-0.27* (0.05)	0.28* (0.05)
Manufacturing	-0.04 (0.79)	0.31** (0.03)	0.55*** (0.00)	0.20 (0.18)	0.05 (0.75)	0.01 (0.93)	0.48*** (0.00)	-0.19 (0.18)	0.45*** (0.00)
Small Firms	0.09 (0.51)	0.06 (0.68)	0.16 (0.26)	0.15 (0.33)	0.49*** (0.00)	0.36*** (0.01)	0.11 (0.45)	-0.41*** (0.00)	0.26* (0.07)
HPI Growth	-0.02 (0.91)	-0.27* (0.05)	-0.53*** (0.00)	-0.01 (0.96)	0.07 (0.61)	0.08 (0.60)	-0.53*** (0.00)	0.20 (0.17)	-0.45*** (0.00)

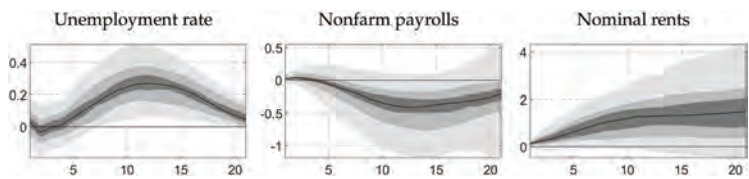
Note: GDP = real GDP, PCE = real personal consumption expenditure, HPI = real house price index, HOR = homeownership rate, MDR = mortgage delinquency rate, LRI = land-use restriction index of Herkenhoff, Othman, and Prescott (2018) for the year 2014, Debt gap = max. value of Hamilton-filtered mortgage debt-to-income ratio over 1999–2019, HW (Avg.) = average housing wealth over 1999–2019, HW (Std.) = standard deviation of housing wealth over 1999–2019, Sand state = dummy variable for Arizona, California, Florida, and Nevada, RTW = dummy variable for right-to-work states, Manufacturing = share of manufacturing in state-level GDP (average over 1999–2019), Small firms = share of small firms in total employment (average over 1999–2019), HPI growth = growth rate of house prices over 1999–2019, Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure C.1. Principal Components and One-Year U.S. Treasury Rate



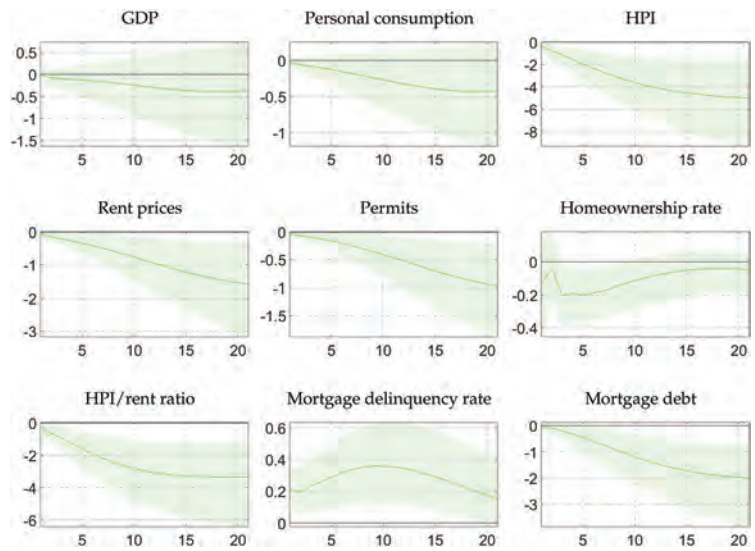
Note: Five unobserved factors (principal components) obtained from the normalized data as described in Table B.1 as well as the single observed factor (one-year Treasury rate). The gray shaded areas are the bootstrapped confidence bands (90 percent) around the principal components based on Gonçalves and Perron (2020).

Figure C.2. Dispersion of Additional State-Level Impulse Response Functions



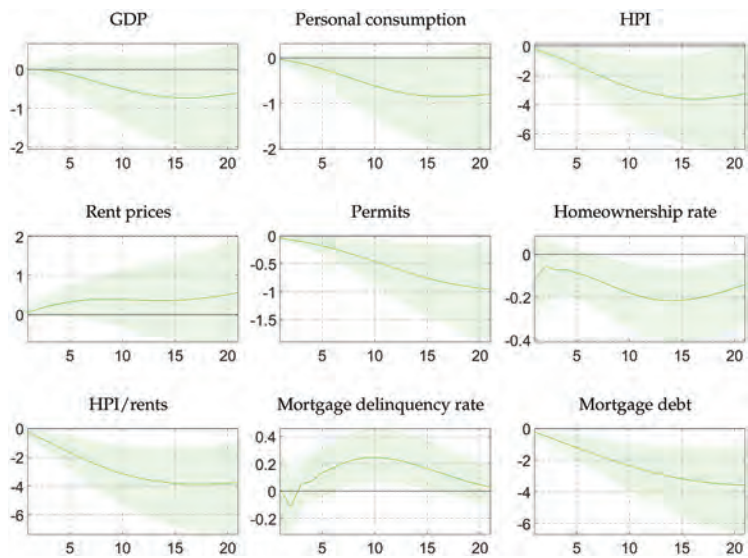
Note: See Figure 3.

Figure C.3. Differences in IRFs across Herkenhoff, Ohanian, and Prescott (2018) LRI (decile averages)



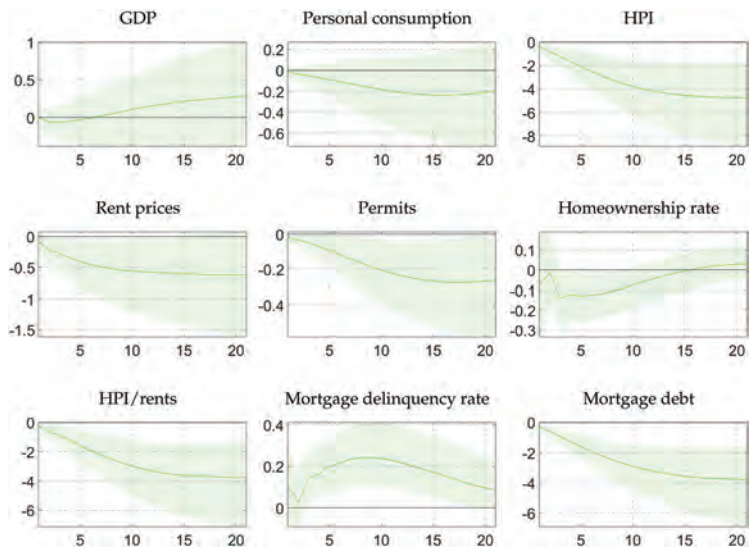
Note: Posterior distribution of the differences between the average (cumulative) IRFs across U.S. states that belong to the top and bottom decile of the respective state characteristic. The shaded area reflects the 68 percent HDI.

Figure C.4. Differences in IRFs across the Debt Gap Distribution (decile averages)



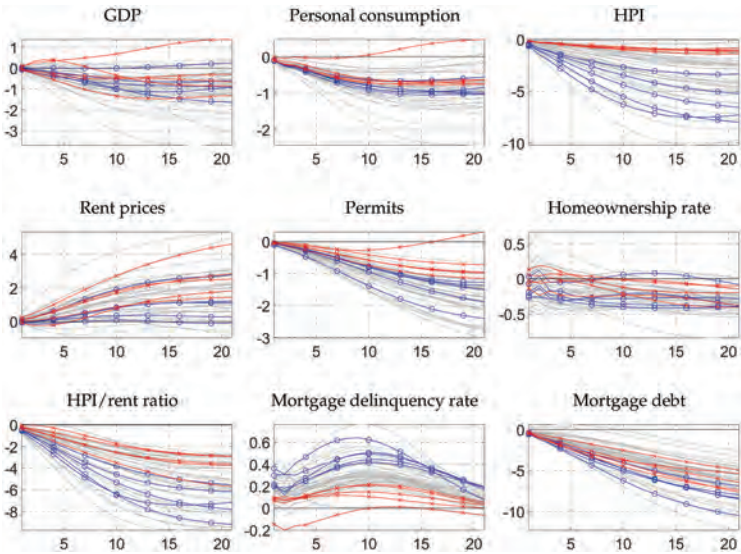
Note: See Figure C.3.

Figure C.5. Differences in IRFs across Housing Wealth Volatility (decile averages)



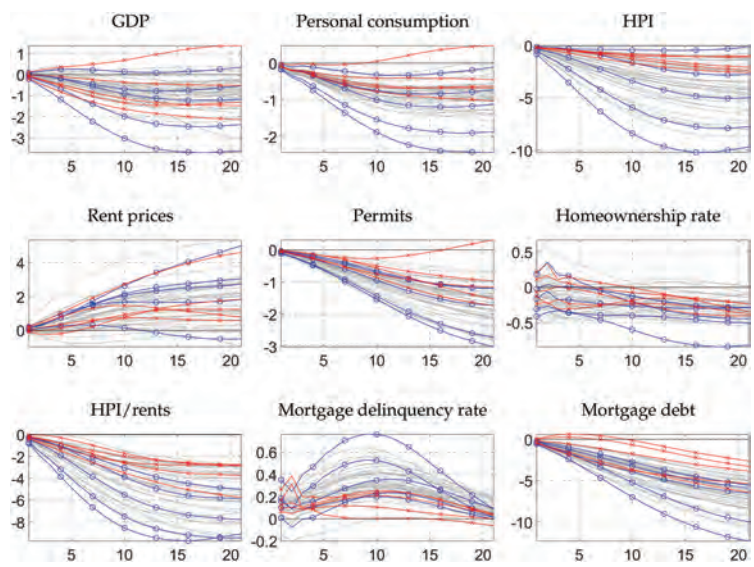
Note: See Figure C.3.

Figure C.6. Impulse Responses across Herkenhoff, Ohanian, and Prescott (2018) LRI



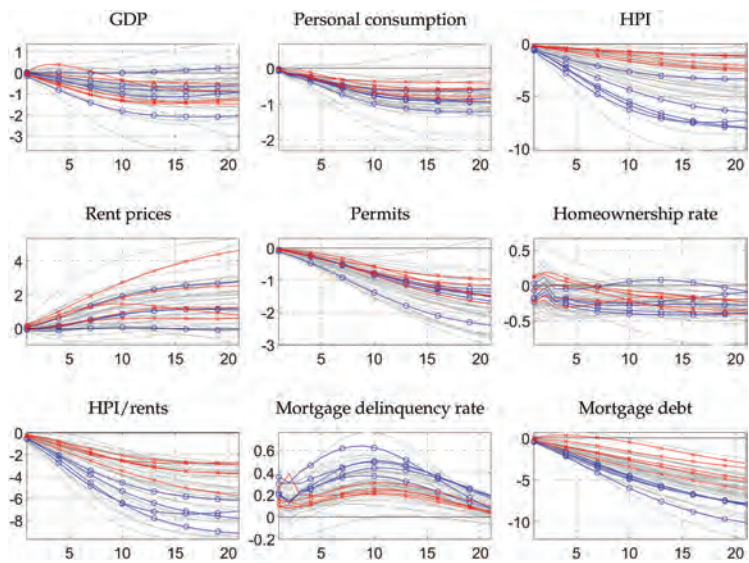
Note: Median (cumulative) IRFs across U.S. states after a monetary policy tightening that increases the one-year Treasury rate by 25 bps. The blue (red) lines with circles (crosses) show the responses of the states belonging to the top (bottom) decile of the respective state characteristic.

Figure C.7. Impulse Responses across the Debt Gap Distribution



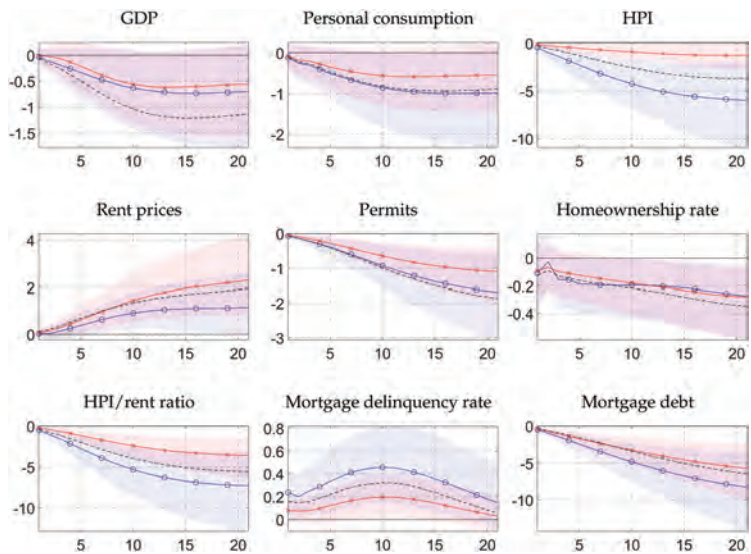
Note: See Figure C.6.

**Figure C.8. Impulse Responses across
Housing Wealth Volatility**



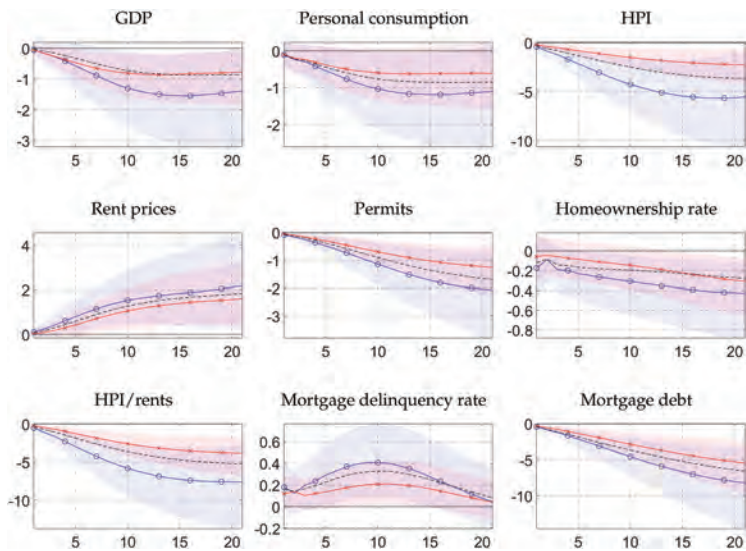
Note: See Figure C.6.

Figure C.9. Impulse Responses across Herkenhoff, Ohanian, and Prescott (2018) LRI (quintile averages)



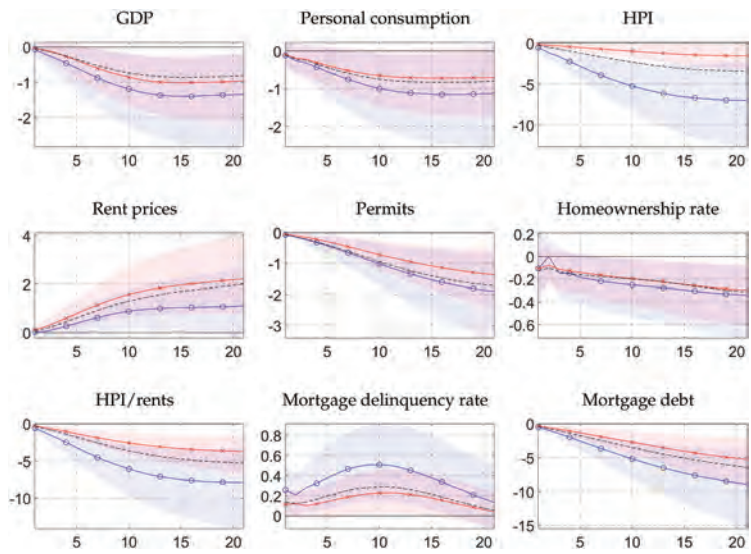
Note: Posterior distributions of the average (cumulative) IRFs across U.S. states after a monetary policy tightening that increases the one-year Treasury rate by 25 bps. The blue (red) line with circles (crosses) indicates the median (over all MCMC draws) of the average responses of the states belonging to the top (bottom) quintile of the respective state characteristic. Shaded areas reflect the 68 percent HDI. The dashed black line is the median of the average IRFs across all the other states.

Figure C.10. Impulse Responses across the Debt Gap Distribution (quintile averages)



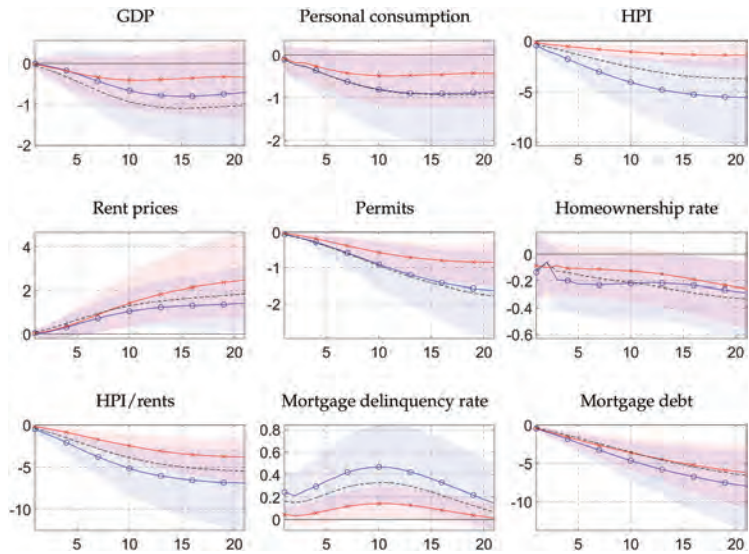
Note: See Figure C.9.

Figure C.11. Impulse Responses across Housing Wealth Volatility (quintile averages)



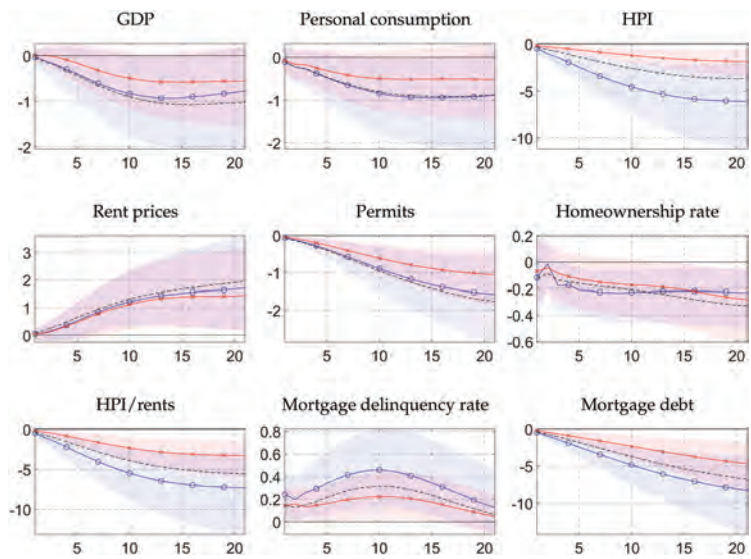
Note: See Figure C.9.

**Figure C.12. Impulse Responses across
Herkenhoff, Ohanian, and Prescott (2018)
LRI in 2000 (decile averages)**



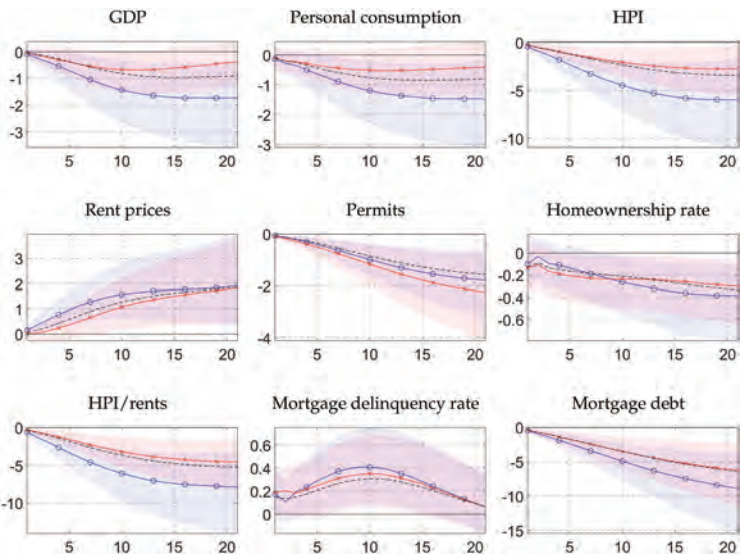
Note: See Figure 5.

Figure C.13. Impulse Responses across Home Values in 2000 (decile averages)



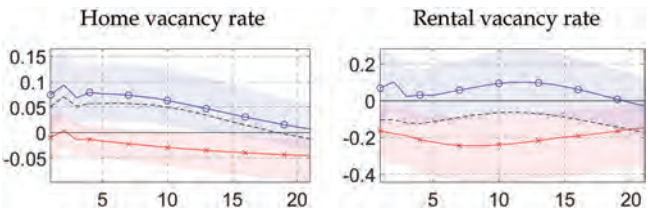
Note: See Figure 5.

Figure C.14. Impulse Responses across the House Price-to-Rent Ratio (decile averages)



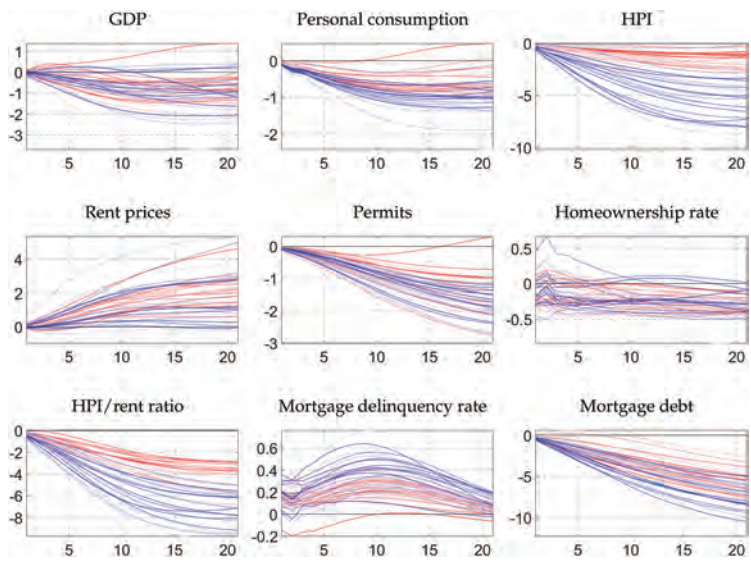
Note: See Figure 5.

Figure C.15. Impulse Responses of House Vacancy Rates across Herkenhoff, Ohanian, and Prescott (2018) LRI (decile averages)



Note: See Figure 5.

Figure C.16. IRFs across Housing Supply Elasticities from Blue (inelastic) to Red (elastic)



Note: Posterior median (cumulative) IRFs across U.S. states after a monetary policy tightening that increases the one-year Treasury rate by 25 bps. The coloring indicates the value of the respective state characteristic, ranging from blue (high) to red (low) values.

Figure C.17. Correlation between Land-Use Restrictions in 2014 (Herkenhoff, Ohanian, and Prescott 2018) and Housing Wealth per capita (avg. 1999–2019)

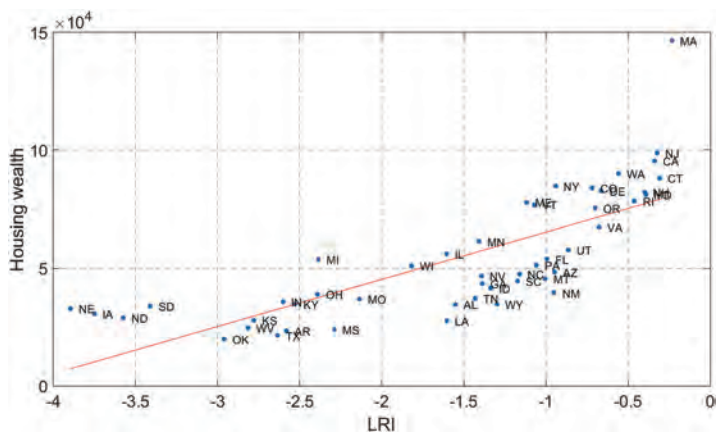


Figure C.18. Correlation between Land-Use Restrictions in 2014 (Herkenhoff, Ohanian, and Prescott 2018) and Consumption per capita (avg. 1999–2019)

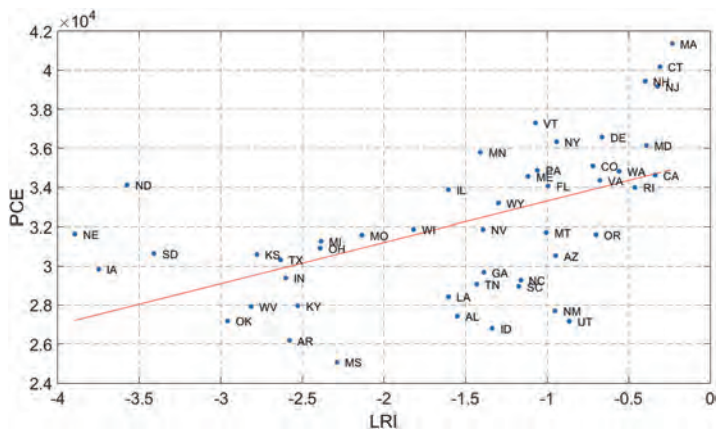


Figure C.19. Correlation between Land-Use Restrictions in 2014 (Herkenhoff, Ohanian, and Prescott 2018) and Personal Disposable Income per capita (avg. 1999–2019)

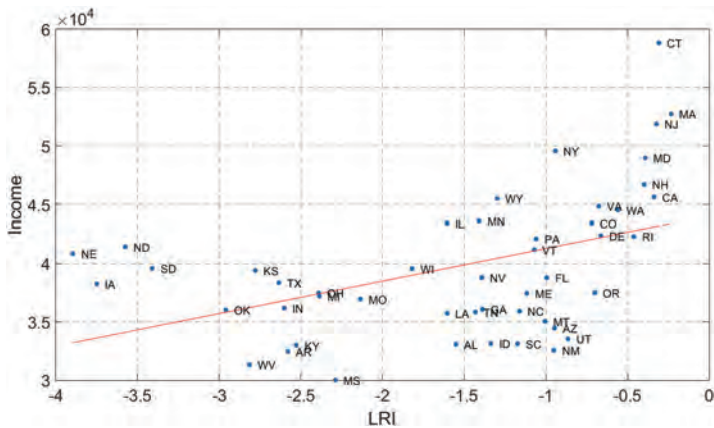


Figure C.20. Correlation between Land-Use Restrictions in 2014 (Herkenhoff, Ohanian, and Prescott 2018) and the Debt Gap (max. value over 1999–2019)

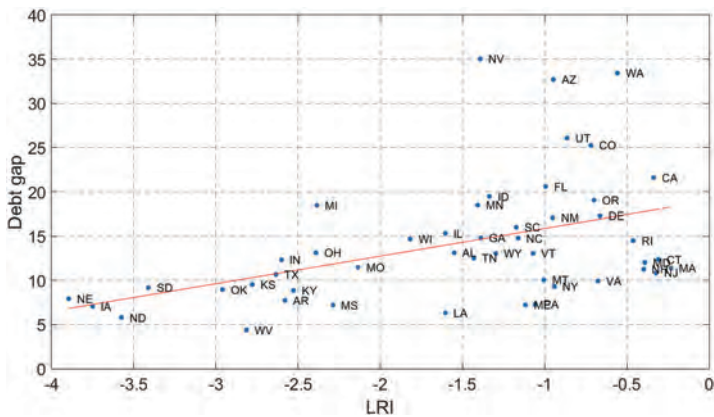
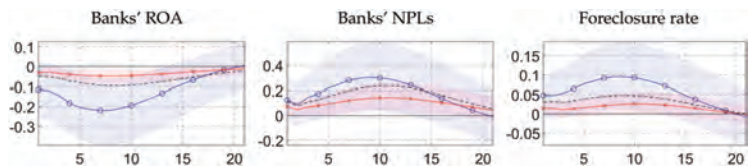
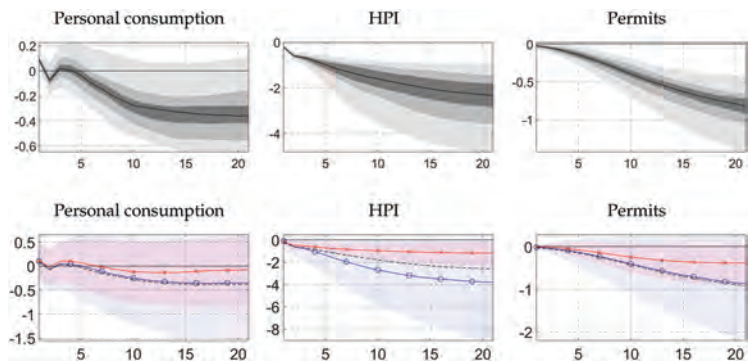


Figure C.21. Impulse Responses of Selected Banks' Indicators across the Debt Gap Distribution (decile averages)



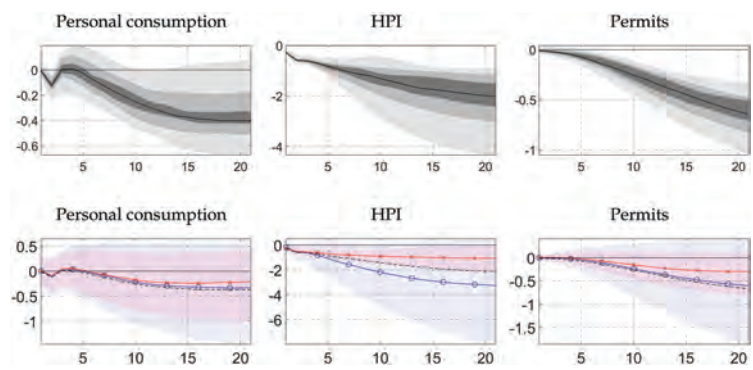
Note: See Figure 5.

Figure C.22. Alternative Monetary Policy Surprises (Swanson 2021 Forward Guidance)



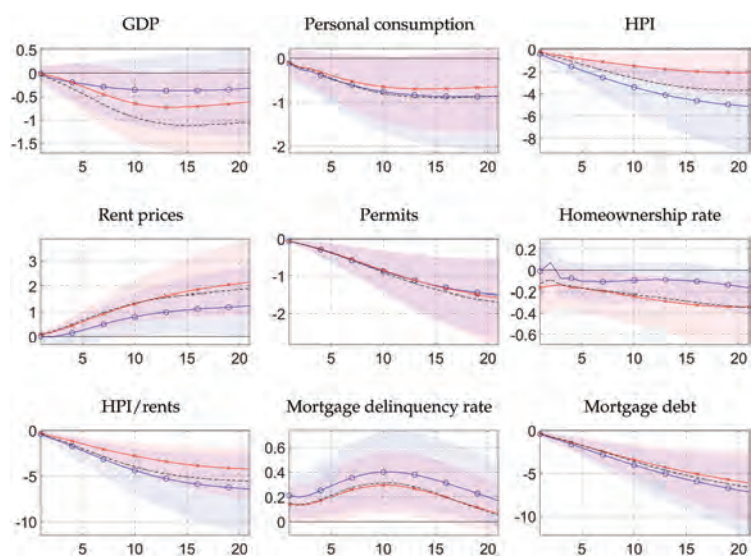
Note: The first row of this figure shows the distribution of the median (cumulative) IRFs across U.S. states after a monetary policy tightening that increases the one-year Treasury rate by 25 bps. The black line is the median response of all state-level (median) responses. The gray areas include 30 percent (35–65 percentile), 60 percent (20–80 percentile), and 90 percent (5–95 percentile) of the median responses, respectively, going from dark to lighter gray. The second row shows the posterior distributions of the average (cumulative) IRFs across U.S. states. The blue (red) line with circles (crosses) indicates the median (over all MCMC draws) of the average responses of the states belonging to the top (bottom) decile of the land-use restriction index in 2014 (Herkenhoff, Ohanian, and Prescott 2018). Shaded areas reflect the 68 percent HDI. The dashed black line is the median of the average IRFs across all the other states.

Figure C.23. Alternative Monetary Policy Surprises (Jarociński and Karadi 2020)



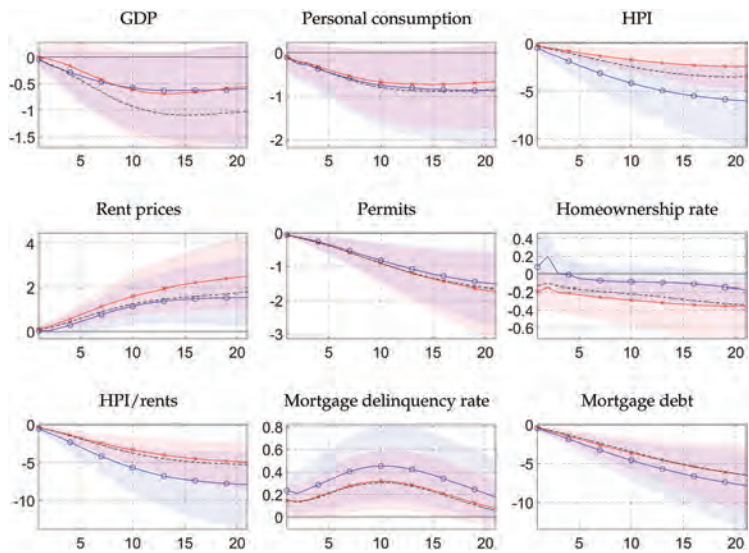
Note: See Figure C.22.

Figure C.24. Impulse Responses across Aastveit, Albuquerque, and Anundsen (2023) Supply Elasticities over 1996–2006 (decile averages)



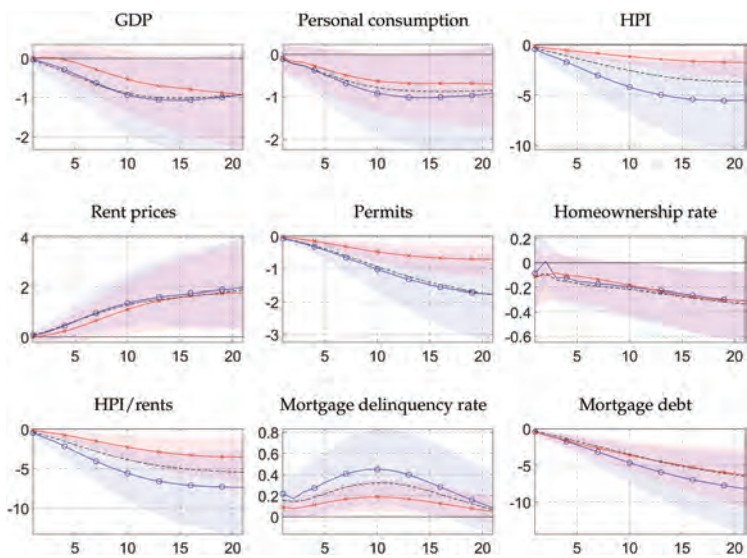
Note: See Figure 5.

Figure C.25. Impulse Responses across Aastveit, Albuquerque, and Anundsen (2023) Supply Elasticities over 2012–19 (decile averages)



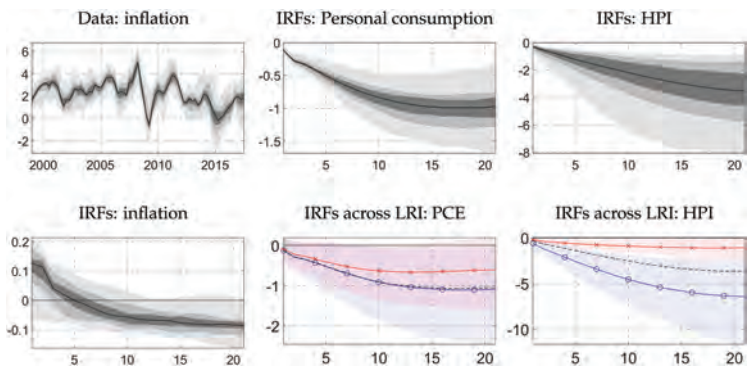
Note: See Figure 5.

Figure C.26. Impulse Responses across Saiz (2010) Supply Elasticities (decile averages)



Note: See Figure 5.

Figure C.27. Alternative State-Level Inflation Series (Hazell et al. 2022)



Note: See Figure B.1 (first chart), Figure 3 (second, third, and fourth chart), and Figure 5 (fifth and sixth chart).

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The Reliability of the Nominal GDP Expectations Gap*

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Arguments for nominal income targeting are often dismissed because it is an unreliable measure. To assess these concerns, we compare the real-time performance of several nominal and real measures of economic slack. We find that the nominal GDP expectations gap—the difference between nominal GDP and average projections thereof from surveys of professional forecasters—performs well as a measure of economic slack: its historical revisions are two to three times smaller than other measures, it significantly improves real-time forecasts of inflation since the pandemic, and it makes monetary policy rules up to 40 percent less volatile. Overall, concerns about nominal income targets are misplaced.

JEL Codes: C53, E32, E37, E47.

1. Introduction

Many economists have argued that a nominal income target can produce better economic outcomes than other monetary policy frameworks; see, for example, Hall and Mankiw (1994), Frankel and Chinn (1995), Orphanides (2003), Sheedy (2014), Bullard and Singh (2020), and Beckworth and Hendrickson (2020). However, there are also

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critiques of a nominal GDP (NGDP) targeting framework; see, for example, Ball (1999) and Svensson (2020a). One concern is whether NGDP estimates are reliable enough to be targeted. For example, when Federal Reserve officials discussed alternative monetary frameworks during the Federal Open Market Committee (FOMC) meeting in November 2011, then-president of the Federal Reserve Bank of Philadelphia Charles Plosser, voicing the concerns of several FOMC members, argued that under an NGDP targeting framework, if monetary policymakers “misestimate the so-called gap that needs to be closed, we could make serious policy errors, and those would accumulate over time.”¹ Thus, these policy errors could have lasting impacts.

Concern about the real-time measurement of policy-relevant gaps is not unique to NGDP. Potential GDP is unobservable and difficult to measure in real time. Estimates of the output gap vary greatly at any given time and are subject to substantial revisions. This has had important implications for predicting inflation and the implementation of monetary policy; see, for example, Orphanides (2001), Orphanides and van Norden (2002), and Orphanides and van Norden (2005). Recent evaluations of the performance of policy-relevant output gaps indicate that while these concerns have diminished, they continue to persist; see, for example, Edge and Rudd (2016), Champagne, Poulin-Bellisle, and Sekkel (2018), Berge (2023), and Furlanetto et al. (2023). This has spurred the search for reliable real-time estimates; see, for example, Kamber, Morley, and Wong (2018), Quast and Wolters (2022), and Barbarino, Berge, and Stella (2024).

Despite the extensive literature on the reliability of output gaps and how to improve them, real-time performance has not been evaluated in nominal terms. This paper assesses the reliability of NGDP gaps and compares their performance with real gaps. We focus on the NGDP expectations gap proposed by Beckworth (2020) and extended by Schibuola and Martinez (2021), which uses historical surveys of professional forecasters’ NGDP projections to construct a gap that measures the difference between what NGDP was and what

¹Excerpted from the transcript of the Meeting of the FOMC on November 1–2, 2011. Available online from the Federal Reserve Board at <https://www.federalreserve.gov/monetarypolicy/files/FOMC20111102meeting.pdf> (last accessed October 4, 2022).

it was expected to be. This measure differs from typical NGDP rules and is similar to forecast targeting in that it allows the target to grow at a nonconstant rate; see Svensson (2020b). We compare the NGDP expectations gap against the real-time performance of nominal and real measures of the output gap from the Federal Reserve Board (FRB), the Congressional Budget Office (CBO), and forecast-based measures.

The NGDP expectations gap performs well. First, its historical revisions are two to three times smaller than measures produced by the FRB staff or the CBO and are comparable with or slightly better than statistical output gap measures. Second, it significantly improves forecasts of core personal consumption expenditures (PCE) inflation following the COVID-19 pandemic. Finally, the implied interest rate targets from standard Taylor rules are up to 40 percent less volatile in real time when using the NGDP expectations gap than when using output gaps. Overall, this indicates that the concerns associated with the real-time implementation of NGDP targets and the implications for policymaking are misplaced.

The rest of the paper proceeds as follows: Section 2 describes existing forecast-based measures of economic slack. Section 3 presents the NGDP expectations gap. Section 4 compares the real-time properties of the NGDP expectations gap with other nominal and real gap measures. Subsections 4.2 and 4.3 evaluate how well the various gap measures predict future inflation, and interest rate targets. Section 5 concludes.

2. Forecast-Based Measures of Economic Slack

There are many ways to measure economic slack, with differing implications for economic policy. As Kiley (2013) argues, the various approaches can be broadly classified into three definitions of what output has deviated from: (i) an underlying trend, (ii) the implied production-consistent level of output, and (iii) the “natural” level of output. While all three definitions have important considerations, this paper focuses on the first definition.² This section sets the stage for describing our methods by briefly reviewing two existing

²For example, see Blanchard and Quah (1989), Justiniano and Pimiceri (2008), and Shackleton (2018) for discussions on the implications of the other definitions.

forecast-based output gap measures and discussing the conceptual similarities and differences between them.

2.1 Beveridge-Nelson Decomposition

A forecast-based measure of the output gap can be obtained from the Beveridge and Nelson (1981) decomposition. According to Morley (2002), for some integrated series, y_t , the Beveridge-Nelson (BN) trend is the minimum mean square error forecast of the long-run level of the series excluding the deterministic drift. This can be represented as the contemporaneous level of the series plus the sum of the H -period-ahead forecasts of the first differences of y_t :

$$\text{BN}_t \equiv \lim_{H \rightarrow \infty} \mathbb{E}_t [y_{t+H} - H\mu] = y_t + \lim_{H \rightarrow \infty} \sum_{h=1}^H \mathbb{E}_t [\Delta y_{t+h} - \mu], \quad (1)$$

where μ represents the deterministic drift, $\Delta y_t = y_t - y_{t-1}$, and $\mathbb{E}_t[\cdot]$ captures the expected value based on information at time period t . The difference between the BN trend and the data is calculated as the implied forecast error by subtracting y_t from Equation (1) to obtain the BN cyclical component. This implied BN output gap appears in numerous studies; see, for example, Morley (2011) and Berger, Morley, and Wong (2020).

The BN cyclical component, i.e., the second term on the right-hand side of Equation (1), is obtained after assuming which model generates the most accurate long-run forecast. For example, if an autoregressive model with p lags, i.e. an $\text{AR}(p)$, produces the most accurate long-run forecasts of y_t , then the implied output gap from the BN decomposition becomes

$$y_t - \text{BN}_t = - \left(\frac{\alpha(L)}{1 - \alpha(L)} \right) (\Delta y_t - \mu), \quad (2)$$

where $\alpha(L) = \sum_{j=1}^p a_j L^{j-1}$ and where L is the lag operator such that $L^p y_t = y_{t-p}$ and a_j are the autoregressive parameters. Parameters are estimated using Δy_t , even though it may not produce the best forecast for y_t at longer horizons; see, for example, Clements and Hendry (1993, 2001a, 2001b).

BN estimates often indicate cycles with smaller amplitudes than other measures. Kamber, Morley, and Wong (2018) impose restrictions on the parameter estimates to increase the amplitude of BN cycles. They show that increases in the persistence of the estimated autoregressive parameters align BN estimates more closely with policy-relevant measures of the output gap while retaining the reliability of the estimation procedure in real time.

2.2 *Hamilton Filter*

The Hamilton (2018) filter is a forecast-based measure developed due to concerns about the Hodrick and Prescott (1997) filter.³ Like the BN decomposition, the Hamilton filter is based on long-run forecasts. Specifically, Hamilton (2018) captures the underlying trend using eight-period-ahead forecasts from an AR(4) model of the level of y_t . Quast and Wolters (2022) modify this approach by averaging over forecasts from multiple horizons to better capture a wider range of business cycle frequencies:

$$MHF_t \equiv y_t - \frac{1}{9} \sum_{h=4}^{12} \hat{\mathbb{E}}_{t-h} [y_t], \quad (3)$$

where the forecast generated for time period t using an AR(4) model is represented by $\hat{\mathbb{E}}_{t-h} [y_t]$.

This modified Hamilton filter (MHF) averages over past forecasts ranging between 4 and 12 quarters prior to the period of interest. However, Quast and Wolters (2022) also show that estimates of the trend centered around the 10-quarter-ahead forecast horizon would allow for the inclusion of longer financial/credit cycles in the standard business cycle; see Beaudry, Galizia, and Portier (2020). They also show that wider windows are more closely correlated with National Bureau of Economic Research (NBER) recession definitions and policy-relevant output gaps such as those from the FRB and the CBO.

³Phillips and Shi (2021) and others show how to improve the performance of the HP filter.

3. The NGDP Expectations Gap

As noted above, this paper focuses on the deviation from trends approach to measuring economic slack as outlined by Kiley (2013). The BN decomposition and the Hamilton filter employ this approach, but assume parameter constancy and that the data generation process of y_t is well represented by the assumed model. However, the appropriate model at any given time is generally unknown and the large fluctuations in output over the past several decades indicate clear evidence of structural breaks. This section presents the expectations gap as an alternative approach that addresses these challenges.

3.1 *Expectations Gap*

One way to circumvent the concerns surrounding parameter constancy and model reliability while still capturing the expected trend is to use estimates from a set of models rather than any single procedure.⁴ Following Beckworth (2020) and Schibuola and Martinez (2021), we use surveys of professional forecasters to measure deviations from the expected trend.⁵ The idea is that forecasters generate projections using the best available methods at the time to represent their expectations about future trends. Moreover, aggregating forecasts reduces the influence of each model and allows for the possibility that the best model for predicting y_t may evolve over time.

We combine professional forecasts of the growth rate at each horizon with the latest observed log-level of y_t to obtain the expected level:⁶

$$\tilde{\mathbb{E}}_{t-h} [y_{i,t}] = y_{t-h-1} + \sum_{j=0}^h \ln \left(1 + \tilde{g}_{i,t-h+j||t-h} \right), \quad (4)$$

⁴Other studies have considered model-based combinations; see, for example, Morley and Piger (2012), Guérin, Maurin, and Mohr (2015), Pichette et al. (2019).

⁵This is related to an approach in Coibion, Gorodnichenko, and Ulate (2018) where the output gap is based on survey forecasts of long-run growth.

⁶In practice we use the level of output. The log-level is used here to facilitate comparisons with existing approaches.

where $\tilde{\mathbb{E}}_t[\cdot]$ is an estimate of the expected value given the information available at time t , $\tilde{g}_{i,t||t-h}$ is the expected quarterly output growth rate from a given forecaster or group of forecasters, i , generated from the survey at period $t-h$ for all quarters up through period t , and y_{t-h-1} is the last available log-level of output that would have been observable when the forecast was generated.

While we allow y_{t-h-1} to vary with new data vintages, forecasts of the growth rates are fixed across vintages since these are only available in real time. This means that the expectations gap is inherently quasi-real-time. It also circumvents one of the largest sensitivities of output gap measures by limiting how much forecasts change across data vintages; see Orphanides and van Norden (2002).⁷

The measure of expectations in Equation (4) is similar to those measured by the BN trend in Equation (1) or the Hamilton filter in Equation (3).⁸ While the BN trend is based on forecasts using information up through time period t , the main difference is the choice of models. For example, if professional forecasters all relied on the same AR(4) model to predict future the expected value would be essentially identical to the trend derived using a modified Hamilton filter.

We construct an average expected trend by averaging estimates of expected output across a range of forecast horizons. Beckworth (2020) refers to this estimate as the “neutral level” of output. The gap between observed output y_t and its average expected value at time period t is the expectations gap

$$EGAP_t \equiv y_t - \frac{1}{20} \sum_{h=0}^{19} \tilde{\mathbb{E}}_{t-h}[y_{i,t}], \quad (5)$$

⁷Using fixed growth rate forecasts can be interpreted as a restricted version of Quast and Wolters (2022) where the expected growth rates are estimated over shorter samples and older vintages of data. We can analyze how estimates of expected output are revised in real time with changes to the initial level of output by adding additional notation such that the initial level of output for a given vintage of data would be denoted as $y_{t-h-1,v}$ where v specifies the vintage.

⁸Robust prediction methods suggest links across measures; see Martinez, Castle, and Hendry (2022). Showing this is left to future research.

where the 20-quarter range implies that the estimate is centered around 10 quarters ahead, which is longer than the modified Hamilton filter. While the extended range means that some cyclical aspects of the forecasts could enter estimates of the trend at shorter horizons (see Isiklar and Lahiri 2007), an extended range centered around 10 quarters ahead is able to filter out longer financial cycles, consistent with the results in Quast and Wolters (2022).

The choice of forecaster or group of forecasters is important for the expectations gap. While it is possible to construct estimates of the expected trend, as in Equation (5), for individual forecasters, constructing a consensus estimate based on the mean or median forecaster helps robustify against model choice.⁹ As the average of all individual forecasters represents the central tendency, it excludes and can be distorted by information that is contained in the full range of forecasters. For example, forecaster disagreement has been linked to the business cycle; see, for example, Patton and Timmermann (2010) and Bürgi and Sinclair (2021). A large dispersion around the central tendency also indicates greater uncertainty about the expected trend; see Lahiri and Sheng (2010). This type of uncertainty can be quantified by constructing Equations (4) and (5) using forecasters that represent a specific range, e.g. by generating a measure for all i where $i \in \{10\text{th percentile, median, } 90\text{th percentile}\}$.¹⁰

3.2 *The Expectations Gap Using Survey Forecasts*

The expectations gap uses surveys of professional forecasters in its construction. The two surveys we focus on are the Survey of Professional Forecasters (SPF) and the Blue Chip Economic Indicators (BCEI). The SPF is a quarterly survey of about 40 academic and business forecasters first published in 1968 as the American Statistical Association/National Bureau of Economic Research Outlook Survey. Management of the survey was transferred to the Federal

⁹A long literature shows that pooling forecasts can be more accurate in the context of structural change and model uncertainty; see, for example, Bates and Granger (1969), Hendry and Clements (2004), Timmermann (2006), Genre et al. (2013).

¹⁰Another way to measure uncertainty is to use a factor stochastic volatility model of real-time estimates; see Berge (2023).

Reserve Bank of Philadelphia in 1990. The BCEI is a monthly survey of more than 50 business economists that began in 1976 and is maintained by the firm Wolters Kluwer.

The SPF is published in the second month of every quarter with quarterly forecasts that extend up to four quarters ahead, annual forecasts that extend out two years, and forecasts of annual average growth rates that extend out 10 years. The BCEI is published around the 10th day of each month with quarterly forecasts that extend up to eight quarters ahead, annual forecasts that extend out two years, and forecasts of annual averages that go to 10 years.

The quarterly forecasts from both surveys directly provide the near-term growth rate forecasts necessary to construct Equation (4). However, since we extend the level forecasts 20 quarters ahead, it is necessary to impose an assumption about forecasters' expectations about longer-term growth rates.

The simplest assumption is that the last available quarterly growth rate forecast is the expected long-run equilibrium growth rate. For the SPF, this implies that the expected growth rates in Equation (4) would be $\tilde{g}_{i,t-h+j||t-h} = \tilde{g}_{i,t-h+4||t-h}$ for the $j \geq 4$ forecast horizons. While this assumption is generally innocuous, since forecasters typically do not have much information beyond the one- to two-year forecast horizon, it may be problematic around turning points if forecasters are expecting a recession at the end of the forecast horizon or a slow recovery, which would distort the implied long-run expected growth rate. Thus, we choose this option only when there are no longer-term forecasts available.

Our preferred assumption is that the annual average growth rate forecasts can serve as a proxy for quarterly growth rates at horizons beyond one year ahead and that these expected long-run growth rates prevail immediately after the last available quarterly growth forecast. This allows for the quarterly growth forecasts to have cyclical information but longer-run growth forecasts capture the long-run equilibrium growth rates.

In practice, we use a mix of these assumptions across both surveys. The BCEI updates its long-run GDP growth rate forecasts on a semiannual basis. Thus, it meets the conditions for our preferred assumption and we extend the quarterly growth rate forecasts beyond eight quarters ahead with the last available annual growth rate forecasts. However, the SPF only produces long-run forecasts of

GDP growth and CPI inflation, which it updates at an annual and quarterly frequency respectively. Furthermore, the SPF's long-run forecasts are only available from 1990 onward.

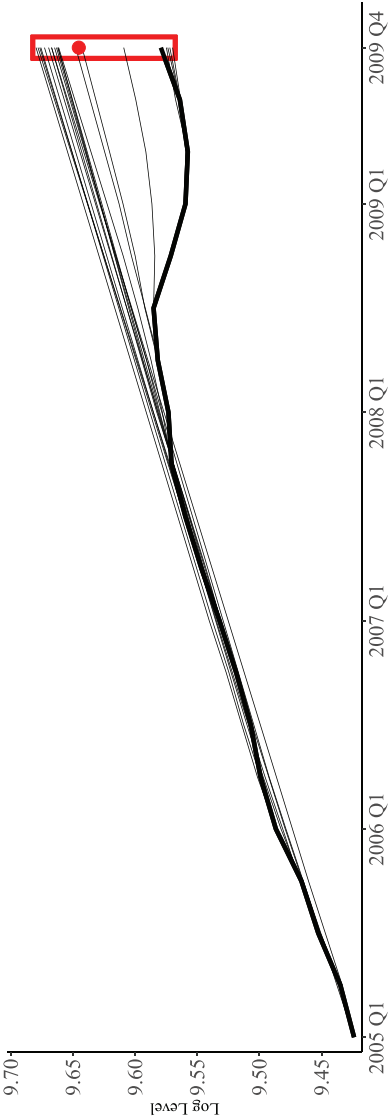
To circumvent these limitations for the SPF, we construct a proxy long-run NGDP growth rate forecast by combining the expected long-run real GDP growth rate forecasts with the long-run CPI inflation forecasts adjusted for the expected gap between CPI and GDP inflation that prevails at shorter forecast horizons and then use that to extend out the quarterly growth rate forecasts beyond four quarters ahead. We also extend the SPF back prior to 1990 using the assumption that the last available quarterly forecast growth is the expected long-run growth rate. These choices ultimately have very little impact from an empirical or practical standpoint since we average across many different forecasts and forecast horizons. This makes estimates of the expectations gap less sensitive to how any given forecast is constructed.

Figure 1 illustrates our approach for generating the SPF's expected level of NGDP for 2009:Q4. We start by constructing a series of log-level NGDP forecasts for 2009:Q4 using quarterly SPF NGDP growth rate forecasts for each quarter since 2005:Q1 (i.e., 19 quarters prior to 2009:Q4). Each thin black line represents an NGDP forecast path constructed according to Equation (4). In total there are 20 thin lines which terminate in 2009:Q4. The rectangular box denotes the range of these forecasts. Next, we take the average of these forecasts for 2009:Q4, as represented by the large red dot. The expectations gap in Equation (5) is then constructed by taking the difference between the dot and the actual value as represented by the thick black line. We repeat this process every quarter and generate a series of expectations gaps.

We can also construct a similar series using the BCEI. However, since the BCEI is published monthly, there is more than one forecast available for each quarterly horizon. To account for this, we average across all available monthly forecasts for the horizons of interest. Thus, since the forecast horizon of interest in Equation (5) extends up to 20 quarters, we average over 60 monthly forecasts rather than 20 quarterly forecasts.

To capture the uncertainty around the expectations gap, we use information on the dispersion across individual forecasts. For the SPF, which publishes individual forecasts, it is possible to calculate

Figure 1. Estimating the SPF's Expected NGDP Level Forecast for 2009:Q4



forecaster percentiles and a range of forecaster disagreement. A cruder proxy of forecaster disagreement is available using the BCEI averages of the top and bottom 10 forecasters, which are available beginning in 1990.

3.3 *Estimates*

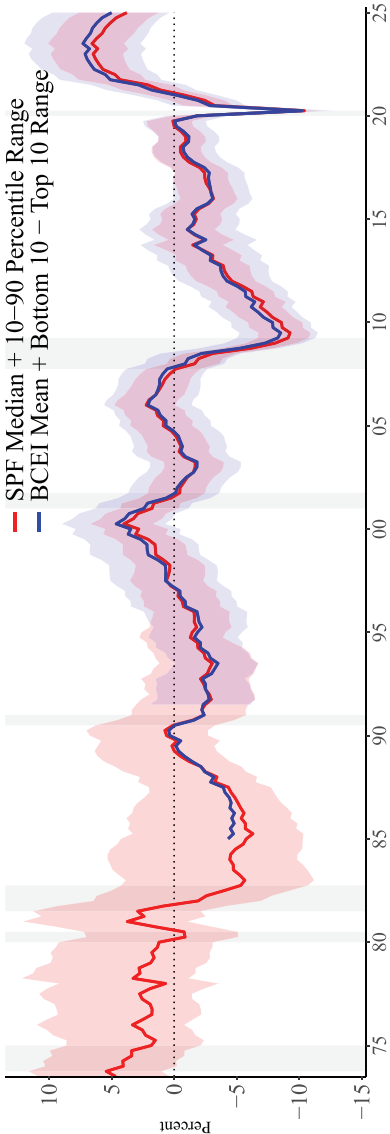
Figure 2 shows the NGDP expectations gaps and their ranges from the SPF and the BCEI. To extend the SPF estimates as far back as possible, we fill in missing long-run growth forecasts with the longest available quarterly growth rate forecast at the time as a proxy for the long-run growth rate up to five years ahead as described above. Doing so allows us to extend estimates for the SPF back to 1974.

Figure 2 shows that, except for the mid-to-late 1980s, the BCEI and SPF measures are very similar and appear to capture important features associated with economic cycles. Except for the 1973–75 recession, the gaps always turn negative during a recession. The gaps also capture the sharp economic downturns surrounding the 1981–82 recession, the 2008–09 Great Recession, and the COVID-19 pandemic-induced recession in 2020. The gaps also indicate episodes of a boom economy around periods associated with the late 1990s dot-com bubble, the housing bubble of the mid-2000s, and the inflationary period of the pandemic recovery in 2021–24.

The measures of forecaster disagreement surrounding the estimates of the expectations gap indicate five episodes coinciding with important economic periods during which the economy clearly performed substantially above or below most individual forecasters' expectations. The first episode occurred during the dot-com era boom; the second during and after the 2008–09 recession, when the economy grew much slower than expected; the third during the so-called invisible recession of 2015–16;¹¹ and the fourth episode during the recession in 2020 induced by the COVID-19 pandemic. Finally, during the inflationary period of the pandemic recovery in 2021–24 the entire range moved above zero. Each of these episodes corresponds to important economic events and supports

¹¹See Irwin (2018). Alternatively, Baumeister, Leiva-Léon, and Sims (2024) refer to this episode as the 2014–16 oil price slump.

Figure 2. NGDP Expectations Gap



Note: Shaded bars denote NBER recession dates.

the interpretation of the NGDP expectations gap as an indicator of changes in the business cycle.

The range of disagreement also conveys how the surveys' designs and how forecasters' views of the economy have evolved. In the 1970s and 1980s, there was large forecaster disagreement about the state of the economy, with the range only ever fully going above zero in 1974 but otherwise showing a large spread for most of the 1970s. The widespread disagreement among forecasters about the underlying economic trends persisted through the 1980s. This result is robust even if the post-1990 measures are constructed using the furthest available short-run growth rate forecast as a proxy measure for the long-run growth rates. However, it may be sensitive to changes in how the survey has been conducted since 1990 or the Federal Reserve's adoption of an implicit and then explicit inflation target in the 2000s. Notably, while in 2021–24 economic growth and inflation returned to a period reminiscent of the early 1980s, forecasts do not signal a return to the pre-1990s level of disagreement.

4. Nominal and Real Gaps in Real Time

This section compares and contrasts various real-time estimates of nominal and real gaps, starting with a visual comparison of alternative measures and then a formal examination of the revision properties. Next, we analyze the performance of the various measures in real time by comparing their ability to predict future inflation. Finally, we examine the implied interest rate target from Taylor rules using the various real-time gap estimates. While there are similarities across the measures, the NGDP expectations gap performs well. Its historical revisions are two to three times smaller than standard output gap measures and it significantly improves near-term forecasts of core PCE inflation following the COVID-19 pandemic. Finally, when used inside of a Taylor rule, the NGDP expectations gap generates implied interest rate targets that are up to 40 percent less volatile than those implied from standard output gap measures.

This analysis compares the real-time estimates of the nominal and real expectations gaps with four alternative real-time measures: (i) the output gaps and the implied nominal and real expectations gaps constructed from the FRB staff's forecasts, (ii) the CBO's nominal and real output gaps, (iii) the gaps generated using the modified

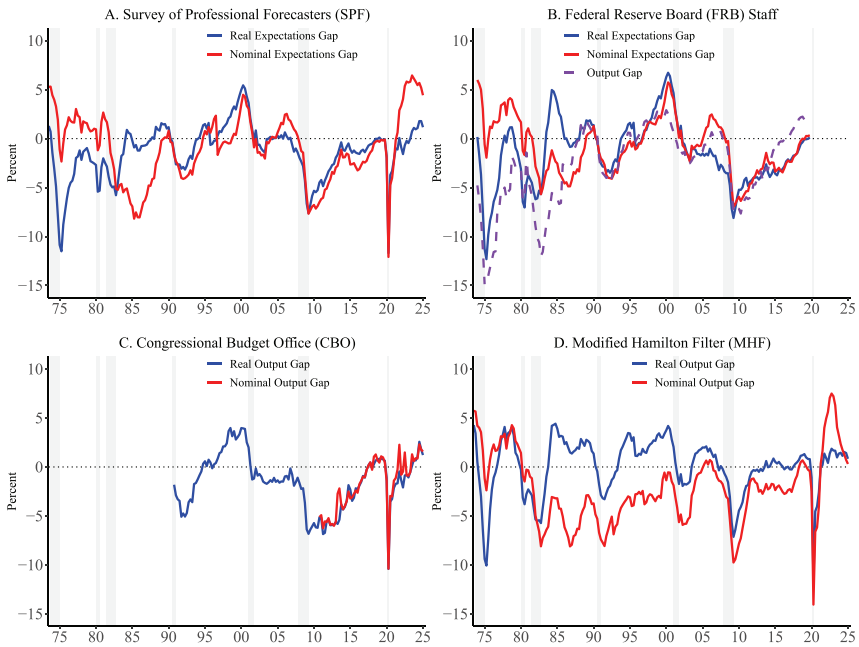
Hamilton filter of Quast and Wolters (2022), and (iv) the gaps generated using the modified BN decomposition of Kamber, Morley, and Wong (2018). All estimates are constructed based on real-time monthly vintages of nominal and real GDP from the Federal Reserve Bank of Philadelphia's Real-Time Dataset for Macroeconomists; see Croushore and Stark (2001). We treat the first month in which a particular quarter's advance estimate of GDP was available as the real-time estimate. This is motivated by the special emphasis given to the advance estimate in economic policy settings. However, the results are robust to using the second or third release of GDP.

Panel A of Figure 3 plots the real-time nominal and real expectations gaps derived from the SPF. Note that the real gap is constructed using the same procedure as the nominal gap above, relying solely on the real GDP forecasts. The real-time nominal gap is directly comparable to the latest estimate in Figure 2 and suggests that the real-time estimates are not substantially different from the latest vintage, as will be shown quantitatively below. For much of the sample, the nominal and real gaps follow each other fairly closely. However, there are several important deviations. First, in the late 1990s the real gap signals overheating more quickly than the nominal gap does. Second, in the mid-2000s, when home prices and construction costs were rising, the nominal gap shows stronger signs of overheating. Finally, the nominal gap indicates more slack in the mid-2010s, during the invisible recession.

Prior to the 1990s there were much larger and prolonged differences between the nominal and real gaps. For much of the stagflationary period of the 1970s, the nominal gap generally remains positive (except around recessions), whereas the real gap is persistently negative until 1985, after which the measures gradually converge. These differences illustrate that the nominal and real measures can convey very different signals about slack during high inflationary periods. This is also the case in 2022, when the real measure turned negative while the nominal measure neared record highs.

Panel B of Figure 3 plots the implied real-time real and nominal expectations gaps constructed from the FRB staff GDP growth forecasts (as computed using our methodology) and the real-time output gap through 2019 (the last publicly available vintage of the staff's

Figure 3. Real-Time Comparison of Nominal and Real Gaps



Note: Shaded bars denote NBER recession dates. Real-time vintages use advance estimates of GDP.

forecasts).¹² The FRB staff’s expectations gaps are similar to those derived from the SPF for most of the period, except for the mid-1980s, when there is a larger spike in the FRB real gap, and the late 1990s, when both the FRB’s nominal and real gaps indicate overheating. The real-time output gap starts out more closely aligned with the real expectations gap in the 1970s but does not exhibit the same spike as the real expectations gap does in the mid-1980s. By 1990, all three measures move together closely. Interestingly, the output gap indicates less overheating than both expectations gaps in the late 1990s but more overheating than the real expectations

¹²Federal Reserve staff forecasts and output gaps come from the Federal Reserve Bank of Philadelphia’s real-time data center. Real-time output gaps are extended using the historical data set from Edge and Rudd (2016).

gap, which indicates additional slack in the mid-2000s. By 2019, the real-time output gap indicates overheating, whereas both expectations gaps are neutral. While there are many periods in which the real-time expectations gaps and output gap provided similar signals, the expectations gaps, especially the nominal variant, often provide evidence of overheating (or the lack thereof) in real time just prior to economic downturns.

Panel C of Figure 3 plots the real-time estimate of the CBO's nominal and real output gaps, which the CBO constructs to be identical on purpose. Differences between the gaps arise in real time since the CBO only updates its estimates of potential on a 6- to 12-month basis.¹³ While the CBO's real-time output gap detected overheating during the dot-com boom, it failed to detect any overheating during the mid-2000s, which are commonly associated with a housing bubble. Since 2021 the CBO's nominal gap has become positive, while the real gap turned positive in 2023.

Panel D of Figure 3 plots real-time measures of the real and nominal output gaps using the modified Hamilton filter (MHF) of Quast and Wolters (2022). While the MHF appears to capture the business cycle using real GDP, the NGDP measure is less convincing. The implied real-time real output gap turns negative for all recessions and indicates overheating in the late 1970s, the late 1990s, and the mid-2000s. It also indicates overheating since 2021. On the other hand, the implied nominal output gap never signals slack in the 1970s and then spends most of the 1990s and 2000s in negative territory before surging following the pandemic recession. This suggests that the AR(4) model underlying the MHF is misspecified. Consequently, it does not correctly capture the trend or the dynamics in NGDP in the 1970s and 1980s, which illustrates the risk of using a single misspecified model to estimate the gaps.

Overall, the real-time nominal expectations gap captures the most important features of the business cycle. Our analysis also suggests that there are important differences between the nominal and real expectations gaps. The stagflationary episode in the 1970s, the housing bubble in the mid-2000s, the invisible recession

¹³Historical vintages of the CBO's estimates of real and nominal potential GDP are from the Federal Reserve Bank of St. Louis' FRED and ALFRED databases.

in 2015–16, and more recently the pandemic recovery period suggest that including nominal fluctuations provides a clearer picture of overheating and underperformance associated with episodes of broader price changes which might otherwise be muddled by slow real growth. Conversely, both the real expectations gap and conventional output gaps have missed or underestimated these economic developments.

4.1 Revision Properties

This subsection quantifies the reliability of the various real-time gap estimates.¹⁴ Reliability is measured based on the size of revisions by: the standard deviation (SD) and the root mean square (RMS). These measures are normalized using the standard deviation of the ex post estimate of the gap for each method to facilitate comparability. A “noise-to-signal ratio” (NSR) captures how large the variation of the revision is relative to the true variation of the data. Methods with lower NSRs indicate smaller revisions. We also calculate the frequency with which the real-time estimates have the same sign as the ex post estimate. Methods that have a large share of same-sign estimates are considered to be more reliable.

Two sample periods are considered in order to capture the properties of as many measures as possible. The first sample spans 1975–2018 and uses the November 2019 vintage as the ex post estimates. This allows comparison against the FRB staff estimates, which were available through 2019 at the time of this analysis. The second sample spans 1991–2023 and uses the December 2024 vintages as the ex post estimates for that sample. This enables a direct comparison against the CBO estimates, which are available from 1991.

Table 1 presents the results for the real gaps. The 1975–2018 sample indicates the following: First, the FRB staff’s output gap (O-Gap) has the highest NSRs. Edge and Rudd (2016) and Barbarino, Berge, and Stella (2024) show that this is driven by the volatility in the earlier part of the sample. Interestingly, the NSR for the FRB staff’s implied real expectations gap (E-Gap), which is based on the same information the FRB staff had when they

¹⁴Following the approaches in Orphanides and van Norden (2002), Edge and Rudd (2016), and Barbarino, Berge, and Stella (2024).

Table 1. Revision Properties for Real Gaps

	1975–2018 (Obs: 176)			1991–2023 (Obs: 132)		
	NSR		% sign agree	NSR		% sign agree
	SD	RMS		SD	RMS	
CBO O-Gap	.	.	.	0.71	0.71	73
FRB O-Gap	1.13	1.20	82	.	.	.
FRB E-Gap	0.33	0.41	86	.	.	.
SPF E-Gap	0.39	0.47	78	0.36	0.42	73
MHF O-Gap	0.28	0.29	94	0.32	0.32	94
MBN O-Gap	0.42	0.47	86	0.34	0.37	92
Note: Bolded values denote best performance by metric and sample. MHF is the modified Hamilton filter from Quast and Wolters (2022). MBN is the modified Beveridge-Nelson decomposition from Kamber, Morley, and Wong (2018). NSR: Noise-to-signal ratio. RMS: Root mean square deviation. SD: Standard deviation.						

generated their O-Gap, is just over one-third of their O-Gap. The SPF’s real E-Gap performs roughly in line with the FRB’s E-Gap. However, the measure with the lowest NSR this period is the MHF, which is one-fourth the FRB’s O-Gap, and also has the highest share of agreeing signs at 94 percent.

The results for the 1991–2023 sample are similar. The CBO’s estimates have the largest NSR and smallest share of sign agreements. The SPF’s real E-Gap NSRs are roughly half the size of the CBO’s. The SPF’s real E-Gap also performs better than in the first sample. The MHF has the lowest NSR and the highest share of estimates with the same sign, followed by the modified BN decomposition (MBN) from Kamber, Morley, and Wong (2018). These results are consistent with Barbarino, Berge, and Stella (2024)’s findings for a broader set of methods and across different subsamples and are also consistent with Kamber, Morley, and Wong (2018)’s findings that simpler models are subject to fewer revisions because they have fewer parameters to estimate and are less likely to overfit the data.

Table 2 presents the performance of the nominal gaps. In contrast to real gaps, the MHF is the worst performer across both subsamples. In the first subsample, the NSRs for the FRB staff’s nominal E-Gap are essentially unchanged from their real E-Gap performance and

Table 2. Revision Properties for Nominal Gaps

	1975–2018 (Obs: 176)			1991–2023 (Obs: 132)		
	NSR		% sign agree	NSR		% sign agree
	SD	RMS		SD	RMS	
BCEI E-Gap	.	.	.	0.28	0.31	93
FRB E-Gap	0.34	0.40	93	.	.	.
SPF E-Gap	0.31	0.35	91	0.26	0.29	92
MHF O-Gap	0.75	1.23	60	0.48	0.61	78
MBN O-Gap	0.31	0.50	84	0.29	0.35	94
Note: Bolded values denote best performance by metric and sample. MHF is the modified Hamilton filter from Quast and Wolters (2022). MBN is the modified Beveridge-Nelson decomposition from Kamber, Morley, and Wong (2018). NSR: Noise-to-signal ratio. RMS: Root mean square deviation. SD: Standard deviation.						

the SPF exhibits lower NSRs than for the real E-Gap. The SPF’s nominal E-Gap and the MBN have the lowest NSRs for the first sample period, while the FRB staff’s E-Gap has the largest share of sign agreement. In the second sample period, the SPF has the lowest NSRs while the MBN has the highest share of sign agreement. The SPF was also on par with the best performance of the real gaps in Table 1. The BCEI NGDP E-Gap also performs as well as these measures. These results, and those for the real gaps, are robust to the choice of data vintage; see the appendix.

Overall, the nominal gaps perform as well as or better than real gap measures. All of the NGDP E-Gaps have NSRs that are two to three times smaller than the FRB or CBO real O-Gaps. Furthermore, both the nominal and real measures of the FRB E-Gap dramatically outperform the FRB O-Gap despite being constructed from the same information set. While the nominal E-Gaps do not consistently outperform simple statistical procedures, they are more robust to those methods in cases where the underlying model may be misspecified.

4.2 Forecasting Core PCE Inflation

Another way to evaluate competing gap measures is to assess their effectiveness in forecasts of inflation using the Phillips curve. This

exercise utilizes Yellen (2015, 2017)'s model of core PCE inflation, which extends the typical autoregressive distributed lag model used in previous evaluation exercises—see Orphanides and van Norden (2005)—that has been widely used in macroeconomic policy settings. The unrestricted model is specified as

$$\pi_t^c = \beta_1 \pi_t^e + \beta_2 \pi_{t-1}^c + \beta_3 \pi_{t-2}^c + \beta_4 \text{GAP}_t + \beta_5 \text{RPIM}_t + \epsilon_t, \quad (6)$$

where π_t^c denotes the real-time vintage of the growth rate (expressed in annualized log differences) of core PCE prices;¹⁵ π_t^e is expected long-run inflation proxied by the latest available real-time forecast of long-run PCE inflation reported in the SPF, RPIM_t is a control for the effect of changes in the relative price of core imported goods and is defined as the annualized growth rate of the price index for core imported goods (excluding petroleum, natural gas, computers, and semiconductors), less the lagged four-quarter change in core PCE inflation, all multiplied by the share of nominal core imported goods in NGDP, and GAP_t captures the level of measured resource utilization.

In the model's original formulation, GAP_t is approximated by the unemployment rate gap (U-Gap) calculated as the unemployment rate less the CBO's measure of the long-run natural rate of unemployment. This exercise compares three alternative measures. The first of these is the FRB staff's real-time measure of the O-Gap and forecasts thereof for all vintages from 1996 until 2019. The sample is extended with real-time CBO measures of potential output and the BCEI's real-time monthly forecasts of real GDP. Second, real-time measures of the U-Gap are derived from the FRB staff's estimates of the natural rate and real-time forecasts of the unemployment rate. The sample is extended using real-time measures of the CBO's natural rate of unemployment and the BCEI's real-time monthly forecasts of the unemployment rate. Third, the real-time monthly vintages of the BCEI's NGDP E-Gap are considered along with forward-looking estimates of this gap. The

¹⁵Real-time vintages of core PCE are from the Federal Reserve Bank of Philadelphia's real-time database; see Croushore and Stark (2001).

forward-looking estimates are constructed by reformulating Equation (5) as a j -quarter-ahead forecast of the expected gap,

$$\mathbb{E}_t[EGAP_{t+j}] = \tilde{\mathbb{E}}_t[y_{i,t+j}] - \frac{1}{20} \sum_{h=0}^{19-j} \left(\tilde{\mathbb{E}}_{t-h}[y_{i,t+j}] + j\tilde{\mathbb{E}}_t[y_{i,t+j}] \right), \quad (7)$$

where at each quarter ahead additional weight is given to the latest estimate of expected output. This is consistent with the notion that estimates of the underlying trend follow a random walk such that the most recent forecast should receive the most weight; see Morley (2011). This formulation also implies that when evaluated against forecasts of NGDP, any notion of slack or overheating is expected to dissipate within 20 quarters or five years by construction.

The model in Equation (6) is reestimated in real time for each alternative GAP measure every month starting in February 1996 using an expanding estimation window starting in 1990:Q1. For each monthly vintage, a conditional forecast of quarterly core PCE inflation is generated for the current quarter up through four quarters ahead based on the estimated model. It is assumed that π_t^e and $RPIM_t$ follow a random walk such that forecasts are simply extrapolations of the last available real-time observation.¹⁶ Finally, the forecasts are evaluated against the third estimate of core PCE inflation, as is common in the literature; see, for example, Sinclair and Stekler (2013).

Table 3 presents the out-of-sample forecast accuracy results for the alternative GAP measures. This analysis treats the forecasts using the real-time FRB/CBO O-Gap as a relative baseline and uses it to compare the forecasts derived from the FRB/CBO U-Gap and the BCEI NGDP E-Gap respectively. The results are presented in terms of the relative root mean square forecast errors (RMSEs) across three different samples: the Great Moderation sample from 1996:Q1 through 2007:Q4, the Great Recession sample from 2008:Q1 through 2019:Q4, and the pandemic sample of 2020:Q1 through 2024:Q4. This allows for evaluation of forecast performance across several different economic cycles. A value less than one implies that

¹⁶Note that our measure of $RPIM_t$ is only quasi-real-time in that we use the latest vintage without accounting for data revisions.

**Table 3. Core PCE Inflation Forecasts:
RMSEs Relative to the O-Gap**

h	Great Moderation 1996:Q1–2007:Q4		Great Recession 2008:Q1–2019:Q4		Pandemic 2020:Q1–2024:Q4	
	U-Gap	E-Gap	U-Gap	E-Gap	U-Gap	E-Gap
0	0.99	0.96	0.98	1.00	0.99	0.96*
1	0.98	0.97	0.98	1.03	1.00	0.94*
2	0.98	0.96	1.01	1.03	1.01	0.95
3	0.96	0.95	0.98	1.00	1.01	0.96
4	0.94*	0.97	0.98	0.98	1.01	0.97
Note: Bold terms denote best performance by sample/horizon. Stars show statistically significant differences: ** has a p-value less than 1 percent and * has a p-value less than 5 percent. The Great Moderation sample has 131 observations, the Great Recession sample has 141, and the pandemic sample has 62.						

a measure performs better than the FRB/CBO O-Gap measure, while a value greater than one indicates that it performs worse. Asterisks denote whether the differences are statistically significant using standard Diebold and Mariano (1995) tests.

The results for the Great Moderation sample in Table 3 indicate that all three measures of slack perform similarly. Forecasts using the O-Gap have the largest RMSEs across all horizons, while the E-Gap generally has the smallest errors until the longest horizon. Only the U-Gap produces significantly better forecasts at the longest horizon. The results flip during the Great Recession sample, when the E-Gap generally has the largest RMSEs, although none of the differences are statistically significant.

The pandemic era results indicate that forecasts based on the E-Gap perform best. The E-Gap has a smaller RMSE than both the O-Gap and the U-Gap at every forecast horizon. Furthermore, the differences are significant at the 2 percent level for the nowcast and the one-quarter-ahead horizon and at the 9 percent level (not shown) for the two- to four-quarter-ahead horizons. This illustrates that real-time estimates of the forward-looking NGDP expectations gap perform as well as the FRB/CBO’s measures of the O-Gap and the U-Gap in normal times and that the NGDP expectations gap is able to outperform other measures during periods of high inflation.

4.3 Real-Time Taylor Rule Reliability

This last exercise analyzes the sensitivity of Taylor rules for setting interest rates using various real-time measures of slack. To do so, we extend the analysis done in Orphanides (2001). We use a simple Taylor rule following the original specification in Taylor (1993):

$$i_t = r_t^* + \pi_t + 0.5(\pi_t - \pi_t^*) + 0.5\text{GAP}_t, \quad (8)$$

where i_t is the predicted nominal federal funds rate, r_t^* is the natural real rate of interest, π_t is a measure of 12-month inflation, π_t^* is the inflation target, and GAP_t represents a measure of economic slack. When considering the O-Gap, then $\text{GAP}_t = y_t - y_t^*$ and $\pi_t^* = 2$. For the NGDP E-Gap, if we note that $\pi_t = p_t - p_{t-1}$ and $n_t = y_t + p_t$ is nominal income, then for a general inflation target $\pi_t^* = p_t^* - p_{t-1}$, (8) simplifies to

$$i_t = r_t^* + \pi_t + 0.5(n_t - n_t^*), \quad (9)$$

which is the standard NGDP targeting rule. The change in n_t^* is generally presumed to be the sum of the inflation target and the growth rate of potential real GDP. In the NGDP E-Gap framework, n_t^* is more flexible and develops according to forecasters' expectations as they evolve over time.

Following Knotek et al. (2016), real-time estimates of the natural real rate of interest are obtained from the FOMC's quarterly summary of economic projections (SEP) median projections of the longer-run federal funds rate minus long-run median expectations of core PCE inflation. For periods when these were not available, we use a fixed 2 percent real interest rate until 2009 as a real-time proxy and then use the long-run Blue Chip Financial Forecasters surveys' estimates of the real federal funds rate thereafter.

Table 4 presents the revision properties for the FRB/CBO O-Gap and the BCEI NGDP E-Gap. The NGDP expectations gap has the smallest real-time revisions and volatility across all metrics and all measures of inflation. This is consistent with the results in subsection 4.1 and reconfirms earlier findings by Orphanides (2001) that the volatility in monetary policy rules is driven in large part by instability in estimates of slack. Our results indicate that despite improvements in output gap measurement over time, the NGDP expectations gap continues to outperform these estimates.

Table 4. Revision Properties of Taylor Rules by Measure of Inflation

Slack	GDP Deflator			PCE			Core PCE		
	s.d.	NSR		s.d.	NSR		s.d.	NSR	
		SD	RMS		SD	RMS		SD	RMS
O-Gap	0.94	0.39	0.25	0.90	0.36	0.24	1.09	0.53	0.32
E-Gap	0.59	0.22	0.15	0.57	0.21	0.14	0.67	0.27	0.18
Note: Bold terms denote best performance by sample/horizon. The sample is 1996:Q1–2024:Q4.									

Table 5. Revision Properties of Forward-Looking Taylor Rules by Horizon

Slack	h=0			h=2			h=4		
	s.d.	NSR		s.d.	NSR		s.d.	NSR	
		SD	RMS		SD	RMS		SD	RMS
O-Gap	1.10	0.46	0.27	1.91	0.80	0.47	2.52	1.05	0.61
E-Gap	0.88	0.32	0.22	1.73	0.63	0.40	2.39	0.88	0.54
Note: Bold terms denote best performance by sample/horizon. The sample is 1996:Q1–2024:Q4.									

Next, the real-time performance of a forward-looking Taylor rule is considered. The analysis uses forecasts of the GDP deflator inflation measure, since these are available from both the FRB staff and BCEI forecasters. Table 5 presents the results for forecast horizons from the nowcast up through four quarters ahead. The results again show that the Taylor rule using the E-Gap has smaller real-time revisions than when using the O-Gap, even at longer horizons.

Overall, the NGDP E-Gap generates interest rate targets that are up to 40 percent less volatile than those suggested by standard O-Gap measures. There are two obvious reasons for these gains. First, the results in subsection 4.1 clearly show that the NGDP E-Gap is less sensitive to revisions in real time, in part because the

forecasts are not revised for a given survey publication date. Second, the NGDP E-Gap combines the inflation and real O-Gaps into a single NGDP gap measure that further reduces the volatility of estimates from a Taylor rule. These results are also consistent with the results for other studies using alternative monetary policy rules; see, for example, Beckworth and Hendrickson (2020) and Beckworth and Horan (2022).

5. Conclusion

This paper compares the real-time performance of NGDP gaps against other measures of economic slack. To do so, it uses the NGDP expectations gap, which is constructed as the difference between observed NGDP and what surveys of professional forecasters expected it to be, to compare the real-time performance of nominal and real gap measures from the FRB staff, the CBO, and several forecast-based measures. The paper evaluates the measures in the context of several policy-relevant applications, including real-time forecasts of core PCE inflation and interest rate targets.

The analysis shows that the NGDP expectations gap performs as well as or better than other measures. First, its historical revisions are two to three times smaller than the output gap measures produced by the FRB staff and the CBO. Moreover, they are comparable with or slightly better than other statistical output gap measures such as Kamber, Morley, and Wong (2018) and Quast and Wolters (2022). Second, forecasts of core PCE inflation following the COVID-19 pandemic that are based on the NGDP expectations gap are significantly more accurate than those based on the output gap or unemployment rate gap measures. Finally, Taylor rules for setting interest rates are up to 40 percent less volatile in real time when using the NGDP expectations gap.

The results of this paper indicate that the fears associated with real-time implementation of nominal targets are overblown. While all measures of economic slack are subject to mismeasurement and revisions, the NGDP expectations gap performs among the best across the available measures and provides a useful measure of economic slack in real-time settings. This is especially true in the high-inflation environment following the COVID-19 pandemic, when economic slack was of particular concern for policymakers. Thus, while the

Table A.2. Revision Properties for Nominal Gaps across Multiple Data Vintages: Advance, Second, and Third

	1975–2018 (Obs: 528)			1991–2023 (Obs: 396)		
	NSR		% sign agree	NSR		% sign agree
	SD	RMS		SD	RMS	
BCEI E-Gap	.	.	.	0.27	0.29	93
FRB E-Gap	0.33	0.39	93	.	.	.
SPF E-Gap	0.30	0.34	91	0.25	0.28	92
MHF O-Gap	0.75	1.22	61	0.48	0.60	78
MBN O-Gap	0.31	0.49	83	0.28	0.34	94
Note: Bolded values denote best performance by metric and sample. MHF is the modified Hamilton filter from Quast and Wolters (2022). MBN is the modified Beveridge-Nelson decomposition from Kamber, Morley, and Wong (2018). NSR: Noise-to-signal ratio. RMS: Root mean square deviation. SD: Standard deviation.						

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