THE TRANSMISSION OF MACROPRUDENTIAL POLICY IN THE TAILS: EVIDENCE FROM A NARRATIVE APPROACH

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Abstract

We estimate the causal effects of macroprudential policies on the entire distribution of GDP growth for advanced European economies using a narrative-identification strategy in a quantile-regression framework. While macroprudential policy has near-zero effects on the centre of the GDP-growth distribution, tighter policy brings benefits by reducing the variance of future growth, significantly boosting the left tail while simultaneously reducing the right. Assessing a range of channels through which these effects materialise, we find that macroprudential policy particularly operates through 'credit-at-risk': it reduces the right tail of future credit growth, dampening booms, in turn reducing the likelihood of extreme GDP-growth outturns.

Keywords: growth-at-risk, macroprudential policy, narrative identification, quantile local projections.

JEL classification: E32, E58, G28.

Resumen

Este trabajo estima los efectos causales de las políticas macroprudenciales sobre toda la distribución del crecimiento del PIB en las economías avanzadas de Europa, utilizando una estrategia de identificación narrativa en un marco de regresión cuantílica. Mientras que la política macroprudencial tiene efectos casi nulos en el centro de la distribución del crecimiento del PIB, un endurecimiento de la política aporta beneficios al reducir la varianza del crecimiento futuro, elevando significativamente la cola izquierda y reduciendo simultáneamente la cola derecha. Al evaluar los canales mediante los cuales se materializan estos efectos, encontramos que la política macroprudencial opera principalmente a través del *credit-at-risk:* reduce la cola derecha del crecimiento futuro del crédito, atenuando los auges y, a su vez, disminuyendo la probabilidad de resultados extremos en el crecimiento del PIB.

Palabras clave: riesgo de crecimiento, política macroprudencial, identificación narrativa, proyecciones locales cuantílicas.

Códigos JEL: E32, E58, G28.

1 Introduction

Macroprudential policies are now an increasingly important part of policymakers' toolkits. Targeted at maintaining financial stability, a key aim of macroprudential policy is to reduce 'tail risks'—i.e., minimise the potential economic costs of negative shocks by bolstering the resilience of the financial sector (Carney, 2020). However, building this resilience may not always be costless. While macroprudential policies can contain risks and contribute to macroeconomic stability, they may also have macroeconomic costs by constraining economic growth.

To gauge these potential costs and benefits, it is important to obtain accurate estimates of the causal effects of macroprudential policies on the *entire distribution* of potential macroeconomic outcomes. Such estimates are scarce in the extant literature, which has typically focused on intermediate outcomes (e.g., bank credit) or specific policies. While the development of quantile-regression techniques to estimate growth-at-risk—i.e., the size of potential '1-in-x bad outcomes'—offer policymakers a better understanding of the drivers of macroeconomic tail risks when monitoring financial stability (see, e.g., Adrian, Boyarchenko, and Giannone, 2019; Aikman, Bridges, Hacıoglu Hoke, O'Neill, and Raja, 2019; Adrian, Grinberg, Liang, Malik, and Yu, 2022; Lloyd, Manuel, and Panchev, 2023), identifying the causal effects of macroprudential policies on growth-at-risk presents a number of important empirical challenges. Crucially, as with other macroeconomic policies, macroprudential policy is not 'randomly assigned' and may be anticipated by economic agents. So, a simple comparison of future economic outcomes under different policies is unlikely to uncover reliable estimates of causal effects.

In this paper, our key contribution is to estimate the causal effects of macroprudential policies on the entire GDP-growth distribution by incorporating a novel narrative-identification strategy within a quantile-regression framework. Narrative methods have been used to uncover the effects of monetary policy (Romer and Romer, 1989, 2023) and fiscal policy (Romer and Romer, 2010; Cloyne, Martinez, Mumtaz, and Surico, 2023; Cloyne, Dimsdale, and Postel-Vinay, 2024), and have recently been employed in the macroprudential-policy literature (Richter, Schularick, and Shim, 2019; Rojas, Vegh, and Vuletin, 2022; Fernández-Gallardo, 2023). We build on this work by looking beyond the effects of specific prudential instruments and beyond just mean outcomes for GDP, considering the tails of the GDP-growth distribution. To do so, we exploit the Macroprudential Policy Evaluation Database (MaPPED), which covers a range of macroprudential policy actions across advanced European economies (Budnik and Kleibl, 2018).

This dataset includes a wealth of information on each policy action. Most importantly for our narrative identification, it records whether a policy action has countercyclical motivation or design—i.e., whether it is set *in response to* changes in the current or expected macroeconomic environment.

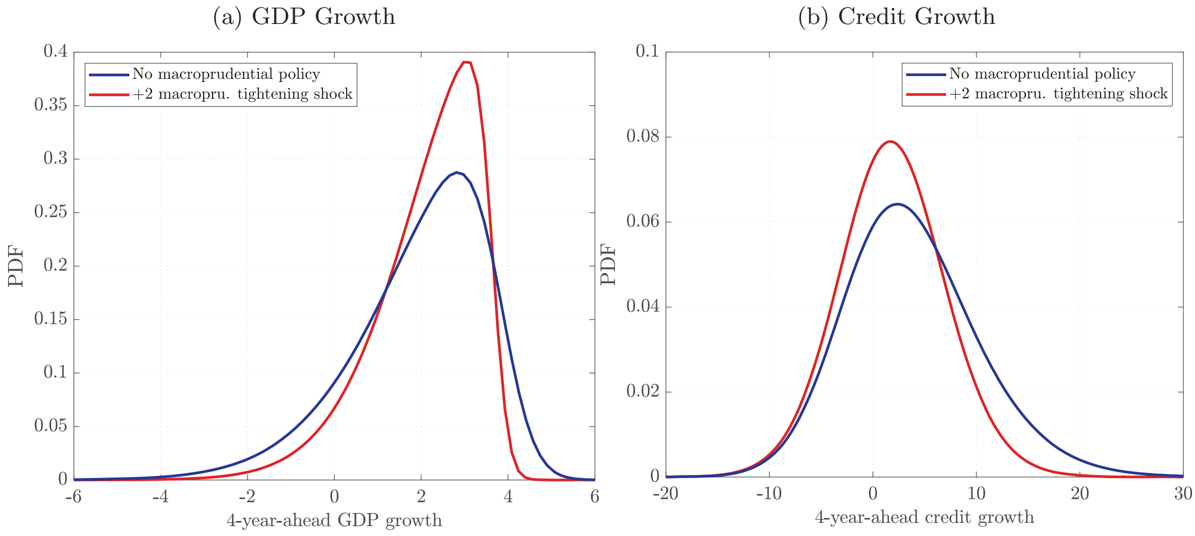
Other features of the dataset and our empirical strategy additionally contribute to the novelty of our approach. We supplement the narrative information provided within MaPPED with real-time policy documents containing detailed descriptions of the motivation for key policy actions. This approach provides a more granular picture and allows us to establish that the key macroprudential policy actions in our dataset were indeed exogenous, closely mirroring the approach of Romer and Romer (2010). Moreover, the detailed information on announcement and enforcement dates of policy actions recorded within MaPPED helps to account for policy-implementation lags, and strip away policy-anticipation effects.[1] Alongside this, we build on recent advances to the identification of dynamic causal effects within quantile-regression settings (Lloyd and Manuel, 2024) by controlling for factors that potentially feature in macroprudential policymakers' reaction function through a 'one-step' estimation approach. We show that this approach identifies causal effects under a standard 'selection-on-observable' assumption, in comparison to a 'two-step' approach in which the policy reaction function is first estimated and then its residuals are used in a second-stage quantile regression. By combining this approach with our narrative identification, we effectively pin down unanticipated and exogenous macroprudential policy 'shocks' to identify causal effects.

Applying these shocks, we document how macroprudential policy affects different parts of the conditional distribution of future GDP growth. We find that tighter macroprudential policy significantly and robustly boosts the left tail of GDP growth (i.e., reduces downside tail risk or 'GDP-at-Risk'), while reducing the right tail (i.e., reducing upside tail risk), with broadly zero effect on the centre of the distribution. The left-hand side of Figure 1 presents this visually, demonstrating how the predictive distribution of GDP growth shifts in response to a tightening in macroprudential policy (of +2 tightening activations),[2] when all other control variables are set to their cross-country and cross-time average for the 12 economies in our 1990Q1-2017Q4 sample. We specifically plot the 4-year-ahead predictive distribution—the horizon at which we find the largest peak causal effects in our analysis. As we show later, however, the predictive distributions

[1] Similar information has previously been employed in the fiscal-policy literature to account for potential confounding factors to narratively-identified shocks (Mertens and Ravn, 2012).

[2] The values of our macroprudential shock series range from -2 to 6. So a +2 tightening lies well within that range.

Figure 1: Illustration of main results: The effect of a macroprudential policy tightening shock on the distributions of 4-year-ahead GDP and credit growth



Notes: Blue lines show distributions of 4-year-ahead GDP (Panel (a)) and credit (Panel (b)) growth when all control variables are set to their cross-country and cross-time average values, and the macroprudential policy index is 0. Red lines show the same distribution when the macroprudential policy index is +2 (that index ranges from −2 to 6 in our sample), which corresponds to a macroprudential policy tightening shock, with all other variables kept at their average values. Distributions approximated by fitting skew-t distribution to quantile-regression estimates at $\tau = [0.1, 0.25, 0.5, 0.75, 0.9]$.

at other horizons would provide a similar qualitative picture. The figure illustrates our main empirical result: that macroprudential policy reduces the risk of large economic downturns in 'bad' states of the world, although restricts economic growth in 'good' states—in turn, reducing the variance of future GDP growth.

Armed with this result, we then consider the channels through which macroprudential policies affect the GDP-growth distribution. Amongst the channels we consider, it is the link from macroprudential policy to GDP-at-Risk via *credit quantities* that is most significant—in line with the extensive literature linking periods of high credit growth with financial-stability risks (e.g., Schularick and Taylor, 2012; Müller and Verner, 2024). Using our quantile-regression framework, we demonstrate that tighter macroprudential policy reduces credit growth, with this effect being particularly large at the right tail of the credit-growth distribution. This highlights that macroprudential policy can be particularly effective at mitigating 'excessive' credit booms, operating through a 'credit-at-risk' channel.[3] The right-hand side of Figure 1 presents this logic, illustrating how a tightening in macroprudential policy especially impacts the right tail of the credit-growth distribution over the same 4-year horizon shown for GDP.

[3]Throughout this paper, we use the term 'credit-at-risk' to refer specifically to the right tail of the credit growth distribution.

Interacting an indicator variable for periods of high credit growth with credit growth itself in our GDP-at-Risk regression, we then show that it is precisely these credit booms that are a significant driver of GDP-at-Risk. So, while an implication of previous growth-at-risk studies is that lower credit growth can be effective at reducing risks to financial stability (e.g., Aikman et al., 2019; Adrian et al., 2022; Lloyd et al., 2023), our findings go one step further. Tighter macroprudential policy can be effective at reducing the likelihood of extreme GDP-growth outturns *because* it reduces the right tail of credit growth. By dampening the impact of credit booms on GDP tail risks, macroprudential policy influences growth-at-risk through 'credit-at-risk'.

To further understand the links between macroprudential policy, and tail risks to credit and GDP, we also consider the effects of policy on the composition of credit. Preexisting work has shown that household credit booms and business credit booms are strongly associated with financial-stability risks, subsequent declines in GDP growth and deeper financial recessions (Mian, Sufi, and Verner, 2017; Ivashina, Kalemli-Özcan, Laeven, and Müller, 2024). Within our quantile-regression framework, we find that tighter macroprudential policy appears to be equally effective at preventing household *and* business credit booms. Finally, amongst the other channels we consider, we find limited evidence of significant transmission through house prices.

Overall, our results provide novel evidence about the causal effects of macroprudential policy on the distribution of future macroeconomic outcomes. In particular, our results suggest that, by defusing upside credit-at-risk (i.e., credit booms), tighter macroprudential policy can be effective in mitigating extreme—downside and upside—macroeconomic risks.

Related Literature. Our paper contributes to three main strands of literature. First, our work builds on studies applying quantile-regression techniques to assess the drivers of macroeconomic tail risks (e.g., Adrian et al., 2019, 2022; Lloyd et al., 2023). While some papers have sought to assess the association between macroprudential policy and tail risks to GDP growth (Aikman et al., 2019; Galán, 2020; Franta and Gambacorta, 2020), we build on recent developments in macroprudential policy measurement and quantile identification to plausibly identify causal impacts.[4] The dataset we use is crucial in this regard, allowing us to overcome

[4] Using a heterogeneous panel of advanced and emerging economies, Franta and Gambacorta (2020) investigate the impact of two macroprudential measures: loan-to-value (LTV) and loan-loss provisioning, on the tails of the GDP-growth distribution. To estimate the impact of loan-loss provisioning, they do not identify plausible exogenous variation in the policy, which means their estimates cannot be interpreted causally under general assumptions. However, when assessing the impact of changes in LTV, they utilise the dataset of Richter et al. (2019), which contains LTV changes that can be considered orthogonal to the real business cycle (e.g., real GDP), but not to the financial cycle (e.g., total credit). As a result, their findings for LTV changes could be interpreted causally under some assumptions: for example, no policy anticipation, and no systematic correlation between real and financial cycles.

concerns regarding both endogeneity and anticipation effects. Our baseline identification strategy relies on using narrative information provided within the MaPPED database to separate counter-cyclically motivated policies from those that are plausibly exogenous to the macrofinancial cycle. We build on this along several dimensions. First, we provide detailed information from real-time documents from the Bank of International Settlements (BIS) to assess whether the largest macroprudential shocks in our cycle were indeed exogenous (given the motivation described in these policy documents), closely following the approach of Romer and Romer (2010). We show that our results are robust to including just this subset of large, exogenous shocks. Further, we complement our narrative approach with additional robustness exercises to demonstrate that our results are broadly unchanged when additionally controlling for macroeconomic forecasts made at the time of macroprudential policy decisions (similar in spirit to the identification strategy of Romer and Romer (2004) to estimate monetary-policy effects). To do this, we build on recent work on the identification of dynamic causal effects with confounding factors in a quantile-regression setting in Lloyd and Manuel (2024). This work demonstrates that previous attempts to identify the effects of macroprudential policies within a quantile regression by first estimating a series of 'policy shocks' from a first-stage regression (Brandão-Marques, Gelos, Narita, and Nier, 2021; Gelos, Gornicka, Koepke, Sahay, and Sgherri, 2022) suffer from a form of omitted-variable bias, which we avoid by employing an alternate 'one-step' quantile regression estimator with variables that potentially feature in the policy reaction function as controls.

Second, we contribute to a more general literature identifying the effects of macroprudential policy. While previous literature (Richter et al., 2019; Rojas et al., 2022; Fernández-Gallardo, 2023) has employed narrative methods to separately estimate potential costs (e.g., reductions in economic growth) and benefits (e.g., reduced probability of financial crises) of macroprudential policies, by using quantile-regression techniques we are able to *simultaneously* assess costs and benefits by examining the effects of macroprudential policy across the entire distribution of GDP-outcomes. This is significant given the growing attention paid to the growth-at-risk framework in the assessment of macro-prudential policies within international policy organisations, including both the IMF and the ESRB. For example, Cecchetti and Suarez (2021) propose that, in order to operationalise an assessment of macroprudential policies: (*emphasis* added): "policymakers require *accurate estimates of the elasticity of the distribution of the objective with respect to their macroprudential instruments* [... where] it is important to keep in mind that what matters for policy design is not the historical correlation between tools and the relevant economic outcomes, but the *causal impact* of the tools on such outcomes." These estimates are exactly what our empirical framework delivers. Relatedly, our quantile-regression framework allows us to capture the effectiveness of macroprudential policy in mitigating financial stability concerns without relying on binary financial crisis indicators. This mitigates well-known concerns related to the use of binary indicators that are necessarily subjective and capture only specific dimensions of financial distress (see e.g., Sufi and Taylor, 2021).

Third, we contribute to a range of work assessing the transmission channels of macroprudential policy to the macroeconomy through the financial system. A key finding in the previous literature is that tighter macroprudential policy can be effective at reducing rapid credit growth (Claessens, Ghosh, and Mihet, 2013; Cerutti, Claessens, and Laeven, 2017a; Forbes, 2021; Acharya, Bergant, Crosignani, Eisert, and Mccann, 2022), and in turn that reduced credit growth is associated with lower financial-crises probabilities (Belkhir, Naceur, Candelon, and Wijnandts, 2022; Fernández-Gallardo, 2023). We show that tighter macroprudential policy can be effective at reducing tail risks to GDP growth precisely because it reduces the probability of credit booms (i.e. reduces the right tail of the credit-growth distribution).

Outline. The remainder of this paper is structured as follows. Section 2 describes our empirical specification, data and narrative-identification strategy. Section 3 presents our baseline results for the effects of macroprudential policy on the distribution of future GDP growth. Section 4 investigates the role of different transmission channels. Section 5 concludes.

2 Empirical Specification, Data and Identification

In this section, we present our overarching empirical framework. We describe our narrative measure of macroprudential policy and explain how we tackle the challenge of identifying macroprudential policy shocks—which form a key part of our contribution to the growth-at-risk literature.

2.1 Empirical Specification

As in previous growth-at-risk studies (e.g., Adrian et al., 2019), we employ a quantile-regression framework to assess how changes in a set of conditioning variables—in our case, macroprudential policy in particular—are associated with the distribution of our dependent variable—future GDP growth, and (later on) credit growth and asset prices. To leverage as much data as possible for the precision of our estimates, we present our approach within a panel setting with time denoted by $t = 1, ..., T$ and the countries for whom we estimate the conditional distribution of GDP labelled $i = 1, ..., N$.

We specify the following local-projection model (Jordà, 2005) for the conditional quantile function Q of h-period-ahead annual average GDP growth, which we denote by: $\Delta^h y_{i,t+h} \equiv (y_{i,t+h} - y_{i,t})/(h/4)$ for $h = 1, ..., H$:

$$Q_{\Delta^h y_{i,t+h}}(\tau|\Delta MaPP_{i,t}, \mathbf{x}_{i,t}) = \boldsymbol{\alpha}_i^h(\tau) + \Delta MaPP_{i,t}\beta^h(\tau) + \mathbf{x}'_{i,t}\boldsymbol{\theta}^h(\tau), \quad \tau \in (0,1) \tag{1}$$

where Q computes quantiles τ of the distribution of $\Delta^h y_{i,t+h}$ given covariates. $\Delta MaPP_{i,t}$ denotes the scalar narrative-based macroprudential policy shock for country i at time t, with associated parameter $\beta^h(\tau)$. The $K \times 1$ vector of control covariates $\mathbf{x}_{i,t}$ has associated parameter vector $\boldsymbol{\theta}^h(\tau)$.

In equation (1), $\boldsymbol{\alpha}_i^h(\tau)$ represents country- and quantile-specific fixed effects, which control for time-invariant unobserved heterogeneity. For our baseline panel specification, we estimate these fixed effects following the approach of Kato, Galvao Jr, and Montes-Rojas (2012), who show that for panel quantile regressions like ours, with many time periods compared to the number of cross-sectional units (i.e., $T \gg N$), this fixed-effects estimator is consistent and asymptotically normal.[5]

In our baseline specification, we include the following controls in $\mathbf{x}_{i,t}$ to account for time- and country-varying observed macro-financial conditions: the annual growth of real credit; the annual growth of real house prices; the annual growth of general CPI prices; contemporaneous and lagged values of the dependent variable; and the US VIX. These span both domestic and foreign drivers of the GDP-growth distribution, over and above macroprudential policy. The US VIX, in particular, helps to account for global factors affecting the distribution of GDP growth (Lloyd et al., 2023). We include both the contemporaneous and first lags of each of our controls in our baseline specification. In robustness analyses, we carry out extensive tests on the sensitivity of our results to alternative controls.

Our baseline sample runs from 1990Q1 to 2017Q4, at quarterly frequency for 12 advanced European economies. The selection of this sample is determined by the availability of narrative-based macroprudential policy series $\Delta MaPP_{i,t}$ that we explain subsequently in Section 2.2. For inference, as in Aikman et al. (2019) and Lloyd et al. (2023), we follow the block-bootstrap procedure of Kapetanios (2008), resampling the data across all countries simultaneously over blocks of different time-series dimensions to generate coefficient standard errors for respective

[5] In robustness analysis, we also present results using country- and quantile-specific fixed effects estimated following the approach of Machado and Santos Silva (2019).

quantiles. We resample time-series observations using 8 blocks, replicating the bootstrap 1000 times.

With this specification, and armed with the discussion of identification in Section 2.3, our coefficient of interest $\beta^h(\tau)$ can be interpreted as the *causal* response of the τ-th conditional quantile of GDP growth at horizon h to a tightening in macroprudential policy that is activated at time t. Throughout, we focus the majority of our presentation on the 10th, 50th and 90th percentiles of GDP growth.[6] We choose those quantiles to estimate the impact of a macroprudential policy shock not only on the median, but also on the tails of the GDP-growth distribution, which constitute measures of the macroeconomic downside and upside risk, respectively. Therefore, those percentiles can be interpreted as representing how 'bad' ('good') growth may be under adverse (favourable) circumstances.

2.2 Macroprudential Policy Index

We construct our macroprudential policy index $\Delta MaPP_{i,t}$ by using the Macroprudential Policies Evaluation Database (MaPPED). This database contains around 480 policy actions taken between 1990Q1 and 2017Q4 for the following 12 advanced economies: Belgium, Denmark, Germany, Ireland, Spain, France, Italy, Netherlands, Finland, Sweden, Portugal and the UK.[7] The dataset covers 11 categories of policy instruments, spanning: capital requirements, capital buffers, risk weights, leverage ratios, provisioning systems, lending standards restrictions, limits on credit growth, taxes on financial activities, limits on large exposures, liquidity requirements and limits on currency and maturity mismatch, and other measures. Our sample end date of 2017Q4 marks the last publicly-available update of the dataset.

Relative to other macroprudential policy databases such as the IMF's Integrated Macroprudential Policy (iMaPP) Database (Alam, Alter, Eiseman, Gelos, Kang, Narita, Nier, and Wang, 2019) and the International Banking Research Network's Prudential Policy Database (Cerutti, Correa, Fiorentino, and Segalla, 2017b), MaPPED has several advantages for our purposes. Importantly, the survey designed for MaPPED ensures that policy tools and actions are

[6] We additionally use estimates for the 25th and 75th percentiles to fit the skew-t distributions to quantile-regression outputs in Figure 1.

[7] For the post-1995 period, MaPPED includes all policy actions—both those in force prior to 1995 and new policy activations post-1995. However, pre-1995, the dataset only includes policies that still remained in force in 1995. Therefore, the dataset does not include policies activated prior to 1995 that were deactivated between 1990 and 1994. Nevertheless, the dataset is still likely to include the vast majority, if not all, of the policies implemented between 1990 and 1994 for two reasons. First, macroprudential policy deactivations represent a very small percentage of total policy actions (only 2%). Second, within MaPPED, policies that are eventually deactivated have an average duration of around 14 years. Therefore, it is unlikely that during the first years of the sample, 1990-1994, policies were enforced and deactivated prior to 1995.

reported in the same manner across countries. Therefore, MaPPED allows for comparability of policy actions across countries, avoiding potential biases from unstandardised open-text questionnaires and lending itself most naturally to our panel specification. Furthermore, MaPPED includes a wealth of information on each policy action. It tracks the life cycle of each policy instrument, including when it was activated, recalibrated and (if relevant) deactivated. So it allows us to account for the potentially differential effects of different types of policy changes. It also distinguishes between announcement and enforcement dates of policies, allowing us to control for policy anticipation effects. Most importantly though, in tracking the nature (loosening, tightening, or ambiguous) of each policy action, it also records narrative information about whether a policy action has countercyclical motivation and/or design. This feature is key to our identification strategy, as we explain in detail in Section 2.3.[8]

In our baseline specification, we construct an overall macroprudential policy shock index $\Delta MaPP_{i,t}$ for each country in the sample by combining all non-systematic policy actions—an approach also followed by Fernández-Gallardo and Payá (2023).[9] To do so, we use the announcement date of the policy to assign a value to each policy action, giving a positive value to tightening actions, a negative value to loosening actions, and a value of zero to policy actions with ambiguous impacts or no announced policy action in a given period. We focus on announcement dates, since it is common for there to be lags in the implementation of macroprudential policy following announcements that financial entities might respond to at the time of initial communication. These lags can arise because of legal requirements around policies (e.g., increases in bank capital buffers should be announced one year prior coming into force), but also because, in practice, policymakers may prefer to announce and communicate policy changes in advance to allow financial entities time to prepare. As an example, MaPPED records the fact that the January 2014 changes to the risk weights assigned to loans backed by residential and commercial property in the UK were first announced and communicated by the Bank of England's Financial Policy Committee (FPC) over six months before, in June 2013.

When constructing the macroprudential policy index, we also account for the fact that different types of policy actions—e.g., activations/deactivations, changes in scope of existing policies, renewals of existing policies—may have heterogeneous importance. In our baseline, we assign different weights to different policy actions based on importance, following the weighting

[8]We refer the reader to Budnik and Kleibl (2018) for detailed information on the advantages of MaPPED over other existing macroprudential policy databases.

[9]In robustness analysis, we analyse potential heterogeneity across different 'types' of macroprudential policies (e.g., borrower- vs. lender-based measures).

scheme proposed by Meuleman and Vander Vennet (2020). Under this scheme, activations and deactivations are given the highest weights. Second-tier actions, including changes in the existing level or scope of the policy are given a lower weight. Finally, an announcement that reaffirms or maintains existing policy levels is given the lowest weight. Appendix A details the weights assigned to the different policy actions. However, this weighting does not materially influence our main qualitative results; these are robust to alternative weighting schemes.

2.3 Identification of Macroprudential Policy Shocks

In order to apply the macroprudential index to study the impact of policy changes on the GDP-growth distribution, we ensure that our measure $\Delta MaPP_{i,t}$ pins down only the 'non-systematic' component of macroprudential policy—a step that is crucial for identifying causal effects. We refer to the non-systematic component of the policy actions—actions that do not systematically respond to short- to medium-term economic fluctuations—as macroprudential policy 'shocks'.

Overall, we face two empirical challenges to identify macroprudential policy shocks. First, some macroprudential policies are endogenous, as they are activated or adjusted in response to current or future economic conditions. Those policies are likely contaminated by reverse causality and therefore are invalid to recover causal effects. Second, lags between the announcement and activation of macroprudential policies—either due to legally-binding implementation lags or a preference amongst policymakers to announce a policy in advance to allow firms time to prepare—can pose an empirical challenge for disentangling the relationship between macroprudential policy and the GDP-growth distribution. In either case, policy actions subject to a delay between announcements and the implementation of the legislation may be partially anticipated, implying that economic agents can endogenously react to such prudential policy news. Policies with and without implementation lags can therefore have very different effects on macroeconomic variables, as shown by Mertens and Ravn (2012) in the fiscal context.[10]

Building the Narrative Series. In our baseline specification, we address the first threat to identification, endogeneity, using the narrative information provided in MaPPED. Our approach builds on that proposed by Fernández-Gallardo (2023), and we apply it within our quantile-regression framework. To identify macroprudential policy actions that are plausibly

[10] Note that our main identifying assumption—which permits interpreting the coefficients as capturing the causal effects of macroprudential policy—is simply that the policy changes we identify are exogenous to other determinants of the GDP-growth distribution (see Appendix B). This causal interpretation is valid even if the policy changes were partially anticipated, although the role of anticipation is relevant to the extent that anticipated and unanticipated policy changes may have different effects. For related discussion in the context of monetary-policy identification, see Coglianese, Olsson, and Patterson (2023).

tions are primarily aimed at short- to medium-term stabilisation rather than implemented to address structural vulnerabilities in the financial system. In the MaPPED questionnaire, an instrument is defined as having a countercyclical design if its calibration is regularly revised along with judgements about the intensity of cyclical systemic risk (Budnik and Kleibl, 2018). For the 12 advanced economies selected in this paper, countercyclical policies in MaPPED represent approximately 10% of the total set of policies covered by the database. They include, amongst other policies, changes in countercyclical capital buffer (CCyB) rates, which are altered in response to cyclical systemic risk.[11]

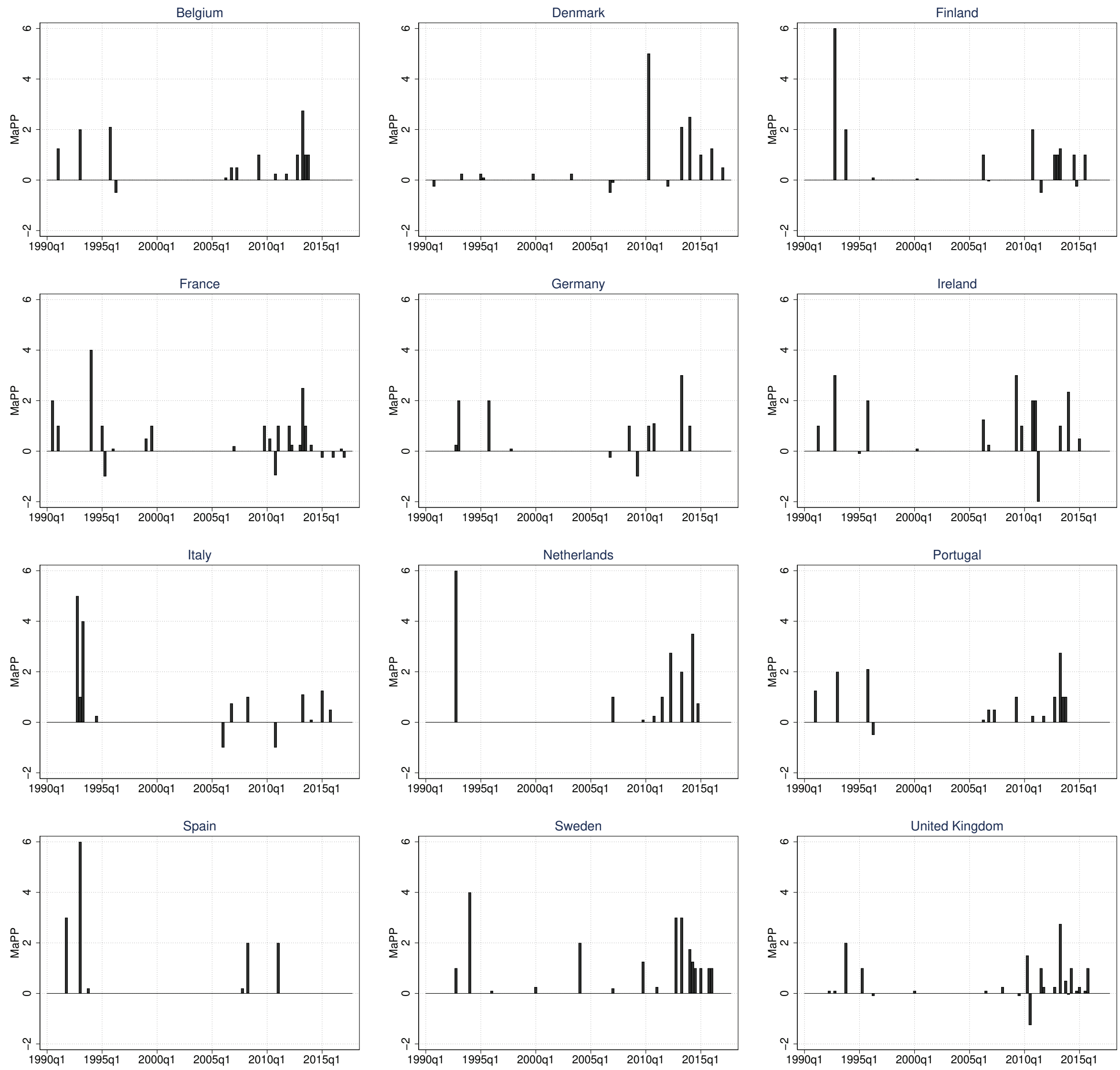
We therefore construct our $\Delta MaPP_{i,t}$ indicator for each country in the sample following the steps explained in Section 2.2 by excluding policy actions with countercyclical design and motivation. By doing this, our index therefore focuses on policy actions that are legitimate observations for identifying causal effects because such policies are less likely to be systematically correlated with other underlying factors affecting the GDP-growth distribution.

The resulting index can be interpreted as a composite measure of overall macroprudential policy 'shocks' in each of the advanced economies. Despite excluding countercyclical policies, it still retains information on a range of other macroprudential tools (e.g., systemic risk buffers, risk weights) aimed at addressing systemic risks of a long-term, non-cyclical nature. Figure 2 plots the changes in our narrative macroprudential policy index over time for each country in the sample. The index displays significant heterogeneity across countries, reflecting the fact that different macroprudential policy authorities in different countries have chosen to tighten or loosen policies to different extents over time. Focusing on the UK as an example (bottom-right panel), we see moves associated with a range of policy actions.[12] For example, the UK index takes numerous positive values post-2010, reflecting the range of macroprudential tools implemented following the 2007-9 Global Financial Crisis (GFC). These capture a combination of policy tightenings and loosenings, including the June 2013 announcement of new CET1 and Tier-1 capital ratios that came into force in January 2014, the loosening of risk weights on loans backed by commercial property in 2013Q2, as well as the December 2015 announcement of a capital buffer for Global Systematically Important Institutions that came into force in January 2017. Importantly, however, the index excludes changes in the UK CCyB rate owing to this policy's explicit countercylical design. So, for example, the values in Figure 2 do not reflect

[11]In particular, among the eleven categories in which MaPPED classifies macroprudential policy instruments, five of them include *at least one* (but not necessarily all) policy action(s) that has countercyclical motivation and/or design. These five policy categories are capital buffers, lending standards restrictions, provisioning systems, risk weights, and a final category labeled 'other measures'.

[12]Further narrative information on other countries in our sample can be found here.

Figure 2: Changes in the Narrative-Based Macroprudential Policy Index over Time



Notes: Plot of narrative-based $\Delta MaPP_{i,t}$ over time for each advanced-economy in our sample. Period is 1990Q1-2017Q4.

either the announced increase in the CCyB rate from 0 to 0.5% March 2016 following a cyclical build-up of domestic credit, or the announced reduction in that same rate, back to 0%, in July of the same year in response to the potential macroeconomic consequences of the UK's vote to leave the European Union.

To support our identification strategy, we provide a range of evidence that the macropruduential policy index we construct indeed reflects exogenous changes in policy. We do so both through statistical tests of predictabiltiy and through detailed narrative information on the motivation for key macroprudential policy changes.

Predictability of Macroprudential-Policy Changes. First, we carry out a predictability test to assess whether our narrative macroprudential shock series can be regarded as plausibly exogenous to short-term economic and financial conditions. In particular, we test whether our narrative measure of macroprudential policy can be predicted by several real, financial, and monetary variables, along with projected macroeconomic conditions. We use these variables as proxies for short-term macroeconomic conditions around policy announcements. Some of these factors have been shown to be major predictors of financial crises, such as private credit growth or house-price growth (Schularick and Taylor, 2012; Jordà et al., 2013; Greenwood et al., 2022).

Table 1 shows the main findings from this analysis. It shows that our narratively-identified policy index is unpredictable by a set of variables capturing current and expected economic, financial, and monetary conditions around the policy implementation. So, this supports our claim that our narrative approach is indeed successful at identifying changes in macroprudential policy that were independent of prevailing economic conditions.

Table 1: Coefficient estimates: predictability of narrative macroprudential policy changes

	$\Delta MaPP_{i,t}^{Narrative}$
Lag Annual Growth Real GDP	0.332
	(0.275)
Lag Annual Inflation	0.481
	(0.509)
Lag Annual Growth Real House Prices	0.793
	(0.507)
Lag Annual Growth Real Credit	-0.016
	(0.125)
Lag Short-term interest rate	0.983
	(1.556)
Lag Change GDP Expectation	-0.075
	(0.169)
Lag MaPP	0.234
	(1.865)
F-test, all coefficients equal to zero, *p-value*	0.292
Observations	1314

Notes: This table presents coefficients from the predictability tests of narrative macroprudential policy changes. The narative index includes all macroprudential policy shocks. Coefficient estimates for the fixed effects and controls are not reported. Sample period is 1990Q1-2017Q4, for 12 advanced European economies. Driscoll and Kraay (1998) standard errors shown in parenthesis with: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Case Study of Biggest Shocks. To further support our identification strategy, we present a few case studies based on the largest shocks in our sample, providing detailed narrative evidence that these reflected exogenous changes in policy. In particular, for those episodes in which the policy shocks are particularly large in magnitude, we consult comprehensive narrative information on the motivation behind them, mirroring the approach that Romer and Romer apply to monetary policy (1989, 2023) and fiscal policy (2010). We show that these policies were motivated either by past decisions, external concerns, or by long-run economic goals, in line with the criteria for exogeneity employed by Romer and Romer (2010). We provide a summary of this analysis in Table 2. Further, in Section 3, we show that the effects of macroprudential policy on the GDP-growth distribution that we estimate hold when restricting our shock series to only these largest realisations.

Our method proceeds as follows. First, to select the largest shocks in our sample across all countries, we compute an aggregate measure of our country-based macroprudential-policy index.[13] Then, we standardise the aggregate index and select the shocks above +2 standard deviations (sd). Based on that criterion, we find three shocks corresponding to the largest

Table 2: Example of Narrative Evidence for Biggest Shocks in Sample

Date	1992Q4/1993Q1
(i) Action motivated by past and external concerns:	
"*In the early 1980s, the onset of the Latin American debt crisis heightened the Committee's concerns that the capital ratios of the main international banks were deteriorating at a time of growing international risks. Backed by the G10 Governors, Committee members resolved to halt the erosion of capital standards in their banking systems and to work towards greater convergence in the measurement of capital adequacy.*"	
(ii) Action with long-run goals:	
"*There was strong recognition within the Committee of the overriding need for a multinational accord to strengthen the stability of the international banking system and to remove a source of competitive inequality arising from differences in national capital requirements.*"	
Date	2013Q2
(i) Action motivated by past decisions or concerns:	
"*Even before Lehman Brothers collapsed in September 2008, the need for a fundamental strengthening of the Basel II framework had become apparent. The banking sector entered the financial crisis with too much leverage and inadequate liquidity buffers. These weaknesses were accompanied by poor governance and risk management, as well as inappropriate incentive structures. The dangerous combination of these factors was demonstrated by the mispricing of credit and liquidity risks, and excess credit growth.*"	
(ii) Action with long-run goals:	
"*The net benefits are measured in terms of the long-run change in the yearly level of output from its pre-reform path, with its trend growth rate unchanged. The origin corresponds to the historical average level of the capital ratio and frequency of banking crises – a proxy for the pre-reform steady state. The range of results shown reflects various estimates of the costs of banking crises, depending on whether costs are estimated as, permanent but moderate – which also corresponds to the median estimate across all comparable studies of such costs (red line) – or only temporary (green line). At the same time, taking a conservative approach, the results assume that institutions pass the added costs arising from strengthened regulations on to borrowers in their entirety while maintaining pre-reform levels for the return on equity, interest costs of liabilities and operating expenses. Thus, the costs of meeting the standards may be close to an upper bound.*" "*The report focuses exclusively on the long run, or endpoint of the reforms. It assesses the shift from one steady state to another (with and without the reforms). As such, it does not assess the costs associated with the transition phase itself.*"	

Notes: This table provides key quotations from official documents highlighting that the motivation for the largest policy shocks in our dataset were indeed driven by either past / external concerns, or long-run goals.

[13] The aggregate index have been computed as the sum of the individual $\Delta MaPP_{i,t}$ indices in each country in our sample.

shocks in our sample: 1992Q4, 1993Q1, and 2013Q2, corresponding to 5.39, 3.11, and 6.16 sds, respectively.

We then carry out a detailed case study of those shocks to provide a more comprehensive description of them and to assess their exogeneity. To do so, we collect and review additional narrative information from historical real-time documents from the Bank for International Settlements (BIS) around the periods when the shocks occurred. We use this information to identify and understand the motivation behind the implementation of these policies.

Within our framework, we need to separate macroprudential policy changes into two broad categories: those taken in response to other factors likely to affect output growth in the near future (i.e. endogenous changes), and those taken for other reasons, (i.e. exogenous changes). To make such a classification, we closely follow the exogeneity criteria of Romer and Romer (2010). Policies are considered exogenous if either (i) they are motivated by past decisions or concerns or (ii) they are implemented pursuing long-run goals (e.g., the goal is to raise normal growth, not to offset shocks that reduce growth relative to normal).

Armed with this, we use the information extracted from the BIS documents to confirm that the largest shocks in our sample are exogenous, as their motivation falls into either (or both) of the categories above.[14] This analysis is summarized in Table 2. BIS documents reveal that the second and third-largest shocks in our sample—those in 1992Q4 and 1993Q1—were motivated by the first set of international banking regulations developed by the Basel Committee on Banking Supervision (Basel I). Based on quotes from real-time documents, the main historical event motivating these policy shocks was the Latin American debt crisis of the early 1980s—nearly a decade before the policy change occurred. This crisis heightened concerns that the capital ratios of the main international banks were deteriorating at a time of growing international risk. Driven by such episodes, Committee members worked towards greater convergence in the measurement of capital adequacy. As a consequence, for these two shocks we have narrative evidence indicating that these policies were motivated by past and external concerns (i.e., constituting genuinely exogenous changes in policy). Similarly, these policies meet the second criterion, insofar as they were motivated by long-run goals: reducing competitive inequality and improving overall banking-system stability.

Moreover, BIS documents reveal that the key motivation behind the biggest shock in our sample—occurring in 2013Q2—was a desire to increase economic growth in the long run and a

[14]Note that one is enough to be considered as exogenous according to Romer and Romer (2010).

strong recognition, even before the Great Financial Crisis within the Committee of the overriding need of a fundamental strengthening of the Basel II framework.[15] Therefore, we have evidence that the largest shock in our sample was motivated by past concerns and pursued long-run economic goals, and so appears genuinely exogenous.[16] In addition, we have further evidence to reinforce our claim of exogeneity regarding the policies implemented in 2013Q2. In particular, our narrative evidence aligns with Bluwstein and Patozi (2022), who demonstrated that the implementation of such policies in the UK in 2013Q2 came as a surprise to financial markets, and led to moves in bank-equity and credit default swaps theoretically consistent with a macro-prudential tightening. This point implies that the largest shock in our sample is not only exogenous according to Romer and Romer (2010)'s exogeneity criteria but also according to alternative identification strategies, such as high-frequency identification of policy shocks.

Addressing Additional Threats to Identification. An alternate approach, frequently employed in the literature, is to identify 'as good as random' moves in macroprudential policies by controlling for variables that plausibly feature in a macroprudential 'reaction function'. The underlying identifying assumption in this approach is 'selection on observables': conditioning on a set of observable factors, changes in macroprudential policy are exogenous with respect to all other drivers of future outturns in the outcome variable of interest. Although not typically explained in this way in the macroprudential-policy literature, this is the assumption underpinning identification via propensity scores (Forbes, Fratzscher, and Straub, 2015; Richter, Schularick, and Shim, 2019), as well as via the estimation of policy shocks as the residuals from a reaction function (Ahnert, Forbes, Friedrich, and Reinhardt, 2021; Chari, Dilts-Stedman, and Forbes, 2022; Gelos, Gornicka, Koepke, Sahay, and Sgherri, 2022).

There are well-known challenges with this approach, most notably that it may be infeasible in practice to correctly identify and then control for all the variables that potentially feature in the policy reaction function, especially in the face of limited dimensionality in finite samples. For example, controlling for the wide range of economic and financial indicators that feed into

[15] Based on quotes from real-time documents and reports, it is clear that a key consideration was the long-term economic consequences of such policies. In particular, The Basel Committee on Banking Supervision produced a report assessing the Long-term Economic Impact (LEI) of the Basel Committee's December 2009 proposed reforms to the capital and liquidity frameworks. Its purpose was to assess the economic benefits and costs of more stringent capital and liquidity requirements once banks have completed the transition to the new requirements. The report was produced by the Basel Committee's Long-term Economic Impact (LEI) working group, chaired by Claudio Borio (BIS) and Thomas Huertas (UK FSA).

[16] In Romer and Romer (2010)'s words: "*The quintessential exogenous change might be a tax cut motivated by a belief that lower marginal tax rates will raise output in the long run. Such an action is fundamentally different from the countercyclical actions discussed above because the goal is to raise normal growth, not to offset shocks acting to reduce growth relative to normal.*"

judgements on the overall level of systemic risk and subsequent CCyB policy decisions poses significant challenges.[17]

An advantage of our approach is that we directly utilise detailed information on policies from narrative records to effectively exclude those with a specific countercyclical design and/or motivation. However, our approach does not preclude identification by additionally controlling for potential confounding factors. To ensure our approach is 'doubly robust' we can further control for variables which plausibly simultaneously drive macroprudential policy decisions and macroeconomic outcomes. In our baseline specification we include a range of control variables intended to capture the level of cyclical systemic risk—including measures of credit growth, house-price growth and financial conditions. Moreover, in the spirit of Romer and Romer (2004), we explore the sensitivity of our baseline results to the inclusion of forecasted GDP growth as an additional control. This allows us to account for the information set that policymakers have available on the future state of the economy when the policy is announced.[18]

To operationalise this robustness exercise, we build on recent work on estimating dynamic causal effects in a quantile-regression setting in Lloyd and Manuel (2024). A common approach in the literature is to first estimate policy shocks as a residual from a regression of the policy reaction function, and then to employ these shocks within a second-stage regression (typically a local projection). Recent approaches to estimate the effects of macroprudential policies within a quantile-regression framework have followed this approach (Gelos et al., 2022; Brandão-Marques et al., 2021). In OLS settings, this 'two-step' approach is equivalent to directly regressing the outcome variable on the policy variable with reaction function variables as controls, a result that follows directly from the Frisch-Waugh-Lovell theorem. But in quantile regression, Lloyd and Manuel (2024) show that coefficient estimates from this two-step approach suffer from a form of quantile-regression omitted-variable bias and so generally fail to identify causal effects under a selection-on-observables assumption.[19] Based on the results in Lloyd and Manuel (2024), we therefore employ an alternate 'one-step' quantile-regression estimator of the outcome variable

[17] Taking the UK again as an example, the Bank of England's FPC publishes a set of around 20 core indicators that they use when assessing systemic risk, but their CCyB Policy Statement explicitly states that (*emphasis* added): "These indicators are only a *subset* of the *wide range* of economic and financial indicators, and the *wealth* of supervisory and market intelligence that support the FPC's assessment of the risk environment and its judgements on the CCyB." Controlling for this entire information set beyond just these core indicators is likely to be infeasible.

[18] A key insight of Romer and Romer (2004) is that forecasts of the outcome variable at the time of policy decisions alone can act as a sufficient statistic to control for all potential confounding factors that simultaneously drive policy and outcomes (see comment by Cochrane, 2004).

[19] This result draws on the econometric insight from Angrist, Chernozhukov, and Fernández-Val (2006) that quantile regression can be interpreted as a weighted-least squares estimator, with weights that are a function of quantiles.

on the macroprudential policy variable, which includes potential-reaction-function variables as controls. We formalise this argument and provide an exact formula for the bias in the frequently-used two-step approach in Appendix B.

In robustness, we also deal with another threat to identification: anticipation due to policy news. Here we use the information provided by MaPPED on the announcement and enforcement date of each policy action in the sample. This is a key advantage of MaPPED relative to other databases, allowing us to identify policies that are subject to implementation lag and therefore may be anticipated by economic agents. This approach mirrors that previously employed in the fiscal policy literature (Mertens and Ravn, 2012).

We argue that our approach—employing narrative methods within a one-step quantile regression that potentially controls for any residual endogeneity—is crucial to plausibly identify the causal effects of macroprudential policy on conditional quantiles of GDP growth.

3 Empirical Results: Macroprudential Policy and GDP

In this section, armed with our narratively-identified macroprudential policy shocks and the empirical framework presented in Section 2, we present results summarising the causal links between macroprudential policy and the entire distribution of future GDP growth.

3.1 Baseline Specification

Figure 3 presents the impulse responses of quantiles of the conditional GDP-growth distribution (for $\tau = 0.1, 0.5, 0.9$) to changes in our narrative macroprudential policy index across different horizons h from our baseline specification (1). Panel A of Table 3 presents the corresponding coefficient point estimates and standard errors at selected horizons for all shocks in our sample, alongside complementary OLS-regression estimates in Panel B for comparison. Panels C and D present coefficient estimates for the same regressions, but using the biggest shocks instead of all shocks. The latter analyses facilitate the interpretation of macroprudential policy shocks for estimated IRFs. The results highlight notable asymmetries in the effects of macroprudential policies on quantiles of the GDP-growth distribution, which can differ markedly from OLS estimates.

Comparing the estimates for the 10th and 90th percentiles with those for the median, we find that macroprudential policies affect the tails of the GDP-growth distribution disproportionately more than at the median. In fact, Panel (b) and (e) of Figure 3 indicates that the impact of tighter macroprudential policy on median GDP growth is small and statistically insignificant across most horizons—and likewise for the mean implied by the OLS estimates in Panel B of Table 3.

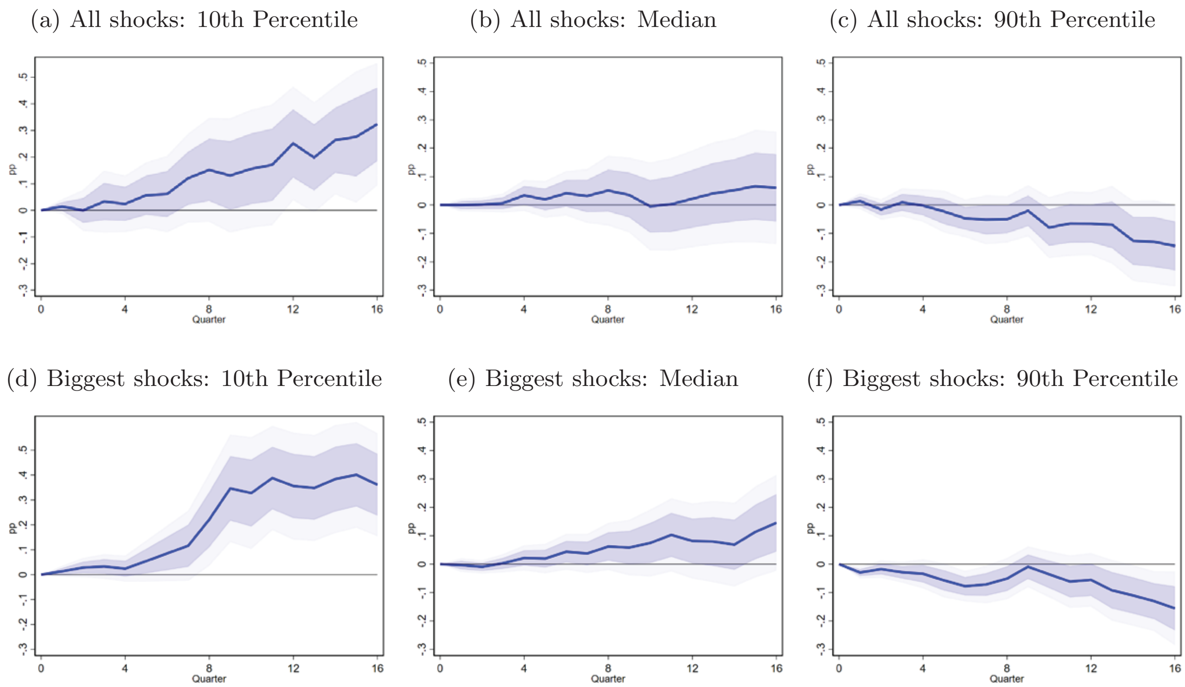
Nevertheless, macroprudential policy does have significant impacts on the tails of GDP growth. Comparing Panels (a) and (c) and (d) and (f) of Figure 3 indicates that tighter macroprudential policies have opposing effects to the downside (i.e., the $\tau = 0.1$ left tail) and upside (i.e., the $\tau = 0.9$ right tail). In particular, while a tightening macroprudential policy shock has an insignificant effect at the median across different horizons, it has a positive (negative) effect on the left (right) tail of the GDP-growth distribution that persists over the long term. Effects in both tails are statistically significant after 2-3 years. However, our estimates reveal that the quantitative impact on the left tail is larger in magnitude than the impact on the right tail, suggesting that tighter macroprudential policy alters the skew of the GDP-growth distribution—an implication that we presented visually in the left-hand plot of Figure 1 after fitting skew-t distributions to the fitted values implied by these quantile-regression estimates.

Table 3: Coefficient estimates $\beta^h(\tau)$ from baseline specification: regression of GDP growth on narrative measure macroprudential policy and controls

	$h = 4$	$h = 8$	$h = 12$	$h = 16$
Panel A: Quantile-Regression Estimates for All Macroprudential Policy Shocks				
$\tau = 0.1$	0.02	0.15ˆ	0.25**	0.32**
	(0.06)	(0.12)	(0.13)	(0.14)
$\tau = 0.5$	0.03ˆ	0.05	0.02	0.06
	(0.03)	(0.07)	(0.10)	(0.12)
$\tau = 0.9$	-0.00	-0.05ˆ	-0.07	-0.14**
	(0.03)	(0.05)	(0.07)	(0.09)
Panel B: OLS-Regression Estimates for All Macroprudential Policy Shocks				
OLS	0.02	0.02	0.03	0.06
	(0.03)	(0.06)	(0.08)	(0.10)
Panel C: Quantile-Regression Estimates for Biggest Macroprudential Policy Shocks				
$\tau = 0.1$	0.02	0.22**	0.36***	0.36***
	(0.03)	(0.11)	(0.13)	(0.12)
$\tau = 0.5$	0.02	0.06ˆ	0.08ˆ	0.15ˆ
	(0.03)	(0.05)	(0.08)	(0.10)
$\tau = 0.9$	-0.03ˆ	-0.05ˆ	-0.06	-0.16**
	(0.03)	(0.04)	(0.06)	(0.08)
Panel D: OLS-Regression Estimates for Biggest Macroprudential Policy Shocks				
OLS	0.00	0.12**	0.16**	0.19ˆ
	(0.03)	(0.07)	(0.10)	(0.14)

Notes: Panel A and C present coefficient estimates for the causal effects of a 1-unit tightening in macroprudential policy on the τ-th percentile of annual average real GDP growth at horizons $h = 4, 8, 12, 16$ for all shocks and biggest shocks in our sample, respectively. Panel B and D present corresponding estimates from OLS regressions. Coefficient estimates for the fixed effects and controls are not reported. Sample period is 1990Q1-2017Q4, for 12 advanced European economies. Standard errors are based on block-bootstrap with 1000 replications and are shown in parenthesis with: $\hat{}\, p < 0.32$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 3: Dynamic Response of GDP-Growth Quantiles to Macroprudential Policy Tightenings



(a) All shocks: 10th Percentile (b) All shocks: Median (c) All shocks: 90th Percentile

(d) Biggest shocks: 10th Percentile (e) Biggest shocks: Median (f) Biggest shocks: 90th Percentile

Notes: Estimated change in the τ-th percentile of annual average real GDP growth at horizon $h = 1, 2, ..., 16$, following a tightening macroprudential policy activation. All shocks in panels (a)-(c); Biggest shocks in panels (d)-(f). Sample period is 1990Q1-2017Q4, for 12 advanced economies. Shaded areas denote the 90% (light blue) and 68% (dark blue) confidence intervals based on block-bootstrapping with 1000 replications.

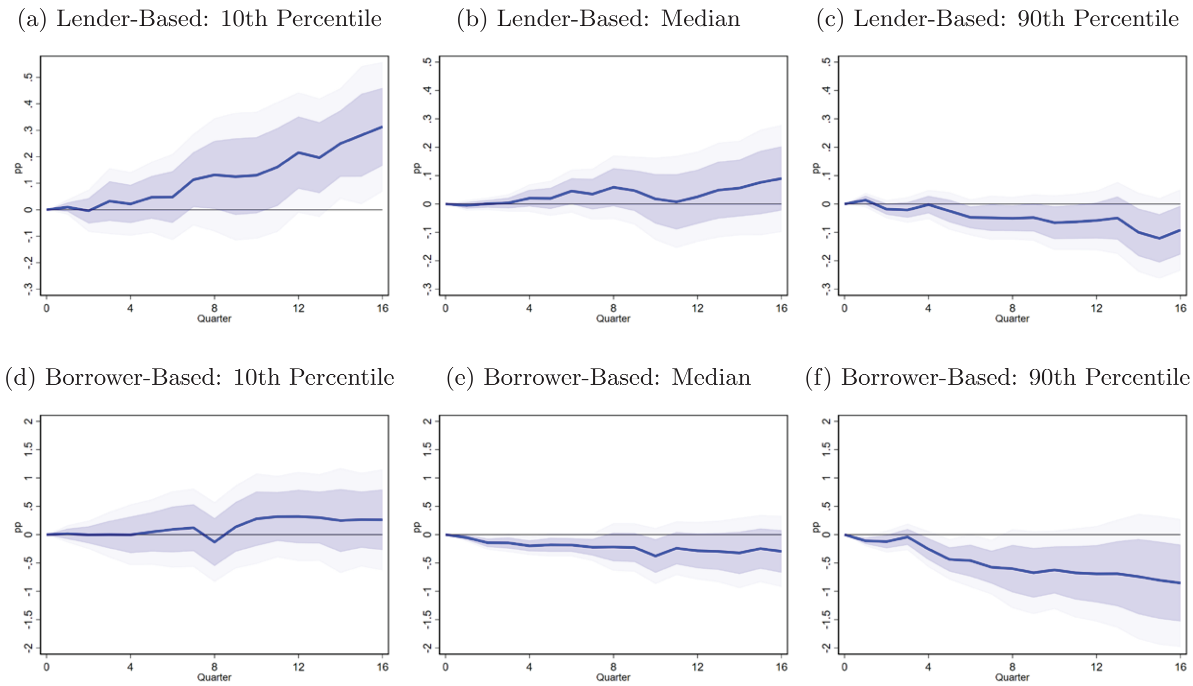
These results suggest that the benefits of tighter macroprudential policy on GDP growth are most clear at the left tail of the GDP-growth distribution—consistent with previous studies of growth-at-risk and macroprudential policy (Galán, 2020; Franta and Gambacorta, 2020). As Panel (a) and (d) of Figure 1 shows, tighter macroprudential policy shifts the left-tail of the GDP-growth distribution to the right, reducing the probability of a severe contraction in GDP growth. Otherwise put, our results show that tighter macroprudential policy can improve 'growth-at-risk'—a now standard measure of how bad growth can be under adverse circumstances typically associated with systemic distress (Adrian et al., 2019). Our results additionally imply that macroprudential policy can be effective at dampening tail risks to economic growth, and reducing the variance of future growth, without significant impacts at the centre of the distribution—again visible in Panel (a) and (d) of Figure 1. As a result, tighter macroprudential policy can make the growth outlook more resilient to negative future economic shocks. In what follows, all subsequent analyses, will be based on all the shocks within the sample.

Heterogeneity Across Macroprudential Measures. Our baseline results come from a single policy index that aggregates all macroprudential-policy actions. However, that index covers a range of different types of policies which could, in principle, have differential effects. To uncover heterogeneity across policy instruments, we distinguish between borrower- and lender-based measures, two widely used categorisations of macroprudential policy. Lender-based measures—which comprise the majority of measures in MaPPED—predominantly consistent of capital measures, while borrower-based measures primarily include LTV and debt-service-to-income ratios.

We construct separate macroprudential-policy shock series for the non-systematic lender- and borrower-based measures in the dataset. We then use these shock series in our baseline regression (1). The results are depicted in Figure 4, with impulse responses at the 10th, 50th and 90th percentiles for lender-based measures in Panels (a)-(c), and the corresponding impulse responses for borrower-based measures in Panels (d)-(f).

Reflecting the fact that lender-based measures explain the majority of variation in our aggregate macroprudential measures, the results for lender-based measures are broadly consistent with those in Figure 3 for our aggregate macroprudential policy shock. Tighter lender-based measures significantly increase the left tail of the future GDP-growth distribution, while simultaneously reducing the right tail. The former effect quantitatively dominates the latter such that

Figure 4: Dynamic Response of GDP-Growth Quantiles to Lender- and Borrower-Based Macroprudential Policy Tightenings



Notes: Estimated change in the τ-th percentile of annual average real GDP growth at horizon $h = 1, 2, ..., 16$, following a tightening macroprudential policy activation. Lender-based measures only in panels (a)-(c); borrower-based measures only in panels (d)-(f). Sample period is 1990Q1-2017Q4, for 12 advanced economies. Shaded areas denote the 90% (light blue) and 68% (dark blue) confidence intervals based on block-bootstrapping with 1000 replications.

the reduced variance of future GDP growth is also associated with a change in the skew—akin to that plotted in Panel (a) of Figure 1. Moreover, while our estimates for the impulse response to lender-based measures at the median are quantitatively small, they are marginally significant. Thus our results suggest that, if anything, tighter lender-based policies boost the centre of the GDP-growth distribution slightly.

Since borrower-based measures are less prevalent in our aggregate index, the estimated impulse responses in Panels (d)-(f) of Figure 4 have wider standard-error bands. In contrast to lender-based measures, however, the results suggest that borrower-based measures have larger effects on the right tail of the future GDP-growth distribution. While point estimates at the 10th percentile are broadly positive, they are statistically insignificant, and smaller in magnitude than the negative and significant coefficients for the 90th percentile. Therefore, our results indicate that tighter borrower-based macroprudential policy contributes to reduced macroeconomic tail risks by weighing particularly on right-tail outturns. Along with this, our estimates for the median are negative and statistically significant, albeit small in magnitude. Combined with the marginally positive and significant median effects of lender-based measures, these marginally negative and significant median effects for borrower-based measures help to explain why our median effects of the aggregate macroprudential index in Panel (b) of Figure 3 are insignificant. Moreover, the result for borrower-based measures is consistent with Richter et al. (2019), who find that borrower-based measures have a negative impact on average output growth over a four-year horizon.

Overall, therefore, our results imply that tighter macroprudential policy serves to reduce the variance of future GDP growth, boosting the left tail especially, as well as by reducing the right. Lender-based measures explain the majority of our aggregate results, though borrower-based measures also contribute to macroprudential policy's role in making the growth outlook more resilient to future economic shocks.

3.2 Robustness Analyses

In this sub-section, we demonstrate how the results presented in Figure 3 and Table 3 are robust to a number of checks. We summarise each below. Full results are presented in Appendix C.

Accounting for Macroeconomic Expectations. Our benchmark results can be interpreted as capturing the causal effects of macroprudential policy on the GDP-growth distribution *provided that* the policies included in the narrative measure $MaPP_{i,t}$ are not systematically correlated with macroeconomic expectations. To test this, we expand our baseline specification to include changes in expected GDP growth—formally, forecasted GDP growth over the following two quarters—as an additional control to proxy the information set available to policymakers when policy is announced—similar in spirit to Romer and Romer (2004).[20] If our narrative macroprudential policy shock was *not* exogenous with respect to expectations in the baseline, then the addition of expected output growth as additional control should imply a significant change in the estimated impulse responses. However, we find that the results with and without the expected economic outlook in the set of controls are similar.

Lags in Policy Implementation. A key advantage of MaPPED is that it contains information on both the announcement and the enforcement date of each policy action in the sample. As discussed, we use the announcement date for all non-systematic policies in our baseline specification. However, some policies may have implementation lags that could influence estimated impulse responses. To account for this, we reconstruct the narrative shock index using policies with no implementation lag only.[21] Re-estimating our regression with these alternative shocks, our benchmark results and their economic implications are qualitatively unchanged. Estimation uncertainty is, however, larger than in the baseline with *all* non-systematic policies are included.[22]

Alternative Controls. We also assess the robustness of our findings to a range of alternative control specifications. First, we augment our specification with a financial conditions index (FCI) in our set of controls—that used by Adrian et al. (2022) and Lloyd et al. (2023). Our results are qualitatively similar when controlling for these FCIs. Second, we augment our specification to include controls for monetary policy—following recent research by Loria et al. (2025) who show that monetary policy has heterogeneous effects on the GDP-growth distribution.[23] This specification also helps to control for potential interlinkages between both monetary and

[20] Our goal here is not to include all forward-looking variables that potentially feature in a macroprudential 'reaction function'. Rather, we wish to control for any potential endogeneity between macroprudential policies and our outcome variable of interest, future GDP growth. In this case, controlling for GDP forecasts—and not forecasts of other variables—should be sufficient to remove this particular threat to identification (see comment by Cochrane, 2004).

[21] To operationalise this, we formally define policies with an implementation lag as those for which there is a delay between the announcement and the enforcement date of at least 90 days. Around 20% of the policy actions included in MaPPED suffer from implementation lag according to this definition. Mertens and Ravn (2012) use a similar 90-day threshold to account for fiscal policy implementation lags.

[22] The larger uncertainty in this specification can be explained by the lower variation in $MaPP_{i,t}$ when we only use policies without implementation lags to construct the index.

[23] In particular, they find that the 10th percentile of the predictive growth distributions responds about three times more than the median to a monetary policy shock.

macroprudential policies (e.g., Kim and Mehrotra, 2018; Altavilla, Laeven, and Peydró, 2020; Coman and Lloyd, 2022). Augmenting our baseline specification with short-term interest rates as an additional control, we find similar results to our baseline.

Alternative Macroprudential Policy Index. As discussed, our baseline macroprudential policy measure weights different policy actions depending on their type: e.g., activation, recalibration, deactivation. However, this information is not included in other datasets, which instead assign integer values to quantify policy tightenings and loosenings. To connect our results to alternative macroprudential policy datasets, we transform our $MaPP$ index into a five-value discrete variable, and re-estimate our main regression.[24] The results with the discrete macroprudential policy index are qualitatively similar to the baseline results.

Sample Stability: Excluding the GFC. We re-estimate our baseline regressions for GDP growth using data up to 2007. We find that excluding the post-2007 period does not result in significant changes, and our baseline results remain qualitatively the same. This exercise confirms that the baseline estimates were not entirely driven by the economic events during the GFC, or the macroprudential policy reforms initiated following it.

Alternative Country Fixed Effects. We also explore robustness around the use of Kato et al. (2012) country-fixed effects, by re-estimating equation (1) using the Machado and Santos Silva (2019) quantiles-via-moments estimator. Our baseline results and their economic implications are not sensitive to the particular manner in which country-fixed effects are estimated.

4 Exploring the Transmission Channels

So far, we have shown that tighter macroprudential policy has causal positive (negative) effects on the left (right) tail of the GDP-growth distribution, reducing its variance. In this section, we turn to analysing how macroprudential policy transmits in this way to the GDP-growth

[24] Formally, we allow $\Delta MaPP_{i,t}^{new}$ to take values {-2,-1, 0, 1, 2}, where $\Delta MaPP_{i,t}^{new} = -2$ if $\Delta MaPP_{i,t} < -1$, -1 if $0 > \Delta MaPP_{i,t} \geq -1$, $= 0$ if $\Delta MaPP_{i,t} = 0$, $= 1$ if $0 < \Delta MaPP_{i,t} \leq 1$, and $= 2$ if $\Delta MaPP_{i,t} > 1$. This alternative weighting scheme is consistent with previous work on macroprudential policy (e.g. Alam et al., 2019; Gelos et al., 2022) and reflects, as a general rule and in net terms, whether there was more than one loosening measure, one loosening measure, no change, one tightening measure, or more than one tightening measures in a given quarter, respectively. In practice, given the history of quarterly macroprudential policy implementations in our database, this alternative weighting scheme is very similar but not exactly equal to assigning equal weight to all policy actions.

distribution, exploring the mechanisms behind these results. Why does macroprudential policy have positive effects on the lower end of the GDP growth distribution? Why do these policies have the opposite effect on the upper end of the GDP-growth distribution? To do so, we first investigate the impact of macroprudential policy on intermediate variables, like credit growth, and then link the effects of these intermediate variables to the GDP-growth distribution.

4.1 Quantity of Credit: A 'Credit-at-Risk' Channel

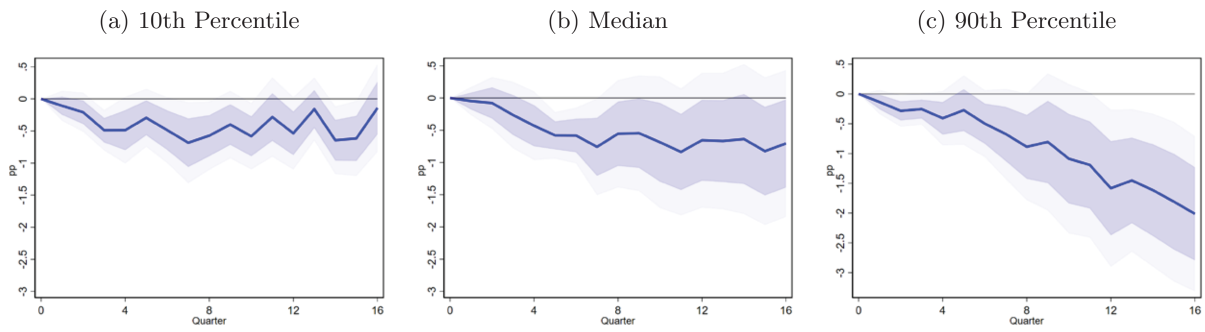
Preexisting empirical work has consistently found that financial booms, particularly credit booms, often precede financial crises (Schularick and Taylor, 2012; Jordà, Schularick, and Taylor, 2015; Richter, Schularick, and Wachtel, 2021). Therefore, the prevention and mitigation of credit booms is a natural candidate channel through which macroprudential policy can reduce the probability of tail outturns. Quantile regressions offer an ideal framework to explore this mechanism in detail.

Our approach consists of two steps. First, we show that macroprudential policy is particularly effective at mitigating 'excessive' credit growth. Specifically, we find that tightening macroprudential policy particularly pushes down the 90th percentile of the credit distribution. Second, we show that the upper tail of the credit distribution—i.e., the 90th percentile of credit growth—is strongly and systematically negatively (positively) related to the left tail (right tail) of the GDP growth distribution. This second step allows us to show that how macroprudential policy works through a 'credit-at-risk' channel: systematically reducing the likelihood of extreme GDP-growth outturns by influencing the tails of the credit-growth distribution.

Causal Effects of Macroprudential Policy on Credit-at-Risk. We start by estimating the responses of future credit quantiles to a tightening macroprudential shock by re-estimating our local-projection specification (1) for credit growth. Hence, $\Delta y_{i,t+h}$ refers to the annual average real private credit growth over h quarters. This allows us to formally explore whether macroprudential policies have an asymmetric impact on the credit-growth distribution.

We plot the impulse responses of the credit quantiles after a contractionary policy shock in Figure 5, with corresponding point estimates and standard errors shown in Table 4. We focus again on the 10th, 50th and 90th percentiles. The main takeaway from Figure 5 is that there is a clear asymmetry in the response of credit after a macroprudential policy shock. In particular, the 90th percentile responds more than the median, which in turn moves more than the 10th

Figure 5: Impulse Response of Quantiles of the Credit-Growth Distribution to Macroprudential Policy Tightenings



Notes: Estimated change in the τ-th percentile of annual average real credit growth at horizon $h = 1, 2, ..., 16$, following a tightening macroprudential policy activation. Sample period is 1990Q1-2017Q4. Shaded areas denote the 90% (light blue) and 68% (dark blue) confidence intervals based on block-bootstrapping with 1000 replications.

Table 4: Coefficient estimates for $\beta^h(\tau)$ from regression of credit growth on narrative measure of macroprudential policy and controls

	$h = 4$	$h = 8$	$h = 12$	$h = 16$
PANEL A: Quantile-Regression Estimates				
$\tau = 0.1$	-0.49ˆ	-0.57**	-0.54ˆ	-0.14
	(0.31)	(0.32)	(0.34)	(0.41)
$\tau = 0.5$	-0.43ˆ	-0.55ˆ	-0.65ˆ	-0.71ˆ
	(0.32)	(0.50)	(0.63)	(0.68)
$\tau = 0.9$	-0.41ˆ	-0.89**	-1.58**	-2.01***
	(0.27)	(0.54)	(0.79)	(0.78)
PANEL B: OLS-Regression Estimates				
OLS	-0.39**	-0.59ˆ	-0.67ˆ	-0.87ˆ
	(0.16)	(0.41)	(0.57)	(0.66)

Notes: Panel A presents coefficient estimates for the causal effects of a 1-unit tightening in macroprudential policy on the τ-th percentile of annual average real credit growth at horizon $h = 4, 8, 12, 16$. Panel B presents the corresponding estimates from OLS regressions. Coefficient estimates for the fixed effects and controls are not reported. Sample period is 1990Q1-2017Q4, for 12 advanced European economies. Standard errors are based on block-bootstrapping with 1000 replications and are shown in parenthesis with: ˆ $p < 0.32$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

percentile. We find that a tightening prudential shock pushes down the right tail more strongly than other parts of the distribution.[25]

Together, these results imply that tighter macroprudential policy reduces the variance of future credit growth. But, as Panel (b) of Figure 1 demonstrates, the effect is most pronounced in the right tail of the distribution. If anything tighter macroprudential policy shifts the centre

[25] Following a macroprudential policy tightening, we see similar changes in the quantiles of country-specific FCIs. As we show in Appendix D.1, tighter macroprudential policy is associated with a reduction in financial 'stress' in the medium term, and disproportionately so in the right tail.

of the credit-growth distribution slightly to the left. However, how this maps to GDP-growth is a separate question, which we turn to now.

Effects of Credit-at-Risk on GDP-at-Risk. In our second step, we formally explore the role that credit-at-risk plays in shaping both downside and upside risks to the GDP growth by estimating the following quantile local projections:

$$
\begin{aligned}
Q_{\Delta y_{i,t+h}}(\tau|\Delta Credit_{i,t}, \mathbb{1}^{Boom}_{i,t}, X_{i,t}) =& \boldsymbol{\alpha}^h_i(\tau) + \Delta Credit_{i,t}\beta^h(\tau) + \mathbb{1}^{Boom}_{i,t}\theta^h(\tau) \\
&+ \Delta Credit_{i,t} \ \times \ \mathbb{1}^{Boom}_{i,t}\gamma^h(\tau) + \mathbf{x}'_{i,t}\boldsymbol{\vartheta}^h(\tau), \quad \tau \in (0,1)
\end{aligned}
\tag{2}
$$

where the dependent variable $\Delta y_{i,t+h}$ now refers to h-period-ahead annual average real GDP growth. As before, $\boldsymbol{\alpha}^h_i$ refers to country- and quantile-specific fixed effects and $h = 1, 2, ..., H$, with $H = 16$. We continue to focus on the 10th, 50th and 90th percentiles to capture the potentially non-linear impact of credit growth on the GDP-growth distribution. The set of controls $\mathbf{x}_{i,t}$ also includes changes in our macroprudential policy index.

The key novelty of equation (2) comes from the fact that we create an indicator variable for credit booms $\mathbb{1}^{Boom}_{i,t}$ based on the distribution of 2-year credit growth. In particular, we define the credit-boom indicator as:

$$
\mathbb{1}^{Boom}_{i,t} = \begin{cases} 1 & \text{if} \quad \Delta_8 Credit_{i,t} > \Delta_8 Credit_{i,90th\ percentile} \\ 0 & \text{otherwise} \end{cases}
\tag{3}
$$

Our credit-boom definition based on the distribution of past credit growth is consistent with the existing literature on credit-boom measurement (see, e.g., Greenwood, Hanson, Shleifer, and Sørensen, 2022). In particular, we use the 2-year change in credit to capture persistent changes in this indicator, which have been shown to be a leading predictor of financial crises (e.g., Schularick and Taylor, 2012). The 90th percentile and therefore the assignment thresholds are country-specific, as they are based on the distribution of within-country credit growth.

Given these definitions, $\beta^h(\tau)$ in regression (2) captures the association between real credit growth and the GDP-growth distribution in non-boom periods. $\gamma^h(\tau)$, in turn, tracks how the response of the τ-th percentile of real annual GDP growth following a +1 standard deviation in credit growth differs in boom versus non-boom periods. Therefore, $\beta^h(\tau) + \gamma^h(\tau)$ allows us to compute the average impact of real credit growth on the future GDP-growth distribution when the economy is already in a credit boom.

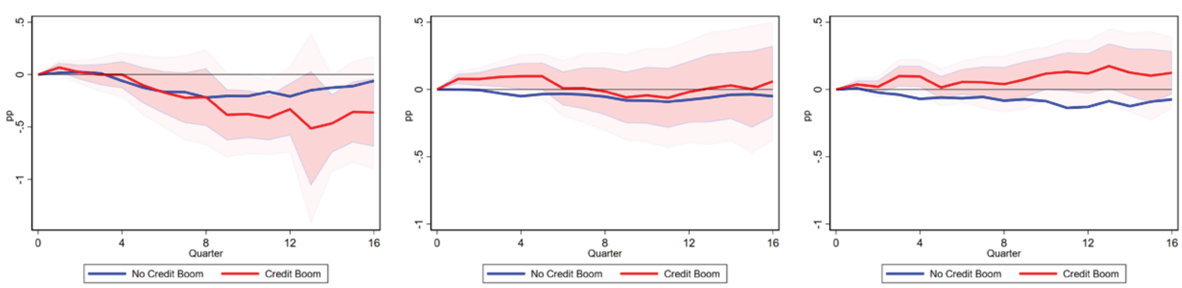
Figure 6 presents the main results from this exercise. Overall credit growth is associated with a significant reduction in the left tail of annual average domestic GDP growth, as Panel (a) demonstrates. This result holds in both boom and non-boom periods. However, this negative effect is particularly strong when the economy is already experiencing a credit boom, suggesting that credit growth is especially associated with a deterioration in growth-at-risk over the medium term in financial boom episodes.

In contrast, as Panel (c) shows, credit growth has an association with the right tail of the GDP-growth distribution that varies in sign, depending on whether there is a credit boom in the economy. While credit growth does not have a significant impact on the right tail of the distribution in non-boom periods, it does increase the right tail of the distribution of GDP growth in boom periods.

Taken together, our results have an important policy implication. High credit growth affects both downside and upside macroeconomic tail risks. In credit booms, increases in credit growth are associated with higher overall variance in the future GDP-growth distribution, shifting the left tail further left and shifting the right tail to the right. This is the case, even though the estimates in Panel (b) suggest that credit growth does not have significant impacts on median GDP growth over the medium term, independently of whether there is a credit boom or not. Taken together, our empirical findings, therefore, suggest that by defusing upside credit-at-risk (i.e., excessive credit growth) macroprudential policy can be effective in reducing GDP-growth volatility and mitigating macroeconomic tail risks.

Figure 6: Impulse Response of Quantiles of the GDP-Growth Distribution to +1 Standard Deviation Increase Credit Growth with and without a Credit Boom

(a) 10th Percentile (b) Median (c) 90th Percentile



Notes: Estimated change in the τ-th percentile of annual average real GDP growth at horizon $h = 1, 2, ..., 16$, following a +1 standard deviation increase in credit growth. Non-linearity: credit booms versus non-credit booms periods. Sample period is 1990Q1-2017Q4. Shaded areas denote the 68% (dark red) and 90% (light red) confidence intervals based on block-bootstrapping with 1000 replications.

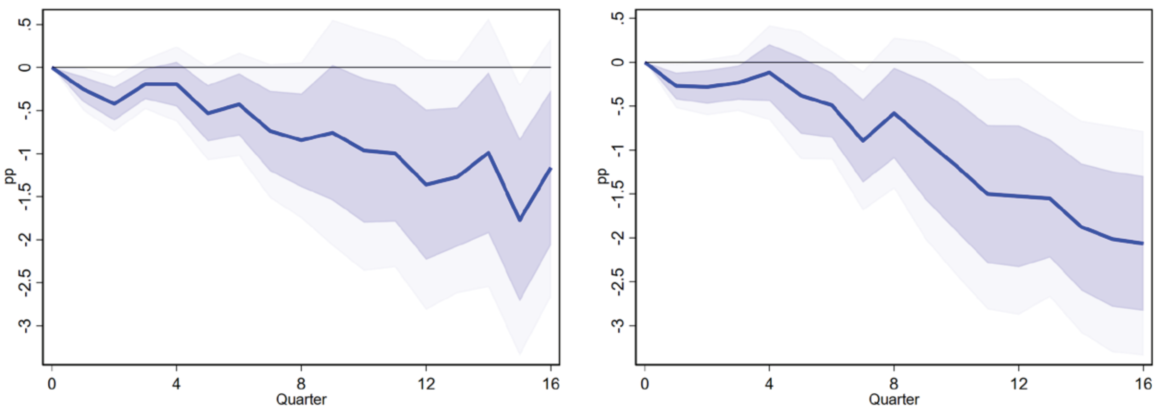
4.2 Composition of Credit

In this section, we investigate the transmission of macroprudential policy through its effect on the composition of credit. In particular, we explore the extent to which macroprudential policy is equally effective at preventing both household and business credit booms.

To gain deeper insights into this mechanism, we use quantile local projections and estimate the responses of real household and business credit following a tightening macroprudential policy shock. Given the findings in the previous sub-section, we focus on the 90th percentile of the

Figure 7: Impulse Response of 90th percentile of the Credit-Growth Distribution to Macroprudential Policy Tightenings

(a) Household Credit (b) Business Credit



Notes: Estimated change in the 90th percentile of annual average real household and business credit at horizon $h = 1, 2, ..., 16$, following a tightening macroprudential policy activation. Sample period is 1990Q1-2017Q4. Shaded areas denote the 90% (light blue) and 68% (dark blue) confidence intervals based on block-bootstrapping with 1000 replications.

credit distribution to explore the effectiveness of macroprudential policy to prevent household and business credit booms. The set of controls is the same as in our previous specification which had total credit as the dependent variable.

We present the response of the upper end of household versus business credit distribution following a macroprudential policy shock in Figure 7, and the coefficient estimates in Table 5. The main conclusion from Figure 7 is that macroprudential policy has a similar impact on the right tails of household and business credit growth. That is, macroprudential policy defuses both upside household and corporate credit tail risks.

Table 5: Coefficient estimates for $\beta^h(\tau)$ from regression of household and business credit growth on narrative measure of macroprudential policy and baseline controls at 90th percentile

	$h = 4$	$h = 8$	$h = 12$	$h = 16$
Panel A: Quantile-Regression Estimates for Household Credit				
$\tau = 0.9$	-0.19	-0.84ˆ	-1.36ˆ	-1.16ˆ
	(0.26)	(0.54)	(0.87)	(0.90)
Panel B: Quantile-Regression Estimates for Business Credit				
$\tau = 0.9$	-0.12	-0.58ˆ	-1.53**	-2.06***
	(0.32)	(0.51)	(0.81)	(0.77)

Notes: This table presents coefficient estimates reflecting the association between the 90th percentile of annual average real household and business credit growth at horizon $h = 1, 2, ..., 16$ and a tightening macroprudential policy activation. Coefficient estimates of fixed effects and controls not reported. Sample period is 1990Q1-2017Q4, for 12 advanced economies. Standard errors are based on block-bootstrapping with 1000 replications and are shown in parenthesis with: ˆ $p < 0.32$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

4.3 House Prices

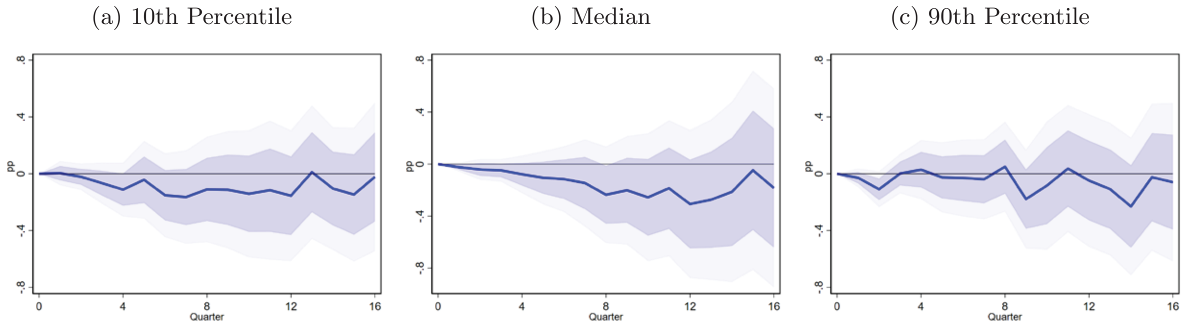
In addition to the 'credit-at-risk channel', another candidate and interrelated channel through which macroprudential policy could affect the GDP-growth distribution is through changes in the house-price distribution. Asset-price dynamics, and in particular house-price bubbles, have been shown to be systematically associated with future severe financial crises, especially when fuelled by credit expansions (Jordà et al., 2015; Richter et al., 2021). Therefore, to the extent to which macroprudential policy may defuse upside house-price growth risks, it may prevent extreme GDP-growth outturns.

In this sub-section, we formally explore the extent to which shifts in the house-price distribution following a tightening macroprudential policy activation are consistent with the observed post-policy tail-growth dynamic responses. To do so, we estimate quantile local-projection regression (1) for house prices. $\Delta y_{i,t+h}$ now denotes the annual average real house-price growth over h quarters. This specification allows us to formally explore whether macroprudential policies have a heterogeneous impact on the house-price distribution, and if so, to what extent the house-price channel can explain the opposite effects we found on the left and right tail of the GDP-growth distribution after a tightening prudential shock.

Figure 8 presents the impulse response of quantiles of the conditional house-price growth distribution (for $\tau = 0.1, 0.5, 0.9$) to changes in our narrative macroprudential policy index across different horizons h.[26] Our results point to a small negative impact of macroprudential policy on quantiles of the house-price growth distribution. However, the estimation uncertainty

[26] Corresponding point estimates and standard errors are tabulated in Appendix D.3, Table D.4.1.

Figure 8: Impulse Response of Quantiles of the House-Price Distribution to Macroprudential Policy Tightenings



Notes: Estimated change in the τ-th percentile of annual average real house prices growth at horizon $h = 1, 2, ..., 16$, following a tightening macroprudential policy activation. Sample period is 1990Q1-2017Q4. Shaded areas denote the 90% (light blue) and 68% (dark blue) confidence intervals based on block-bootstrapping with 1000 replications.

is very large, and the effect is not statistically significant across any horizons or quantiles. The main implication of our findings is that the responses across the whole GDP-growth distribution following a tightening macroprudential policy activation are unlikely to be driven by the house-price dynamics after a tightening prudential policy activation.

Overall, we find limited evidence of other significant channels through which macroprudential policy affects the conditional distribution of GDP-growth and conclude that the 'credit-at-risk' channel plays a major role in explaining the heterogeneous impact of macroprudential policy on the tails of GDP growth.

5 Conclusion

What are the causal effects of macroprudential policy across the GDP-growth distribution? And what are the channels? In this paper, we answer both questions by exploiting a dataset covering a range of macroprudential policy actions across advanced European economies. We identify unanticipated and exogenous narrative macroprudential policy 'shocks' and employ them within a quantile-regression setup to identify causal effects across the distribution of future macroeconomic outcomes. Our main finding is that while macroprudential policy has muted effects on the centre of the GDP-growth distribution, tighter policy reduces the variance of future economic growth. A key implication from our analysis is that macroprudential policy can effectively enhance financial stability by significantly reducing the likelihood of extreme GDP-growth outturns. We further show that the 'credit-at-risk' channel is crucial to account for the dynamic effects of macroprudential policy in the tails of future GDP growth. In particular, our results suggest that by defusing upside credit-growth risk tighter macroprudential policy can be effective in mitigating downside and upside macroeconomic tail risks. Overall, our paper provides novel evidence on the causal effects of macroprudential policies on the entire distribution of potential macroeconomic outcomes.

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Appendix

A Weighting Scheme, Data Sources and Summary Statistics

Table A1: Weighting Scheme for Different Macroprudential Policy Actions in Narrative Measure

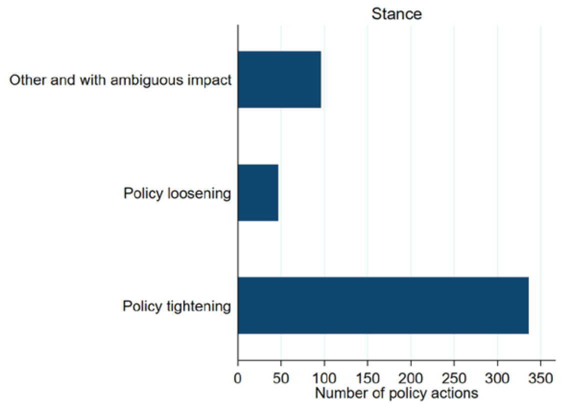
Type of Policy Action	Weight	Strengthening / Loosening	Sign	Final Weight
Activation	1	Tightening	+	1
		Other/ambiguous impact		0
		Loosening	-	-1
Change in the Level	0.25	Tightening	+	0.25
		Other/ambiguous impact		0
		Loosening	-	-0.25
Change in the Scope	0.10	Tightening	+	0.10
		Other/ambiguous impact		0
		Loosening	-	-0.10
Maintaning the Existing Level and Scope	0.05	Tightening	+	0.05
		Other/ambiguous impact		0
		Loosening	-	-0.05
Deactivation	Dependent on the life-cycle of the tool (cumulative index drops to zero)			

Notes: Description of the weights used to construct the cumulative index for each policy instrument based on Meuleman and Vander Vennet (2020).

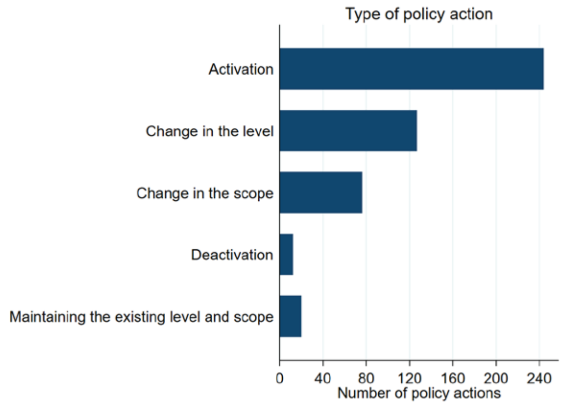
Table A2: List of Data Sources

Variables	Source
Gross Domestic Product (GDP)	OECD database
Consumer Price Index (CPI)	Federal Bank Reserve of St.Louis (FRED)
Total Credit to the Private Non-Financial Sector	Bank for International Settlements (BIS)
Total Credit to Households	Bank for International Settlements (BIS)
Total Credit to non-financial corporations	Bank for International Settlements (BIS)
House Prices	Bank for International Settlements (BIS)
VIX	Datastream
GDP forecast	OECD database
3-Month or 90-day Rates and Yields: Interbank Rates	IFS + FRED
Macroprudential Policy Index ($MaPP$)	Authors' estimation using MaPPED database

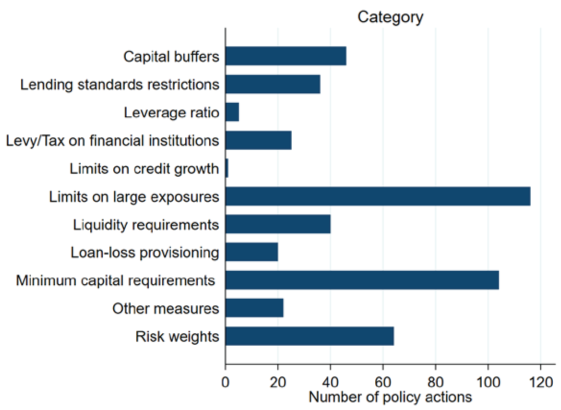
Figure A1: Number of policy actions by stance, category, type and country



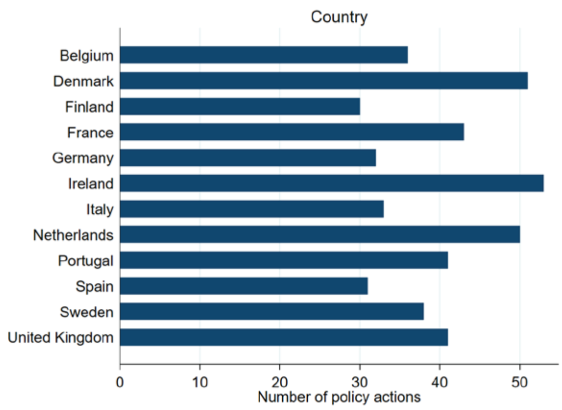
(a) Stance of Policy Action



(b) Type of Policy Action



(c) Categories of Policy Action



(d) Policy Actions by Country

B Identification in Quantile Regression with Confounders

In this Appendix, we formalise some of the discussion around identification in Section 2.3. Our exposition follows closely that in Angrist et al. (2006) and Jordà et al. (2013), and builds on work on identification in quantile regression in Lloyd and Manuel (2024).

We are interested in formalising the assumptions under which our estimated coefficients can be interpreted as capturing the causal effect of macroprudential policies. We define y_{t+h} and $MaPP_t$ as in Section 2.1, as h-period ahead annual average real GDP growth and our narrative-based macroprudential policy indicator, where we drop the cross-sectional country index in the panel for ease of exposition. We also define $\mathbf{x}_t$ as a set of conditioning variables capturing the state of the macro-financial environment at time t. We then define the potential outcome $y_{t,h}(z)$ as the value that the observed outcome variable y_{t+h} would have taken if $MaPP_t = z$ for all possible values z. We first define the causal effect of setting $\Delta MaPP_t = 1$ relative to some benchmark value $\Delta MaPP_t = 0$ on conditional quantiles of y_t as:

$$\mathbb{Q}_\tau \left(y_{t,h}(1)|\mathbf{x}_t, \Delta MaPP_t\right) - \mathbb{Q}_\tau \left(y_{t,h}(0)|\mathbf{x}_t, \Delta MaPP_t\right) \tag{4}$$

This equates to our causal effect of interest, capturing how macroprudential policies affect the entire distribution of future GDP growth outcomes conditional on the current macro-financial environment. We never observe counterfactual outcomes and so the quantiles in equation (4) cannot be estimated directly. One route to identification is the following assumption:

Conditional Independence (CI) $y_{t,h}(z) \perp\!\!\!\perp \Delta MaPP_t|\mathbf{x}_t$ for all z

This states that potential outcomes are fully independent of policy conditional on $\mathbf{x}_t$. This can be understood as a slightly weaker assumption than the claim that potential outcomes are unconditionally independent of policy, i.e.:

Unconditional Independence (UI) $y_{t,h}(z) \perp\!\!\!\perp \Delta MaPP_t$ for all z

In effect, CI allows that our narrative measure $\Delta MaPP_t$ is not fully exogenous, but that the set of conditioning variables $\mathbf{x}_t$ successfully captures all confounding factors that simultaneously drive our narrative-based measure and the outcome variable. Under CI, $\beta^h(\tau)$ in our baseline quantile regression (1) is equal to the causal effect of interest from equation (4). To see this, note that under CI, we can write the causal effect of interest in terms of observable conditional quantiles:

$$\begin{aligned}\mathbb{Q}_\tau \left(y_{t,h}(1)|\mathbf{x}_t, \Delta MaPP_t\right) &- \mathbb{Q}_\tau \left(y_{t,h}(0)|\mathbf{x}_t, \Delta MaPP_t\right) \\ &= \mathbb{Q}\left(y_{t+h}|\mathbf{x}_t, \Delta MaPP_t = 1\right) - \mathbb{Q}\left(y_{t+h}|\mathbf{x}_t, \Delta MaPP_t = 0\right)\end{aligned}$$

Since our quantile regression from equation (1) provides a direct estimate of the right-hand side of this equation, under CI, the coefficient $\beta^h(\tau)$ identifies the causal effect of interest.

An alternate estimation strategy in the literature on the quantile treatment effects of macroprudential policies employs a two-step estimation strategy. This amounts to estimating an OLS regression of the policy variable on potential confounding factors and then a quantile regression of the outcome variable on the residual from the first-stage. Defining ϵ_t as the OLS residual from the first-stage, the coefficient of interest from this two-step procedure $\beta^h_{2S}(\tau)$ is estimated via the following quantile regression:

$$\mathbb{Q}\left(y_{t+h}|\epsilon_t\right) = \boldsymbol{\alpha}^h_{2S}(\tau) + \epsilon_t \beta^h_{2S}(\tau) \quad \tau \in (0,1) \tag{5}$$

The key insight from Lloyd and Manuel (2024) is that, while such an approach is valid for estimating causal effects under CI in an OLS setting, in quantile regression this two-step approach can suffer from a form of quantile-regression omitted variable bias. Intuitively, although ϵ_t is uncorrelated with $\mathbf{x}_t$ by construction, this is insufficient for partialling out the effect of $\mathbf{x}_t$ on y_t across all conditional quantiles. In particular, they show that the difference between the coefficient from our one-step quantile regression $\beta^h(\tau)$ and the two-step coefficient $\beta^h_{2S}(\tau)$ can be expressed as:

$$\beta^h_{2S}(\tau) = \beta^h(\tau) + \boldsymbol{\phi}_{\mathbf{1}}(\tau)\frac{\mathbb{E}\left[w_\tau \varepsilon_t \mathbf{x}'_t\right]}{\mathbb{E}\left[w_\tau \varepsilon_t^2\right]}$$

where the second term amounts to the formula for omitted-variable bias for quantile regression (Angrist et al., 2006) with $w_\tau = \int_0^1 f_{u_\tau^{Hyb}}\left[u\ \left(\varepsilon_t \beta(\tau) - \mathbf{x}'_t \boldsymbol{\phi}(\tau) - \varepsilon_t \beta_{hyb}(\tau)\right)|\varepsilon_t, \mathbf{x}'_t\right]\ \mathrm{d}u/2$ and additional terms are defined from the following "hybrid" quantile regression:

$$y_{t+h} = \boldsymbol{\alpha}^h_{Hyb}(\tau) + \epsilon_t \beta^h_{Hyb}(\tau) + \boldsymbol{\phi}(\tau)\mathbf{x}'_t + u_\tau^{Hyb} \quad \tau \in (0,1)$$

C Sensitivity Checks

In this appendix, we present our findings from all the robustness exercises described in Section 3.2.

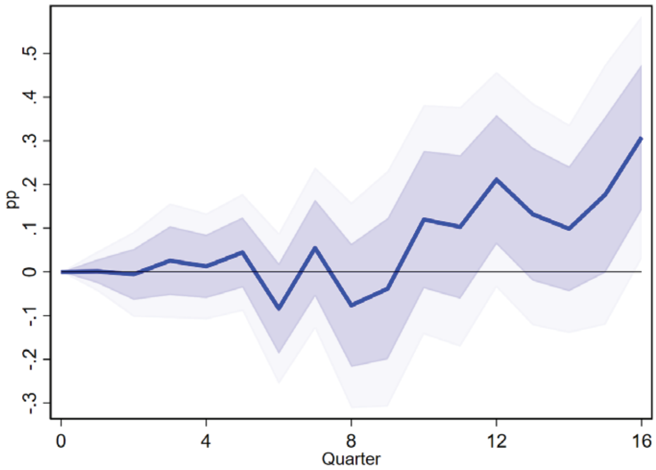
Table C.1: Baseline and Robustness estimation results: GDP-growth distribution

	$\tau = 0.1$								
	Baseline	Biggest Shocks Subsample	No Implementation Lag	Expectation Data	Alternative Macroprudential Index	Control-augmented: FCI	Control-augmented: Monetary Policy	Subsample: Excluding GFC	Alternative CFE
$h = 4$	0.02	0.02	0.01	0.04	0.02	0.01	0.01	0.03	0.01
	(0.06)	(0.03)	(0.07)	(0.06)	(0.08)	(0.07)	(0.05)	(0.09)	(0.06)
$h = 8$	0.15^	0.22**	-0.08	0.15^	-0.03	0.14^	0.11^	0.02	0.10
	(0.12)	(0.11)	(0.14)	(0.10)	(0.12)	(0.11)	(0.08)	(0.21)	(0.12)
$h = 12$	0.25**	0.36***	0.21^	0.24**	0.18^	0.18^	0.20**	0.18^	0.21^
	(0.13)	(0.13)	(0.15)	(0.11)	(0.12)	(0.11)	(0.09)	(0.18)	(0.17)
$h = 16$	0.32**	0.36***	0.31**	0.31***	0.27**	0.19**	0.25**	0.22^	0.26^
	(0.14)	(0.12)	(0.17)	(0.12)	(0.14)	(0.11)	(0.10)	(0.16)	(0.19)
	$\tau = 0.5$								
	Baseline	Biggest Shocks Subsample	No Implementation Lag	Expectation Data	Alternative Macroprudential Index	Control-augmented: FCI	Control-augmented: Monetary Policy	Subsample: Excluding GFC	Alternative CFE
$h = 4$	0.03^	0.02	0.01	0.04^	0.02	0.00	0.01	-0.00	0.02
	(0.03)	(0.03)	(0.04)	(0.03)	(0.04)	(0.03)	(0.03)	(0.05)	(0.04)
$h = 8$	0.05	0.06^	0.05	0.06	0.03	0.01	0.03	0.00	0.02
	(0.07)	(0.05)	(0.09)	(0.07)	(0.08)	(0.06)	(0.08)	(0.19)	(0.07)
$h = 12$	0.02	0.08^	0.00	0.03	-0.04	-0.01	-0.01	-0.00	0.01
	(0.10)	(0.08)	(0.13)	(0.09)	(0.10)	(0.09)	(0.07)	(0.23)	(0.10)
$h = 16$	0.06	0.15^	0.09	0.05	0.00	0.05	0.02	0.09	0.04
	(0.12)	(0.10)	(0.15)	(0.11)	(0.12)	(0.11)	(0.10)	(0.22)	(0.13)
	$\tau = 0.9$								
	Baseline	Biggest Shocks Subsample	No Implementation Lag	Expectation Data	Alternative Macroprudential Index	Control-augmented: FCI	Control-augmented: Monetary Policy	Subsample: Excluding GFC	Alternative CFE
$h = 4$	-0.00	-0.03^	-0.00	-0.00	-0.00	0.01	-0.01	-0.01	0.03
	(0.03)	(0.03)	(0.04)	(0.03)	(0.05)	(0.03)	(0.04)	(0.05)	(0.07)
$h = 8$	-0.05^	-0.05^	-0.08^	-0.06^	-0.08^	-0.02	-0.05^	-0.05	-0.04
	(0.05)	(0.04)	(0.07)	(0.05)	(0.07)	(0.06)	(0.05)	(0.10)	(0.07)
$h = 12$	-0.07	-0.06	-0.06	-0.07^	-0.15**	-0.14**	-0.09^	-0.13	-0.11^
	(0.07)	(0.06)	(0.09)	(0.07)	(0.08)	(0.08)	(0.07)	(0.14)	(0.10)
$h = 16$	-0.14**	-0.16**	-0.05	-0.09	-0.19**	-0.13^	-0.12^	-0.12	-0.11
	(0.09)	(0.08)	(0.11)	(0.09)	(0.10)	(0.08)	(0.09)	(0.18)	(0.12)

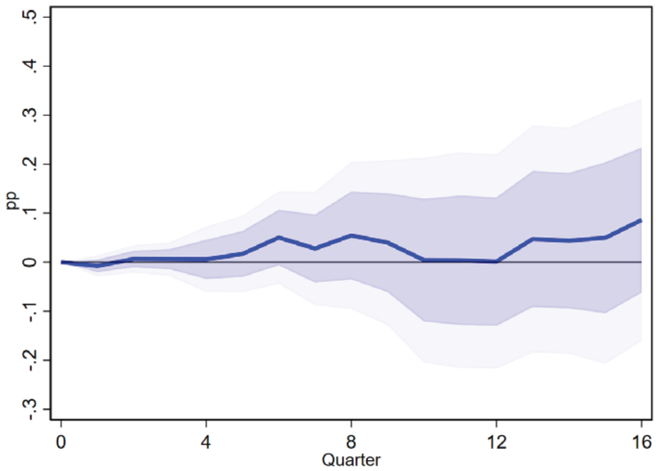
Notes: This table presents coefficient estimates reflecting the change in the τ-th percentile of annual average real output growth at horizon $h = 4, 8, 12$ and 16, following a tightening macroprudential policy activation. Coefficient estimates of fixed effects and controls not reported. Sample period is 1990Q1-2017Q4. Standard errors are based on block-bootstrapping with 1000 replications and show in parenthesis. ^ $p < 0.32$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure C.1: **Lags in Policy Implementation**. Impulse Response of Quantiles of the GDP-Growth Distribution to Macroprudential Policy Tightenings.

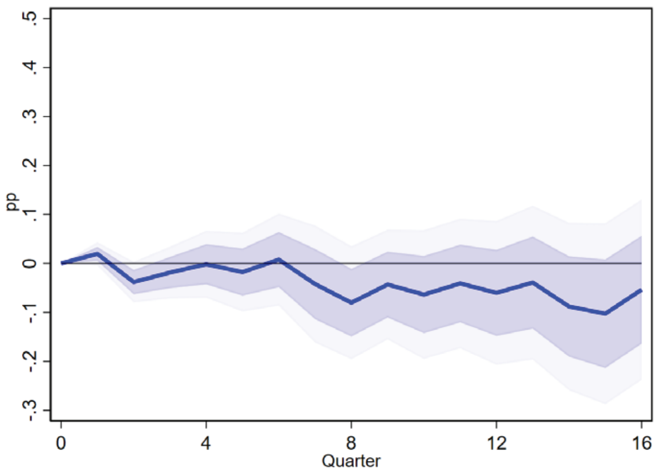
Panel (a): 10th Percentile



Panel (b): Median



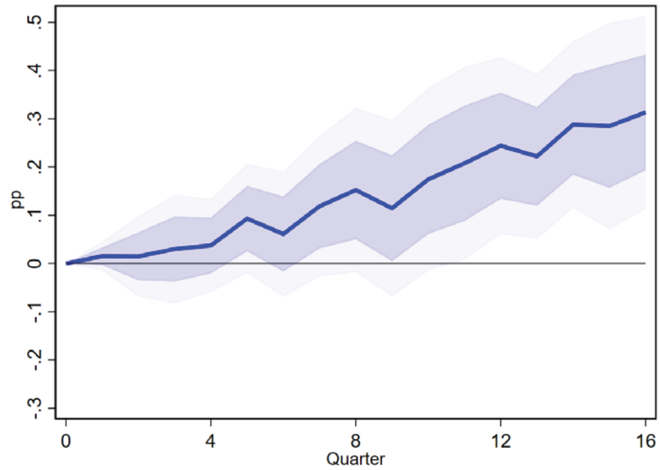
Panel (c): 90th Percentile



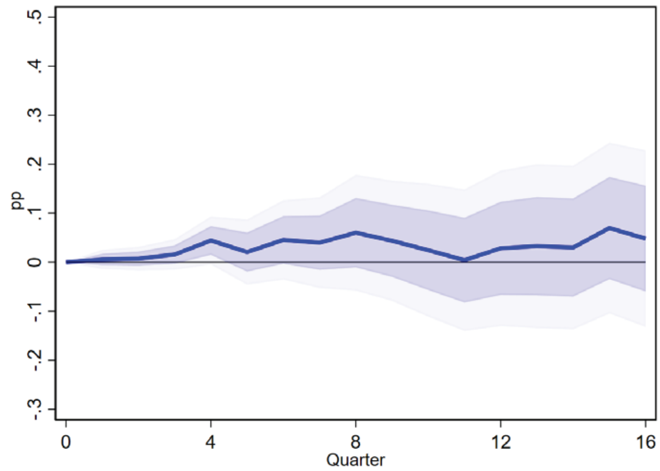
Notes: Excluding policies with implementation lag according to the 90 days threshold of Mertens and Ravn (2012). Estimated change in the τ-th percentile of annual average real GDP growth at horizon $h = 1, 2, ..., 16$, following a tightening macroprudential policy activation. Sample period is 1990Q1-2017Q4. Shaded areas denote the 90% (light blue) and 68% (dark blue) confidence intervals based on block-bootstrapping with 1000 replications.

Figure C.2: **Accounting for expectations**. Impulse Response of Quantiles of the GDP-Growth Distribution to Macroprudential Policy Tightenings.

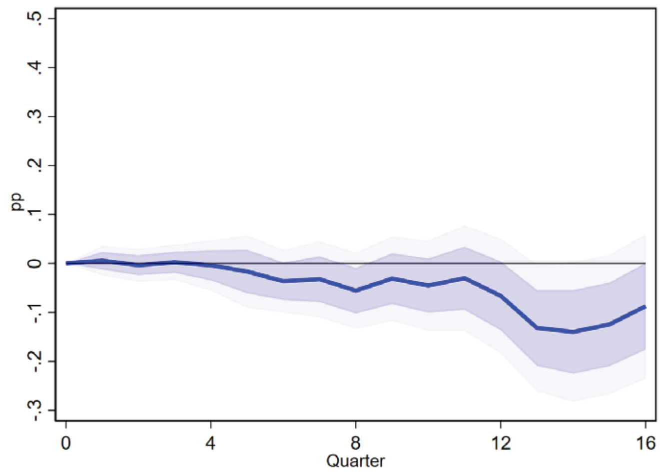
Panel (a): 10th Percentile



Panel (b): Median



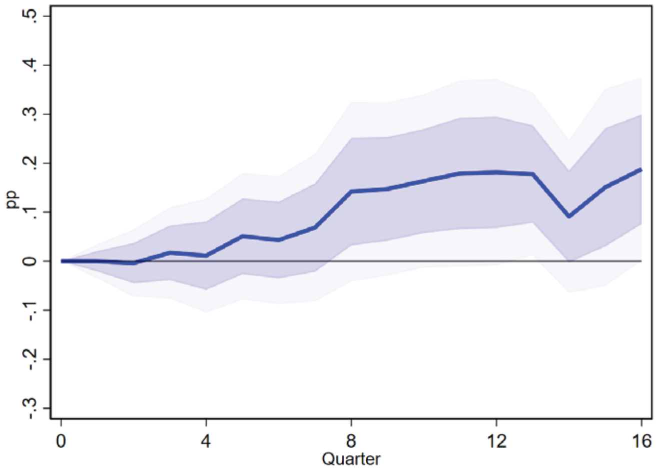
Panel (c): 90th Percentile



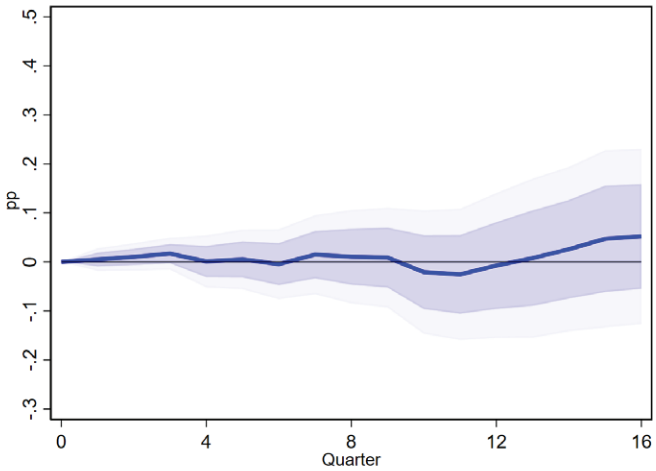
Notes: GDP growth forecast as an additional control. Estimated change in the τ-th percentile of annual average real GDP growth at horizon $h = 1, 2, ..., 16$, following a tightening macroprudential policy activation. Sample period is 1990Q1-2017Q4. Shaded areas denote the 90% (light blue) and 68% (dark blue) confidence intervals based on block-bootstrapping with 1000 replications.

Figure C.3: **Alternative Controls: Financial Conditions Index (FCI)**. Impulse Response of Quantiles of the GDP-Growth Distribution to Macroprudential Policy Tightenings.

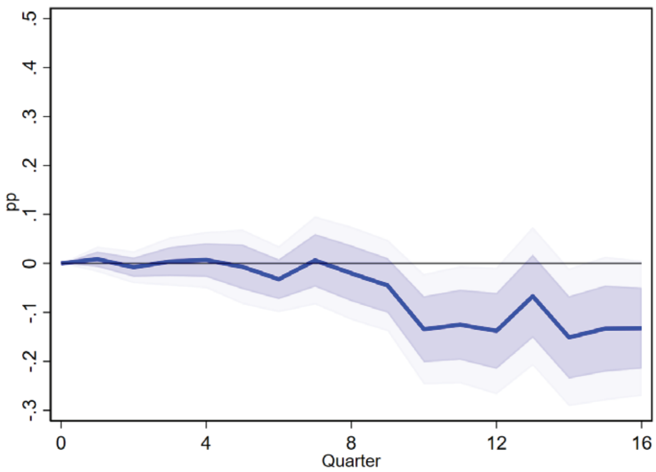
Panel (a): 10th Percentile



Panel (b): Median



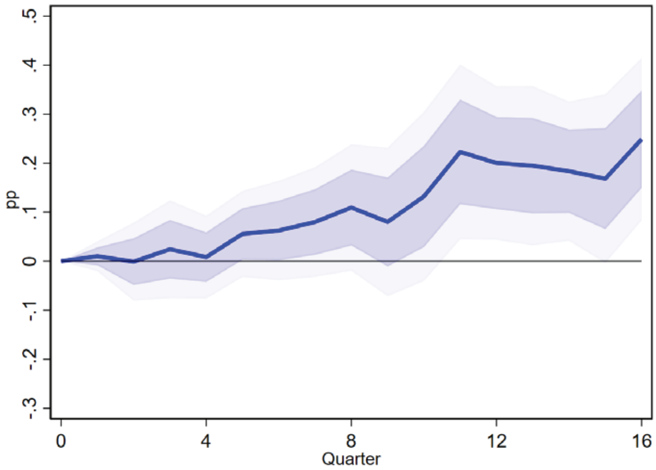
Panel (c): 90th Percentile



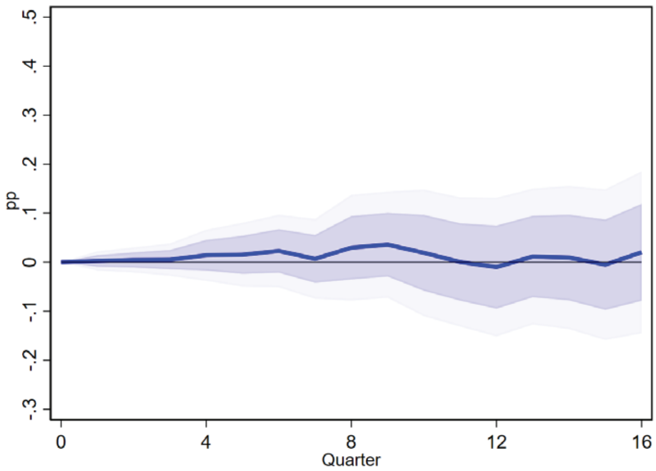
Notes: Control-augmented quantile local projections: Financial Condition Index (FCI). Estimated change in the τ-th percentile of annual average real GDP growth at horizon $h = 1, 2, ..., 16$, following a tightening macroprudential policy activation. Sample period is 1990Q1-2017Q4. Shaded areas denote the 90% (light blue) and 68% (dark blue) confidence intervals based on block-bootstrapping with 1000 replications.

Figure C.4: **Alternative Controls: Monetary Policy**. Impulse Response of Quantiles of the GDP-Growth Distribution to Macroprudential Policy Tightenings.

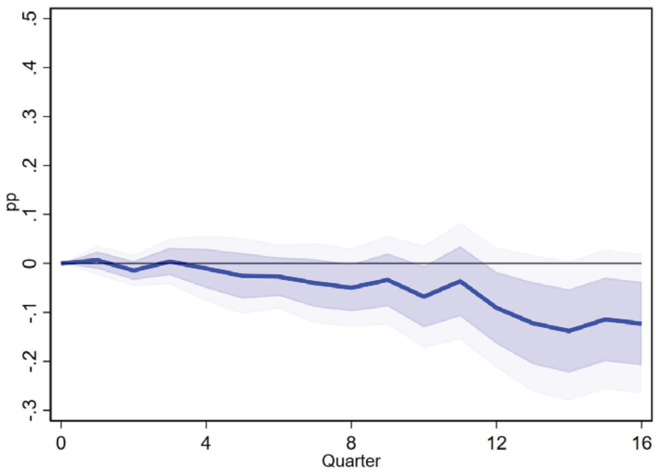
Panel (a): 10th Percentile



Panel (b): Median



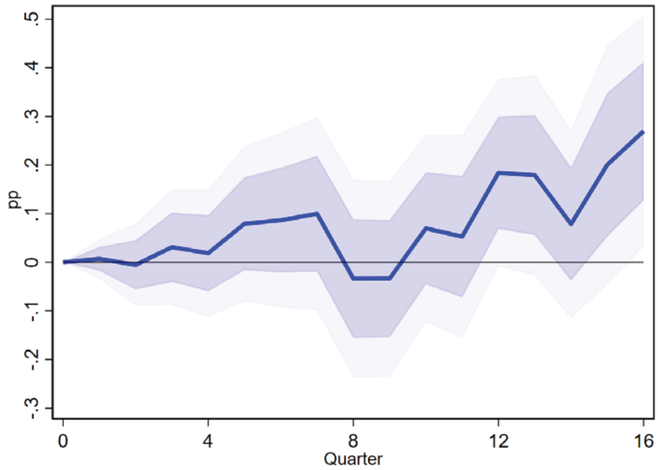
Panel (c): 90th Percentile



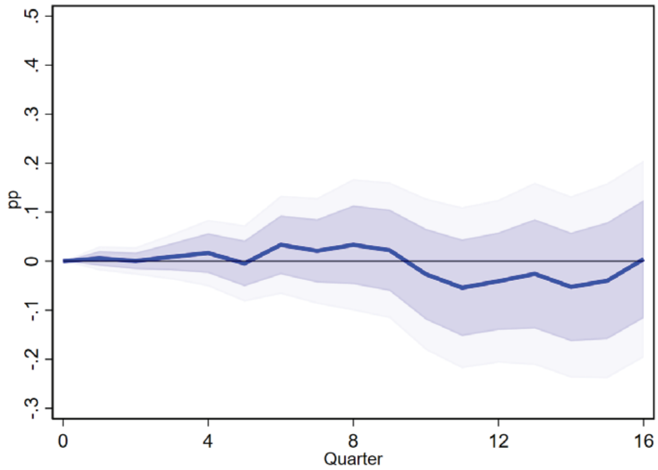
Notes: Control-augmented quantile local projections: Short-term interest rate. Estimated change in the τ-th percentile of annual average real GDP growth at horizon $h = 1, 2, ..., 16$, following a tightening macroprudential policy activation. Sample period is 1990Q1-2017Q4. Shaded areas denote the 90% (light blue) and 68% (dark blue) confidence intervals based on block-bootstrapping with 1000 replications.

Figure C.5: **Alternative Macroprudential Policy Index**. Impulse Response of Quantiles of the GDP-Growth Distribution to Macroprudential Policy Tightenings.

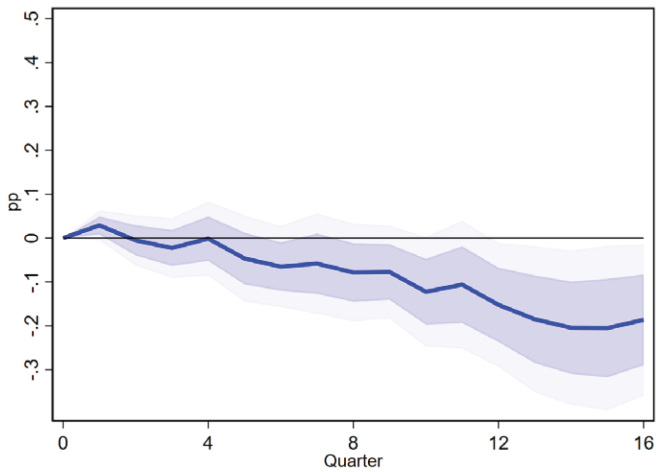
Panel (a): 10th Percentile



Panel (b): Median



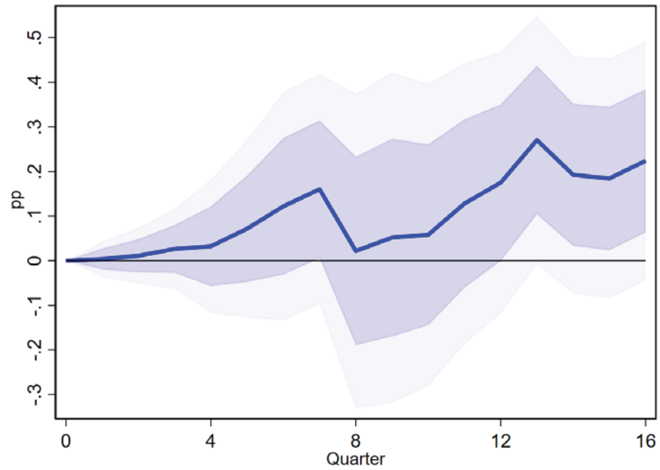
Panel (c): 90th Percentile



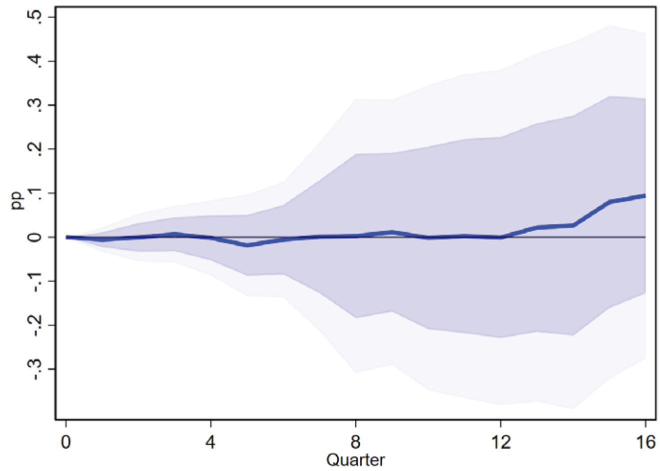
Notes: Alternative Macroprudential Policy Index. Estimated change in the τ-th percentile of annual average real GDP growth at horizon $h = 1, 2, ..., 16$, following a tightening macroprudential policy activation. Sample period is 1990Q1-2017Q4. Shaded areas denote the 90% (light blue) and 68% (dark blue) confidence intervals based on block-bootstrapping with 1000 replications.

Figure C.6: **Subsample: Excluding the Great Financial Crisis (GFC)**. Impulse Response of Quantiles of the GDP-Growth Distribution to Macroprudential Policy Tightenings.

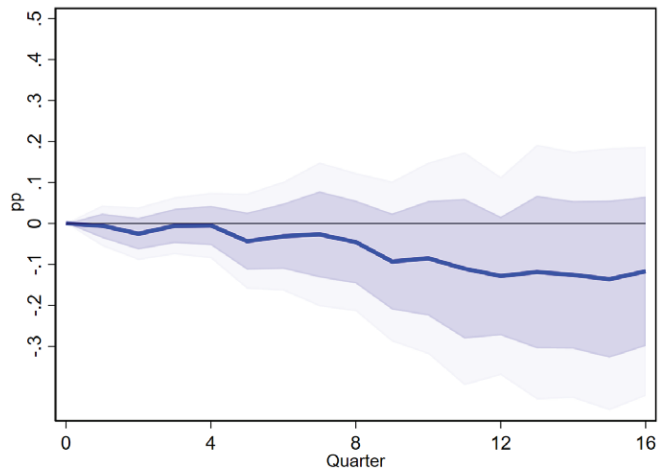
Panel (a): 10th Percentile



Panel (b): Median

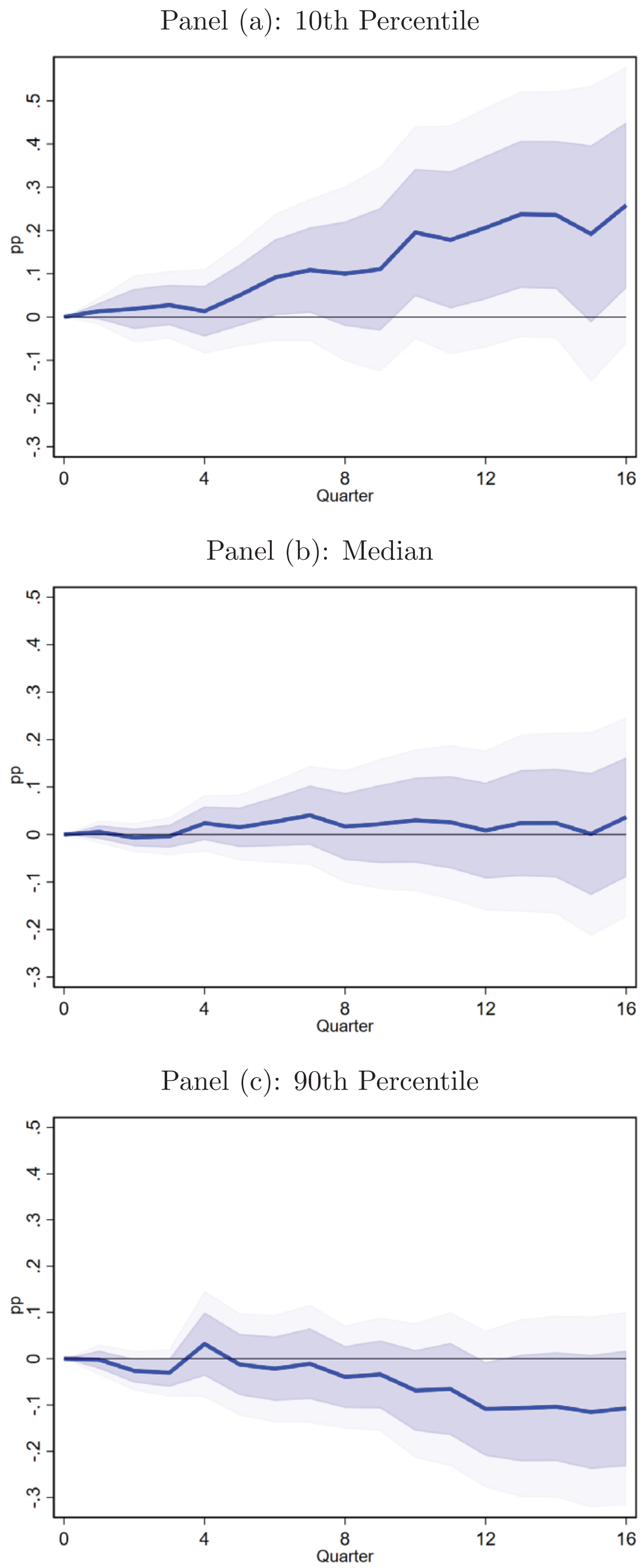


Panel (c): 90th Percentile



Notes: Subsample: Excluding the Great Financial Crisis (GFC). Estimated change in the τ-th percentile of annual average real GDP growth at horizon $h = 1, 2, ..., 16$, following a tightening macroprudential policy activation. Sample period is 1985Q1-2017Q4. Shaded areas denote the 90% (light blue) and 68% (dark blue) confidence intervals based on block-bootstrapping with 1000 replications.

Figure C.7: **Alternative country fixed-effects**. Impulse Response of Quantiles of the GDP-Growth Distribution to Macroprudential Policy Tightenings.



Notes: Machado and Santos Silva (2019) country fixed-effects. Estimated change in the τ-th percentile of annual average real GDP growth at horizon $h = 1, 2, ..., 16$, following a tightening macroprudential policy activation. Sample period is 1990Q1-2017Q4. Shaded areas denote the 90% (light blue) and 68% (dark blue) confidence intervals based on block-bootstrapping with 1000 replications.

D Additional Results

D.1 Financial Conditions

Financial conditions have been shown to play an important role in explaining observed growth vulnerability dynamics (Adrian et al., 2019, 2022). We therefore study whether the financial conditions channel can be a driver of the positive effects that macroprudential policy exercises on the tails of the GDP-growth distribution. To do so, we follow Adrian et al. (2019, 2022) and use a domestic financial condition index (FCI) to measure financial conditions in the economy.[27] We then explore the extent to which macroprudential policy has an asymmetric impact on future financial conditions quantiles using local projections. We therefore now use the annual change in the FCI as our dependent variable in this specification, i.e., $\Delta y_{i,t+h}$ is the annual change in the FCI over h quarters.

Coefficient estimates from the estimated impulse response and standard errors are shown in Table D.2.1, and Figure D.2.1. We focus again on the 10th, 50th and 90th percentiles. We find there is a drop in the FCI (a loosening of financial conditions) over the medium-term in response to a tightening macroprudential policy shock. This effect appears most pronounced in the right-tail (at the 2− and 3−year horizon particularly), pointing to a role for tighter macroprudential policy in reducing the probability and severity of a sharp tightening in financial conditions in the medium-term.

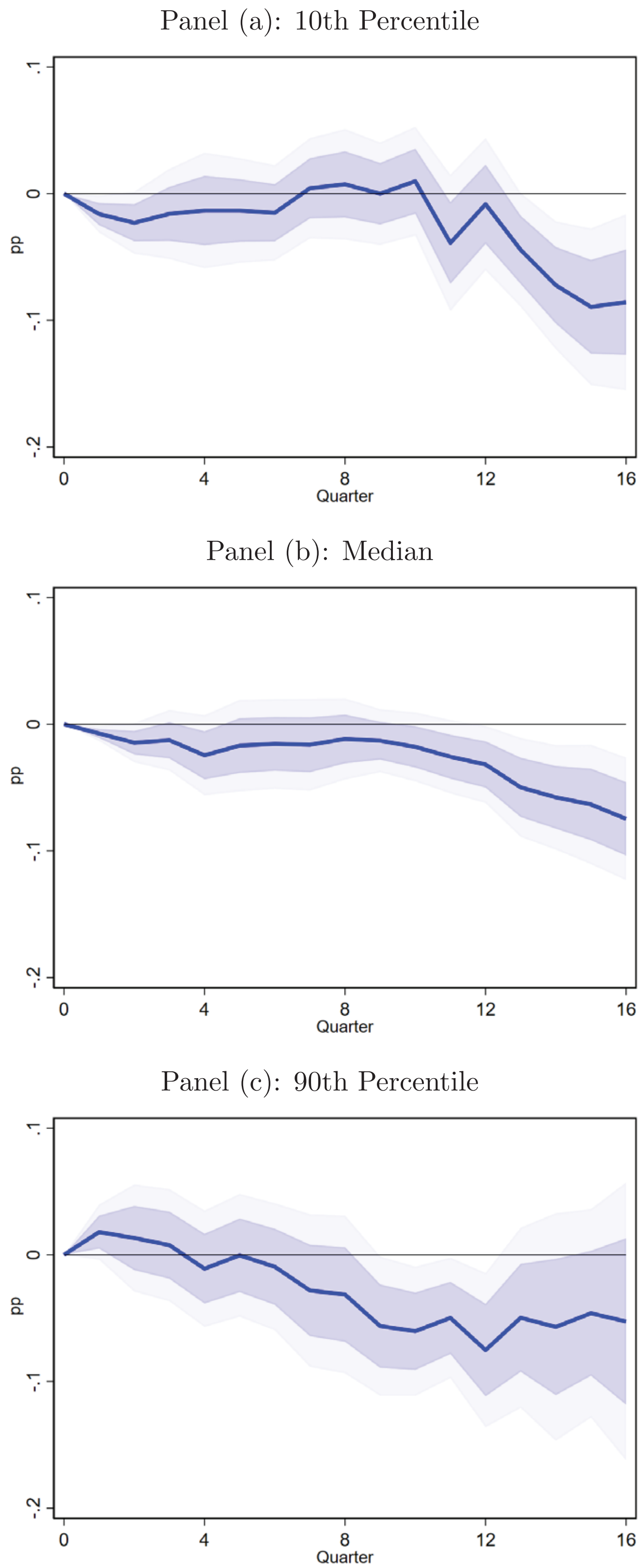
Table D.2.1: Coefficient estimates for $\beta^h(\tau)$ from regression of financial conditions change on narrative measure of macroprudential policy and controls

	$h = 4$	$h = 8$	$h = 12$	$h = 16$
PANEL A: Quantile-Regression Estimates				
$\tau = 0.1$	-0.01	0.01	-0.01	-0.09**
	(0.03)	(0.03)	(0.03)	(0.04)
$\tau = 0.5$	-0.02ˆ	-0.01	-0.03**	-0.07***
	(0.02)	(0.02)	(0.02)	(0.03)
$\tau = 0.9$	-0.01	-0.03	-0.08**	-0.05
	(0.03)	(0.04)	(0.04)	(0.07)
PANEL B: OLS-Regression Estimates				
OLS	-0.02ˆ	0.00	-0.01ˆ	-0.04***
	(0.02)	(0.01)	(0.01)	(0.01)

Notes: Panel A presents coefficient estimates for the causal effects of a 1-unit tightening in macroprudential policy on the τ-th percentile of annual average financial conditions change at horizon $h = 4, 8, 12, 16$. Panel B presents corresponding estimates from OLS regressions. Coefficient estimates for the fixed effects and controls are not reported. Sample period is 1990Q1-2017Q4, for 12 advanced economies. Standard errors are based on block-bootstrapping with 1000 replications and are shown in parenthesis with: ˆ $p < 0.32$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[27] The financial conditions index provides a weekly estimate of domestic financial conditions in money markets, debt and equity markets, and the traditional and shadow banking systems. Adrian, Boyarchenko, and Giannone (2019) show that the conditional quantile function is more sensitive to the overall FCI than other standard measures of financial conditions such as equity volatility, term spread or credit spread.

Figure D.2.1: Impulse Response of Quantiles of the Financial Conditions Index (FCI) Distribution to Macroprudential Policy Tightenings



Notes: Estimated change in the τ-th percentile of annual average financial conditions change at horizon $h = 1, 2, ..., 16$, following a tightening macroprudential policy activation. Sample period is 1990Q1-2017Q4. Shaded areas denote the 90% (light blue) and 68% (dark blue) confidence intervals based on block-bootstrapping with 1000 replications.

D.2 Heterogeneity: Lender- and Borrower-Based Macroprudential Policy

Table D.3.1 presents the dynamic response of GDP-growth to lender- and borrower-based macroprudential policy tightenings, from Section **??**, in tabular form.

Table D.3.1: Coefficient estimates $\beta^h(\tau)$ from baseline specification: regression of GDP growth on lender- and borrower-based narrative measure macroprudential policy and controls

	$h = 4$	$h = 8$	$h = 12$	$h = 16$
PANEL A: Quantile-Regression Estimates for Lender-based Macroprudential Policy				
$\tau = 0.1$	0.02	0.13ˆ	0.22ˆ	0.31**
	(0.07)	(0.13)	(0.14)	(0.15)
$\tau = 0.5$	0.02	0.06	0.03	0.09
	(0.03)	(0.07)	(0.10)	(0.11)
$\tau = 0.9$	-0.00	-0.05ˆ	-0.06	-0.09ˆ
	(0.03)	(0.04)	(0.06)	(0.09)
PANEL B: Quantile-Regression Estimates for Borrower-based Macroprudential Policy				
$\tau = 0.1$	-0.00	-0.13	0.32	0.26
	(0.32)	(0.42)	(0.47)	(0.53)
$\tau = 0.5$	-0.20**	-0.22	-0.28	-0.30
	(0.10)	(0.25)	(0.30)	(0.37)
$\tau = 0.9$	-0.26ˆ	-0.60ˆ	-0.69ˆ	-0.85ˆ
	(0.19)	(0.42)	(0.50)	(0.68)

Notes: This table presents coefficient estimates for the causal effects of a 1-unit tightening in lender- and borrower-based macroprudential policy on the τ-th percentile of annual average real GDP growth at horizon $h = 4, 8, 12, 16$. Coefficient estimates for the fixed effects and controls are not reported. Sample period is 1990Q1-2017Q4, for 12 advanced economies. Standard errors are based on block-bootstrapping with 1000 replications and are shown in parenthesis with: ˆ $p < 0.32$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

D.3 House Prices

Table D.4.1 presents the results for house prices, from Section 4.3, in tabular form.

Table D.4.1: Coefficient estimates for $\beta^h(\tau)$ from regression of house-price growth on narrative measure of macroprudential policy and controls

	$h = 4$	$h = 8$	$h = 12$	$h = 16$
PANEL A: Quantile-Regression Estimates				
$\tau = 0.1$	-0.11	-0.11	-0.15	-0.02
	(0.11)	(0.22)	(0.28)	(0.31)
$\tau = 0.5$	-0.08	-0.24ˆ	-0.31	-0.18
	(0.09)	(0.22)	(0.34)	(0.46)
$\tau = 0.9$	0.03	0.05	-0.05	-0.06
	(0.12)	(0.19)	(0.28)	(0.33)
PANEL B: OLS-Regression Estimates				
OLS	-0.04	-0.16	-0.20	-0.18
	(0.09)	(0.18)	(0.25)	(0.32)

Notes: Panel A presents coefficient estimates for the causal effects of a 1-unit tightening in macroprudential policy on the τ-th percentile of annual average real house-price growth at horizon $h = 4, 8, 12, 16$. Panel B presents corresponding estimates from OLS regressions. Coefficient estimates for the fixed effects and controls are not reported. Sample period is 1990Q1-2017Q4, for 12 advanced economies. Standard errors are based on block-bootstrapping with 1000 replications and are shown in parenthesis with: ˆ $p < 0.32$, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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