# FIVE ESSAYS ON MONETARY AND MACROPRUDENTIAL POLICY, LENDING STANDARDS AND LOCKDOWN SHOCKS


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# Preface

This doctoral thesis analyses the economic and political consequences of the three large crises that shaped the European economy over the last fifteen years. The global financial crisis of 2007/08 that was initiated by the burst of the US housing market bubble and amplified by the insolvency of the US bank Lehman Brothers, resulted in the so-called Great Recession. Although the financial crisis began in the US, its impact on other economies, including European markets, was extensive, in particular, as it created a high level of uncertainty. Consequently, creditors reevaluated borrowers' solvency. In this reevaluation process, agents also questioned the solvency of the sovereign bond market in some European countries leading to the so-called European sovereign debt crisis, which marks the second crisis over the last 15 years. Market participants had to adjust their assessment of the probability of default, in particular, for those countries with high debt levels and weak economic fundamentals, namely Greece, Ireland, Italy, Portugal and Spain, resulting in higher yields which again led to higher debt burdens. Hence, the global financial crisis and the subsequent European sovereign debt crisis are interconnected and they are both the result of market failures. In contrast to that, the COVID-crisis, which was the latest crisis, can be seen as a shock outside the financial system. Nevertheless, the role of the financial markets and fiscal and monetary policy measures during the crisis is of interest as these could absorb or amplify the shock to some degree.

Academia put the economic crises center stage as they generate high economic costs and, more importantly, crises could unveil structural problems that cause or amplify economic shocks. Put differently, understanding the mechanisms behind crises allows developing new policy measures to prevent or mitigate future financial crises.

National and EU authorities as well as monetary policymakers responded quickly to all three crises with the policy tools they have had at hand. For example, the ECB lowered the main refinancing operations rate from 4.25% in 2007 to 1% in 2009 and switched from variable rate tenders to fixed rate tender procedures with full allotment after the global financial crisis. As the sovereign debt crisis unfold, the ECB's policy toolkit was already constrained by the zero lower bound. Consequently, the ECB implemented so-called unconventional monetary policy measures such as purchases of debt securities to support the economy further. Similarly, the ECB introduced a temporary asset purchase programme of private and public sector securities after the outbreak of the COVID-crisis. Fiscal policymakers were responding via an increase in government spending after the global financial crisis and the Covid-crisis. However, these policy responses mainly aim at minimizing the economic damage of adverse shocks, i.e. they "clean up" after a financial crisis.

In addition to that, new policy measures were introduced that should safeguard the financial system so that the likelihood of future financial crises decreases in the long-run. Specifically, as a response to the global financial crisis, the role of prudential policy measures was strengthened on a larger scale, e.g. the Basel Accords were amended. The European sovereign debt crisis led to the establishment of the European Stability Mechanism.

In this vein, this doctoral thesis adds to the discussion on the lessons learned from the crises. Hence, it empirically evaluates the effectiveness of monetary policy and (macro)prudential measures in the euro area. Consequently, this thesis analyses both, the immediate response of policymakers to mitigate the crises' impact and the structural policy changes. Additionally, it investigates some of the key mechanisms behind the crises. Specifically, it shows how various forms of (market) uncertainty reacted to an exacerbation of lockdown measures during the Covid-crisis in the US. Moreover, it analyses the macroeconomic consequences of credit supply and bank risk taking shocks, which is of substantial interest as lax credit standards have contributed to the environment leading to the 2007/08 great financial crisis.

In total, this thesis consists of five studies that can be grouped into analyses focusing on bank lending, monetary policy and the identification of lockdown shocks. They are briefly summarized as follows.

The two papers focusing on bank lending analyze the role of changes in lending standards and assess the impact of (macro)prudential policy measures, respectively. In the Federal Reserve's Senior Loan Officer Opinion Survey on Bank Lending Practices (SLOOS), banks' Senior Loan Officers are asked about the development of credit conditions over the last three months. The study on lending standards is the first that uses changes of financial market variables on the SLOOS release days as an indicator for unexpected shifts in credit conditions. More precisely, we run an instrumental variable local projection where the change of a corporate bond spread on the meeting day is the instrument. The rationale behind the use of the corporate bond spread is that bank lending and bonds are alternative funding sources for firms and, hence, can be seen as imperfect substitutes. We find that a drop in lending standards increases economic activity, prices and stock market prices, while the excess bond premium falls. However, lax lending standards may be the result of extensive credit supply or high bank risk taking. Our second contribution is that we can discern between these two sources. Specifically, we add a second instrument, namely the change of the VIX on the release days, and run a VAR model with sign restrictions on the instruments. The results show that while the VIX, the excess bond premium and stock prices decrease after a credit supply shock, they increase after a risk-taking shock.

The study on (macro)prudential policies empirically quantifies the average effect of a tightening in the prudential policy stance on the development of the total amount of loans and rents in euro area countries. However, estimating these effects is difficult due to endogeneity issues, e.g. policymakers tighten macroprudential measures during a credit boom. In contrast to the literature, we rely on a propensity score matching approach that is particularly well suited to mitigate this endogeneity issue. Moreover, we apply an approach that generates impulse-response functions. We find that tighter prudential policies decrease lending on average. Taking a more granular perspective, we observe that the decrease in lending is more pronounced when policymakers have not communicated the implementation of measures before and EU institutions rather than national authorities initiated the change in the prudential policy stance. In contrast to that, we find no significant response of rents to changes in the prudential policy stance.

This thesis includes two studies that estimate the impact of monetary policy empirically. The first paper looks at the monetary policy transmission. It analyzes euro area-wide as well as country-specific responses to (unconventional) monetary policy shocks. More precisely, we first estimate an external instrument Vector autoregression (VAR) model where changes of the German bunds at meeting days of the Governing Council and selected inter-meeting announcements serve as the instrument. The underlying assumption is that on ECB announcement days, the instrument reflects only the policy surprise, i.e. all other news are white noise. We find that an expansionary monetary policy increases consumer prices and real activity and compresses the corporate bond spread and the real exchange rate. However, the shock remains ineffective in pushing credit and stock market variables significantly. We then plug the monetary policy shock from the VAR into country-specific local projections to evaluate the heterogeneity in the transmission process across euro area countries. We observe that, in particular, the response of unemployment, credit and the stock market vary considerably across euro area countries and, hence, conclude that the transmission through equity prices and through the banking system is severely dampened in some countries.

The second paper on monetary policy analyses its distributional consequences. Specifically, it investigates how the income distribution changes in response to a monetary policy shock. We focus on six countries, three with a relatively low degree of income inequality (Sweden, Czech Republic and Hungary) as measured by the Gini coefficient and three with a relatively high degree of income inequality (US, Canada and South Korea). We observe that income inequality before tax and transfers increases in response to an expansionary monetary policy shock in all six countries. However, net income inequality only increases in those countries with a relatively high degree of inequality. Hence, we conclude that tax and redistribution policies can dampen the distributional impact of monetary policy. Furthermore, we investigate the channels through which monetary policy affects inequality. We find that inequality rises after an expansionary monetary policy shock because capital owners profit disproportionally relative.

The last paper of this thesis puts the identification of lockdown shocks during the COVID-crisis center stage. It uses a daily VAR identified with sign restrictions to disentangle a lockdown shock from a real business cycle shock. With this identification strategy at hand, we investigate how various forms of (market) uncertainty react after a lockdown shock. We find that lockdown shocks do not create more uncertainty than contractionary real business cycle shocks.

For various reasons, I would like to take the opportunity to thank my primary supervisor Prof. Dr. Peter Tillmann. First, his lectures have sparked my interest in monetary policy, macroeconomics and econometrics. Second, his constant fruitful feedback had an enormous positive impact on each of the five papers. Third, his empathetic way of supporting me whenever I struggled and his way of leading the entire team at his chair were import factors behind this thesis. Finally, he is a co-author in two papers. Hence, it is not conceivable that this outcome could have been achieved without his courageous support. I would also like to thank my secondary supervisor Prof. Dr. Peter Winker for his highly valuable feedback. In particular, his support in selecting appropriate empirical approaches to the research question was fundamental.

A great thank also goes to my colleagues Dr. Annette Meinusch, Dr. Immaculate Machasio, Dr. Jörg Schmidt, Paul Rudel and David Finck. Various discussions during brown bag sessions or via the office grapevine contributed substantially. Last but not least, I would like to thank our secretary Cornelia Strack and our student assistants Sinem Kandemir, Anisa Tiza-Mimun, Florian Viereck, Salah Hassanin, Omar Omari, Niklas Benner and Moritz Grebe.

I dedicate this doctoral thesis to my girlfriend Katharina Plutz and my family,

namely my parents Regina and Wolfgang and my brother Normen. Thank you for your mental support.

This thesis is structured as follows. While Section 2 analyzes the impact of changes in lending standards, Section 3 estimates the average treatment effect of a tightening in the macroprudential policy stance for eurozone countries. Sections 4 and 5 include the two studies on monetary policy. We begin with the study on the monetary policy transmission in the euro area and continue with the paper that analyzes the consequences of monetary policy changes for income inequality. Finally, Section 6 concludes.

# Contents

# 1 Lending Standards and the Business Cycle: Evidence from Loan Survey Releases

This paper is available under[1]

Hafemann, L., and Tillmann, P. (2021): 'Lending Standards and the Business Cycle: Evidence from Loan Survey Releases', *MAGKS Joint Discussion Paper Series in Economics* No. 31-2021.
https://www.uni-marburg.de/en/fb02/research-groups/economics/macroeconomics/research/magks-joint-discussion-papers-in-economics/papers/2021-papers/31-2021_hafemann.pdf

[1]We thank David Finck for sharing his MATLAB toolbox for local projections.

# Lending Standards and the Business Cycle: Evidence from Loan Survey Releases

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The Fed's Senior Loan Officer Opinion Survey (SLOOS) is widely considered a good indicator of banks' lending conditions. We use the change in corporate bond spreads on SLOOS release days to instrument changes in lending standards. A series of estimated IV local projections shows that lending standards have highly significant effects on macroeconomic and financial variables. A relaxation of standards expands economic activity and eases financial conditions. We then use the change in spreads and the change in the VIX index on release days to identify a pure credit supply shock and a risk-taking shock using sign restrictions in a Bayesian VAR model. We find that an easing in lending has different consequences for both types of shocks. While the VIX, the excess bond premium and stock prices decrease after a pure credit supply shock, they increase after a risk-taking shock.

**Keywords:** loan survey, credit supply, risk-taking, instrumental variable local projections, shock identification.

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## 1.1 Introduction

Banks play a major role in firms' financing decisions. This is true even in countries such as the US, where market finance dominates bank finance. As a consequence, changes in banks' credit conditions drive economic activity and financial markets. One key element of credit conditions are the standards banks apply when extending or curtailing credit to firms. A large literature to be surveyed below estimates the economic and financial effects of exogenous changes to banks' lending standards using the lending standards banks self-report in surveys such as the Fed's Senior Loan Officer Opinion Survey (SLOOS).

The challenge for empirical work is how to identify exogenous changes to lending standards. As a matter of fact, standards are endogenous and reflect aggregate economic conditions, competition in the banking sector and bank-specific characteristics. Many researchers estimate vector autoregression (VAR) models comprising lending standards and standard business cycle variables and impose restrictions on the contemporaneous interaction of the variables, e.g. Basset et al. (2014). The drawback of this approach is that the restrictions are relatively ad hoc and they already predetermine some of the model's results.

In this paper, we revisit the estimation of the effect of changes in lending standards and make two contributions. First, we introduce a novel identification strategy. The release of the results of the SLOOS prompts a market response. Corporate bond yields change when the lending standards reported in the survey are surprisingly lax or tight, respectively. We use the change in the spread between interest rates on low-quality and AAA-rated corporate bonds on SLOOS release days as an exogenous instrument. The assumption is that the change in the corporate bond spread is not systematically affected by other news on SLOOS release days.

The economic rationale for using changes in corporate spreads is as follows. Suppose banks tighten standards. If the demand for credit is unchanged, firms substitute bank lending with bond financing and turn to the corporate bond market, see Becker and Ivashina (2014), and Kashyap et al. (1993). The yields on corporate bonds increase. This bank-bond substitution is more difficult for firms in weak financial conditions, for which access to the bond market is strenuous and external financing is particularly expensive, see Bell and Young (2010). This is why yields on low-rated bonds should rise more than yields on AAA-rated bonds. As a result, the corporate credit spread widens.

We estimate a series of local projections a la Jordà (2005) for financial and business cycle variables and use the response of spreads in order to instrument changes in lending standards. Thus, we estimate instrumental variables (IV) local projections

as in Stock and Watson (2018). The advantage of this approach is that the identification is relatively light on assumptions. In particular, we do not need to impose an ordering onto the variables or any restrictions on the signs and the magnitudes of the responses such as in VAR models.
Second, we acknowledge that changes in lending standards as reported in the survey can be decomposed into two alternative structural shocks. The spread instrument introduced before elicits the responses to changes in standards as such but is unable to help us differentiate between these two underlying driving forces. Suppose banks report an increase in standards. One way to interpret the higher standards is as an adverse credit supply shock: banks curtail the amount of lending for a given willingness to accept a certain exposure to risk. An alternative interpretation is a drop in the bank's willingness to accept risk for a given loan volume. Our second contribution is a decomposition of lending standards into these two alternative shocks. We draw on the work of Jarocinski and Karadi (2020) and use a second instrument, the change in the VIX index on SLOOS release days, besides the change in spreads on release days. We estimate a Bayesian VAR model, in which imposing sign restrictions on the instruments allows us to disentangle both shocks.
We argue that a risk-taking shock narrows the spread on release days and increases the VIX as banks are willing to increase their risk exposure when making their lending decision. A pure credit supply shock, in contrast, also leads to a narrowing of the corporate bond spread on release days but reduces the VIX. As financial conditions ease, the fear of financial stress abates. Hence, a pure credit supply shock describes an expansion of credit for unchanged risk preferences, while the risk-taking shock is a credit expansion that goes hand in hand with more risk-taking. While the macroeconomic effects of both shocks might be similar, the consequences for financial stability are not. In particular, the risk-taking shock contributes to a build-up of financial risk and instability.
We find that a relaxation of lending standards has strong and highly significant effects on macroeconomic and financial variables. A drop of one percentage point in the net percentage share of banks tightening their standards increases industrial production and consumer prices by 0.1%. The excess bond premium of Gilchrist and Zakrajsek (2012) falls by five basis points and the S&P 500 stock market index increases by 0.4%. These are economically sizable effects. The demand for credit, which is also elicited in the loan survey, remains unaffected. This supports the notion that the estimated effects are driven by the supply rather than the demand for credit.
The pure credit supply shock and the risk-taking shock, which we obtain from

the estimated Bayesian VAR model with the two external instruments, both cause an easing of financial conditions. Credit conditions as reflected in the Chicago Fed Financial Conditions index improve, lending standards fall and spreads narrow. The stock market indices (the VIX index) decrease (increases) after a risk-taking shock and improve (declines) after an expansionary credit supply shock. The Fed tightens monetary conditions after an expansionary pure credit supply shock, but not after a risk-taking shock.

This paper combines lending standards with news announcements and, hence, relates to both strands of the literature. Let us briefly highlight the relationship to either branch. Lown and Morgan (2006) investigate the nexus between macroeconomic variables and changes in lending standards according to the SLOOS. They observe that tighter lending standards negatively correlate with commercial loan growth and real activity. To account for the possible endogeneity of these variables, they estimate a six variables VAR model identified via a Cholesky decomposition. They find that shocks to lending standards affect lending and output and that a positive aggregate loan shock leads to tighter standards. The role of credit supply and lending standards gained momentum after the global financial crisis. Building on granular bank-level information from the SLOOS, Basset et al. (2014) develop a credit supply indicator that is free of macroeconomic factors and bank-specific characteristics. They estimate a standard VAR model where the credit supply indicator, real GDP, core lending capacities of banks[1], inflation and the credit spread are endogenous variables. They identify the VAR via a recursive ordering and find that credit supply shocks significantly impact all variables.

Following Basset et al. (2014), Altavilla, Darracq-Pariès and Nicoletti (2015) also construct a credit tightening indicator that is not contaminated by the prevailing credit demand conditions from the Bank Lending Survey for the euro area. Rather than estimating a VAR with the indicator as an endogenous variable, they use it as an external instrument in a VAR a la Stock and Watson (2012) and Mertens and Ravn (2013). Their analysis indicates that real activity and credit volumes drop and bank lending spreads widen after a credit tightening shock. Lucidi and Semmler (2020) rely on an instrument to disentangle the endogenous relationship between credit standards and the real economy. Specifically, they use rotations of external auditors within banks in the euro area an exogenous source of variation and find a significant impact of credit standards on real and financial variables.

A separate branch of the literature studies the role of banks' lending standards

[1]The lending capacity of banks is defined as the sum of outstanding core loans and the corresponding unused commitments.

for the credit channel and the risk-taking channel of the transmission of monetary policy. Ciccarelli, Maddaloni and Peydró (2015) use data from the SLOOS and the BLS to analyze the credit channel of monetary policy. They show that credit demand and supply amplify monetary policy shocks in the US and the euro area. Darracq-Paries and De Santis (2015) show that the ECB's long-term refinancing operations in 2011 and 2012 led to relaxed lending conditions. Similarly, Kurtzman et al. (2018) come to the conclusion that the first and third round of quantitative easing in the US significantly lowered lending standards. Buch et al. (2014) use the Federal Reserve's Survey of Terms of Business Lending to show that expansionary monetary policy increases the degree of bank risk-taking. Likewise, Paligorova and Santos (2017) employ bank-level information from the SLOOS and show that banks grant riskier loans when the Fed eases monetary policy.

The paper is also related to the vast literature on the responses of financial markets to news releases. Fleming and Remolona (1999), Gürkaynak et al. (2005a) and Altavilla et al. (2017) show that macroeconomic surprises can affect the entire term structure. Kuttner (2001) and Gürkaynak et al. (2005b) exploit changes on federal funds futures around FOMC announcements to unveil monetary policy shocks. In a similar vein, Känzig (2021) relies on oil futures prices around OPEC production announcements to identify oil supply news. A contractionary oil supply shock increases oil prices and inflation expectations but decreases oil and industrial production. Focusing on news related to lending, Mokas and Giuliodori (2021) analyze how announcements of loan-to-value restrictions impact EU economies. They find that announcements of tighter restrictions lead to a decrease in household credit and house prices. Patrella and Resti (2013), Flannery et al. (2017) and Fernandes et al. (2020) show that stress test releases affect returns for the stress-tested banks. Consequently, trading volumes increase on the disclosure dates. Building on that, Guerrieri and Modugno (2021) analyze whether this reaction stems from the immediate impact on capital distribution plans to investors, whose approval by the Fed is linked to the stress test results, or whether it is driven by the fact that stress test results unveil information about the ability of banks to withstand harsh economic conditions. They find that both transmission mechanisms are relevant.

As we identify lending standards shocks via changes in yields for corporate bonds across the rating spectrum, our paper is also related to the literature on credit spreads. Meeks (2012) provides evidence that changes in the lending spreads drive the macroeconomy. Gilchrist, Sim and Zakrajsek (2014) analyze the relationship between uncertainty, investments and credit spreads on corporate bonds within a structural VAR model and find that uncertainty shocks are to a large extend transmitted through credit spreads. Focusing on uncertainty of financial regulation policy, Nodari (2014) finds that for the US credit spreads widen in response to an increase in uncertainty.

The remainder of this paper is structured as follows. In Section two, we review the SLOOS and derive the instrument from release days. Section three introduces the local projection model and discusses the results. The decomposition in credit supply and risk-taking shocks is presented in Section four, while Section five draws conclusions.

## 1.2 Releases of the Fed's loan officer survey

In the Fed's Senior Loan Officer Opinion Survey on Bank Lending Practices (SLOOS), loan officers are asked about their assessment of lending conditions and credit demand for various loan categories. Specifically, they indicate whether they have eased or tightened standards in comparison to the previous quarter or whether they remain unchanged. Accordingly, for loan demand, they report whether loan demand was stronger, weaker or unchanged.[2]

The survey is conducted on a quarterly frequency since 1990. In total, the survey covers up to eighty large domestic banks and twenty-four US branches and agencies of foreign banks.[3] Banks can answer the survey in a time window of ten days. The window closes about four weeks prior to the release. The July 2019 survey, for example, was conducted between June 24 and July 5, 2019. The results were released on August 9, 2019. Members of the Federal Open Market Committee (FOMC) had the results available for their July 30/31 meeting.

The survey responses from the individual banks are not reported. Instead, the Fed provides market participants with aggregate estimates of lending conditions and demand across all banks. The release contains the so-called net percentage change. It is given by the share of banks that report a tightening of lending standards ("tightened considerably" or "tightened somewhat") minus the share of banks reporting an easing ("eased considerably" or "eased somewhat"). For credit

[2]The specific question is

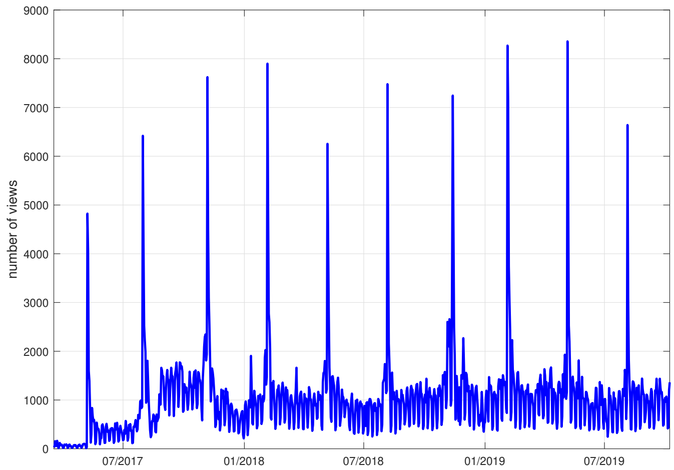
> "Over the past three months, how have your bank's credit standards for approving applications for C&I loans or credit lines - other than those to be used to finance mergers and acquisitions - to large and middle-market firms and to small firms changed?"

Respondents can choose among the following answers: tightened considerably, tightened somewhat, remained basically unchanged, eased somewhat and eased considerably.

[3]See the Fed website for details: https://www.federalreserve.gov/econres/notes/feds-notes/an-aggregate-view-of-bank-lending-standards-and-demand-20200504.html.

demand, it is the share of banks observing a stronger demand minus the share reporting a weaker demand.[4]

Figure 1.1: Views of "Data" section of Fed website




*Notes:* Daily number of views of the "Data" section of the Website of the Federal Reserve. The outliers correspond to release dates of the loan survey. Sample: March 18, 2017 to October 8, 2019

Starting with Lown and Morgan (2006), academics use the SLOOS results to understand the tightness of credit markets. Moreover, the release of the results from the loan survey receives a lot of attention from market participants and the media. To illustrate the public's interest in the survey results, Figure (1.1) shows the daily number of views of the "Data" section of the Fed's website between 2017 and October 2019.[5] This section contains the detailed set of survey results. Importantly, the SLOOS release days are clearly visible as extreme outliers in the series. On release days, the "Data section" receives between 5,000 and 8,000 views, while the number of views fluctuates between 1,000 and 2,000 on normal days. The huge interest the loan survey receives motivates us to exploit the market response on the release days. Changes in loan conditions, with the evolution of banks' lending standards being center stage, should contain information on the future path of the macroeconomy and financial markets. Consequently, market participants update their assessment of the credit market when the SLOOS is released.

For a given loan demand, a change in lending standards shifts loan supply. Suppose banks tighten credit standards. As demand for credit is unchanged, firms substitute

[4]In the aggregation process, the individual bank responses are typically unweighted. However, net percentage changes that are weighted by banks' holdings of the relevant loan category are also available.

[5]See Tillmann (2021) for details on this data set.

bank lending with bond financing and turn to the corporate bond market, see Becker and Ivashina (2014) and Kashyap et al. (1993). Hence, the supply of bonds increases and their prices fall. The yield on corporate bonds increases. This substitution is more expansive for firms in weak financial conditions, for which access to the bond market is strenuous, see Bell and Young (2010). This is why yields on BAA-rated bonds should rise more than yields on AAA-rated bonds. As a result, the credit spread widens.

To the extent the changes in lending conditions come as a surprise, they should prompt an adjustment of credit spreads on the release day. Therefore, we draw information contained in the response of spreads on release days in order to construct an instrument for changes in credit standards.

### 1.2.1 Constructing our instrument for lending standard changes

We collect the release days from the individual survey releases (before 2010) and from ALFRED (since 2010). Table (1.7) lists the release dates considered. For each release day, we construct the change in the spread between BAA- and AAA-rated corporate bonds relative to the day before the release. Hence, our daily series of surprise changes is

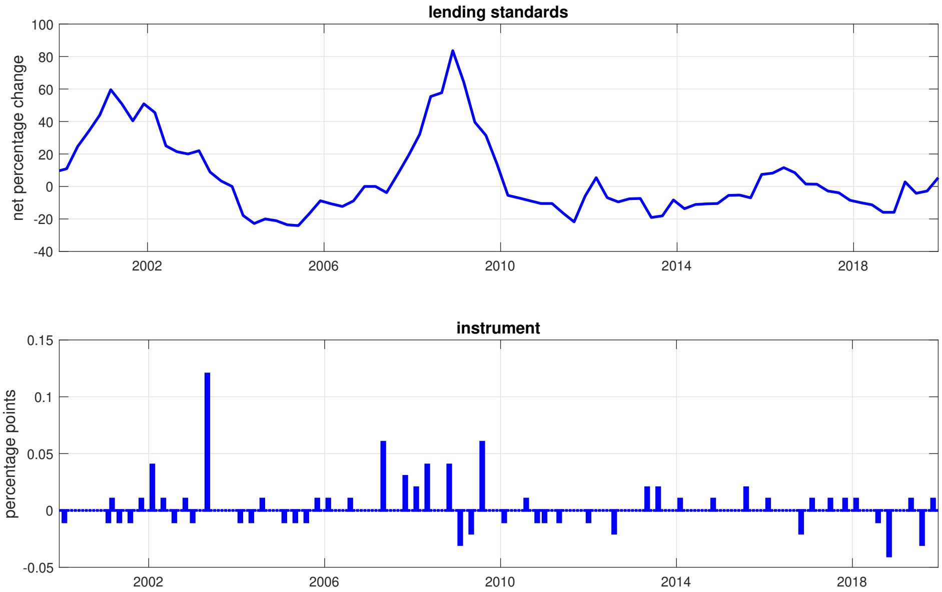
$$z_{t,d}^{spread} = \left(R_{t,d}^{BAA} - R_{t,d}^{AAA}\right) - \left(R_{t,d-1}^{BAA} - R_{t,d-1}^{AAA}\right), \tag{1.1}$$

where $t$ and $d$ indicate the month and the day of the release. Here, $R_{t,d}^{BAA}$ and $R_{t,d}^{AAA}$ are the yields on BAA- and AAA-rated corporate bonds on the release day, respectively. For non-release days, the surprise is zero. Finally, we transform the daily surprise series into a monthly instrument, which we use for the empirical analysis below. We obtain a monthly instrument series by assigning every release date to the corresponding month. For months without any release, the instrument series receives a zero. Accordingly, the instrument series is given by

$$z_t^{spread} = \begin{cases} z_{t,d}^{spread} & \text{if release in } t \\ 0 & \text{if no release in } t \end{cases}$$

Under the standard identifying assumption in the news announcement literature that other factors are white noise and, hence, do not affect the corporate bond spread on SLOOS release dates on average, the daily changes in the spread between low rated corporate bonds and their higher rated peers on these days is an indicator of unanticipated changes in the credit standards.

Figure 1.2: Lending standards and surprise on loan survey release days




*Notes:* The upper panel shows the net percentage of domestic banks tightening lending standards. The lower panel shows the change in the spread between BAA- and AAA-rated corporate bonds on SLOOS release days (in percentage points). See Appendix A for data sources.

Figure (1.2) shows the net percentage change of banks tightening lending standards (upper panel) and the instrument series, $z_t^{spread}$ (lower panel). The most pronounced surprises were on 9 May 2003, followed by 17 May 2007 and 17 August 2009. As expected, the volatility increases during the 2008/09 financial crisis and the subsequent recession.

### 1.2.2 Properties of our instrument

In this subsection, we evaluate the characteristics of the instrument series constructed in the previous subsection. We begin by studying the information content of the instrument. Table (1.1) sheds light on the information content of the change in the spread on release days. The upper panel compares the mean and the standard deviation on the release dates with the mean and the standard deviations on 5, 15 and 30 trading days prior and after the release dates, respectively. In all six cases, the standard deviation of changes of the spread on the release date is larger. In five cases, the difference is statistically significant on a 5% level. Thus, the change in spreads on release days contains significantly more information compared to alternative days.

Table 1.1: The information content of the instruments

| | change in BAA-AAA spread | | | | | | |
|---|---|---|---|---|---|---|---|
| | release day | release day $+k$ | | | | | |
| | | $k=-5$ | $k=5$ | $k=-15$ | $k=15$ | $k=-30$ | $k=30$ |
| mean ($\times 10$) | 0.092 | 0.030 | 0.066 | 0.018 | 0.265 | -0.011 | -0.128 |
| std. dev. | 0.115 | 0.080*** | 0.073*** | 0.082*** | 0.111 | 0.077*** | 0.065*** |
| | change in VIX | | | | | | |
| | release day | release day $+k$ | | | | | |
| | | $k=-5$ | $k=5$ | $k=-15$ | $k=15$ | $k=-30$ | $k=30$ |
| mean ($\times 10$) | 0.040 | -0.022** | -0.006 | 0.004 | -0.047** | -0.001 | -0.011 |
| std. dev. | 0.210 | 0.015*** | 0.019 | 0.016*** | 0.020 | 0.151 | 0.020*** |

*Notes:* The upper (lower) panel shows the mean and standard deviation of the change in the BAA-AAA corporate bond spread (the change in the VIX) on the SLOOS release dates and compares them with the first and second moment 5, 10 and 15 trading days before and after the release. The corresponding significance level of 1%, 5% and 10% is marked by ***, **, *, respectively.

The information content of the change in spreads on SLOOS release days could be impaired when other macroeconomic news are released on the same days. Scotti (2016) constructs an indicator of US macroeconomic surprises. The index summarizes the weighted surprise components of the most important data releases such as nonfarm employment, GDP, retail sales and others. We study the correlation between the change in spreads on release days against the level and the percentage change of the Scotti (2016) macroeconomic surprise index. The correlation of our instrument with the level (change) of the surprise index is 0.07 (-0.03).

We now turn to our monthly shock series, which we compare to two alternative series of monetary policy shocks, that is, the shocks derived by Swanson (2021) and Bu et al. (2021). Both monetary policy shocks are only weakly correlated with our shock series with both correlation coefficients equal to -0.15. Hence, the shock we identify is not systematically related to news about monetary policy.

We now compare the instrument with the net percentage change of lending standards itself. We identify changes in the lending standards indirectly via the market response because the change in standards itself might be predictable using information available before the release. It is imperative that the variable is a true shock, i.e. that it is not predictable. In fact, a simple forecast exercise reveals that while the net percentage change is to some extent predictable, the change in the spread can be seen as a surprise. This forecast exercise is based on a least-squares estimation of the following regression

$$y_t = c + \beta(x_{t-1} - x_{t-2}) + \varepsilon_t, \tag{1.2}$$

where $y_t$ is either the net percentage change or the instrument in $t$. On the right hand-side of the equation, $c$ is a constant, $x_{t-1} - x_{t-2}$ is the change in the exogenous variable from $t-2$ to $t-1$ and $\varepsilon_t$ describes the error term.

Since the time span during which banks respond to the survey usually includes the end of the first and the beginning of the second month in each quarter, it is not always possible to assign the net percentage change to a specific month. Hence, we estimate the equation with quarterly data. The equation allows us to quantify whether changes in economic or financial variables help to predict the net percentage change of lending standards in the upcoming period. The list of exogenous variables covers the (log) Dow Jones, the (log) S&P 500, (log) real GDP, (log) loans, the GZ spread, the excess bond premium, the (shadow) short rate and the BAA-AAA corporate bond spread. We run a separate regression for each variable and report Newey-West standard errors.

Table 1.2: Forecast of dependent variable

| $x_t$ | dependent variable: net percentage change in standards | | | dependent variable: change of spreads | | |
|---|---|---|---|---|---|---|
| | constant | $x_{t-1} - x_{t-2}$ | $R^2$ | constant | $x_{t-1} - x_{t-2}$ | $R^2$ |
| Dow-Jones (log) | 6.643 (0.181) | −1.140 (0.015) | 0.124 | 0.004 (0.100) | 0.000 (0.982) | 0.000 |
| S&P 500 (log) | 6.720 (0.135) | −1.390 (0.002) | 0.196 | 0.004 (0.118) | 0.000 (0.795) | 0.002 |
| GDP (log) | 12.00 (0.027) | −13.91 (0.001) | 0.184 | 0.007 (0.058) | −0.005 (0.158) | 0.032 |
| loans (log) | 6.633 (0.202) | −1.475 (0.292) | 0.025 | 0.005 (0.075) | 0.000 (0.664) | 0.003 |
| GZ spread | 5.090 (0.305) | 9.917 (0.142) | 0.062 | 0.004 (0.084) | −0.004 (0.398) | 0.011 |
| EBP | 5.061 (0.324) | 7.515 (0.418) | 0.019 | 0.004 (0.088) | −0.010 (0.178) | 0.042 |
| rate | 3.743 (0.340) | −23.79 (0.000) | 0.285 | 0.004 (0.078) | −0.005 (0.246) | 0.015 |
| spread | 4.946 (0.318) | 19.86 (0.105) | 0.057 | 0.004 (0.081) | −0.011 (0.177) | 0.023 |

*Notes:* The dependent variable are the net percentage change in lending standards (left panel) and our instrument from Equation (1.2) (right panel). Log in the exogenous variables refer to log differences and can hence be interpreted as growth rates. The presented p-values are constructed via Newey-West standard errors and displayed in brackets.

Table (1.2) shows the results. For the net percentage change, all eight variables have the expected sign. Four of them are significant on a 95% confidence level. Hence, current quarter-on-quarter (qoq) growth rates of the Dow Jones Index, the S&P 500 and GDP as well as qoq changes in the shadow rate contain valuable information about the net percentage change of lending standards in the next quarter. In contrast to that, the instrument series is not predictable based on any of the eight variables. Finally, we also assess the autocorrelation of the two series. This allows us to take a stand on whether the variables can be predicted by their own lags. Table (1.3) reports the autocorrelation and partial autocorrelation as well as the Ljung-Box Q-statistic. According to the latter, the net percentage change (change of spreads) exhibits (no sign of) serial correlation.

Table 1.3: Autocorrelation of change in lending standards and spreads

| lag | net percentage change in standards | | | change of spreads | | |
|---|---|---|---|---|---|---|
| | AC | PAC | $Q$-statistic | AC | PAC | $Q$-statistic |
| 1 | 0.919 | 0.919 | 70.169 (0.000) | -0.053 | -0.053 | 0.2366 (0.627) |
| 2 | 0.797 | -0.310 | 123.58 (0.000) | 0.019 | 0.016 | 0.2663 (0.875) |
| 3 | 0.664 | -0.075 | 161.16 (0.000) | 0.008 | 0.010 | 0.2721 (0.965) |
| 4 | 0.489 | -0.368 | 181.80 (0.000) | 0.016 | 0.017 | 0.2948 (0.990) |
| 5 | 0.309 | -0.033 | 190.13 (0.000) | 0.138 | 0.140 | 1.9593 (0.855) |
| 6 | 0.151 | 0.005 | 192.14 (0.000) | 0.076 | 0.093 | 2.4731 (0.871) |
| 7 | 0.024 | 0.126 | 192.19 (0.000) | -0.120 | -0.118 | 3.7608 (0.807) |
| 8 | -0.080 | -0.058 | c192.78 (0.000) | -0.157 | -0.186 | 6.0157 (0.645) |
| 9 | -0.178 | -0.190 | 195.70 (0.000) | 0.020 | -0.005 | 6.0512 (0.735) |
| 10 | -0.251 | -0.055 | 201.60 (0.000) | -0.049 | -0.060 | 6.2761 (0.792) |

*Notes:* The left panel shows the autocorrelation (AC), the partial correlation (PAC) and the Ljung-Box Q-statistics from the net percentage change in lending standards. The p-values for the Ljung-Box Q-statistics are displayed in brackets. The underlying null hypothesis assumes no autocorrelation of order $k$. The right panel reports the corresponding results for our instrument from Equation (1.2).

Below, we rely on a second instrument that allows us to distinguish banks' risk-taking behavior from changes to their credit supply. For that purpose, we build on the daily growth rate of the CBOE Volatility Index (VIX), which we also receive from FRED. On release days, the standard deviation of the growth rate is again larger than 5, 15 and 30 trading days before or after the announcement, see the lower panel of Table (1.1). This finding is significant in three cases.

## 1.3 Evidence from local projections

Our aim is to estimate the impact of surprise changes to lending standards on macroeconomic and financial conditions. For that purpose, we estimate a series of local projections a la Jordà (2005) and instrument lending standards with the response of spreads on SLOOS release days. Hence, we estimate instrumental variable local projections (IV-LP) following Stock and Watson (2018).

### 1.3.1 Model

We regress the dependent variable $y_t$ at time $t+h$ on a constant, $\alpha_h$, the net percentage change of lending standards, $stand_t$, and a vector of control variables, $\mathbf{x}_t$, which also includes lags of the dependent variable using 2SLS,

$$y_{t+h} = \alpha_h + \beta_h \widehat{stand}_t + \gamma_h \mathbf{x}_t + u_{t+h}, \tag{1.3}$$

where we use $z_t^{spread}$ as an instrument for $stand_t$. Hence, $\widehat{stand_t}$ are the fitted values of lending standards obtain from the first-stage regression of lending standards on the instrument and the control variables.

The estimate of $\beta_h$ is the coefficient of interest. Plotting $\beta_h$ as a function of $h = 0, ..., 30$ provides us with an impulse response function. We follow Jordà (2005) and apply a Newey-West correction to our standard errors, which we use below to construct confidence bands around the impulse responses. The maximum lag for the Newey-West correction is set to $h+1$.

The list of dependent variables includes industrial production (in logs), consumer prices (in logs), the short-term interest rates, the VIX volatility index, the excess bond premium of Gilchrist and Zakrajsek (2012), the GZ spread of Gilchrist and Zakrajsek (2012), the spread between high-yield bonds and AAA-rated bonds, the loan volume (in log), the Credit Subindex of the Financial Conditions Index of the Federal Reserve Bank of Chicago, nonfarm payroll employment (in log), the overall volume of commercial and industrial loans (in logs), the Dow Jones and S&P 500 equity price indices (in logs) and the index of house prices (in logs). The net percentage change of lending standards is taken from the SLOOS.

The data frequency is monthly and the estimation sample is 2000:1 to 2019:12. Table (1.5) provides details on the definition of the macroeconomic and financial variables and the data sources. Table (1.6) lists the variables from the loan survey, such as the net percentage changes, which are linearly interpolated from quarterly to monthly frequency.

The vector of controls includes the log of industrial production, the log of the PCE price level, the excess bond premium and the Wu-Xia shadow federal funds rate. For these variables, we include the realization in $t$ and two lags. The vector also includes two lags of the dependent variable. If one of the control variables is used as the dependent variable, the vector $\mathbf{x}_t$ is adjusted accordingly. Overall, the results appear very insensitive to the choice of control variables and their lag structure.

To be a valid instrument, $z_t^{spread}$ has (i) to meet the relevance condition, i.e. it must be correlated with the variable to be instrumented, (ii) to be contemporaneously exogenous with respect to $u_t$ and (iii) must be uncorrelated with all leads and lags of $u_t$. The first property is evaluated using the $F-$statistic in the first-stage regression. The second property is met by construction: the change in the BAA-AAA spread on release days should be exogenous with respect to the other variables in the equation. To meet the lead-lag exogeneity assumption, we follow Stock and Watson (2018) and include two lags of the instrument as well as lags of the endogenous variable and the control variables from Equation (1.3) in the first-stage regression. The number of lags of $z_t^{spread}$ is chosen by minimizing the Akaike Information Criterion. This procedure recommends two lags in the first-stage regression throughout all specifications.

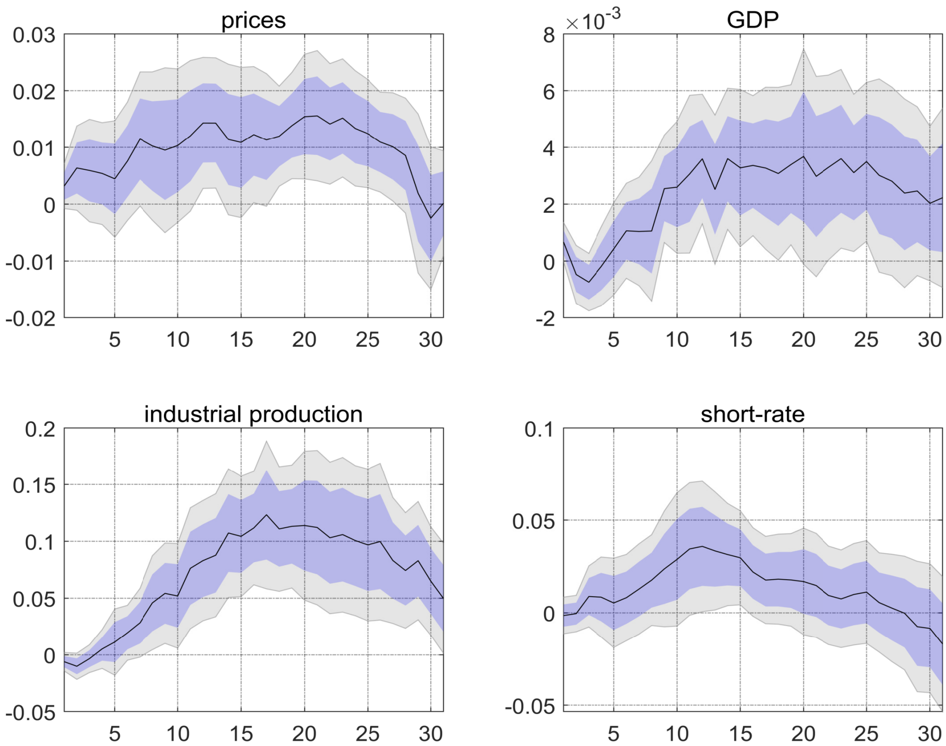
As the set of right-hand side variables differs across the estimated models due to lags of the dependent variables, we obtain an $F-$statistic for each model. For the baseline model with GDP as the dependent variable, the heteroscedasticity-robust $F-$statistic is $F^{HAC} = 12.36$, and the conventional standard $F-$statistic is $F^{Hom} = 12.96$. The $F-$statistic lies above the critical value of 10, which is typically used in the applied literature, for all endogenous variables other than the variables taken from the SLOOS itself. When estimating the response of banks' perceived demand for credit, which is elicited in the survey, the $F-$statistic drops to 3.8. This is not surprising: in this case, the first-stage regression includes lending standards and credit demand, both taken from the survey. Since both are strongly negatively correlated, the instrument losses its explanatory power. Hence, we need to remain cautious when interpreting the response of credit demand to the identified shock.

### 1.3.2 Results

Figures (1.3) to (1.6) report the estimated impulse responses. The confidence bands cover 65% and 90% of the potential estimates, respectively. All figures show the response to a fall of one percentage point in the net percentage of banks tightening lending standards. Hence, the shock is expansionary in nature.

Figure (1.3) shows the shock impact on the business cycle. An easing of credit standards leads to a significant improvement of economic activity as reflected in GDP and industrial production, respectively. Moreover, consumer prices increase. As a result of the increase in both activity and prices, short-term interest rates rise. Hence, the Fed tightens monetary policy conditions.

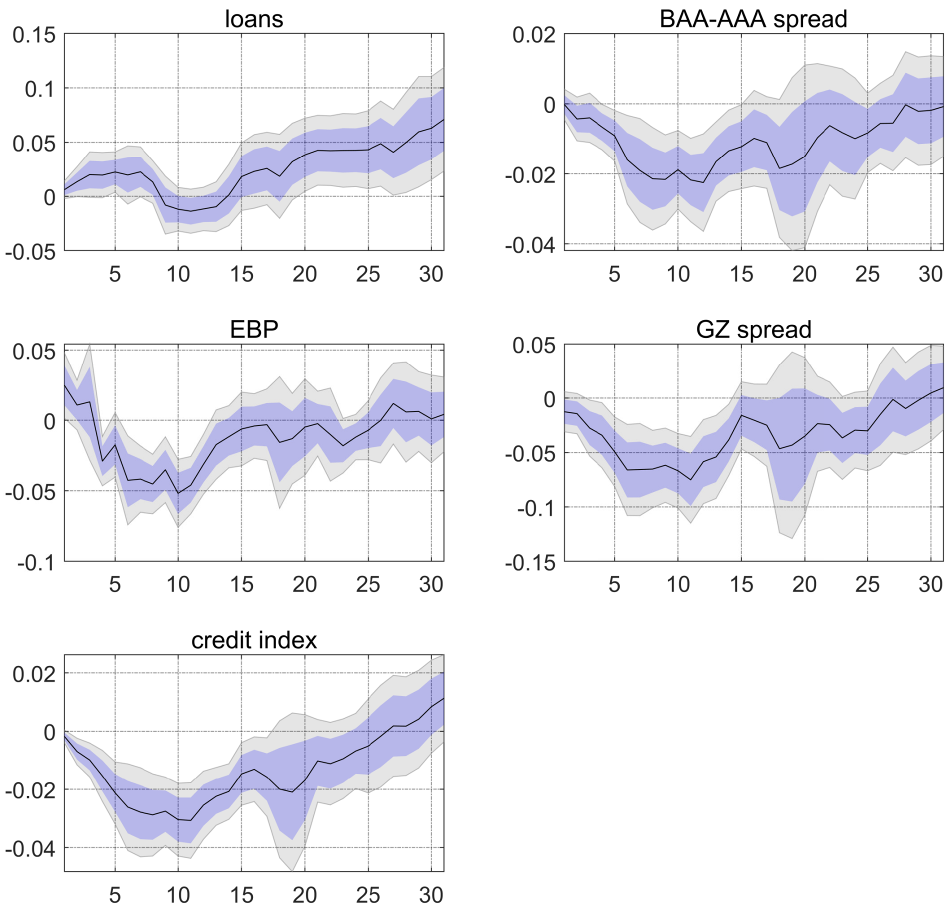
Figure 1.3: Response to credit standards shock I




*Notes:* The figure shows the estimated $\beta_h$ coefficient (solid line), i.e. the response to a change in the BAA-AAA spread of one basis point on SLOOS release days. The blue and grey shaded areas display the 68% and 90% confidence bands, respectively. They are constructed using Newey-West standard errors.

The credit market eases after a surprise fall in credit standards. Figure (1.4) shows that spreads, both the high-yield/AAA spread and the GZ spread, narrow significantly following the shock. Furthermore, the excess bond premium falls and peaks ten months after the shock. The overall loan volume remains stable in the first year after the shock before it eventually increases. Credit conditions as reflected in the Credit Subindex of the Chicago Fed's Financial Conditions Index ease significantly after the relaxation of lending standards.

Figure 1.4: Response to credit standards shock II

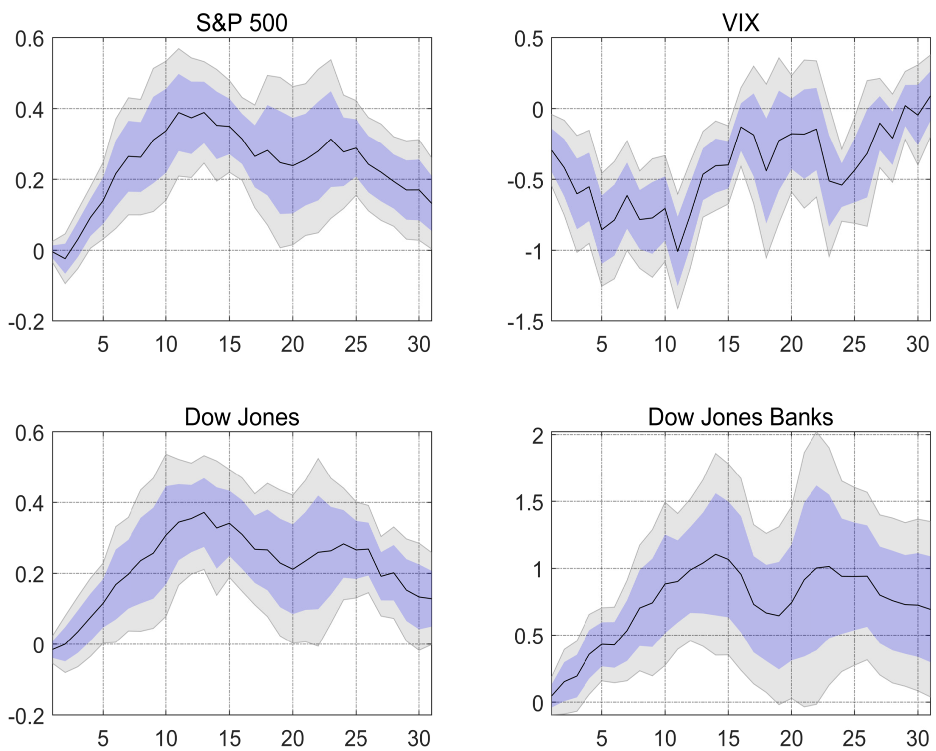



*Notes:* The figure shows the estimated $\beta_h$ coefficient (solid line), i.e. the response to a change in the BAA-AAA spread of one basis point on SLOOS release days. The blue and grey shaded areas display the 68% and 90% confidence bands, respectively. They are constructed using Newey-West standard errors.

Figure (1.5) reports the sensitivity of various asset prices to credit standards. The level of stock prices increases significantly. While the peak response of the overall Dow Jones and S&P 500 indices is about 0.4%, the subindex of the Dow Jones covering the banking industry increases more than twice as much. Looser standards also reduce equity price volatility as reflected by the VIX index. The responses of employment, house prices and credit demand are shown in Figure (1.6). Employment increases strongly after the shock, with the peak occurring two years after the shock. This is consistent with the response of GDP discussed before and the nature of employment as a lagging indicator of the business cycle. Throughout the 30 months shown in the Figure, house prices remain insensitive to shocks to lending standards.

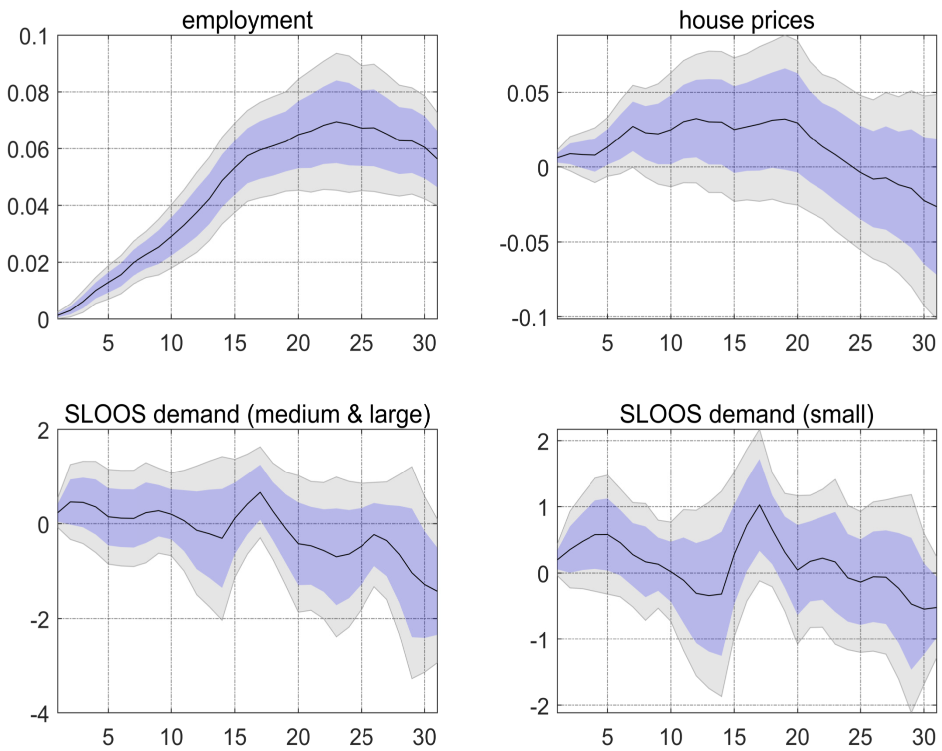
The responses of credit demand reported in the SLOOS are shown in the bottom half of Figure (1.6). The loan demand of medium and large firms appears insensitive to lending standards shocks, while the demand from small firms tends to fall. When interpreting these responses, though, we have to remember the low $F-$statistic from the first-stage regression for credit demand. The instrument loses its information content in this case, thus pointing to a weak instrument problem.
To summarize, a surprise easing of credit standards causes a significant expansion of financial conditions and economic activity. In the next section, we decompose these responses into two alternative channels.

Figure 1.5: Response to credit standards shock III




*Notes:* The figure shows the estimated $\beta_h$ coefficient (solid line), i.e. the response to a change in the BAA-AAA spread of one basis point on SLOOS release days. The blue and grey shaded areas display the 68% and 90% confidence bands, respectively. They are constructed using Newey-West standard errors.

Figure 1.6: Response to credit standards shock IV




*Notes:* The figure shows the estimated $\beta_h$ coefficient (solid line), i.e. the response to a change in the BAA-AAA spread of one basis point on SLOOS release days. The blue and grey shaded areas display the 68% and 90% confidence bands, respectively. They are constructed using Newey-West standard errors.

## 1.4 Credit supply vs. risk-taking shocks

Our analysis in the previous section highlights that an unanticipated easing in lending standards, identified by a decrease in spreads on SLOOS release dates, has macroeconomic consequences for real and financial market variables. An easing of lending standards by banks can result from two alternative motives: First, banks extend the supply of credit for a given degree of risk-taking. Thus, they give more loans to firms of equal quality. Second, banks increase the amount of risk they are willing to accept when giving loans and provide loans to creditors of lower quality. The aggregate business cycle implications of both types of shocks might be similar, but the implications for financial stability are not.

We now disentangle these two channels with a BVAR model following Jarocinski and Karadi (2020), in which we differentiate between two instruments. Besides the change in the spread on release days introduced before, we also use information from daily changes of the VIX on SLOOS release days. Specifically, we identify an increase in credit supply (risk-taking) via a decrease in the spread accompanied by a decrease (increase) in the VIX. We outline the methodology and the shock identification in Section (1.4.1) in more detail and present the corresponding results in Section (1.4.2).

### 1.4.1 Model

Following Jarocinski and Karadi (2020), the BVAR model includes macroeconomic and financial variables on a monthly frequency as well as higher frequency changes of instruments around pre-specified events. In our case, the pre-specified events are SLOOS release dates. We consider daily changes in the spread and the VIX from the eve on the day before until the end of the day of the release to discern between credit supply shocks and risk-taking shocks. As before, we receive a monthly time series for each instrument by assigning each release date to the corresponding month. If there is no release date in a month, both instruments receive zeros. In the baseline scenario, the list of monthly variables includes the (shadow) short rate, (log) employment, (log) prices, (log) VIX, the EBP and bank lending standards from the SLOOS. Hence, we estimate a VAR model with six monthly variables and two instruments. Let $z_t^{spread}$ and $z_t^{VIX}$ be the monthly instrument series for the spread and the VIX, respectively, such that $z_t = [z_t^{spread} \; z_t^{VIX}]'$ holds. In a more general case, $N$ reflects the number of instruments. The $M \times 1$ vector of the monthly series is given by $y_t$.

The model's special feature are the instruments. As it is standard in VAR models, they are allowed to affect the monthly variables on impact and with some delay. In contrast to that, we assume that the lags of all eight variables have no impact on the instruments. The rationale behind that assumption is that agents have all relevant information on the eve before the release dates including information on the lags of the other variables. Hence, they cannot affect the instrument.[6]

Moreover, we do not include a constant for the instruments as they are surprises that should have a mean of zero. The model can be described as follows

$$\begin{pmatrix} z_t \\ y_t \end{pmatrix} = \sum_{p=1}^{P} \begin{pmatrix} 0 & 0 \\ B_p^{yz} & B_p^{yy} \end{pmatrix} \begin{pmatrix} z_{t-p} \\ y_{t-p} \end{pmatrix} + \begin{pmatrix} 0 \\ c^y \end{pmatrix} + \begin{pmatrix} u_t^z \\ u_t^y \end{pmatrix}, \tag{1.4}$$

where $P$ denotes the lag length, $\mathbf{B_p^{yz}}$ and $\mathbf{B_p^{yy}}$ are a $N \times (N + M)$ and an $M \times (N +$

[6] As shown 2.2 a simple forecast exercise reveals that the the daily changes of the high yield AAA spread on the SLOOS release days are indeed not linked to macroeconomic developments or its own lagged values.

$M$) matrices of coefficients capturing lagged influences of the instrument and the monthly data, respectively. While $c^y$ is a vector of constants for the monthly series, $u_t^z$ and $u_t^y$ capture the normally distributed error terms with a mean of zero and a variance-covariance matrix $\mathbf{\Sigma}$.

BVAR models require the elicitation of prior distributions for all estimated coefficients and the variance-covariance matrix. Following Jarocinski and Karadi (2020), we rely on Minnesota priors, where the variance-covariance matrix is assumed to follow an Inverted Wishard distribution. We employ a Gibbs sampler to generate draws from the posterior.[7]

The shock identification deserves special attention. As before, we assume that relaxed credit standards narrow the credit spread. Consequently, changes in the spread on the release day signal agents' perception of the information on the SLOOS. More precisely, a decrease in the spread signals looser standards than expected before the release. However, an unexpected loosening in credit standards can either be associated with lower or higher overall market risk. Specifically, market risk increases indicate that agents question whether the looser lending conditions are well anchored in the macroeconomic environment. If standards are too lax, they create stability concerns that are reflected by higher market uncertainty. Put differently, banks increase their risk-taking behavior as their standards decrease and risk increases simultaneously. In contrast to that, market participants appreciate a decrease in lending standards when it is accompanied by a decrease in market risk. In this case, agents believe that the relaxed credit conditions boost lending and thereby economic activity, which ultimately decreases market risk. As we assume that the VIX adequately captures the market risk, its changes on the release day serve as the second instrument series. For convenience, we label the former shock a "risk-taking shock" and the latter a "pure credit supply shock".

Overall, the two surprises have a non-significant positive correlation of 0.044. In 31 (25) of 80 cases, both surprises show the same (opposing) signs. In 24 cases, we observe no change in one of the two variables. In line with the theory, we observe that while the majority of risk-taking shocks occurred prior to the Lehman Brothers collapse, the majority of credit supply shocks unveiled thereafter. Put differently, releases in lending standards were mainly caused by banks' risk appetite prior to the recession.

Table (1.4) displays the identifying restrictions for the two shocks formally. Despite the sign restrictions mentioned on the surprises, we leave all other variables unrestricted. Additionally, we rely on the uncontroversial assumption that other shocks

[7]More details on the applied priors can be found in Jarocinski and Karadi (2020).

have no effect on the surprises.

Table 1.4: Identifying restrictions

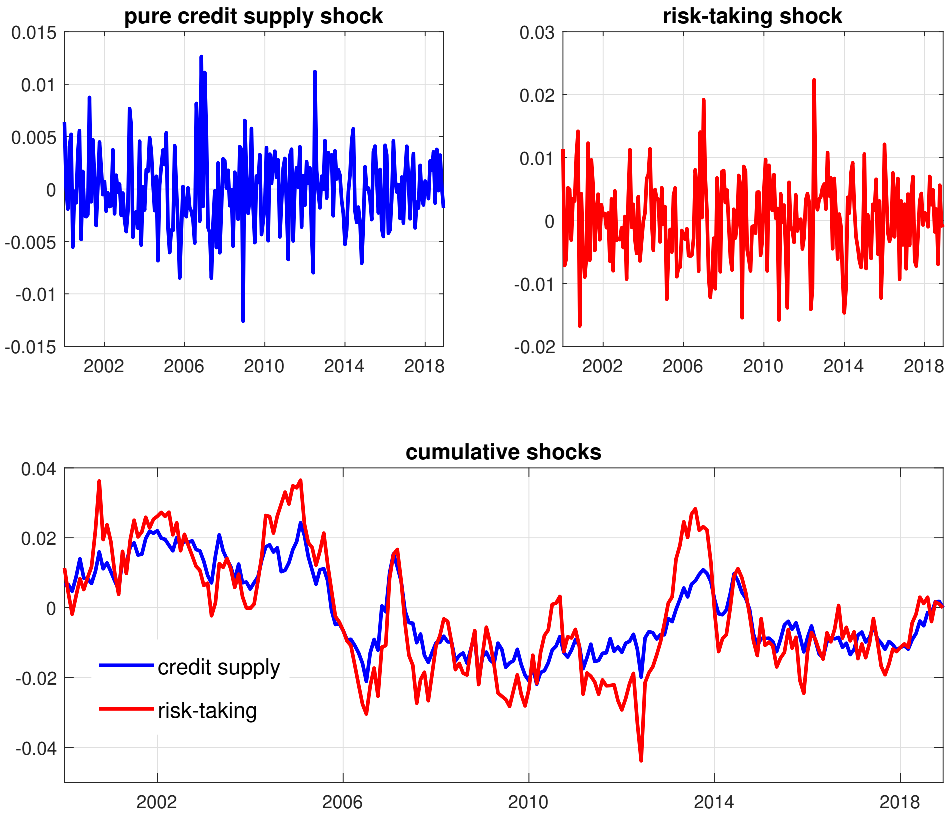
| variable | risk-taking | shock<br>pure credit supply | other |
|---|---|---|---|
| $z_t^{spread}$ | - | - | 0 |
| $z_t^{VIX}$ | - | + | 0 |
| $y_t$ | unrestricted | unrestricted | unrestricted |

*Notes:* The identifying assumptions are imposed on impact, where "+" corresponds to an increase and "-" to a decrease in the underlying variable. "0" marks zero restrictions, i.e. the underlying variable is not allowed to respond on impact.

Following Jarocinski and Karadi (2020), we set the lag length to 12. However, the results are largely unaffected by other lag length choices. For reasons of comparability, we standardize both shocks so that they are associated with a one basis point drop in the spread on the release date.

Figure (1.7) shows the time series of the pure credit supply and the risk-taking shock, respectively. Note that the series is defined such that a positive realization of either shock is an easing of credit conditions. The cumulative series reported in the bottom panel of the figure reveals interesting differences across the two shocks. A sequence of shocks in one direction implies that the cumulative series persistently deviate from zero. In 2005, i.e. before the financial crisis, the risk-taking shock was particularly expansionary. Put differently, there was a sequence of expansionary risk-taking shocks, which is consistent with the view that increased risk-taking contributed to the build-up of financial imbalances. After 2008, the cumulative risk-taking shock indicates a particularly restrictive contribution of the shock as banks curtailed their exposure to risky borrowers.

Figure 1.7: Decomposed shock series




*Notes:* The graph shows the pure credit supply shock (upper left panel) and the risk-taking shock (upper right panel). The Figure in the bottom panel shows the cumulative series of both shocks.

### 1.4.2 Results

Figures (1.8) and (1.9) in the Appendix display the impulse-response functions for our baseline model. The consequences of an expansionary pure credit supply shock are shown in Figure (1.8), while the impact of the risk-taking shock is reported in Figure (1.9).

All variables behave as expected and in line with our findings in Section (1.3). Both shocks lead to a drop in the lending standards. According to the peak responses, the credit supply shock is more substantial and its effect lasts longer. The fact that the VIX decreases (increases) after a pure credit supply (risk-taking) shock shows that our identification strategy is successfully disentangling the two shocks. The results of the EBP are similar to those of the VIX. We find an increase after a risk-taking shock and the tendency of a decrease after a pure credit-supply shock. Interestingly, the short rate's reaction is more pronounced in the aftermath of a pure credit-supply shock. One possible interpretation is that pure credit-supply shocks are a bigger threat to price stability in the eyes of policymakers. Another explanation could be that the Fed wants to position itself as an institution that is not leaning against the wind so that they do not respond to financial imbalances associated with the risk-taking behavior. Although both expansionary shocks positively affect the median response of employment, the 16th percentile response is at no point in time positive. In a similar vein, prices tend to increase after both shocks. However, this time, the increases after the risk-taking shock are, in particular in the first periods, stronger. This can be the result of the weaker policy response to the risk-taking shock.

We now estimate a number of seven variable BVAR model where the additional variables stem from the following list: the lending standards of small firms, the credit demand from large and medium size firms as well as from small firms, the corresponding lending spreads, (log) S&P 500, the (log) overall and bank-specific Dow Jones Index, the spread between high yields and AAA-rated corporate bonds, the GZ spread, the Credit Subindex of the Chicago Fed's Financial Conditions Index, (log) loans, (log) real GDP, (log) industrial production and (log) house prices. As in Section (1.3), this allows us to gain more granular information on the behavior of macroeconomic and financial variables. Moreover, we can assess the accuracy of our previous findings. Figures (1.10) and (1.11) show the impulse responses for the additional SLOOS variables. In line with the standards for large and medium enterprises, the standards for small firms also decrease after both shocks. The fact that we observe no clear drop in credit demand in three of the four cases indicates that our shock strategy is not accidentally identifying a credit demand shock. The lending spread tends to decrease after a pure credit-supply shock. In contrast to that, the risk-taking shock has no substantial impact.

Figures (1.12) and (1.13) display the response of the financial variables to the pure credit-supply and the risk-taking shock, respectively. Interestingly, while all three stock indices (S&P 500, Dow Jones and Dow Jones Banks) tend to increase after a credit supply shock, they decrease after a risk-taking shock. The response to the credit supply shock is in line with the theory, as bank lending can spur economic growth. For the risk-taking shock, an additional opposing channel exists. Specifically, the higher risk lead to drops in share prices. Moreover, the bank index shows a stronger reaction than the other two indices after a credit supply shock. The high Yield AAA spread decreases after a credit supply shock but shows no clear pattern after a risk-taking shock. In contrast to that, the GZ spread tends to drop after both types of shocks. Credit conditions, as reflected in the Credit

Subindex of the Chicago Fed's Financial Conditions Index, ease after both shocks. The easing is slightly more pronounced after the risk-taking shock.
Finally, Figures (1.14) and (1.15) outline how macroeconomic variables react to both shocks. Interestingly, loans and industrial production only increase after the pure credit supply shock. GDP and house prices tend to increase after both analyzed shocks in the medium term.

## 1.5 Conclusions

This paper analyzes the impact of banks' credit conditions on macroeconomic and financial variables. Specifically, we focus on the lending standards that banks report in the Fed's Senior Loan Officer Opinion Survey (SLOOS). The difficulty in assessing these effects arises from the endogenous nature of the variables. Put differently, banks change their credit conditions for a reason, e.g. they ease lending standards when the economic outlook improves. Vice versa, credit conditions affect loans and, hence, the real economy. The bulk of the empirical literature assesses the nexus between lending standards and economic and financial variables via VAR models.
In contrast to that, our first contribution is that our method relies on information on SLOOS release days. We use the change of the spread between BAA and AAA-rated corporate bonds as an instrument for unexpected changes in lending standards. The reason for this choice is that bank credit and corporate bonds are alternative funding sources for firms and, hence, (imperfect) substitutes. However, firms with weaker balance sheets find it more difficult and more expansive to substitute so that the spread widens when lending standards tighten more than expected.
With this instrument at hand, we then estimate instrumental variables local projections following Stock and Watson (2018). Specifically, we regress a number of macroeconomic and financial variables on the net percentage change where the series of daily changes of the BAA-AAA corporate bond spread is the instrument via local projections. We find that tighter standards reduce economic activity and weaken financial conditions significantly.
Building on that, we acknowledge that unexpected changes in lending standards could be associated with changes in the credit supply or the risk-taking behavior of banks. We show that a second instrument, the change of the VIX on release dates, allows us to differentiate between them. We receive impulse responses for pure credit supply and risk-taking shocks from a VAR with sign restrictions on the instrument a la Jarocinski and Karadi (2020). Our results show that while both shocks have similar effects on real economic variables, including consumer prices, they impact

financial variables and risk measures differently.

Our paper has several implications for policymakers. First, changes in lending standards impact the economy even when one controls for anticipation effects. Second, the release of the survey creates a market reaction. Hence, policymakers that know the outcome of the survey before all other agents might have the opportunity to use the first-mover advantage to create a room where the impact of the shocks can be damped, e.g. via forward guidance. Third, changes in lending standards can be the result of a pure credit supply or a risk-taking shock. As variables such as the VIX react differently to both kinds of shocks, policymakers should monitor these developments so that they can identify the shocks in real time.

Several extensions of the paper are feasible, but beyond the scope of this research. First, we refrain from a structural model that decomposes the a risk-taking shock from a credit supply. Second, we do not consider non-linear or time-varying effects in our empirical estimation approach. Third, the empirical model could also be applied to other economies such as the euro area.

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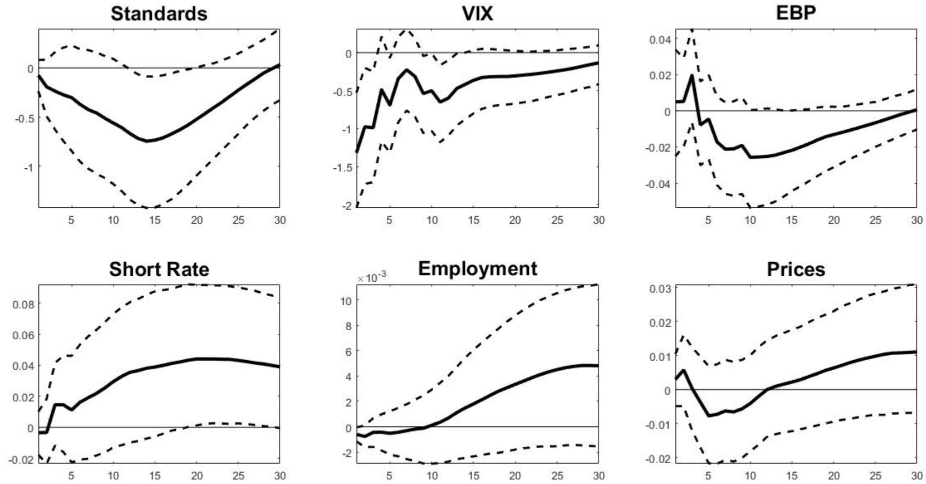
## 1.6 Appendix

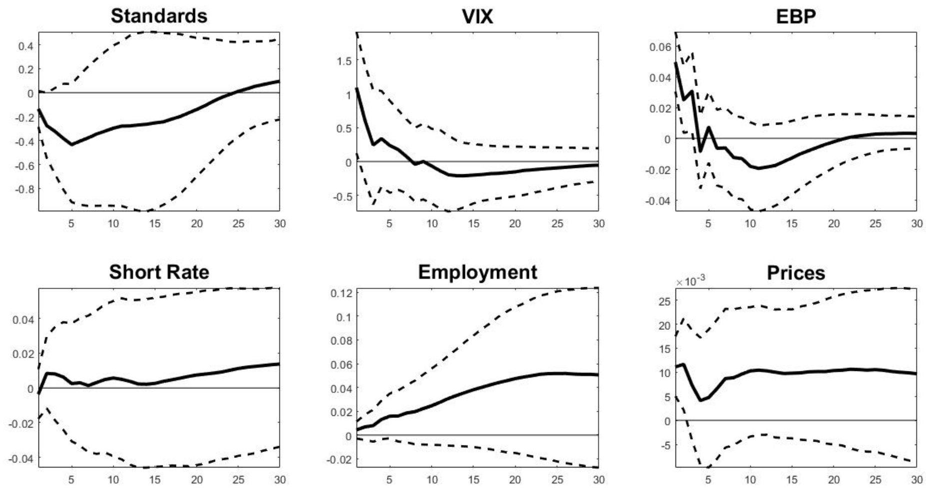
### Tables and figures

Figure 1.8: Baseline VAR model (pure credit supply shock)



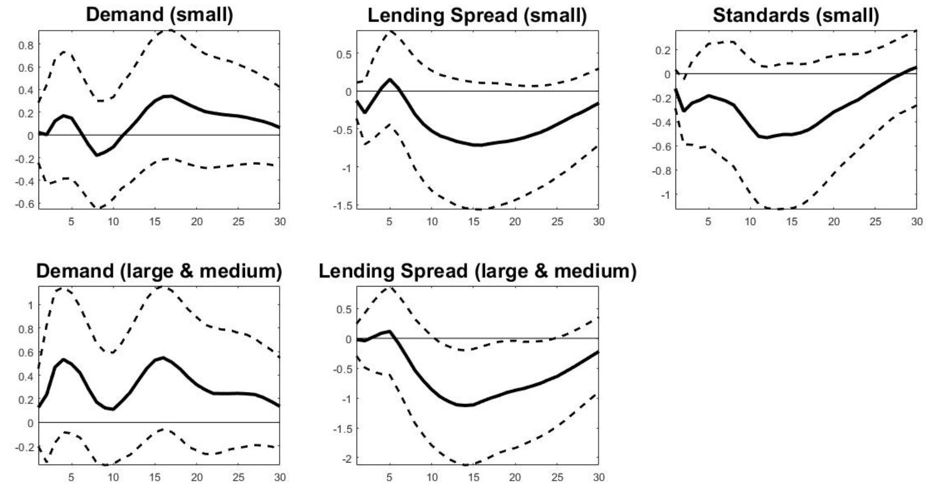

*Notes:* The sold line depicts the median response. The dotted lines are the 16th and 84th percentiles.

Figure 1.9: Baseline VAR model (risk-taking shock)



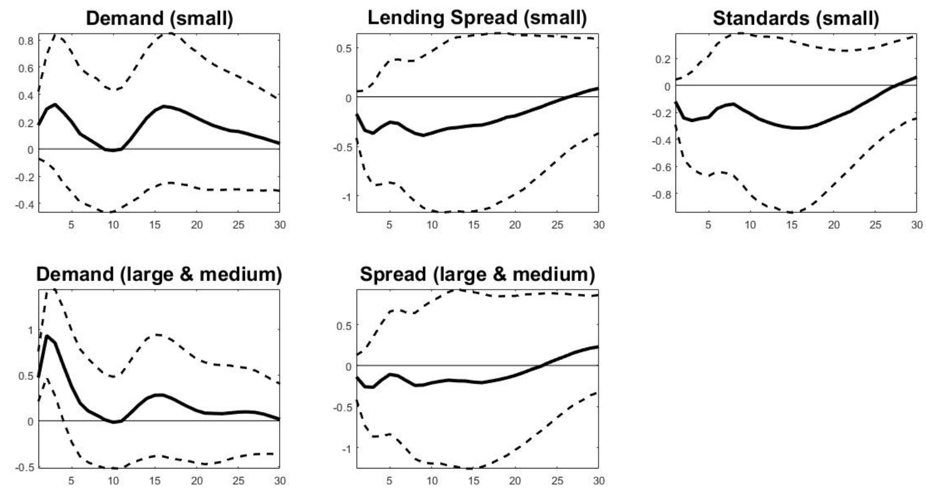

*Notes:* The sold line depicts the median response. The dotted lines are the 16th and 84th percentiles.

Figure 1.10: SLOOS variables (pure credit supply shock)



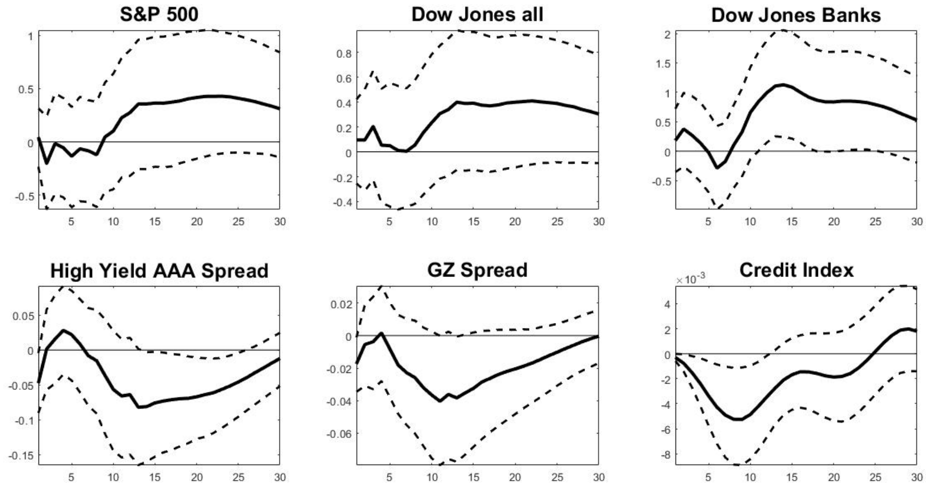

*Notes:* The sold line depicts the median response. The dotted lines are the 16th and 84th percentiles.

Figure 1.11: SLOOS variables (risk-taking shock)



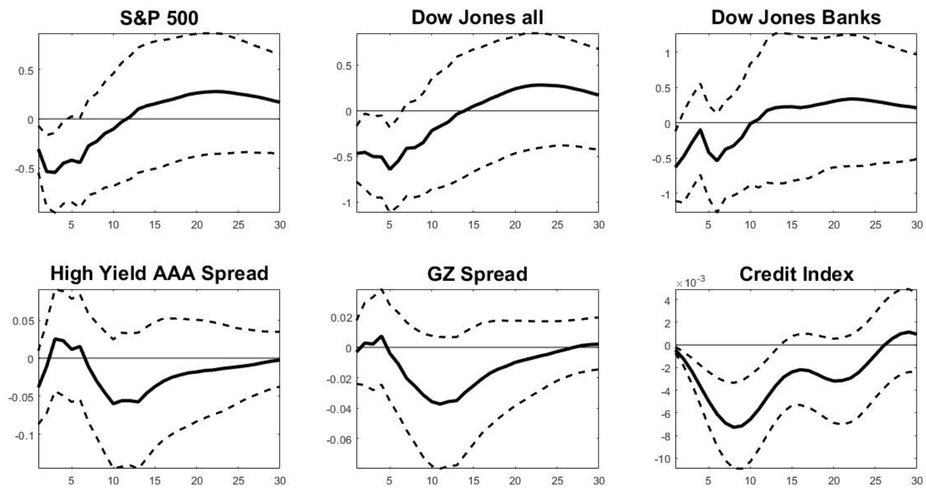

*Notes:* The sold line depicts the median response. The dotted lines are the 16th and 84th percentiles.

Figure 1.12: Financial variables (pure credit supply shock)



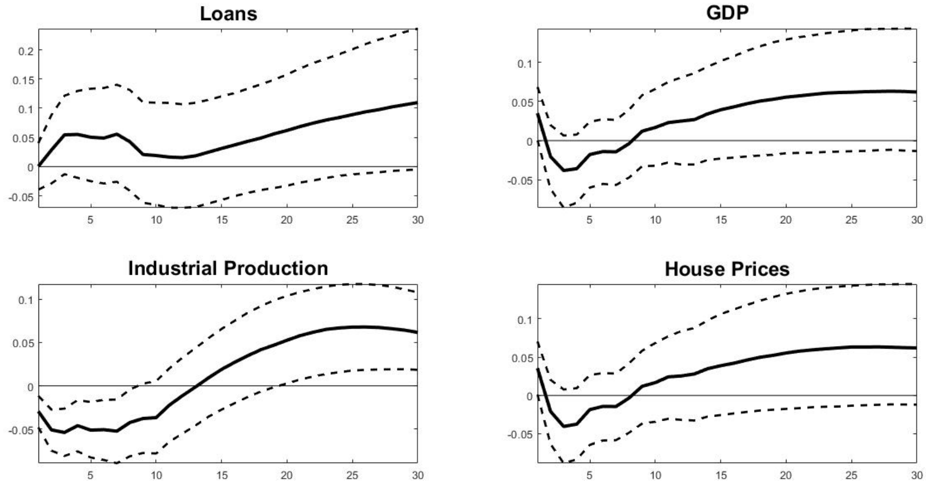

*Notes:* The sold line depicts the median response. The dotted lines are the 16th and 84th percentiles.

Figure 1.13: Financial variables (risk-taking shock)



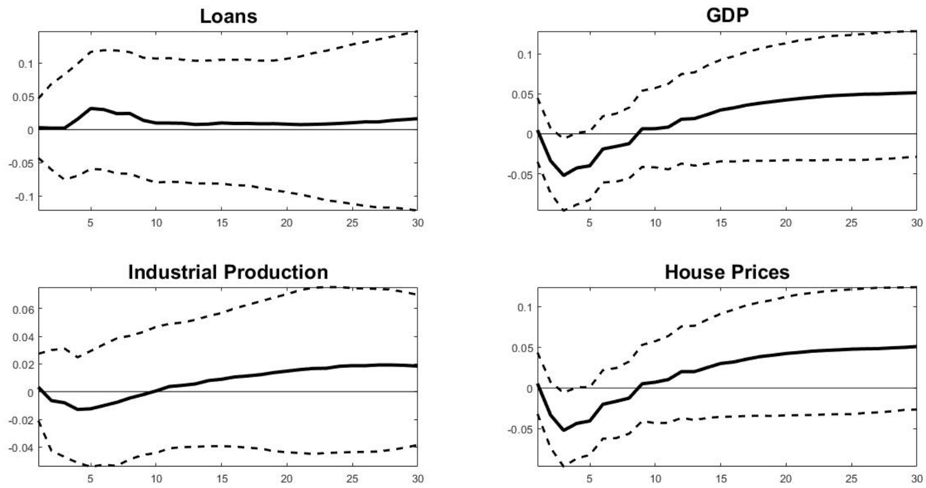

*Notes:* The sold line depicts the median response. The dotted lines are the 16th and 84th percentiles.

Figure 1.14: Macroeconomic variables (pure credit supply shock)




*Notes:* The sold line depicts the median response. The dotted lines are the 16th and 84th percentiles.

Figure 1.15: Macroeconomic variables (risk-taking shock)




*Notes:* The sold line depicts the median response. The dotted lines are the 16th and 84th percentiles.

## Data sources and definitions

This appendix contains details about the data series used in this paper.

Table 1.5: Data series I

| series | definition | units | frequency | source |
|---|---|---|---|---|
| industrial production | Industrial Production: Total Index s.a. | 2012=100 log | monthly | FRED |
| prices | Personal Consumption Expenditures: Chain-type Price Index s.a. | 2012=100 log | monthly | FRED |
| short rate | 2-Year Treasury Constant Maturity Rate | percent | monthly | FRED |
| VIX | CBOE Volatility Index: VIX | index log | daily/monthly | FRED |
| EBP | excess bond premium of Gilchrist and Zakrajšek (2012) | percent | monthly | FED |
| GZ spread | credit spread of Gilchrist and Zakrajšek (2012) | percent | monthly | FED |
| AAA yield | Moody's Seasoned Aaa Corporate Bond Yield | percent | daily/monthly | FRED |
| BAA yield | Moody's Seasoned Baa Corporate Bond Yield | percent | daily/monthly | FRED |
| high yield | ICE BofA US High Yield Index Effective Yield | percent | daily/monthly | FRED |
| employment | total nonfarm employees | log | monthly | FRED |
| S&P 500 | stock price index | log | monthly | Thomson Reuters |
| Dow Jones | stock price index | log | monthly | Thomson Reuters |
| loans | Bank Credit, All Commercial Banks, s.a. | log | monthly | FRED |
| house prices | Purchase Only House Price Index, s.a. | log | monthly | FRED |
| credit conditions | FRBCHI Financial Conditions Credit Subindex | deviation from mean | monthly | FRED |

Table 1.6: Data series II

| series | definition | units | frequency | source |
|---|---|---|---|---|
| credit standards | Net Percentage of Domestic Banks Tightening Standards for Commercial and Industrial Loans | net percentage | interpolated from quarterly to monthly | FRED |
| credit demand | Net Percentage of Domestic Banks Reporting Stronger Demand for Commercial and Industrial Loans | net percentage | interpolated from quarterly to monthly | FRED |
| spread | Spreads of loan rates over bank's cost of funds | net percentage | interpolated from quarterly | FRED |

## Release dates

This appendix contains the SLOOS release days used in this paper. The dates are listed in Table (1.7).

Table 1.7: Release dates

| | | | |
|---|---|---|---|
| 08.02.2000 | 07.02.2005 | 03.05.2010 | 03.08.2015 |
| 19.05.2000 | 09.05.2005 | 16.08.2010 | 02.11.2015 |
| 25.08.2000 | 15.08.2005 | 08.11.2010 | 01.02.2016 |
| 17.11.2000 | 07.11.2005 | 31.01.2011 | 02.05.2016 |
| 05.02.2001 | 08.02.2006 | 02.05.2011 | 01.08.2016 |
| 26.03.2001 | 15.05.2006 | 15.08.2011 | 07.11.2016 |
| 17.05.2001 | 14.08.2006 | 07.11.2011 | 06.02.2017 |
| 24.08.2001 | 30.10.2006 | 30.01.2012 | 08.05.2017 |
| 13.11.2001 | 05.02.2007 | 30.04.2012 | 31.07.2017 |
| 04.02.2002 | 17.05.2007 | 06.08.2012 | 06.11.2017 |
| 10.05.2002 | 13.08.2007 | 31.10.2012 | 05.02.2018 |
| 19.08.2002 | 05.11.2007 | 04.02.2013 | 04.05.2018 |
| 12.11.2002 | 04.02.2008 | 06.05.2013 | 06.08.2018 |
| 31.01.2003 | 05.05.2008 | 05.08.2013 | 13.11.2018 |
| 09.05.2003 | 11.08.2008 | 04.11.2013 | 04.02.2019 |
| 15.08.2003 | 03.11.2008 | 03.02.2014 | 06.05.2019 |
| 03.11.2003 | 02.02.2009 | 05.05.2014 | 05.08.2019 |
| 03.02.2004 | 04.05.2009 | 04.08.2014 | 04.11.2019 |
| 07.05.2004 | 17.08.2009 | 03.11.2014 | |
| 16.08.2004 | 09.11.2009 | 02.02.2015 | |
| 15.11.2004 | 01.02.2010 | 04.05.2015 | |

*Notes:* The dates are taken from the individual survey releases (before 2010) and from ALFRED (after 2010).

# 2 Prudential Policies in the Eurozone: A Propensity Score Matching Approach

This paper is available under[1]

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https://www.uni-marburg.de/en/fb02/research-groups/economics/macroeconomics/research/magks-joint-discussion-papers-in-economics/papers/2021-papers/09-2021_hafemann.pdf

This paper was accepted at the following conferences[2]:

I. Society for Nonlinear Dynamics and Econometrics (SNDE):
28$^{th}$ Annual Conference, 03/2020, Zagreb, Croatia.

II. Computing in Economics and Finance (CEF):
26$^{th}$ Annual Conference, 06/2020, Warsaw, Poland.

[1]We are grateful to David Finck, Paul Rudel, Peter Tillmann, Anisa Tiza Mimun, and other researchers at the University of Gießen for valuable feedback.

[2]Due to the pandemic both conferences were cancelled.

# Prudential Policies in the Eurozone: A Propensity Score Matching Approach

Lucas Hafemann[*,1]

This paper studies the effectiveness of micro- and macroprudential policy tools in the euro area. The established empirical literature on macroprudential policy generally considers panel estimations that suffer from two estimation biases, i.e., a selection bias and a time bias. We control for the former by a propensity score matching approach. Based on a logit model, we estimate the probability of a policy tightening for every country at each point in time. Matching procedures then find one or more matching partners for every tightening event with a similar likelihood of a tightening but no shift in the prudential policy stance. An iterative approach ensures that we offset the time bias, which exists if the estimation does not control for effects of preceding and subsequent prudential policy changes. We find that the announcement of a prudential policy tightening reduces credit growth significantly by about 1% on average. We further differentiate between effects along three dimensions. First, we observe that lending is more affected when policymakers have not communicated the implementation of measures before. Second, the effects are more substantial when EU/EA institutions are behind changes in the prudential policy stance. Third, microprudential policy measures have a bigger impact than macroprudential policies.

**Keywords:** Macroprudential policies, Financial cycles, Credit growth, Propensity score matching.

**JEL classification:** E44, E58, G18, G28

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## 2.1 Introduction

The global financial crisis of 2007/08 and the subsequent Great Recession were initially triggered by a burst of the US housing bubble. This underscores that distress in the financial markets can ultimately result in deep recessions. In highly interconnected financial markets, the stress in a subset of financial markets, such as the housing market, or stress of individual financial institutions, such as the Lehman Brothers' bankruptcy, can become systemic, leading to severe distortions of the entire financial system. Moreover, the crisis has proven that bank regulation on a microprudential level does not sufficiently limit systemic risk. In particular, market failures such as "too big" or "too connected" banks or the overstate of collaterals can lead to excessive, procyclical bank lending. Consequently, macroprudential policy measures have been introduced to tackle the weaknesses in the architecture with respect to banking. Among others, these measures include countercyclical capital buffers, liquidity ratios, and loan-loss provisions on the lender side. On the borrower side, measures such as limits on the loan-to-value (LTV) or the debt-to-income (DTI) ratio aim to avoid excessive leverage.

This paper empirically quantifies the effects of shifts in the prudential policy stance in euro area member states on credit growth and rents. The latter serves as a proxy for house prices. Overall, we find that the announcement of a prudential policy tightening reduces credit growth by about 1% on average. Our analysis is built on the data set provided by Budnik and Kleibl (2018), which lists all changes in prudential policy measures in EU member countries from 1999 to 2018. The data set includes information on which prudential measure was changed and whether the change can be interpreted as a tightening or an easing. Furthermore, information about the announcement and the implementation date for each measure is given and whether national and/or European institutions introduced the policy change. Since the changes' intensities are not always displayed, we focus on average treatment effects (ATE). The treatment group consists of all observations where policymakers tighten prudential policy measures.[1] Consequently, the control group lists all observations where no change in the prudential policy stance is observed.

In contrast to the bulk of the empirical literature, we rely on propensity score matching (PSM) approaches that are, according to Rosenbaum and Rubin (1985), designed to remove structural differences between treatment and control groups. These differences are present as prudential policies are a direct reaction to economic

[1] Further differentiating between tightening and loosening is theoretically an option. However, the vast majority of policy changes are tightening events so that the sample size for loosening events becomes too small after all adjustments.

fundamentals. In our case, countries that change macroprudential policies might, for instance, face higher bank leverage or house prices than states that do not alter prudential policies. PSM requires two steps. In a first step, we estimate the likelihood of a policy tightening for each observation, the so-called propensity scores, via a logit estimation. In a second step, we then match for every tightening observation one or more observations that display almost identical propensity scores but no changes in the prudential policy. Differences in the dependent variable between the two matching partners finally allow us to estimate the ATE. An iterative algorithm further ensures that we correctly specify the effects of several measures that were conducted in a small time window. This way, we can generate impulse-responses, which is a major contribution to the empirical literature. Impulse-responses are, for instance, necessary to compare the outcome of structural New Keynesian models with empirical results.

Besides introducing an iterative PSM approach to the prudential policy literature, this paper has two further major contributions. First, we focus on the euro area (EA), which is of particular interest, as national authorities, as well as EA and EU institutions, are equipped with a macroprudential mandate. Hence, we can investigate whether measures conducted by national authorities are more effective than measures by EA/EU institutions. Measures initiated by national authorities might display more substantial effects as national institutions can target the domestic market more explicitly than EA or EU institutions. However, measures conduct by national authorities might be bypassed in an integrated European market. Which of these two opposing channels is more pronounced remains an empirical question. We observe that measures based on EU/EA legislation have stronger effects than measures conducted solely by national authorities. Furthermore, we find that primarily changes in EU/EA variables lead to changes in prudential policies. Moreover, we differentiate between announcement and implementation effects. This is relevant as the announcement effect of future prudential policy changes is unclear from a theoretical point of view. On the one hand, banks might reduce lending timely after the announcement of a prudential policy tightening. This way, they smoothly adjust to the new standards. On the other hand, bank lending might increase in the short run. If credit institutions and households anticipate tighter prudential policy in the future, they might move lending to a time before the tighter policies become binding. In this case, we would observe a J-curve where the number of total credits increases in the short run, i.e., before the implementation and decreases after the implementation. However, we do not observe this J-curve empirically. Nevertheless, we find that the ATE is stronger when measures are not announced before, i.e., they

are implemented right away.
Structural models show that macroprudential policies can influence bank lending and, thereby, systemic risk. In general, these dynamic stochastic general equilibrium (DSGE) models include collateral constraints, as in Iacoviello (2005). Macroprudential policy and, in particular, limits on the loan-to-value (LTV) ratio then influence this collateral constraint. Based on a news-driven model that incorporates the housing market, Lambertini, Mendicino and Punzi (2013) find that LTV measures reduce macroeconomic volatility when implemented countercyclically with respect to the credit cycle. Funke and Paetz (2012) study a model in which LTV ratios only adjust to excessive levels of house price inflation. They conclude that LTV measures can effectively dampen property price booms. Alpanda and Zubairy (2017) evaluate whether macroprudential, monetary or tax policies are best suited to reduce household debt. They find that tightening the LTV ratio and reducing the tax-deductibility of mortgage interest are the most effective and least costly policy instruments for reducing household debt. For the euro area, Quinta and Rabanal (2014) show that macroprudential policy can reduce macroeconomic volatility. This reduction is higher if nominal credit and not the credit-to-GDP gap is included in a macroprudential policy rule. Furthermore, they show that macroprudential policy accelerates the effectiveness of the monetary policy. Building on a model of two economies within a monetary union, Brzoza-Brzezina, Kolasa and Makarski (2015) simulate imbalances that mainly hit the periphery countries in the euro area. They show that appropriate adjustments in the LTV ratio indeed reduce the volatility of credit and output in the periphery. However, this only holds when macroprudential policies are conducted decentralized.
Concerning the underlying data, the empirical literature can be split into two strands. While some studies focus on national data (e.g., Jimenez et al. (2017) and Ayiar, Calomiris and Wieladek (2014)), the majority of the empirical literature estimates the effectiveness of macroprudential policy on panel data series covering a multitude of countries. Building on the Spanish credit register, Jimenez et al. (2017) investigate the effect of dynamic provisioning that essentially works as a countercyclical bank capital buffer. They find that dynamic provisioning reduces the amplitudes of the credit cycle. In a similar manner, Ayiar, Calomiris and Wieladek (2014) focus on capital requirements in the UK. They find that domestic banks lower credit supply in response to tighter capital requirements. However, the total amount of outstanding debt does not decrease because foreign banks increase lending. Using an IMF survey answered by national prudential policy authorities, Cerutti, Claessens and Laeven (2017) evaluate how the overall tightness of prudential

regulation affects credit growth. More precisely, they construct an index that describes an economy's tightness by summing over all active measures. They then relate this index to credit growth changes and find that credit growth is lower when the prudential policy is tighter. In a similar vein, Akinci and Olmstead-Rumsey (2018) construct a macroprudential policy index that sums over all prudential policy changes relative to a base period. Their results indicate that tighter macroprudential policy leads to lower credit growth and house price inflation. Lim et al. (2011) further show that macroprudential policy can dampen credit growth's procyclicality. Zhang and Zoli (2014) find that primarily housing-related prudential policy measures reduce credit growth and house price inflation. Bruno, Shim and Shin (2017) show that macroprudential policies are more effective in Asia-Pacific economies if they complement monetary policy as they reinforce another.

Altogether, the empirical literature is still in its infancy and lags behind the theoretical considerations. For instance, structural models precisely show how changes in the macroprudential policy stance affect financial variables and the real economy over time. In contrast to that, the mainly applied policy indices do not directly measure the dynamic of a tightening or a loosening of a particular policy measure. The literature refrains mostly from analyzing impulse-responses because the already existent time bias would be amplified. This time bias is present when the endogenous variable responds with some delay. Distinguishing between the effects of prudential policy changes conducted in a small time window is then troublesome. Furthermore, a selection bias might be present because countries that change macroprudential policies might differ structurally from countries that do not alter prudential policies. Assuming that the economic fundamentals determine changes in the prudential policy stance, one would have to control for the economic fundamentals. However, precisely controlling for economic variables requires the knowledge of the underlying structural model. The empirical literature mainly focuses on linear models. In contrast to that, Funke and Paetz (2012) argue that macroprudential policies tightening primarily occur during excessive bank lending.

According to Rosenbaum and Rubin (1985), the PSM approach used in this paper approach can solve this selection bias. Propensity score matching approaches are relatively new to macroeconomics. Forbes, Fratzscher and Straub (2015) and Richter, Schularick and Shim (2019) are closest to our paper. Fratzscher and Straub (2015) analyze the effects of capital-flow management measures, which are only a subset of macroprudential policy measures. Since capital flows adjust timely, they do not face the time bias issue. The results indicate that some capital-flow management measures are capable of influencing capital flows. Building on that, Pandey et al. (2015) investigate capital controls in India. Forbes and Klein (2015) use PSM to evaluate how countries best respond to sudden stops in capital flows. Richter, Schularick and Shim (2019) rely on inverse propensity weights (IPW) to detect the impact of LTV measures for a panel of 56 countries and find that tighter LTV measures reduce credit and house prices. IPW and PSM are closely related. In fact, both require estimating propensity scores in a first step. In the second step, the IPW weights treated observations higher with a low probability of receiving treatment. The treated observations with a low propensity score are arguably closer to the control group. Put differently, while the PSM approach adjusts the control group to match the treatment group, the IPW also adjusts the treatment group to be closer to the control group. Austin and Stuart (2017) show that both methods yield similar results, but the PSM is preferable when the propensity score model is correctly specified.

The paper proceeds as follows: Section two describes the institutional framework behind prudential policies in the EMU. In Section three, we apply a standard panel estimation to our data set to bridge the gap between the PSM approach and those primarily applied in the current empirical literature on prudential policies. Afterward, we present empirical evidence from the PSM approach that removes the selection and the time bias. Section five concludes.

## 2.2 Prudential Policies in the Euro Area

The regulatory framework in the euro area is shaped by national authorities, EA and European Union (EU) institutions such as the European System of Financial Supervision (ESFS) and the Basel Accords. Prior to the global financial crisis of 2007/08 and the subsequent European debt crisis, prudential policies were primarily managed by national authorities. In the build-up to the crises, imbalances appeared on an internationally integrated financial market. Consequently, the ESFS has been introduced in 2011 with the main objective to monitor and harmonize prudential policies across member states. It consists of the European Systemic Risk Board (ESRB) and the European Supervisory Authorities (ESAs)[2] . While the ESAs are responsible for microprudential policy, the ERSB's objective is to identify systemic stress in the EU and supervise the macroprudential regulation of national institutions. In general, national authorities are still responsible for the implementation of all kinds of prudential measures.

[2]The ESAs consists of the European Banking Authority, the European Securities and Markets Authority and the European Insurance and Occupational Pensions Authority.

Since November 2014, the ECB functions as the direct prudential supervisor for the "systemically relevant banks" in the EA.[3] All banks with a value of its assets that is (i) above 30 billion or (ii) above 5 billion and exceeds 20% of national domestic GDP are categorized as a "systemically relevant bank". Furthermore, the ECB directly supervises prudential policies for banks that have applied for financial assistance under the European Stability Mechanism (ESM) or the European Financial Stability Facility (EFSF).

The Basel Accords set a more global framework for banking supervision. Since the Basel Committee is not endowed with a legislative mandate, the accords' enforcement is subject to national or EU-wide regulations. In the EU, the Basel Accords were implemented through the so-called Capital Requirements Regulations (CRR) and the Capital Requirement Directives (CRD). Both include measures that go beyond the scope of the Basel Accords. While the regulation is a binding legislative act, the directives are enforced through national law.

We analyze the role of prudential policy measures based on the data set provided by Budnik and Kleibl (2018) that also includes all CRR and CRD changes. On a quarterly frequency, their data contains information on all macroprudential policy measures and microprudential measures that are "likely to have a significant impact on the whole banking system" that were enforced in EU member states between 1995 and 2018. For each measure, the data set provides information on a large number of characteristics.[4] Most importantly, it states whether a measure is an easing or a tightening of prudential policies or it has an ambiguous objective. Additionally, information on whether the measure has been introduced by national authorities or is the result of EU/EA legislation is provided. Regarding the time dimension, information on both the announcement as well as the implementation is given. Finally, the data set differentiates between eleven categories and 53 subcategories of regulation that can again be grouped into borrower- and lender-based measures as well as primarily micro- or macroprudential measures. The detailed information stems from questionnaires that were completed by national central banks and other supervisory authorities.

[3]While the participation of EA countries is obligatory, EU-members that are not part of the Euro-system are allowed to participate voluntarily.

[4]We solely focus on the characteristics that are relevant for our empirical analysis. A more detailed description of the data set is provided by Kleibl and Budnik (2018).

## 2.3 Evidence from Panel Analysis

The bulk of the empirical literature on macroprudential policy relies on panel estimation.[5] Consequently, panel analyses set a natural starting point for our investigation. Generally, these studies consider the overall macroprudential policy tightness of an economy and estimate its influence on credit growth and/or house price inflation. In contrast to that, our focus is on how the marginal effect of a particular policy tightening or easing evolves over time. Thus far, the empirical literature struggles to quantify these developments over time. Akinci and Olmstead-Rumsey (2018), for instance, justify their use of an index representing the overall macroprudential state of the economy with the argument that their empirical estimation is not capable of specifying the effect of a particular macroprudential loosening or tightening. More precisely, their approach would only allow them to estimate the impact in the quarter after a change in the macroprudential policy stance occurred. Furthermore, prudential policy measures conducted in consecutive quarters would introduce a bias on the estimation results when adjusting to prudential policy measures lasts longer than a quarter (time bias). Given that, in particular, (potential) borrowers are not expected to be informed about every change in the prudential policy stance, a full adjustment of the endogenous variable within a quarter is questionable. We propose an empirical method that is capable of dealing with these issues.

Put differently, this paper differs from the bulk of the literature by examining the role of flow variables rather than stock variables. To bridge the gap between our empirical approach and the one primarily observed in the literature, we nevertheless first apply the standard approach to our data set, where we look at the overall status of the prudential policy. Afterwards, we estimate how the marginal effect of a prudential easing and tightening develops over time.

### 2.3.1 Estimation with a Prudential Policy Index

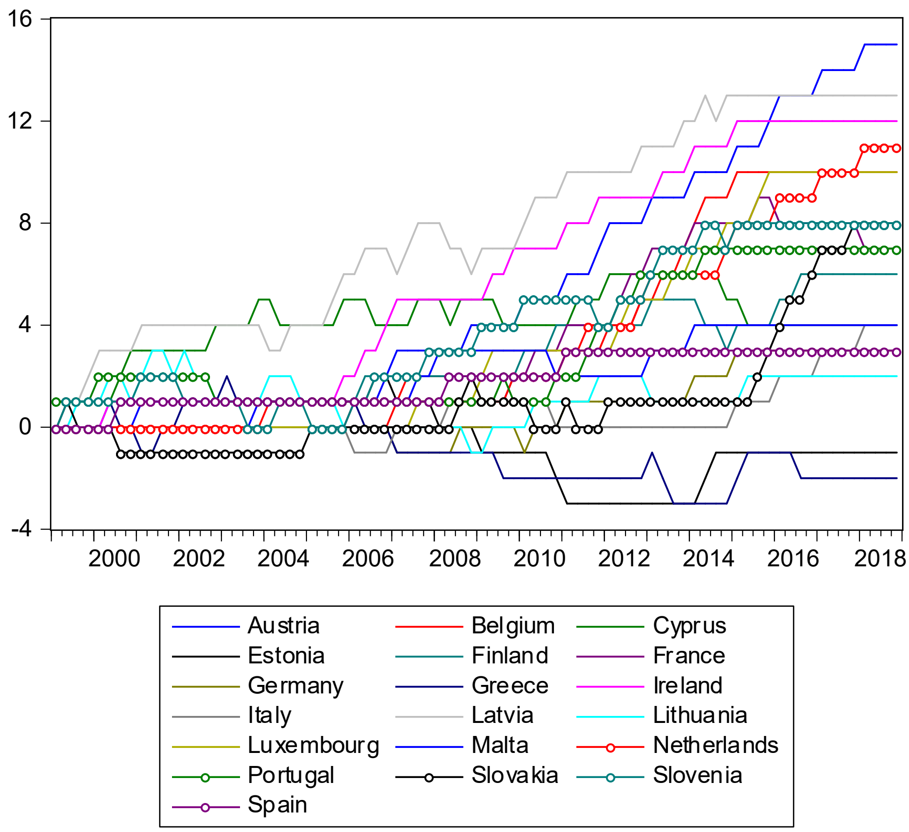
Our panel data set generally covers all 19 EMU member states from the date of accession to 2018:Q4 on a quarterly frequency. However, depending on the endogenous variables' data availability, the sample does not always cover every member state.

Following Akinci and Olmstead-Rumsey (2018), we introduce a Prudential Policy Index (PPI) that displays the tightness of the policy relative to a base period for every EA country. In our case, the establishment of the euro serves as the base

[5] These comprise, among others, Akinci and Olmstead-Rumsey (2018) and Cerutti, Claessens and Laeven (2017).

date. Hence, the PPI is zero at the start of 1999:Q1.[6] In every period in which policymakers tighten (loosen) at least one measure, the PPI increases (decreases) by one. As in Akinci and Olmstead-Rumsey (2018) and Cerutti, Claessens and Laeven (2017), the data set does not allow us to take a stand on the intensity of a policy change. In principle, one could also sum up the number of measures that became tighter (looser) within a quarter. However, since most of these simultaneous changes in different measures are, in fact, a package of measures, and we do not capture the intensity of changes, we refrain from that. When in a given quarter, no changes in the prudential policy stance are conducted, the PPI series remains unchanged. This also holds for periods where loosening of some measures and tightening of other measures happened simultaneously.

Figure 2.1: Prudential Policy Index across countries



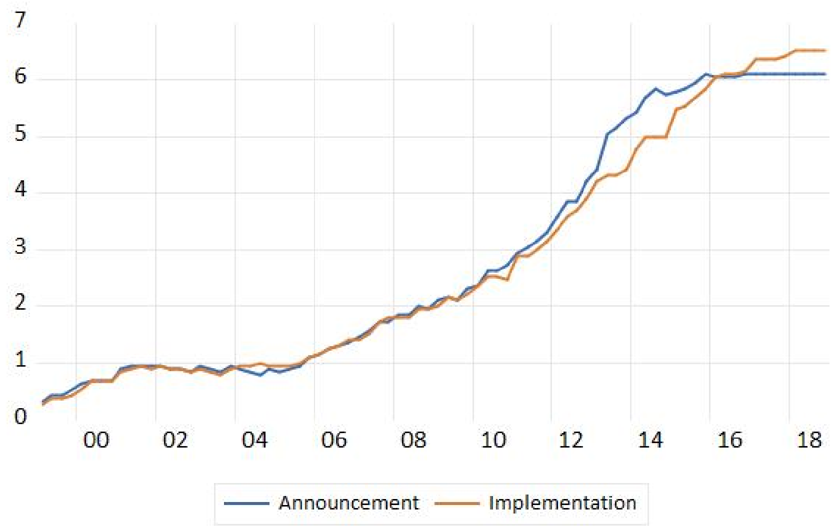

*Notes:* The horizontal (vertical) axis corresponds to years (the value of the PPI).

As outlined above, we distinguish between announcement and implementation. Hence, we separately create the PPI series for a case where the announcement date and a

[6]For reasons of comparability, this also holds for countries that entered the EMU after 1999.

case where the implementation date is decisive for constructing the PPI. Figure (2.1) displays the PPI according to the implementation date on a national basis. Overall, a strong tendency of a tightening over time is observed. Until 2018:Q4, Austria tightened the most relative to the base period with a PPI of 15, followed by Ireland and Latvia with a PPI of 13 and 12, respectively. On the lower end, Greece and Estonia appear to be the only countries with a somewhat looser prudential policy in 2018 in comparison to 1998:Q4 (net tightening of -2 and -1, respectively). The development of the EA mean PPI is outlined in Figure (2.2).[7] Hereby, we distinguish between the two PPI series. We again observe the tendency of a tighter prudential policy over time for both series. In line with our expectations, the announcement PPI series is generally a leading indicator for the implementation PPI series. However, since 2016:Q2 the implementation index exceeds the announcement index. The reasoning behind this is that several measures were announced at the same point in time, but their implantation date varied.[8]

Figure 2.2: Prudential Policy Indices - comparison of means




*Notes:* The horizontal (vertical) axis corresponds to years (the value of the PPI).

We follow the empirical literature and focus on the response of credit growth and house prices to prudential policy changes. Data on credit is available at the Bank for

[7]Note that the PPI series represents the mean of all observations, i.e. it is not a (GDP-)weighted average. However, an index that is based on a GDP-weighted average yields similar results.

[8]To be more illustrative, suppose that one country announces the tightening of two measures simultaneously. The series for national PPI announcements increases by one as a tightening of at least one measure was announced. In contrast to that, the PPI series for implementation will increase by one for each implementation. Hence, the total increase in the PPI is two, if the implementation dates of the two measures differ.

International Settlements (BIS). We consider the credit to the non-financial sector from all sectors at market value. We merge this data set with the ECB's data set on non-financial cooperation debt in order to reduce the number of missing values.[9]
Data on housing prices are more difficult to find. Concerning the real estate type, the considered area (capital city or the whole country) and the frequency, the BIS' data set is not consistent across all EA countries. The ECB's data set does not capture observations prior to 2005. As the number of treatment events is limited, we do not want to lose observations by further cutting the sample. Therefore, we use actual rentals for housing from the HICP as a proxy for house prices as rents are a fundamental determinant of the value of housing, see e.g. Brunnermeier and Julliard (2008) and Plazzi, Torous and Valkanov (2010). Specifically, the present value of its future rents determines the price of a commercial property from an asset pricing perspective. Besides the fundamentals, a bubble term drives house prices. Hence, rents can be thought of as a proxy for the underlying fundamental value of house prices only. Empirically, Manganelli, Morano and Tajani (2014) find that house prices affect rents in Italy. For the US, Gallin (2008) showed that the house price-to-rent ratio is a reliable indicator of the valuation in the housing market.
Additionally, we rely on a number of control variables. Namely, we include the national output gaps, the short-term money market rate and the CBOE Volatility Index (VIX) in the analysis below. The corresponding data sources are Eurostat and the Fred Database, respectively. The shadow rate by Wu and Xia (2016), which is available from 2003:Q3, allows us to deal with the zero lower bound (ZLB). From 1999:Q1 to 2003:Q3, the Eonia serves as our short term interest rate, which we receive from Thomson Reuters Datastream.[10]
Following Akinci and Olmstead-Rumsey (2018), our estimation equation can be described by (2.1). The quarter-on-quarter credit or rents growth rate for country $i$ at time $t$ is the endogenous variable which is regressed on a country-specific constant, its own lagged values, a number of control variables and the PPI, which we are primarily interested in. The error term is described by $u_{i,t}$. We reduce endogeneity as much as possible by analyzing the lagged values of the PPI. In a similar manner, we generally consider lagged values of the control variables. In line with Akinci and Olmstead-Rumsey (2018), we include three lags of the endogenous variable into our regression.[11] The (log) VIX is the only variable that is allowed to have a contemporaneous influence on credit growth since its value is determined by the US market. However, lagging the VIX does not substantially alter the results. In

[9]Priority is given to the BIS' data set.
[10]A complete list of all variables and their sources can be found in Table (2.6).
[11]The results are not sensitive to other lag lengths for the endogenous variable.

contrast to Akinci and Olmstead-Rumsey (2018), we consider GDP gaps rather than GDP growth rates because GDP gaps are better suited to capture the business cycle's current state. Finally, our last control variable is the (lagged) change in the short term interest rate with respect to the quarter one year ago.

$$Y_{i,t} = c_i + \sum_{j=1}^{3} \rho_j \cdot Y_{i,t-j} + \alpha \cdot VIX_{i,t} + \beta \cdot GDP_{t-1} + \delta \cdot interest_{i,t-1} + \gamma \cdot PPI_{i,t-1} + u_{i,t} \quad (2.1)$$

To further reduce the endogeneity of PPI in Equation (2.1), Akinci and Olmstead-Rumsey (2018) apply an Arellano-Bond[12] (AB) General Methods of Moments (GMM) estimator. However, this estimator is only unbiased when the number of countries ($N$) exceeds the time dimension ($T$), which is not the case here. A feasible alternative is a bias-corrected Least Square Dummy Variable (LSDV) estimator.[13] We adopt the bias-correction by Bruno (2005), which can also be applied to unbalanced panels.

Table 2.1: Panel estimation results for PPI (bias-corrected LSDV estimator)

| | $Y_t$ = Credit Growth | | $Y_t$ = Rents Growth | |
|---|---|---|---|---|
| Y, t-1 | 0.146*** | 0.147*** | 0.147*** | 0,147 |
| Y, t-2 | 0.090*** | 0.091*** | -0,031 | -0,031 |
| Y, t-3 | -0,013 | -0,013 | 0.155*** | 0.155*** |
| VIX (log) | -0,013 | 0,018 | -0,149 | -0,143 |
| GDP Gap, t-1 | -0,042 | -0,048 | 0.069*** | 0.069** |
| Pol. Rate chg., t-1 | 0,035 | 0,037 | -0,015 | -0,015 |
| PPI Impl., t-1 | -0.129*** | | -0,003 | |
| PPI Announc., t-1 | | -0.122*** | | -0,001 |

*Notes:* The panel estimation relies on the bias-corrected LSDV estimator by Bruno (2005). We include country-fixed effects and consider robust standard errors clustered by country. Significance on the 1%, 5% and 10% level are displayed by ***, ** and *, respectively.

The results of the bias-corrected LSDV estimator are displayed in Table (2.1). For the sake of comparison, we also present evidence for the Blundell-Bond estimator[14] (Table (2.2)) and for the non-adjusted LSDV estimator (Table (2.3)). The latter's results match those of the bias-corrected estimator quite well, indicating that the correction is not substantial. Credit growth is positively dependent on the previous two periods' credit growth, while all other control variables have no significant

[12]The dynamic panel estimator was introduced by Arellano and Bond (1991).

[13]In fact, Monte Carlo simulations by Judson and Owen (1999) indicate that the bias-corrected LSDV estimator is preferable to GMM estimators when $N$ is relatively small.

[14]The Blundell-Bond estimator is an extension of the AB estimator that performs better under a limited sample size, see Blundell and Bond (1998).

impact. These findings hold for all three models. Our variable of interest, namely the PPI, display the expected negative sign and is statistically significant on a confidence interval of 99%. An increase of the PPI Implementation series, for instance, reduces credit growth by 0.146% on average, indicating that tighter prudential policy regimes reduce credit growth in general. In line with our expectations, the impact of the PPI announcement series is of similar magnitude.

Table 2.2: Panel estimation results for PPI (Blundell-Bond estimator)

| | $Y_t$ = Credit Growth | | $Y_t$ = Rents Growth | |
|---|---|---|---|---|
| Y, t-1 | 0,108 | 0,110 | 0,136 | 0,137 |
| Y, t-2 | 0,071 | 0,072 | -0,023 | -0,023 |
| Y, t-3 | 0,004 | 0,004 | 0.167*** | 0.167*** |
| VIX (log) | 0,079 | 0,107 | -0,235 | -0,229 |
| GDP Gap, t-1 | -0,031 | -0,038 | 0.079** | 0.079** |
| Pol. Rate chg., t-1 | 0,020 | 0,022 | -0,020 | -0,020 |
| PPI Impl., t-1 | -0.148*** | | -0,005 | |
| PPI Announc., t-1 | | -0.143*** | | -0,003 |

*Notes:* The panel estimation relies on the estimator by Blundell and Bond (1998). We include country-fixed effects and consider robust standard errors clustered by country. Significance on the 1%, 5% and 10% level are displayed by ***, ** and *, respectively.

Table 2.3: Panel estimation results for PPI (LSDV estimator without bias-correction)

| | $Y_t$ = Credit Growth | | $Y_t$ = Rents Growth | |
|---|---|---|---|---|
| Y, t-1 | 0.146*** | 0.129*** | 0.129*** | 0.129*** |
| Y, t-2 | 0.091*** | 0.092*** | -0,032 | -0,032 |
| Y, t-3 | -0.013 | -0,010 | 0.155*** | 0.155*** |
| VIX (log) | -0,013 | 0,021 | -0,146 | -0,140 |
| GDP Gap, t-1 | -0,042 | -0,049 | 0.071*** | 0.070*** |
| Pol. Rate chg., t-1 | 0,035 | 0,038 | -0,015 | -0,015 |
| PPI Impl., t-1 | -0.129*** | | -0,003 | |
| PPI Announc., t-1 | | -0.123*** | | -0,001 |

*Notes:* The panel estimation relies on an LSDV estimator. We include country-fixed effects and consider robust standard errors clustered by country. Significance on the 1%, 5% and 10% level are displayed by ***, ** and *, respectively.

A somewhat different picture arises for rents. Growth of rents is positively influenced by its lagged value of orders one and three. Increases in global uncertainty, measured by the VIX, and changes in the short term money market rate, do not significantly affect rents. Higher output levels are associated with higher rents, as indicated by the significant GDP gap coefficient. This finding describes demand side effects on

the housing market. The effect of the PPI on rents is less pronounced than its effect on credit growth. Although the expected negative sign is observed, its impact is insignificant. A plausible explanation for this finding is that bank lending is only one of many determinants of house prices and rents. Therefore, a reduction of credit growth due to tighter prudential policies does not necessarily decrease rents one-to-one. Put differently, prudential policymakers have better control over credit growth than over rents. All results hold for both equations, i.e. regardless of whether the implementation or the announcement PPI is implemented in the model.
The interpretation of the results presented above is only valid when the endogenous variable fully adjusts within one quarter after the change. To see that, consider a case where a country tightens monetary policy but then eases twice in the subsequent two quarters. If the endogenous variable takes some time to adjust, the tightening effect would be attributed to a lower PPI. Hence, the estimated results of Equation (2.1) are biased. As we find no substantial difference between the announcement and the implementation index, it is tempting to conclude that the effects already appear after the announcement of prudential policies. However, this interpretation is misleading because we estimate the role of the prudential policy stance, which is by nature similar to announcement and implementation dates, and do not estimate effects after a change in the policy.

### 2.3.2 Impulse Responses to a Prudential Tightening/Loosening

In this section, we outline a way to correct for the time bias in Equation (2.1). This method further allows us to see how the endogenous variables respond to a prudential tightening or loosening over time. Yet, the method cannot solve the selection bias, which will be done in the following section.
It is common practice in the field of counterfactual analysis to make use of regression results to offset the effects of endogenous variables, see, e.g., Taylor (2007) and Mohaddes and Pesaran (2016). Building on that, we propose an iterative approach that disentangles the effects of a particular change in a prudential policy measure of the impact of preceding and subsequent policy changes. We proceed as follows. First, we introduce the underlying estimation equation. Afterward, we describe how we adjust results by the iterative approach.
In line with our research question, we want to assess how a prudential tightening (loosening) influences credit growth and rents over time. Hence, the left-hand-side of our estimation Equation (2.2) is given by the percentage change of the endogenous variable from before a shift in prudential policies until $q$ quarters after that shift. The explanatory variables we incorporate are country-fixed effects, a linear time-trend,

a set of control variables and two dummy variables that indicate policy tightening and loosening events, respectively. More precisely, $D^T_{i,t}$ ($D^L_{i,t}$) equals one whenever country $i$ announces the tightening (loosening) of at least one measure at time $t$. Since we correct for the time bias, lagging the dummy variables is not necessary. For the moment, we assume that the announcement and not the implementation of prudential policies moves the endogenous variable. In principle, one could separately add dummies for the implementation. Yet, the iterative algorithm outlined below does not converge towards a local minimum under these circumstances. In Section (2.4), we further disentangle the announcement from the implementation effect. The set of (lagged) control variables $X_{i,t-1}$ contains the change in the (shadow) short rate, output gap, year-on-year inflation, and the credit-to-GDP gap. All changes are expressed relative to the previous year. The former two variables are identical to those from Section (2.3.1). Inflation is taken from Eurostat. The credit-to-GDP gap series is derived from the BIS. Gaps in the time series are filled by the ECB's data set on "non-financial cooperation outstanding debt to GDP". Analogously to the BIS data, we receive gaps by applying an HP-filter with a $\lambda$ of 400,000.[15]

$$\frac{Y_{i,t+q} - Y_{i,t-1}}{Y_{i,t-1}} = c_i + \gamma_{1,q} \cdot D^T_{i,t} + \gamma_{2,q} \cdot D^L_{i,t} + \beta \cdot X_{i,t-1} + \delta \cdot t + u_{i,t} \tag{2.2}$$

The iterative approach proceeds as follows. First, we estimate Equation (2.2) using the biased-corrected LSDV estimator. In a local projections[16] style, we vary the time horizon of the change in the dependent variable. We assume that the endogenous variable fully adjusts within four quarters. Hence, we separately estimate Equation (2.2) for every possible $q \in \{0, 1, 2, 3, 4\}$ and always save the coefficients $\gamma_{1,q}$ and $\gamma_{2,q}$.[17] With those estimates at hand, we can calculate hypothetical values of the endogenous variables under the assumption that a particular shift in prudential policies had not happened. Thus, we can discern between two changes in the prudential policy stance conducted in a short period of time. We then manipulate the endogenous variable to offset the effects of prudential policy measures conducted earlier or thereafter. In an iterative process, we rerun all the regressions and readjust the endogenous variable until the adjustment is negligible. Our algorithm stops when the change in $\gamma_{1,q}$ and $\gamma_{2,q}$ for $q \in \{0, 1, 2, 3, 4\}$ is below 0.0001 for every parameter. To be more illustrative, consider the following example. Austria announces a tightening of one measure in 2010:Q1 and again in 2010:Q4. In the first round, Equation

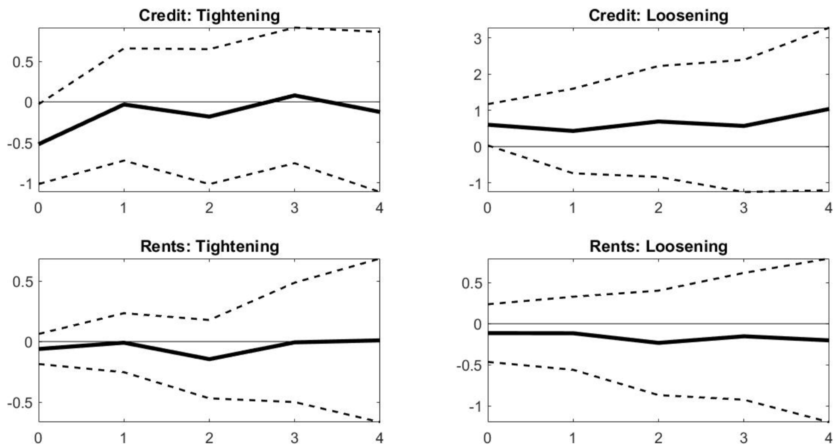
[15]See Table (2.6) for an overview of all variables used throughout this paper and their sources.

[16]Local projections were introduced by Jorda et al. (2005). Jorda, Schularick and Taylor (2013) apply this estimation technique to a panel data set.

[17]Note that, in contrast to local projections, the variable of interest is not a purely exogenous shock.

(2.2) is separately estimated for all permissible $q$. In that estimation, we refrain from these two tightening events' interaction effect on the endogenous variable. Obviously, this estimation is biased as, for instance, the movement in the endogenous variable in 2010:Q4 is, in fact, a combination of the responses to both tightening events. If the effects of these two tightenings exactly equal the average tightening effect and no white noise are present, the increase in the endogenous variable $\frac{Y_{i,t}-Y_{i,t-1}}{Y_{i,t-1}}$ in 2010:Q4 is given by $\gamma_{1,0} + \frac{\gamma_{1,3}-\gamma_{1,2}}{1+\gamma_{1,2}}$. The latter term describes the effect that the announcement in 2010:Q1 had on the credit growth between 2010:Q3 and 2010:Q4. Vice versa, the effect of the 2010:Q1 and the 2010:Q4 tightening in 2010:Q4 is $\frac{Y_{i,t}-Y_{i,t-1}}{Y_{i,t-1}} - \frac{\gamma_{1,3}-\gamma_{1,2}}{1+\gamma_{1,2}}$ and $\frac{Y_{i,t}-Y_{i,t-1}}{Y_{i,t-1}} - \gamma_{1,0}$. Hence, we ultimately will have an unbiased estimator, when we subtract the effects of previous and preceding measures. For the deduction process, we first consider the estimates for $\gamma_{1,q}$ and $\gamma_{2,q}$ from the initial estimations. We then iteratively reestimate Equation (2.2) and adjust the subtraction parameters until the convergence condition is met.

Figure 2.3: Panel estimation results: responses to changes in the prudential policy




*Notes:* The panel estimation relies on the bias-corrected LSDV estimator by Bruno (2005). We include country-fixed effects and consider robust standard errors clustered by country. The solid line represents the mean response, the 90% confidence bands are displayed via the dotted lines.

Figure (2.3) plots the mean estimator along with its 90% confidence bands for $\gamma_{1,q}$ and $\gamma_{2,q}$ as a function of $q$. The upper panel displays the average response of credit growth to shifts in the prudential policy stance. In line with our expectations, a tighter (looser) prudential policy stance decreases (increases) credit growth. The size of the effect is similar for tightening and loosening. It is only significant in the first period. The lower panel of Figure (2.3) shows the responses of rents to a change in prudential policy. In line with the estimations from Section (2.3.1), we observe no significant impact of prudential policies on rents. However, one has to be very cautious with the interpretation here as the LSDV approach might still suffer from the selection bias.

## 2.4 A Propensity Score Matching Approach

As described above, the LSDV panel analysis only mitigates the endogeneity issue. Least squares estimation might still suffer from a selection bias for two reasons. First, the least-squares estimator requires a linear relationship between the endogenous and exogenous variables with a known functional form, e.g., lag-structure. Biased estimates occur whenever the regression is based on an incorrect functional form. In contrast to that, the propensity score matching (PSM) approach used below does not require a precise functional form, which is of special interest in the field of prudential policies where the empirical literature is still in its infancy. Second, the least-squares estimator does not put high weights on those observations with similar economic fundamentals. It weighs observations higher that have a more equal distribution between receiving and not receiving the treatment, i.e., tightening and non-tightening of a prudential policy measure. As opposed to that, the PSM approach puts the highest emphasis on those observations with a high treatment probability that do not receive the treatment. This way, structural differences between observations with and without treatment are minimized.

We start by summarizing the methodology. Afterward, we estimate which macroeconomic variables influence the likelihood of a prudential tightening/loosening, i.e., the first stage of the PSM approach. Finally, we show how the endogenous variables (credit growth and house prices) react to a change in the prudential policy stance, i.e., the second stage of the PSM approach.

### 2.4.1 Methodology

Propensity score matching estimates the effect of a binary treatment on an endogenous variable, the so-called average treatment effect (ATE). In our case, we set up a binary variable for the tightening of prudential policy measures. Intuitively, we find a matching partner for every tightening event that has the same probability of tightening but does not alter its policy stance. The difference of the endogenous variable gives the impact of the tightening. Taking the average over all events results in the ATE. The binary variable $D_{i,t} = \{0, 1\}$ defines whether the observation of country $i$ at time $t$ belongs to the treatment ($D_{i,t} = 1$) or the control group ($D_{i,t} = 0$). Now let $Z_{0,i,t}$ be the outcome of the endogenous variable if country $i$ decides not to carry out any prudential policy action in $t$ and $Z_{1,i,t}$ be the outcome if policymakers tighten at least one measure. Apparently, we only observe one of these outcomes in every period, namely $Z_{1,i,t}$ for the treatment and $Z_{0,i,t}$ for the control group. Put differently, $Z_{1,i,t}|D_{i,t} = 1$ and $Z_{0,i,t}|D_{i,t} = 0$ are known, while $Z_{1,i,t}|D_{i,t} = 0$ and $Z_{0,i,t}|D_{i,t} = 1$ are unknown. Consequently, we are able to identify differences between the two observable variables, the left-hand-side of Equation (2.3).

$$\begin{aligned} E[Z_{1,i,t}|D_{i,t} = 1] - E[Z_{0,i,t}|D_{i,t} = 0] = E[Z_{1,i,t} - Z_{0,i,t}|D_{i,t} = 1] \\ +[E[Z_{0,i,t}|D_{i,t} = 1] - E[Z_{0,i,t}|D_{i,t} = 0]] \end{aligned} \tag{2.3}$$

The right-hand side of Equation (2.3) consists of the ATE, ($E[Z_{1,i,t} - Z_{0,i,t}|D_{i,t} = 1]$), and a selection bias, ($[E[Z_{0,i,t}|D_{i,t} = 1] - E[Z_{0,i,t}|D_{i,t} = 0]$). The former term describes the expected value of the differences between the observed outcome of the endogenous variable and the hypothetical outcome if the tightening had not occurred for each tightening event. Hence, this term measures the average effect of a policy tightening on the endogenous variable. The selection bias measures the part of $[E[Z_{0,i,t}|D_{i,t} = 1] - E[Z_{0,i,t}|D_{i,t} = 0]$ that stems from structural differences between the treatment and the control group. It is zero only if the sample is free of pre-treatment differences between the two groups. However, this is very unlikely in the case of macroeconomic variables as policy changes happen for a reason. For instance, a prudential tightening is expected to occur primarily when excessive bank lending or a mortgage boom is present. Not accounting for these circumstances leads to biased estimation results.

According to Rosenbaum and Rubin (1985), the selection bias is removed if treated variables are matched with control variables with the same probability of receiving the treatment. This exactly describes the intuition behind the PSM methodology. Suppose that a matrix of exogenous variables $X_{i,t-1}$ exists that determines whether a prudential tightening occurs. In our case, the exogenous variables could be changes in interest rates or the credit-to-GDP ratio. Estimating the likelihood of a tightening, i.e., the propensity scores can then be achieved by a logit model according to Equation (2.4). The influence of the exogenous variables are captured in $\Psi$ and $\alpha$ is a constant. However, changes in the prudential policy stance might again have an effect on $X_{i,t}$. In order to overcome this endogeneity issue in the first stage of the PSM, we consider pre-treatment variables, i.e., we lag the exogenous variables by one quarter.

$$ln\left(\frac{Pr[D_{i,t} = 1|X_{i,t-1}]}{Pr[D_{i,t} = 0|X_{i,t-1}]}\right) = \alpha + \Psi \cdot X_{i,t-1} + \epsilon_{i,t} \tag{2.4}$$

The ATE is then given by $E[Z_{1,i,t} - Z_{0,i*,t*}|Pr[D_{i,t} = 1] \approx Pr[D_{i*,t*} = 1]]$ where

$i*$ and $t*$ display country and time of the matching partner within the control group. Matching algorithms, such as nearest neighbor matching, ensure that the difference in the treatment probability between the treated variables and their matching partner is minimized. As we are interested in the development of the endogenous variable's response to a policy change over time, we estimate the ATE for different time horizons. Thus, we introduce the time horizon $q \in \{0, 1, 2, 3, 4\}$ into the endogenous variables $Z^q_{1,i,t}$ which is now given by Equation (2.5). In line with Section (2.3.2), $Y_{i,t}$ describes credit to the nonfictional sector or rents for country $i$ at time $t$.

$$Z^q_{1,i,t} = \frac{Y_{i,t+q} - Y_{i,t-1}}{Y_{i,t-1}} \tag{2.5}$$

Finally, we control for the time bias. For this task, we draw on the iterative approach described in Section (2.3.2). We escape from the time bias in the logit estimation by including the number of tightening and loosening events in the previous year as exogenous variables. Hence, the time bias-correction only considers the second stage of the PSM approach. This procedure's advantage is that we do not have to limit the sample size in the first step already. As before, we first run the PSM estimation without any adjustments. For each change in the prudential policy, the estimated coefficients allow us to control for other policy measures that also might affect the endogenous variable. We iteratively reestimate all equations and then update the coefficients we apply to control for other policy changes. We assume that convergence is achieved when the change of every coefficient between iterations is below 0.0001.

### 2.4.2 Logit Estimation

Since calculating reasonable propensity scores is crucial for the correct estimation of the ATE, the logit model deserves some special attention. The binary variable $D_{i,t} = \{0, 1\}$ in Equation (2.4) is one (zero) whenever a tightening of at least one measure and no loosening of any other measure was announced (whenever neither a tightening nor a loosening of any prudential policy measure was announced). Furthermore, we have to discard entries that would lead to biased estimation. We drop observations whenever the implementation of a before announced policy change occurs.

Via $X_{i,t-1}$ on the right-hand side of Equation (2.4), we estimate the influence of macroeconomic variables on the likelihood of a policy tightening. The list of exogenous variables covers the shadow (short) rate, GDP gap, headline inflation, credit-to-GDP gap and changes in rents. As before, changes refer to the previous year. These variables have already been introduced in the previous Section. Prudential policies are conducted by national authorities and EA/EU institutions. Hence, we always separately include national and EA-wide variables. Furthermore, systemic risk lets policymakers change the prudential policy stance. For the EA, we include changes in the Composite Indicator of Systemic Stress (CISS). On a national basis, the CISS is not available for every country. We overcome this issue by relying on the Country-Level Index of Financial Stress (CLIFS). We receive both indices from the ECB's statistical data warehouse. As described above, we lag all exogenous variables by one lag to minimize possible endogeneity issues. Finally, we incorporate a linear time trend as well as the number of tightenings and the number of loosenings in prudential policies over the previous year.

Table 2.4: Logit Estimation

| Variable | Coef. | p-value |
|---|---|---|
| Const. | -4.4508 | 0.0007 |
| Pol. Rate Change | 0.3027 | 0.0073 |
| Nat GDP Gap | -0.0115 | 0.8827 |
| EMU GDP Gap | -0.2537 | 0.1281 |
| Nat. Inflation | -0.1111 | 0.3874 |
| EMU Inflation | 0.2572 | 0.2602 |
| Nat. Cr/GDP Gap | 0.0223 | 0.1014 |
| EMU Cr/GDP Gap | 0.1821 | 0.0000 |
| Nat. Rents Change | -0.0368 | 0.3973 |
| EMU Rents Change | 0.4103 | 0.4603 |
| Nat. CLIFS Change | -0.3568 | 07181 |
| EMU CISS Change | 1.2023 | 0.0635 |
| Lin. Trend | 0.0291 | 0.0103 |
| # Loosening prev. Year | -0.1185 | 0.5390 |
| # Tightening prev. Year | 0.2690 | 0.0397 |

*Notes:* $D_{i,t} = \{0, 1\}$ is the dependent variable. It is one whenever a tightening of at least one measure and no loosening of any other measure was announced and zero otherwise. After all adjustments, we are left with 109 observations for $D_{i,t} = 1$ and 911 observations for $D_{i,t} = 0$.

The results of this exercise are outlined in Table (2.4). We find that the prudential policy primarily reacts to EA-wide developments. On a 10% significance level, the probability of a policy tightening raises with a higher EA credit-to-GDP gap and increasing systemic stress as indicated by the CISS. Moreover, prudential policy is empirically a complement of monetary policy, as both tend to tighten simultaneously. Finally, the likelihood of a policy tightening increases with the number of tightening events in the previous year. All these results are plausible. As all national variables do not significantly alter the likelihood of a policy tightening, it is tempting to conclude that solely EA variables lead to changes in prudential policy measures. However, this is too short-sighted as the national credit-to-GDP gap is just not significant.

### 2.4.3 Average Treatment Effect

With the propensity scores at hand, we estimate the ATE. However, we first have to discard some observations. For the logit estimation, we identified 109 announcements of a prudential policy tightening across EA member states. Since we are not able to estimate an ATE for loosening in the prudential policy, we exclude all periods when a loosening of at least one measure is present. Throughout the paper, we assume that the full adjustment to a prudential policy change happens within a year.[18] Therefore, we have to exclude the four periods after each loosening event as well. As the model does not allow us to specify the implementation effect of a measure announced before, we also exclude the four periods following these implementations. This exclusion further reduces the amount of policy tightening events to a total number of 45. Additionally, the matching approach requires that all observations must have a treatment probability in the interval $[0, 1]$. Since the probabilities are within 0.006 and 0.250, this requirement is met without any adjustment.

An observation is placed in the control group whenever neither a tightening nor a loosening has been announced or implemented in the respective quarter or the four quarters before. We count 911 observations in the control group. Finally, we set up an exclusion period for every treatment observation. More precisely, an observation in the treatment group can only be matched with an observation of the same country if they differ by at least one year. This is necessary as some exogenous variables in the first stage of the regression refer to changes over the previous year. Hence, two observations of a country within a year are also likely to have a similar treatment probability. Estimating the treatment effect out of these two observations is troublesome when the endogenous variable does not fully adjust within a quarter.

We present evidence based on three different matching approaches, i.e., the nearest neighbor, radius and kernel matching. For each of these identification strategies, we show that the results are robust to variations in the number of matching partners considered. For every observation in the treatment group, the nearest neighbor approach considers the $n$ observations in the control group with the smallest difference in the treatment probability. The radius matching considers all observations that are within a given radius around each treated variable. For these two matching

[18] As outlined below, this assumption is in line with our estimation results.

approaches, each observation's treatment effect is then given by the difference in the endogenous variable between the treatment variable and the average of the matching partners. Put differently, each identified matching partner receives an equal weight, while all other observations in the control group receive a weight of zero. In contrast to that, the kernel approach assigns positive weights to all observations. In this approach, variables in the control group that are more similar to the treated variable receive higher weights. As the names suggest, the weighting is achieved through a kernel function. We consider an Epanechnikov Kernel[19] as outlined by Equation (2.6), where $s$ is the adjusted difference in the propensity score between the treatment and the control variable. The adjustment is achieved by multiplying the difference with $\frac{1}{bw}$, where $bw$ is a pre-specified bandwidth. Our sample's highest probability of a policy tightening is 0.25, which serves as our benchmark bandwidth. We check the robustness of this bandwidth by considering lower and higher values, i.e., 0.05 and 0.5. Finally, all weights are rebased via the rule of proportion so that they sum up to one.

$$K(s) = \frac{3}{4}(1-s)^2 \tag{2.6}$$

The ATE is less sensitive to effects stemming from the matching partner by considering a higher number of matching partners, a wider radius or a larger bandwidth. On the other hand, relying on matching partners that have more different propensity scores also increases the likelihood that observations with structural differences are matched. For all matching methods, the ATE is then given by the average differences between the treated variables and the (weighted) average of their matching partner.
Before we turn to the estimation results, we first evaluate whether the matching approaches were actually able to remove substantial differences between the treatment and the control group. We present evidence based on the same variables that were included in the logit estimation.[20] Table (2.5) displays mean values for the treatment group and compares them with the mean of various control groups.[21] To save space, we only show results of one set-up for each matching method, i.e., we set the "number of nearest neighbors" $n$ to five, the radius to 0.01 and the bandwidth to 0.25. These are the median values of the models outlined below. Other set-ups yield similar results.
Before any matching, the untreated observations differ significantly from the treatment

[19]Other Kernel functions lead to similar results.

[20]We refrain from the number of loosening events in the previous year as we exclude all observations one year after a loosening in the policy stance.

[21]For the nearest neighbor and the radius matching, the control group consists of all observations that serve at least once as a matching partner. As outlined above, the kernel matching approach puts a different emphasis on all untreated observations. The mean of the untreated is then calculated via the rebased weights.

group among seven of 13 variables on a 5% significance level, see Table (2.5). The matching approaches were able to remove some of these differences. The radius matching and the kernel matching algorithm are the most successful. Only two variables differ significantly across the two groups. The nearest neighbor matching still exhibits structural differences among four variables. Although the matching approach reduces overall differences between control and treatment groups, some differences remain.

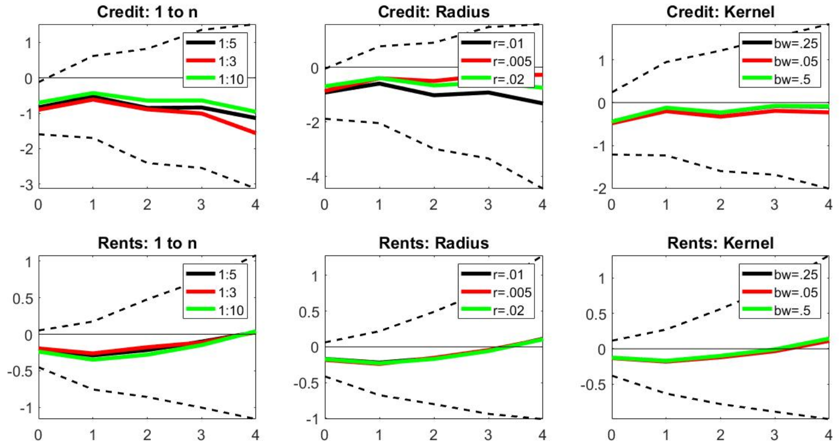
Table 2.5: Differences between treatment and control group

| | Prior to Matching | | Control Group After Matching | | |
|---|---|---|---|---|---|
| | Treated | Untreated | Near. Neighb. | Radius | Kernel |
| Pol. Rate Change | -0,1451 | -0,6283** | -0,6391** | -0,5606* | -0,5696* |
| Nat GDP Gap | -0,4382 | 0,0671** | -0,2574 | -0,2775 | 0,0236 |
| EMU GDP Gap | -0,2166 | 0,0079 | -0,1089 | -0,1321 | -0,0157 |
| Nat. Inflation | 1,8190 | 1,8354 | 2,0149 | 2,0328 | 1,9025 |
| EMU Inflation | 1,9049 | 1,6898** | 1,8116 | 1,9288 | 1,7747 |
| Nat. Cr/GDP Gap | 6,0560 | 1,6290*** | 2,4212** | 3,2993 | 2,1003** |
| EMU Cr/GDP Gap | 2,1345 | -0,4187*** | -0,3812*** | -0,3284*** | -0,2085*** |
| Nat. Rents Change | 1,2926 | 2,5485** | 2,3873* | 2,4023* | 1,9684 |
| EMU Rents Change | 1,6097 | 1,5344 | 1,6341 | 1,6737 | 1,5657 |
| Nat. CLIFS Change | -0,0035 | -0,0011 | -0,0045 | -0,0054 | -0,0023 |
| EMU CISS Change | -0,0553 | 0,0047* | -0,0079 | -0,0011 | -0,0039 |
| Lin. Trend | 41,9333 | 23,8982*** | 37,2959 | 34,6233** | 42,5138 |
| # Tightening prev. Year | 0,5111 | 0,5 | 0,2544** | 0,3151* | 0,4713 |

*Notes:* Comparison of structural variables between the treatment group and control groups. ***, ** and * display significant differences among the two groups on a 1%, 5% and 10% significance level, respectively.

We now evaluate the impact of a tightening in the prudential policy stance. The ATE as a function of $q$ and the corresponding 90% confidence bands are plotted in Figure (2.4). The confidence bands are generated via bootstrapping. The upper panel displays the effects on credit growth. We find that a tightening of the prudential policy stance reduces credit growth on average. The size of the effect varies with the matching method, but it is around 1% on impact for the majority of estimations. We see that credit adjusts within one quarter. In line with our expectations, we do not observe a hump-shaped response indicating that the effect does not vanish over time. Furthermore, only the initial impact is statistically different from zero in two of three considered matching methods. The lower panel of Figure (2.4) depicts the response of rents. Again we find that rents tend to decrease when prudential policy tightens. This finding is not significant on a 10% level. Compared to credit growth, the impact on rents is, additionally, of a smaller magnitude, i.e., around 0.25%. This is not surprising since house prices are also determined by other factors and adjust sluggishly.

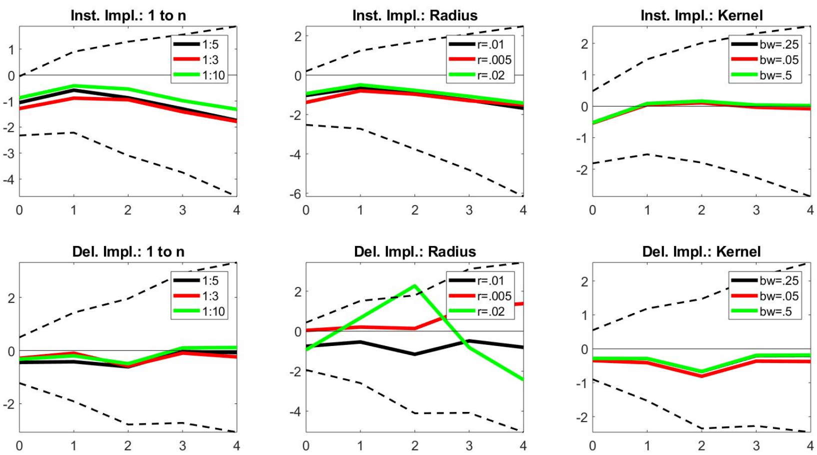
Figure 2.4: Average Treatment Effect




*Notes:* The ATE is estimated for the three different matching algorithms. The solid line represents the mean response, the 90% confidence bands are displayed via the dotted lines. The upper (lower) panel describes the response of credit (rents) growth.

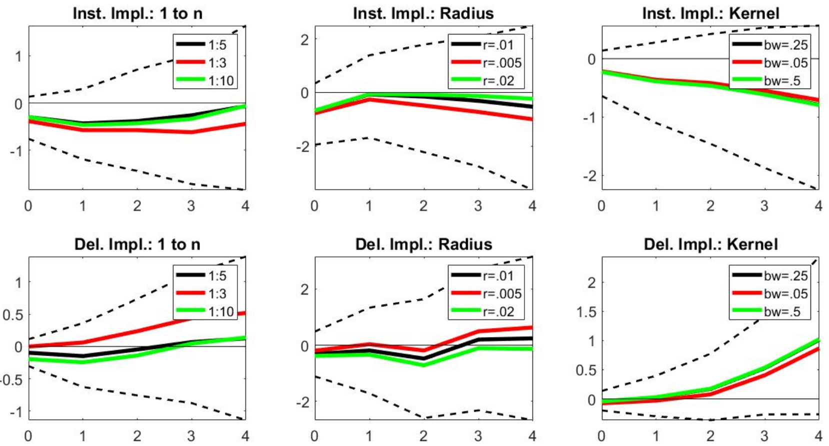
Figures (2.5) and (2.6) allow us to take a stand on whether the announcement or the implementation of a prudential policy measure is decisive. Therefore, we calculate two separate ATEs based on the timing of the implementation. While the first only considers events characterized by an instant implementation, i.e., announcement and implementation occur in the same quarter, the second focuses on those announcements accompanied by an implementation in the following periods (delayed implementation). For the latter group, the implementation lagged on average 3.3 quarter behind. We count 24 instant implementation and 21 delayed implementation events. Due to the smaller sample size, estimation uncertainty increases and error bands widen. Note that we do not alter the logit estimation. Thus, we implicitly assume that the macroeconomic circumstances influence the likelihood that a prudential policy tightening is announced but has no substantial impact on whether the implementation of the measure happens right away or with some delay. In any case, the instant implementation should reduce credit growth in the short to medium run, no matter whether the announcement or the implementation is decisive. From a theoretical point of view, the effect of a delayed implementation is unclear. On the one hand, banks might reduce lending at the time of the announcement so that they meet the regulatory criteria once they become binding. On the other hand, banks might increase lending in response to the announcement because they anticipate tighter policy in the future and thus move lending activities from the near future into the present. The latter channel is arguably of particular relevance for borrower based measures. Which of these two effects predominates is an empirical question in the end. According to Figure (2.5), there is indeed a tendency that instant implementations (upper panel) have stronger effects than delayed implementations (lower panel). This underpins that the implementation of a prudential tightening primarily moves credit growth. However, the differences between delayed and instant implementation are not statistically significant, possibly reflecting the small sample sizes. The fact that delayed implementations have smaller effects on credit growth on average might also indicate that the announcement of some measures, e.g., borrower based measures, leads to increases in credit growth in the short-run. If policymakers aim for the highest impact, they should not announce the implementation of future policy measures beforehand. However, this interpretation leaves out the fact that prudential policymakers, households and financial intermediaries are not playing a one-shot game. Similar to monetary policy, communication is potentially preferred when announcements reduce market uncertainty. According to Figure (2.6), the different effects on credit growth also tend to translate into different responses of rents. However, the responses of rents are again not significant.

Figure 2.5: Credit: Instant vs. Delayed Implementation




*Notes:* The ATE is estimated for the three different matching algorithms. The solid line represents the mean response, the 90% confidence bands are displayed via the dotted lines. The upper (lower) panel describes the response of instant (delayed) implementation.

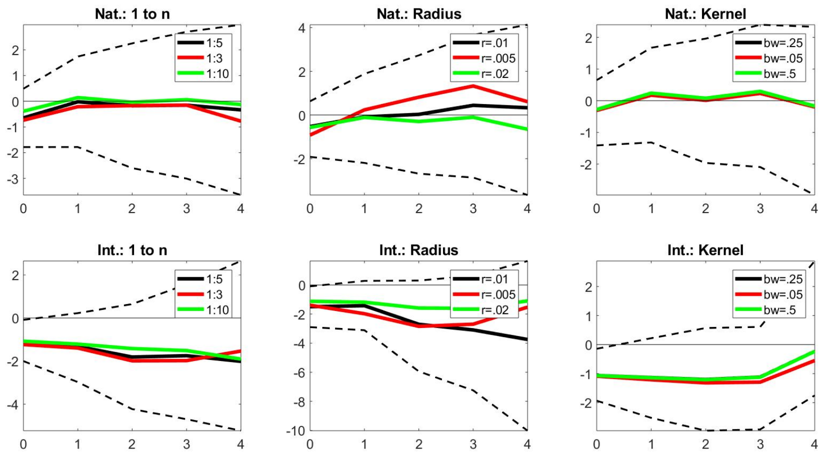
Figure 2.6: Rents: Instant vs. Delayed Implementation




*Notes:* The ATE is estimated for the three different matching algorithms. The solid line represents the mean response, the 90% confidence bands are displayed via the dotted lines. The upper (lower) panel describes the response of instant (delayed) implementation.

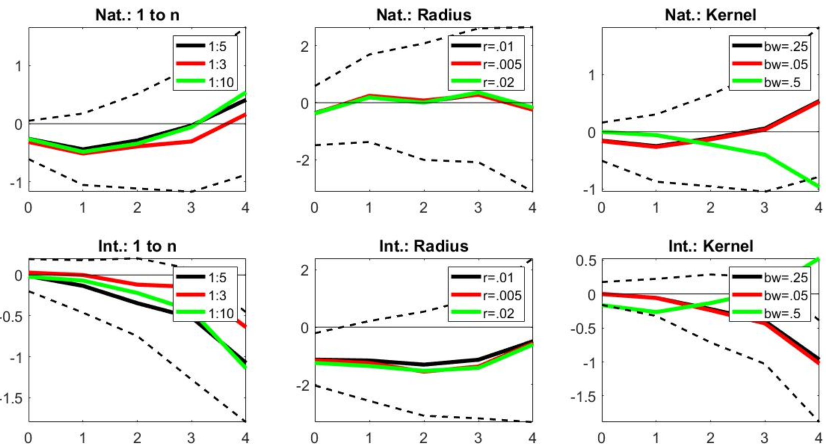
Figures (2.7) and (2.8) consider the collaboration between national authorities and EU/EA institutions. Hence, we compare the ATE of measures that were conducted solely by national authorities (32 observations) and the ATE for measures where both, EU/EA institutions and national authorities are decision-makers (13 observations). Again, the outcome is not clear-cut from a theoretical viewpoint. National authorities might know their economy better and therefore pick prudential policy measures that are better suited for the domestic market. However, missing international collaboration might open up the opportunity for globally active credit institutions to bypass tighter measures. Empirically, we find that credit growth responses are more pronounced when international institutions are behind these measures. Again for rents, the differences are less clear-cut, see Figure (2.8).

Figure 2.7: Credit: National vs. EU/EA Authorities




*Notes:* The ATE is estimated for the three different matching algorithms. The solid line represents the mean response, the 90% confidence bands are displayed via the dotted lines. The upper (lower) panel describes the response of changes stemming from national authorities only (a cooperation of national and EU/EA authorities).

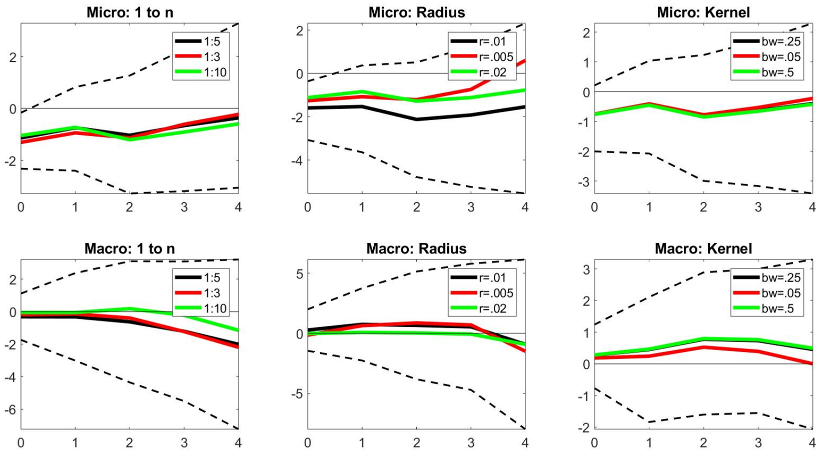
Figure 2.8: Rents: National vs. EU/EA Authorities




*Notes:* The ATE is estimated for the three different matching algorithms. The solid line represents the mean response, the 90% confidence bands are displayed via the dotted lines. The upper (lower) panel describes the response of changes stemming from national authorities only (a cooperation of national and EU/EA authorities).

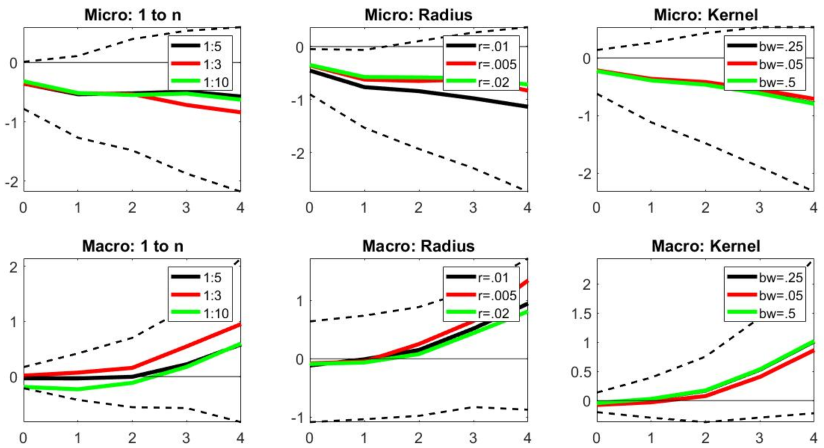
Finally, we differentiate between micro- (25 observations) and macroprudential policy measures (16 observations). In principle, we would expect that macroprudential policy has a greater impact since it is targeted towards the entire financial system. According to the Figures (2.9) and (2.10), we observe the opposite. Only microprudential measures are capable of reducing credit growth and rents. However, one has to keep in mind that the survey by Budnik and Kleibl (2018) only includes microprudential measures that are "likely to have a significant impact on the whole banking system".

Figure 2.9: Credit: Micro- vs. Macroprudential Policy




*Notes:* The ATE is estimated for the three different matching algorithms. The solid line represents the mean response, the 90% confidence bands are displayed via the dotted lines. The upper (lower) panel describes the response of changes stemming from microprudential (macroprudential) policy measures.

Figure 2.10: Rents: Micro- vs. Macroprudential Policy




*Notes:* The ATE is estimated for the three different matching algorithms. The solid line represents the mean response, the 90% confidence bands are displayed via the dotted lines. The upper (lower) panel describes the response of changes stemming from microprudential (macroprudential) policy measures.

## 2.5 Conclusions

In this paper, we study the role of prudential policies on bank lending and rents in the EA from an empirical point of view. We, therefore, draw on the data set provided by Budnik and Kleibl (2018). For each member state, this data set lists all dates of announcement and implementation for every macroprudential measure as well as for microprudential measures that are "likely to have a significant impact on the whole banking system".

Building on the established empirical literature of macroprudential policies, we first analyze how the economy's overall prudential stance influences rents and credit growth via panel estimation. We evaluate the tightness of the prudential policies compared to a base period by summing up over policy changes.[22] Our results suggest that tighter prudential policy reduces credit growth significantly. The effect on rents also displays the expected negative sign but is insignificant. However, one has to be cautious since the estimation is subject to a time bias and a selection bias.

We then subsequently remove the time bias and selection bias. The correction of the time bias allows us to evaluate how a prudential tightening or loosening affects the endogenous variable's development over time. Leaving aside the selection bias correction, we observe plausible negative mean responses of credit growth and rents to a prudential tightening.

Finally, we build on a propensity score matching approach to remove the selection bias. The propensity scores for prudential policy changes are based on a logit model. We include 14 exogenous variables that potentially determine the likelihood of a policy tightening, which consists of national and EA aggregate data. According to the estimation results, primarily the EA variables determine the prudential policy stance. More precisely, the credit-to-GDP gap, as well as changes in the CISS and the policy rate, are all positively related to the likelihood of tighter prudential policy. Based on these propensity scores, matching approaches find for each tightening event one or more partners from the control group that have a similar probability of tightening. We present evidence that matching approaches are indeed capable of reducing the selection bias. The propensity score matching's estimation results show that a prudential policy tightening significantly decreases credit growth by approximately 1% on average. The adjustment happens right away. Although rents also tend to decrease after a prudential tightening, we find no significant relationship. In line with our expectations, we find that the effects are stronger when policy measures are directly implemented and not communicated before. We further

[22]More precisely, we sum over all prudential tightening events and deduct all loosening events.

observe that measures that rest on an EA/EU basis but were implemented nationally have a more substantial impact on credit growth than measures conducted solely on a national mandate. This is a plausible result in highly integrated European markets. Finally, microprudential policy measures that are "likely to have a significant impact on the whole banking system" display somewhat more pronounced effects on credit growth than macroprudential policy measures.

This paper's lessons for policy-makers are manifold. First, policymakers are capable of altering bank lending. Second, policymakers have to internalize that the market adjusts fast, i.e., within one quarter, to changes in the prudential policy stance. Third, if policymakers aim for the biggest impact, they should not communicate policy changes before. However, this interpretation leaves out the fact that policymakers, financial intermediaries and agents are not playing a one-shot game. In fact, the announcement of measures might be welfare maximizing if communication reduces market uncertainty and volatility. Fourth, as measures based on an EA/EU mandate have a stronger impact, international cooperation is important.

Several expansions to this paper would be fruitful but are currently not feasible. In particular, one would prefer to further disentangle measures initiated on an EU-level from measures initiated on an EA-level. However, doing so would require us to incorporate aggregate data on EU and EMU levels into the logit model, which would lead to almost perfect collinearity. Moreover, we are not able to quantify measure-specific effects due to limited data availability. A starting point would be to decompose borrower-based from lender-based measures. However, the subset of borrower-based measures is too small to calculate any effect robustly. In fact, only nine of the 59 observations describe borrower-based measures. For the same reason, we cannot discern between tightening and loosening of prudential policy measures.

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## 2.6 Appendix

### Data Sources and Definitions

Table 2.6: Data Sources and Definitions

| Data Series | Source | Description |
|---|---|---|
| Bank Assets to GDP | ECB | |
| CBOE Volatility Index VIX | FRED St. Louis | |
| Credit | BIS | Credit to the non-financial sector |
| Credit | ECB | Non financial cooperation debt |
| Credit-to-GDP | BIS | Credit to the non-financial sector |
| Credit-to-GDP | ECB | Non financial cooperation debt to GDP |
| Eonia | Datastream | Euro Over Night Index Average |
| GDP GAP | Eurostat | |
| House Prices | BIS | |
| Inflation | Eurostat | |
| Real Effective Exchange Rate | BIS | Broad Index |
| Shadow (Short) Rate | Wu and Xia (2016) | |

# 3 The Aggregate and Country-Specific Effectiveness of ECB Policy: Evidence from an External Instruments VAR Approach

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Presenter: Lucas Hafemann.

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Presenter: Lucas Hafemann.

# The Aggregate and Country-Specific Effectiveness of ECB Policy: Evidence from an External Instruments VAR Approach

Lucas Hafemann[*,1] Peter Tillmann[*,2]

This paper studies the transmission of ECB policy, both at the aggregate euro-area level and the country level. We estimate a VAR model for the euro area in which monetary policy shocks are identified using an external instrument that reflects unexpected changes in the policy stance. For that purpose, we use changes in German bunds at meeting days of the Governing Council and selected intermeeting announcements. We also decompose policy shocks into pure policy surprises and information shocks. The resulting impulse responses are robust with respect to the choice of the instrument. Expansionary monetary policy affects prices and real activity but remains ineffective in pushing credit and stock markets. We show that pure policy shocks, i.e., shocks net of the new information revealed on meeting days, also have a significant effect on credit and stock prices. The identified monetary policy shock is then put into country-specific local projections in order to derive country-specific impulse responses. The transmission is heterogeneous across member countries with credit and financial markets being unevenly affected by monetary policy.

**Keywords:** Euro area, VAR, external instrument, local projections, monetary policy.

**JEL classification:** E52, E44, E32.

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## 3.1 Introduction

In the aftermath of the 2008/09 financial crisis and the subsequent European debt crisis, the European Central Bank (ECB) adopted a series of unconventional policy measures. More precisely, with short-term interest rates at the effective lower bound, the ECB used unconventional monetary policy such as the Asset Purchase Programme (APP) and the Targeted Longer-Term Refinancing Operations (TLTROs) to provide additional stimulus. Given the persistently low level of inflation and the sluggish recovery despite several years of expansionary monetary policy, the assessment of ECB policy is controversial. Only very recently, the recovery in the euro area gained momentum. Since 2008 analyzing monetary policy has become more difficult as the overall policy stance is no longer appropriately summarized by the short-term policy rate. In fact, the ECB uses several instruments at the same time. Moreover, with a large share of monetary policy being transmitted through asset markets and this share becoming larger over the recent years, identifying monetary policy shocks has become more difficult. The traditional triangular identification scheme applied to vector autoregressive (VAR) models that imposes restrictions on the contemporaneous interaction among the variables, is not suitable with financial data. Sign-restrictions, a popular alternative to the Cholesky ordering, require imposing more or less controversial restrictions onto the dynamic interaction.
In this paper, we study the monetary policy transmission in the eurozone, both at the aggregate euro area level and the disaggregated country level. For that purpose, we use an external instruments VAR approach to identify an ECB policy shock. The external instruments approach, which has recently been made popular by the work of Stock and Watson (2012), Mertens and Ravn (2013) and Gertler and Karadi (2015) identifies the simultaneous dynamics of monetary policy and asset prices with the help of the behavior of an instrument on central bank meeting days. The assumption is that around an ECB announcement, the instrument reflects only the policy surprise, which is orthogonal to other potential shocks driving the VAR system.

Based on the identified policy shock, we make the following contributions: First, we provide evidence on the effects of a monetary policy shock at the aggregate euro area level for a full 2002-2016 and a post-crisis sample. Expansionary monetary policy affects consumer prices and real activity and leads to a depreciation of the euro in real terms. While the shock also compresses the corporate bond spread, monetary policy remains ineffective in pushing credit and stock markets.

Second, we take account of the recent literature on information shocks, highlighting the fact that policy decisions by central banks also reveal information about the central bank's assessment of the economy (Nakamura and Steinsson (2018), Jarocinski and Karadi (2018) and Miranda-Agrippino and Ricco (2021)). Under incomplete information, these information shocks are distinct from the pure monetary policy component of shocks. We use a principal component analysis to decompose the monetary policy shock into an information shock and a pure policy shock and find plausible impulse responses to both shocks. Based on this decomposition, we are able to show that the baseline results remain robust when we exclude the information revealed on meeting days from the monetary policy surprise.

Third, we use the identified euro area policy shock to estimate several country-specific impulse response functions from local projections a la Jordà (2005). This provides us with the effects of the common monetary policy on individual countries and excludes the feedback from the country level to ECB policy. The assumption is that the ECB is, in line with its mandate, directing policy to the euro area aggregate, not to specific countries. The results show homogenous cross-country responses for consumer prices and industrial production, but heterogeneity in the effects of monetary policy across members on unemployment, credit and the stock market. In several countries the transmission through equity prices and through the banking system in terms of bank lending is severely dampened. The cross-country heterogeneity in the effects of bank lending and stock markets reflects the insignificant responses of both variables at the euro area level.

Our project connects several strands of the recent literature: Hachula et al. (2019) and Andrade et al. (2016) also use an external instruments approach to estimate euro area VAR models. However, their focus is different. The first paper estimates the effects of monetary policy shocks on fiscal policy variables in the euro area and studies whether fiscal discipline deteriorates after a monetary policy easing. The authors indeed find an increase in public expenditure after an expansionary policy shock. Andrade et al. (2016) focus on the ECB's Asset Purchase Programme, implemented since January 2015.[1] Two other recent papers, namely Cesa-Bianchi et al. (2016) and Ha (2016), use the external instruments approach for shock identification in an open economy VAR model and put the shock series into local projections.[2]

Furthermore, Wieladek and Pascual (2016) use a Bayesian VAR model with a battery of alternative identification schemes to study the euro area in 2012-2016. Counterfactuals for the euro area and the country level, respectively, show that

[1] A very useful survey of the transmission channel of unconventional ECB policy is provided by Fiedler et al. (2016).

[2] Altavilla et al. (2015) construct an indicator of credit supply tightening in the euro area and include it as an external instrument in a VAR model.

monetary policy has a very large effect. Since January 2015 it has lead to real GDP being 1.3% higher than in the absence of Quantitative Easing (QE). The same policy has benefitted Spain the most and Italy the least. Boeckx et al. (2017) use a sign-restricted VAR model to study the effects of unconventional monetary policy shocks that drive up the ECB's balance sheet.[3] Based on a Bayesian VAR model, Mandler et al. (2016) provide evidence for heterogeneous ECB policy transmission across the four largest economies of the euro area.

While the previously mentioned papers work with monthly or quarterly data, Fratzscher et al. (2016) use daily data to study the responses of a broad range of asset prices to ECB announcements prior to 2013. They find that unconventional policy boosts asset prices and spills over to other economies' equity markets but not to other bond markets. The work by Burriel and Galesi (2018) also focuses on euro area and country-specific effects of policy. The authors estimate a Global VAR model for the euro area which allows for spillovers among euro area countries. They find these intra-EMU spillovers to be sizable. In addition, they document a large heterogeneity of cross-country effects of monetary policy shocks.

This paper proceeds as follows: section two outlines the VAR model with an external instrument, which is our benchmark model, as well as the data used. The section also discusses our findings for the aggregate euro area, presents results for variables that describe specific transmission channels of monetary policy and also introduces the decomposition of policy surprises into pure policy shocks and information shocks. Section three introduces the local projections approach and discusses the country-specific results. Section four draws on these findings and discusses policy implications.

## 3.2 A euro area VAR model with an external instruments

### 3.2.1 Methodology

In this subsection, we describe how we combine the conventional VAR methodology with the event study approach. We build upon the methodology of Stock and Watson (2012), Mertens and Ravn (2013) and Gertler and Karadi (2015) in order to overcome the problems of endogeneity without imposing sign or zero restrictions. The endogeneity issue is particularly relevant for financial variables, which are

[3] Hristov et al. (2014), Altavilla et al. (2016) and De Santis (2016) provide additional evidence on selected ECB programs, such as the OMT program and the Asset Purchase Programme, respectively.

supposed to react instantly to a monetary policy shock. In line with e.g. Gambetti and Musso (2017), we expect unconventional monetary policy to influence financial variables. Therefore, a Cholesky ordering can potentially provide misleading results. It is also hard to argue in favor of sign or zero restrictions. Upon imposing restrictions, presumptions about the behavior of the included variables have to be made. This is problematic in the case of unconventional monetary policy, where we know very little about its transmission. However, under the assumption that an accurate instrument can be found, we are able to capture the transmission of the complete set of monetary policy tools.

Our goal is to derive the structural VAR model according to Equation (3.1)

$$S^{-1}Y_t = C + \sum_{j=1}^{p} B_j Y_{t-j} + \sum_{k=0}^{q} D_k X_{t-k} + u_t. \tag{3.1}$$

Hereby, $Y_t$ represents the endogenous and $X_t$ the set of exogenous variables at time $t$. While $C$ captures constants, the matrices $B_j$ and $D_k$ contain the coefficients on the lags of the endogenous and exogenous variables up to lag length $j$ and $k$, respectively. The simultaneous effect of one endogenous variable to another is captured by $S^{-1}$ and $u_t$ stands for the vector of error terms.

Due to the endogenous nature of the variables in $Y_t$, we are not able to solve the structural VAR uniquely. Hence, we first estimate the reduced form VAR, which results after multiplying each side of Equation (3.1) by $S$

$$Y_t = S \cdot C + \sum_{j=1}^{p} S \cdot B_j Y_{t-j} + \sum_{k=0}^{q} S \cdot D_k X_{t-k} + \varepsilon_t. \tag{3.2}$$

The reduced form innovations are then given by Equation (3.3)

$$\varepsilon_t = S \cdot u_t. \tag{3.3}$$

Here $S$ is a square matrix with the dimension equal to the number of endogenous variables. The $i$-th column in $S$ captures the response of the vector of reduced form innovations, $\varepsilon_t$, to an increase in the $i$-th element of the matrix of structural shocks $u_t$. As we are only interested in the responses to a structural monetary policy shock $u_t^{MP}$, we just have to identify the column $s$ in $S$ that captures the impact of $u_t^{MP}$ on the vector $\varepsilon_t$. Now let $\varepsilon_t^{MP}$ be the reduced form innovation of the monetary policy equation and $s^{MP}$ be the element of $s$ that describes its response to the structural shock, $u_t^{MP}$, such that Equation (3.4) holds

$$\varepsilon_t^{MP} = s^{MP} \cdot u_t^{MP}. \tag{3.4}$$

Accordingly, $\varepsilon_t^q$ and $s^q$ are reduced form error terms and the respective elements in $s$ that correspond to other variables

$$\varepsilon_t^q = s^q \cdot u_t^{MP}. \tag{3.5}$$

Solving for $u_t^{MP}$ in Equations (3.4) and (3.5) leads to

$$u_t^{MP} = \frac{\varepsilon_t^{MP}}{s^{MP}} = \frac{\varepsilon_t^q}{s^q}, \tag{3.6}$$

which can be rearranged to

$$\varepsilon_t^q = \frac{s^q}{s^{MP}} \varepsilon_t^{MP}. \tag{3.7}$$

Finally, with the reduced form error terms as both the dependent and the explanatory variable, respectively, an estimate for $\frac{s^q}{s^{MP}}$ can be found. In order to overcome the possible endogeneity of $\varepsilon_t^q$ and $\varepsilon_t^{MP}$, we apply a two-stage least squares approach. From the first stage we receive $\widehat{\varepsilon_t^{MP}}$ as an estimate that only captures changes in monetary policy that do not stem from a simultaneous change in $\varepsilon_t^q$. In the second stage, we then simply run the following OLS regression

$$\varepsilon_t^q = \frac{s^q}{s^{MP}} \widehat{\varepsilon_t^{MP}} + \xi_t. \tag{3.8}$$

Given these estimates and the variance covariance matrix of the reduced form VAR model, we are able to uniquely identify all components of $s$. The crucial point in this framework is to find an accurate instrument $Z_t$ which is, by definition, correlated with $\varepsilon_t^{MP}$ but orthogonal to $\varepsilon_t^q$.

### 3.2.2 Data

For our baseline euro-wide model the vector of the endogenous variables consist of the log of industrial production (excluding construction), the log of the Harmonized Index of Consumer Prices, a corporate bond spread and the (shadow) short rate.[4] Following Sims (1992) we further add (the log of) oil prices as an exogenous variable in order to avoid the price puzzle.

Prior to the financial crisis, the ECB conducted Open Market Operations in order to move the key policy rate. With the zero lower bound (ZLB) and the introduction of unconventional monetary policy, the ECB extended its policy toolkit. It is for this reason that we use the (shadow) short rate provided by Wu and Xia (2016) for the

[4] In the baseline model, the corporate bond spread is the spread between the yield on BBB rated and AA rated bonds. However, spreads between corporate bonds with other ratings lead to similar results.

interval available (i.e. from 2004:09 until the end of our sample) as the measure of the monetary policy.[5] Until 2004:08 the EONIA rate represents the monetary policy stance, which we receive from Thomson Reuters Datastream. We generally draw on seasonally adjusted data for the changing composition of the EMU. Financial variables that are not expected to contain seasonal patterns are not adjusted. A complete list of all variables, their adjustment and their sources can be found in Table (3.3). The sample consists of monthly data from 2002:01 until 2016:10.[6]

We include six lags as suggested by the Akaike information criteria and the final prediction error. However, as outlined below, choices of other lag lengths lead to similar results.

After estimating the baseline four-variable model, we add a fifth variable to our baseline model to shed light on several aspects of the transmission process. This fifth variable is taken from the following list of variables: euro area government bond yields, the unemployment rate, the log of the real exchange rate, the log of the Euro Stoxx 50, the log of the rent component of the HICP, the log of the loan volume granted by financial institutions and the net percentage change of credit standards and credit demand, both obtained from the Bank Lending Survey.[7]

### 3.2.3 Choosing an instrument

The choice of the instrument deserves special attention. We use changes in the German 10-year government bond yield on meeting days and a small number of other selected dates as the instrument.

The rationale behind the use of daily changes, rather than intra-day data, lies in the timing of ECB communication on meeting days of the Governing Council. The press release at 13:45 CET on every meeting day is followed by a press conference at 14:30 CET. Since our instrument has to capture the market response to the press conference as well, we cannot apply the widely used 30-minutes window.[8] The data

[5]We also apply the shadow rates provided by Krippner (2012) and Lemke and Vladu (2017) as well as the zero-coupon 1-year German government bond rate. As it will be outlined below, the results from the three different short rates are indeed complementary.

[6]Data from the ECB's Bank Lending Survey as well as loan data are only available from 2003 onwards. Hence, a shorter sample size is used for models containing these variables.

[7]Within the Bank Lending Survey the banks answer whether they tightened lending standards "considerably", "somewhat", eased "somewhat" or "considerably" or left the standards unchanged. The net percentage change is the difference in the percentage of banks that tightened their lending standards (either "somewhat" or "considerably") and the share of banks that eased them. Accordingly, the net percentage change in the credit demand is the share of banks that expect an increase in the demand for loans (either "considerably" or "somewhat") minus the share that expect a decrease in the demand.

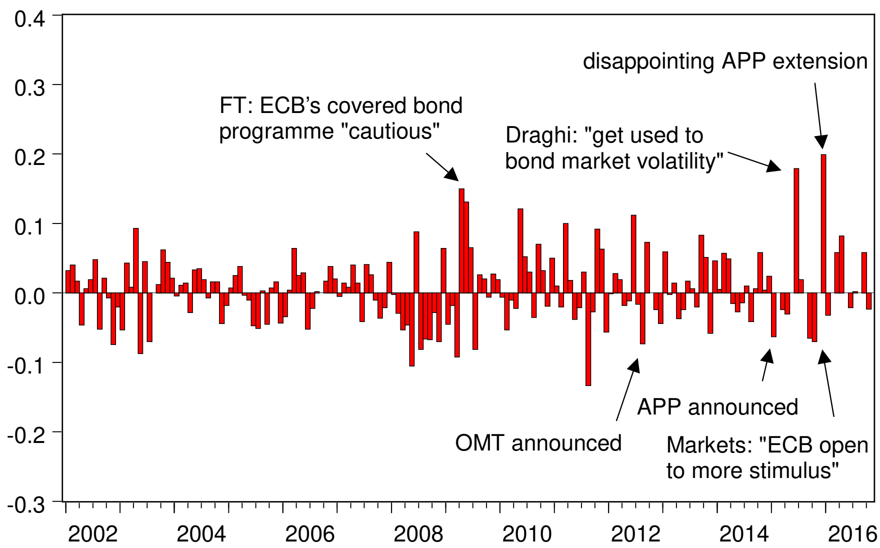
[8]In fact, Gürkaynak et al. (2005) find that daily changes in Federal fund futures on FOMC meeting days are akin to changes in a 30-minutes window around the release in the time span

of all external instruments stems from Thomson Reuters Eikon.
The financial crisis and the subsequent European debt crisis opened up an interest-rate spread between government bonds of various euro-area countries. While the yield on German government bonds serves as a risk-free rate throughout the entire sample, the status of government bonds of other countries switches from a risk-free to an exposed asset. With the choice of German government bonds, we avoid the issue of a structural break within our instrument variable. Furthermore, we consider 10-year bonds since our applied instrument also has to reflect changes in investors' expectations through unconventional monetary policy measures such as forward guidance.
Our identification method rests on the efficient market hypothesis (EMH). The EMH states that movements in asset prices only appear if new information is received. Thus, under the assumption that news other than the monetary policy decisions on the meeting days and the selected special events are white noise, the changes in German bond yields on these days represent changes in the monetary policy stance. For example, an increase in the German bond yield on these days, i.e. a positive surprise component, reflects a monetary tightening.
With the adoption of unconventional policies, important news about monetary policy also emerged on non-meeting days. Hence, we supplement the set of meeting days by three additional events. These are the announcement of the two tranches of the Securites Markets Programme (SMP) on 05/10/2010 and 08/07/2011, respectively, as well as President Draghi's "Whatever-it-takes"-speech on July 26, 2012. The monthly series for our instrument consists of the change in German yields on these specific days, that is if the Governing Council meets on one Thursday in a given month, the yield change on this day is used as the monthly entry in the instrument series. If there is both a Governing Council meeting and one of the additional events in a given month, we sum up the yield changes on these two days in order to get an estimate for the surprise component of that month.
This measure for the monetary policy stance has several advantages. First, the surprise component serves as a consistent measure for the entire monetary policy toolkit. With the ECB adopting unconventional policies, it extended its set of policy instruments. By having one measure reflecting the entire set of policy instruments, we do not face the problem of disentangling the effects of each instrument, which is particularly challenging as those have been used simultaneously.
Second, the focus on market reactions allows us to directly measure the unanticipated

from 1994 - 2004. Thus, they conclude that "... the surprise component of monetary policy announcements can be measured very well using daily data."

part of a policy change. This is better suited for identifying a policy shock, as according to the EMH only those should influence asset prices. For example, an increase in the interest rate that is lower than expected is recognized as an expansionary monetary policy in the view of market participants. Finally, the external instruments approach clearly defines an unexpected monetary policy shock, which is the starting point of every analysis within the VAR model.

Figure 3.1: Monetary Policy Surprises Obtained from 10-Year Bunds




*Notes:* Policy surprises are defined as the change in the yield on 10-year German bunds on ECB meeting days and selected other days. This series is used as an external instrument in the VAR identification. The annotation refers to the Financial Times (FT), the Outright Monetary Transactions (OMT) program and the Asset Purchase Programme (APP).

The series of the surprise component from 2002 until 2016 is plotted in Figure (3.1). As the surprise component fluctuates around zero, it can be concluded that there is no systematic bias in the market expectations.[9] The largest swings are found after the financial crisis in 2007. President Draghi's remark "get used to market volatility" on June 2015 and the disappointment about the size of the additional stimulus adopted in December 2015 account for the peaks in the surprise component. In contrast, the announcements of the Outright Monetary Transactions (OMT) program in September 2012 and APP in January 2015 are reflected in negative surprises. In other words, monetary policy was surprisingly expansionary.
Before we turn to the results of our VAR model, we check if the considered instrument

[9]On a ten percent significance level, a $t$-test confirms that the mean of the surprise component is not different from zero.

is accurate. First, we test for the information content of the instrument in an event study. We run the regression

$$\Delta y_t^{daily} = \alpha + \beta \cdot \Delta Z_t^{events} + \epsilon_t, \tag{3.9}$$

where the daily changes in asset prices, $y_t^{daily}$ are regressed on a constant and the surprise component, i.e. the changes in the German 10 year bond yield $Z_t^{events}$, using OLS. For this estimation we *only* consider the selected events, i.e. meeting days of the Governing Council and three selected special events, which leaves us with a total of 175 observations. The list of dependent variables consists of the log of the U.S. dollar exchange rate to the euro, the Euribor futures rate[10] and the corporate bond spread[11].

Table 3.1: Monetary Policy Surprises in an Event Study

| $y_t$ | | coef. | $p$-value |
|---|---|---|---|
| (log) Exchange Rate | $\hat{\alpha}$ | 0.000 | 0.43 |
| | $\hat{\beta}$ | 0.071 | 0.00 |
| Euribor Future | $\hat{\alpha}$ | -0.006 | 0.15 |
| | $\hat{\beta}$ | 0.890 | 0.00 |
| Corporate Bond Spread | $\hat{\alpha}$ | -0.005 | 0.12 |
| | $\hat{\beta}$ | 0.192 | 0.00 |

*Notes:* Results from an event study regression of $y_t$ on policy surprise series with the slope coefficient $\beta$ and a constant $\alpha$.

The results of the regressions are presented in Table (3.1). An unexpected increase in the government bond yield on meeting days, i.e. a surprise tightening, leads to an appreciation of the euro and increases in the Euribor future and the corporate bond spread. This suggests that changes in the German bond yield indeed contain information about unexpected changes in the ECB's monetary policy stance.

We further evaluate the properties of the instrument by testing for the occurrence of a weak instruments problem. The explanatory power of the instrument can be examined by regressing the reduced form VAR residuals of the monetary policy equation on a constant and the external instrument. As described by Li and Zanetti (2016), this equals the first stage in our two-stage least squares regression from Equation (3.8). For the changes in the German 10 year bond yields, the

[10] At any point in time, we consider the future that is the $8^{th}$ next to deliver. Note that the first six delivery months are consecutive in time. Given that the subsequent delivery months, namely March, June, September or December, settle on a quarterly frequency, the delivery of the future that we consider is roughly in one year. Our presented results are robust to other continuation futures.

[11] The corporate bond spread presented here is the spread between A and BBB rated bonds.

corresponding F-statistic in the baseline case is 10.44. Following Stock et al. (2002), a value for the F-statistic lower than ten indicates a weak instrument issue. With the German bond yields avoiding the weak instrument problem and showing plausible results for the event study regression, we are confident about our choice of an accurate instrument. Hence, we are able to estimate the impulse responses from our VAR model. The results are discussed below.

### 3.2.4 Results

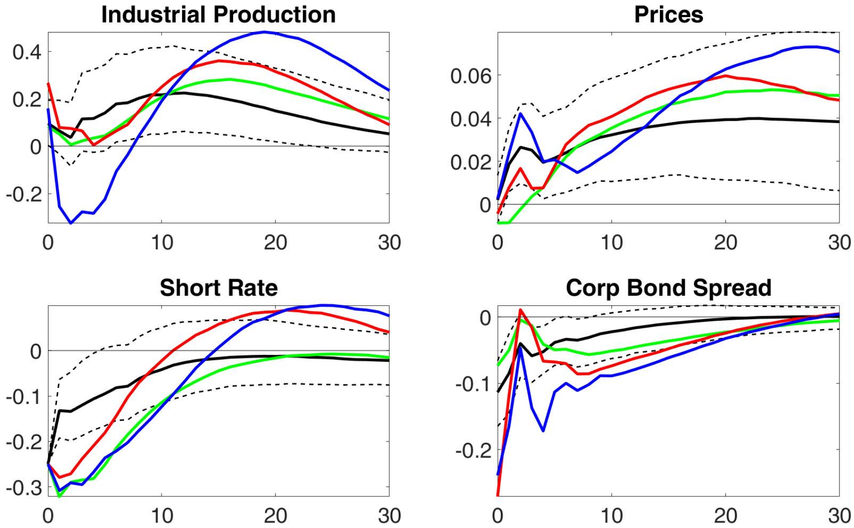
We start by estimating the effect of an expansionary monetary policy that leads to a 25 basis points (bp) drop in the shadow rate. All results are presented as impulse response functions together with a 90% confidence interval.

*Baseline model*

The results from the baseline VAR model are presented in Figure (3.2). As indicated by the black lines, the responses of industrial production, prices and the corporate bond spread to an expansionary shock have the expected sign and are statistically significant. As noted above, we circumvent the price puzzle by adding oil prices as an exogenous variable, so that a monetary easing immediately increases prices. The responses of the CPI and the industrial production index indicate that ECB policy stimulated both inflation and real economic activity. Boeckx et al. (2017) find similar results by imposing sign restrictions in a euro area VAR model. Following Zhu (2013), the corporate bond spread reflects the external finance premium and, hence, the credit channel of monetary policy transmission. We find that spreads narrow immediately upon the monetary easing, which is consistent with the presence of the credit channel.

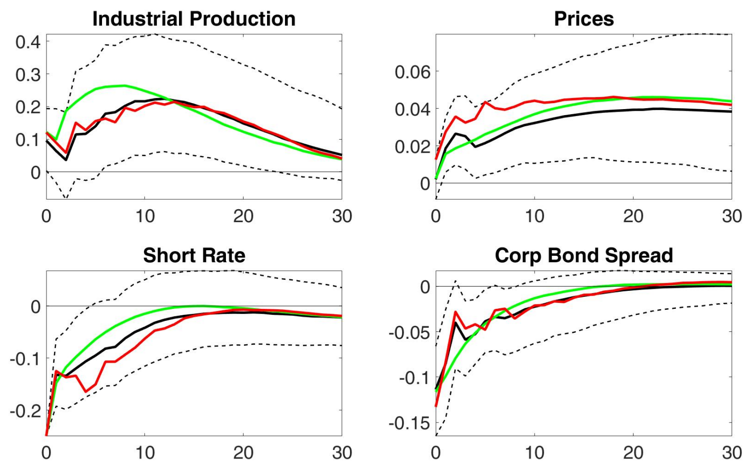
Further on, we review the accuracy of our outcome by altering the shadow rate and the lag-length. In this respect, the green and blue lines in Figure (3.2) show the impulse responses based on the shadow rates of Krippner (2012) and Lemke and Vladu (2017), respectively. The results turn out to be similar. Gertler and Karadi (2015) have used a safe interest rate with a maturity of one year, proxied by the U.S. government bond rate, instead of the shadow rate. For reasons of comparability we also present results based on the interest rate on a zero-coupon 1-year German government bond (red line). Again, the outcomes for industrial production, the price level and the interest rate are very similar. Only the negative response of the corporate bond spread is more pronounced. Since corporate financing is more dependent on long term credit conditions, this finding does not come as a surprise. However, one has to keep in mind that, in contrast to shadow rates, the short term government bond rate hits the zero lower bound during the financial crises. As displayed in Figure (3.3), altering the lag length does not change the results qualitatively.

Figure 3.2: Baseline VAR Model




*Notes:* Responses to an expansionary monetary policy shock of 25bp obtained from the VAR model with external instruments and 90% confidence band. The black line is the response in the model based on the Wu-Xia (2016) shadow rate, the green line is based the shadow rate of Krippner (2012), the blue line is based on the shadow rate of Lemke and Vladu (2017) and the red line is estimated based on the 1-year German government bond zero coupon rate.

Figure 3.3: Baseline VAR Model: Alternative Lag Structure



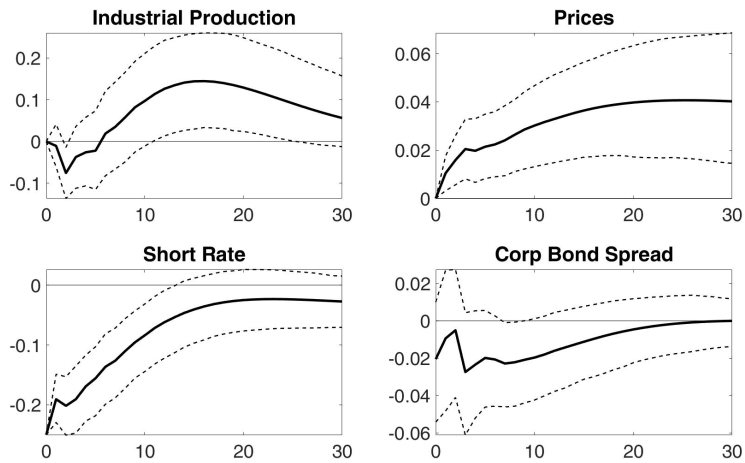

*Notes:* Responses to an expansionary monetary policy shock of 25bp obtained from the VAR model with external instruments and 90% confidence band. The black line represents the baseline model with a lag length of four. The green (red) line is the impulse response for the model with two (six) lags.

*Cholesky identification*

For a comparison, we apply a Cholesky identification instead of the external instruments approach. The implied ordering of the variables is the following: log of industrial production, log of consumer prices, the shadow short rate and the corporate bond spread. The restriction imposed implies that monetary policy affects the spread contemporaneously, but all other variable with a time lag of one month.

The results are shown in Figure (3.4). While prices and industrial production exhibit responses which are very similar to the baseline findings, the corporate bond spread does not react significantly. This might be the result of the endogenous nature of both the shadow rate and the bond spread, which is not adequately captured by the Cholesky identification. This also lends support to the external instruments approach that we use for identification in our baseline model.

Figure 3.4: Baseline VAR Model: Cholesky Identification




*Notes:* Responses to an expansionary monetary policy shock of 25bp obtained from the VAR model identified recursively and 90% confidence band.

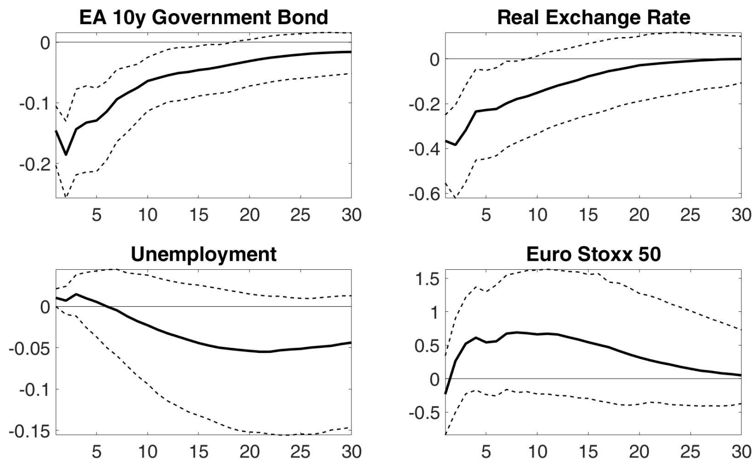
*Extending the baseline model with other real and nominal variables*

We now turn to the responses of additional variables which were not included in our baseline model. As outlined in the previous sub-section, we add one variable at a time as a fifth variable to our model. To save space, we only report the impulse response for the fifth variable.[12] Figure (3.5) shows the results for euro area government bond yields, the real exchange rate, unemployment and the Euro Stoxx 50. Bond yields immediately fall after a monetary easing. Furthermore, the instant depreciation of the euro indicates the existence of the exchange rate channel. Surprisingly, the increase in industrial production found before is not

[12]Throughout the different VAR models the responses of the four variables in the baseline model remain qualitatively unchanged.

accompanied by a significant decrease in the unemployment rate. Though the sign of the unemployment response is negative, on a ten percent confidence level, it cannot be ruled out that its response is actually zero. One explanation for the modest decrease in unemployment might be the heterogeneity of business cycles in the euro area. Our results might reflect that, since the European debt crisis, unemployment in core and periphery countries respond differently to a monetary policy shock. This hypothesis is supported by our country-specific results presented below.

Figure 3.5: Alternative 5th Variable: Additional Real and Nominal Variables




*Notes:* Responses of alternative choices for the 5th variable to an expansionary monetary policy shock of 25bp obtained from the VAR model with external instruments and 90% confidence band.

Although the Euro Stoxx 50 has the expected positive sign, its response turns out to be insignificant. Hence, for the entire time span, we do not find evidence for a policy transmission through the stock market.[13] At a first glance, this seems surprising as expansionary monetary policy leads to a bull market from a theoretical point of view.[14] However, as outlined in e.g. Romer and Romer (2000), Nakamura and Steinsson (2018) and Jarocinski and Karadi (2018), a monetary policy shock also contains information about the policymakers' perceptions of the economic situation. Under the assumption that market participants value this information, the responses of (financial market) variables are also driven by the information component. While a decrease in the short rate due to monetary easing is expected to increase stock prices, a decrease in the policy rate due to weaker economic fundamentals potentially leads to a reduction in stock prices. Below, we follow Jarocinski and Karadi (2018)

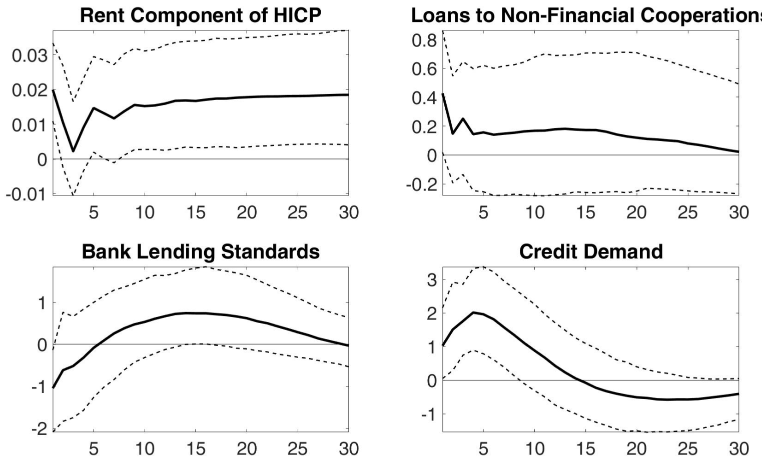
[13]The reaction of the MSCI Euro Index is virtually identical to the one from the Euro Stoxx 50. These results, along with impulse-responses from policy uncertainty, the VSTOXX and the monetary base, are available on request.

[14]Indeed, Gambetti and Musso (2017) find evidence that the ECB's Asset Purchase Programme increased stock prices.

and further disentangle the information shock from the pure monetary policy shock. Indeed, we find that a pure monetary policy shock (information shock) increases (decreases) the Euro Stoxx 50.

According to Figure (3.6), a monetary easing increases the rent component of the HICP, which serves as a monthly proxy for house prices. A monetary expansion relaxes bank lending standards, thus supporting the existence of a risk taking channel. The demand for credit increases. A significant reaction in both bank lending and credit demand is also found by Ciccarelli et al. (2015). Furthermore, we find that the total loan volume to non-financial institutions increases.

Figure 3.6: Alternative 5th Variable: Credit Market




*Notes:* Responses of alternative choices for the 5th variable to an expansionary monetary policy shock of 25bp obtained from the VAR model with external instruments and 90% confidence band.

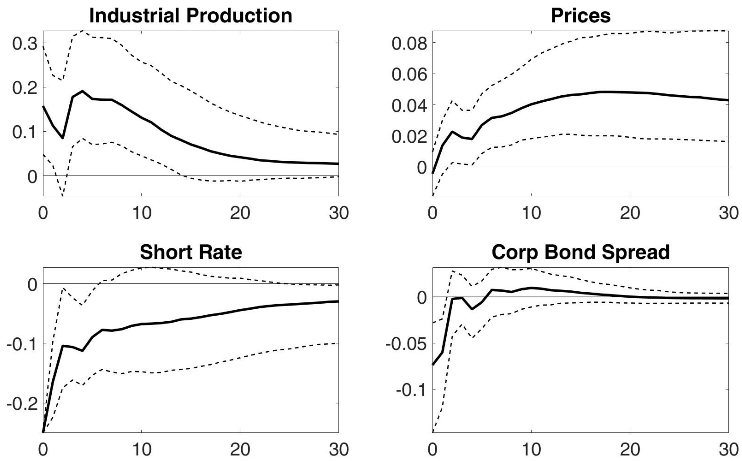
*The post-2008 sample*

In order to address the question of how unconventional monetary policy is transmitted, we present evidence from the crisis period only. We interpret the sharp decrease in the ECB's key interest rate as the beginning of the era of unconventional monetary policy. The results based on a sample from 2008:10 until 2016:10 are shown in Figure (3.7), Figure (3.8) and Figure (3.9). With the shorter time span, we reduce our lag length to three as again indicated by the Akaike criteria and the final prediction error.

Overall the reactions remain similar to those from the full sample VAR model. However, Figure (3.8) reveals a weaker reaction of the real exchange rate in the subsample. In the 2002-2016 sample the responses of the unemployment rate and the Euro Stoxx 50 display the expected sign, although their responses are at no point significantly different from zero, see Figure (3.5). In contrast to that, the sign of the reaction of the unemployment rate and the Euro Stoxx 50 in the 2008-2016 sample is less clear as the responses cross the zero-line several times, see Figure (3.8). Hence, we conclude that the policy transmission through employment and the stock market is particularly impaired in the post-2008 era. This era is characterized by sizable intra-euro area government bond spreads, indicating that national characteristics play a major role for market participants during this time. This suggests that we can obtain more information from a country-specific perspective, which is pursued in the next section.

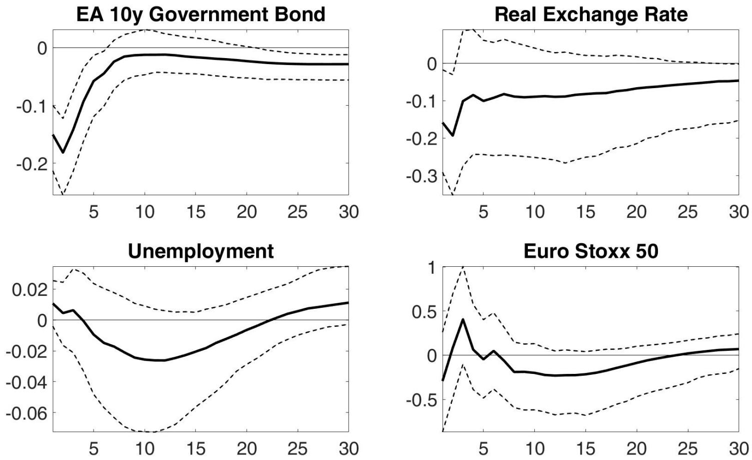
Figure (3.9) displays impulse-responses for the credit market variables. Though not significant, the reaction of rent prices and lending standards are in line with the findings for the 2002-2016 sample. In contrast, the increase in credit demand is substantially higher in the post-crisis sample. Interestingly, an expansionary monetary policy shock lowers the total loan volume to non-financial institutions. Our findings underpin the structural problems of the euro area credit market: aggregate lending does not increase despite relaxed standards and higher credit demand.

Figure 3.7: Baseline VAR Model: 2008:10 - 2016:10



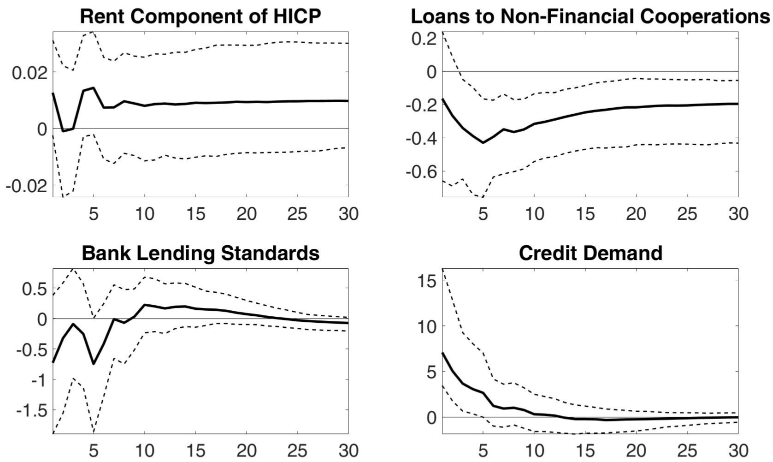

*Notes:* Responses to an expansionary monetary policy shock of 25bp obtained from the VAR model estimated over the post-crisis sample with external instruments and 90% confidence band.

Figure 3.8: Alternative 5th Variable: Additional Real and Nominal Variables (2008:10 - 2016:10)




*Notes:* Responses of alternative choices for the 5th variable to an expansionary monetary policy shock of 25bp obtained from the VAR model estimated over the post-crisis sample with external instruments and 90% confidence band.

Figure 3.9: Alternative 5th Variable: Credit Market (2008:10 - 2016:10)




*Notes:* Responses of alternative choices for the 5th variable to an expansionary monetary policy shock of 25bp obtained from the VAR model estimated over the post-crisis sample with external instruments and 90% confidence band.

### 3.2.5 Pure Monetary Policy Shocks vs. Information Shocks

Romer and Romer (2000), Nakamura and Steinsson (2018), Miranda-Agrippino and Ricco (2021) and Jarocinski and Karadi (2018), among others, have pointed out that Governing Council decisions also unveil information about variables that do not represent policy instruments. The rationale behind this argument is that policymakers react to economic conditions (i.e. inflation and unemployment). An unanticipated decrease in the main refinancing rate might indicate that inflation is lower than expected and/or unemployment is higher than expected.

Jarocinski and Karadi (2018) disentangle the information component of monetary policy shocks from the pure policy shock by differentiating between responses of interest rates and stock prices on Governing Council meeting days. While interest rates decrease and stock prices increase after a monetary policy easing, both financial market variables move in tandem after an information shock. Therefore, Jarocinski and Karadi (2018) estimate a VAR-model with instruments and impose sign-restrictions in order to identify pure policy shocks and information shocks, respectively.

We follow their concept of discerning between movements of interest rates and stock prices on monetary policy announcement days. We incorporate changes in both variables into a principal component analysis. More precisely, we include standardized changes in the yield of German government bonds with maturities of two, three, five and ten years on announcement days as well as standardized changes of the Euro Stoxx 50 and the FTSE Euro 100 stock price index.[15] By applying principal component analysis, we are more agnostic than Jarocinski and Karadi (2018) as we let the data speak without imposing restrictions. In fact, principal components and factor analyses are a common empirical tool in the news announcement literature, see e.g. Gürkaynak et. al (2005) and Barakchian and Crowe (2013).

Table (3.2) displays the loadings on the first two components. The cumulative proportion of information explained by these two principal components is roughly 92%. While in the first component all variables are loaded with a positive sign, the second component loads changes in bond yields with a negative sign and changes in the stock market with a positive sign. Hence, we interpret the first component as the information shock and the second component as the pure policy shock.[16] The interpretation of the pure policy shock is further supported by the fact that the loadings on the lower end of the yield curve are higher (in absolute terms). According to the pure policy shock, the two biggest surprises were the decision not to raise the volume of the APP in December 2015 and the announcement of the SMP program in May 2010.

Figure (3.10) shows the results from the baseline model following a pure policy shock and an information shock, respectively. The pure policy shock displays results which are qualitatively similar to those following the monetary policy shock discussed

[15] Each variable is standardized to have a mean of zero and a standard deviation of one.

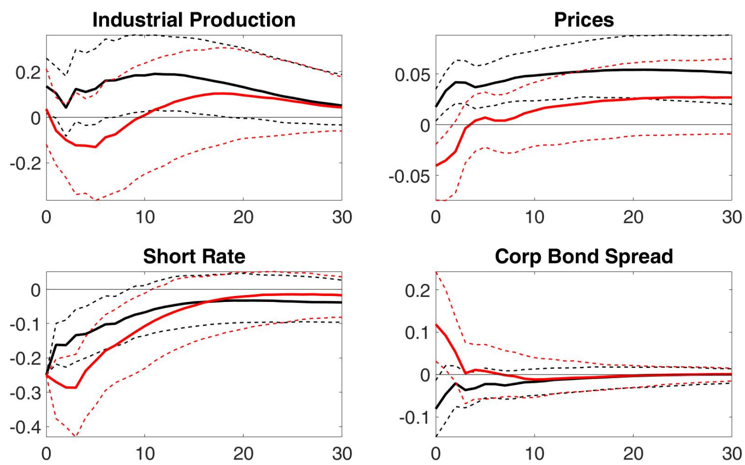
[16] We multiply the pure policy shock with $-1$ such that, in line with the monetary surprise component from above, a positive surprise represents a monetary tightening.

Table 3.2: Loadings on the first two principal components

| Variable | PC 1 | PC 2 |
|---|---|---|
| German Government Bond 2Y | 0.4433 | -0.2401 |
| German Government Bond 3Y | 0.4699 | -0.2176 |
| German Government Bond 5Y | 0.4744 | -0.2082 |
| German Government Bond 10Y | 0.4272 | -0.1683 |
| Euro Stoxx 50 | 0.3028 | 0.6337 |
| FTSE Euro 100 | 0.2887 | 0.6494 |

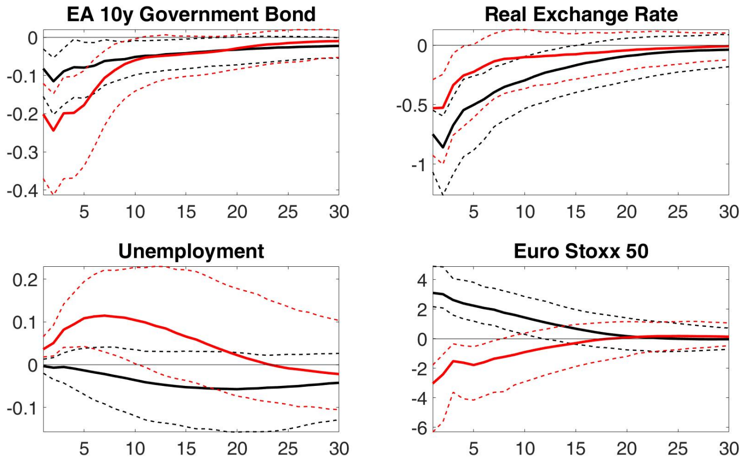
before. A negative information shock means that policymakers lower the interest rate due to weak economic fundamentals. In line with the findings of Jarocinski and Karadi (2018), a negative information shock decreases the price level and increases the corporate bond spread immediately. We do not find evidence that this shock decreases industrial production. Nevertheless, we observe a reduction in economic activity as indicated by the hike in unemployment, see Figure (3.11). The stock market reacts in line with our expectations, i.e. a drop in the interest rate due to a pure policy shock (information shock) increases (decreases) the Euro Stoxx 50 on impact. A depreciation of the euro as well as decreases in government bond yields can be the consequence of both types of shocks.

Figure 3.10: Pure Policy and Information Shock




*Notes:* Responses to an expansionary monetary policy shock of 25bp obtained from the baseline VAR model estimated with external instruments and 90% confidence band. The black (red) line indicates the impulse responses for the pure monetary policy shock (information shock).

Figure 3.11: Pure Policy and Information Shock: Additional Real and Nominal Variables



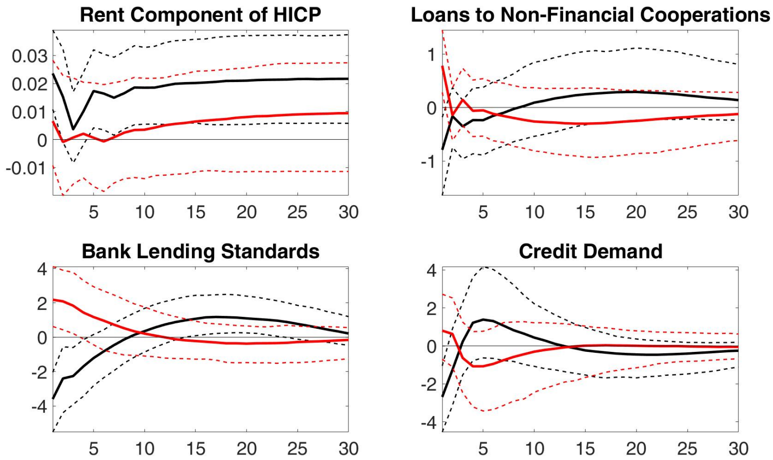

*Notes:* Responses to an expansionary monetary policy shock of 25bp obtained from the baseline VAR model estimated with external instruments and 90% confidence band. The black (red) line indicates the impulse responses to the pure monetary policy shock (information shock).

From Figure (3.12) we observe that rents only respond to a pure policy shock. As before, lending standards decrease and credit demand strengthens after a pure policy shock. A negative information shock leads to tighter lending standards, but has no effect on credit demand. Once again, the loans to the private sector respond counter-intuitively with respect to both shocks. Apart from loans, all impulse responses to both types of shocks are well in line with the theory and the findings in the literature. Overall, we find that the pure policy shock leads to impulse responses that are in line with those observed from the VAR model, where changes in the yield on 10-year German bunds on ECB meeting days serve as an instrument. Thus, we conclude that our instrument in the baseline model captures policy surprises well and is not overly distorted by the revelation of new information on meeting days.

## 3.3 Country-specific effects of euro area monetary policy

In this section, we study the country-specific responses to a common monetary policy shock. Hence, at this stage, we want to exclude the feedback from domestic economic conditions to euro area monetary policy. Since we have identified a common monetary policy shock in the previous section, there is no identification problem to solve at this stage. Therefore, in order to derive country-specific responses to a common euro area shock, we use local projections as suggested by Jordà (2005).

Figure 3.12: Pure Policy and Information Shock: Credit Market




*Notes:* Responses to an expansionary monetary policy shock of 25bp obtained from the baseline VAR model estimated with external instruments and 90% confidence band. The black (red) line indicates the impulse responses to the pure monetary policy shock (information shock).

An impulse response is defined as the response of a variable $h$ periods ahead to a monetary policy shock at time $t$. This response is not derived from a full-scale VAR model with interactions among all endogenous variables, but rather from a single-equation framework that does not allow for a feedback from the endogenous variable to monetary policy. We estimate a series of regressions of a dependent variable dated $t+h$ on the monetary policy shock in $t$ as well as a set of control variables. The estimated model is the following

$$y_{t+h} - y_{t-1} = \alpha_h + \beta_h mp_t^{EA} + \gamma_h' \sum_{s=0}^{q} \mathbf{x}_{t-s} + \varepsilon_{t+h}, \tag{3.10}$$

where $y_t$ is the dependent variable and $\mathbf{x}_t$ is a vector of country-specific control variables. We include up to $q$ lags of control variables. The euro area monetary policy shock is denoted by $mp_t^{EA}$. Hence, the coefficient $\beta_h$ measures the impact of a change in policy at $t$ on the dependent variable $h$ periods ahead. Plotting $\beta_h$ as a function of $h$ provides us with an impulse response function.

For our purpose, local projections are advantageous for two reasons: (1) they rest on a very small number of parameters to be estimated. (2) Since we estimate a single equation only, the results are more robust to misspecifications in other parts of the model. While we typically model dynamic systems of equations, e.g. VAR models, *because* we want to capture the feedback from the economy to policy, we deliberately exclude this feedback here.

Due to the fact that the dependent variable is $h$ periods ahead, the error terms will exhibit serial correlation. We therefore apply a Newey-West correction to our estimation errors, which we use to construct a confidence band around the estimated series of $\beta_h$ coefficients. As suggested by Jordà (2005), the maximum lag for the Newey-West correction is set to $h + 1$.

We estimate local projections for 10 member countries, which together account for more than 95% of euro area GDP: Germany, France, Spain, Italy, Portugal, Greece, Ireland, Netherlands, Finland and Austria. To contrast the country-specific responses with the area-wide responses, we also estimate the model for a synthetic euro area that consists of these ten countries only.[17] The sample period is 2002:1 to 2016:1 and the data frequency is monthly. The sample is slightly shorter than the sample used in the previous section due to limited data availability. We estimate the model for each of the following variables: (log) industrial production, (log) price level as measured by the HICP, unemployment rate, (log) real exchange rate, (log) stock prices and (log) loans to the private sector.

We keep the list of control variables relatively short and include country-specific cyclical variables such as unemployment, prices, industrial production and the real exchange rate. We also include the shadow short term interest rate to reflect monetary conditions. Note that the latter is supposed to reflect the level of policy accommodation, but not the policy shock, which is reflected by $mp_t^{EA}$. Changing the vector of control variables has no substantive effect on our estimated impulse response functions.

The euro area monetary policy shock, $mp_t^{EA}$, is based on the identification of policy surprises discussed before. The relation between the structural shock $u_t$ of the VAR model and the reduced form shock $\varepsilon_t$ is given by $U_t = S^{-1} \cdot \varepsilon_t$. From the estimation of the baseline model in 2.4 we receive the reduced form error terms as well as the row in the matrix $S^{-1}$ that captures the contemporaneous responses to the structural shock. With these variables at hand, we are thus able to uniquely identify our policy shock series, $mp_t^{EA}$.

### 3.3.1 Results

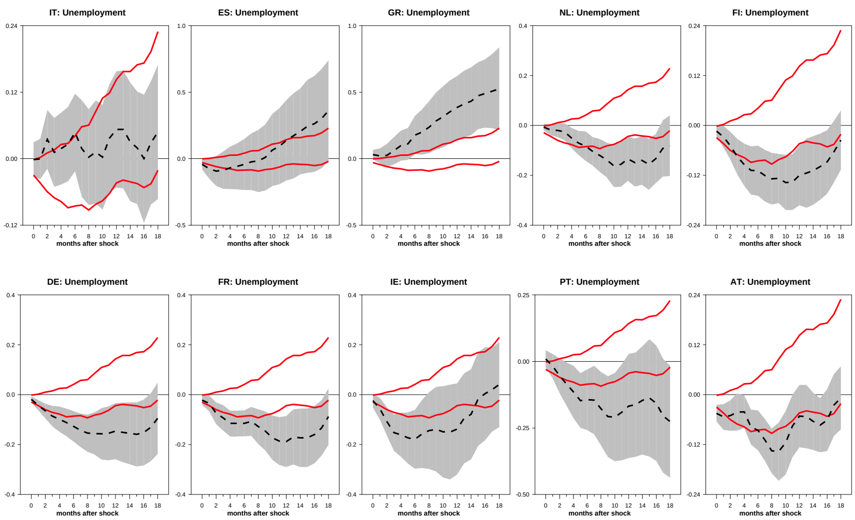
The results are presented in Figures (3.13) to (3.18). In each figure, we plot the impulse response function following a monetary policy easing shock, the 90% error band around this impulse response and, as a pair of red lines, the error band around the estimated impulse response for the synthetic euro area variable. Thus, comparing

[17] The euro area time series for each variable is constructed as the weighted average of the country-specific variables. For that purpose, the GDP weights from the ECB website have been normalized in order to account for member countries which are not included here, that is, the GDP weights for the 10 countries used here always add up to 100 percent.

the dotted country-specific impulse response and the red error bands allows us to assess whether a given country's response deviates significantly from the response of the euro area as a whole.

We find that following a monetary policy shock, unemployment decreases significantly in core countries such as Germany, France and the Netherlands, see Figure (3.13). Some periphery countries, namely Italy, Spain and Greece, in contrast, could not benefit from the monetary expansion implemented by the ECB. In these countries, unemployment does not fall. As a consequence of this heterogeneity, the area-wide unemployment rate does not respond significantly, which is consistent with the finding presented in the previous section.

Figure 3.13: Country-Specific Responses of Unemployment

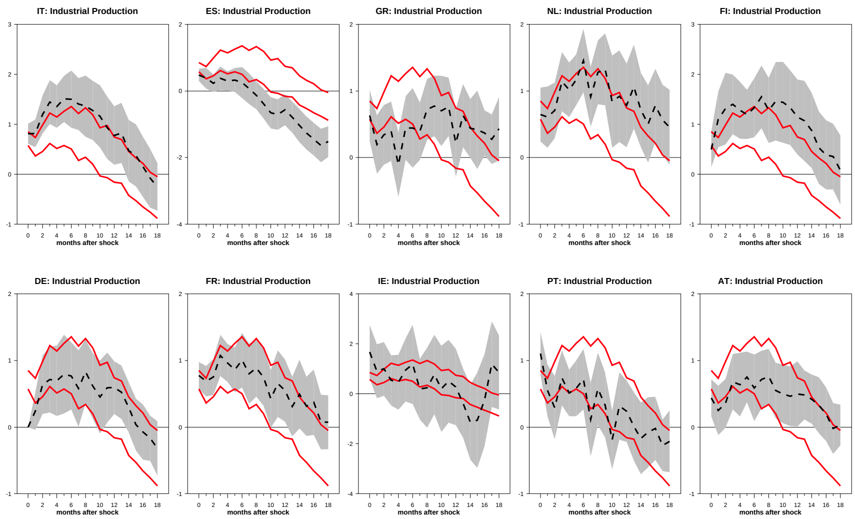



*Notes:* Country-specific response to a euro area monetary policy easing shock of 25bp (dotted line) obtained from local projections and 90% error bands (shaded area). The solid lines are the error bands around the average euro area response.

Figure (3.14) reports the responses of industrial production. Manufacturing activity improves in all countries following the expansionary policy shock. Interestingly, the responses are much more homogeneous across countries. Only the response of industrial production in Spain deviates markedly from the euro area average response. In contrast to that, manufacturing activity rises in Italy and Greece after a monetary easing while employment does not improve. Possibly, unemployment rates in these countries are mainly driven by other sectors.[18] In a nutshell, we only observe heterogeneous effects on real activity if it is proxied by the unemployment rate and not by industrial production.

[18]In fact, the service sector accounts for more than two thirds of GDP in both countries.

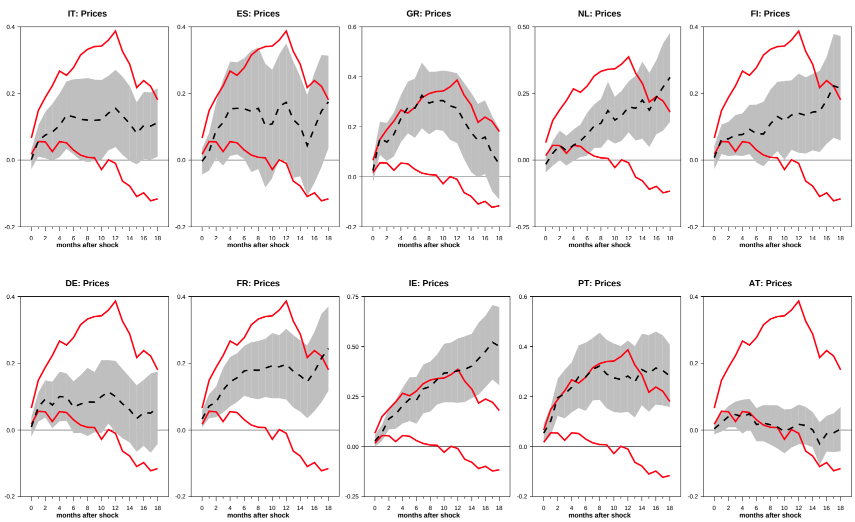
Figure 3.14: Country-Specific Responses of Industrial Production




*Notes:* Country-specific response to a euro area monetary policy easing shock of 25bp (dotted line) obtained from local projections and 90% error bands (shaded area). The solid lines are the error bands around the average euro area response.

Figure (3.15) shows the responses of consumer prices, which increase moderately following a monetary policy shock. The responses are well in line with the average response of the euro area price level and might be a result of the single European market.

Figure 3.15: Country-Specific Responses of Consumer Prices

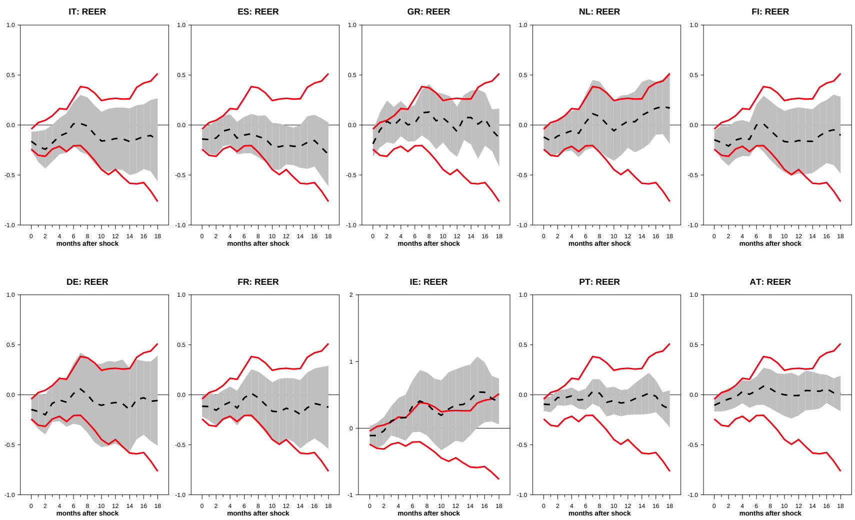



*Notes:* Country-specific response to a euro area monetary policy easing shock of 25bp (dotted line) obtained from local projections and 90% error bands (shaded area). The solid lines are the error bands around the average euro area response.

In all countries, the real effective exchange rate depreciates on impact, see Figure

(3.16). Different responses of real exchange rates among EMU members can only occur when price levels react differently. As we find homogeneous responses of price levels to a monetary policy shock, we also observe homogeneous movements of the real exchange rate. The size of the depreciation for the euro area as a whole is in a range similar to the one observed in the VAR model (see Figure (3.5)). However, the confidence bands in the local projections framework are somewhat wider, suggesting that cutting the feedback from the real economy to monetary policy results in higher estimation uncertainty.

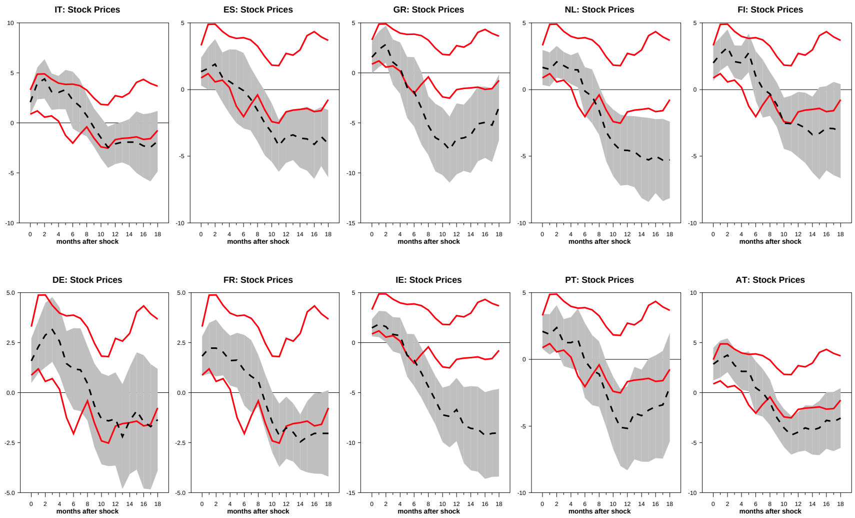
Figure 3.16: Country-Specific Responses of the Real Exchange Rate




*Notes:* Country-specific response to a euro area monetary policy easing shock of 25bp (dotted line) obtained from local projections and 90% error bands (shaded area). The solid lines are the error bands around the average euro area response.

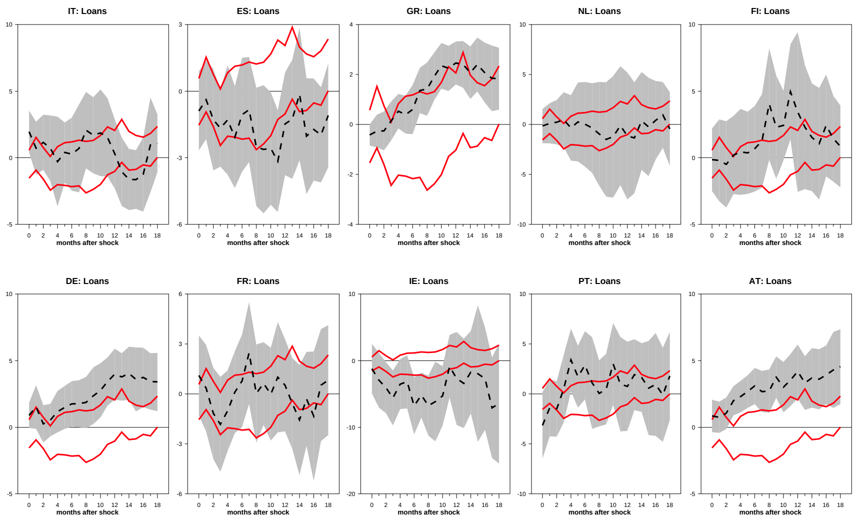
The response of the main stock price indexes, see Figure (3.17), exhibits the expected positive sign for the overall eurozone in the short run. For some countries, i.e. Spain, Greece, the Netherlands, Ireland, Portugal and Austria, the responses deviate negatively from the average euro area response after about ten months. The results are also in line with the country-specific findings provided by Wieladek and Pascual (2016). These authors also document an insignificant and even negative effect of a policy easing on stock prices in some euro area countries.

Figure 3.17: Country-Specific Responses of Stock Prices




*Notes:* Country-specific response to a euro area monetary policy easing shock of 25bp (dotted line) obtained from local projections and 90% error bands (shaded area). The solid lines are the error bands around the average euro area response.

Figure 3.18: Country-Specific Responses of Loans




*Notes:* Country-specific response to a euro area monetary policy easing shock of 25bp (dotted line) obtained from local projections and 90% error bands (shaded area). The solid lines are the error bands around the average euro area response.

Finally, Figure (3.18) suggests that the ECB is not effective in stimulating credit to non-financial corporations. Germany, Austria and Greece appear to be the only countries in which credit increases significantly following the monetary expansion. In most other countries, the response of bank credit remains insignificant. As derived from the VAR model, this is in line with the insignificant response of aggregate credit in the euro area.[19]

Overall, we find the responses of unemployment, stock prices and bank lending to be different across member countries, while consumer prices and industrial production are much more homogeneous. The results suggest that the impaired transmission through the financial system, i.e. the stock market and the credit market, as well as structural frictions in the adjustment of the labor market, might hold the key to understanding the uneven transmission of ECB policy.

## 3.4 Conclusions

In this paper, we studied the monetary transmission mechanism in the euro area - both based on aggregate and country-specific data. To identify a monetary policy shock, we estimated an external instruments VAR that solves the contemporaneous correlation between monetary policy and financial variables in the euro area.

Our findings are threefold: First, identifying a VAR with an external instrument helps to disentangle the simultaneous interaction of the ECB and the financial market. A principal component analysis of the responses of bond yields and stock prices combined with the interaction among the variables in the VAR model generates a plausible decomposition of policy surprises into the pure policy shock and the information shock arising from ECB decisions.

Second, we document the heterogeneity of the monetary transmission process across transmission channels. Overall, monetary policy is less effective with regard to stimulating bank lending and increasing the valuation of the stock market. These findings suggest that monetary transmission is severely hampered by the state of banking systems, e.g. the ongoing deleveraging and the burden of non-performing loans.

Third, we shed light on the heterogeneity of policy transmission across member countries. For that purpose, we included the ECB's monetary policy shock in country-specific regressions. This makes sure that the policy shock is the same across countries and that a feedback from country-specific variables to euro area monetary policy is excluded. We show that the responses of some variables, most notably prices and industrial production, are relatively similar across countries, while the transmission through the financial system, i.e. the responses of stock prices and bank lending, varies among member countries. Since our results are purely positive, we should be careful not to overemphasize the normative implications. Nevertheless, the results suggest that a "one-size-fits-all" monetary policy might not be the best

[19]This finding is in line with the results of Boeckx et al. (2017).

tool to boost demand if national banking systems are blocked - not least since banks provide most financing in continental Europe. Over many years since the eruption of the European debt crisis, monetary policy was overburdened with the task of reviving economic activity. In light of the findings presented here, this has supported inflation throughout the eurozone. However, the effects on real activity are heterogeneous, especially if one focuses on unemployment, where core countries benefit from a monetary easing disproportionately.

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## 3.5 Appendix

### Data Sources and Definitions

Table 3.3: Data Sources

| Variable | Adj. | Source |
|---|---|---|
| Bank Lending Standards | nsa | Bank Lending Survey |
| Credit Demand | nsa | Bank Lending Survey |
| Crude Oil Prices (Brent Europe) | nsa | FRED |
| EONIA Rate | nsa | Datastream |
| Euriobor Future | nsa | Eikon |
| Euro Stoxx 50 | nsa | Datastream |
| Eurobond 10y all Ratings | nsa | ECB |
| FTSE Euro 100 Stock Price Index | nsa | Datastream |
| German Government Bond Yield | nsa | Eikon |
| Harmonised Index of Consumer Prices | sa | ECB |
| Industrial Production (excl. Construction) | sa | ECB |
| Loans to Non-Financial Institutions | sa | ECB |
| Real Exchange Rate (vis-a-vis group of 19 trading partners) | nsa | ECB |
| Shadow Rate | nsa | Wu and Xia (2016) |
| (Alternative) Shadow Rate | nsa | Krippner (2012) |
| (Alternative) Shadow Rate | nsa | Lemke & Vladu (2017) |
| S&P Eurozone Corporate Bond Yield | nsa | Datastream |
| Unemployment Rate | sa | ECB |

*Notes:* (Not) Seasonally adjusted data series are indicated by ”sa” (”nsa”).

# 4 Moving Closer or Drifting Apart: Distributional Effects of Monetary Policy

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# Moving Closer or Drifting Apart: Distributional Effects of Monetary Policy

Lucas Hafemann*,1 Paul Rudel*,2 Jörg Schmidt*,3

The heating debate about increasing income inequality forces monetary policymakers and academia to (re-)assess the nexus between (unconventional) monetary policy and inequality. We use a VAR framework to unveil the distributional effects of monetary policy and the role of redistribution in six advanced economies. While all of them experience an increase in Gini coefficients of gross income due to an expansionary monetary policy shock, only countries with relatively little redistribution display a significant response of net income inequality as well. To examine the underlying transmission channels we take a closer look at the sources of income, i.e. labor and capital income. Our findings suggest that the disproportional surge in capital income is the driving force behind the increase in net income inequality.

**Keywords:** income inequality, factor income distribution, monetary policy, redistribution.

**JEL classification:** E24, E25, E52, E64

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## 4.1 Introduction

*"All economic policy-makers have some distributional impact as a result of the measures they introduce - yet until relatively recently, such consequences have been largely ignored in the theory and practice of monetary policy."* Yves Mersch (ECB), 2014.

The Financial Crisis has set the limit of conventional monetary policy measures for the majority of the advanced economies. To stabilize financial markets and stimulate the economy major central banks around the world steadily lowered their policy rates up to the zero lower bound (ZLB). As this lowering was often not sufficient to fulfill their mandate, the central banks imposed unconventional measures including i.a. large-scale asset purchase programs (LSAP) and forward guidance on policy rates. As a consequence, equity and housing prices increased, while, at the same time, interest rates and returns on savings remained at an all-time low. In public, this constellation strengthened the perception of rising inequality arguing that such measures benefit already wealthy capital owners disproportionately. The public arousal forces policymakers and academia to discuss the distributional consequences of monetary policy.
However, no central bank pursues equality per mandate.[1] Nonetheless, economic key indicators that are within the scope of central banks, like inflation and growth, have distributional effects themselves. For example, Doepke and Schneider (2006), Albanesi (2007), and Adam and Zhu (2016) find that unexpected inflation coincides with higher level of inequality. The analysis by Romer and Romer (1999) indicates a positive relationship between inequality and both, average inflation and volatility of nominal GDP growth. Thus, every policy measure that addresses one or both of the key indicators will have inevitably distributive effects.
Still, policymakers might have an intrinsic interest in moderate levels of inequality: Areosa and Areosa (2016) and Auclert (2019) ascertain that higher levels of inequality coincide with less stimulating power of monetary policy.
There are several mechanisms through which monetary policy may affect the distribution of income and wealth. Since we are interested in the nexus between monetary policy and income inequality, we limit our analyses to the following channels:
The *employment channel*: Labor income is the major earnings source for the vast majority of households. However, high-skilled and low-skilled households respond

[1] Also because it is troublesome to measure a (socially accepted) "natural level of inequality". Still, some attempts were made. See for example Rodriguez et al. (2002) or Heer and Maussner (2009). Mankiw (2015) describes anecdotally, why some inequality is necessary for prosperity.

differently to monetary-policy-induced fluctuations on the labor market. If low-skilled households are more likely to be affected by unemployment in an economic downturn, monetary stimulus benefits those households disproportionately and alleviates an increase in income inequality.
The *income composition channel*: Households differ in terms of their primary incomes. If monetary policy benefits capital income more than labor income, e.g. through boosting dividends or stock returns, as it can be observed since the introduction of quantitative easing (QE), income inequality will increase because capital income receivers are primary high-income households.
Neither is the transmission of monetary policy to inequality unambiguous, nor the findings in the literature. Mumtaz and Theophilopoulou (2015) and Coibion et al. (2017) find that contractionary monetary policy shocks increase inequality in earnings, income, and consumption. In their analysis for the US, Coibion et al. (2017) draw a number of conclusions. Following a monetary policy shock, wage earnings for those in upper percentiles of the wage distribution recover notably faster than for those at the bottom of the distribution. The total income effect is smaller because low-income households disproportionately rely on transfers which in turn react counter-cyclically. Lansing and Markiewicz (2016) and Coibion et al. (2017) state that the distributional effects of monetary policy were mitigated by governmental redistribution in the US.[2] In contrast, Davtyan (2016) finds evidence for the US that contractionary monetary policy shocks are associated with lower income dispersion in the long-run.
Primarily unconventional monetary policy measures are suspected to be one of the main drivers of increasing inequality in recent years. The argument is that ultra-loose monetary policy disproportionately benefits asset holders because the returns of a broad variety of assets surged due to LSAPs and low long-term yields. The stimulating effect elevated corporate profits faster than employment. Overall, the contribution of unconventional monetary policy measures to increasing inequality is not clear cut and respective research is limited.
Mumtaz and Theophilopoulou (2015) gauge an additional effect on inequality from unconventional measures taken by the Bank of England in the aftermath of the Financial Crisis. Adam and Tzamourani (2016) find that the ECB's 2012 announced Outright Monetary Transactions program influenced market prices such that the top 5% wealth group benefited disproportionately. Domanski, Scatigna and Zabai (2016) find that wealth inequality in advanced economies has risen since the Financial

[2]In addition, Ostry, Berg and Tsangarides (2014) show that redistribution can be pro-growth due to positive effects of lower levels of inequality.

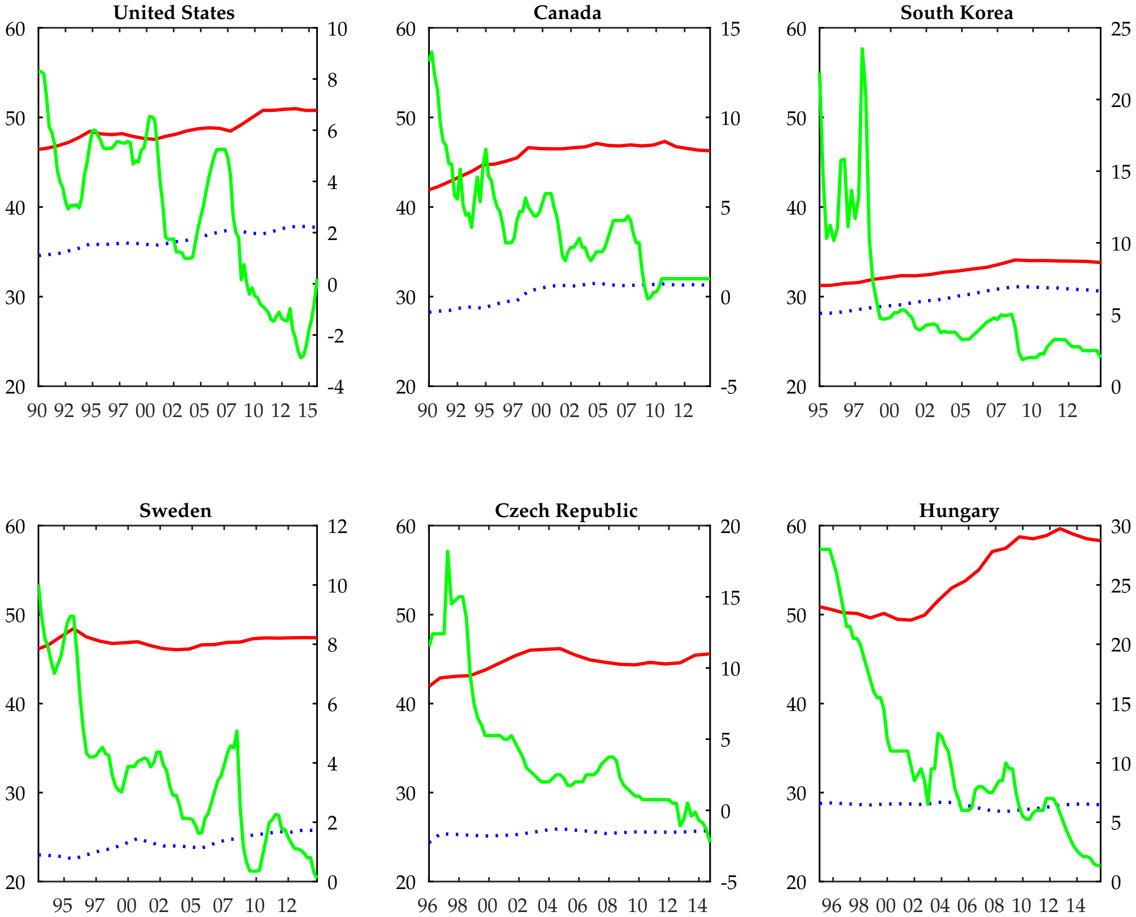
Crisis. They identify surging equity prices as the key driver.

Our contribution to the outlined controversy is twofold. Cross-country analyses unveil the role of redistribution in the nexus between (gross and net) income inequality and monetary policy. Data on factor income from national accounts uncover the underlying transmission mechanisms.

We focus on the transmission channels of monetary policy on income inequality, namely the income composition and employment channel. The procedure outlined in Bernanke and Gertler (1995) is used to analyze the potential mechanisms that drive the Gini measures after an expansionary 25 basis points (bp) monetary policy shock. Moreover, this work shall expose the role of redistribution. For this task, we choose countries that a) have an independent and autonomous central bank, and b) differ in their scope of redistribution. Thus, our analysis relies on the US, Canada, South Korea, Sweden, the Czech Republic, and Hungary. To incorporate redistributive effects, we examine the impulse responses of both, Gini of gross income (pre-tax, pre-transfers, *Gini gross* hereafter) and Gini of disposable income (post-tax, post-transfers, *Gini net* hereafter).

Figure (4.1) provides an overview of the Ginis for gross (red solid) and net (dotted) income as well as the policy rates (green solid) for the chosen countries. All countries but Sweden show an upward trend in Gini gross. The US, Sweden, and Hungary show the highest levels of gross income dispersion. Concerning net income dispersion, Sweden, the Czech Republic, and Hungary kept their levels in the considered periods while the US, Canada, and South Korea show an increase in the Gini net. Furthermore, the interest rates dropped in all countries.

Figure 4.1: Income Inequality and Policy Rates




*Notes:* Red solid (blue dotted) line depicts Gini gross (net)left ordinate, in percent. Green solid present key policy rates, in percent, right ordinate.

However, the major findings of our paper are threefold: Firstly, we observe an increase in inequality of Gini gross for all countries included in this paper when facing expansionary monetary policy. Secondly, we find that the effect on the Gini net remains positive for countries with minor redistribution. In contrast to this, countries with high relative redistribution do not face the same positive reaction in their net income inequality. Thirdly, we show that monetary policy is transmitted via overall employment, labor income, and capital income. Moreover, the disproportional surge in capital income is the driving force behind the increase in net income inequality.

The remainder of the paper is organized as follows. First, we introduce our data and the methodology. Section (4.3) covers the analysis of the nexus between monetary policy shocks and income inequality. In Section (4.4) we take a closer look at the underlying transmission mechanisms. The conclusion follows after a robustness Section.

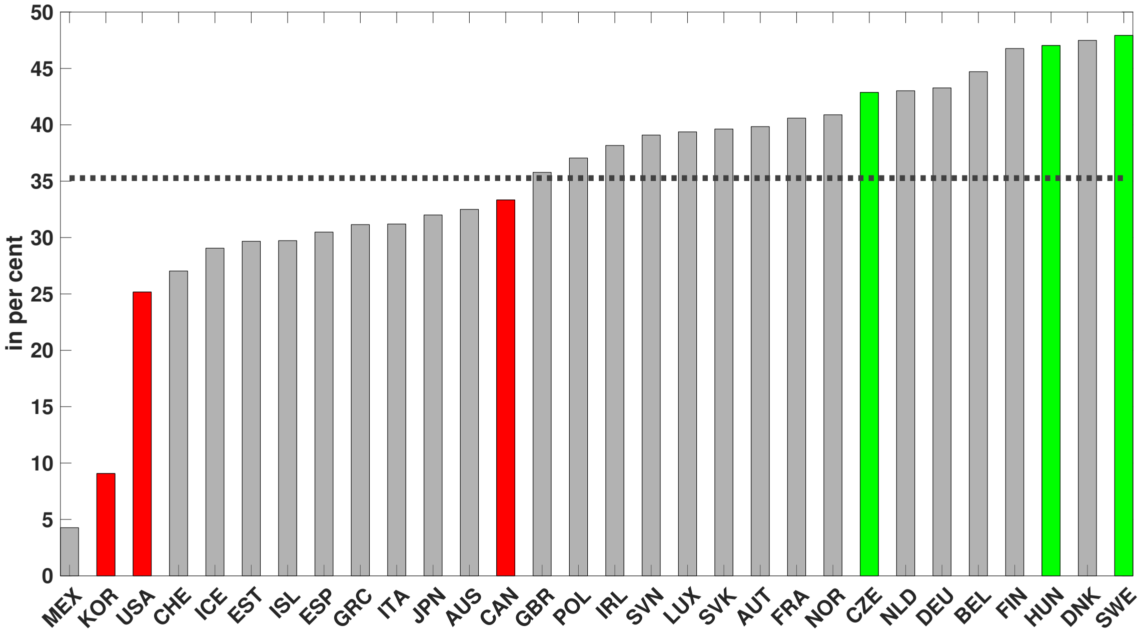
## 4.2 Data and Methodology

Before we proceed to our analysis of the nexus between monetary policy and its impact on the distribution of income as well as the underlying channels of transmission, we want to take a closer look at the data and methodology.

### 4.2.1 Data

The main objective of this paper is to examine the transmission mechanisms through which gross and net income distribution respond to monetary policy surprises and thus, obliquely, the role of governmental redistribution.
On the subject of redistribution, we select among OECD members regarding their relative redistribution.[3]

Figure 4.2: Relative Redistribution among OECD Members




*Notes:* Relative redistribution is computed as $100 \times (Gini\,gross - Gini\,net) \times Gini\,gross^{-1}$. Dotted line depicts cross-country mean.

Figure (4.2) depicts the average relative redistribution from 1995 to 2015, taken from the Standardized World Income Inequality Database (SWIID). Redistribution among OECD countries varies remarkably. At the upper end, Sweden, Denmark, and Hungary almost halve income inequality through redistribution, i.a. by way of taxes and transfers. In contrast, the US, South Korea, and lastly Mexico are the

[3]The relative redistribution is computed as the difference between gross and net income Gini divided by the gross income Gini and multiplied by 100.

countries with the lowest relative redistribution.[4] It stands out that predominately European countries show the highest levels of redistribution among OECD members. For example, out of the countries with relatively much redistribution, Canada is the non-European country with the highest relative redistribution: they lower income inequality by 33% through governmental intervention.

To examine the reciprocation of (various measures of) inequality to monetary policy shocks in a meaningful manner, we exclude all countries that are either part of a monetary policy union (i.e. the euro area) or directly peg their currency to others for a substantial period, in other words, have no independent monetary policy.

That said, we are left with Sweden, Hungary, and the Czech Republic as surrogates for highly redistributing countries on the one hand and Canada, the US, and South Korea on the other hand.[5]

In a first step, we want to capture the reaction of Gini coefficients of gross incomes to monetary policy. We then evaluate in how far monetary policy shocks propagate to the dispersion of households' net income. For both exercises, we use the corresponding mean estimators from the SWIID data set, compiled by Solt (2016), for all countries included in this paper. Since we use a VAR model with quarterly data, we linearly interpolate all Gini variables.[6]

Finally, we take a look at the transmission channels. Following the idea of Bernanke and Gertler (1995), we substitute the Gini coefficients with variables that are affiliated to the transmission mechanisms discussed above. In this respect, we analyze how the total number of employed persons as well as capital and labor income reacts to a monetary policy shock. The data for the channel variables stem mainly from the OECD.[7]

We conduct baseline vector-auto-regressions that (separately) include the inequality measures (i.e. the Gini coefficient or the channel variable) for each of the six countries in our sample, additional to the standard macroeconomic variables real GDP, consumer prices, a short-term interest rate, and the trade-weighted real effective exchange rate (REER). All non-stationary variables enter our model in log-

[4]Note that little redistribution does not necessarily correspond with a high level of inequality. South Korea, for example, already has a low level of inequality such that there is less need for redistribution to reach some sort of income equality. At the end, it remains a social decision how much redistribution a society desires.

[5]At the first glance, Mexico seems to be a valid candidate, too. In Section (4.4) we compare the responses of labor-related income and capital income to a monetary policy shock where we ground our analysis on OECD data which are unfortunately not available for Mexico.

[6]One might argue that the variables could be sensitive to altering interpolation methods. We believe that the interpolation method does not alter the results in a notable manner due to the inherent inertia of the variables. Nonetheless, we provide estimates with yearly data and get similar results.

[7]A more detailed description of the respective data is provided in Section (4.4).

levels. This assures that we take possible (long-run) cointegration relations between the variables into account. For example, Davtyan (2016) shows that there is a long-run relationship between monetary policy and inequality. The REER is incorporated because five of the six analyzed countries are small open economies where the exchange rate channel appears to be a relevant monetary transmission mechanism. Data on real GDP and CPI are taken from Datastream. Exchange rates stem from the Bank of International Settlements.[8]

Since we do not exclude periods of financial stress, we control for market uncertainty by including the CBOE Volatility Index (VIX). The VIX enters as an endogenous variable into the US model and as an exogenous variable into the VAR model of the remaining countries.

Our applied short-term interest rates deserve some special attention. We generally prefer the use of money market rates because monetary policymakers aim at the short-term inter-bank refinancing conditions as their intermediate objective. However, for Hungary and the Czech Republic, money market data is not available for the considered period. Therefore, we have to use the key policy rate in these two countries.

Furthermore, the ZLB becomes an issue in Canada, the Czech Republic, and the US.[9] For the latter, the interest rate variable is the wu and Xia (2016) shadow rate, available since 2003, and the effective Federal Funds Rate for previous periods. For Canada, we use a shadow interest rate estimated by MacDonald and Popiel (2016).[10] Unfortunately, shadow interest rates are not available for the Czech Republic. Hence, we use the euro area shadow rate from Q4 2012 onward because the short term interest rate dropped to 0.05% at this point in time. In 2013, the Koruna-Euro exchange rate reached its upper limit set by the Czech National Bank. Euro area shadow short-term rates are therefore an eligible alternative.

The start of our sample is restricted by data availability. For the US and Canada, our samples start in 1990 because this marks the starting point of the VIX. For the four remaining countries, the OECD data set is the limiting factor, such that 1993 (1995) marks the beginning of the sample for Sweden (South Korea, the Czech Republic, and Hungary). Furthermore, by 1995, the Czech Republic and Hungary had already

[8]For the sake of comparability, we include the exchange rate in the US model, although it is not a small open economy.

[9]In fact, in Hungary and South Korea the short-term interest rate remains above 1% throughout the entire time considered. In Sweden, the short term interest rate is 0.5 from Q3 2009 to Q2 2010 and from Q3 2014. However, due to the quick recovery in 2010 and the small number of periods where the ZLB might have been binding, we restrain from the incorporation of a shadow rate.

[10]We want to thank the authors for data provision.

undertaken major transformations after the dissolution of the Soviet Union. Our sample ends with the last observation available in the SWIID 6.0 database, i.e. in 2014 for Korea, Sweden, and the Czech Republic and in 2015 for the US, Canada, and Hungary.

### 4.2.2 Methodology

With the described variables at hand, we estimate the following reduced-form VAR model with external variables and lag length $P$, i.e. a VARX($P$) model:

$$Y_t = C + A_p(L)Y_{t-p} + \Gamma_q(L)X_{t-q} + \varepsilon_t \, . \tag{4.1}$$

$A_p(L)$ and $\Gamma_q(L)$ are lag-polynomial matrices of order $p$ and $q$ in the lag-operator $L$, where $p = 1, \ldots, P$ and $q = 0, \ldots, P$. $C$ captures deterministic components (i.e an included constant) and $\varepsilon_t$ is a column vector of reduced-form white noise error-terms and covariance matrix $\Sigma_\epsilon$. $X$ captures exogenous variables (i.e. the VIX for all non-US models). The lag-length $P$ is determined by the Akaike criteria.[11]
Identification of our underlying, unknown structural model of the form

$$B_0 Y_t = D + B_p(L)Y_{t-p} + \Theta_q(L)X_{t-q} + u_t, \tag{4.2}$$

and the respective shocks linked to it is conducted via sign restrictions. This requires a priori assumptions about the specific relations between the variables included in the VAR model. These assumptions can root in theoretical considerations as well as in empirically robust common wisdom.[12]
As we are interested in the interpretation of the effects of monetary policy shocks in a sensible manner, we only focus on the identification of the monetary policy shock and ignore other structural innovations to the model. Table (4.1) shows the assumed restriction scheme.

Table 4.1: Sign restrictions for an expansionary monetary policy shock

| Variable | Gini | GDP | Prices | Interest Rate | REER |
|---|---|---|---|---|---|
| Imposed restriction | unrestricted | + | + | - | - |

*Notes:* Gini is a surrogate for all inequality measures and variables related to the factor income that are considered in this paper. The VIX is unrestricted in the US model. Imposed restrictions hold for four periods, but the results are not very sensitive to shorter durations.

[11] The information criteria suggests a VAR(1)-model for the United States, Sweden, Czech Republic, and Hungary, a VAR(2)-model for Canada and a VAR(3)-model for South Korea.
[12] A detailed description of the idea and methodology can be found in Uhlig (2005).

We justify these assumptions as follows:
Expansionary monetary policy lowers overall market interest rates, either via policy rate cuts or monetary base expansion. This results in a stimulus of overall demand or at least does not cause demand to fall simultaneously. Overall prices should also adjust due to excess demand, or at least cannot be expected to decrease. The real exchange rate reaction is assumed to be negative because of capital outflows caused by overall lower yields in the economy. To capture the research question of this paper and pick up the controversy outlined in the discussed literature we leave the variables related to income inequality unrestricted. All restrictions are theory-implied and also confirmed in many empirical applications. We think that identification via sign restrictions is appropriate because we use fast-reacting financial markets variables as well as sticky variables such as the GDP or prices and thus do not want to restrict contemporaneous relations between the variables via e.g. an assumed ordering.

## 4.3 Monetary Policy and Income Inequality

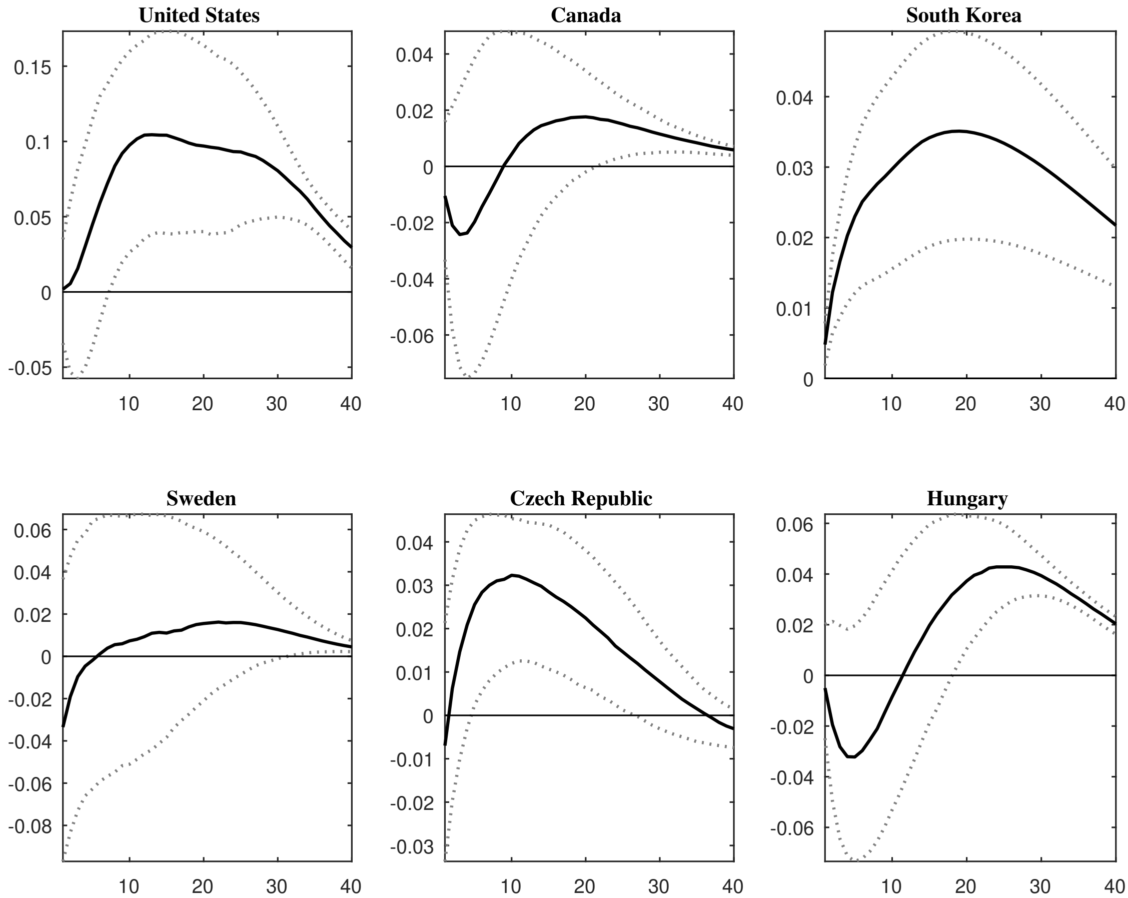
The ultimate goal of this paper is a) to examine the nexus between income inequality and monetary policy, b) emphasize the role of redistribution, and c) trace the channels of transmission. By usage of the aforementioned restrictions, we can pursue this goal.
To examine the linkage between monetary policy and income inequality as well as the role of governmental redistribution, we distinguish between the Gini of gross income and net income. Thus, we can scrutinize the respective responses to monetary policy shocks. Since the discrepancy of gross and net incomes stems from paid and received (income-) taxes and transfers, we are thus able to tackle the question concerning the role of governmental redistribution.

### 4.3.1 Response of Gross Income Inequality

First, we evaluate the effect of expansionary monetary policy on the distribution of gross income.[13] Figure (4.3) depicts the responses of Gini gross to an expansionary 25bp monetary policy shock.

[13]For the sake of greater clarity, we only depict the responses of the Gini indexes. Since we use sign restrictions, the fundamentals react as intended. Nevertheless, the complete set of impulse responses is available upon request.

Figure 4.3: Response of Gini Gross




*Notes:* Impulse responses of Gini gross to an 25bp expansionary monetary shock. The solid line depicts the median response. The dotted lines are the $16^{th}$ and $84^{th}$ percentiles.

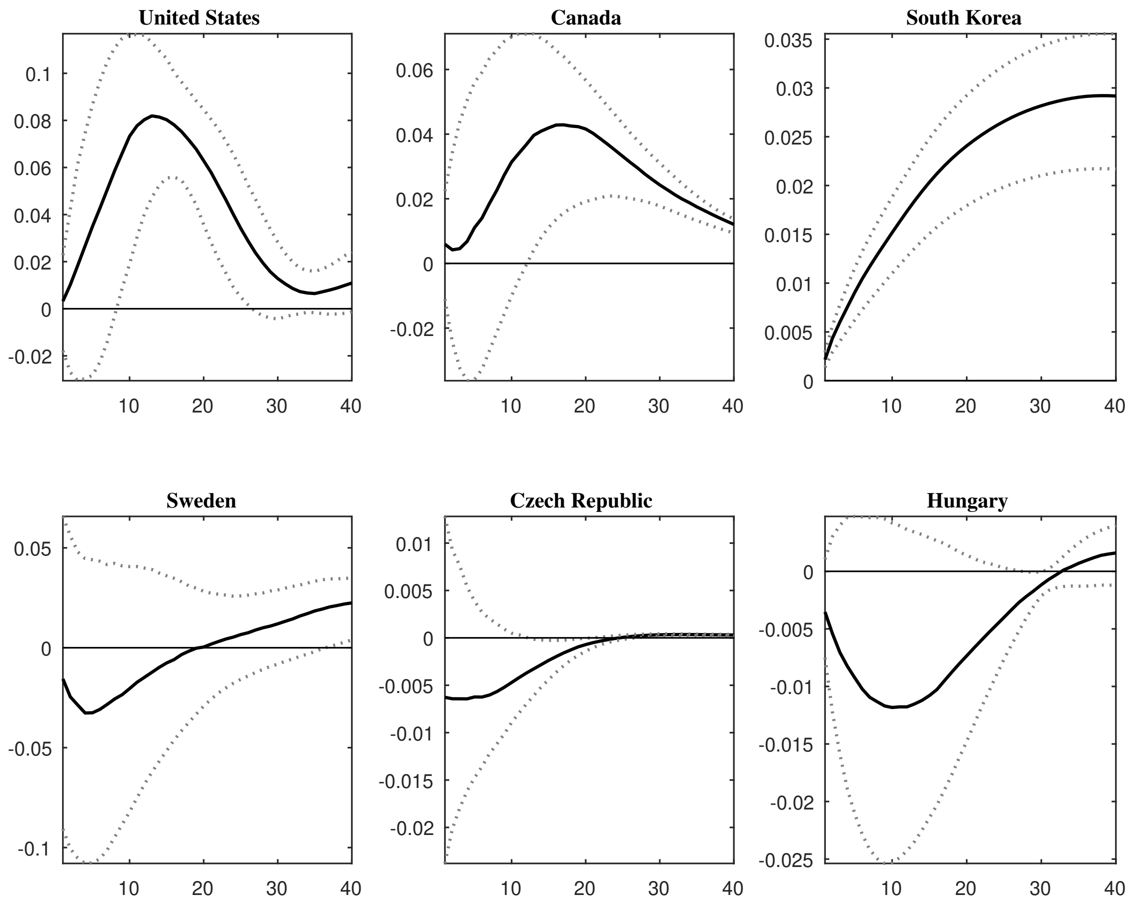
Two findings stand out. Inequality increases in all countries. The effect is most pronounced for the US, given a peak median response of 0.1 pp after 12 quarters (solid line), followed by Hungary. For the remaining countries, the peak response of the Gini index is above 0.015 pp.

Besides South Korea, the effect comes with some delay. It takes between eight and 30 quarters until the probability bands surpass the zero line. This finding comes at no surprise since the Gini index itself is rather sticky. Accordingly, the effect seems to be persistent since it seldomly dies out after 40 quarters.

### 4.3.2 Response of Net Income Inequality

Focusing on net income Gini coefficients brings several advantages. First, the general debate about equitable income distribution is predominantly based on net values, such that potential dampening effects through governmental redistribution are incorporated. Furthermore, wealth is largely accumulated by savings that stem from the remaining share of income. Thus, a steady increase in income inequality might embrace an accelerating effect: low-income households are barely able to save and thus cannot accumulate wealth while, at the same time, high-income households amass wealth progressively, which in turn might increase inequality furthermore. Hence, monetary policy actions that benefit the latter disproportionately might even expedite this process. However, ultra-loose monetary policy as well as unconventional monetary policy measures are under suspicion to be such policy actions. In this respect, Montecino and Epstein (2015), Mumtaz and Theophilopoulou (2017), and Saiki and Frost (2014) find a positive relation between unconventional monetary policy and inequality hikes for the US, UK, and Japan, respectively. As their analysis excludes top-income households or ends before the introduction of unconventional measures, these papers might even underestimate the unveiled effects.

Figure 4.4: Response of Gini Net




*Notes:* Impulse responses of Gini net to an 25bp expansionary monetary shock. The solid line depicts the median response. The dotted lines are the $16^{th}$ and $84^{th}$ percentiles.

Figure (4.4) outlines the results of our baseline model including the Gini of net income as our measure of inequality. It stands out that the effect of an expansionary shock is mostly tempered, compared to the response of Gini gross in Figure (4.3). For the US, we find a positive reaction in the short-term perspective that is notably smaller, namely 0.08 pp at its peak, than the rigid increase in Gini gross with its maximum at 0.1 pp. The difference between Gini gross and Gini net is most pronounced in Sweden, the Czech Republic, and Hungary - the countries with the highest relative redistribution in our sample. Here, the tendency for an increase in inequality is immensely mitigated. Finally, we find no notable difference in the response of the Gini net in Canada as against the response of Gini gross.

In summary, we find that governmental redistribution can dampen the effect of expansionary monetary policy on income inequality. Furthermore, it seems that the extent of redistribution matters more than the initial level of inequality. Sweden, the Czech Republic, as well as Hungary - countries with the highest relative redistribution in our sample - experience the strongest dampening effect. South Korea, that has low levels of income inequality combined with low levels of redistribution, faces similar effects as the US and Canada.

Our findings are in line with Saiki and Frost (2014), Montecino and Epstein (2015), and Mumtaz and Theophilopoulou (2017), but contrast the much-noticed work by Coibion et al. (2017). The discrepancies in the findings are likely linked to the following issues: Firstly, our Gini measures differ. Coibion et al. (2017) derive their Gini measures from household survey data that do not cover the top 1% of the income distribution. This is troublesome given the dominant role of top income households among the income distribution, as emphasized by Atkinson, Piketty and Saez (2011). For example, in 2007 the top 1% accounts for about 23% of the total received income in the US. Therefore, we rely on the mean estimator from the SWIID which incorporates the complete income distribution. Another merit of this database is that it enables cross-country comparability. Secondly, the debate about increasing income inequality gained momentum especially since the Financial Crisis and the associated conduct of monetary policy. We take this extraordinary period into account. Lastly, we apply a substantially different estimation approach.

## 4.4 Transmission of Monetary Policy on Inequality

In this section, we want to elaborate what channel-related variables are involved in the transmission of monetary impulses to overall income dispersion. As outlined above, we focus on the employment channel and the income composition channel. We pick up the ideas of Bernanke and Gertler (1995) who disentangle overall transmission of monetary policy shocks to the real economy by taking a closer look at variables assumed to be involved in the transmission. With this approach, they shed light on major driving forces and related channels of monetary transmission linked to them. Similarly, we use variables related to the channels outlined previously to account for the variety of possible mechanisms that drive the observed movement in the overall Gini coefficients presented in Section (4.3). These variables replace our Gini coefficient in the baseline VAR model while identification assumptions remain unchanged. We proceed as follows: First, we examine in how far the employment channel is involved in the transmission of monetary policy. Second, we separately include both components of the income composition channel in our VAR model. Third, we relate them to each other to figure out in how far their ratio is affected by monetary policy, or, in other words: Does the reaction of one income component dominate the reaction of the other. Thus, we need variables that can be assigned to the channels to assess the importance and overall role each channel plays in the six countries. We describe them in the following in more detail.

### 4.4.1 Employment Channel

#### Data

To take a closer look at the employment channel, we check in how far employment reacts to monetary policy shocks. In contrast to most literature, we do not use unemployment rates, but overall employment instead because the officially reported rates are often biased since not every unemployed person registers. Additionally, changes in the labor force participation might distort unemployment rates although overall employment remains less affected or even unchanged. Thus, our measure captures more precisely the real utilization of the factor labor in our samples. To have a common data source, we rely on total employment provided by the OECD.[14]

#### Results

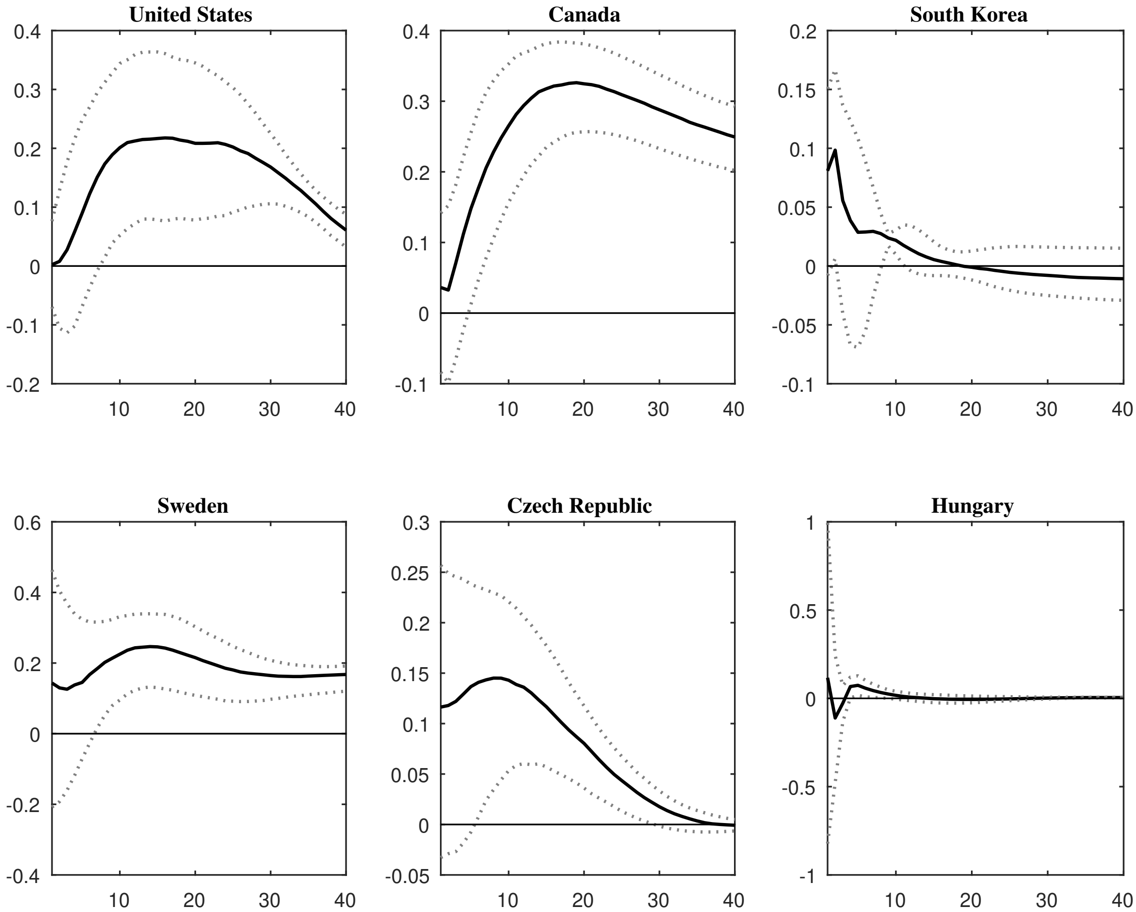
According to the employment channel, an expansionary monetary policy shock lowers income inequality via its stimulating effect on the labor market. Typically low-skilled low-income households benefit from this channel. To evaluate the relevance of this channel, we substitute the Gini variable with the log of total employment in the respective country.

Figure (4.5) shows the impulse responses of employment to an expansionary monetary policy shock. Such shocks have a notable stimulating impact on employment in all

[14]Due to data issues for the US we proxy the total number of employed persons by the employed workers according to the non-farm payroll statistics.

countries. The reaction in employment is in general weaker in the countries with high redistribution. This can probably be linked to their more regulated labor markets, e.g. higher degrees of dismissal protections.

Figure 4.5: Monetary Policy Shocks and Employment




*Notes:* Impulse responses of employment to an 25bp expansionary monetary policy shock. The solid line reflects the median response, the dotted lines show the $16^{th}$ and $84^{th}$ percentiles.

The contrasting responses of the Gini net on the one hand and employment on the other hand indicate that the employment channel is dominated by other driving forces. Hence, we take a more detailed look at the primary factor income sources of households: labor and capital.

### 4.4.2 Income Composition Channel

#### Data

The income composition channel distinguishes between major sources of households' overall earnings: labor-related income and capital pay-offs. Thus, we include these different sources into our analysis. As we are primarily interested in net effects, we focus on disposable income. National accounts and income statistics provide detailed data to construct different variables based on the sub-components related to the production factors capital and labor. More precisely, in our analysis capital income consists of net interest income, dividends after taxes and net rental income. It is computed as the sum of net operating surplus, which is gross operating surplus (GOS) less consumption of fixed capital for the corporate sector, and net mixed income (NOS+NMI). Labor income incorporates solely (net) compensation of employees, i.e. wages, salaries, and employers' social contributions.[15] Again, we rely on data from the OECD to overcome possible problems of cross-country comparability.[16] For South Korea, all income data are only available on a yearly frequency. Thus, we need to interpolate capital- and labor-related income. For the Czech Republic, the net operating surplus and the mixed income is only available from 1999. Since gross operating surplus and mixed income (GOS+MI) is accessible from 1995, we construct NOS+NMI from 1995 to 1998 by assuming the share of NOS+NMI in GOS+NMI in this time is identical to the share in 1999.[17] For Hungary, the OECD provides quarterly data for labor-related income and GOS+NMI, but only yearly data for NOS+NMI. This time we first construct each quarters' share in the yearly values of GOS+NMI. We then assume that the share for NOS+MI is identical.

### Response of Labor-Related Income

We replace the Gini variable in the baseline model by the log of labor-related income. Since labor income and employment are strongly correlated, their outcomes are expected to be similar, too.

The results are represented in Figure (4.6). In all countries, labor-related income increases after an expansionary shock. The peak median responses vary between about 1.1 pp (Sweden) and about 0.1 pp (South Korea).

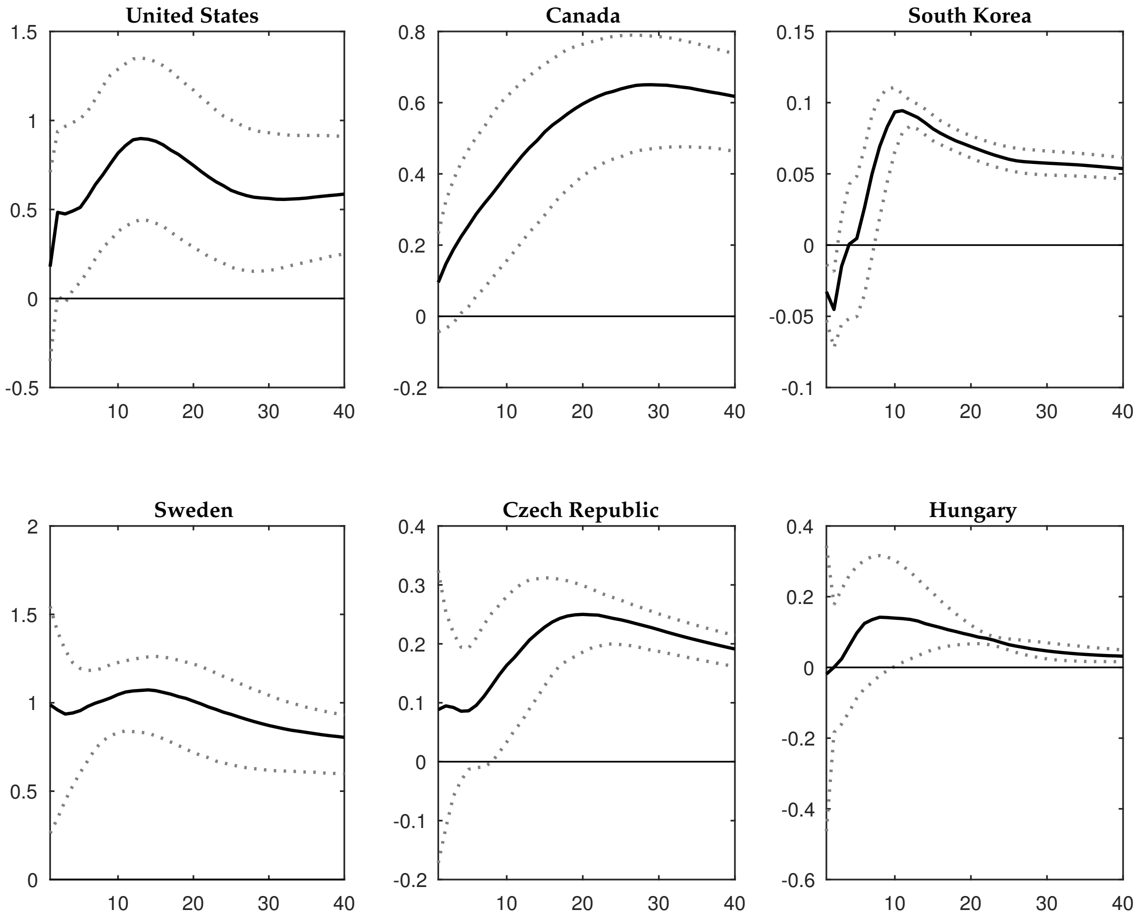
Unfortunately, we cannot draw conclusions about the distribution of labor income across households. Nevertheless, wages are the primary income source for the vast majority of households. In combination with the findings we draw from the employment channel, the results on labor-related income indicate that employees benefit from an expansionary monetary policy shock.

[15] Including transfers, for some households the dominant income source, would have been an option if all countries collect and process data on a similar approach and provide them for sufficiently long periods. Unfortunately, for the sake of cross-country comparability, we cannot include them in a meaningful manner.

[16] All data are seasonally adjusted and denoted in constant prices.

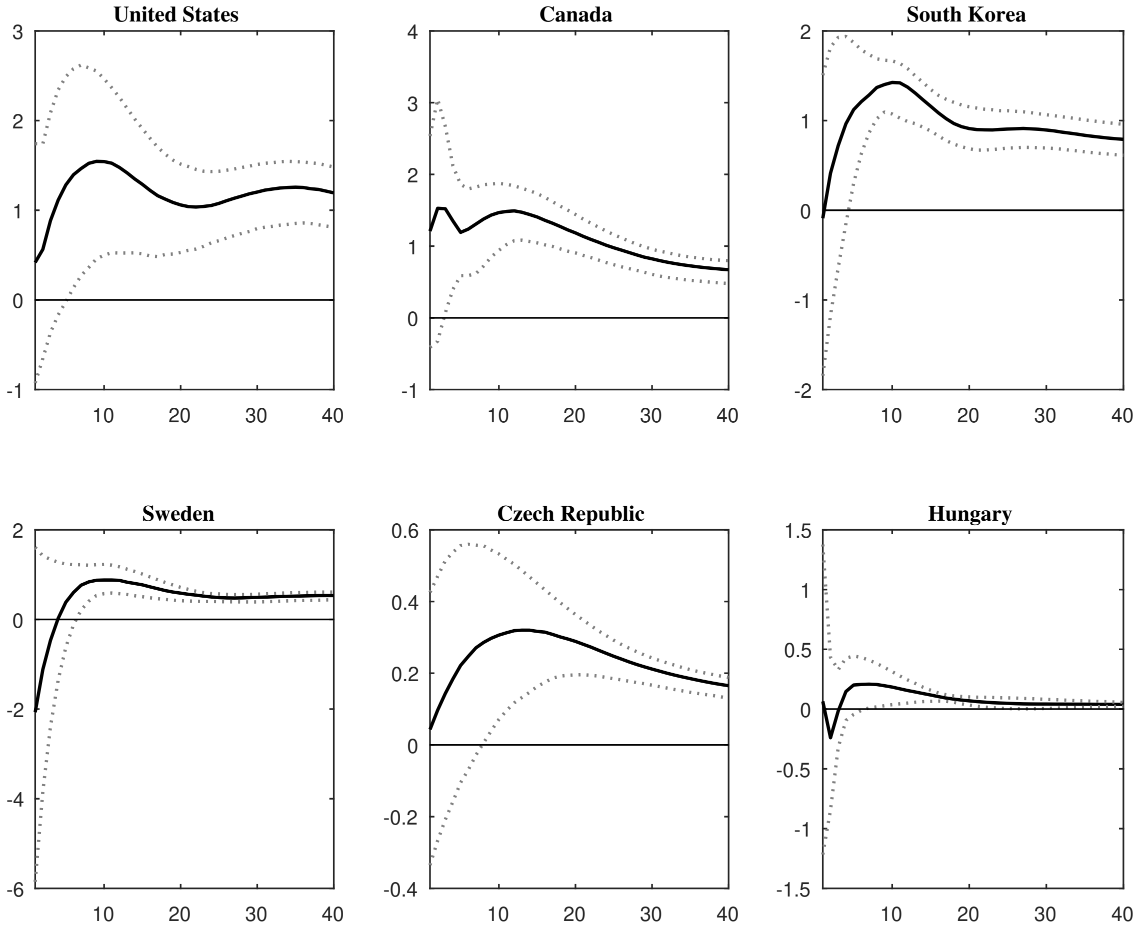
[17] The share of NOS+NMI in GOS+NMI varies between 55% and 62% from 1999 to 2014.

Figure 4.6: Monetary Policy and Labor Income




*Notes:* Impulse responses of labor-related income to an 25bp expansionary monetary policy shock. The solid line reflects the median response, the dotted lines show the $16^{th}$ and $84^{th}$ percentiles.

Figure 4.7: Monetary Policy and Capital Income




*Notes:* Impulse responses of capital-related income to an 25bp expansionary monetary policy shock. The solid line reflects the median response, the dotted lines show the $16^{th}$ and $84^{th}$ percentiles.

## Response of Capital Income

To obtain aggregate net capital income, we sum up net operating surplus and net mixed income. Following Atkinson, Piketty and Saez (2016), we assume that high-income households are the main receivers of capital income. Thus, an increase in capital income indicates that these households benefit disproportionately, as opposed to low- and middle-income households.

Figure (4.7) indicates a similar pattern of net capital income to labor income. There is a notable increase in capital income in all countries. Besides the boost in asset prices, the stimulus of real activity leads to e.g. increasing corporate profits or rents and thus higher capital earnings for shareholders or real estate owners.

We find the most pronounced responses for countries with relative little redistribution, i.e. peak responses greater than one. While the size of the responses of the Czech Republic and Hungary are quite small, the impulse response of Sweden is similar to those of less redistributing countries.

### Response of the Capital-Wage-Ratio

As has been shown above, an expansionary monetary policy shock leads to an increase in both, capital income and labor-related income. Depending on the composition of income among households, this leads to either a rise or a fall in inequality. The income composition channel states that income inequality grows if capital income receivers benefit disproportionately, and vice versa. Hence, we finally evaluate the relevance of the income composition channel via the response of the capital-wage-ratio after such a monetary policy shock. Since labor-related income also represents changes in employment, the capital-wage-ratio is not only suited for the evaluation of the income composition channel. It also indicates whether the income composition channel is dominating the employment channel.

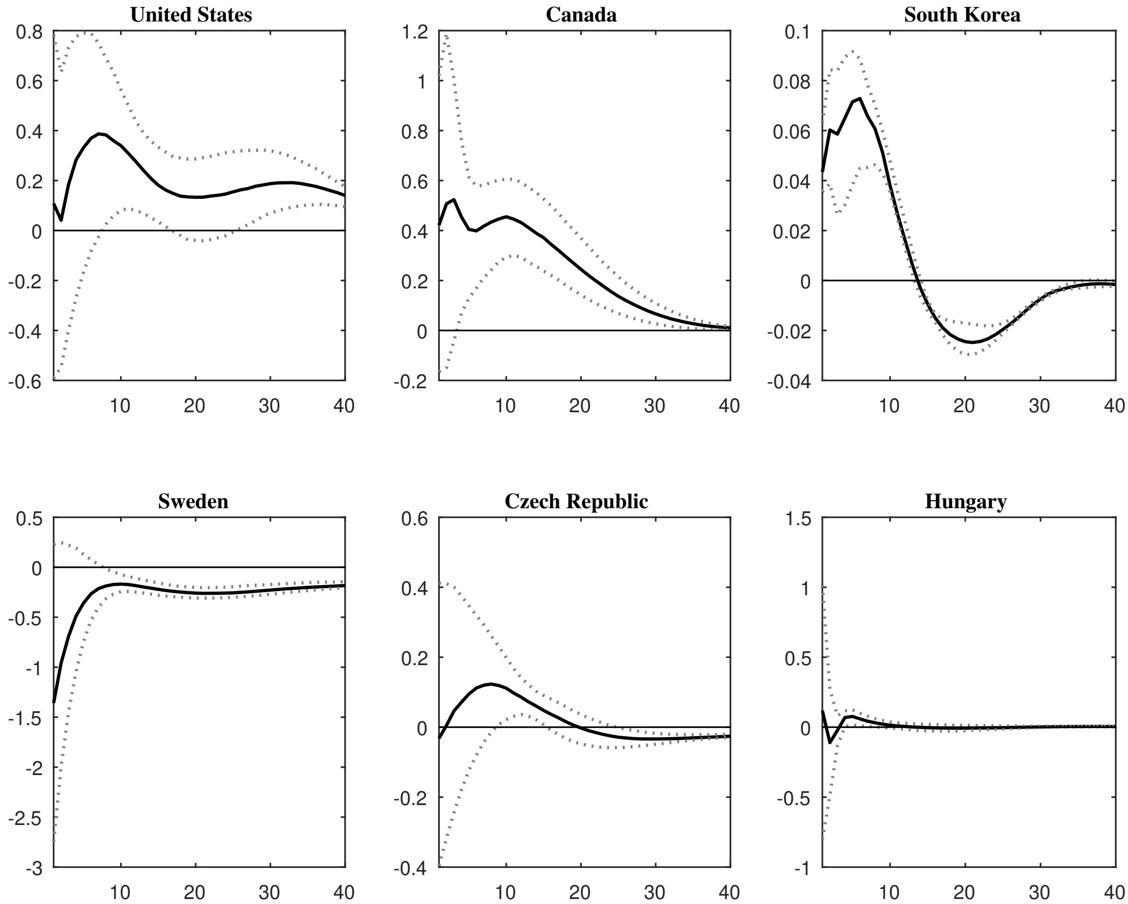
The respective impulse responses are presented in Figure (4.8). Out of the countries with relatively little redistribution, the US and Canada exhibit a clear increase in the capital-wage-ratio. In contrast, the responses of the extensively redistributing countries show either no clear response (Czech Republic and Hungary) or even a negative reaction (Sweden). South Korea stands out as a special case here. While capital owners benefit disproportionately in the short-term in South Korea, this effect is reversed after roughly 15 quarters.

How does monetary policy impact inequality? We find evidence that the primary mechanism is the composition of income. The increase in Gini cannot be explained by the employment channel because we expect that low-income households benefit from a stimulated labor market. Additionally, the labor market reacts stronger in less redistributing countries. The increase in the Gini net is in contrast to that finding. At the same time, we find that capital income increases more than labor income. Taken together, it is likely that the income composition channel explains the nexus between monetary policy and income inequality. This is true for both, much redistributing and less redistributing countries. Nevertheless, since the first-mentioned do not show an increase in net income inequality after expansionary monetary policy shocks and no increase in the capital-wage-ratio, both types of net factor income benefit more equally from expansionary monetary policy shocks in these countries. Thus, we conclude that income composition plays the primary role in the transmission of monetary policy shocks to income inequality.

## 4.5 Robustness

In this section we asses whether the results hold under different model specifications. One major concern about the methodology applied above is about the use of interpo-

Figure 4.8: Monetary Policy and the Capital-Wage-Ratio




*Notes:* Impulse responses of the capital-wage-ratio to an 25bp expansionary monetary policy shock. The solid line reflects the median response, the dotted lines show the $16^{th}$ and $84^{th}$ percentiles.

lated data. We verify whether our results hold if we incorporate yearly data instead. Furthermore, we follow Coibion et al. (2017) and present evidence that uses local projections as additional robustness. Finally, we check the sensitivity of our results to various samples.

### 4.5.1 Yearly VAR Model

The results found above rely on the assumption that the quarterly data we receive from linear interpolation of the Gini coefficients are similar to the true but unknown quarterly Gini coefficients. Thus, we test the outcome of our model by applying a VAR model with yearly data. If the results of the yearly and the quarterly VAR model are similar, we are confident that linear interpolation does not substantially affect the estimates. With the resulting shorter sample size we now incorporate only one lag and reduce our restriction duration to one period as well. Nevertheless, the short sample boosts uncertainty in the estimation and thus the resulting percentiles of the presented model should be treated with caution. Despite that, the major outcomes remain unchanged.

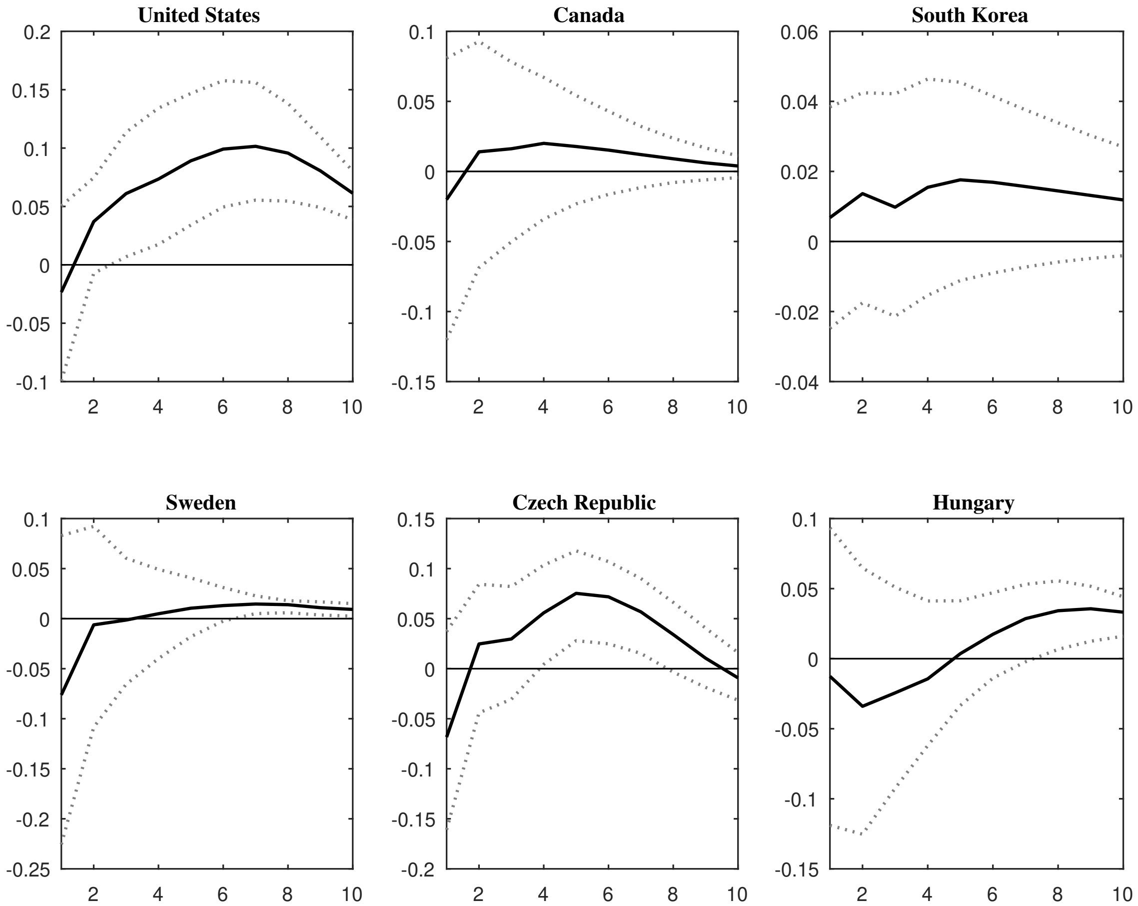
We again start by showing impulse responses of Gini gross (see Figure (4.9)). In line with the findings from Section (4.3) there is a tendency of an increase in inequality after an expansionary monetary policy shock in five of the six countries. Only Canada shows no clear pattern.

The impact of expansionary monetary policy shocks on the Gini net, Figure (4.10), is again quite heterogeneous across countries. In economies characterized by a high degree of redistribution, we can observe a lower sensitivity to a shock. In contrast, countries with little governmental intervention show no or, for the US, a positive reaction. The mitigating effects of governmental interventions can hence be supported by these outcomes.

### 4.5.2 Local Projections

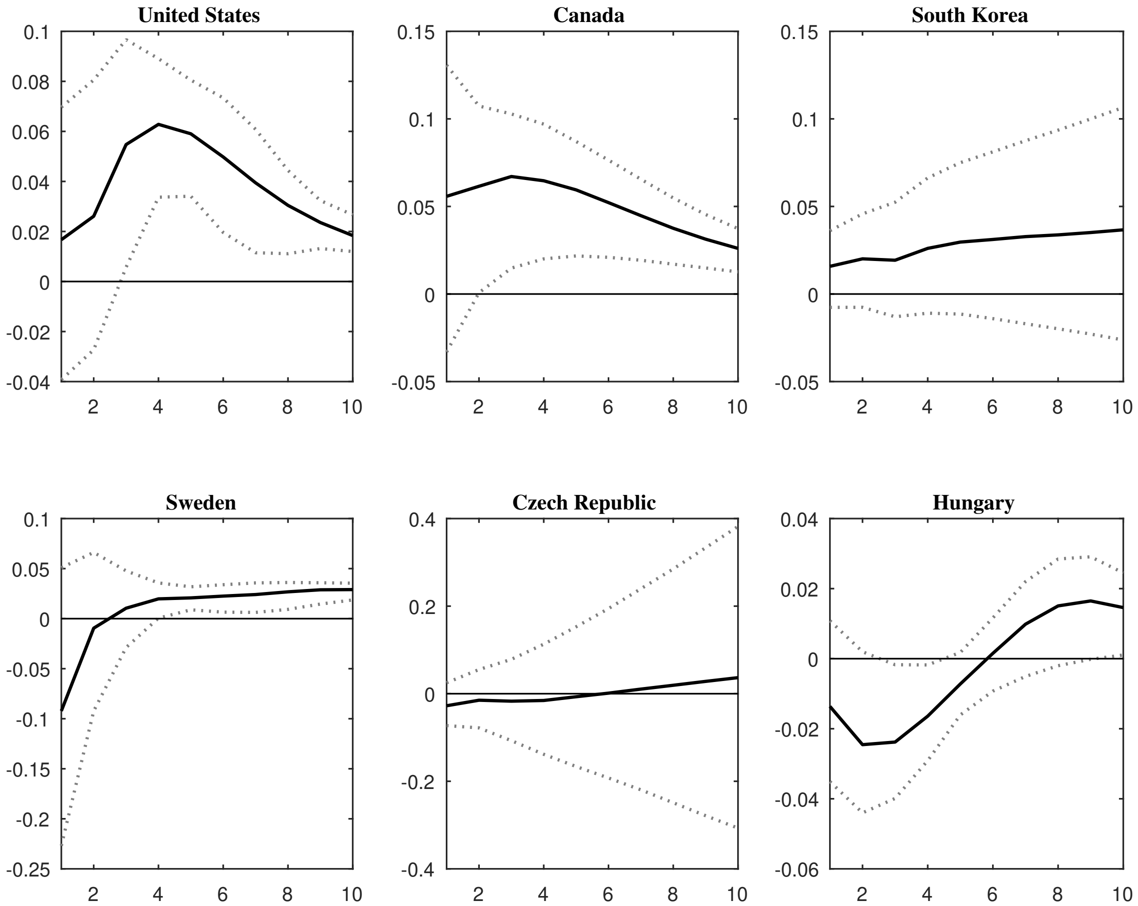
Thus far, we have solely considered VAR models. We now follow Coibion et al. (2017) and assess the role of monetary policy shocks for income distributions via impulse responses from local projections, as suggested by Jorda (2005). This methodology describes the response of an endogenous variable (i.e. Gini coefficient) to a monetary policy shock that enters as an exogenous variable into the model. The choice of the incorporated exogenous shock deserves further attention. Coibion et al. (2017) use Romer and Romer (2004) shocks for this purpose. This approach does not suite our specific data set for two reasons. First, Romer and Romer (2004)

Figure 4.9: Monetary Policy and Gini gross, yearly model




*Notes:* Impulse responses of Gini gross to an expansionary monetary policy shock. Estimates with yearly data. The solid lines reflect the median responses, the dotted lines are the $16^{th}$ and $84^{th}$ percentiles.

Figure 4.10: Monetary Policy and Gini net, yearly model




*Notes:* Impulse responses of Gini net to an expansionary monetary policy shock. Estimates with yearly data. The solid lines reflect the median responses, the dotted lines are the $16^{th}$ and $84^{th}$ percentiles.

shocks are only available up to the point where short-term interest rates hit the ZLB. We explicitly want to account for periods characterized by unconventional monetary policy. Second, as Romer and Romer (2004) shocks are only available for the US, our analysis would lose its cross-country dimension. Thus, we derive our exogenous quarterly monetary policy shock for each country from a standard three variable VAR model consisting of real GDP, consumer prices, and key policy rates, identified via recursive ordering which relies on the assumption that monetary policy reacts contemporaneously to output and prices, but not vice versa.[18]

With these exogenous shocks at hand, we estimate local projections. Following Jorda (2005), our model is given by

$$y_{t+h} = c + \beta_h \hat{u}_t^{MP} + \gamma_h' \sum_{s=1}^{q} y_{t-s} + \varepsilon_{t+h}\,. \tag{4.3}$$

Hereby, $y_t$ is the inequality measure and $\hat{u}_t^{MP}$ the policy shock that stems from the VAR model described above. We set $q = 4$ so that the four latest inequality measures that appeared before the shock are incorporated as control variables.[19] By plotting $\beta_h$ as a function of $h$ along with error bands we get impulse responses. To circumvent serial correlation among the residuals, we apply Newey-West standard error correction. The resulting impulse responses are depicted in Figure (4.11) and (4.12).

In line with our VAR findings, we observe in four out of six countries a clear increase in the Gini gross after an expansionary monetary policy shock. South Korea and the Czech Republic display an increase in at least some periods.

Regarding the Gini net, local projections confirm our previously presented results as well. Countries with a high degree of redistribution show no clear pattern. This indicates that governmental intervention is able to dampen the effect of monetary policy shocks on gross income dispersion.

It is worth noting that despite the use of a similar methodology as Coibion et al. (2017) we obtain diverging results. Hence, we next test whether the differences stem from different samples.

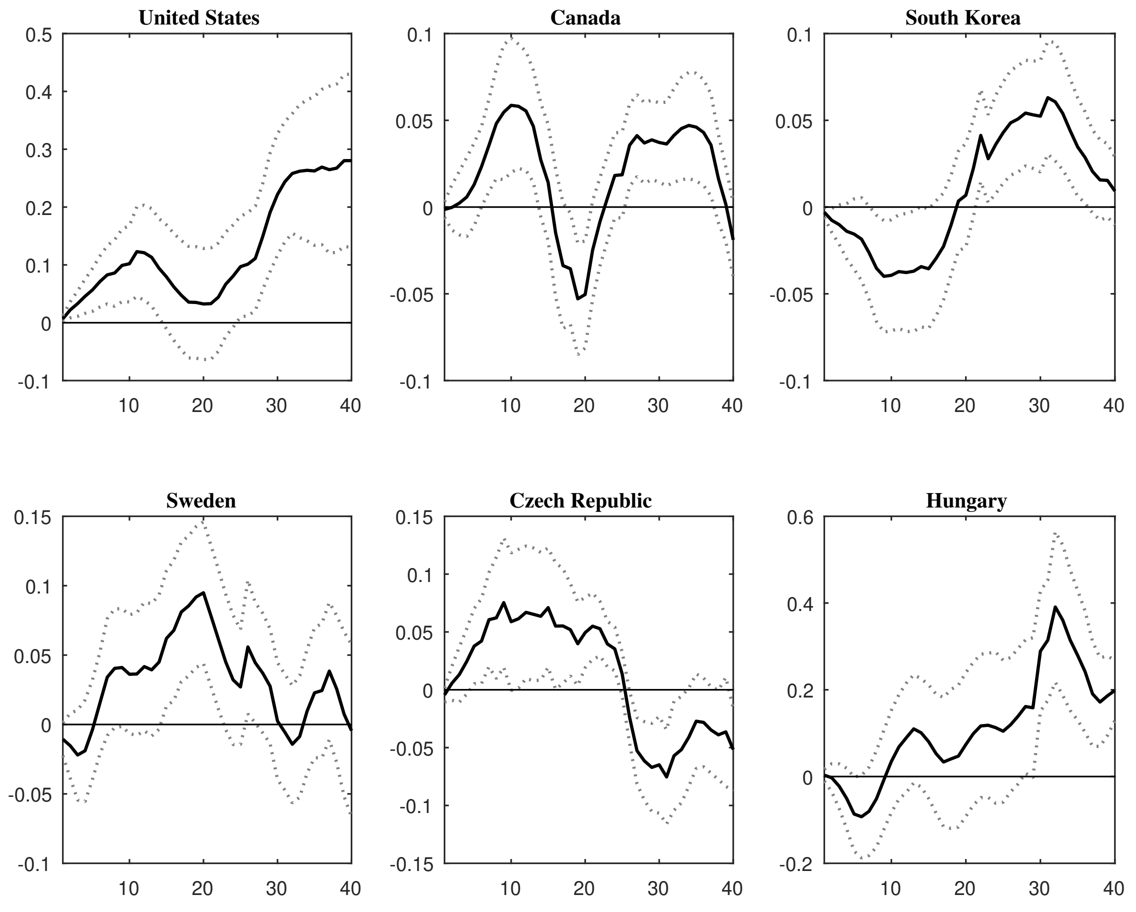
### 4.5.3 Sample Size

Thus far, data availability limited the analyzed estimation horizon from the beginning of the 1990s to 2014 or 2015, respectively. To ensure the comparability with Coibion

[18]The correlation between the resulting shock series (for the US economy) and the quarterly aggregated Romer and Romer (2004) shocks is about 0.6 for the available period (1990 to 2007).

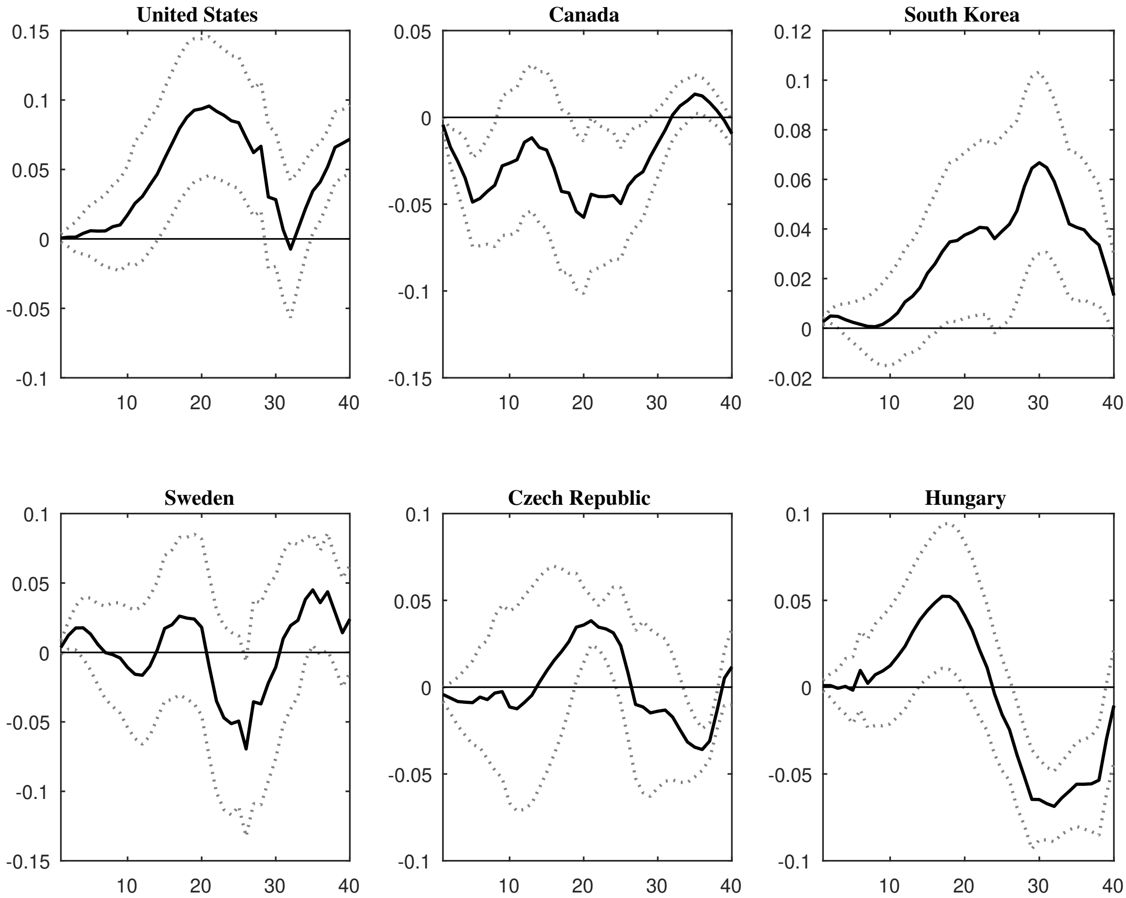
[19]Altering $q$ does not yield substantially different results.

Figure 4.11: Local Projections for Gini Gross



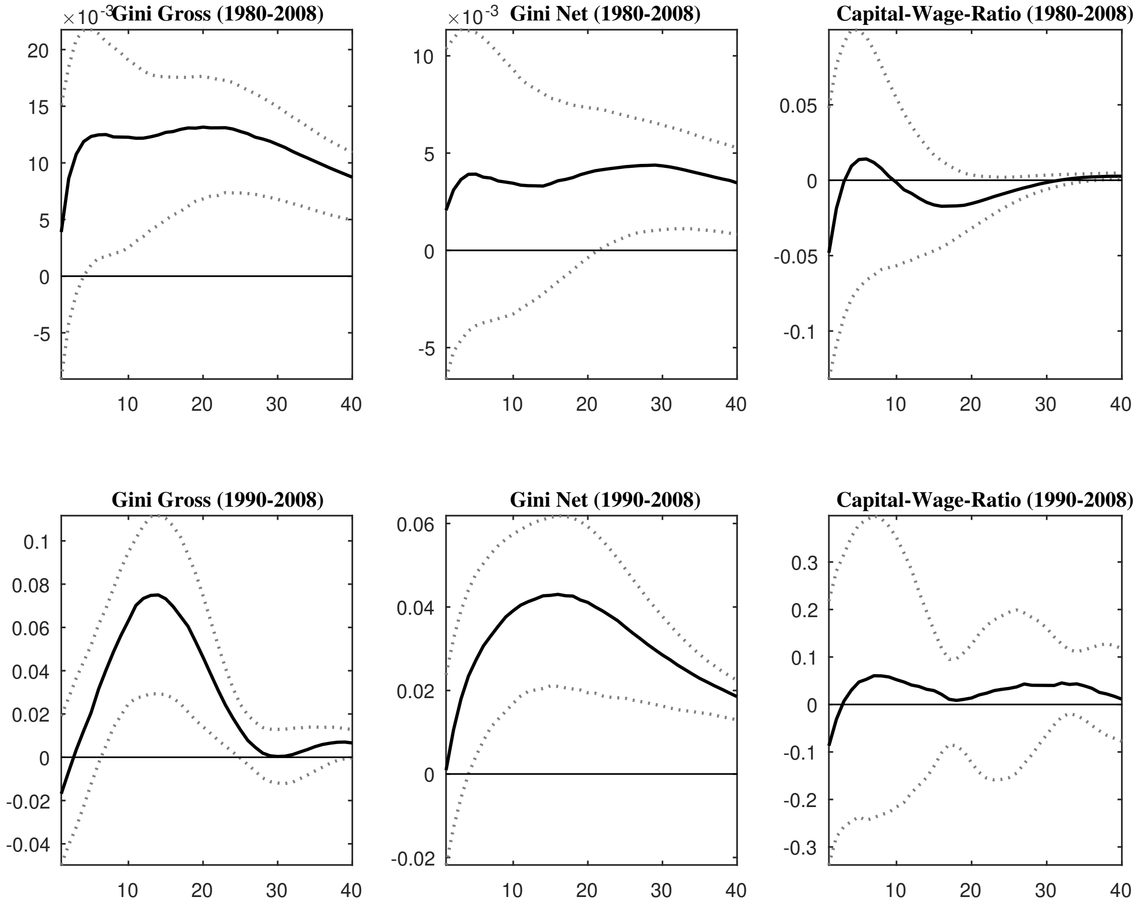

*Notes:* Local projections for $\beta_h$ (solid line) and the respective one standard error bands (dotted lines). Shock measured in standard deviation units and inverted to reflect expansionary shocks.

Figure 4.12: Local Projections for Gini Net




*Notes:* Local projections for $\beta_h$ (solid line) and the respective one standard error bands (dotted lines). Shock measured in standard deviation units and inverted to reflect expansionary shocks.

Figure 4.13: US: Excluding Financial Crisis




*Notes:* Impulse responses of net Gini to an expansionary monetary policy shock. The solid lines reflect the median responses, the dotted lines are the $16^{th}$ and $84^{th}$ percentiles. VIX exluded in sample 1980-2008 (upper row) and included in sample 1990-2008 (lower row).

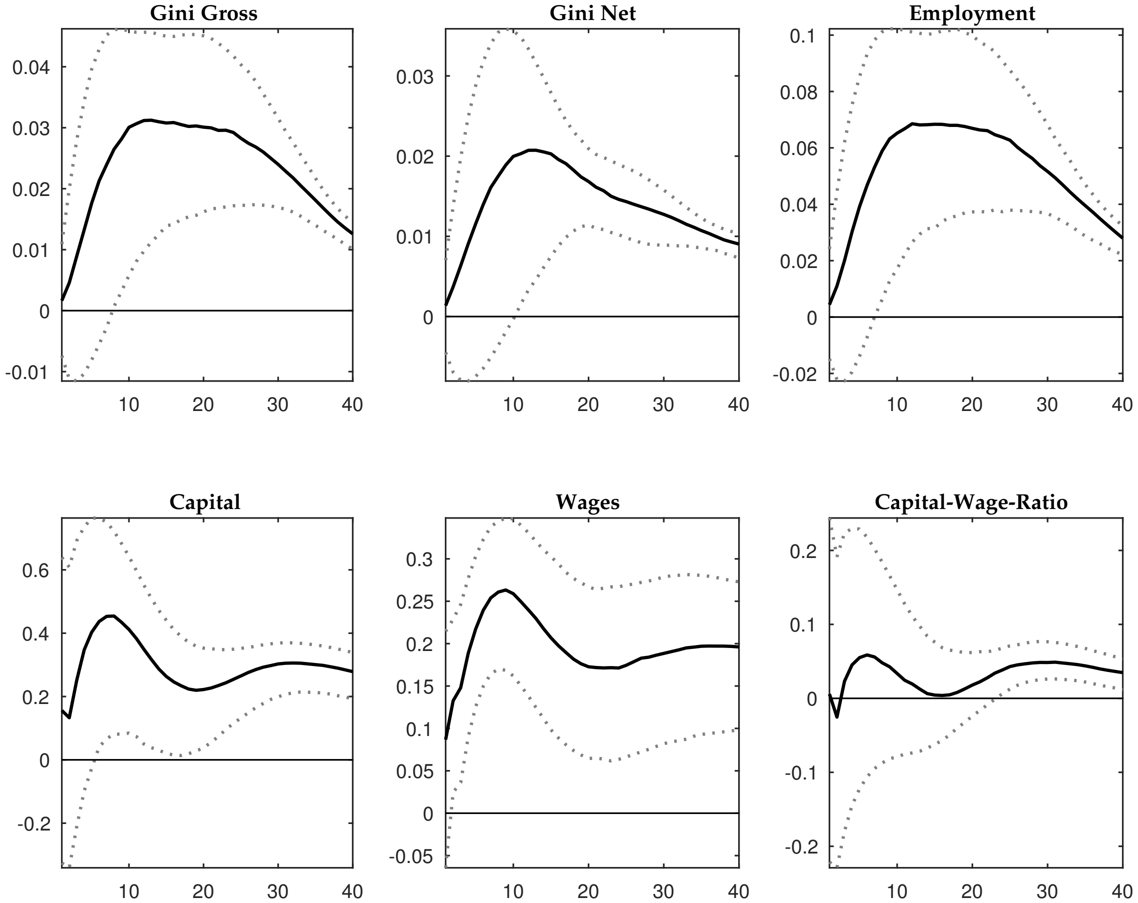
et al. (2017) we perform two further robustness exercises: we firstly estimate a US model akin Coibion et al. (2017) before we incorporate the recent financial crisis to examine its effect on the nexus between monetary policy shocks and income inequality.

One discussed driving force for the nexus between income inequality and expansionary monetary policy are the unconventional monetary policy measures following the Financial Crisis.

Figure (4.13) presents model outcomes for 1980 Q1/1990 Q1 - 2008 Q4. The results do not differ notably from our results so far, no matter whether we estimate a model with or without the VIX (1990 Q1 vs. 1980 Q1).

If we include the recent Financial Crisis into our sample, the magnitude of the response of inequality to a monetary policy shock increases, see Figure (4.14). As pointed out by Montecino and Epstein (2015), unconventional monetary policy measures have indeed raised income inequality in the US. Our analysis supports

Figure 4.14: US: Long Sample 1980 - 2014




*Notes:* Impulse responses of net Gini to an expansionary monetary policy shock. The solid lines reflect the median responses, the dotted lines are the $16^{th}$ and $84^{th}$ percentiles.

their findings.

In brief, our results are qualitatively robust to a variety of methodological as well as sample selection aspects. Expansionary monetary policy shocks increase income inequality.

## 4.6 Conclusion

In the recent decade, the issue of rising income inequality gained more and more attention in the public perception as well as in the political debate. The nowadays observable historically high levels of income dispersion are accompanied by an environment of very expansionary monetary policy. In this respect, we add new empirical evidence to the current controversy. To assess the effects of monetary policy shocks, we incorporate Gini coefficients in a standard macroeconomic VAR model consisting of GDP, consumer prices, a monetary policy variable, and the corresponding real exchange rate. Gini coefficients of gross incomes increase in all countries, namely the US, Canada, South Korea, Sweden, the Czech Republic, and Hungary, when facing expansionary monetary policy shocks. In contrast, the reaction of net income dispersion varies between the countries under consideration. Countries with a relatively low degree of redistribution, i.e. the US, Canada, and South Korea, show notable positive reactions of Gini net in the presence of expansionary monetary policy shocks. On the contrary, this measure does not increase in countries with a high degree of redistribution.

Furthermore, we take a more detailed look at the importance of two major transmission channels, the employment channel and the income composition channel. The reaction in employment, captured by the total number of employed people, shows the expected positive sign in all countries. Again, the reaction is weaker and less pronounced in countries with a high degree of redistribution. By splitting the composition of net national income into its major parts, labor-related income and capital-related income, we can evaluate which income category benefits disproportionately. While both components are in general affected positively, their ratio indicates that in the US, Canada, and South Korea capital owners benefit disproportionately. As the increase in employment cannot offset the surge in net income inequality we conclude that the composition of income outweighs the positive labor market effects. The capital-wage-ratio indicates that in countries with a high degree of redistribution both income sources seem to profit similarly. We conclude that the distributional effects of monetary policy (on disposable income) can be addressed by the degree of governmental intervention.

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# 5 The Nexus between Lockdown Shocks and Economic Uncertainty: Empirical Evidence from a VAR Model

This paper is available under[1]

Hafemann, L. (2021): 'The Nexus between Lockdown Shocks and Economic Uncertainty: Empirical Evidence from a VAR Model', *MAGKS Joint Discussion Paper Series in Economics* No. 32-2021.
https://www.uni-marburg.de/en/fb02/research-groups/economics/macroeconomics/research/magks-joint-discussion-papers-in-economics/papers/2021-papers/32-2021_hafemann.pdf

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[1]We are grateful to David Finck and Peter Tillmann for valuable feedback.

# The Nexus between Lockdown Shocks and Economic Uncertainty: Empirical Evidence from a VAR model

Lucas Hafemann*,[1]

The contribution of this paper is twofold. First, we introduce a daily vector autoregression (VAR) model for the US economy that allows discerning between lockdown shocks and a real business cycle shocks. With this methodology at hand, we then evaluate the impact of lockdown measures on economic uncertainty in a second step. Overall, we only find a moderate positive impact on uncertainty levels that is, in particular, weaker than the impact of the real business cycle shock. Taking a more granular perspective, we observe that in particular uncertainty related to entitlement programs increases and monetary policy uncertainty decreases after a lockdown shock.

**Keywords:** COVID-19, lockdown, shock identification, market uncertainty

**JEL classification:** E60, E62, E65, G01

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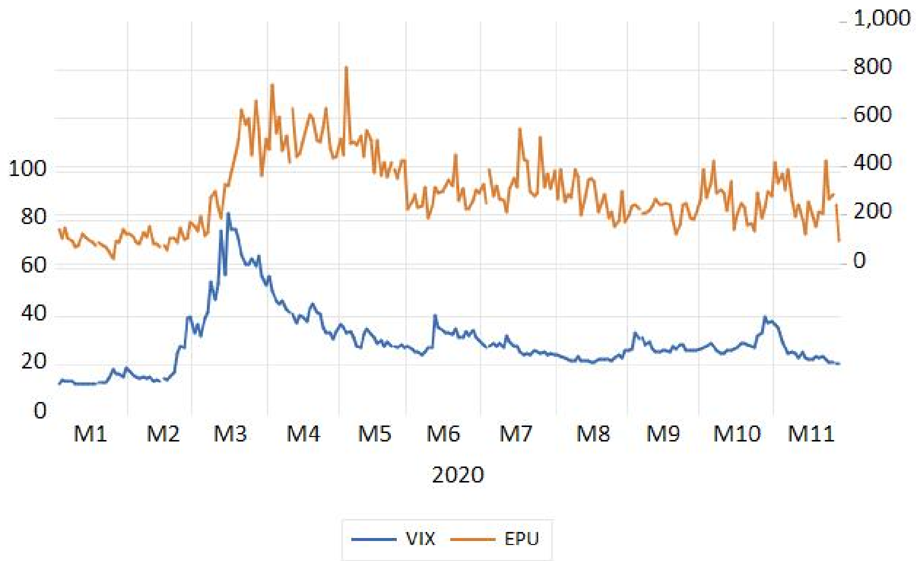
## 5.1 Introduction

*"We still have a lot of uncertainty about, you know, the current pandemic, about its development, about the lockdown measures, the containments, their length, and our forecast, our projection from December, which we believe is still valid, was predicated on lockdown measures continuing through the whole of the first quarter of 2021, and vaccination progressing very gradually."* Christine Lagarde, ECB Press Conference, January 21st 2021.

The nexus between (market) uncertainty and the real economy has been studied intensively in recent years and is at the forefront of political discussion. It gained additional momentum during the Covid-crisis as (i) the impact of the crisis on consumer behavior is unclear, (ii) new policy measures were introduced whose effects and costs are difficult to evaluate and (iii) the length and intensity of the crisis itself are unknown. With the increasing length of the crisis, market participants gained a better understanding of some of these key drivers of uncertainty. Consequently, aggregate uncertainty levels reached their peak during the first wave of infections, i.e. in March 2020, see Figure (5.1). This is also mirrored by the fact that the second and third waves affected asset prices considerably less than the first wave despite higher numbers of infections, hospitalizations and deaths.

From an empirical perspective, little is known about the contributors to the spike in uncertainty. In particular, the literature largely refrains from assessing the impact of lockdown measures on uncertainty, most probably, due to endogeneity concerns in the estimation process. In this vein, disentangling the effects of lockdown measures from other factors is troublesome as the economy is hit by a multitude of shocks over the business cycle. Since 2020, lockdown shocks are an additional source leading to fluctuations in real economic variables. The endogeneity arises from the fact that policymakers base their decision on many factors, including economic considerations. We close this gap in the literature and isolate the lockdown shock from other business cycle shocks. Specifically, we estimate daily and weekly VARs for the US economy via a mixture of sign and zero restrictions. The identification builds on the stylized fact that, contrary to broader stock market indices, the Amazon share price increases when lockdown measures are tightened. Consequently, we assume that a lockdown shock increases the share price of Amazon while it decreases the S&P 500 Industrials total return index. In contrast to that, the other business cycle shocks are described by a situation where the Amazon stock price and the S&P 500 move in tandem.

Figure 5.1: Developments of Uncertainty and Volatility in 2020




*Notes:* The left (right) axis corresponds to the VIX (the Economic Policy Uncertainty Index by Baker, Bloom and Davis (2016)).

Our contribution is, thus, twofold. First, we are, to the best of our knowledge, the first to identify lockdown shocks within a daily VAR framework. Second, we assess the impact of lockdown measures on a wide range of uncertainty measures. Overall, we find that lockdown shocks have a smaller impact on uncertainty than other contractionary real business cycle shocks. We argue that several opposing effects associated with the enforcement of lockdowns are likely behind this finding. When we take a more granular perspective, we observe that, in particular, monetary policy, government spending and regulation uncertainty decrease after a lockdown shock.

Several studies analyze the impact of the COVID-crises on uncertainty and volatility. Albulescu (2021) finds that Covid cases, as well as fatality ratios, positively affect market volatility in the US. Bakas and Triantafyllou (2020) investigate the impact of economic uncertainty associated with the pandemic on the volatility of commodity prices and find a strong positive relationship. Caggiano, Castelnuovo, and Kima (2020) show that the COVID-related uncertainty results in a 14% cumulative loss in the world-wide industrial production levels over one year, under the assumption that from February 16 to March 16 2020, the VIX exclusively moved due to the COVID outbreak. Zaremba et al. (2020) is the work closest to ours, as they also analyze the relationship between policy responses to the COVID-crisis and stock market volatility. For a panel of 67 countries, they find that policy responses such as school closures or public event cancellations increase equity market volatility. The paper is structured as follows. Section (5.2) introduces the data set and the methodology. Section (5.3) reports our empirical results and Section (5.4) concludes.

## 5.2 Identifying lockdown Shocks

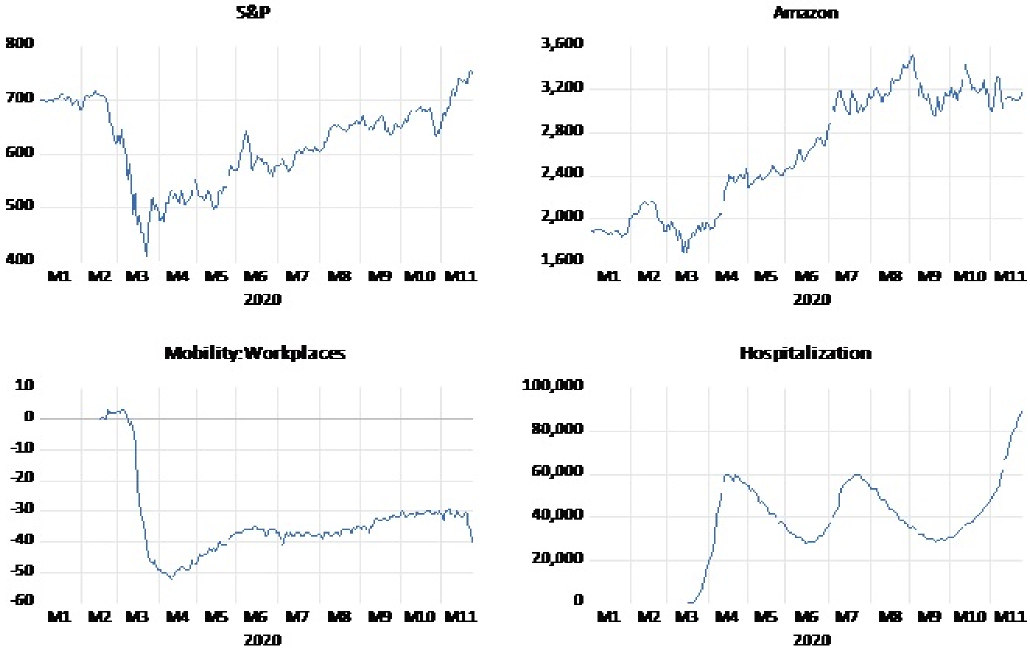
As outlined above, our goal is to analyze the effect of lockdowns within a VAR framework. Before we outline the methodology behind the VAR in Section (5.2.2), we first introduce our data set in Section (5.2.1).

### 5.2.1 Data

Our findings stem from a series of VAR models with five variables each for the US economy covering the period from 03/17/2020 to 11/27/2020. The vector of endogenous variables includes the S&P 500 Industrials total return index ($SPI_t$), the total return stock index for Amazon ($Amazon_t$), the Google Community Mobility Reports index for workplaces ($Workplaces_t$), an uncertainty index ($Uncertainty_t$) and the number of patients in hospitals that have been tested positive for Covid-19 ($Hospitalized_t$). Google $Workplaces_t$ describes by how much the aggregate mobility changes in comparison to a baseline scenario. More precisely, it indicates to what degree employees have been at their workplace, i.e. the percentage change from the baseline scenario.[1] We consider the log of all other raw time series and multiply them with 100 so that the impulse responses show deviations from the trend in percent. The data for the stock indices are taken from Thomson Reuters Datastream. The COVID Tracking Project provides us with data on active cases and hospitalisation.[2] Data availability on hospitalization is the limiting factor for our sample, as the data set exhibits no entry prior to 03/17/2020. We distinguish between a core set of variables that are necessary for the identification of a lockdwon shock and non-core variables indicating the responses of variables of interest to the lockdown shock. The list of core variables includes the stock indices, the mobility index and the number of hospitalized persons as these variables identify the lockdown shock. These variables remain unchanged in all estimations. Additionally, in every model, one non-core variable is included. The replacement of the non-core variable allows us estimating alternative models.

[1] See Aktay et al. (2020) for a detailed description of how the index is constructed.
[2] See https://covidtracking.com/data/national for more details.

Figure 5.2: Developments Core Variables in 2020




*Notes:* The upper row displays the stock market indices and the lower row shows the development of the mobility index and the hospitalisation.

Figure (5.2) outlines the development of the core variables before any transformation has been made. As the number of hospitalized persons indicates, there are three waves of the pandemic in our sample. The first wave occurred in March and April, the second in June and July and the third in October and November. For the S&P Index, we observe a decline of 43% from 2/12 until 3/23, i.e. during the first wave of infections. Afterward, we see a strong rebound. Since mid-November, the index is above pre-crisis levels despite high numbers of infections and hospitalization associated with the second and third wave. Some economists thus argue that the stock market is not well anchored any more. While the question of stock market sustainability is beyond the scope of this study, such an argument is only valid if one argues that the efficient market hypothesis is not valid, which is a strong assumption. However, as we outline below, we receive qualitatively similar results when we rely on the household‘s expectations of the coronavirus impact on GDP as the real economic variable instead. The corresponding data stems from the Cleveland Fed‘s daily consumer survey.[3] In comparison to the S&P Industrials Index, the decrease of the share price of Amazon in March is small. Furthermore, the trough of the Amazon stock price occurs earlier and the rebound is more pronounced. In fact, the Amazon stock price more than doubles from March to September. The correlation between

[3]See https://www.clevelandfed.org/en/our-research/indicators-and-data/consumers-and-covid-19.aspx for details.

daily returns of the two stock market indicators is 0.36. The mobility index also decreases substantially during the first wave. Among the core variables, it displays the weakest rebound. During the third wave, the index decreases further.

We are interested in the change of economic or market uncertainty in response to lockdown shocks. However, uncertainty is a concept rather than a measurable time series. In line with the literature on economic uncertainty, we focus on two methodologies that allow for the construction of proxies. More precisely, we focus on market-based volatility measures and textual analysis. For the former, we rely on the CBOE Volatility Index (VIX), which we receive from Datastream. Baker, Bloom and Davis (2016) construct a daily Economic Policy Uncertainty (EPU) index based on newspaper coverage frequency. They count the relative amount of articles that include a combination of pre-specified buzzwords. Specifically, the EPU marks the share of articles that contain the following triple: "economic" or "economy"; "uncertain" or "uncertainty"; and one or more of "congress", "deficit", "Federal Reserve", "legislation", "regulation" or "White House". Baker, Bloom and Davis (2016) also expand the list of buzzwords to measure a more specific category of uncertainty. For instance, articles that additionally contain the term "taxes", "tax", "taxation" or "taxed" would be included in the taxes uncertainty sub-index. In a similar vein, Baker et al. (2019) construct an Infectious Disease Equity Market Volatility Tracker (IDEMVT). Finally, Baker et al. (2020) construct uncertainty indices via data from Twitter. They differentiate between economic (TEU) and market uncertainty (TMU). More precisely, they collect all tweets containing buzzwords related to uncertainty as well as keywords related to the economy or related to equity markets.

We always aim for a model with daily data. However, from the uncertainty measures, only the VIX, the Twitter-based indices, the broad EPU and the DEMVT are along with the rest of the variables in $Y_t$ available on a daily frequency.[4] The other sub-indices are available on a monthly frequency only. We overcome this issue as follows. First, we always estimate VARs with daily data when the underlying uncertainty series is available in that frequency. Second, we show that a VAR with weekly data yields similar results for those variables. Finally, we interpolate the uncertainty sub-indices available on a monthly frequency to weekly data and then estimate weekly VARs. For the uncertainty measures, we always assign the monthly entry to the last Friday in a month and then interpolate the gaps. For the rest of the variables in $Y_t$, we always consider the entry on each Friday.[5]

[4]The Twitter indices are furthermore only available until 09/15/20.

[5]If a Friday is a bank holiday, we refer to the previous Thursday instead.

### 5.2.2 Methodology

We quantify the effect of a lockdown within an structural VAR (SVAR) framework. Since the seminal work by Sims (1980), the SVAR became the "workhorse" econometric model when dealing with endogenous variables. Due to the endogenous nature of the variables we analyze, a VAR approach is a logical approach to unveil linkages. To be more illustrative, consider the decision making process behind a lockdown. Policymakers face a trade-off between the economic damage (proxied by $SPI_t$) and low numbers of infections to not overwhelm the health system (proxied by $Hospitalized_t$), see e.g. Alfano and Ercolano (2020). Hence, the strength of a lockdown decreases when hospitalisation is low which in return pushes up stock prices. In contrast to that, Elenev, Landvoigt and Van Nieuwerburgh (2020) and Basu et al. (2020) show that Amazon benefits from lockdowns. Accordingly, their stock prices should drop, when $Hospitalized_t$ decreases. Our model can be written as

$$Y_t = C + A_0 Y_t + A_1 Y_{t-1} + ... + A_p Y_{t-p} + \varepsilon_t, \tag{5.1}$$

where $Y_t$ is a 5x1 vector of endogenous variables and $C$ captures deterministic effects. Furthermore, $A_0$ and $A_1$ to $A_p$ are 5x5 matrices that capture effects of contemporaneous and lagged changes in $Y_t$. Finally, $\varepsilon_t$ are the structural error terms and $p$ describes the lag-length. In line with the Hannan-Quinn information criterion, we set $p$ to two for the daily model. For the weekly model, we assume $p = 1$ so that the degrees of freedom are maximal.

However, the SVAR model is not yet identified. To overcome this issue, we utilize a combination of sign and zero restrictions. We rely on a Bayesian framework. More precisely, we follow Arias, Rubio-Ramirez and Waggoner (2018) who implement an algorithm that draws from a conjugate uniform-normal-inverse-Wishart posterior over the orthogonal reduced-form parameterization and transform the draws into the structural parameterization.

The identification via sign and zero restrictions deserves special attention. Our goal is to disentangle the lockdown shock from a business cycle shock, such as a monetary policy or an aggregate supply or demand shock. While both contractionary shocks reduce $SPI_t$ by definition, their impact on the Amazon stock price differs, see Table (5.1). According to Elenev, Landvoigt and Van Nieuwerburgh (2020) and Basu et al. (2020) Amazon is benefiting from lockdowns. For this reason, we assume the positive sign for the lockdown shock. Moreover, we assign a decrease in $Workplaces_t$ as businesses were closed and more employees are working remotely. Finally, a lockdown leads to a lower amount of hospitalization in comparison to a scenario with no lockdown. To account for the fact that the hospitalization falls with some delay, we restrict the periods 10 to 12 working days after the shock. For all other variables, we assume that the restrictions hold for $t \in \{0;1;2\}$. When the weekly model is applied, we assume that $Hospitalized_t$ decreases in the following two weeks and all other restrictions hold in $t = 0$ only. In contrast to that, the business cycle shock also leads to reductions in the share price for Amazon. Furthermore, it does not change peoples' mobility with respect to their workplaces, i.e. we apply a zero-restriction that holds in the first period only. Finally, we do not impose any restriction on the hospitalization. As $Amazon_t$ is the only variable that is expected to increase after a lockdown and decrease after a business cycle shock, it is the key variable in the disentangling process of the two shocks.

Table 5.1: Shock Identification

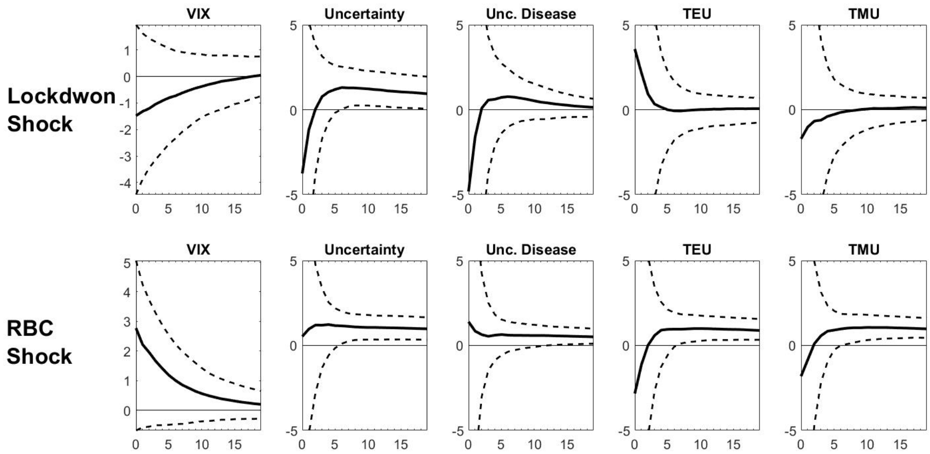
| Variables | $Hospotalized_t$ | $SPI_t$ | $Amazon_t$ | $Workplaces_t$ | $Uncertainty_t$ |
|---|---|---|---|---|---|
| Lockdown Shock | $-^*$ | - | + | - | unrestricted |
| Business Cycle Shock | unrestricted | - | - | 0 | unrestricted |

*Notes:* In the daily (weekly) model, '+' describes an increase and '-' a decrease in the underlying variable on impact and for the subsequent two periods (on impact only). Moreover, '$-^*$' represents a decrease in the underlying variable in periods ten to twelve in the daily model and one and two in the weekly model. Finally, '0' refers to a zero restriction on impact.

## 5.3 Results

Figure (5.3) outlines the IRFs for the uncertainty measures from the daily VAR models. We focus on the uncertainty variables as the other variables' reactions are predetermined by the imposed restrictions. For a better comparison, we standardize both shocks so that they imply a 1% decrease in the $SPI_t$. Interestingly, we find that the VIX tends to decrease after a lockdown shock and increases after the business cycle shock. The explanation for the business cycle shock is straightforward, as bad news increase market uncertainty. In fact, GARCH and particularly EGARCH models reflect this stylized fact, see e.g. Brandt and Jones (2006). The response to the lockdown shock is more complex. The fact that tighter lockdown measures are enforced might be interpreted as a signal that the pandemic situation is worse than expected. Besides the bad news story, other signaling effects also play a role. If policymakers enforce a lockdown, they implicitly unveil their willingness to fight the disease, which could reduce uncertainty. Finally, the implementation of a lockdown reduces uncertainty about the lockdown itself. Prior to any announcement, households have certain believes about the future paths of lockdowns. With the announcement, these beliefs are updated. In this sense, the announcement of a lockdown contains a form of forward guidance. In a similar vein, the Twitter Economic and Market Uncertainty measures also unveil that the median response of the lockdown shock is substantially lower in comparison to the response of the business cycle shock in the medium term. Moreover, the 16th percentile of the two Twitter Uncertainty measures only increase after the business cycle shock has hit the economy.

Figure 5.3: Daily VAR models

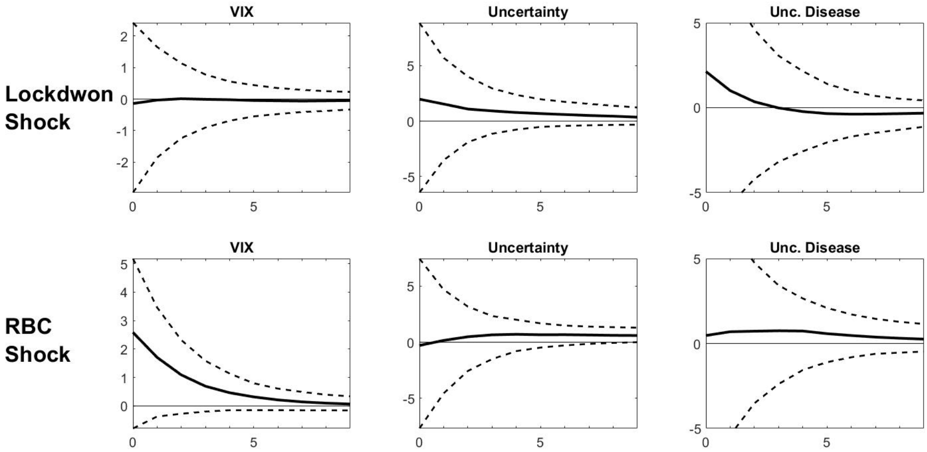



*Notes:* The solid line represents the median response, the 16th and 84th percentiles are displayed via the dotted lines. The upper (lower) panel describes the response to a lockdown shock (the contractionary business cycle shock).

For the Policy Uncertainty, we find that a lockdown leads to an increase in uncertainty. In comparison to the Policy Uncertainty's response to the business cycle shock, the drop in uncertainty is of comparable size but occurs with some delay. We observe similar patterns for the IDEMVT, although the impact of the business cycle shock is stronger after ten trading days. This is also mirrored by the fact that only the 16th percentile of the business cycle shock is above zero from period twelve onward. Altogether, we find that a contractionary business cycle shock leads to higher uncertainty levels in four of the five analyzed cases highlighting that either signaling effects or updates of households' beliefes play a crucial role in the transmission of lockdown shocks.

Figure (5.4) shows the developments of the mentioned variables in the weekly model. We refrain from the estimation of the Twitter indices, as the number of observations is too small after all adjustments. As before, the VIX tends to increase after the business cycle shock only. After six weeks, we again observe that the contractionary business cycle shock implicates higher uncertainty levels than the lockdown shock for all three variables.

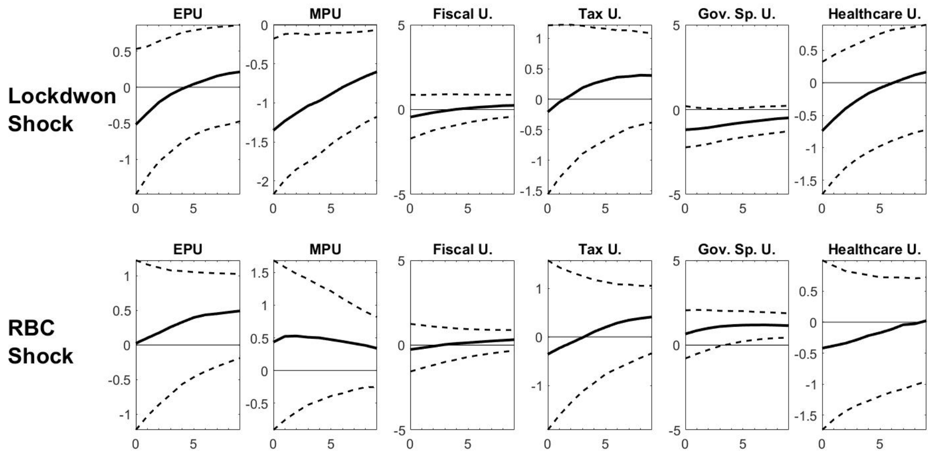
Figure 5.4: Weekly VAR models I




*Notes:* The solid line represents the median response, the 16th and 84th percentiles are displayed via the dotted lines. The upper (lower) panel describes the response to a lockdown shock (the contractionary business cycle shock).

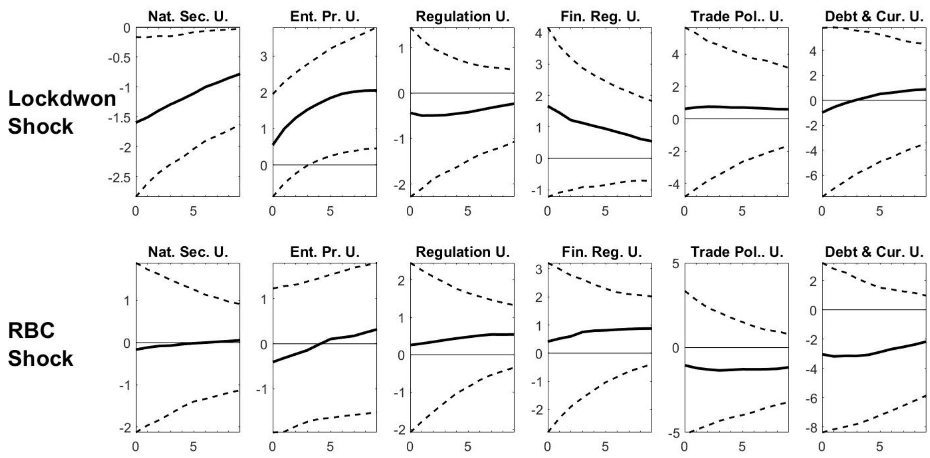
Finally, Figures (5.5) and (5.6) unveil the response of the sub-indices. While we present evidence on all sub-indices, we restrict our attention to the most relevant results. We find that, in particular, monetary policy, government spending and regulation uncertainty tend to decrease after a lockdown shock while they increase after the business cycle shock. Furthermore, as financial regulation tends to increase after both shocks, the drop in regulation in response to lockdowns stems from the regulation of other sectors. In line with the argumentation from above, the announcement of the lockdown reveals information on all kind of regulation so that uncertainty decrease. In a similar vein, the governments communicate compensation plans for the sectors largely affected by the lockdown with the announcements. However, the communication on the entitlement programs could be more precise, as uncertainty related to it increases disproportionally after a lockdown shock. Through forward guidance, the Fed also laid down its response to further lockdown measures. For instance, in an FOMC statement on 03/03/20, the Committee expresses that it "is closely monitoring developments and their implications for the economic outlook and will use its tools and act as appropriate to support the economy." The fact that monetary policy uncertainty decreases, shows that the Fed's communication strategy works. Interestingly, uncertainty about healthcare does not display a clear reaction after the lockdown shock.

Figure 5.5: Weekly VAR models II




*Notes:* The solid line represents the median response, the 16th and 84th percentiles are displayed via the dotted lines. The upper (lower) panel describes the response to a lockdown shock (the contractionary business cycle shock).

Figure 5.6: Weekly VAR models III




*Notes:* The solid line represents the median response, the 16th and 84th percentiles are displayed via the dotted lines. The upper (lower) panel describes the response to a lockdown shock (the contractionary business cycle shock).

All results are robust to changes in (i) the lag-length of the VAR, (ii) the length for the restrictions to hold, (iii) the variables included [6], (iv) "controversial" sign-restrictions such as hospitalisation or the zero restriction and (v) adding dummy variables that control for day of the week effects. All impulse-response functions are available upon request.

[6]In particular, other mobility data (e.g. home) and other indicators for pandemic situation such as the number of active cases lead to similar results.

## 5.4 Conclusions

This paper has two main contributions to the literature that assesses macroeconomic consequences of lockdowns. First, we introduce a daily (weekly) VAR model that takes the endogeneity of the underlying variables into account and allows discerning between lockdown shocks and a real business cycle shock. Second, we analyze how lockdown shocks influence policy uncertainty. Overall, we find that lockdowns have only a moderate impact that is smaller than the impact of the business cycle shock identified in our model. Nevertheless, we observe that lockdown shocks lead to sizable increases in fiscal and tax policy uncertainty as well as in uncertainty that is related to entitlement programs. Other sectors, such as monetary policy uncertainty see no decline. It is by now standard that monetary policymakers guide market participants via communication. Hence, one possible interpretation is that the rising uncertainty levels are caused by an unclear communication strategy in the fiscal sector.

Several expansions are feasible but beyond the scope of this analysis. Obviously, the identification strategy could be exploited to analyze other research questions. For instance, other research might estimate the effect on the yield curve. Moreover, we do not analyze the mechanism behind the reaction of uncertainty levels. In particular, this paper does not include a structural model that helps explain the different movements in uncertainties.

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# 6 Concluding Remarks

This doctoral thesis puts the three most recent economic crises in Europe center stage. It analyses some of the driving mechanisms behind them. More precisely, it investigates how unexpected changes in credit supply and risk-taking in the US economy affect several macroeconomic and financial variables. This is of particular interest as these two variables contributed to the environment in which the great financial crisis of 2007/08 could unfold. We find that, in particular, real economic variables such as real GDP and employment react similarly to both shocks. In contrast to that the VIX, the excess bond premium and stock prices decrease after an expansionary credit supply shock and increase after an expansionary risk-taking shock. The (shadow) short rate responds only to the credit supply shock.

Furthermore, this thesis evaluates policymakers' responses to the crises. In this respect, one paper focuses on the effectiveness of macroprudential policy measures in the euro area. Its expansion on a larger scale was a consequence of the great financial crisis. The results show that a tightening in the prudential policy stance leads to negative credit growth.

In addition to changes in the macroprudential policy framework, monetary policymakers responded to the crisis and the subsequent European sovereign debt crisis by several easings in the policy stance. Building on that, the paper evaluates the effectiveness of aggregate and country-specific monetary policy in the euro area. It finds that monetary policy shocks that are free of signaling effects influence price levels, real economic activity, as well as stock and credit markets. The country-specific results show that the monetary policy transmission is heterogeneous across countries.

Moreover, this thesis assesses the distributional consequences of monetary policy. We find that gross income inequality increases after a monetary easing. However, as net income inequality only increases in those countries with little redistribution, we conclude that tax and transfer policies can mitigate or offset monetary policy's impact on inequality. Additionally, we unveil the channels through which monetary policy affects the income distribution. We observe that in those countries with a relatively low degree of redistribution, inequality increases after expansionary

monetary policy because credit income increases disproportionally.
Finally, this thesis also includes a paper that disentangles a lockdown shock from other cyclical movements, which is essential as lockdown measures are a new source of policy tools that largely influence economic activity. We focus on the impact of lockdown shocks on (economic) uncertainty and find that it is not more pronounced than the impact of the real business cycle.
All papers are relevant for policymakers as they unveil how changes in the policy stance affect real and financial variables. According to our results, macroprudential policymakers are capable of influencing lending but not house prices. The lessons of this thesis for monetary policymakers are manifold. In the euro area, the ECB affects real and financial variables not only by monetary policy shocks but also by releasing information on the development of macroeconomic variables, i.e. the so-called "information shock". Taking a more granular perspective, we show that some of the monetary policy transmission mechanisms are impaired in some countries. This is, in particular, true for countries with weak economic fundamentals. Hence, the idea that the ECB "buys time" for weak economies must be questioned. Furthermore, this thesis outlines that monetary and fiscal policymakers should keep an eye on the distributional consequences of monetary policy. Although inequality is not directly included in the mandate of central banks, it might be indirectly important, when high levels of inequality obstruct the monetary transmission channels. Since redistribution can mitigate the distributional effects of changes in the monetary policy stance, fiscal policymakers' decisions are of special interest. Regarding policymakers that are responsible for lockdowns, this thesis finds that lockdowns have only a moderate impact on economic (uncertainty) that is smaller than the effect of the real business cycle. Moreover, we show that policymakers should pay attention to banks' lending conditions and their risk-taking behavior as they also affect real and financial variables.

# 7 Affidavit

Ich erkläre hiermit, dass ich die vorgelegten und nachfolgend aufgelisteten Aufsätze selbstständig und nur mit den Hilfen angefertigt habe, die im jeweiligen Aufsatz angegeben sind. In der Zusammenarbeit mit den angeführten Koautoren war ich mindestens anteilig beteiligt. Bei den von mir durchgeführten und in den Aufsätzen erwähnten Untersuchungen habe ich die Grundsätze guter wissenschaftlicher Praxis, wie sie in der Satzung der Justus-Liebig-Universität Gießen zur Sicherung guter wissenschaftlicher Praxis niedergelegt sind, eingehalten.

Lucas Hafemann, Gießen, den 10. September 2021

I hereby declare that I completed the papers submitted and listed hereafter independently and only with those forms of support mentioned in the relevant paper. When working with the authors listed, I contributed no less than a proportionate share of the work. In the analyses that I have conducted and to which I refer in the papers, I have followed the principles of good academic practice, as stated in the Statute of Justus-Liebig-University Gießen for ensuring good scientific practice.

Lucas Hafemann, Gießen, September 10th, 2021

**Submitted Papers:**

I. Hafemann, L., and Tillmann, P. (2021): 'Lending Standards and the Business Cycle: Evidence from Loan Survey Releases', *MAGKS Joint Discussion Paper Series in Economics* No. 31-2021.
https://www.uni-marburg.de/en/fb02/research-groups/economics/macroeconomics/research/magks-joint-discussion-papers-in-economics/papers/2021-papers/31-2021_hafemann.pdf

II. Hafemann, L. (2021): 'Prudential Policies in the Eurozone: A Propensity Score Matching Approach', *MAGKS Joint Discussion Paper Series in Economics* No. 9-2021.
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III. Hafemann, L., and Tillmann, P. (2020): 'The Aggregate and Country-Specific Effectiveness of ECB Policy: Evidence from an External Instruments VAR Approach', *International Journal of Central Banking* 65, 97 - 136.
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