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Dissertation Thesis

2023 **Anna Pestova**

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Essays in Applied Macroeconomics

Dissertation Thesis

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Abstract

This Thesis studies the role of domestic and foreign credit shocks in aggregate fluctuations and how households respond to those shocks. The first chapter explores the role of credit in shaping aggregate fluctuations in a panel of advanced and emerging countries. We decompose total credit by aggregate shocks and by borrower type. We establish that the overall boom-bust recession response to bank credit is due to exclusively household credit expansions, primarily when these expansions are driven by shocks to aggregate demand. In contrast, corporate credit expansions exhibit no boom-bust effects and immediately increase the risk of recession, and this increase is mainly driven by shocks to credit supply. The second chapter analyzes the transmission of credit supply shocks to household balance sheets and labor market outcomes. We leverage differences in credit conditions across U.S. states and compare household outcomes of the population residing in states that witnessed credit conditions easing of various intensities. We show that positive credit shocks lead to greater household defaults in the future if they increase the household mortgage-to-income ratio. We document that positive credit supply shocks induce (i) shifts of employment between the tradable and non-tradable sectors, (ii) changes in household income and (iii) in house prices, which shape the accumulation of default risks. The third chapter studies the effects of sanctions which can be thought of as a realization of a negative international credit supply shock. We introduce a novel approach to identify the sanctions shock—a *high-frequency identification* (HFI) based on the US sanction announcements and daily data on Russia's US Dollar-denominated sovereign bonds. We show that the sanction announcements in 2014–2015 were very potent: the underlying sanctions could lead to a GDP decline of up to 3.2%, which is twice as large as estimated in the previous work. Finally, the fourth chapter introduces a new identification of country-spread shocks in emerging economies. This identification is grounded on the experience of sudden stops – large capital reversals during crises in these countries. I conclude by describing alternative shock identification procedures.

Abstrakt

Tato práce studuje roli domácích a zahraničních úvěrových šoků v agregátních fluktuacích a jak na tyto šoky reagují domácnosti. První kapitola zkoumá roli úvěru při utváření agregátních fluktuací v panelu vyspělých a rozvíjejících se zemí. Celkový úvěr rozkládáme podle agregátních šoků a podle typu dlužníka. Zjišťujeme, že celková odezva boom-propad recese na bankovní úvěry je způsobena výhradně expanzemi úvěrů domácnostem, především když jsou tyto expanze poháněny šoky agregátní poptávky. Naproti tomu expanze podnikových úvěrů nevykazují žádné účinky boom-propad a okamžitě zvyšují riziko recese, přičemž tento nárůst je způsoben především šoky v nabídce úvěrů. Druhá kapitola analyzuje přenos šoků nabídky úvěrů do bilancí domácností a výsledků trhu práce. Využíváme rozdíly v úvěrových podmínkách napříč státy USA a porovnáváme výsledky domácností obyvatel žijících ve státech, které zaznamenaly různé intenzity uvolnění úvěrových podmínek. Ukazujeme, že pozitivní úvěrové šoky vedou k většímu nesplácení domácností v budoucnu, pokud zvýší poměr hypoték domácností k příjmu. Dokumentujeme, že pozitivní úvěrové nabídkové šoky vyvolávají (i) přesuny zaměstnanosti mezi obchodovatelným a neobchodovatelným sektorem, (ii) změny v příjmech domácností a (iii) v cenách nemovitostí, které formují akumulaci rizik selhání. Třetí kapitola studuje dopady sankcí, které lze považovat za realizaci negativního mezinárodního úvěrového nabídkového šoku. Představujeme nový přístup k identifikaci sankčního šoku – *vysokofrekvenční identifikace* (HFI) na základě oznámení o sankcích USA a denních údajů o ruských státních dluhopisech denominovaných v amerických dolarech. Ukázali jsme, že oznámení o sankcích v letech 2014–2015 byla velmi účinná: základní sankce by mohly vést k poklesu HDP až o 3,2%, což je dvakrát více, než se odhadovalo v předchozí práci. Konečně čtvrtá kapitola představuje novou identifikaci šoků v rozvíjejících se ekonomikách. Tato identifikace je založena na zkušenostech náhlých zastavení – velkých kapitálových obratů během krizí v těchto zemích. Na závěr popisuji alternativní postupy identifikace šoku.

Keywords: Business Cycle, Predicting recessions, Exuberance Indicators, Global financial cycle, Aggregate demand, Credit supply shocks, Sanctions, Difference-in-differences, High-frequency identification (HFI).

Klíčová slova: Obchodní cyklus, Predikce recese, Indikátory exuberance, Globální finanční cyklus, Agregovaná poptávka, Šoky v nabídce úvěrů, Sankce, Rozdíly v rozdílech, Vysokofrekvenční identifikace (HFI).

Length of the work: 461453 characters with spaces, without abstract and appendices

Declaration

1. I hereby declare that I have compiled this thesis using the listed literature and resources only.
2. I hereby declare that my thesis has not been used to gain any other academic title.
3. I fully agree to my work being used for study and scientific purposes.

In Prague on 5 September 2023 Anna Pestova

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Contents

Introduction

This dissertation studies the role of credit supply shocks in aggregate fluctuations and accumulation of recession risks and estimates micro-level household responses to such shocks.

Credit supply shocks are important drivers of business cycle fluctuations in both advanced and emerging countries, and these shocks received a lot of attention recently following the Global Financial Crisis. Credit supply shocks can manifest as exogenous changes in domestic credit conditions in both advanced and emerging economies and they can also be identified as changes in external borrowing conditions unrelated to domestic fundamentals.

In the first chapter, we study the relationship between current credit expansions and future recession risks and how the business cycle shocks shape it. Using a quarterly dataset on 25 emerging and advanced economies, we document the boom-bust real effects of credit expansion: a one standard deviation increase in bank credit growth reduces the risk of recession by 3 pp in one year but then raises it by nearly 10 pp in three years. We establish that the boom-bust recession response to bank credit is exclusively due to household credit expansion, in particular when household credit expansion is driven by shocks to aggregate demand. In contrast, firm credit expansion exhibits no boom-bust effects but immediately increases the risk of recession, and this increase is primarily driven by an exogenous easing of credit supply by banks. As for the mechanism, we find that firm credit supply expansion leads to a reduction of the total factor productivity growth. We show that firm credit supply expansion further increases recession risk if accompanied by asset price booms, while there is no such pattern for household credit expansion. We conclude that the recessionary effects of credit expansions vary substantially with the type of credit and the type of aggregate shocks driving credit.

In the second chapter, we ask whether disruptions in the mortgage market are a consequence of financial imbalances accumulated in the past. We study the effects of positive credit supply (CS) shocks on subsequent household defaults on debt over the last four decades in the U.S. We apply sign restrictions within a VAR framework to isolate state-level CS shocks and identify that 1984 and 2004 were the years of systemic, countrywide, *positive* CS shocks. By employing a difference-in-differences estimation, we find that positive CS shocks lead to greater household defaults in the future if they also increase mortgage-to-income ratios. We show that the CS shocks induce (i) shifts of employment between the tradable and non-tradable sectors, (ii) changes in household income, and (iii) changes in house prices, which shape the accumulation of default risks. Our results indicate that positive CS shocks that occurred in 1984 did not raise household defaults by more in more exposed states than in less exposed states because the shocks increased both future income and mortgage debt, while not affecting mortgage-to-income ratios. In contrast, the 2004 CS shocks increased mortgage-to-income ratios in subsequent years, thereby raising debt delinquencies and household defaults. These results are useful in understanding the accumulation of financial risks and prudential policy design.

In the third chapter, we ask how much sanctions harm the sanctioned economy. We analyze the case of Russia, which faced three major waves of international restrictions over the last decade (in 2014, 2017, and 2022). In a VAR model of the Russian economy, we first apply sign restrictions to isolate the innovations to international credit supply, to proxy for the financial sanctions shocks. We then examine the effects of the overall sanctions shocks (financial, trade, technological, etc.) by employing a high-frequency identification (HFI) approach. Our HFI is based on each OFAC's / EU sanction announcement and the associated daily changes in the yield-to-maturity of Russia's US Dollar-denominated sovereign bonds. Our macroeconomic estimates indicate that Russia's GDP may have lost up to 0.8% due to the financial sanctions shock, and up to 3.2% due to the overall sanctions shock cumulatively over 2014–2015. In 2017, the respective effects are 0 and 0.5%, and in 2022, they are 8 and 12%. Our cross-sectional estimates show that the real income of richer households declines by 1.5–2.0% during the first year after the sanctions shock, whereas the real income of poorer households rises by 1.2% over the same period. Finally, we find that the real total revenue of large firms with high (low) TFPs declines by 2.2 (4.0)% during the first year after the sanctions shock, whereas the effects on small firms are close to zero. Overall, our results indicate heterogeneous effects of sanctions with richer households residing in big cities and larger firms with high TFPs being affected the most.

In the fourth chapter, I study a new identification of the country spread shocks in emerging economies. The identification is grounded on the experience of sudden stops – large capital reversals during crises in these countries. I present background facts about sudden stops in emerging economies, which yield the identifying sign restrictions. I lay down a calibrated open economy model to study if the model supports my identification. I find that the model parameter space supporting the identification is narrow and unlikely to be supported by the data. I conclude by describing alternative shock identification procedures

1 Credit and the Risk of Recession: The Role of Business Cycle Shocks

1.1 Introduction

Over the past fifteen years it has become clear that excessive availability and cheapness of bank credit push the economy into deep crises, such as the 2007–2009 Great Recession (Mian and Sufi, 2009; Gertler and Gilchrist, 2018). It has been widely documented that credit expansions are not harmless for many economies across the world and can lead to long-lasting financial crises and a slump of output growth rates in 2–3 years (Schularick and Taylor, 2012; Aikman et al., 2014; Mian et al., 2017; Greenwood et al., 2022), with slow recoveries afterwards (Reinhart and Rogoff, 2009; Gertler et al., 2019).[1] However, it is also clear that credit availability is central to supporting the real decisions of firms and households in the short run: as, for example, the Lehman Brothers collapse vividly demonstrated: negative shocks to credit availability immediately and abruptly reduced firm employment and shrunk household consumption (Chodorow-Reich, 2014; Jensen and Johannesen, 2017).

The co-existence of positive short-run and negative medium-run real effects of credit expansions implies that an increase in bank credit supply first reduces the risk of recession (the *positive effect*) but then, at longer horizons, it raises the likelihood of economic downturn (the *negative effect*). Both effects are consistent with recent macroeconomic theories.[2] Less is known, however, about the magnitudes of the positive and negative effects of the *same* credit expansions and how potent different business cycle shocks are in fueling these expansions.[3] Our research provides an instrumental framework for a comprehensive exploration of the nexus between bank credit and recession risks through the lens of business cycle shocks.

Recently, the existence of a medium-run negative response of output to credit growth was challenged by Brunnermeier et al. (2021), who find that the combination of a credit shock

[1] As of 2022, the global economy is again under threat: the current level of global debt is at an all-time high (Boone et al., 2022) and recession risks are soaring (Del Negro et al., 2022).

[2] The positive short-run effect appears in the models in which firms finance (a part of) their investment or wage bills with local or global bank funds (Gertler et al., 2019; Bianchi and Mendoza, 2018). The negative medium-run effect manifests in models in which (*i*) debt is considered to be a stock variable with diminishing return (Beaudry et al., 2020), (*ii*) agents are impatient and there is a pecuniary externality (Schmitt-Grohe and Uribe, 2021), (*iii*) banks switch from loose and tight credit standards when borrowers' quality deteriorates sufficiently (Farboodi and Kondor, 2020), or (*iv*) there is an overreaction to positive news in decision-making on debt limits (Bordalo et al., 2018).

[3] If, for example, the negative effects of a credit expansion exceed the positive effects in magnitude, then limiting the credit cycle would be beneficial from a policymaker's perspective. Otherwise, a "stay out" strategy would be preferable. This paper provides a unified framework for the assessment of both short-run positive and medium-run negative effects of credit expansions.

and a monetary shock which keeps the interest rate constant eliminates any negative output response. Thus, it is important to understand if a recession response to credit depends on the type of shock fueling credit expansion. Do monetary shocks exclusively drive the recession response to bank credit (Brunnermeier et al., 2021) or is there some influence from other shocks, such as aggregate demand (Brianti and Cormun, 2022) and/or credit supply shocks (Mian et al., 2017)? We answer these questions in a setting of a panel of countries with a standard business cycle shock identification.

It has been documented that, under particular states of the economy, the financial and macroeconomic risks of credit expansion are amplified. In particular, recent theories of information production over the credit cycle suggest that, during collateral price booms, the stock of information about projects becomes depleted, which makes credit booms more costly in terms of output losses (Asriyan et al., 2022). Along these lines, in this paper, we further ask if the recession response to credit is larger under a high asset price growth regime (Greenwood et al., 2022) and if the relative magnitudes of short-run positive effects and medium-run negative recessionary effects of credit expansion change under certain regimes.

To answer these questions, we collect a cross-country quarterly macroeconomic and financial dataset covering 25 advanced and emerging economies over the past 40 years. Our empirical analysis consists of two parts.

In the first part (*reduced-form analysis*), we use Jorda (2005) local projections to flexibly quantify the time structure of recession response to *bank* credit. Our estimation delivers strong statistical support for the suggested pattern of recession response to bank credit: a one standard deviation increase in bank credit-to-GDP growth over the past year reduces the likelihood of output contractions by up to 3 pp at the one-year horizon, but then raises the probability of a recession by up to 10 pp at the three-year horizon. This result is robust if we account for dynamic dependence in business cycle phases and consider a large set of control variables—classical recession predictors including term spread, and recent monetary conditions. Our medium-run effects of bank credit on recessions are larger than those in Schularick and Taylor (2012) and Greenwood et al. (2022), who estimate the medium-run effect of bank credit expansion on the probability of financial crises to be equal to +2.6-2.8 pp. This is not surprising, given that they use annual data and in their data, financial crises are rarer than recessions in our data: only 3-4 percent of observations in Greenwood et al. (2022)'s dataset is classified as financial crises, while recessions are recorded in 15 percent of observations in our sample.

We further show that controlling for recent monetary policy stance by including the short-term interest rate does not alter our finding that larger bank credit growth increases recession risk in the medium run. Our analysis addresses the criticism of Brunnermeier et al. (2021) and suggests that their result becomes much less pronounced in the wider sample of countries. This can be explained by cross-country differences in monetary reaction functions, due to variations in the information processing lags, differences in the degree of monetary independence, and a greater reliance of other countries on foreign banks and international borrowings than in the US.[4]

In the second part (*drivers and channels*), we explore what drives the boom-bust recession response to credit. We start with the investigation of which business cycle shocks are responsible for the particular time-structure response of recession risk to bank credit. We consider four major macroeconomic shocks, which we extract from a country-level sign-identified structural VAR (SVAR) model: aggregate demand (AD), aggregate supply (AS), monetary policy (MP), and credit supply (CS) shocks. The first three shocks—AD, AS, and MP—are known to explain the bulk of macroeconomic fluctuations during normal times (Smets and Wouters, 2007), while the fourth—credit supply shock—is important during large recessions (Gerali et al., 2010; Gambetti and Musso, 2017). We impose distinct sign restrictions to ensure that shocks are not "masquerading", Wolf (2020). In the first stage, we isolate the part of bank credit growth variation that is driven by each of the four domestic shocks using historical decomposition in the VAR. In the second stage, we replace the actual overall credit growth with its counterfactual dynamics driven by each shock of interest from the first stage, and estimate Jorda (2005) local projections, as in the previous stage.[5] Our results indicate that the overall pattern of recession response is driven by aggregate demand shocks. Most of the overall recession response to credit can be explained by the response of recession to credit driven by aggregate demand shocks. The short-run effect of credit on recession is –3 pp in case we consider the overall variation in bank credit and –2 pp in case of demand shocks. The medium-run effect is +10 pp in the baseline case and +4 pp if credit is driven by demand shocks. None of the other domestic shocks are able to generate a boom-bust recession response to bank credit expansion. Instead, other expansionary shocks increase recession risk if transmitted through bank credit. The existence of the boom-bust recession response to credit driven by AD shocks is consistent with expectations- and sentiment-driven

[4] In our sample, international debt issues are 47 percent of GDP in the countries other than the U.S. and 19 percent of GDP in the U.S., as an average in 2000–2020. Source: World Bank Global Financial Development Database.

[5] A similar two-stage approach was employed in Lopez-Salido et al. (2017) who investigate the effect of credit spreads on future GDP growth rates in the U.S., and by Mian et al. (2017) who estimate the effects of mortgage-sovereign spreads on GDP growth rates through household debt in a cross-country setting. Note that in the aforementioned papers, the instrumental variable framework involves the estimation of the first-stage regression, whereas in our case we obtain a variation in credit that is driven by each shock naturally within the historical decomposition in the VAR, i.e., without any additional estimations.

credit and business cycles (Bordalo et al., 2018; Kantorovitch, 2021; Asriyan et al., 2022; Brianti and Cormun, 2022).

We then examine whether the recession response to credit expansion is more pronounced under high asset price growth. Previously, Greenwood et al. (2022) showed that the probability of a financial crisis is higher if a credit expansion is accompanied by rapid asset price growth. We consider recessions and employ the state-dependent local projection approach. We test for differences in responses across high and low-asset price growth regimes. We show that the recession response is indeed twice as strong at its peak under a high asset price growth regime (+20-30 pp increase in the probability of recession, compared to +10 pp in the baseline case). The boom-bust recession response is observed only under high real estate price growth, while if stock market prices grow above normal the recession risk only increases in response to credit. Under a high asset price growth regime, the peak recession response shifts forward in time compared to the baseline case, suggesting that a recession arrives sooner in response to a credit expansion if it is accompanied by an asset price boom. Our empirical result is consistent with the theoretical view of decreased incentives for information acquisition under a collateral boom, which ends in deeper crises (Kantorovitch, 2021; Asriyan et al., 2022).

We further show that the boom-bust recession response to credit is driven exclusively by a household credit expansion and is only present in developed countries, consistent with findings in Mian et al. (2017). The economic expansion following household credit growth is amplified under a real estate boom—and not under a stock market boom—and is associated with a total factor productivity acceleration. This may suggest the operation of the geographical relocation channel—higher demand for housing in opportunity areas as a result of geographical mobility,—which may explain both TFP improvement and recession to household credit response amplification under high real estate price growth. Further research on more granular data may shed more light on these findings.

Differently from household credit, a firm credit expansion only increases recession risk, and this response is present in both emerging and advanced economies. We show that the increase in recession risks in response to firm credit is driven by credit supply shocks, which means that recession risks accumulate when a firm credit expansion is driven by an exogenous credit easing unrelated to borrowers' fundamentals. This may be explained by increased bank risk-taking under positive credit supply shocks, Degryse et al. (2019). We further uncover that a firm credit supply expansion reduces total factor productivity in the medium run, and the increased recession risk is reinforced under a collateral boom, pointing to increased misallocation and consistent with the information production mechanism of Asriyan et al.

(2022).[6] This empirical result also resonates with the finding of Gopinath et al. (2017), who show that capital inflow to Southern Europe under an exogenously given low-interest rate—clearly, a positive credit supply shock—led to capital misallocation to firms with higher net worth but which were not necessarily more productive.

The two closest papers to ours are Mian et al. (2017) and Brunnermeier et al. (2021). Mian et al. (2017), however, don't consider shocks and focus on the medium-term output response to credit.[7] In contrast, we focus on both short- and medium-term parts of recession (and output) responses to credit and explore its sensitivity to various business cycle shocks driving credit. Compared to Brunnermeier et al. (2021), we use a multi-country setting, apply structural model-based shock identification, and directly control for monetary conditions. Although our work has many findings that are in line with Mian et al. (2017), we differ in the key macroeconomic shock found to drive the boom-bust cycle. Mian et al. (2017) show that the boom-bust cycle is driven by a decrease in credit spread, which suggests that the credit supply shock accounts for booms and busts. In contrast, we consider a broader set of shocks that are mutually exclusive, and we consistently find that the boom-bust recession response is driven by aggregate demand shock only, not credit supply shock. We provide the argument that shifts in beliefs and spendings are the source of boom and bust, in line with Kaplan et al. (2020), as opposed to Mian and Sufi (2009) and Mian et al. (2017) who find that the main source of boom and bust was the outward shift in credit supply.

Our study contributes to several strands of the literature. First, we employ the uniform framework to assess both the short-run and medium-run effects of credit expansions on recession risk.[8] Unlike the previous literature we estimate and compare both effects and find that the negative medium-run effects of a bank credit expansion on the risk of recessions exceed the positive short-run effects. Mian et al. (2017) find a similarly-shaped economic response to household credit expansions on the cross-country sample of annual frequency. However, they focus on the medium-run effects and do not consider shocks. Brunnermeier et al. (2021) challenge the view that rapid credit expansion predicts lower GDP growth by highlighting the role of endogenous monetary tightening in explaining output drop in response to a positive shock to household credit. We show that credit still predicts elevated

[6] They indicate that a collateral boom facilitates reallocation of capital from screened to unscreened projects.

[7] They use an instrumental variable approach to isolate a part of credit variation driven by mortgage spread.

[8] Aikman et al. (2020) document a similar shape of output response to an *easing of financial conditions* when credit is above its trend: output initially rises but then decreases. Adrian et al. (2022) uncover a similar shape of the Growth-at-Risk response—conditional GDP growth at the lower 5th percentile—to *an easing of financial conditions*. Note that both papers find similar time patterns of output response to financial conditions, not to credit growth.

recession risks and lower output growth if we control for recent monetary conditions. Our finding, therefore, restores the previous consensus.

Second, our research is related to the empirical literature on the identification of business cycle shocks and their contribution to economic activity (Peersman and Straub, 2009; Furlanetto et al., 2017; Gambetti and Musso, 2017; Geiger and Scharler, 2021). Our paper adds novel evidence that business cycle shocks propagated through bank credit affect the probability of a recession differently. Our paper is the first to use the historical contribution of business cycle shocks to credit as an exogenous variation. Previous literature has employed an instrumental variable (IV) approach, see e.g., Mian et al. (2017); Lopez-Salido et al. (2017). Compared to IV, our approach allows us to skip the estimation of the first stage because we obtain the historical contribution of shocks to bank credit directly from the SVAR. We believe that the estimated counterfactual development of macroeconomic variables based on our SVAR historical decomposition can be employed in other empirical applications.

Third, our empirical analysis is related to recently revived endogenous business and credit cycle theories that highlight the predictability of cycle phase changes based on past debt dynamics (Beaudry et al., 2020; Schmitt-Grohe and Uribe, 2021) and past lending conditions (Bordalo et al., 2018; Farboodi and Kondor, 2020). We provide support to these theories via empirical evidence of the boom-bust recession response to bank credit.

Lastly, our paper is related to the literature which finds that the effects of a credit boom depend on driving shocks and on collateral price dynamics (Gorton and Ordonez, 2019; Kantorovitch, 2021; Asriyan et al., 2022; Brianti and Cormun, 2022; Greenwood et al., 2022). We contribute by documenting that demand-driven credit expansion results in the boom-bust type of recession response, and that the recession response is amplified and shifted forward in time if a credit boom comes together with a collateral boom.

The rest of the chapter is structured as follows. In Section 3.3.2 we describe our data and variable construction. Section 1.3 presents our baseline empirical results on the time structure of recession response to bank credit. Section 1.4 presents SVAR-based business cycle shocks and describes which of them are responsible for the main result. It also unfolds this result across different states of the economy and across different country groups. Section 2.4 explores the sensitivity of our baseline results.

1.2 Data

1.2.1 Data sources

We assemble a quarterly country-panel database on macro-financial characteristics. In contrast to previous studies (Schularick and Taylor, 2012; Mian et al., 2017; Greenwood et al., 2022), we cannot rely on annual data and historical databases because we are interested in the identification of business cycle shocks that operate at higher than annual frequencies. We collect and merge quarterly cross-country macroeconomic and financial data from several sources. Most of the macroeconomic data come from the IMF International Financial Statistics database. We use the data on bank credit from the BIS database because IFS data contains many gaps, as well as several changes in methodology, which all complicate the construction of combined series. In contrast, BIS publishes credit data as a readily available database. In the baseline case, we use domestic bank credit to the private non-financial sector as a measure of credit.[9] We collect data on several classical recession predictors: on business and consumer confidence and on the short- and long-term interest rates from the OECD Statistical database. Even though the IFS database also contains data on interest rates, the OECD database provides longer time series and better coverage of European countries. The full description of the data and construction of variables for our empirical analysis is provided in Appendix no. 2.

Our initial sample covers 35 countries over the 1978–2019 time span, in a quarterly format.[10] The sample includes large emerging and advanced economies and is mostly limited by the data availability in the OECD database. These 35 countries are the OECD member countries plus six non-OECD major emerging economies for which the OECD reports macroeconomic data.[11] Due to data limitations on several countries and missing observations, our final sample shrinks further and includes 25 countries over the 1981–2019 time period.[12]

[9]We check the robustness of the medium-run effect of credit on recession to alternative measures of credit. In the sensitivity analysis, we use credit to the private non-financial sector from *all* sectors which, in addition to bank credit, includes financing from other sources: other domestic financial corporations, non-financial corporations, and non-residents. We also check if the medium-run effect of credit on recession holds for household and firm credit. These results are presented in Section 2.4.

[10]We exclude the COVID-19 recession due to the large role of restrictive government policies prohibiting movement and certain business activities.

[11]Australia, Austria, Belgium, Brazil, Canada, Chile, Colombia, Czech Republic, Denmark, Finland, France, Germany, Greece, Hungary, Indonesia, Ireland, Israel, Italy, Japan, Korea, Luxembourg, Mexico, Netherlands, New Zealand, Norway, Poland, Portugal, Russia, South Africa, Spain, Sweden, Switzerland, Turkey, United Kingdom, United States.

[12]These 25 countries are: Australia, Austria, Belgium, Canada, Czech Republic, Denmark, Finland, France, Germany, Greece, Hungary, Ireland, Italy, Japan, Mexico, Netherlands, New Zealand, Portugal, Russia, South Africa, Spain, Sweden, Switzerland, United Kingdom, United States. The final sample has the following data filling: between 1981 and 1991 we have data on only 4 countries; then, between 1991 and

We compile the data in a reproducible way and make all data manipulations open. To do this, we write a Python code that automatically downloads all the data from their respective sources by APIs/Data services. The code assembles the data in a unified database.

1.2.2 Choice of variables

Dependent variable. We date recessions using the Bry-Boshan Quarterly (BBQ) algorithm proposed in Bry and Boschan (1971) and adapted for quarterly data by Harding and Pagan (2002). This algorithm finds local peaks and troughs in the levels of economic activity.[13] As a measure of the economic activity of each country in our panel, we use a seasonally adjusted GDP volume index from the IFS database. We classify periods spanning from peaks to troughs as *recessions* and from troughs to peaks as *expansions*. This classification algorithm is designed to mimic the one used by NBER to date the business cycle in the US.[14] Ferroni and Canova (2022) show that the BBQ algorithm approximates well the Euro area turning points provided by the CEPR dating committee and replicates key business cycle moments.

Credit growth. Similar to Aikman et al. (2014), we use the annual log growth of the bank credit to GDP ratio. Mian et al. (2017) and Greenwood et al. (2022) consider similar measures but over a three-year period. In the sensitivity analysis, we consider alternative proxies for credit expansion used in the literature and show that our preferred measure delivers the best forecasting performance in terms of predictive power. The alternative credit growth measures are annual log growth of real outstanding credit (deflated by CPI; in line with Schularick and Taylor, 2012), and a deviation of the logarithm of loans to GDP ratio from its HP trend with $\lambda = 1,600, 26,000, 400,000$ (Alessi and Detken, 2011; Drehmann et al., 2011).[15] In all cases, the deviations from HP trends are computed in quasi-real time, i.e., based on the data up to the current point, and then re-computed for each new time point. Note also that in our case "annual" means "over the four quarters".[16]

1997 another 17 countries appear in the sample; and, finally, from 1998 to 2019 all countries are present.

[13] We use the Matlab program written by James Engel, which is available on https://www.ncer.edu.au/resources/data-and-code.php.

[14] Other papers that apply the BBQ algorithm for dating recessions and/or financial cycles are Bluedorn et al. (2016) for G7 countries and Claessens et al. (2012) for 21 advanced OECD and 23 emerging economies.

[15] Following Drehmann et al. (2011), we consider three different values of λ's. These values encompass the uncertainty around the duration of the credit cycle relative to the business cycle. If $\lambda = 400,000$, it is assumed that the credit cycle is four times longer than the business cycle; with $\lambda = 26,000$, this difference shrinks by a factor of two; and if $\lambda = 16,000$ it is assumed that they are of the same length.

[16] Greenwood et al. (2022) and Antunes et al. (2018) use specific percentiles of their credit and housing price variables to capture large changes that are likely associated with growing financial imbalances. In our baseline analysis, we do not rely on thresholds dictated by specific percentiles of bank credit distribution. However, we analyze the heterogeneity of recession response across four quartiles of bank credit growth.

Classical and other recession predictors. We consider five classical recession predictors commonly used in the literature as control variables. We also consider several other imbalance indicators that are known to predict recession at medium-run horizons. We discuss our choice of classical and other recession predictors in detail and provide information on variable construction in Appendix no. 1. Details on all data sources and the transformation of all variables are provided in Appendix no. 2.

Descriptive statistics of the dependent variable and recession predictors are reported in Table 1 in Appendix no. 3. Overall, we reveal that 15% of all country-quarter observations correspond to the recessionary state of the business cycle, thus delivering a sufficient amount of information to further estimate logit models of recessions. The sample is well-balanced; we have observations with both positive and negative growth rates for each variable considered.

1.3 Predicting recessions with bank credit

1.3.1 Flexible estimation with Jorda local projection

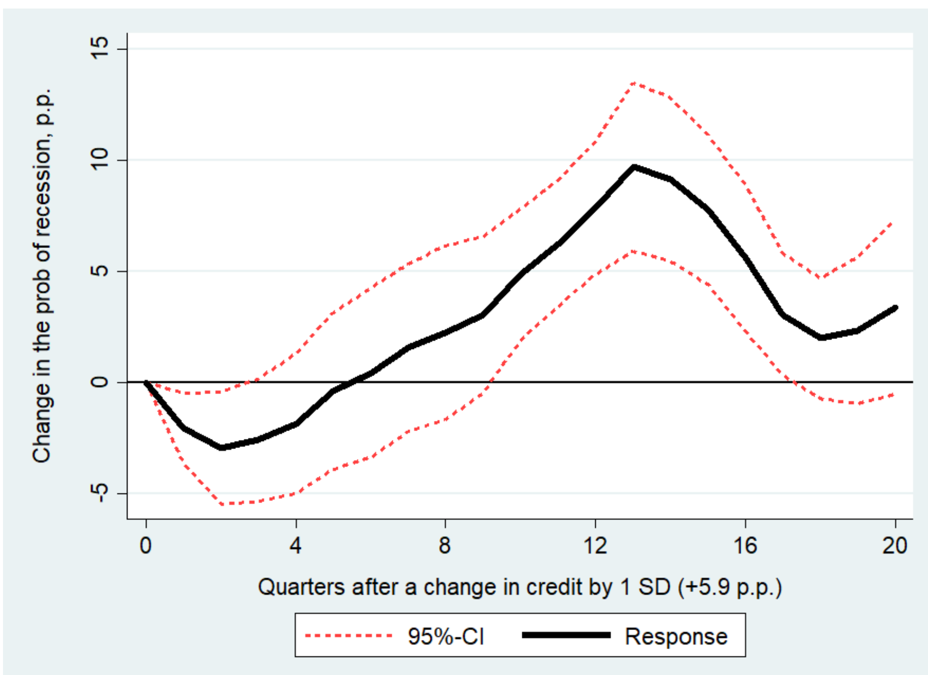
Baseline result

We are interested in the time structure of the impulse response of the risk of recession to credit expansions. We use Jorda (2005) local projections (LPs) to flexibly answer this question. Differently from Mian et al. (2017) and Baron et al. (2020) who use Jorda LPs to trace responses of GDP growth to credit and bank equity, we focus on the likelihood of recession:

$$Pr\Big(\mathrm{Y}_{i,t+h} = 1 \,\Big|\, \mathrm{Credit}_{i,t}, \mathbf{X}_{i,t}, \mathrm{Y}_{i,t}\Big) = \Lambda\Big(\alpha_i^{(h)} + \beta^{(h)}\mathrm{Credit}_{i,t} + \mathbf{X}'_{i,t}\Gamma^{(h)} + \mathrm{Y}'_{i,t}\Psi^{(h)}\Big) \qquad (1)$$

where h runs from 1 to 20 quarters ahead, the dependent variable is a binary indicator that equals 1, $\mathrm{Y}_{i,t+h} = 1$ if a country i is in a recession in quarter $t+h$, and 0 otherwise. $Credit_{i,t}$ is the annual, four-quarter, log growth of the ratio of bank credit to GDP in country i in quarter t, $\mathbf{X}_{i,t}$ includes five classical recession predictors in quarters t, $t-4$, and $t-8$ and bank credit growth in quarter $t-4$ and $t-8$. The right-hand side variable $\mathrm{Y}_{i,t}$ accommodates the binary indicator of recession in quarters t, $t-4$, and $t-8$. $\alpha_i^{(h)}$ is the country fixed effect and the underlying regression error $\varepsilon_{i,t}$ is assumed to obey logistic distribution $\Lambda(\cdot)$. The impulse responses of the probability of recession to changes in $Credit_{i,t}$ are estimated as the sequence of coefficients $\beta^{(h)}$ for $h = 1...20$. To account for serial correlation in the regression errors, we cluster the coefficients' standard errors on country i and quarter t levels, as in Mian et al. (2017).

The estimated impulse response appears in Figure 1. The figure depicts the response of recession risk to one standard deviation of bank credit growth (+5.9 pp in our sample) multiplied by the marginal effect associated with $\beta^{(h)}$. We obtain a clear boom-bust type recession response to bank credit growth. Within the first four quarters from a credit expansion (*short* horizon), the estimated response of the recession risk is negative and highly significant, reaching its trough at –3 pp. However, as time passes after the initial credit expansion (*medium-run* horizon), the estimated response turns positive and peaks at almost +10 pp in twelve quarters. The obtained medium-run economic effects obtained lie above those obtained by Schularick and Taylor (2012) and Greenwood et al. (2022), who estimate the effect of a one standard deviation credit expansion on the risk of the financial crisis as +2.6-2.8 pp. This is not surprising, given that they use annual data and predict rarer events—financial crises, which are recorded in 3-4 percent of cases in their annual samples, while we study recessions on the quarterly data, which are recorded in 15 percent of cases in our sample.



Note: The figure reports Jorda LP estimation results, as implied by the sequences of coefficients $\beta^{(h)}$ for $h = 1, 2, ..., 20$ quarters after a bank credit expansion, see equation (1). The initial impact of bank credit is normalized to its one standard deviation (+5.9 pp). The bank credit variable is the annual log growth of the domestic credit to GDP ratio. Standard errors are clustered on country and year levels.

Figure 1. Impulse response of the probability of recession to a bank credit expansion

It is important to understand which part of the credit distribution drives the impulse response reported in Figure 1. As Greenwood et al. (2022) show, only large credit expansions are harmful to financial stability in the future. Here, we test if this holds for recessions. For this purpose we split our main explanatory variable—annual log growth of domestic credit to GDP ratio—into four quartiles. With this division, we run the same regression (1), as before, except that now we have four instead of one credit variable: a $\jmath^{th}$ credit variable equals credit to GDP growth rates if its value is in quartile $Q_\jmath$ of credit growth distribution ($\jmath = 1...4$) and 0 if otherwise.[17] The estimated impulse responses of the recession risk are reported in Figure 1 (see Appendix no. 4). The results clearly show that our baseline result is driven by the fourth quartile of credit distribution. Therefore, we obtain evidence that only large credit expansions drive the overall boom-bust recession response to bank credit.

Further, we check if the medium-run response of credit expansion is robust if we use GDP growth rates instead of recession probability. Compared to the previous literature, our paper is the first to consider recession risk instead of the GDP growth rates (Mian et al., 2017; Baron et al., 2020; Greenwood et al., 2022). Therefore, it is important to check that our results hold for GDP growth. Estimation results are reported below, see Figure 2.

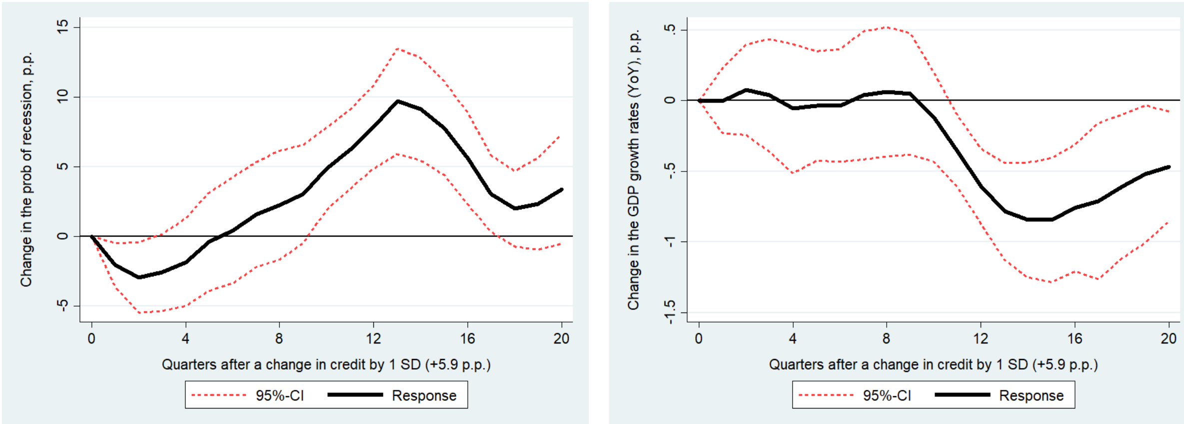
Overall, the medium-run response is robust if we use GDP growth instead of recession though the short-run effect is no longer significant. This observation, taken together with the recession response discussed above, suggests the existence of a nontrivial shift in output growth forecast density following a credit expansion. First, a growth of credit reduces recession risks while keeping output growth unaffected in the short run (tighter forecast growth distribution). Second, as time passes, credit expansion increases recession risks and decreases output growth in the medium run (left-skewed and negative mean shift of the forecast growth distribution).

1.3.2 Dynamic logit approach: controlling for the past dependence

The research on recession prediction concludes that accounting for inertia in business cycle phases[18] is important because dynamic models outperform static in out-of-sample crises prediction (Kauppi and Saikkonen, 2008; Candelon et al., 2014; Antunes et al., 2018). Along these lines, we employ a dynamic logit approach to predict recessions $Y_{i,t}$. Differently from Jorda (2005) LPs employed in the previous sections, we now use past information on *bank credit growth*, $Credit_{i,t-k}$, and the classical recession predictors, $\mathbf{X}_{i,t-k}$ to predict the current state of the economy. As before, we are interested in the ability of credit to predict recessions

[17]Differences in credit dynamics are substantial across the four quartiles. Maximum annual log growth of domestic credit to GDP ratio equals to as high as +23% in Q_4, only 5.6% in Q_3, 2.3% in Q_2, and 0% in Q_1. The first quartile contains only negative growth rates.

[18]If an economy is in an expansion today, it is likely to stay in the expansion next period.



(a) The probability of recession (*baseline*)

(b) GDP growth rates (YoY)

Note: The figure reports Jorda LP estimation results, as implied by the sequences of coefficients $\beta^{(h)}$ for $h = 1, 2, ..., 20$ quarters after a credit expansion, see equation (1). In subfigure (a), the dependent variable is the probability of a recession (baseline) and in subfigure (b) the dependent variable is the GDP growth rates, in %. Credit expansion is set as one standard deviation increase of bank credit (+5.9 pp). The bank credit variable is the annual log growth of the domestic credit to GDP ratio. Standard errors are clustered on country and year levels.

Figure 2. Impulse responses of the probability of recession and GDP growth rates to a bank credit expansion

on short horizons, $1 \leq k \leq 4$, and on medium-run horizons, $4 < k \leq 20$ quarters. To address potential omitted variable bias and treat all regressors equally, we apply the same lag structure to classical predictors $\mathbf{X}_{i,t}$.[19] The dynamic logit regression takes the following form:

$$Pr\Big(\mathrm{Y}_{i,t} = 1 \,\Big|\, \mathrm{Credit}_{i,t-k}, \mathbf{X}_{i,t-k}, \mathrm{Y}_{i,t-k}\Big) = \Lambda\Big(\beta_1 \mathrm{Credit}_{i,t-k} + \sum_{j=4(4)}^{20} \beta_j \mathrm{Credit}_{i,t-j} + \gamma_1 \mathbf{X}_{i,t-k} + \sum_{j=4(4)}^{20} \gamma_j \mathbf{X}_{i,t-j} + \rho \mathrm{Y}_{i,t-k} + \alpha_i\Big) \quad (2)$$

where $Y_{i,t} = 1$ if a country is in a recession, and 0 otherwise. $Y_{i,t-k}$ is the lagged dependent variable. $Credit_{i,t-k}$ is the annual log growth of the domestic bank credit to GDP ratio. $X_{i,t-k}$ represent classical recession predictors and include stock market index growth (annual log growth, in constant prices), a common component of business- and consumer confidence (sentiment) indicators, the state of the global economy, term spread, real short-term interest rate. We set $k = 1$ quarter for the baseline estimations (we vary k between 1 and 4 quarters as

[19]Previous literature has used short-term lags of classical predictors $\mathbf{X}_{i,t}$, typically up to one year.

the robustness check[20]). Following Schularick and Taylor (2012), we use the information on credit and other variables up to five years prior to recessions, i.e., a half-life of a typical business cycle according to Beaudry et al. (2020)'s analysis. We consider a maximum 20-quarter lag of credit with a step of four quarters to replicate analysis on annual data performed in Schularick and Taylor (2012); Mian et al. (2017).[21] α_i is a country fixed effect and the underlying regression error $\varepsilon_{i,t}$ is assumed to obey logistic distribution.

Recall that our hypothesis is that the relationship between bank credit growth and the probability of recession is changing sign, with short lags of credit decreasing recession risk and medium-run lags capturing *pro*-recessionary effects. Thus, we expect the following signs of coefficients:

$$\beta_1 < 0 \quad \text{and} \quad \sum_{j=4(4)}^{20} \beta_j > 0 \tag{3}$$

The estimation results of the dynamic logit model (2) appear in Table 1. In column 1 we start with a baseline specification which includes the short and longer lags of bank credit and only short lags of each of the five classical recession predictors, $\mathbf{X}_{i,t-1}$. In columns 2–6 we add medium-run lags of each of the five classical recession predictors, $\mathbf{X}_{i,t-1}$, one by one. Consistently across all columns, we obtain negative and highly significant coefficients on the short lag of credit and, conversely, a positive and also highly significant sum of the coefficients on the medium-run lags, confirming our hypothesis, see equation (3).[22] Our estimation of the dynamic recession model confirms the previous finding that the effect of bank credit growth on the future probability of recession switches from negative on short horizons to positive on medium-run horizons, controlling for classical recession predictors and past business cycle states on the same forecasting horizon.

Our estimates indicate that the coefficient on the one-quarter lag is negative across all specifications. The underlying economic effect associated with the one-quarter lag of credit

[20] In Appendix no. 12, we show that our main result survives if we choose deeper short lags of bank credit and classical predictors: $k = 2$, $k = 3$, or $k = 4$ instead of $k = 1$, i.e., predict recession 2, 3, and 4 quarters ahead instead of 1 quarter ahead in the baseline case; see Table 1. Further, we also run a modified version of the same model, in which we simultaneously include all short lags of the bank credit variable. As the results indicate, only the coefficient on the first quarter lag in this case preserves negative sign and statistical significance, whereas coefficients on the other short lags switch signs and appear largely insignificant, see Figure 1. Overall, the main result is robust to considering various short-run lags of credit.

[21] We also run a version of the model in which we include all quarterly lags from the fourth to twentieth, i.e., with a step of one quarter. The main result of the recession response to credit does not change, and we thus appeal to a more parsimonious specification. Note also that Antunes et al. (2018) use the same forecasting horizon as ourselves, but follow the general-to-specific approach to lag selection and remove insignificant lags. We instead employ all intermediate lags between 4 and 20 quarters with a step of 4 quarters to avoid an arbitrary choice of lag order.

[22] This result holds when we switch from the 1st to 2nd, 3rd or 4th quarter lags of bank credit, Table 1.

Table 1. Short- and medium-run effects of credit on recession under dynamic business cycle phase dependence

$X_{i,t}$:		Stock market	Confidence	World Economy	Term spread	Short-term interest rate
	(1)	(2)	(3)	(4)	(5)	(6)
Panel 1: Domestic bank credit go GDP, annual log growth ($Credit_{i,t}$)						
Short lag: $Credit_{i,t-1}$	–7.708***	–8.461***	–6.304***	–9.419***	–9.460***	–8.714***
	(2.211)	(2.367)	(2.123)	(2.152)	(2.417)	(2.064)
Medium-run lags:						
$Credit_{i,t-4}$	5.181*	7.352**	8.460***	8.811***	7.764***	6.096**
	(2.650)	(2.917)	(2.672)	(2.737)	(2.785)	(2.533)
$Credit_{i,t-8}$	0.851	1.245	2.567	–0.049	0.303	–0.153
	(2.532)	(2.774)	(2.636)	(2.399)	(2.631)	(2.465)
$Credit_{i,t-12}$	7.775***	7.945***	10.079***	9.405***	9.730***	9.054***
	2.539)	(2.663)	(2.573)	(2.348)	(2.531)	(2.399)
$Credit_{i,t-16}$	–3.517	–3.639	–1.914	–4.033*	–3.276	–2.892
	(2.162)	(2.335)	(2.151)	(2.085)	(2.327)	(2.084)
$Credit_{i,t-20}$	3.703*	3.301*	5.453***	4.517**	3.649*	4.365**
	(1.907)	(2.006)	(1.910)	(1.844)	(2.154)	(1.716)
Cumulative $\sum_{j=4}^{20} Credit_{i,t-j}$	13.994***	16.204***	24.645***	18.651***	18.170***	16.470***
	(2.789)	(3.308)	(3.737)	(3.288)	(3.512)	(2.898)
Panel 2: Classical recession predictors ($X_{i,t}$):						
Short lag: $X_{i,t-1}$	Yes	–2.364***	–0.202	–19.739**	–0.206***	0.871
		(0.580)	(0.135)	(8.308)	(0.060)	(5.918)
Medium-run lags:						
Cumulative $\sum_{j=4}^{20} X_{i,t-j}$		–0.006	–0.375**	30.488**	0.136	–2.272
		(0.947)	(0.185)	(15.486)	(0.084)	(5.887)
N Obs.	2,261	2,261	2,282	2,391	2,250	2,338
N Countries	25	25	25	25	25	25
Pseudo R^2	0.526	0.536	0.540	0.542	0.527	0.527
Δ AUROC w.r.t. no $Credit_{i,t}$		0.008***	0.009***	0.007**	0.008***	0.010***
Δ $\Pr(Y_{i,t}=1 \mid \Delta Credit_{i,t-1}=\sigma_L)$	–0.025***	–0.027***	–0.021***	–0.031***	–0.031***	–0.029***
Δ $\Pr(Y_{i,t}=1 \mid \Delta \sum_{j=4}^{20} Credit_{i,t-j}=\sigma_L)$	0.046***	0.052***	0.081***	0.061***	0.060***	0.056***
Δ $\Pr(Y_{i,t}=1 \mid \Delta X_{i,t-1}=\sigma_X)$		–0.031***	–0.013	–0.017**	–0.024***	0.001
Δ $\Pr(Y_{i,t}=1 \mid \Delta \sum_{j=4}^{20} X_{i,t-j}=\sigma_X)$		0.000	–0.024**	0.027**	0.016	–0.003

Note: *Stock* is Δ log (Real stock market index) (annual log growth). *Confidence* is the first principal component of OECD Business and Consumer confidence indicators. *World economy* is a composite leading indicator for the OECD economies. Each column contains (i) all five classical recession predictors on the short horizon (lag=1), (ii) Lagged dependent variable (lag=1 quarter), and (iii) Country fixed effects, which are not reported to preserve space. Hereinafter, $j = 4(4)20$ means j runs from 4 to 20, with a step of 4 quarters. σ_L and σ_X are one standard deviation of annual log bank credit growth ($Credit_{i,t}$) and a given classical recession predictor ($X_{i,t}$) across all countries and quarters in the sample.
***, **, * indicate that a coefficient is significant at the 1%, 5%, 10% level, respectively. Robust standard errors appear in brackets under the estimated coefficients.

is bounded between –3.1 and –2.1 pp of the probability of recession and is thus sizeable. Moreover, the effect is consistent with the Jorda local projection specification presented in the previous section (–3 pp).

Our estimates clearly favor reversal of the short-run effect: the sum of the coefficients on fourth to twentieth quarter lags of bank credit turns positive across all specifications, again irrespective of whether the classical recession predictors are controlled for at the medium-run forecasting horizon. A one standard deviation increase in bank credit growth over the previous five years is associated with an increase in the probability of a recession by 4.6-8.1 pp

Omitted monetary policy? As before, a potential concern for our baseline result is that the medium-run positive relationship between bank credit growth and the risk of a recession could be driven by omitting endogenous monetary policy tightening (Brunnermeier et al., 2021). Note, however, that in all columns of Table 1 we include a one-quarter short-run lag of the short-term interest rate, while in column (6) of Table 1 we add medium-run lags of the short-term interest rate. Thus, in the dynamic recession prediction model, we control everywhere for the *previous period* monetary conditions, and in column (6) of Table 1 we additionally control for all past information on monetary conditions and still obtain the same time pattern of recession response. Therefore, we again confirm that our baseline result of the dynamic relationship between bank credit and recession risk is not driven by omitted monetary conditions.

Is credit unique in predicting recession over the medium term?. Several papers reveal that other predictors can also affect the recession risk on the medium-run horizons. For example, Alessi and Detken (2018) show that the growth of residential property prices predicts financial instability. Schmitt-Grohe and Uribe (2021) show the existence of recurrent endogenous cycles in open economies, which are associated with movements in current account balance and appreciation/depreciation of the real effective exchange rate. Below we investigate if these three indicators affect the recession risk on the medium-run horizon and if omitting them caused bias in our previous results.

To address this issue, we re-run equation (2), in which we include the one-quarter lag and the longer lags of (*i*) the annual log growth of the real residential property prices index (RPI), (*ii*) negative of the current account balance to GDP ratio (–CAB) as a proxy for changes in foreign capital inflow (a similar proxy was employed in Davis et al., 2016), and (*iii*) annual log growth of the real effective exchange rate (REER).

We report the estimation results in Table 2. In all three cases, we still obtain a negative and highly significant coefficient on the one-quarter lag of bank credit and a positive and highly significant sum of the longer lags of bank credit. Quantitatively, the underlying economic effects remain very similar to the baseline analogs. Our baseline result survives.

Regarding the three imbalance indicators themselves, we find that RPI works on the short horizon but does not predict recession on medium-run horizons, controlling for bank credit growth. Negative CAB to GDP ratio and REER, in contrast, do not work on the short horizon but do predict recession on the medium-run horizon. Note that the CAB deficit has a large economic effect on recession (as denoted by $\Delta\ \Pr(Y_{i,t} = 1 \mid \Delta \sum_{j=4}^{20} Imb_{i,t-j} = \sigma_{Imb}))$ on top the effect of bank credit, confirming that if credit expansion is fueled by foreign borrowing its negative effects on financial and macroeconomic stability are amplified, (Davis et al., 2016).

Table 2. Is credit unique in predicting recession over the medium term? Dynamic recession model with other imbalance indicators

$Imb_{i,t}$:	RPI	–CAB	REER
	(1)	(2)	(3)
Panel 1: Domestic bank credit ($Credit_{i,t}$)			
Short lag: $Credit_{i,t-1}$	–7.984***	–6.451***	–6.008***
	(2.962)	(2.106)	(2.173)
Longer lags:			
Cumulative $\sum_{j=4}^{20} Credit_{i,t-j}$	23.449***	10.478***	14.696***
	(4.615)	(2.997)	(3.052)
Panel 2: Other imbalance indicators ($Imb_{i,t}$)*:*			
Short lag: $Imb_{i,t-1}$	–12.938***	–5.378	–2.980
	(2.932)	(8.499)	(2.068)
Longer lags:			
Cumulative $\sum_{j=4}^{20} Imb_{i,t-j}$	–0.812	21.050***	7.814*
	(3.052)	(9.520)	(4.756)
N Obs.	1,969	1,786	1,738
N Countries	25	25	25
Pseudo R^2	0.537	0.529	0.529
Δ AUROC w.r.t. no *Credit*	0.005*	0.005**	0.008***
$\Delta\ \Pr(Y_{i,t} = 1 \mid \Delta Credit_{i,t-1} = \sigma_L)$	–0.023***	–0.024***	–0.024***
$\Delta\ \Pr(Y_{i,t} = 1 \mid \Delta \sum_{j=4}^{20} Credit_{i,t-j} = \sigma_L)$	0.069***	0.039***	0.058***
$\Delta\ \Pr(Y_{i,t} = 1 \mid \Delta Imb_{i,t-1} = \sigma_{Imb})$	–0.051***	–0.015	–0.011
$\Delta\ \Pr(Y_{i,t} = 1 \mid \Delta \sum_{j=4}^{20} Imb_{i,t-j} = \sigma_{Imb})$	–0.003	0.060***	0.028*

Note: *RPI* is Δ log (Real Residential Property Index) (annual log growth). *–CAB* is the negative ratio of the annual, four-quarter, sum of the current account balance to the annual flow of GDP. Here, taking "negative" makes this variable reflect movements in cross-border capital flows. *REER* is Δ log (Real Effective Exchange Rate) (annual log growth)). Each column contains (i) the other five classical recession predictors (lag=1), (ii) lagged dependent variable (lag=1), and (iii) country fixed effects and constant term, which are not reported to preserve space.
***, **, * indicate that a coefficient is significant at the 1%, 5%, 10% level, respectively. Robust standard errors appear in brackets under the estimated coefficients.

A technical note. We show that the information contained in the medium-run lags of credit growth substantially improves the forecasting accuracy of the dynamic logit model. As is demonstrated by the increases of respective areas under ROC curves (AUROCs) relative to corresponding specifications without credit, adding longer lags of credit significantly raises the share of correctly predicted states of the business cycle. This is an important result because there is not much space left for an improvement on the one-quarter horizon (the ROC curve is rather steep, and the area under the ROC curve is large, see Figure 1 in Appendix no. 5. Intuitively, on the one-quarter horizon, an econometrician knows a lot: she already observes the state of the business cycle in the previous quarter, and the previous quarter's realizations of other classical predictors, including consumer and business confidence, which are all informative.[23]

1.3.3 Discussion of the results

As discussed in the introduction, there are two strands of related literature: one examines the short-run effects of credit expansions on the economy and real decisions of economic agents, and the other explores the medium-run effects.

Short-run effects. We find that a credit expansion is associated with a significant decline in the recession probabilities in up to four quarters. Qualitatively, this is consistent with the two streams of literature that find the short-run positive effect of greater credit availability (or *negative* effect of credit *tightening*) on macroeconomic aggregates and micro-level responses. First, the empirical macroeconomic literature shows that GDP growth rates rise following negative shocks to credit spreads (Gilchrist and Zakrajsek, 2012) or positive shocks to credit supply (Gambetti and Musso, 2017) in the short run. Second, loan-level data analysis suggests that negative shocks to bank credit supply lead to a reduction of (*i*) firms' employment (Chodorow-Reich, 2014), investment and sales (Gropp et al., 2018; Chopra et al., 2020) and (*ii*) households' consumption (Jensen and Johannesen, 2017). Our results are consistent with these studies on the short-run effect of financial disintermediation.

Medium-run effects. Our results confirm that credit expansion is associated with a significant rise in the recession probabilities on the eighth- to twelfth-quarter horizon and that the underlying economic effects are at least 3 times larger in magnitude than the short-run effects. The medium-run effects of credit expansions can be explained through several mechanisms recently explored in the literature. The literature on endogenous business cycles establishes that the quality of bank loans deteriorates following low lending standards triggered by buoyant credit market sentiments, but then turns back to normal as lenders tighten the lending standards (Farboodi and Kondor, 2020). The literature on information production

[23] Though we admit that in real life, dating of the turning points of the business cycle and macroeconomic statistics lags behind.

shows that costly screening of borrowers induces banks to rely more on the value of collateral, which has negative dynamic implications: in the longer run, the quality of information on the banks' borrowers worsens substantially, thus sowing the seeds of future recessions (Asriyan et al., 2022). From a macro perspective, an over-accumulation of debt can be detrimental for long-run solvency and macroeconomic stability because of the approaching of binding collateral constraint as debt accumulates (Schmitt-Grohe and Uribe, 2021; Benigno et al., 2020) and negative effect of debt on demand—the debt-deflation mechanism highlighted by Mian et al. (2017) and explored in Mian et al. (2021).

Overall, both of our effects, short-term and medium-term, are consistent with recent empirical findings and can be rationalized with the recent theoretical work.

1.4 What explains the overall recession response to bank credit? Exploring the channels and driving forces

In the previous section, we have established the boom-bust type of recession response to bank credit growth. We now ask which economic forces are responsible for this type of response and through which channels this effect is transmitted.

1.4.1 The role of business cycle shocks

Is the boom-bust-type relationship between bank credit and recession risk driven by changes in credit supply (Mian et al., 2017)? Or is this relationship driven by restrictive monetary policy shocks that can cause a decline of the economy by endogenously responding to rising consumer prices during the periods of credit expansions (Brunnermeier et al., 2021)? Is there a place for the aggregate demand shock, which is known as the main business cycle shock (Smets and Wouters, 2007; Bilbiie et al., 2022)? The first two shocks come from banks and monetary regulators, whereas the third arises from shifts in preferences and expectations of households and firms, and from spending shocks. To distinguish between shocks as driving forces of credit, we identify domestic business cycle shocks and estimate their relevance in explaining the boom-bust time structure of recession response to bank credit.

We rely on the sign restrictions approach, commonly used in the literature to jointly identify several shocks affecting the economy at the same time (Furlanetto et al., 2017; Gambetti and Musso, 2017; Baurle et al., 2021; Geiger and Scharler, 2021). An alternative popular method of shocks identification—a high-frequency approach—is not suitable for our purposes since it is tailored for one shock at a time, see, e.g., monetary and oil supply news shocks identification in Gertler and Karadi (2015) and Kanzig (2021a), but not for many shocks in many countries as in our case.

We borrow the sign restrictions scheme from Gambetti and Musso (2017) who provide the necessary restrictions from quantitative models with the financial sector. An appealing feature of this approach is that it requires only five variables—output, consumer prices, regulated (short-term) interest rate, the interest rate on loans, and the volume of loans outstanding—in the structural VAR model. This parsimony simplifies the extraction of shocks in a cross-country setting, given the computational time needed for each of the 25 countries. Following Gambetti and Musso (2017), we identify four shocks: aggregate demand (AD), aggregate supply (AS), monetary policy (MP), and credit supply (CS).[24] For the baseline estimation, we impose all restrictions on impact.[25]

We distinguish expansionary AD and AS shocks by imposing distinct restrictions on the price level response: the demand shock leads to an unexpected increase in prices, whereas the supply shock reduces prices below the (model-based) prediction. In turn, an expansionary MP shock is not mixed with the expansionary AD shock because the short-term interest rate decreases in the case of MP shock and rises in the case of AD shock. Therefore, an endogenous monetary tightening in response to expansionary AD shocks is assumed by the identification. Finally, an expansionary CS shock is separated from the AD shock (which is correlated with credit demand) because they move the lending rate in opposite directions. All sign restrictions are reported in Table 1 in Appendix no. 7. Overall, this multiple sign restriction scheme ensures that different shocks do not "masquerade" (Wolf, 2020).

For each country in our sample, we run the five-variable VAR model, isolate the four shocks, then check if the shocks come out during well-known historical events and crises (Figures 1), check if the shocks have proper distribution (Figure 2), and report impulse responses (Figure 3). We provide a description of the SVAR estimation results in Appendix no. 7.

Having obtained reliable estimates of the business cycle shocks, we then conduct a *historical decomposition* (HD) of variables in the VAR model to isolate parts of the overall variation in the bank credit growth which are driven by each of the four shocks separately ('first stage'). Details on the historical decomposition and an example for the U.S. economy are also reported in Appendix no. 7 (see Figure 4).

With the historical decomposition of bank credit growth to four shocks, we turn to explore whether, and by how much, AD-, MP-, and CS-driven parts of the overall credit variation

[24]See details on the econometric procedure in Appendix no. 6 and details on the sign restrictions in Table 1 in Appendix no. 7.

[25]In the sensitivity analysis, we impose all restrictions on impact and in quarter one following shocks. The results become slightly stronger.

are able to explain the boom-bust relation between recession risk and bank credit ('second stage').[26] In particular, we modify our baseline Jorda LP equation (1) by replacing the key explanatory variable—the log annual growth of the domestic bank credit to GDP ratio—by the part of it driven by $\jmath^{th}$ shock ($\jmath = 1, ..., 4$):

$$\begin{aligned} Pr\Big(\mathrm{Y}_{i,t+h} = 1 \,\Big|\, \widehat{Credit}_{i,t}^{(\jmath)}, \mathbf{X}_{i,t}, \mathrm{Y}_{i,t}\Big) \\ = \Lambda\Big(\alpha_i^{(h)} + \beta_\jmath^{(h)} \widehat{Credit}_{i,t}^{(\jmath)} + \mathbf{X}'_{i,t}\Gamma^{(h)} + \mathrm{Y}'_{i,t}\Psi^{(h)}\Big) \end{aligned} \tag{4}$$

where everything follows the lines of equation (1), except that for now credit is replaced with its counterfactual dynamics driven by each shock of interest.

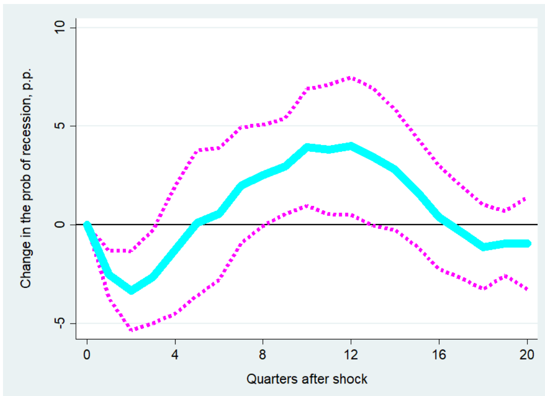
An important technical note is that sign restrictions are typically set under the Bayesian, not frequentist, approach, thus delivering a distribution rather than point estimates of the shocks and historical decomposition of a variable of interest, i.e., $\widehat{Credit}_{i,t}^{(\jmath)}$ in our case. This adds another layer of uncertainty to the results, which we tackle in the sensitivity analysis below. The main estimation results presented here are obtained with *median* shocks.

The estimation of the modified Jorda LP equation (4) appears in Figure 3. On each of the four subfigures, we show responses of the recession risk to a one standard deviation rise of respective $\widehat{Credit}_{i,t}^{(\jmath)}$ (which is between 1.3 and 1.4 pp depending on the shock, thus being four times lower than one standard deviation of credit in the overall variation of credit). As before, we account for the autocorrelation in the regression errors and cluster the coefficients' standard errors on country and year levels.

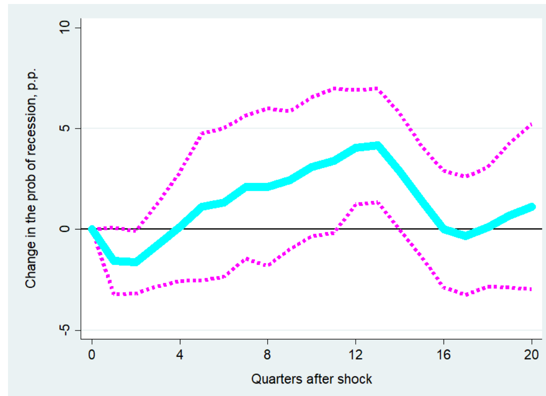
Our estimation results indicate that following a one standard deviation increase in the AD-driven credit to GDP ratio (1.4 pp), the probability of recession declines by 3.5 pp in two quarters and then rebounds to the positive territory, reaching the peak of 4.7 pp in ten quarters after the initial impulse (both estimates are significant at 5% level), see Figure 3.a. Strikingly, we find that only AD shock is able to confidently generate the boom-bust shape of the recession response to credit expansions, whereas other business cycle shocks, when propagating through bank credit, only increase the recession risk on the medium-run horizon of three years. Indeed, as can be inferred from Figure 3.b-d, following a one standard deviation increase in the ratio of either AS-, MP-, or CS-driven credit to GDP (1.3–1.4 pp), the probability of recession tends to drop by 1.5–2.0 pp in one or two quarters but the short-

[26] The idea for this two-stage approach comes from the study by Lopez-Salido et al. (2017) which analyzes credit spread reversals and explores the ability of these reversals to predict the ups and downs of GDP growth in a two-year horizon. As the authors argue, the approach only mechanically resembles IV-2SLS, but the key distinction is that it does not require the exclusion restriction to hold. Differently from their study, we can argue that our shocks are by construction exogenous to any movements in both bank credit growth and the probability of recession.

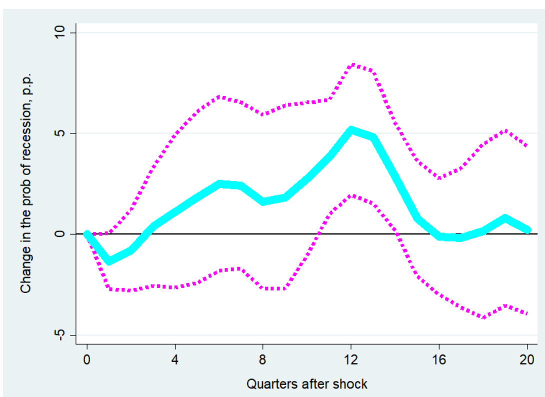
run effects are insignificant. In twelve quarters, the recession response becomes positive and significant approaching the range of 2.7–5.1 pp (significant at 5%), which is comparable with the peak response to the AD-driven credit. Overall, our results indicate that the boom-bust shape of the recession risk response to bank credit obtained with the overall variation in bank credit is explained exclusively by shocks to aggregate demand in the economy.



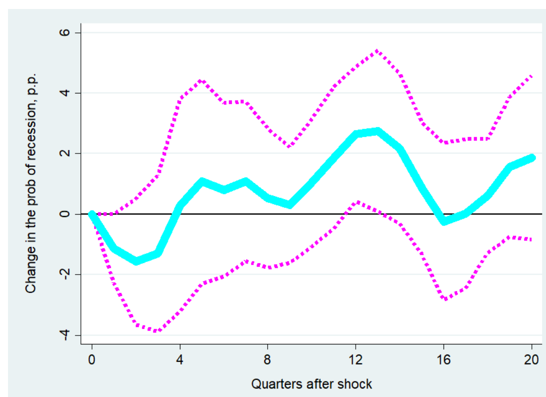
(a) Aggregate demand



(b) Aggregate supply



(c) Monetary policy



(d) Credit supply

Figure 3. Shocks transmission through bank credit: effects on recession

Note: The figure reports Jorda LP estimation results, as implied by the sequences of coefficients $\beta^{(h)}$ for $h = 1, 2, ..., 20$ quarters after a credit expansion, see equation (4). On each of the four subfigures, we show responses of recession risk to a one standard deviation rise of respective $\widehat{Credit}_{i,t}^{(j)}$ (which is between 1.3 and 1.4 pp, depending on the shock). Standard errors are clustered on country and year levels.

In Appendix no. 10, we also show that if we switch the dependent variable from the binary indicator of recessions to the annual growth rates of real GDP, we obtain similar results. We find that following an AD-driven credit expansion, GDP growth rates rise significantly during the first year but then decline significantly in three years after the initial AD shock. Again, this holds only for the AD shock—neither of the other business cycle shocks generates this boom-bust response. We find that GDP growth rates only decline in three years after a credit expansion driven by either MP-, CS-, or AS-shocks. Importantly, if we consider reduced-form equations and estimate the direct effect of each of the four shocks on the probability of recession or GDP growth rates, then the results do not mirror those with the transmission through bank credit. Each of the four business cycle shocks, when regressed directly on the recession probability, affects it differently from the case when shocks are transmitted through bank credit. This suggests that there is a non-trivial transmission of shocks through credit.

Discussion of the results. Our finding of the special role of AD shocks in boom-bust business cycle dynamics is consistent with several streams of the literature. First, recent developments in the literature on boom-bust cycles have shown that the *sentiment*-driven booms predictably end in recessions (Brianti and Cormun, 2022; Kantorovitch, 2021; Asriyan et al., 2022). Second, in a related field, Bordalo et al. (2018) show that a too-optimistic perception of good news by economic agents generates expectations-driven boom-bust swings in *credit* cycles. Third, Angeletos and Lian (2021) present a New Keynesian model with intertemporal substitution in production and bounded rationality, which features shifts in aggregate demand as a driving force of business cycles.

Our empirical findings shed light on the important debate regarding the source of credit and housing boom and bust around the Great Recession. We show that AD shocks exclusively generate the boom-bust response of recession to credit, which is consistent with the finding of Kaplan et al. (2020) who show that shifts in beliefs, *not* credit supply shocks—as emphasized earlier by Mian and Sufi (2009) and Mian et al. (2017),—explain the housing and consumption boom and bust around the Great Recession.

1.4.2 Exploring borrowing sector heterogeneity: household versus firm credit

Having established that the boom-bust response of recession risk to bank credit growth is driven exclusively by AD shocks, we now ask which borrowing sector is responsible for this result. Recall that Mian et al. (2017) find that household credit predicts a decrease in GDP growth rates in three years in a cross-country setting, whereas credit to non-financial firms does not. Based on this evidence, we can expect that household credit generates higher recession risks compared to firm credit on medium-run horizons. We further compare the recessionary effects of both household and firm credit expansions.

To explore these effects, we again employ the Jorda LPs (1) from the previous section and test whether the boom-bust response survives if the domestic bank credit is replaced by household and by firm credit[27] (*overall variation*). We then turn to equation (4) and analyze whether AD shock generates the boom-bust recession response when pushing household and firm demand for credit (*AD-driven variation*). We then compare the results with those obtained under CS-driven variation in household and firm credit.

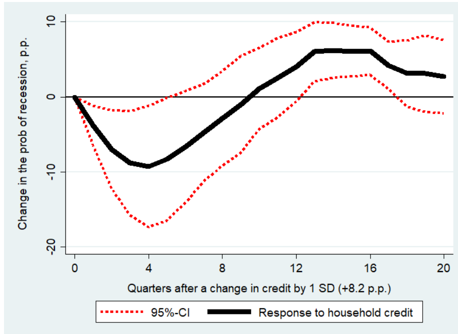
Overall variation in household and firm credit. As can be inferred from comparing Figures 4.*a* and 4.*b*, only household credit expansion results in the boom-bust recession response, whereas firm credit increase is associated with an immediate rise of the likelihood of economic downturn. Notably, we reveal that on the short horizon, a one standard deviation increase in the household credit to GDP ratio (8.2 pp) is associated with up to a 10 pp drop in the probability of recession, which exceeds the analogous estimate for bank credit obtained in the previous section by a factor of 3. Conversely, on the same short horizon, a one standard deviation rise in the firm credit to GDP (6.1 pp) is associated with a spike in the risk of a recession of 7 pp. Therefore, on the short horizon, household and firm credit expansions predict the recession response of opposite signs. As time passes, household credit expansion results in a switch from declining to rising recession risk, which peaks at +6 pp on the medium-run horizon—in three years after the initial impulse to credit—and then moves back to zero. The recession response to firm credit expansion also shrinks to zero beyond the three-year horizon.

AD- and CS-driven variation in household and firm credit.[28] Turning to Figures 4.*c* and 4.*d*, we find that expansionary AD shock generates the boom-bust recession response only when it drives household credit but not credit to firms. We clearly reveal that, following a one standard deviation rise in the AD-driven household credit (1.7 pp), the likelihood of recession slumps by 6 pp in two quarters but then increases by 5 pp in four years after (both estimates are significant at 5%). In contrast, the response of recession risk to AD-driven firm credit, though also exhibiting some boom-bust pattern, remains insignificant throughout all five-year horizon.

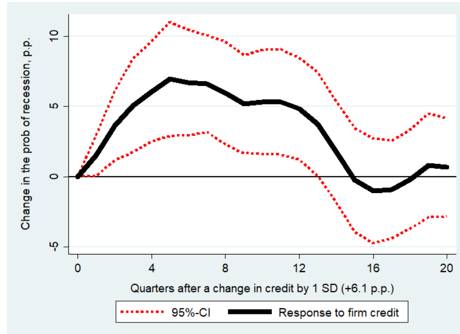
In contrast, when it comes to CS-driven variation in credit, we obtain no boom-bust responses either for households or for firms, see respectively Figures 4.*e* and 4.*f*. Moreover, for households, the response is close to zero and barely significant during the whole five-year horizon, whereas for firms, the response is positive and significant in one year after the shock, reaching +4 pp after a one standard deviation impulse. As we show in Appendix no.

[27] Note that in the BIS credit data, household and firm credit do not sum to domestic bank credit, but they sum to total credit to the private non-financial sector which includes domestic bank credit, non-bank credit, and foreign credit.

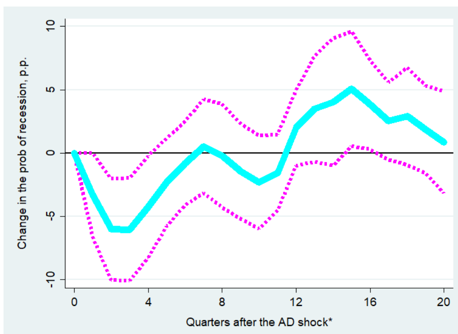
[28] For the decomposition using the other shocks, see Appendix no. 8 for household credit and Appendix no. 9 for firm credit.



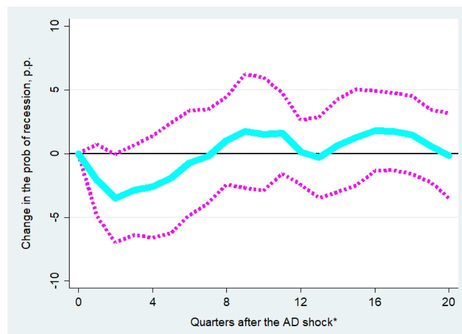
(a) Household credit, *overall variation*



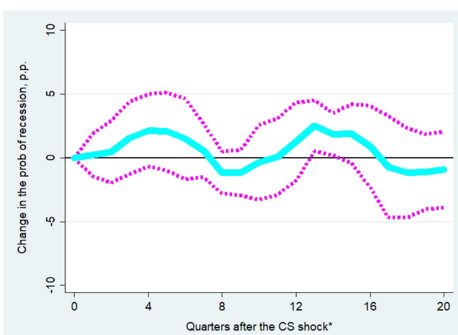
(b) Firm credit, *overall variation*



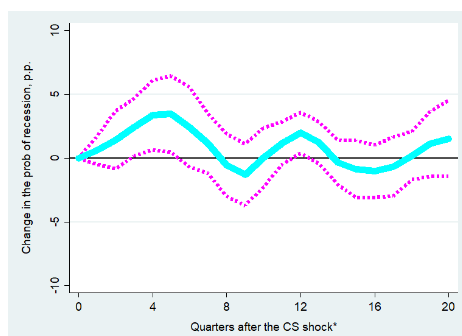
(c) Household credit, *AD-driven variation*



(d) Firm credit, *AD-driven variation*



(e) Household credit, *CS-driven variation*



(f) Firm credit, *CS-driven variation*

Note: The figure reports Jorda LP estimation results for different types of bank credit. Subfigures (a) and (b) report estimates of coefficients $\beta^{(h)}$ ($h = 1, 2, ..., 20$) for household and firm credit as implied by equation (1), i.e., overall variation). We further isolate a part of the variation in household and firm credit that is driven by expansionary shocks to aggregate demand or credit supply in the economy ('first stage'). On each of the six subfigures, we show the responses of recession risk to a one standard deviation rise of household and firm credit (8.2 and 6.1 pp in subfigures a and b), a one standard deviation rise of AD-driven household and firm credit (1.7 pp in subfigures c and d), and a one standard deviation rise of CS-driven household and firm credit (1.6 pp in subfigures e and f). Standard errors are clustered on country and year levels.

Figure 4. Impulse responses of the probability of recession to *household* and *firm* credit, $k = 1...20$ quarters ahead

9, CS-driven variation is the only source that can explain the immediately-positive response of recession risk to firm credit expansions.

1.4.3 Does collateral boom matter for the credit-recession relation?

As the results of the two previous subsections indicate, the boom-bust response of recession to bank credit is exclusively generated by expansionary AD shocks in the economy. Moreover, not all credit expansions have the same implications: the boom-bust recession response is a feature of household credit, whereas a firm credit expansion immediately increases the risk of economic downturns.

In this section, we link these results to collateral prices. As Greenwood et al. (2022) show empirically, credit expansions dramatically increase the probability of financial crises in the future if they are accompanied by soaring asset prices. We hypothesize that the boom-bust response of recession risk to a household credit expansion is amplified through the cyclical movements in the housing market. In particular, when housing prices soar and the value of housing as collateral on mortgages increases, banks endogenously raise the supply of credit to households by more (Kaplan et al., 2020) and reduce interest rates on credit compared to the no asset price boom case. As a result, there is an additional increase in household demand: the economic boom is greater than is the case when the asset-price inflation is low, and the risk of recession slumps. However, when the housing prices turn to contract, banks become less willing to extend new credit and tighten credit conditions to a greater extent (Mian et al., 2013), which reinforces borrower default risk (triggered by a shortage of cash flow, Ganong and Noel, 2022), and further increases the risk of recession.

In contrast, when stock market prices go up and the value of *many* firms thus rise, banks tend to decrease project screening intensity—the result shown empirically by Becker et al. (2020) and Cao et al. (2022), so that the stock of borrower information depletes, as rationalized by Asriyan et al. (2022). Therefore, firm credit expansions, if accompanied by a stock market boom may be risky from the beginning due to increased lending to less screened projects, and, as a result, due to credit misallocation, Gopinath et al. (2017).

To formally test these hypotheses in our setting, we re-estimate our Jorda LP impulse responses of recession risk to credit expansions in two "regimes": high and low asset price growth. We use real housing price growth and real stock market price growth as measures of asset price growth. Following recent literature on the state-dependent effects of macroeconomic shocks (Auerbach and Gorodnichenko, 2012; Tenreyro and Thwaites, 2016; Ramey and Zubairy, 2018; Ben Zeev, 2019), we employ a state-dependent local projection approach in which we explicitly sort all country-quarter observations into two states to estimate two time-series of responses.

Our baseline Jorda LP equation (1) modifies as follows:

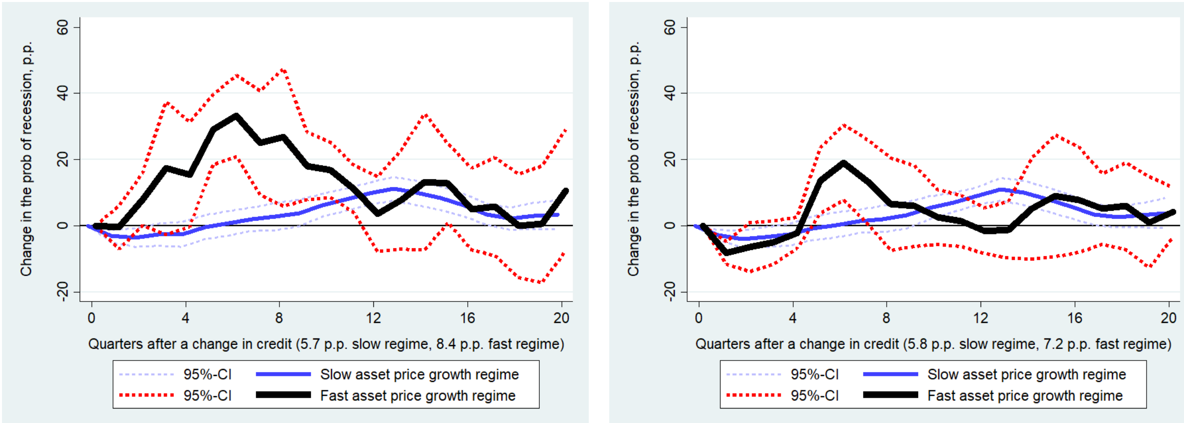
$$
\begin{aligned}
&Pr\Big(\mathrm{Y}_{i,t+h} = 1 \,\Big|\, \mathrm{Credit}_{i,t}, \mathbf{X}_{i,t}, \mathrm{Y}_{i,t}\Big) \\
&= \Lambda\Bigg(\quad \mathbf{I}_{i,t} \quad \cdot \Big[\alpha_{A,i}^{(h)} + \beta_{A}^{(h)}\mathrm{Credit}_{i,t} + \mathbf{X}'_{i,t}\Gamma_{A}^{(h)} + \mathrm{Y}'_{i,t}\Psi_{A}^{(h)}\Big] + \\
&\qquad \Big(1 - \mathbf{I}_{i,t}\Big) \cdot \Big[\alpha_{B,i}^{(h)} + \beta_{B}^{(h)}\mathrm{Credit}_{i,t} + \mathbf{X}'_{i,t}\Gamma_{B}^{(h)} + \mathrm{Y}'_{i,t}\Psi_{B}^{(h)}\Big]\Bigg)
\end{aligned}
\tag{5}
$$

where $\mathbf{I}_{i,t}$ denotes an indicator variable that describes the state of the economy in a country i at time t: fast- or slow-growing asset prices (A or B, for simplicity), where 'asset prices' are either housing or stock market prices. In the baseline specification of equation (5), we set $I_{i,t} = 1$ if the log annual growth of asset prices exceeds the 90^{th} percentile of its distribution, and 0 otherwise.[29] All model coefficients are state-dependent and vary across the regimes A and B. The state-dependent impulse responses of the probability of recession on horizons $t + h$ to the change in bank credit growth in t are estimated as sequences of the marginal effects implied by the coefficients $\beta_A^{(h)}$ and $\beta_B^{(h)}$ multiplied by the standard deviation of bank credit growth. We begin with estimating equation (5) using the overall variation in bank credit growth variable, and we then decompose the estimation for household and firm credit.

Overall variation in bank credit. The estimates of equation (5) with aggregate bank credit growth variable appear in Figure 5. In subfigure (a) all country-quarter observations are divided into a either high *stock market* growth regime ($\mathbf{I}_{i,t}^{Stock} = 1$) or low *stock market* growth regime ($\mathbf{I}_{i,t}^{Stock} = 0$). In subfigure ($b$) all observations are re-sorted into a high *housing prices* growth regime ($\mathbf{I}_{i,t}^{Housing} = 1$) or low *housing prices* growth regime ($\mathbf{I}_{i,t}^{Housing} = 0$).

Strikingly, we obtain that the boom-bust recession response to bank credit disappears under the '$\mathbf{I}_{i,t}^{Stock} = 1$' regime and, by contrast, this response not only preserves but also becomes more pronounced under the '$\mathbf{I}_{i,t}^{Housing} = 1$' regime. In the first case, the estimates suggest that, if a credit expansion is accompanied by (extremely) rapid growth of stock market prices, the probability of recession immediately starts soaring, peaking at 33 pp in six quarters and then

[29] In the sensitivity analysis, we report the results with a milder threshold—67^{th} percentile of the distribution of observations by asset price growth, as employed in Greenwood et al. (2022), see Appendix no. 14. Overall, the results are less pronounced and the differences are often insignificant between alternatively defined regimes with the milder threshold. At the same time, the recession response is larger and rises sooner under moderately high asset price growth, which is qualitatively in line with the results for the 90^{th} percentile. For the stock price growth and firm credit, in contrast, the results with the milder threshold are in line with those with the 90^{th} percentile.



(a) Stock market prices

(b) Residential property prices

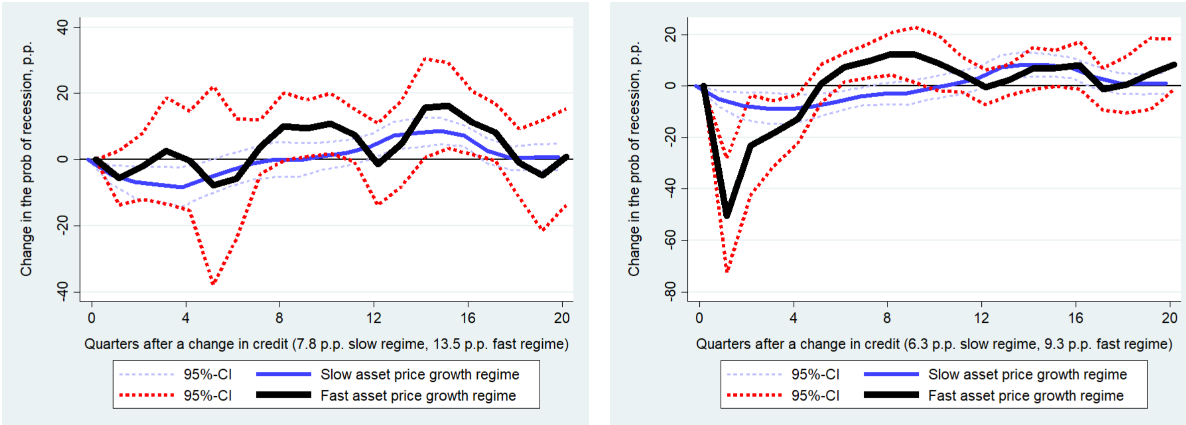
Note: Each subfigure reports a k-step ahead prediction of the probability of a recession in quarters $t + k$, $k = 1...20$, following a one standard deviation change of bank credit to GDP growth in quarter t. On both subfigures, we split the sample into two sub-samples of country-quarter observations: those in which countries experience extremely high growth rates of domestic stock market prices (a) or residential property prices (b) (above the 90^{th} percentile across all countries).

Figure 5. Impulse responses of the probability of recession to a bank credit expansion under *high* and *low asset price growth*, $k = 1...20$ quarters ahead

slowly vanishing in another six quarters. In the case of housing prices, the response is first negative, reaching its trough at –8 pp in one quarter but then rebounding to the positive territory, where it peaks at +20 pp in the same six quarters after the initial expansions; the response attenuates to zero much faster than in case of a high stock market growth regime. Therefore, we conclude that the boom-bust recession response is amplified through the housing market, but not via the stock market. In contrast, high stock market growth displaces the short-run relief—the short-living drop in recession risk under a real estate boom. Instead, under a stock market boom, there is only an increase in the risk of recession in response to bank credit expansion. In addition, in both cases of the '$\mathbf{I}_{i,t}^{Stock} = 1$' and '$\mathbf{I}_{i,t}^{Housing} = 1$' regimes, the recession risks materialize sooner: in 6 quarters following an initial credit expansion, whereas under normal asset prices growth, this appears only in 12 quarters. This means that a recession arrives faster if a credit boom is accompanied by a collateral price boom.

Household credit. The estimates of equation (5) with a household credit growth variable are reported in Figure 6. As before, we split all observations into '$\mathbf{I}_{i,t}^{Stock} = 1$' and '$\mathbf{I}_{i,t}^{Stock} = 0$' regimes, Figure 6(a), and then into '$\mathbf{I}_{i,t}^{Housing} = 1$' and '$\mathbf{I}_{i,t}^{Housing} = 0$' regimes, Figure 6($b$).

When we switch from considering the overall variation of bank credit to household credit



(a) Stock market prices

(b) Residential property prices

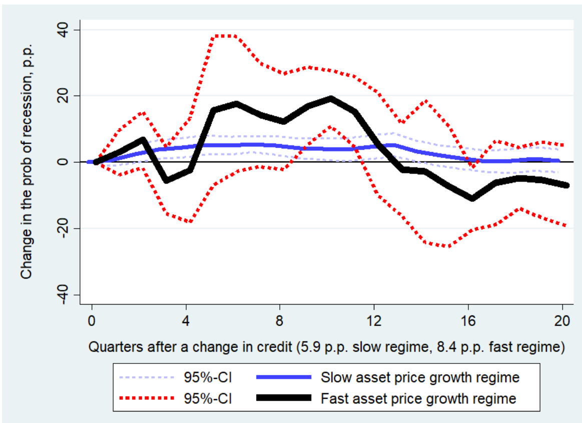
Note: Each subfigure reports a k-step ahead prediction of the probability of a recession in quarters $t+k$, $k = 1...20$, following a one standard deviation change of household credit to GDP growth in quarter t. On both subfigures, we split the sample into the two sub-samples of country-quarter observations: those in which countries experience extremely high growth rates of the domestic stock market (a) or residential property prices (b) (above the 90^{th} percentile across all countries).

Figure 6. Impulse responses of the probability of recession to *household* credit during the periods of *high* and *low asset price growth*, $k = 1...20$ quarters ahead

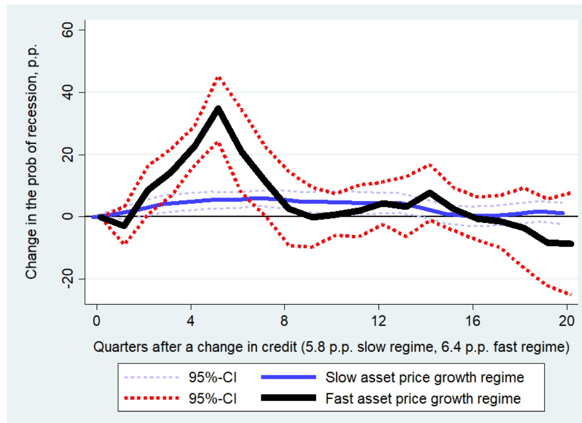
under the '$\mathbf{I}_{i,t}^{Stocks} = 1$' regime, we no longer obtain any significant differences across regimes (Figure 6a). In contrast, when we turn to house prices, Figure 6b), we obtain a much more pronounced boom-bust recession response to household credit growth under the '$\mathbf{I}_{i,t}^{Housing} = 1$' regime as compared to the '$\mathbf{I}_{i,t}^{Housing} = 0$' regime, similar to the result with bank credit growth. When housing prices skyrocket, it reinforces a household credit-driven economic boom: the peak negative magnitude of the recession response is estimated as –50 pp in one quarter following a household credit expansion. In absolute terms, this is roughly ten times larger than, and statistically different from, the respective response under the slow housing prices growth regime. On the medium-run horizon, we also obtain that the recession responses under the two regimes are statistically different, peaking at +12 pp in eight quarters after the initial impulse under the high housing prices growth regime, and at +8 pp in twelve quarters under the slow regime.

Jointly, our findings indicate that the type of collateral is an important determinant of the propagation of a household credit boom. Stock market price growth makes no difference in recession response to household credit. In contrast, housing prices, if growing rapidly, largely amplify both boom and bust compared to the case of slow-growing housing prices.

Firm credit. Differently from household credit, firm credit expansion is amplified under both high asset price growth regimes and is associated with only an increase in recession risk under a collateral price boom (see Figure 7). When stock market prices grow very fast, the response of recession risk to a firm credit expansion is larger and statistically different on the medium-run horizon, compared to the slow stock market growth regime. Recall that in the case of household credit, we obtain no statistical differences under these two regimes. Further, when housing prices soar, the recession response rises high and peaks at +35 pp in five quarters after the initial impulse.



(a) Stock market prices



(b) Residential property prices

Note: Each subfigure reports a k-step ahead prediction of the probability of a recession in quarters $t+k$, $k = 1...20$, following a one standard deviation change of firm credit to GDP growth in quarter t. On both subfigures, we split the sample into the two sub-samples of country-quarter observations: those in which countries experience extremely high growth rates of the domestic stock market (a) or residential property prices (b) (above the 90^{th} percentile across all countries). Thus defined, high asset price growth observations constitute nearly 10% of the total sample.

Figure 7. Impulse responses of the probability of recession to *firm* credit during the periods of *high* and *low asset price growth*, $k = 1...20$ quarters ahead

For both asset price growth variables, the recession response to firm credit exhibits no boom-bust feature, which contrasts with the case of household credit expansion. The increased recession risk in response to firm credit under a high asset price growth regime can be explained by reduced incentives for banks to acquire information about corporate borrowers in this regime. When asset prices are growing fast, a larger fraction of borrowers looks better than they are—a mechanism highlighted in Kantorovitch (2021) and Asriyan et al. (2022)—which may trigger credit misallocation and thus explain increased recession risk. In addition, the more volatile value of the collateral provided by firms—as compared to household lending, which is mostly collateralized by real estate—may further rationalize different recession responses to firm credit as compared to household credit.[30]

1.4.4 Which country group drives the result: Advanced or open and emerging economies?

We now ask how our main result of the boom-bust recession response to bank credit unfolds if projected to two groups of countries—advanced and open and emerging economies (EMEs).

Recent studies on open and emerging economies have established a much larger potential for bank credit in these countries to generate more pronounced boom-bust fluctuations in real activity and financial risks. Farboodi and Kondor (2022) provide a theoretical explanation of why global shocks affect EMEs disproportionately more than advanced economies. Using a quantitative model featuring heterogeneous boom-bust cycles, they find that a global credit crunch shock leads to a deeper recession in periphery countries, as compared to advanced economies. Further, di Giovanni et al. (2021) find that the global financial cycle fuels domestic lending in Turkey, a large emerging economy. Similarly, Davis et al. (2016) show that credit expansions fueled by foreign capital inflow are more harmful in terms of generating financial instability risks. The authors make a distinction between the Nordic European countries and the peripheral European economies: the latter encountered foreign-financed credit booms that ended up in financial crises, whereas the former did not experience neither foreign capital surges nor financial instability around the 2007-2009 global financial crisis. Based on these findings, we can expect that the boom-bust response of recession risk to credit is more pronounced in EMEs as compared to advanced economies.

We divide the sample of 25 countries into two groups: 15 "advanced economies" and 10 "open and emerging economies". Our composition of groups is driven by two motives: first, we want to create country groups comparable in size and, second, we split countries according to their foreign shocks' exposure.[31] We include in the "advanced" group the large and most developed economies in our sample, and we sort highly-open small advanced countries or/and emerging economies into the "open and emerging" group.[32]

[30] As Lian and Ma (2021) and Camara and Sangiacomo (2022) document, 80-85 percent of firm loans are based on cash flows in both advanced and emerging economies.

[31] Our groups of countries correspond to those in Davis et al. (2016) who consider Nordic European countries and the peripheral European economies. Thus, our results may also reflect the greater exposure of open and emerging economies to foreign capital inflows.

[32] Thus defined advanced economies are: Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Japan, Netherlands, New Zealand, Sweden, Switzerland, United Kingdom, and the United States. Open and emerging economies, in turn, include: (highly open and/or post-socialist European economies) Czech Republic, Hungary; (PIIGS) Portugal, Italy, Ireland, Greece, Spain; (emerging) Mexico, Russia, and South Africa.

With this division of countries, we run the state-dependent Jorda LP equation (5) and report the estimation results in Figure 8. As can be inferred from subfigure (a), the recession risk response to bank credit growth in the EMEs group is statistically indistinguishable from this in the advanced group on the short horizon: both responses are negative and at least marginally significant. However, on the medium-run horizon (between three and four years after an initial credit expansion), recession risk rises in both groups, but in the EMEs group the spike is substantially larger, peaking at +22 pp In contrast, in the group of advanced countries, the peak, while reached at roughly the same time, remains well below 10 pp and is statistically different from that in the EMEs group. This result is in line with our expectations and with the theoretical result of Farboodi and Kondor (2022).

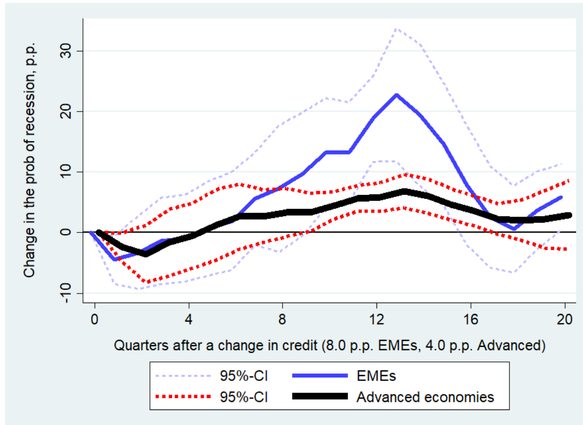
Substituting aggregate bank credit growth with household and firm credit growth, we further obtain two interesting findings. First is that the overall boom-bust pattern of recession response to bank credit is fully driven by credit to households in advanced economies, see Figure 8(b). There is no boom-bust recession response in emerging economies to either household or firm credit. However, the medium-term positive recession response to household credit is much higher in emerging economies as compared to advanced, which could be explained by the higher fraction of non-collateralized household credit in EMEs, lower penetration of banking services, and as a result, less information accumulated on the creditworthiness of borrowers. Second, in both advanced and EMEs, the recession risk only increases in response to firm credit. There are no statistical differences in the responses of recession risk across the two groups of countries.

Overall, the time structure of recession response to household credit is statistically different in advanced and open and emerging economies with a boom-bust shape of recession response present in advanced countries only, while the same response to firm credit is similar in both groups of countries.

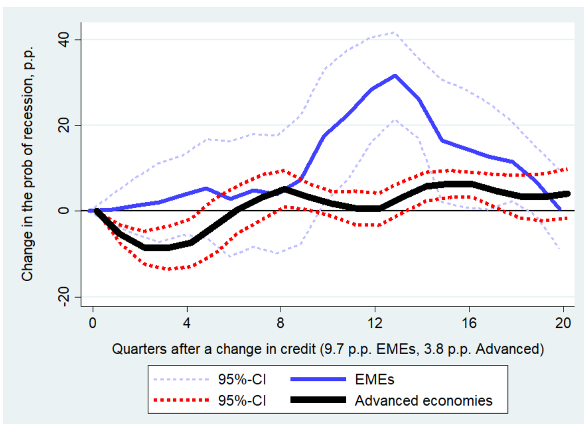
1.4.5 Why does firm credit expansion only increases the recession risk? The role of aggregate productivity

In the previous sections we have shown that a firm credit expansion is followed by an increase in recession risk both in the short-run and on the medium-run horizon. Moreover, we find that this increased recession risk in response to firm credit is amplified under collateral booms. In this subsection, we explore if these findings can be linked to a misallocation of credit, which, if present, may manifest in decreased aggregate productivity.

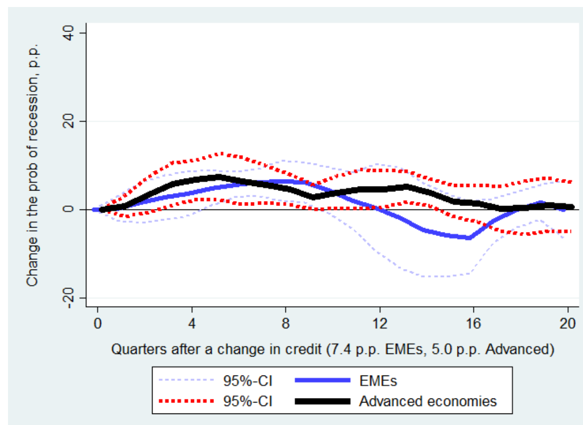
Figure 9 presents estimated responses of the probability of a recession to firm credit expansions



(a) Domestic bank credit



(b) Household credit



(c) Firm credit

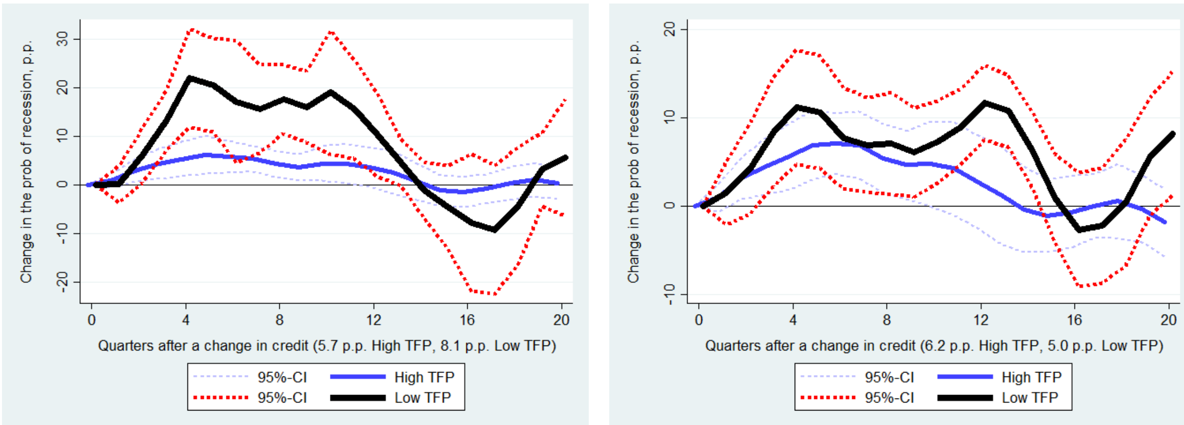
Note: Each subfigure reports a k-step ahead prediction of the probability of a recession in quarters $t+k$, $k = 1...20$, following a one standard deviation change in the credit to GDP growth ratio in quarter t. On each subfigure, we split the sample into the two sub-samples of countries: advanced economies and EMEs. Subfigure (a) checks the overall variation in domestic banks credit while subfigures (b) and (c) do the same for household and firm credit, respectively. Standard errors are clustered on the country and year levels.

Figure 8. Impulse responses of the probability of recession to credit during expansions in advanced economies versus EMEs, $k = 1...20$ quarters ahead

in low and high TFP growth regimes.[33] The estimates are obtained using the state-dependent Jorda LP approach, as implied by equation (5), with the key explanatory variable being the growth rate of *firm* credit to GDP ratio, and the regime variable being an indicator variable that equals 1 if a country is in a low TFP growth regime, and 0 otherwise. We consider two thresholds separating 'low' and 'high' TFP growth regimes—we classify in a distinct "regime" observations with a very low TFP growth, below the 10^{th} percentile (a) or observations with

[33]We obtain country data on TFP growth from the OECD Multifactor productivity database.

a moderately low TFP growth, below the 33^{rd} (b) percentile of observation distribution by TFP growth. In each TFP regime, we estimate responses to a one standard deviation increase in the growth rate of firm credit to GDP ratio.



(a) TFP growth threshold = 10%-tile

(b) TFP growth threshold = 33%-tile

The figure reports a k-step ahead prediction of the probability of a recession in quarter $t+k$, $k = 1...20$, following a one standard deviation increase in the growth rate of firm credit to GDP ratio in quarter t. We split the sample into two sub-samples of country-quarter observations: those in which countries are in a high TFP growth regime and those in a low TFP growth regime, where the threshold dividing 'high' and 'low' is set either at the 10^{th} (a) or 33^{rd} (b) percentile of distribution by TFP growth.

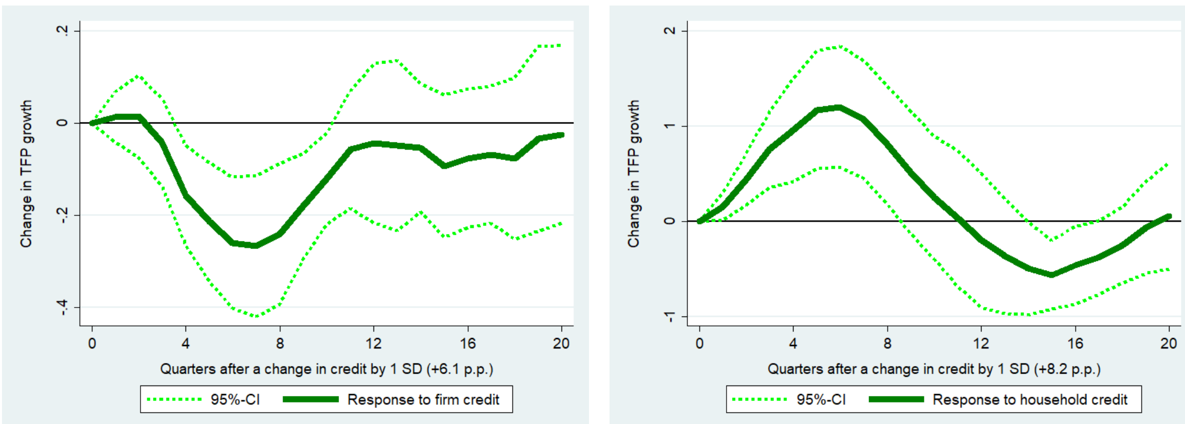
Figure 9. Impulse responses of the probability of recession to *firm* credit expansions in countries in high and low TFP growth regimes, $k = 1...20$ quarters ahead

As can be inferred from Figure 9.a, a one standard deviation increase in the growth rate of firm credit to GDP ratio is associated with a 20 pp rise in the probability of a recession in *very low* TFP growth regime in four quarters and only a 6 pp rise in a high TFP growth regime. The estimated effects are statistically different, and they persist in time until at least the tenth quarter after the firm credit expansion. Further, Figure 9.b confirms these findings qualitatively for a mild TFP growth threshold. Quantitatively, the peak response reaches only 10 pp under the *moderately low* TFP growth in twelve quarters, which is two times lower than under the very low TFP growth but still statistically different from the reaction under the high TFP growth regime. Therefore, we obtain that firm credit expansions are more harmful to the economy if they occur during periods of relatively low total factor productivity growth. We reveal no boom-bust type of response of the economy to a firm credit expansion in either low or high TFP growth regimes. Overall, we find that bank lending to firms under a low TFP growth regime accelerates an accumulation of risks of a recession.

We further test if a firm credit expansion (which we know is associated with an increase in recession risks) is followed by a TFP slowdown. We formally test it by estimating the following Jorda LP equation:

$$\text{TFP}_{i,t+h} = \alpha_i^{(h)} + \beta^{(h)} \cdot \text{Credit}_{i,t} + \mathbf{X'}_{i,t}\Gamma^{(h)} + \text{TFP}'_{i,t}\Psi^{(h)} \tag{6}$$

The $\beta^{(h)}$ coefficient estimates from equation (6) are reported in Figure 10, where subfigure (a) contains the results for the firm credit expansion and in subfigure (b) household credit.



(a) Firm credit expansions

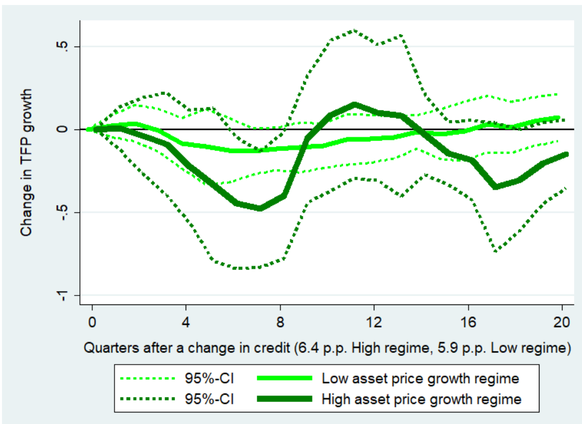
(b) Household credit expansions

Note: The figure reports a k-step ahead prediction of TFP growth in quarter $t + k$, $k = 1...20$, following a one standard deviation increase in the growth rate of firm credit to GDP ratio in quarter t (a) or a one standard deviation increase in the growth rate of household credit to GDP ratio (b). Standard errors are clustered on the country and year levels.

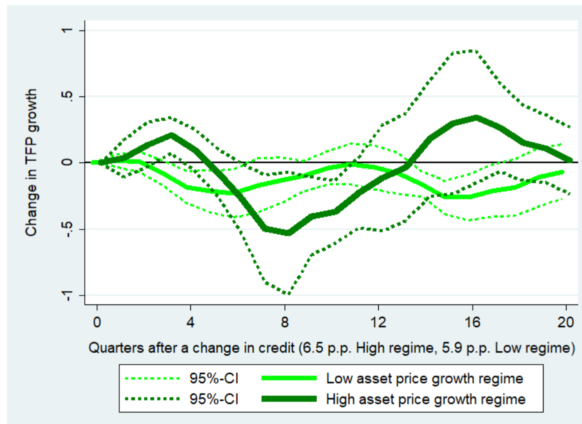
Figure 10. Impulse responses of TFP growth to *firm* and *household* credit expansions, $k = 1...20$ quarters ahead

We obtain that a firm credit expansion predicts only a decrease in future TFP growth, whereas household credit expansions first increase TFP growth and then, as time passes, starts to reduce it (Figure 10). Note that these patterns mirror the baseline results for the risk of a recession. As can be revealed from Figure 10(a), a one standard deviation increase in the growth rate of firm credit to GDP ratio predicts significant declines in TFP growth in four to ten quarters after the initial impulse, and these declines peak at –0.27 pp (roughly corresponds to a one-fifth of the TFP growth one standard deviation). Further, Figure 10(b) shows that a one standard deviation increase in the growth rate of household credit to GDP ratio predicts a significant increase in the TFP growth between the first and eighth quarters after the impulse, which peaks at +1.1 pp and then turns into a fall of TFP growth by 0.5 pp around the sixteenth quarter.

Using state-dependent Jorda LPs, we find that the negative TFP growth reaction is somewhat stronger under the high stock market growth regime—the trough reaction is estimated at –0.5 pp, while under the low growth regime it is only –0.1 pp in seven to eight quarters after the initial credit impulse—though the difference is statistically insignificant (Figure 11.a). Similar deeper negative TFP growth reaction on the medium-run horizon is observed under high housing price growth but the differences are again statistically indistinguishable, Figure 11.b.



(a) Stock market prices



(b) Housing prices

Note: Each subfigure reports a k-step ahead prediction of TFP growth in the economy in quarters $t+k$, $k = 1...20$, following a one standard deviation increase in the growth rate of firm credit to GDP ratio in quarter t. On both subfigures, we split the sample into the two sub-samples of country-quarter observations: those in which countries experience high growth rates of the domestic stock market (a) or residential property prices (b) (above the 67^{th} percentile across all countries).

Figure 11. Impulse responses of TFP growth to *firm* credit expansions under high versus low asset price growth regime, $k = 1...20$ quarters ahead

The revealed negative link between firm credit expansions and future declines in TFP growth is puzzling. Is it always the case that banks expand credit to less productive firms, thus facilitating inefficiency in the economy? Our historical decomposition approach can shed light on this by again exploring which particular business cycle shock drives the credit. We estimate the following equation for this purpose:

$$\mathrm{TFP}_{i,t+h} = \alpha_i^{(h)} + \beta^{(h)} \cdot \widehat{\mathrm{Credit}}_{i,t}^{(j)} + \mathbf{X'}_{i,t}\Gamma^{(h)} + \mathrm{TFP}'_{i,t}\Psi^{(h)} \tag{7}$$

The estimation results appear in Figure 12. As can be inferred from the figure, we obtain that the overall negative reaction of TFP growth on firm credit expansions established above can be explained by expansionary credit supply (CS) and to a lesser extent monetary (MP) shocks driving firm credit, but not by aggregate supply (AS) or aggregate demand (AD) shocks in the economy. Specifically, our estimates suggest that following a one standard deviation increase in the growth rate of *CS*-driven firm credit to GDP ratio, TFP growth first tends to rise during the first two quarters, though insignificantly, then it turns to decline steadily through the quarters, reaching its trough at –0.13 pp in ten quarters after the initial impulse (significant at least at 5%).

When it comes to *AS*-driven firm credit expansions, we obtain only positive effects on TFP growth in the economy, reaching their peaks at +0.2 in four quarters after the initial impulse. And for *AD*-driven firm credit expansions we obtain no significant responses of TFP growth either in the short run or in the medium run.

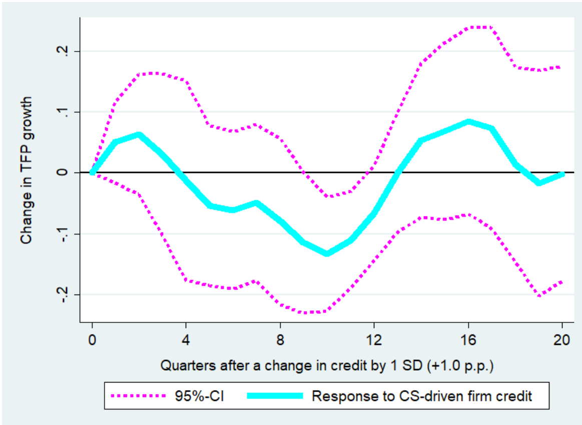
Overall, our estimates imply that positive credit supply shocks are followed by declined TFP growth in the economy. Firm credit expansions facilitate TFP growth if they are driven by technological advancements in the economy.

1.5 Sensitivity

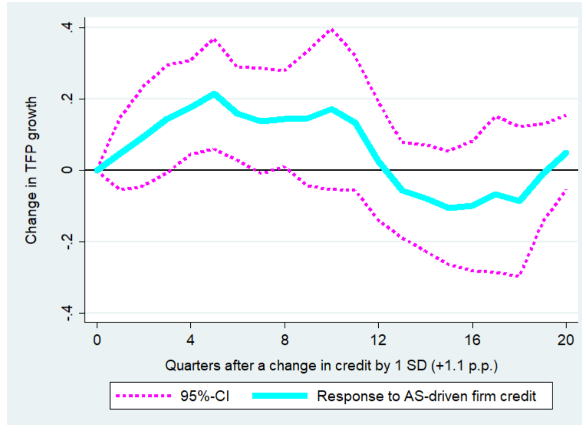
Measures of bank credit and choice of short-run lags of credit. As we discuss above, we have at least five options on how to measure the intensity of bank credit growth: the log annual growth of domestic bank credit to GDP ratio is the baseline measure, while the log annual growth of real loans and the deviation of the log of loans to GDP ratio from its HP trend, with smoothing parameters $\lambda = (1,600; 26,000; 400,000)$, are additional options. We re-run the dynamic logit model (2) with each of the five options, see Appendix no. 11. We still find strong support for the boom-bust type of response: a negative and highly significant coefficient on the first quarter lag of any of the five options to proxy bank credit and a positive and also highly significant sum of the coefficients on lags 4,8...20 quarters. In terms of the associated economic effects, we also obtain a similar picture, with the deviations of credit from the HP trend computed using $\lambda = 26,000$ delivering the largest effects, and our baseline measure producing the second largest effects.

When it comes to choosing a particular short-run lag of the credit growth variable (first, second, third, or fourth) for estimating the dynamic logit regressions, we find that exactly the first lag outperforms the other options, though in all cases we obtain negative coefficients. See Appendix no. 12 for more details.

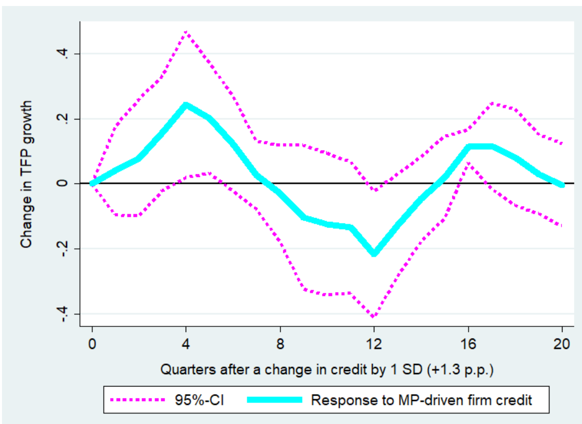
Distribution of AD shocks. We deal with the uncertainty around the estimated aggregate demand (AD) shock. In the baseline case, we use the median draw of AD shock. To explore the sensitivity of our results we re-run our second stage model, as implied by equation (4), using other percentiles of the AD shock distribution—the 16*th* (i.e., a rather small shock)



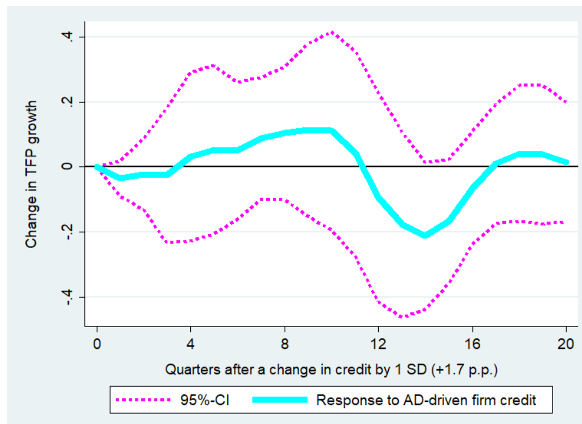
(a) Credit supply (CS) shock



(b) Aggregate supply (AS) shock



(c) Monetary policy (MP) shock



(d) Aggregate demand (AD) shock

Note: The figure reports Jorda LP estimation results, as implied by the sequences of coefficients $\beta^{(h)}$ for $h = 1, 2, ..., 20$ quarters after a firm credit expansion, see equation (7). On each of the four subfigures, we show responses of TFP growth to a one standard deviation increase in respective $\widehat{Credit}_{i,t}^{(j)}$ $(j = 1, 2, 3, 4)$. Standard errors are clustered on country and year levels.

Figure 12. Impulse responses of TFP growth to *firm* credit expansions driven by various business cycle shocks, $k = 1...20$ quarters ahead

and 84*th* (i.e., rather a large shock). In all cases, the boom-bust response of recession risk to credit expansions preserves. Moreover, with the 84*th* percentile of the AD shock, it becomes even more pronounced. See Appendix no. 13 for more details.

Choice of thresholds for high asset prices growth regime. We repeat our regression estimations of equation (4) using the 67*th* instead of the 90*th* percentiles of the distribution of countries by either stock or housing market prices growth, as is done in Greenwood et al. (2022). We predictably obtain less pronounced patterns of the boom-bust responses of recession risk to credit expansions, but they are still there, see Appendix no. 14.

Other issues. Other robustness checks that we perform include (*i*) adding the 12*th* quarter lag of all right-hand side variables of the second stage regression (4) that we estimate under the Jorda (2005) LP approach. This is a more strict test of the non-monotonicity result than the 4*th* and 8*th* quarter lags, given that in the baseline result, we show that the 12*th* lag possesses the largest effect. In addition, the stricter test is in line with Baron et al. (2020)'s regressions with three-year lags of all right-hand side variables. (*ii*) We also squeeze the number of lags to just the 4*th* lag when performing the Jorda (2005) LP approach. (*iii*) In our baseline dynamic logit regression (2) we include not just 1 and 4,8...20 quarter lags of the bank credit growth variable but all quarters 1,2...20. (*iv*) When estimating SVAR models at the country level, we address an issue of relatively short time series, especially for developing countries in our sample, by turning from flat to Minnesota priors for the distribution of unknown coefficients. In all these exercises the relationship between bank credit growth and the probability of recessions preserves, as does the AD-driven nature of this relationship.

2 Credit Supply Shocks and Household Defaults

2.1 Introduction

Empirical evidence from the Great Recession in the U.S. economy suggests that disruptions of the mortgage market (house prices collapse, household defaults, debt restructuring, foreclosures) may have substantial macroeconomic consequences, including a large fall in consumption and employment, which further deepen the economic crisis (Mian and Sufi, 2009, 2014). But what leads to failures of the mortgage market? Recent research has introduced a new insight into the origins of crises and, in particular, shows that they may be caused not only by negative coincident shocks but also by positive shocks in the past that led to an accumulation of economic imbalances.[34] We follow these ideas and investigate whether *positive* credit supply (CS) shocks that occurred in the past lead to higher default rates on loans at the household level in the present. The question is important because it may unveil an understudied link between a credit boom, associated with credit market easing, to a subsequent bust, characterized by a tightening of credit conditions due to the increased risk of borrowers defaulting.

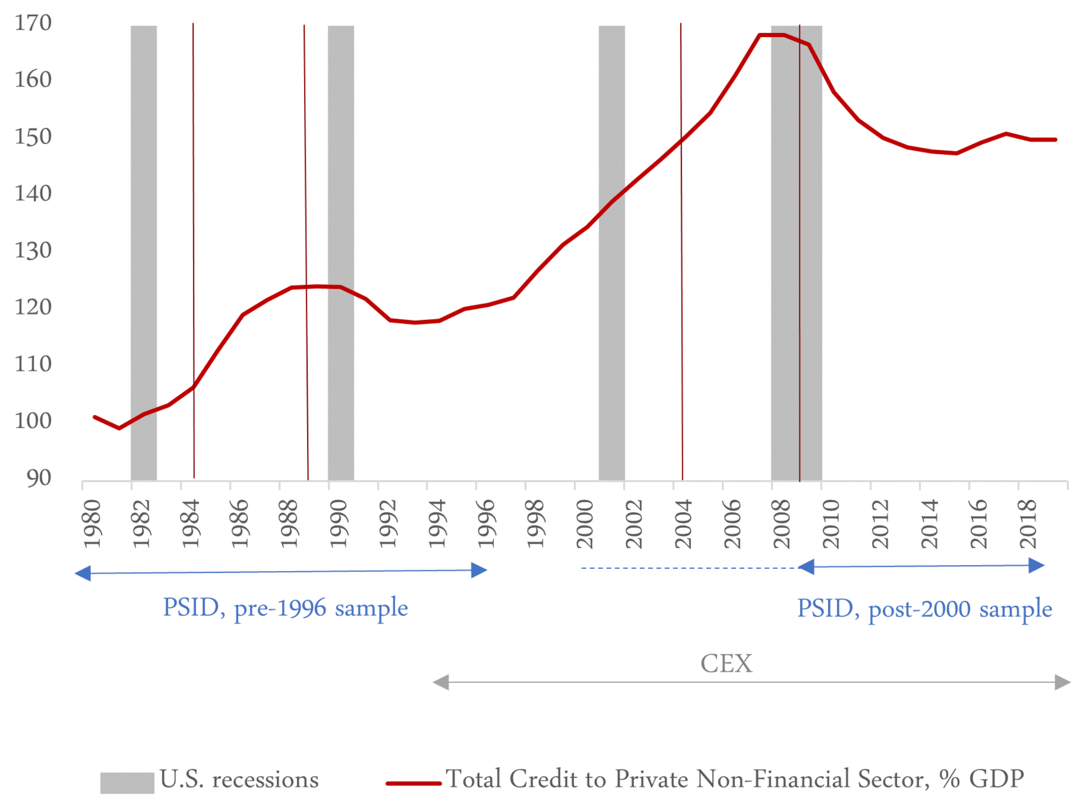
We empirically explore the channels that may rationalize the existence of the effects of CS shocks on household defaults and study time variation of the effects. Our empirical design is based on the differential treatment of U.S. states by CS shocks. In this way, our main source of identification is the variation in CS shock intensities *across* U.S. states.

In the first part of our analysis, we estimate CS shocks at the level of U.S. states using a structural VAR model. In the second part, we rely on a quasi-experimental design and estimate differences in outcomes of households residing in different states before and after CS shock treatments. In particular, we identify in which years, countrywide, or *systemic*, positive CS shocks occurred and split all states in these years into two groups: above and below the median, according to the size of the CS shock in a particular year (treatment and control groups of states). In the second part of the analysis, we trace the effects of the "treatments" on household-level outcomes in a difference-in-differences setting. In this analysis, we focus on systemic credit easing in 1984 and 2004 based on a careful analysis of the distribution of CS shocks across states in different years.

Let us now explain the timing of the analysis. In the difference-in-differences setting, we

[34] The accumulation channel is highlighted in the endogenous business cycles theory by Beaudry et al. (2020). In addition, Lopez-Salido et al. (2017) put forward predictable mean-reversion on the credit market. Finally, Schularick and Taylor (2012) and Mian et al. (2017) show that excessive credit growth predicts future financial/economic crises.

focus on the sub-samples of the 1980s and 2000s while we have to exclude the 1990s for the following reasons. First, the 1980s and 2000s contain clear turning points of the credit cycle, whereas the 1990s witnessed only an expansionary phase, Figure 13). Second, we do not have continuous micro-level data on household defaults covering the 1990s that is suitable for a difference-in-differences analysis: the PSID data on defaults was discontinued in 1996 while the CEX data on mortgage delinquencies is available from 1994.



Note: This graph shows the evolution of the private credit to GDP ratio and highlights the time periods for which the PSID and CEX provide micro-data on household defaults. Grey bars represent the years of U.S. recessions according to NBER dates. Red bars denote the years of systemic positive and negative credit supply shocks, i.e., those in which positive or negative shocks hit most of the states (1984, 1989, 2004, 2009). Further, in our difference-in-difference analysis, we use positive shocks. Detailed data on the fraction of states with positive and negative shocks by years is presented in Figure 2 in the Appendix. Detailed information on PSID and CEX micro-data on household defaults is provided in Section 2.3.2. Data on the total credit to GDP ratio is from the FRED Economic Data portal of St.Louis Fed (source: BIS).

Figure 13. U.S. credit cycles, micro-data availability, and the dates of U.S. recessions

Our main results can be summarized as follows. First, using a panel VAR model with sign restrictions, we construct time series on the CS shocks for the U.S. states from the late 1970s to the late 2010s. We use thus obtained state-year variation of credit supply shocks to identify the periods for which (i) the majority of the states experienced positive CS shocks and (ii) the

PSID or CEX provide data on either household defaults or mortgage delinquencies. These are 1984 and 2004, which were the years of systemic *positive* CS shocks. Both years correspond to well-documented credit expansion episodes. Interestingly, our measure correctly captures at least two endpoints of the credit cycle, years of 1989 and 2009, which brought *negative* CS shocks across almost all states (see Figure 13). Importantly, we document that our SVAR-based measure of CS shocks is significantly and negatively related to the excess bond premium (EBP), a countrywide indicator of borrowers' credit quality (Gilchrist and Zakrajsek, 2012). Second, by employing the state-level CS shocks in a difference-in-differences framework, we trace the effects of each of the two country-wide CS shocks on subsequent paths of nine household-level outcomes and show that positive CS shock effects vary across decades.

Specifically, we find that the positive CS shocks in 1984 had no effects on household default rates. We draw this conclusion based on our finding that there were no relative rises in household defaults after 1984 in states that were more exposed to the shocks than the other states. Conversely, we find evidence that the positive CS shocks in 2004 relatively increased, not decreased, households' mortgage delinquencies in the more exposed states; the implied economic effect is +0.03 points over 2006–2009, which is large since it exceeds the mean delinquency ratio by a factor of 5 and roughly corresponds to three-thirds of the standard deviation of the delinquency ratio computed across CEX cohorts over 1999–2019. This suggests that, on average, positive CS shocks create an overhang of financial risks. Importantly, the 1984 episode had no such effects because, as our estimates indicate, more positive CS shocks occurred in that year in the states that were less financially developed, and these states merely caught up with more developed states; the 1984 shock also stimulated total employment and led to a rise in total household income and mortgages, while the ratio of mortgages to total income remained stable. Also important, we demonstrate that if we switch from our SVAR-based measure of CS shock to the binary indicator of early deregulated states employed by Mian et al. (2020) (1 if a state deregulated before 1983, 0 if after), we obtain the same results. In contrast to 1984, greater exposures to the 2004 positive CS shocks did not cause greater expansion of total income but did lead to further rises of mortgage-to-income ratios; these two findings rationalize why positive CS shocks may result in higher mortgage delinquencies. Our results also indicate that during the 2000s, two channels were in play: the household demand channel (Mian et al., 2020) and the expectations channel (Kaplan et al., 2020). Our study thus shows that the two channels are not necessarily exclusive, which reconciles the debate between Mian et al. (2020) and Kaplan et al. (2020).

We show that our baseline results survive when we (i) choose different approaches to identify CS shocks, (ii) switch from the difference-in-differences approach to the Jorda (2005) local projection method, (iii) aggregate the household-level data to the state level, and (iv) consider alternative measures of the quality of household mortgage debts.

Our study is related to several streams of the literature. *First*, previous research has shown that the "deleveraging shock", negative CS shock in our terminology, leads to decreased consumption and generates recession (Eggertsson and Krugman, 2012; Guerrieri and Lorenzoni, 2017). In contrast, we consider positive CS shocks and study their effects at the household level.[35]

Second, as we noted above, various authors have concluded that excessive growth of credit and household debt predicts financial instability and output reversal (Schularick and Taylor, 2012; Mian and Sufi, 2010; Mian et al., 2017). Based on their evidence, we put forward a hypothesis that one transmission link from credit growth to financial instability could be a rise in defaults on credit, our main variable of interest.

Third, there are several papers studying the causal effects of bank credit supply and bankruptcy protection on household outcomes using quasi-experimental design (difference-in-differences analysis, Jensen and Johannesen, 2017; Damar et al., 2020; Auclert et al., 2019). We employ this empirical setting in the second part of our analysis.

Fourth, several theoretical works rationalize household decisions to default using quantitative models (Chatterjee et al., 2007; Livshits et al., 2010, Mitman, 2016; Antunes et al., 2020). These models show that household decisions to default are affected by state heterogeneity in bankruptcy protection, credit market innovations and credit availability, level of household indebtedness, and income shocks. We capture state heterogeneity by including states' fixed effects in our econometric model; credit market innovations and changes in credit availability are encompassed by our CS shocks, and household debt and income levels are among the household outcomes that we appeal to when studying the transmission channels of CS shocks' on household defaults.

Finally, there is an active academic discussion about the sources of housing booms and busts in the 2000s. On one side, empirical studies by Mian and Sufi (2009), Favara and Imbs (2015), and Mian et al. (2020) show that shifts in credit supply affected house prices. In contrast, Kaplan et al. (2020), using a structural equilibrium approach, argue that shifts in beliefs are the main driver explaining housing booms and busts, thus contradicting the "credit supply view" established in the previous literature (Mian and Sufi, 2017). We relate our findings to this discussion.

[35] Several empirical works explore the macroeconomic implications of the credit market reforms of the 1980s (Jayaratne and Strahan, 1996; Beck et al., 2010; Ludwig et al., 2020; Mian et al., 2020). We also draw on the data on state heterogeneity in the timing of bank deregulation as a substitute for our estimated intensities of positive CS shocks in the early 1980s and show that our results survive this cross-validation.

We contribute to the literature along three dimensions. First, our paper is a pioneering study on whether there is a *causal* link between exogenous changes in credit conditions and subsequent household defaults on loans. Previously, when investigating a similar question, the literature has been silent about the causal interpretation of this link.[36] Second, we introduce CS shocks as an exogenous credit market "treatment". Our shocks, by construction, have time variation. The existing studies are tied to a particular timing of a reform (banking deregulation of the 1980s in Jayaratne and Strahan, 1996; Beck et al., 2010; Mian et al., 2020) or a 2008 financial shock (Chodorow-Reich, 2014; Jensen and Johannesen, 2017; Damar et al., 2020). In contrast, our estimated shocks give us an opportunity to study broader settings than in the literature and estimate time variation of effects on different subsamples while the external validity of existing studies is more narrow.

The remainder of the chapter is structured as follows. In Section 2, we present the methodology of the structural VAR models that we apply to identify credit supply shocks at the level of U.S. states. We then describe the FDIC data we use for this purpose and analyze corresponding estimation results. In Section 3, we outline the methodology of the difference-in-differences analysis linking our state-level credit supply shocks and outcome variables at the household level; we also describe the PSID data and report the baseline estimation results here. We discuss the sensitivity of our results in Section 4. Section 5 concludes the paper.

2.2 Identification of credit supply shocks

2.2.1 Structural vector autoregression model for the identification of the state-level credit supply shocks

To identify credit supply shocks at the level of U.S. states, we specify a 5-variables VAR model which includes the following variables: real GDP, CPI inflation, risk-free interest rate, interest rate on loans, and the outstanding amounts of loans in state s at time t:

$$A\left(L\right) y_{s,t} = u_{s,t} \tag{8}$$

where s is a U.S. state (s = 1...51) and t is year (t = 1977...2017); $y_{s,t}$ is a 5×1 vector of endogenous variables; $A\left(L\right)$ is the lag structure of the VAR model (L is the deepest time lag) and $u_{s,t}$ is a 5×1 vector of a (non-orthogonal) regression error in the respective equation

[36] For example, Mian and Sufi (2009) show that following a more rapid mortgage credit expansion in subprime ZIP codes relative to prime areas, these ZIP codes experienced a relatively sharper increase in default rates. In their other study, Mian and Sufi (2010) show that default rates on household debt grew faster in counties that witnessed larger increases in debt-to-income ratio.

of the system, with $u \sim N(0, \Sigma)$ (assumed).

In the choice of variables and identification of shocks, we closely follow Gambetti and Musso (2017); however, in contrast to them, we employ a constant-coefficients VAR model. We do not consider time variation in coefficients of the VAR model because of the data limitations: we use annual data and therefore do not have enough observations to estimate the time variation. We use annual data, first, because there is no quarterly data on U.S. state-level banking variables at the FDIC Historical Bank Data page (see state-level data description in section 2.2.2 below) and second, because we have micro-data on household defaults of annual frequency from the PSID database (see micro-level data description in section 2.3.2). Given these data limitations, it should be kept in mind that our modelling approach does not capture potential changes of policy rules or macroeconomic linkages due to, e.g. the Great Moderation or Great Recession. Despite these limitations, we believe that we are able to capture time variation in our shock of interest—credit supply shock. This claim relies on the assumption of constant sensitivity of credit market variables to macroeconomic conditions during the period analyzed.

In contrast to Gambetti and Musso (2017), and similarly to Hristov et al. (2012) and Eickmeier and Ng (2015), we specify our VAR model in levels instead of growth rates of non-stationary variables (such as output, prices, and loans). Gambetti and Musso (2017), as well as other studies on time-varying parameters VARs (TVP-VARs, see, for instance, Primiceri, 2005, Gali and Gambetti, 2015) specify VAR models in growth rates of variables. This particular choice of variables' transformation seems to be specific to the TVP estimation procedure. In contrast, in constant-coefficient VAR models, the model is specified in levels of variables. Because we do not aim to estimate time variation in model parameters, we are free to specify the model in a standard way—in levels.

We use a sign restrictions approach to identify credit supply shocks. This approach is widely used in the literature on aggregate shocks and was previously employed in Helbling et al. (2011), Hristov et al. (2012), Eickmeier and Ng (2015), and Gambetti and Musso (2017).[37] In a comparative Monte-Carlo experiment, Mumtaz et al. (2018) show that a sign restrictions approach is superior in recovering DSGE model-based credit supply shocks compared to other shock identification schemes. We impose, among others, the following sign restrictions on the responses of variables: once a positive credit supply shock hits, the lending rate decreases

[37] An alternative approach to isolating credit supply shocks could be relying on differences between financial institutions in exposures to financial crises: varying levels of exposure to losses from mortgage-backed securities during 2007-08 of banks operating in a syndicated loan market in Chodorow-Reich (2014), varying exposures of Canadian banks to the U.S. interbank market in Damar et al. (2020), and differences in the stability of the funding base of Danish banks at the onset of the financial crisis in Jensen and Johannesen (2017).

and loan volume goes up, i.e. these two variables move in opposite directions. This implies an outward shift in the supply of credit along the demand curve. Given that we impose the restrictions on the *residuals* of the VAR, $u_{s,t}$, we interpret the shocks identified as shocks to banks' capacities to lend unrelated to borrowers' fundamentals. The latter stems from the fact that we control for economic activity indicators in the VAR equations, i.e., we remove the component related to the borrowers' risk of default from the VAR residuals. There could be various underlying factors causing credit supply shifts. Among these there are changes in bank funding abilities (due to unexpected losses of assets, capital, or liquidity shortages), changes in bank regulation (imposition and removal of bans on certain operations, changes in regulatory capital requirements, accounting standards), unexpected changes in banks' perception of risk, and deviations of fundamentals from banks' expectations.

Following Gambetti and Musso (2017) and Hristov et al. (2012), we identify four structural shocks. Identifying 4 shocks in the system of 5 equations effectively means that the 5^{th} shock remains unidentified, thus capturing all other possible shocks. In addition to the credit supply shock, we identify aggregate supply, aggregate demand, and monetary policy shocks (see Table 1). We do so because the literature suggests that simultaneous identification of several shocks improves identification of the shock of interest (Paustian, 2007). First, by imposing a particular set of sign restrictions, we make shocks mutually exclusive. Second, by identifying other important macroeconomic shocks together with credit supply shocks, we ensure that credit supply shocks play as an exogenous force, not as an endogenous response to any other shocks.

Table 3. Sign restrictions on shocks

Aggregate shock	Real GDP	Inflation	Short-term interest rate	Lending rate	Loans
Aggregate supply (AS)	+	–	No restriction	No restriction	No restriction
Aggregate demand (AD)	+	+	+	+	No restriction
Monetary policy (MP)	+	+	–	No restriction	No restriction
Credit Supply (CS)	+	+	+	–	+

Note: All restrictions are imposed on the impulse responses on impact of all variables. Red color denotes restrictions on the responses to credit supply shock which are *not* imposed in the Eickmeier and Ng (2015) identification scheme in the sensitivity analysis in the Appendix.

Justification of sign restrictions imposed on credit supply shock comes from the responses of macroeconomic variables to these shocks in several DSGE models with various financial frictions, (Christiano et al., 2014, Curdia and Woodford, 2010, and Gertler and Karadi, 2011); see Gambetti and Musso (2017) for the discussion of these models and their implications for credit supply shock identification. In the baseline identification of credit supply shock, we follow Gambetti and Musso (2017) and restrict the interest rate on loans to decrease, volume of credit to rise, GDP and CPI inflation also to rise (through increased consumer and investment spending and inflationary pressure), and for the short-term interest rate to rise (monetary tightening to circumvent inflation). Eickmeier and Ng (2015) note that some theoretical models produce conflicting results on the responses of inflation and the short-term interest rate to credit supply shock; therefore, they do not restrict the responses of these variables. In the sensitivity analysis in the Appendix we apply this identification scheme and remove restrictions on these variables (denoted red in Table 1).

Given that sign restrictions are usually implemented in a Bayesian framework, we consider a Bayesian VAR model. We use two types of prior on VAR coefficients: first, as a baseline, we use a standard Minnesota prior combined with the sum-of-coefficients and the dummy-initial-observation priors. We use "rule-of-thumb" hyperparameters values of the informative priors: we set the overall tightness of Minnesota prior at 0.2, and lag decay at 1; we set the parameters of the sum-of-coefficients and the dummy-initial-observation priors at a value of 1; all parameters are same as recommended in Sims and Zha (1998). Second, in the sensitivity analysis in the Appendix, we use flat, or uninformative prior in which the posterior is centered around least squares estimates of VAR coefficients. In both cases, we set lag order $p = 2$ and perform 5000 draws from the posterior.

We estimate the VAR model in equation (8) on panel data of U.S. states. A similar approach is employed in Hristov et al. (2012) who pool 11 Euro area countries into a panel dataset to estimate credit supply shocks. In the panel approach, we assume common dynamic relationships across states and, in this respect, disregard potential differences across them. However, given the short length of the annual data on each state, we gain higher informativeness of the data in the panel approach.

We estimate a structural VAR model identified with sign restrictions using the procedure of Arias et al. (2018) and **?**. Their algorithm enables us to draw from a conjugate uniform-normal-inverse-Wishart posterior of VAR coefficients. In particular, for each of the 5,000 draws from the posterior, the algorithm first applies Cholesky factorization of the residuals, and then rotates this structural transformation of VAR residuals until the appropriate rotation matrix Q is found for which sign restrictions are satisfied.

The estimated impulse response functions to a positive credit supply shock and other identified shocks are presented in Figure 1 in Appendix no. 16. The figure shows median IRFs and the respective 16^{th} and 84^{th} percentiles of the IRFs distribution. In general, we observe positive signs of responses of GDP, CPI inflation, risk-free rate and the volume of loans to a positive credit supply shock and a negative response of the interest rate on loans, all in line with the imposed restrictions. Because we set the restrictions to hold only on impact, we obtain quite wide credible sets of the unrestricted responses of variables to shocks.

From the SVAR analysis, we take the estimated credit supply shocks series $\varepsilon_{s,t}^{CS}$ for each state s in all time periods t for use in the subsequent analysis.

2.2.2 Data for the SVAR analysis: U.S. state-level

The data on GDP for all 51 U.S. states is obtained through the Bureau of Economic Analysis (BEA) website, which is available from 1977 onward.[38] This is the first limitation for our analysis: we effectively have no more than 41 annual points. This also justifies our use of the Bayesian approach which (among other benefits) allows us to deal with the "curse of dimensionality" problem. As for the construction of the GDP index, we set the volume of real GDP in 1997 equal to 100 for each state and recompute the values of the index correspondingly. We note that the regional chapter of the St. Louis Fed provides the GDP data for the U.S.states only from 1997.

Due to the unavailability of data on CPI inflation at the state level, we use data on CPI in four U.S. aggregated regions: Northeast, Midwest, South, and West from the Bureau of Labor Statistic (BLS) website.[39] We thus extrapolate data on these four regions to corresponding U.S. states. The measure is available from at least 1970, which is enough for our analysis.

We retrieve the risk-free interest rate from the St. Louis Fed website at the U.S. aggregated level. In particular, we gather daily data on the "1-Year Treasury Constant Maturity Rate, Percent, Daily, Not Seasonally Adjusted" for the 1970–2018 period. We thus assume that the same risk-free rate could be a relevant benchmark in different U.S. states within the period considered. We use the one-year government bond rate instead of the federal funds rate as a proxy for the short-term interest rate because the former captures a forward guidance component, which is particularly important during the zero-lower bound period included in our sample (Gertler and Karadi, 2015).

Finally, we use Historical Bank Data provided by the Federal Insurance Deposit Corporation (FDIC) to obtain aggregate banking data at the U.S. state level since 1970. Ideally, we would need data on bank loans to households and the interest rate on these loans. However, this data is not available there. Instead, the FDIC provides data on various types of loans at different levels of aggregation and some types of interest income received by banks from lending. There is data on loans to individuals; however, there is no disclosed data on the interest income earned on these loans. We thus use data on the amount of total loans issued

[38] Position "Quantity indexes for real GDP by state: All industry total (Quantity index)".
[39] "CPI for All Urban Consumers (CPI-U)".

in the state ("Total Loans & Leases"). We then use interest income on these loans ("Int Inc[40] - Total Loans & Leases") to construct an effective interest rate by dividing this interest income by the total loans and leases.

We draw the reader's attention to the following notes on the state-level banking data we use. First, as mentioned above, the indicator we use, total loans, includes not only loans issued to households (which consist of loans secured by real estate and unsecured loans to individuals) but also commercial and industrial loans granted to non-financial businesses, farm loans, loans to depository institutions, and loans to governments. At the same time, household loans account for the lion's share of the total loans in the U.S. economy—almost 70%, an average in 1984-2020. We keep in mind though that our state-level credit supply shocks measure the overall attitude of banks toward lending to all sectors of the economy, not only to households. Second, we consider data on commercial banks insured by the FDIC and do not include savings institutions in our analysis because the latter account for about 6% of total lending in the economy (2019 data). Third, an alternative to the banking data we use could be Call reports of commercial banks aggregated at the state level, as in Mian et al. (2020). We compared their state-level data on real estate loans and loans to individuals to ours, which we downloaded directly from the FDIC website and found few or no differences.[41] Similarly to us, Mian et al. (2020) do not include data on savings institutions. We thus conclude that our data on state-level bank lending is reliable and comparable to those used in other papers.

2.2.3 State-level credit supply shocks

We begin our analysis of estimated state-level credit supply shocks by comparing our estimated shocks with the existing measure of credit market tightness. In particular, we compare the evolution of the median state-level credit supply shock to the excess bond premium (EBP) proposed in the influential work of Gilchrist and Zakrajsek (2012). The excess bond premium is constructed as the residual component of the corporate bond credit spread net of the default risk of the borrowers. Gilchrist and Zakrajsek (2012) interpret this indicator as capturing the "risk-bearing capacity of the financial sector" unrelated by construction to borrowers' fundamentals; therefore, it corresponds to changes in the supply of credit.

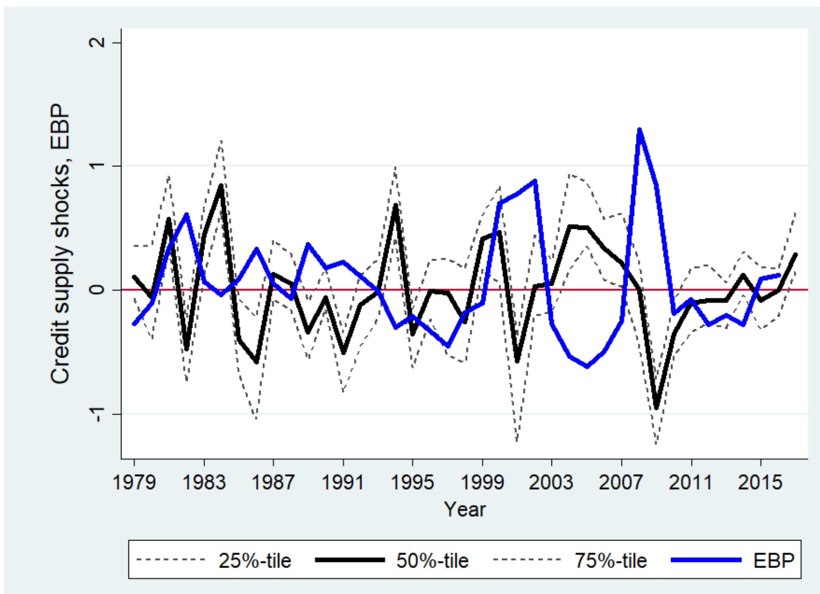
Various credit spreads have been used previously in the literature to identify credit supply shocks. In particular, Eickmeier and Ng (2015) use the spread between the corporate bond and long-term government bond rates in their sign restrictions procedure: following a *negative*

[40] Interest Income.

[41] Replication code and data for the study by Mian et al. (2020) is available online.

credit supply shock, this indicator is restricted not to fall. Mian et al. (2017) perform instrumental variable analysis of the effects of increases in household debt by instrumenting debt with mortgage spread—the difference between the interest rate on mortgage loans and the 10-year government bonds. They show that in the first-stage regression, mortgage spread and household debt are negatively correlated, suggesting that credit supply shocks are the most important driver of changes in household debt.

An analysis of the dynamics of median credit supply shock reveals a substantial negative correlation between our measure of credit supply shocks and the excess bond premium of Gilchrist and Zakrajsek (2012) (see Figure 14). Periods of identified positive credit supply shocks correspond to a decrease in the excess bond premium, thus characterizing an improvement in the credit conditions and vice versa.



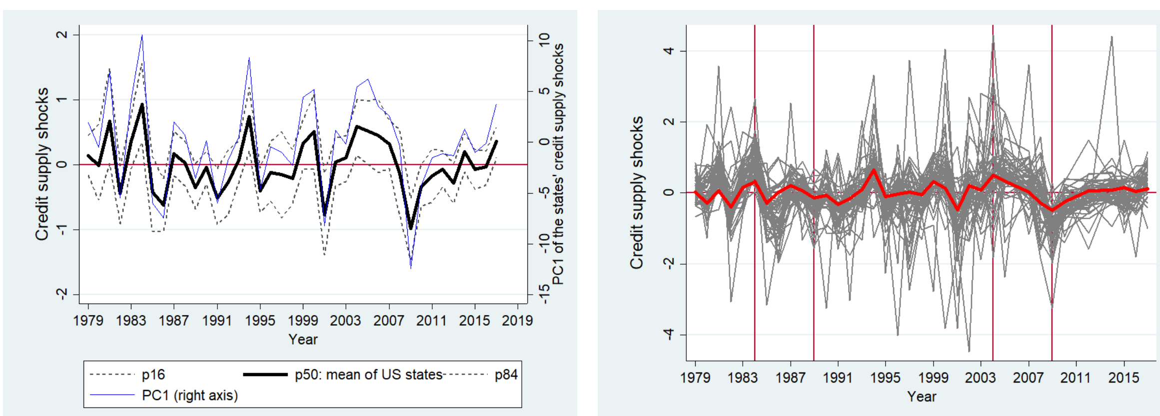
Note: Credit supply shocks are estimated on the panel data of U.S. states with Gambetti and Musso (2017) sign restrictions and Minnesota prior

Figure 14. State-level credit supply shock (median across states, 75th and 25th percentiles) and Gilchrist and Zakrajsek (2012)'s excess bond premium (EBP)

An analysis of time evolution of the median credit supply shock yields several observations. First, negative credit supply shocks tend to appear around either financial crises or recession periods (or both)—1986, 1989, 1991 (the savings and loans crisis and 1991 recession), 2001 (dot-com crisis and 9/11 terrorist attack) and 2009 (the Great Recession). Second, sizable positive credit supply shocks appear in the beginning of the 1980s (financial deregulation), in the first half of 1994 (end of early-1990s recession), in 1999-2000 (dot-com bubble), and around 2004-2006 (before the Great Recession).

Based on our analysis, we conclude that the median tendency of our state-level credit supply shocks first, has an economic interpretation and second is comparable with the established measure of credit market tightness—the excess bond premium.

Further, we analyze comovement across states and state heterogeneity in credit supply shocks. First, we perform a principal component analysis of state-level credit supply shocks. We find that the first principal component of state-level shocks explains 46%—almost half—of the total variation. This suggests the existence of a strong common force that corresponds to an aggregate country-level credit supply shock. Indeed, an extraction of credit supply shocks based on the aggregate U.S. data (using the same SVAR specification with the same prior on coefficients) yields an estimate of aggregate shock evolving close to the first principal component of state-level shocks (see Figure 15a). Second, we analyze state heterogeneity in the size and direction of the credit supply shocks. We note that on top of strong comovement of the shocks described above, there are substantial differences in the size and signs of shocks (see Figure 15b). This suggests the viability of our identification strategy: we rely on the differences in intensity of credit supply shocks across U.S. states and compare outcomes in households residing in states with different availability of credit.



(a) Comovement between states

(b) Heterogeneity across states

Note: Credit supply shocks are estimated on the panel data of U.S. states with Gambetti and Musso (2017) sign restrictions and Minnesota prior. Red bars denote 1984, 1989, 2004, and 2009.

Figure 15. A common component of U.S. state-level credit supply shocks and state heterogeneity in the size of shocks

2.3 The effects of credit supply shocks on household outcomes: difference-in-differences analysis

2.3.1 Methods and empirical strategy

In the first part of our micro-level analysis, we rely on the quasi-experimental design and apply difference-in-differences. Specifically, we compare outcomes of households residing in the states more strongly hit by credit supply shocks ("treated" states) with those of households residing in less affected states ("control" states), before and after a shock.

In designing the empirical estimation we follow previous literature in which the authors explore variations from quasi-natural experiments on the credit market. First, Damar et al. (2020) explore differences in exposure of Canadian banks to the U.S. interbank market prior to the financial crisis of 2007–2009 and trace the impact of these differences on financial outcomes of households banking in those financial institutions. Damar et al. (2020) separate banks into two groups: "exposed" and "unexposed" to the U.S. interbank market, based on the 3% threshold of the share of interbank deposits from the U.S. held by Canadian banks in 2006.

Second, Jensen and Johannesen (2017) follow a similar approach and split Danish banks into two groups based on the stability of their funding base on the eve of the financial crisis. They compare the outcomes of customers in banks with an above-median ratio of loans to deposits in 2007 ("exposed" banks) to those of customers in banks with a below-median ratio ("nonexposed" banks). They also interact an "exposed" dummy variable with a vector of time dummies, from which they omit 2007, i.e., the pre-crisis year. This facilitates interpretation of the coefficients at $Time \times Exposure$ interaction terms as changes, relative to 2007, in the outcome variables of those households that take credit in exposed banks, compared to those households that are customers in nonexposed banks.

Third, Auclert et al. (2019) investigate how differences in consumer bankruptcy protection across U.S. states affected charge-offs and employment rates in the Great Recession. They focus on the state differences in the size of assets that are exempt from seizure by creditors since this size in each state is set exogenously prior to the crisis. They regress household outcomes in a particular location on the protection intensity measure interacted with the time dummy variables, controlling for time and location fixed effects. They also omit one pre-crisis time dummy variable and its respective interaction with the treatment variable. By doing so, they normalize magnitudes of the coefficients on the interaction terms for all previous and subsequent time periods relative to the pre-crisis year, i.e., all the effects are estimated *relative* to the omitted time period.

We follow the approaches of Auclert et al. (2019), Damar et al. (2020), and Jensen and Johannesen (2017) and specify the following four difference-in-differences regressions with time dummies, where each of the four regressions corresponds to one element in $ShockYear$ vector = $\{1984, 2004, 1989, 2009\}$:

$$Y_{i,s,t} = \alpha_i + \delta_s + \gamma_t + \sum_{k \neq ShockYear-1} \beta_k \cdot \mathbf{1}_{\{k=t\}} \cdot TREAT_s^{(ShockYear)} + \theta X_{i,s,t} + \epsilon_{i,s,t} \tag{9}$$

where $Y_{i,s,t}$ is an outcome variable of household i living in state s at time period t, $X_{i,s,t}$ are household *demographic* control variables (sex, race, age, family status, education),[42] α_i, δ_s, and γ_t are household, state, and time fixed effects.

Here the variable $TREAT_s^{(ShockYear)}$ is defined at the state level s in a particular $ShockYear$ and specified differently for positive shocks in 1984 and 2004. Specifically, in 1984 and 2004, this variable takes a value of 1 in the states *above* median according to the size of the *positive* credit supply shock and zero otherwise. In other words, in the years of positive credit shocks, we assign states to the "treatment" group if they are hit by a more sizable shock ("exposed" states in the terminology of Damar et al., 2020 and Jensen and Johannesen, 2017):

$$TREAT_s^{(ShockYear)} = \begin{cases} 1 \text{ if } \varepsilon^{CS}_{s,ShockYear} \geq \overline{\varepsilon}^{CS}_{s,ShockYear} \text{ and ShockYear} = \{1984,\ 2004\} \\ 0 \text{ if } \varepsilon^{CS}_{s,ShockYear} < \overline{\varepsilon}^{CS}_{s,ShockYear} \text{ and ShockYear} = \{1984,\ 2004\} \end{cases} \tag{10}$$

where $\overline{\varepsilon}^{CS}_{s,ShockYear}$ is median CS shock across the states in a given $ShockYear$.

Similarly to Auclert et al. (2019) and Jensen and Johannesen (2017) we interpret coefficients β_k in equation (9) as the differences in outcome variables relative to the normalization year, in our case, the year preceding the $ShockYear$.

In the sensitivity analysis in 4.5, we consider the same difference-in-differences regressions on the level of U.S. states, i.e., we average household outcomes residing in the same states in a particular year and compare average state-level outcomes before and after the shock "treatment":

$$Y_{s,t} = \alpha_s + \gamma_t + \sum_{k \neq ShockYear-1} \beta_k \cdot \mathbf{1}_{\{k=t\}} \cdot TREAT_s + \gamma X_{s,t} + \epsilon_{s,t} \tag{11}$$

Let us now rationalize why we choose 1984 and 2004 as the years of credit supply shock

[42] It is important to consider only demographic controls in our regression and not include those covariates which, in turn, could be affected by credit supply shocks and the division of states based on the size of the shocks. This is the well-known problem of "*bad controls*". Most of such bad controls are actually in the list of our outcome variables $Y_{i,s,t}$ (see the description below).

"interventions". First, we want to cover at least two credit cycles in the U.S. economy. We show above (see Figure 13) that complete credit cycles with credit contractions following credit expansions are observed in the 1980s and 2000s while this is not the case in the 1990s. Therefore, we do not consider shocks in the 1990s in this analysis.

Second, we focus on countrywide, or "systemic" shocks, i.e., the years in which most of the states were hit by a shock of the same sign. We compute the fraction of states hit by positive or negative shocks for each year (see Figure 2 in the Appendix) and conclude that the "systemic" positive shocks within the decades analyzed took place in 1981, 1984, 2004, and 2005.

We choose 1984 instead of 1981 because 1982 was a recession year, and we want to focus on positive credit supply shocks corresponding to the expansionary phase of both credit and business cycles. We choose 2004 instead of 2005 because this was the first year of prevalent positive shock in the 2000s. Following Mian et al. (2020) and other literature on bank deregulation, we exclude South Dakota and Delaware from the analysis as these states are known to be tax havens.

In the sensitivity analysis in Appendix no. 19, we consider an alternative to the difference-in-differences specification—the local projection method of Jorda (2005). With this flexible method, we follow Kehoe et al. (2020), who also estimate the effects of macro shocks (in their case, monetary policy shocks) on micro-level outcomes. By applying Jorda's method, we estimate a set of regressions for each forecasting horizon, equaled to $h = \{0, 1, ..5\}$ years for each t corresponding to the year of shock: $t = ShockYear = \{1984, 2004\}$.

$$Y_{i,s,t+h} = \alpha_h + \beta_h \cdot TREAT_{s,t} + \gamma_h \cdot Y_{i,s,t-1} + \theta X_{i,s,t} + \epsilon_{i,s,t+h} \qquad (12)$$

Since these regressions are effectively cross-sectional (they are estimated for fixed t), we cannot include either household, state, or time fixed effects. Similarly to the difference-in-differences regressions, Y still reflects household outcome variables while X is a vector of household demographic controls. Here the coefficients β_h are interpreted as impulse responses to a differential state "treatment" by credit supply shock.

We consider the following set of outcome variables at the household level, $Y_{i,s,t}$: (1) Household defaults: bankruptcy (PSID, 1980—early 1990s) or mortgage delinquency indicator variable (CEX, 2000s); (2) Employment status, employment in tradable or non-tradable sectors; (3) Real total family income, CPI adjusted; (4) Mortgage debt indicator variable; (5) Real mortgage debt, CPI adjusted; (6) Mortgage debt to income ratio; (7) Homeownership status; (8) Real house value, CPI adjusted.

All data except for that on mortgage delinquency comes from the PSID database (see detailed data description in Section 2.3.2 below).

Though our main variable of interest is the rate of household defaults, we are interested in the exploration of channels through which credit supply shocks affect defaults on loans. For this purpose, we include other outcome variables, which we borrow from the literature. First, Mian et al. (2020) argue that the positive credit supply shock caused by bank deregulation in the early 1980s propagated through local state economies via the *household demand channel*. In particular, they find evidence of a more rapid growth of debt to income and household loans in states that were deregulated early, as well as an increase in employment in the non-tradable sector, alongside an outpacing rise of prices in non-tradable sector relative to the tradable sector in those states. Mian et al. (2020) also show a more pronounced housing boom and bust in early deregulated states compared to the states that deregulated later. We thus consider employment outcomes, including employment breakdown by tradable and non-tradable sectors, mortgage loans, and mortgage debt to income ratio. These indicators allow us to test the operation of the household demand channel, but compared to Mian et al. (2020), we can test the presence of this channel not only in the 1980s as it is done by these authors, but in other decades too, because our state-level CS shock measures are estimated over a 40 year horizon.

Second, Mian and Sufi (2009) find that an increase in mortgage defaults in 2007 is significantly higher in the subprime ZIP-codes in the US. Notably, these are the ZIP-codes which also experienced a relatively higher credit expansion in preceding years. Moreover, Mian and Sufi (2009) reveal a negative correlation between income growth and credit growth from 2002 to 2005 at the ZIP-code level, thus suggesting that income growth can be an important determinant of default rates. Therefore, we also consider household income as an outcome variable to test the *income-based hypothesis* of credit expansion and, again, we are capable of doing so not only on the Great Recession sample, but with the data on other decades.

Third, there is a stream of the literature (surveyed in Mian and Sufi, 2017) arguing that the credit market played only a passive role in the recent housing boom and bust in the U.S. According to this *passive credit view*, credit expansion was the *result* of the housing bubble and not the cause of it. Recently, in an influential work by Kaplan et al. (2020), the authors find that the key driver of house prices and rents was a shift in beliefs and not changes in credit conditions, as the *credit supply view* would suggest. Moreover, Kaplan et al. (2020) outline that "shifts in credit conditions do not move house prices". To test this *passive credit view*, we consider real house value among outcome variables. In particular, in our setting, we can test directly whether credit supply shocks are indeed important in driving house prices or not, as we focus on exogenous variation in credit conditions by construction. Additionally, Kaplan et al. (2020) consider the homeownership rate among the variables through which they test their quantitative model. Following them, we also add this variable to our empirical analysis.

2.3.2 Micro-level data

We use two micro-level databases in our empirical analysis.

Our main data source is the Panel Study of Income Dynamics (PSID) data, the longest available micro-data panel on U.S. households that is representative of the U.S. population. Given the scarcity of the data on household defaults in the PSID, we use two subsamples: one subsample covers data prior to 1996 (1980-1996 sample, "1980s"); and the second sample spans 1999-onward ("2000s") (see Figure 13 for a graphical illustration). The other household outcome variables described above are maintained in the PSID continuously.

Given that data on mortgage distress in the PSID is available only from 2009, and this is not enough to study the effects of credit supply shocks in 2004 and 2009 using the difference-in-difference specification described in equation (9), we use a second data source—the Consumer Expenditure Survey (CEX) data to construct long series (since 1993) on our main outcome variable. In particular, we use CEX data to construct the variable capturing an incidence of mortgage delinquency in a household. We use this variable as a substitute for mortgage distress which is not available over a long enough period in the PSID.

Importantly, in the PSID data, a unit of observation is a household (a panel structure is maintained throughout the time) while in the CEX, we aggregate individuals into five-year birth-year cohorts.[43] In addition, the PSID data has annual frequency until 1999, and biannual afterward, so we have annual data in our first sample (1980-1996), biannual frequency on all outcome variables coming from the PSID, and also annual data on mortgage delinquencies coming from the CEX in our second sample (1999-onward).

We obtain data from the two datasets included in the PSID. First is a collection of Family Data Files provided for each wave. In the PSID, a family is defined as a group of individuals living together and sharing income and expenses. Family Data Files are our main source of data because information on debt, income, and defaults is collected at the family level. The second dataset is a Cross-Year Individual Data File containing a panel of individuals who comprise families. We use data on individuals from individual files to refine their data

[43] In this dataset, each household is repeatedly surveyed only for 5 quarters, i.e. data does not have a panel structure.

in the family files: we observe that in the family files, data on individual characteristics of heads of households could be reported with inaccuracies and errors. We use the "psidtools" program in Stata to assemble a dataset that combines individual and family files. We drop all individuals except for family heads from our sample to avoid observing the same household multiple times. This means that we use heads of families as a unit of observation and build a correspondence between their demographic characteristics and family outcomes.

In the CEX, we match data from family files (FMLI), which contain characteristics of consumer units including the demographics of members and summary expenditures, and data on mortgages (MOR), which contain information on mortgage balance and other characteristics of mortgage loans for each mortgage reported (one consumer unit may have more than one mortgage). For each time period surveyed, mortgage information is reported for the last three months. Unfortunately, there is no direct question on mortgage delinquencies in the CEX. We thus make an estimation of the occurrence of a 1-month delinquency on a mortgage loan if a household reports an unchanged mortgage balance (in other words, an outstanding amount of mortgage debt is not reducing) or if a principal balance payment is reported to be zero in any month. Similarly, we make an estimation of a 3-month delinquency event. We aggregate quarterly CEX data into annual frequency, in view of the annual frequency of our credit supply shocks. In the case of mortgage delinquencies, we assume that a household allows mortgage delinquency of 1- or 3-month duration in a given year if there is a corresponding delinquency event in any quarter of the year.

A closer look at the micro-data on defaults

We begin by describing the household default data in the pre-1996 sample. In 1996, there was a special one-time interview in the PSID in which households were asked about financial distress events. Importantly, households were asked about bankruptcies, not only in the year of interview (1996) but also for *two recent bankruptcies* in other years. We use this information to construct a binary indicator if a household declared bankruptcy in a particular year in the pre-1996 sample.

In the 1996 survey, 526 (6.2% of households) reported that they filed for bankruptcy in a year prior to 1996. After the application of our filters dropping observations on individuals younger than 18, students, and the retired, the number of bankruptcies ever reported prior to 1996 dropped to 426; however, the proportion of positive responses changed only slightly (7.0%). We then checked how many of the households who answered positively we have household characteristics data for. It turns out that many of the households were not surveyed either prior to or in the year in which they reported bankruptcy. This led to a further reduction in bankruptcy events.

Further, we converted the answers on the years of two recent bankruptcies into a binary 0/1 indicator variable. This variable takes a value of "1" in the year of reported bankruptcy and "0" in the years in which households had reported non-zero mortgage or non-mortgage debts (see details on variables construction in the Appendix) and did not report bankruptcy. After applying this procedure to the construction of the bankruptcies' indicator variable, we found 58,219 "no bankruptcy" household-year observations and 246 bankruptcy events, with an average bankruptcy frequency of 0.42%.

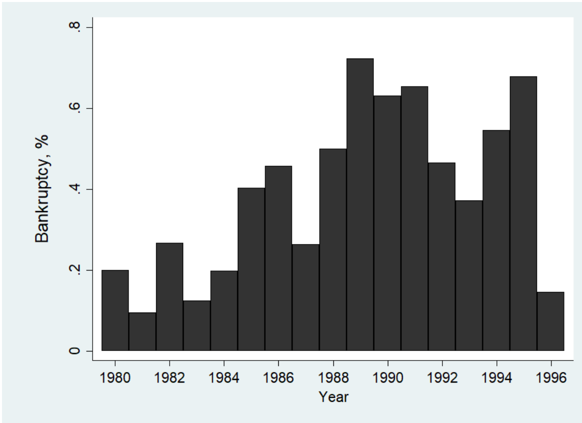
Notice that Fay et al. (2002) use the same pre-1996 PSID dataset in their research on the determinants of household bankruptcies. They also use the years of recalled bankruptcy events to construct a binary dependent variable. They report 254 bankruptcies,[44] with the average frequency of the bankruptcy event equal to 0.32%. They also note that the PSID bankruptcy filing rate is only about half of the national rate.

Importantly, we have a reasonable time-series variation in our indicator of interest (see Figure 16a). In particular, we have a local peak in the bankruptcy rate in 1982 following the 1981-1982 recession. Then there is another long-lived bankruptcy level peak in the years following the S&L crisis in the U.S. financial system (end-1980s) and against the backdrop of the 1990-1991 recession. Though we acknowledge that our measure of a household's balance sheet distress is likely to be noisy, as households are asked to recall all past bankruptcies just once in 1996, if we assume that state differences in the bankruptcy recall rate are negligible, then our identification remains valid.

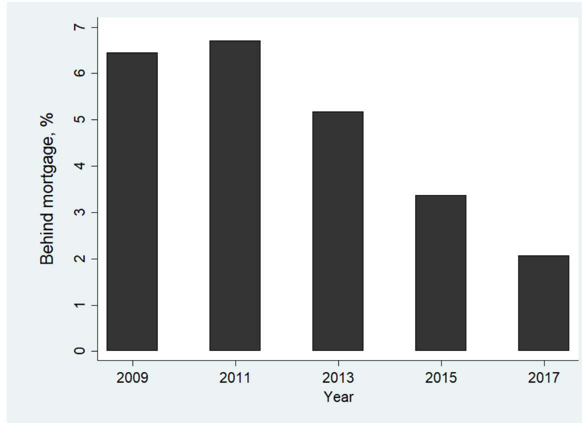
Recently, following the global financial crisis, new questions on mortgage distress were added to the PSID. Since 2009, households have been asked if anyone in a family unit is currently behind on mortgage or loan payments, and for how many months. The annual frequencies of such events are presented in Figure 16b. Unfortunately, this data is not enough to study the main question of this paper in the difference-in-difference framework, because we are interested, among other shocks, in the effects of the 2004 CS shock. Moreover, to investigate the shock of 2009, we need information on pre-trends, which are not available for this variable. Consequently, in a difference-in-difference estimation of the effects of the 2004 and 2009 CS shocks, we switch to the CEX data. A comparison of the frequency of "behind mortgage" events reported in the PSID with the nationwide data on mortgage delinquencies published by FRED[45] yields the conclusion that the time dynamics are very similar, though again the actual national level of delinquencies is 60% higher than compared to the micro-level

[44] These minor differences in the number of events could be explained by different data cleaning procedures applied in our and their studies

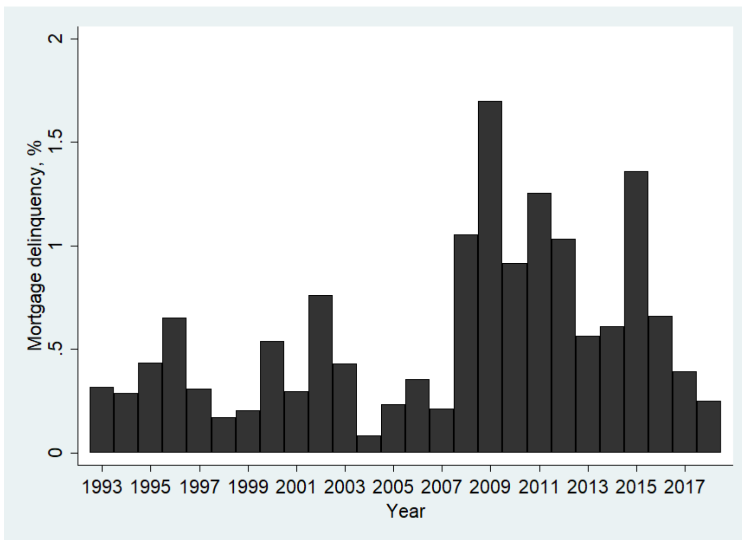
[45] See `https://fred.stlouisfed.org/series/DRSFRMACBS`.



(a) Defaults in 1980–1996 (PSID)



(b) 1-month mortgage delinquencies in 2009–2017 (PSID)



(c) Mortgage 1-month delinquencies in 1994–2019 (CEX)

Figure 16. Empirical frequency of household bankruptcies and households mortgage delinquencies according to the PSID and CEX

estimates (see Figure 1 in Appendix no. 15).

As noted above, we do not have direct data on mortgage delinquencies in the CEX; instead, we estimate the delinquency events based on the dynamics of the reported mortgage principal. Therefore, this data is even more noisy and subject to estimation errors than the PSID data. Indeed, the estimated mortgage delinquency rate amounts to only one-fifth of that reported in the PSID. Nevertheless, the estimated variable has reasonable time variation, see Figure 16c. In particular, there are local peaks of delinquencies in the mid-1990s (after the 1991 recession), in 2002 (after the 2001 recession and 9/11 terrorist attack), and then a huge rise in delinquencies beginning in 2008 (Global financial crisis and Great Recession). Therefore, under the assumption that the estimation errors in mortgage delinquencies have the same time and state distribution, we may use this variable in the subsequent analysis.

Data on other outcome variables

For *employment status*, we use information from the PSID family files on whether a head of a family is employed. We construct an indicator variable equaling one if a person is working now and zero otherwise.

Tradable and non-tradable sector employment. We collect data characterizing the *industry classification* of the household head's main job. We aggregate 3-digit codes from the survey of population and housing into the following categories: agriculture, mining, construction, manufacturing, transportation, wholesale trade, retail trade, finance, insurance and real estate, business and repair, personal services, entertainment and recreation, professional services, public administration, and military services. Following Mian et al. (2020), we assign agriculture, mining, and manufacturing into tradable industries; non-tradable industries include construction, transportation, trade, and all service industries.

Total family income. We collect information on family income (including taxable income and transfers). To avoid negative values, we cut the income variable from below at zero (negative income may arise if business losses are recorded). The household reports whether a family has a mortgage or loan on the property. We use this information to construct an indicator variable *have mortgage*. We construct the amount of *mortgage debt* owed by a household by summing the remaining mortgage principals on the first and second mortgages. The *debt to income* variable is calculated as a ratio of mortgage debt to total family income. *Value of owned house* is estimated by the household selling price of the house or apartment. We divide total income, mortgage debt, and house value by the countrywide CPI (source: Bureau of Labor Statistics) to convert these variables into constant prices. *Home ownership* is an indicator variable characterizing the response to the question of whether a household owns a home or apartment. We assign a value of one if a head reports that he owns or is buying the home, and zero otherwise.

Data on demographic control variables

Following Mian and Sufi (2010), we add the control variables including race, education, industry classification of main job, age of a reference individual, sex, and family status into our regressions. Below we provide a detailed description of each variable. A full description of data sources can be found in Table 1 (see Appendix no. 15).

We employ information about the *race* of an individual and create an indicator variable taking a value of one if an individual is white and zero otherwise.

As a measure of *education*, we use years of completed schooling. In the PSID, questions about education are not asked in all periods, and we, therefore, impute data between points. We collapse data on years of education to three binary indicators corresponding to education grades: high school or lower for 12 years or less of education, some college for 13-15 years of education, and college for 16 or more years.

We collect data on *age* of individuals from both family and individual files to fill in possible missing values. We correct errors and typos in recorded age by calculating the median year of birth of an individual as a median difference between the year of interview and reported age, and assigning linear growth of age if the recorded age produces an inconsistent year of birth. Based on the corrected age, we create four age group indicator variables: below 30, between 20 and 45, between 45 and 60, and above 60.

We use a *gender* indicator variable taking a value of 1 if a head of household is male, and 0 otherwise.

For family status, we use an indicator variable taking a value of 1 if a reference person is *married* or permanently cohabiting and 0 otherwise.

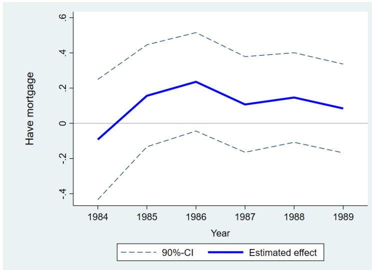
2.3.3 Estimation results: the effects of systemic positive CS shocks

In this section, we describe the difference-in-differences estimation results we obtain for the cases of systemic positive CS shocks in 1984 and 2004 in the U.S. We also replicate our estimates in the setting of Mian et al. (2020). We present the estimation results graphically, tracing the time evolution of estimated coefficients on the yearly interaction terms from Equation (9) for each of the nine household-level outcomes discussed above.

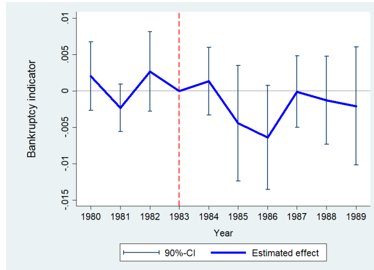
1984 positive CS shock

Let us start with the effects of the systemic positive CS shock of 1984 on subsequent outcomes at the household level. The estimation results appear in Fig. 17 below and depict the time evolution of the effects normalized to the pre-shock year, i.e., 1983.

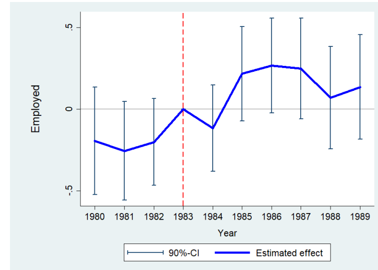
First, from the employment side, we find that the positive CS shock in 1984 had no redistribution effects from the tradable to non-tradable sectors in the states that experienced larger CS shocks (see Fig. 17.d). This is in contrast to the findings of Mian et al. (2020). Moreover, the estimated effects on employment in the non-tradable sector are negative and significant, both statistically and economically: the peak effect was achieved by 1987 and equaled a –50



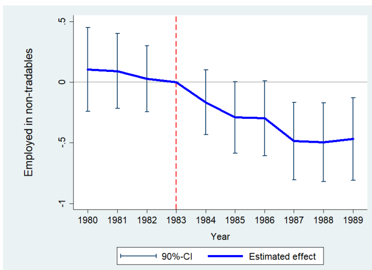
(a) Whether have mortgage



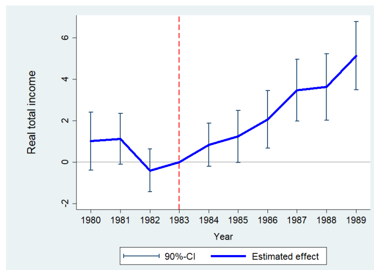
(b) Whether file bankruptcy



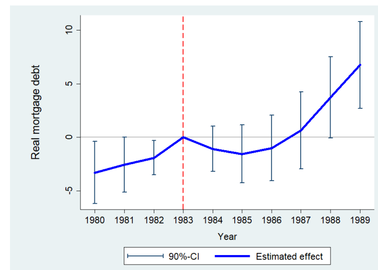
(c) Whether employed



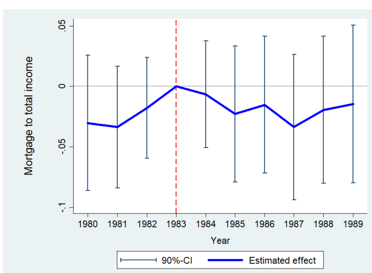
(d) Employed in non-tradables



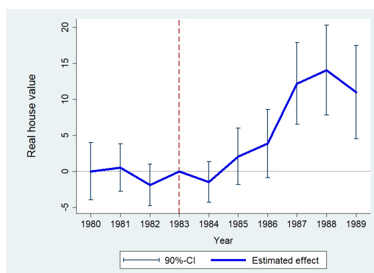
(e) Real total income



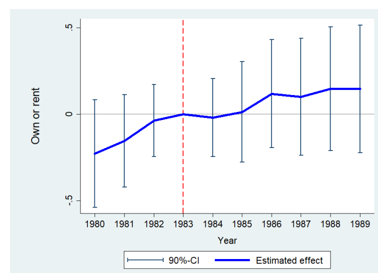
(f) Real mortgage debt



(g) Mortgage to income



(h) Real house value



(i) Whether own or rent

Note: The figure reports the results from estimating equation (9) for a set of nine outcomes measured at the household level in the 1980s and our SVAR-based measure of CS shocks. The pre-shock year is 1983, and we normalize the effect in this year to be equal to zero so that all the coefficients in the years prior or after reflect changes with respect to the pre-shock year.

Figure 17. The effects of the positive CS shock of 1984 on household outcomes

pp change in the likelihood of being employed in the non-tradable sector, and this effect is preserved over time. Overall, the likelihood of being employed after the 1984 positive CS shock rises over time but remains statistically insignificant (see Fig. 17.c), thus allowing for a certain degree of heterogeneity across households.

Second, having increased the likelihood of employment (in tradables), the 1984 positive CS shock led to a prolonged expansion of real total income of households living in the states that experienced larger shocks (see Fig. 17.e), with the peak reaction being a little less than +5 thousands of U.S. dollars. This implies that the shock pushed up local economic activities substantially in respective states in the 1980s. Further, having observed an increasing trend in total income, households could start borrowing more. Indeed, as our estimates suggest, this was the case for both extensive and intensive margins. Regarding the former, we find that the 1984 positive CS shock significantly increased the probability of obtaining a mortgage loan (see Fig. 17.a): the peak reaction occurred in 1986 and equaled a more than +50 pp increase. On the intensive margin, the results indicate that the reaction was rather sluggish in time so that the positive effect had materialized by 1988–1989 and equaled to +5 thousands of U.S. dollars in absolute terms (see Fig. 17.f). Despite these effects, our estimates further suggest that, with increased mortgages, households barely switched their status from renters to owners. The estimated effects are positive, as one would expect, but insignificant (see Fig. 17.i). Further, it is very important that the ratio of (increased) mortgage debt to (also increased) total income has not changed in a statistical sense (see Fig. 17.g), meaning that households' ability to repay debts did not deteriorate after the positive CS shock in 1984. Our complementary analysis indicates that more intensive CS shocks in 1984 occurred in less financially developed states (see Fig. 1 in Appendix no. 17). Though the levels of real mortgage debts rose more rapidly in these states, they barely caught up with the respective levels in more financially developed states. This provides further confirmation of a low risk of debt accumulation following the 1984 positive CS shock.

Third, having established that the 1984 CS shock led to a rise in total income and increased mortgage lending, we further find that real house values also went up, and rather substantially, by as much as +15 thousand U.S. dollars by 1987 (see Fig. 17.h). Notably, we reveal no pre-trends here, which means that the expectation channel, highlighted by Kaplan et al. (2020), was not active during the 1980s. Our results, therefore, support the credit supply view of Mian and Sufi (2017) on the sources of housing booms and busts over the period analyzed and are in line with the findings of Mian et al. (2020).

Fourth, we do not observe a significant rise in the bankruptcy rates of households living in the states that experienced stronger positive CS shocks in 1984 (see Fig. 17.b). Moreover, these states faced lower, though insignificantly, household default rates compared to the other states. This is very much in line with our overall findings that neither expectation nor household demand channels were in place during the 1980s, thus preventing an accumulation of financial risks during the credit expansion phase in that period.

Finally, we repeat the same exercise with the use of a treatment variable constructed based on the early vs. late deregulation dummy variable of Mian et al. (2020) instead of our CS shock measure of 1984. This variable equals one if a state deregulated its inter- or intra-state restrictions prior to 1983 and zero otherwise. The estimation results are reported in Fig. 1 (see Appendix no. 18). In general, we observe qualitatively, and in many cases quantitatively, the same estimation results. The only exception is that here we do not find significant negative responses of employment in the non-tradable sector in the early deregulated states. In the rest, the results are preserved, and thus we can claim that we provide a cross-validation of our SVAR-based CS shocks with the CS shocks originating from the early deregulation of U.S. states.

2004 positive CS shock

We now turn to the episode of systemic positive CS shock in 2004 and compare its subsequent effects with those discussed above for the 1980s. The estimation results appear in Fig. 18 below.

First, we find no effects of the positive CS shock of 2004 on employment (see Fig. 18.c). At the same time, we observe negative pre-trends in the employment rate, which may suggest that the shock occurred in the states with lower overall employment rates. In these states, presumably, there could be higher rates of subprime mortgage borrowers and correspondingly higher rates of mortgage defaults. We also find that, after the 2004 shock, there is a significant shift of employment from the tradable to non-tradable sector (see Fig. 18.d) which, according to the arguments provided in Mian et al. (2020), may indicate the operation of the household demand channel. This result is in contrast to our findings for the 1984 shock episode.

Second, our estimates suggest that there is no outpacing of real total income growth in more exposed states than in less exposed states following the 2004 shock. Indeed, the estimated coefficients reflecting the CS effect on income levels are always insignificant (see Fig. 18.e). We also do not find any positive effects on the level of mortgage debts, while there is a marginally significant positive effect on the mortgage-to-income ratio in 2007 (see Fig. 18.f,g). Note that when applying Jorda's local projection instead of the difference-in-differences approach we obtain a highly significant and positive response of the mortgage-to-income ratio to the 2004 CS shock (see Fig. 2.g in Appendix no. 19). Thus, we have (weak) evidence of mortgage credit expansion against the background of non-rising income. This contrasts dramatically with the 1984 positive CS shock episode. In addition, we again, as for the 1980s, do not find significant effects on the ownership rate and the fraction of mortgagors[46] in more vs. less exposed states (see Fig. 18.a,i).

[46] It could be the case that, due to gradual satiation of the mortgage market, prime borrowers paid off their loans and became homeowners without mortgages, while more subprime borrowers were able to access the market.

(a) Whether have mortgage (b) Whether mortgage delinq. (CEX) (c) Whether employed

(d) Employed in non-tradables (e) Real total income (f) Real mortgage debt

(g) Mortgage to income (h) Real house value (i) Whether own or rent

Note: The figure reports the results from estimating equation (9) for a set of nine outcomes measured at the household level in the 2000s and our SVAR-based measure of CS shocks. The pre-shock year is 2003, and we normalize the effect in this year to be equal to zero so that all the coefficients in the years prior or after reflect changes with respect to the pre-shock year.

Figure 18. The effects of the positive CS shock of 2004 on household outcomes

Third, similarly to the 1980s, we reveal that real house values rise in response to the positive CS shock, in line with Favara and Imbs (2015) (see Fig. 18.h). However, negative pre-trends are also observed, thus indicating that the expectations channel (more rapid credit growth in the states with expectations of higher house price growth) may play a role in housing booms and busts in the 2000s. The latter agrees with the explanation by Kaplan et al. (2020).

Fourth, gathering all these findings together, we rationalize a positive and significant response of household mortgage delinquency rates in 2006 and 2009 to the positive CS shock of 2004. Specifically, the household demand and expectations channels operate, which jointly create higher risks of financial instability (Jorda et al., 2016; Mian et al., 2017, 2020). Put differently, credit expansion in the 2000s seems to be more pronounced in the states with expectations of more rapid house price growth, leading to a disproportional rise of the non-tradable sector. Moreover, more exposed states have witnessed a stagnation of household real total income but rising mortgage-to-income ratios. All these factors contributed to increased risks of financial instability which, in our case, are measured with the mortgage delinquency rates at the household level (see Fig. 18.b). Importantly, our empirical results provide a micro foundation to the well-established link at the aggregate level from a rise of household debt to subsequent financial crises and recessions (Jorda et al., 2016; Mian et al., 2017; Nakajima and Rios-Rull, 2019). We emphasize that the increased level of household defaults may be a bridge from household credit expansion to future losses on bank capital and associated credit crunch, which deepen recessions (Reinhart and Rogoff, 2009) and financial crises (Baron et al., 2020).

A summary of the results for the 1984 and 2004 positive CS shocks episodes

Bringing together our empirical results for different sub-periods of systemic positive CS shocks in the U.S. economy, we make several conclusions.

First, stronger positive CS chocks in the 1980s were *not* associated with an increased risk of subsequent household defaults. This is because household income grew *faster* than leverage, i.e., debt to income was rather stable over time. In addition, positive CS chocks were *stronger* in states with *lower* credit depth; that is, in "treated" states initial mortgage debt level was lower than in those not treated and, as the credit boom of the 1980s proceeded, "treated" states just caught up with "control" states. One more reason why we do not observe rises in default rates in response to the positive CS shock of 1984 is that we do not find evidence of the *household demand* channel operating in the 1980s. This is reflected in that we find no shifts of employment from tradable to *non-tradable* sectors, thus indicating that local household demand and debt-financed consumption were unlikely to disproportionately speed up in response to the positive CS shock.[47] Therefore, the associated financial risks—proxied in

[47] Conversely, Mian et al. (2020) have recently shown that there was a disproportional rise of employment and prices in non-tradable sectors, which they use to rationalize an accumulation of risks and subsequent deepening of recession by late 1980s. The contrast between our results with those of Mian et al. (2020) could be due to data format: we apply more granular data, i.e., on the household level, whereas the authors employ state-level data. We rule out the other potential explanation of the revealed differences in the results on the sectoral employment responses by re-running our exercise with CS shock replaced by the Mian et al. (2020) early deregulation dummy. The results do not change even in this case.

our case by household default rates—were not rising, which is in line with Jorda et al. (2016) and Mian et al. (2017). Importantly, our conclusions on the whole range of household-level outcomes survive even if we consider the division of U.S. states in 1983 according to whether they were early deregulated or not, i.e., even if we apply the Mian et al. (2020) and Ludwig et al. (2020) treatment variable.

Second, in contrast to the 1980s, in the 2000s we do observe *higher* delinquencies on mortgage loans in states with *stronger* CS chocks. The reasons are that (i) the real income growth of households was close to zero or even negative in some states and (ii) we do find evidence of employment shifts from tradable to non-tradable sectors, thus supporting the operation of the household demand channel (Mian et al., 2020) in the 2000s.[48]

Finally, both positive systemic credit shocks of the 1980s and 2000s were accompanied by subsequent house price rises, which is in line with Favara and Imbs (2015) and Mian et al. (2020) results but contrasts with the Kaplan et al. (2020) findings. However, we acknowledge that the 2004 episode had negative pre-trends in the case of real house value, which may indicate that the expectations channel, discussed in Mian and Sufi (2009) and highlighted in the Kaplan et al. (2020) study, was operating in the 2000s.

2.4 Sensitivity analysis

2.4.1 Different measures of credit supply shocks

We begin with switching from our baseline approach to identifying CS shocks, which is the Gambetti and Musso (2017) approach with Minnesota priors, to the three available alternatives. The first is the same scheme of four sign restrictions as in the baseline but with Minnesota priors being replaced by flat priors. One could argue that the Bayesian approach to estimating VAR models, which we use to obtain our baseline results, allows for a certain degree of subjectivity in determining the mean and variance of the VAR's coefficients. With the flat priors, we are thus immune to this concern. We re-run our VAR model on the panel of the 51 U.S. states with flat priors and we then plot the time evolution of the new state-level CS shocks, comparing them with the baseline, see Fig. 3.a,c in Appendix no. 16. We reveal that nothing changes qualitatively; moreover, the magnitudes of the CS shocks across states are even quantitatively close to each other, and the systemic positive CS shocks of 1984 and 2004 are still there.

One more possible objection to our baseline results could be that the baseline sign restrictions

[48]The reason (i) is, in turn, in line with Mian and Sufi (2009) findings.

assume a too strong reaction of monetary authorities to CS shocks. Following Eickmeier and Ng (2015), we thus remove this assumption and again re-run our panel VAR estimates, both under Minnesota and then flat priors. The results appear in the right panel of the same figure, see Fig. 3.b,d in Appendix no. 16. We observe in both cases that the median CS shock estimates across years are very similar to those obtained with the baseline approach (possibly except for the early 1980s) but the across-state variation is now much larger than in the baseline. The consequence is that, during the periods of the two systemic positive CS shocks, we observe more states with negative shocks compared to the baseline. Vice versa, during the periods of the two systemic negative CS shocks, we have more states with positive CS shocks. Put differently, qualitatively the results under Eickmeier and Ng (2015) and Gambetti and Musso (2017) are rather close, but the former brings more uncertainty regarding the key periods while the latter delivers relatively larger precision.

Having re-run the VAR estimates under each of the three alternatives, we then re-run all our difference-in-differences regressions and panel-robust logit regressions linking household-level outcomes with the underlying positive CS shocks. The baseline results survive in each case.[49]

2.4.2 Different estimation approach

CS shocks are fairly exogenous with respect to household outcomes because they are shocks by construction and they are measured at the state level, while the outcomes analyzed are at the household level. One could appeal to a more intuitive and much less demanding estimation tool than our baseline difference-in-differences method—the local projection approach of Jorda (2005) (see technical details in Section 2.3.1 above). We, therefore, re-run all our baseline regressions and report the impulse response functions obtained for each of the nine household outcomes on a five-year horizon in Figs. 1 and 2 for the 1984 and 2004 positive CS shocks. We obtain qualitatively the same results for each of the four episodes. We do not employ this method as a baseline and prefer difference-in-differences because the latter allows us to check the pre-trends, which, as we show in the main text, are rather important for some of the household outcomes.

[49]The estimation results are not reported for the sake of space and are available from the authors upon request.

2.4.3 Different level of data aggregation

In the baseline estimations, we work with the data at the household level, except for the CS shock. An interesting question is whether our results hold at the state level, which is used by Mian et al. (2020) in their study. Towards this end, we achieve greater comparability with the reference paper but we also reduce the statistical power of our estimates due to a substantial reduction in data size.

Nonetheless, we aggregate each of the nine household-level outcomes to the state level and re-run all our difference-in-differences equations for each of the four systemic episodes of CS shocks. Results appear in Fig. 1 (see 4.5). We find that the results are preserved for some episodes but blurred for others.

Specifically, for the 1984 episode of positive CS shock, we see no qualitative differences compared to the baseline results: income and house value rise, while mortgage-to-income ratio does not, thus rationalizing the conclusion that the states more exposed to the shock did not experience greater household defaults.

Overall, we conclude that the state-level results are in line with the household-level results described in Section 2.3.3 though they exhibit somewhat lower statistical power.

3 The Price of War: Macroeconomic and Cross-Sectional Effects of Sanctions on Russia

3.1 Introduction

Since World War II, more than 100 countries have faced economic sanctions imposed by the West with the aim of shifting the countries' unfavorable political regimes by damaging the local economies without launching full-scale wars (Levy, 1999; Etkes and Zimring, 2015; Felbermayr et al., 2020). Although there is a consensus in the literature that sanctions are seldom effective in shifting political regimes, there is strong evidence that the non-financial firms targeted by sanctions are forced to substantially reduce their international and local trade (Crozet et al., 2021) and decrease employment (Ahn and Ludema, 2020). However, there are many ways to evade sanctions. As, e.g., Efing et al. (2019) show, German banks ceased direct lending to the sanctioned firms, but at the same time, the local demand for credit had been caught up by the German banks' subsidiaries in respective sanctioned countries. In addition, as Mamonov et al. (2021) and Nigmatulina (2022) find, the impact of sanctions on targeted banks and firms could be substantially softened if the sanctioned governments have accumulated sufficient buffers in terms of, e.g., the central bank's international reserves and are able to support the economy. Given potential spillovers between sanctioned and non-sanctioned economic agents, however, it is less clear what the overall economic implications of sanctions are for the sanctioned countries. In this chapter, we take a broader perspective and estimate the aggregate effects of sanctions on the macroeconomy and identify the cross-sectional effects of sanctions on different parts of the population (rural vs. city, rich vs. poor) and firms (more vs. less productive, large vs. small).

For this purpose, we examine the case of Russia which provides a valid laboratory to study the effects of sanctions across time because of the three major waves of sanctions sequentially imposed on the country over the last decade. First, following the annexation of the Crimean peninsula by Russia in early 2014, the US, EU, and other Western countries introduced financial and non-financial sanctions on Russian officials, government-owned companies, and banks to restrict their abilities to borrow from abroad, invest in foreign assets, develop international trade, and attract advanced technologies.[50] Second, Western

[50] In general, the sanction regime includes financial restrictions (Mamonov et al., 2021), trade bans (Ahn and Ludema, 2020), travel restrictions and asset freezes imposed on specific Russian officials and business people, an embargo on arms and related materials (including dual-use goods and technologies), and restrictions on technology specific to oil and gas exploration and production.

countries extended existing and launched a new set of international financial restrictions in 2017 in response to Russia's interference in the US presidential election of 2016, including cyber-attacks, and military activities that were supporting the Assad regime in Syria (Welt et al., 2020). Third, after Russia's full-scale invasion of Ukraine in February 2022, Western countries introduced an unprecedented set of blocking sanctions, including freezes of half of the Russian Central Bank's international assets (USD 311 billion), private and corporate asset freezes, a ban on state-owned and politically connected privately-held banks from using the SWIFT international payment system, a full or partial ban on Russia's imports and exports, among others (Berner et al., 2022). In these circumstances, we have clear timing of the sanctions imposition and a large variation in the strength of the underlying sanctions shock across time within one country.

We capture the *financial sanctions shock* by negative innovations to international credit supply (Mian et al., 2017; Ben Zeev, 2019) using standard macroeconometric tools such as structural VARs. We then examine the *overall sanctions shock* by applying a high frequency identification (HFI) approach. With the use of HFI, we introduce a sanctions news shock. We extract this type of shock using daily changes in the yield-to-maturity of Russia's US dollar-denominated sovereign bonds shortly before and after the OFAC/EU announcements of each and every new portion of international restrictions on Russia's officials, state-owned or connected businesses.[51] The difference between the sign restrictions estimates and HFI estimates thus captures the effects of non-financial—trade, technological, etc.—sanctions.

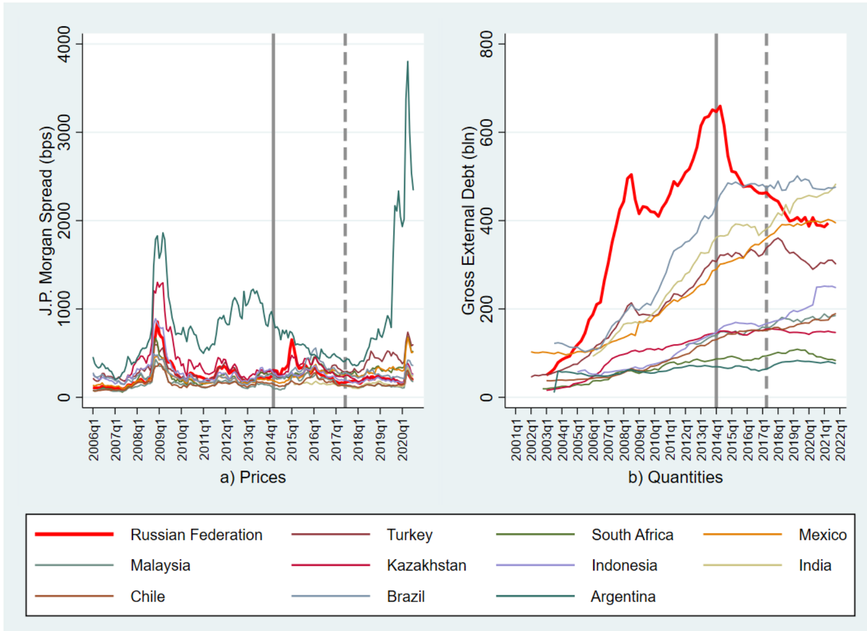
During each of the three waves of sanctions, raw data on the Russian economy reveals a rising country spread, as the price of international credit, and declining amounts of external debt, as the quantity of international credit. For example, during the first wave of sanctions in 2014 Russia's country spread, as measured by the J.P. Morgan Emerging Markets Bond Spread (EMBI+), spiked by roughly 500 bps (Fig. 19.*a*) while the amount of Russia's gross external debt slumped by about USD 103 billion (or by 20%, Fig. 19.*b*).[52] Similar but much less dramatic events occurred in 2017, i.e., during the second wave of sanctions. These events in Russia during the first two waves of sanctions are consistent with the supply-side story.[53]

[51]OFAC—Office of Foreign Assets Control, a division of the US Department of the Treasury responsible for administering of sanction imposition.

[52]In 2013, prior to the first wave of sanctions, the ratio of external debt to GDP amounted to 32%, meaning that the Western financial markets were crucial for Russia. 90% of the total amount of external debt was owed by the corporate sector—banks and non-financial companies. In 2014–2015, the external debt of Russia's banks fell by almost 40% and that of non-financial companies declined by 20%. Notably, 2014 and subsequent years were the first in the Russian market economy's history in which the country's corporate external debt was not rising, except for the global economic crisis of 2007–2009 when it declined by 6%.

[53]Additional exercises with the raw data show that the first two waves of sanctions were unlikely to transmit to the Russian economy through the demand (on foreign borrowings) channel. First, the Central

Importantly, the raw data also eliminates any concerns that rising spreads and declining amounts of external debt were common trends across different emerging market economies.



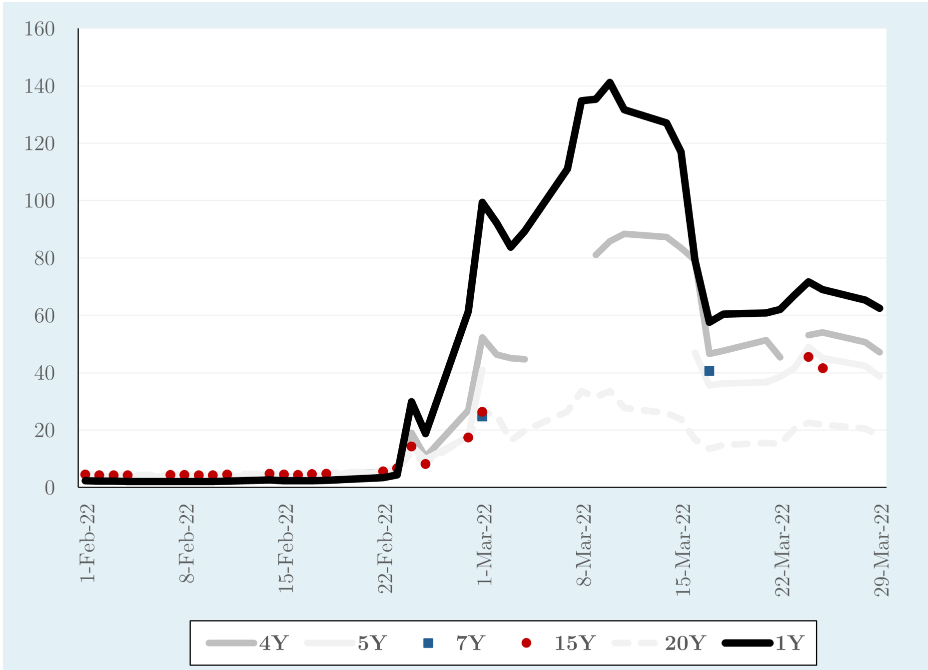
Notes: The figure plots the time evolution of the price of corporate external debt, as measured by the J.P. Morgan spread (*a*), and the amount of the debt outstanding (*b*) for Russia and other emerging market economies over the last 15-20 years. The solid vertical line marks the beginning of the first wave of sanctions against Russia (2014Q1) and the dashed vertical line reflects the start of the second wave (2017Q2).
Source: World Bank/IMF QEDS (Quarterly External Debt Statistics), J.P. Morgan.

Figure 19. *The first and second waves of sanctions*: Corporate external debt in Russia in the context of other emerging economies

As for the third wave of sanctions, data is limited because the Russian government closed access to it, but we can zoom in on the daily data on Russia's country spread during the first days of the war in Ukraine in February–March 2022. Clearly, Russia has experienced the most dramatic rise in the price of international borrowings in its history: the country's sovereign spread soared by 3,500–4,500 bps on average across the debts of different maturity (see Fig. 20).

Despite the clear timing of the sanctions, we, however, encounter certain confounders on the way to estimating the precise effects of the international restrictions on Russia. The first

Bank of Russia's statistics on net foreign debt positions of different economic agents on the eve of and two years after 2014 clearly show that private foreign assets had barely changed over the years (see Appendix no. 21 for further details). Second, Russian banks' balance sheet data indicates that, as of February 2014, i.e., on the eve of the first wave of sanctions, the share of (not yet) asset-sanctioned banks' foreign asset holdings in total foreign assets of the Russian banking system was just 2%, thus limiting substantially the concerns regarding potential asset freezes by Western governments. On the contrary, the share of (not yet) debt-sanctioned banks' foreign debts in total foreign borrowings of the Russian banking system was substantial equaling 63%.



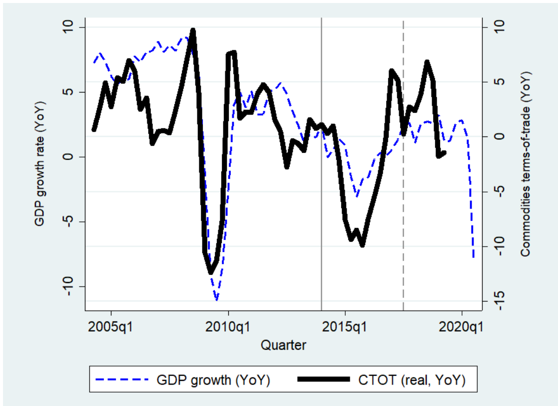
Note: The figure plots the daily data on the yields to maturity of Russia's US dollar-denominated government bonds of different maturity before and during the first weeks of the war in Ukraine in February–March 2022. *Source*: Bloomberg.

Figure 20. *The third wave of sanctions*: Soaring Russian sovereign spreads during the first weeks of the invasion of Ukraine in 2022

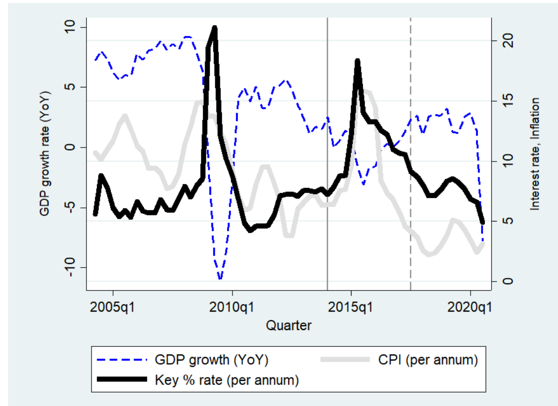
wave of sanctions in 2014 coincided with a *dramatic oil price drop*—from around USD 100 a barrel for Urals crude in the summer of 2014 to under USD 50 a barrel at the start of 2015. This had largely contributed to the observed total decline in the commodities terms-of-trade (CTOT) for Russia that amounted to –10% over that period (see Fig. 21.a).[54] As a result, Russia's ruble lost 90% of its value, the price of imported goods soared and consumer price inflation in the country spiked from 6 to 11% during 2014. In these circumstances, the Bank of Russia turned from soft to tight monetary policy and *raised the regulated interest rate* from 5.5 to 17% over the same period (see Fig. 21.(b)). In contrast, the subsequent expansion of financial sanctions in 2017–2018 (the second wave) and the sanctions of 2022 (the third wave) coincided with an increase in oil prices and soft monetary policy, thus also confounding attempts to disentangle the effects of sanctions. We therefore aim to evaluate the effects of sanctions *net* of oil price fluctuations and endogenous monetary policy responses to rising prices.

We begin our empirical analysis by employing a structural VAR approach to model the

[54]The Russian economy is highly dependent on revenue from oil and gas exports. Oil, oil products, and gas represented 50 to 70% of Russian goods exports in various years (see, e.g., Korhonen and Ledyaeva, 2010 and Cespedes and Velasco, 2012).



(a) Commodities terms-of-trade (CTOT)



(b) Regulated interest rate in Russia

Note: The figure reports the time evolution of commodities terms-of-trade (YoY, %) and the (nominal) Key interest rate of the Central Bank of Russia (%).

Figure 21. Confounders of sanctions: commodities terms-of-trade and monetary policy responses in Russia

Russian economy. The baseline VAR model encompasses the following sets of variables. First, following the literature on small open economies (Uribe and Yue, 2006; Akinci, 2013; Ben Zeev et al., 2017) we include domestic production, final consumption, investment, trade balance, the country's interest rate spread, corporate external debt, and real effective exchange rate (REER). Second, to control for the sanctions' confounders, we include a domestic regulated real interest rate (domestic monetary policy) and a set of exogenous variables—CTOT, the US corporate bond (Baa) spread, and the real US interest rate (global monetary policy).

Using monthly data from January 2000 to December 2018, we run the VAR model and estimate the residuals. We then apply the sign restrictions approach to isolate innovations to *international credit supply* (ICS; see, e.g., Cesa-Bianchi et al., 2018; Ben Zeev, 2019; di Giovanni et al., 2021) from the estimated residuals. We require Russia's country spread to rise and the amount of corporate external debt to decline on impact in the baseline version (and within several months in robustness). To distinguish between supply and demand-side forces, we also isolate innovations to the demand on international credit by forcing the price and quantity variables to change in the same direction. We then plot the time evolution of our ICS shock, and we show that it contains substantial spikes in 2014, i.e., during the first wave of sanctions against Russia. These spikes are the largest after those that our ICS shock variable exhibits for the period of the 2007–9 global economic crisis. By contrast, no visible jumps are observed for the second wave of sanctions in 2017–2018.[55] These results clearly

[55] As Mamonov et al. (2021) find, there was a great deal of in-advance adaptation of international

imply that we can use the variation in the estimated ICS shock to evaluate the effects of financial sanctions on the Russian economy in the 2010s.

However, before doing so, we provide microeconomic evidence favoring our sign restriction approach to back up the ICS shock. We employ data on syndicated loan deals in Russia from January 2011 to December 2017. The data contains information on the amount of loan, interest rate, currency, and maturity, as well as the structure of the underlying syndicate, thus allowing us to analyze the loan contracts between the borrowers—firms or banks, which are either sanctioned after 2014 or never-sanctioned—and their lenders, i.e., the banks that could also be either sanctioned or not. The data covers roughly 300 deals, which is not large in terms of quantity but is extremely large in terms of the volume of loans, being equivalent to nearly 30% of the Russian banking system's total loans to firms. By controlling for industry*month fixed effects, we run a difference-in-difference regression to isolate the supply effects before and after the Crimea-related sanctions.[56] We show that the syndicates with at least one sanctioned bank *reduced* the volume of loans by 72% and charged 1.4 pp *higher* interest rate on those loans after 2014 and as compared to the syndicates without sanctioned banks. The results thus clearly support the sign restriction approach we apply for our VAR analysis.

Having established the effects of financial sanctions that pertain to the ICS shock, we then consider a wider range of sanctions and employ an HFI approach. In the first stage, we run a regression of Russia's country spread on daily changes in the yield-to-maturity of Russia's US dollar-denominated sovereign bonds that occur *shortly before* sanction announcements. We show that there is an informational leakage: news on upcoming sanctions appears several (at least three) days before the official announcements. Exactly with this timing, we obtain the strongest positive coefficient at the first stage. In the second stage, we then run Jorda (2005)'s local projection (LP) approach to predict the effects of the sanctions news shock on the chosen domestic macroeconomic variables in a three-year horizon.

With the SVAR-based ICS shock estimates and the HFI-based estimates of the sanctions news shock, we then quantify the overall macroeconomic effects of each of the three waves of sanctions. Our computations at the monthly frequency show that the industrial production in Russia declines by 1.2% due to the financial sanctions shock and by 4.8% due to the overall sanctions shock cumulatively over 2014–2015. The effects of the second wave are 0 and minus 0.7% in 2017–2018, respectively. And the effects of the third wave are much

operations, including placing new debts, between 2014 and 2017 by not-yet-sanctioned banks in Russia. This could lower the potential strength of the second wave of sanctions, given that these sanctions were nothing new but an extension of the previous ones.

[56] The data is rather limited so that applying firm*month fixed effects is not feasible.

more pronounced: minus 12% and 18%, correspondingly. Turning from monthly to quarterly frequency and assuming 0.67 elasticity of GDP to industrial production (linear regression estimate, significant at 1%), these numbers imply that Russia's real GDP could have lost up to –3.2% in response to the first wave of sanctions, –0.5% as a result of the second wave, and up to –12% during the third wave of sanctions (the largest decline in the Russian economy since the collapse of the USSR in 1991). Overall, these results reveal that conditional on the scope of international financial restrictions, (a) the *financial* sanctions can have substantial *real* implications for the economy, and (b) the strength of the overall sanctions shock is much larger than that of the financial sanctions shock.

We then investigate the cross-sectional implications of sanctions for the representative samples of households and firms in Russia. The idea is that sanctions can hit disproportionately more: (*a*) richer households in larger cities as compared to poorer households in rural areas, and (*b*) more productive and larger firms as compared to less productive and smaller firms. We retrieve data on roughly 5,000 households across Russia from the survey database RLMS-HSE, which has been collected by the Higher School of Economics since 1994, and the data on 7,460 firms from the SPARK-Interfax database from 2012 to 2018.

Households. Using Jorda's LP approach, we show that, in a year after sanctions (as proxied with a negative ICS shock), the real income of richer households declines by 1.5% if residing in regions' capital cities, and by 2.0% if living everywhere else (larger towns, smaller towns, or rural areas). Strikingly, poorer households enjoy rising real income during the first year after the shock: +1.2% if in regions' capital cities, and +1.1% if everywhere else. These estimates control for CTOT and domestic monetary policy and are consistent with the observation that, during crisis times, the Russian government supports first those parts of the population that are more likely to re-elect it during the next electoral cycle. The government support channel is consistent with micro evidence from Mamonov et al. (2021) and Nigmatulina (2022). However, as our estimates suggest, this government help is not enough: in two to three years after the shock, the real income of the poorer households starts to decline, which offsets the growth during the first year after the shock hits.

Firms. First, we apply a popular methodology to estimate firm-level TFPs put forward by Wooldridge (2009) and Petrin and Levinsohn (2012) and employed in many studies that followed (e.g., Gopinath et al., 2017). Second, using Jorda's LP approach, we find that during the first year after sanctions (a negative ICS shock), the real total revenue of large firms with high TFPs declines by 2.2%. This is equivalent to 16% of these firms' overall decline in revenues, controlling for CTOT and monetary policy. For large firms with low TFPs, the effect of the sanctions peaks two years after the shock, reaching –4% (or 29% of the overall decline in revenues for these firms). This clearly shows that productivity matters in softening the effects of sanctions. Conversely, we estimate that the sanctions could have caused no larger than a 1% decline in the real total revenue of small firms with low TFPs and literally zero effect on small firms with high TFPs. This clearly suggests that smaller firms in Russia were much less affected by the sanctions than larger firms.

The contribution of this chapter is fourfold. First, we introduce the sanctions news shock based on the HFI approach. In contrast to Laudati and Pesaran (2021), who build a sanctions news intensity index and employ it in a VAR model to quantify the effects of sanctions on the Iranian economy, we suggest a two-stage procedure that exploits time variation in the yield-to-maturity of Russia's bonds around the sanctions announcements by OFAC/EU (*first stage*) and then uses this variation to capture the effects of sanctions (*second stage*). The idea of the sanctions news shock is inspired by the oil news shock, as embedded in OPEC's announcements, which has been recently introduced by Kanzig (2021a).

Second, our study contributes to the literature on the economic effects of sanctions. While the few existing *macroeconomic* studies focus on specific variables—Russia's ruble exchange rate in Dreger et al. (2016) or GDP growth rates in Barseghyan (2019)—our study is the first to provide a broader picture by covering a larger set of variables describing the real economy, domestic monetary policy, financial sector, and international trade. Dreger et al. (2016) exploit a cointegrated VAR and establish that the drop in oil prices in 2014 had a greater effect on the ruble dynamics than the sanctions. In turn, Barseghyan (2019) uses the synthetic control method and estimates the effects of sanctions to be 1.5% of annual GDP over the 2014–2017 period. In contrast to these studies, we use the concept of negative ICS shocks to estimate the effects of sanctions, which has a clear counterpart in the data, at both macro- and syndicated loan levels. We show that the channel of ruble depreciation is exactly the corporate debt de-leveraging due to sanctions, and we also show that GDP decreases in response to sanctions because consumption and investment fall together by more than the trade balance rises. In addition, we analyze time variation in the effects of sanctions across the three waves that occurred in 2014 after Crimea's annexation, in 2017 after the cyber-attacks in the US, and in 2022 after Russia invaded Ukraine, whereas the mentioned studies focus solely on Crimea's sanctions. Finally, Gutmann et al. (2021) apply an event-study approach in a cross-country setting and reveals that the sanctions lead to a 2.2% decline in consumption and a 24% decline in investment. Our estimates for consumption are larger, but are much more conservative for investment.

Third, by quantifying the cross-sectional implications of sanctions, we also contribute to the *microeconomic* studies on sanctions (Besedes et al., 2017; Efing et al., 2019; Belin and Hanousek, 2021; Ahn and Ludema, 2020; Felbermayr et al., 2020; Crozet et al., 2021; Mamonov et al., 2021). While most of these studies focus on the effect of sanctions on targeted firms or banks after receiving treatment as compared to non-targeted banks and firms, we study the effects of sanctions on different parts of the population and firms. As Ahn and Ludema (2020) show, the Crimea-related sanctions forced targeted firms in Russia to reduce employment by 33% and led to a decline in total revenues by 25%. We, in turn, show that these effects are likely to be concentrated within a group of larger firms with higher levels of TFPs, whereas smaller firms with lower levels of TFPs were unlikely to be affected by the sanctions. Regarding the effects on households, Neuenkirch and Neumeier (2016) find that the sanctions lead to a rising poverty gap, which is very persistent over time. Our results for the cross-section of Russian households open a different angle regarding the effects of sanctions: we show that the real income of richer households is affected negatively by the sanctions, whereas that of poorer households grows first and then declines. An unintended consequence of the sanctions could be a reduction in economic inequality, conditional on the sanctioned government's support for the poorest.

Fourth, our results imply that credit supply shocks matter for the macroeconomy even after controlling for endogenous monetary policy responses. Schularick and Taylor (2012) and Mian et al. (2017) establish a negative long-run effect of credit on output in the US and other major advanced countries. However, Brunnermeier et al. (2021) criticize these and related works for the absence of the monetary policy reaction to rising prices in the reduced-form equations used to establish the result. In our setting with the financial sanctions as episodes of negative (international) credit supply shocks, we show that Russia's industrial production declines by 1.78% in the VAR model containing domestic regulated interest rate and by 1.95% if the model would omit the interest rate variable (as in the previous literature). The price of omitting the accommodative effect of domestic monetary policy is thus significant but not very large.

The chapter is structured as follows. Section 3.2 describes the timing and types of sanctions. Section 3.3 discusses the methods: composition of the VAR model, sign restriction approach to capture the effect of sanctions, micro-level evidence for the sign restriction approach (syndicated loan deals), and the sources of data. Section 3.4 presents the macroeconomic estimates of the effects of sanctions and Section 3.5 further investigates the cross-sectional implications of sanctions. Section **??** contains our final remarks.

3.2 Timing of sanctions on Russia

The first wave of sanctions began in 2014 in response to the Russo-Ukrainian conflict: the annexation of Crimea, and the Russian support for separatist movements in Eastern Ukraine. These sanctions were imposed by the US in coordination with the EU and targeted the same entities (Welt et al., 2020). This allows us to focus on the timing of the US sanctions only. These sanctions are administered by the Treasury Department's Office of Foreign Assets Control (OFAC) and are divided into two groups: those blocking foreign assets of Specially Designated Nationals and Blocked Persons (SDNs) and those prohibiting lending, investment, and trading with entities on the Sectoral Sanctions Identifications (SSI) list. The latter—also called *sectoral sanctions*—is the primary object of our interest in this chapter because they effectively reduced the foreign borrowing capacity of Russian companies and banks.

The US Ukraine-related sanctions date back to March-December 2014 (executive orders 13660, 13661, 13662, and 13685; see Welt et al., 2020). As of 2022, before Russia launched a full-scale war in Ukraine, the sectoral sanctions remained in place and applied to new equity issuance and the loans of various maturities (more than 14-day for entities in the financial sector, more than 60-day lending for the energy sector, and more than 30-day lending for the defense sector). By 2022, OFAC included 13 Russian companies and banks and their 276 subsidiaries on the SSI list. The parent entities list includes the four largest state-owned banks, one development bank, seven major oil, gas, and pipeline companies, and one state-owned defense company.[57]

The second wave of sanctions dates back to 2017–2018 and was introduced in response to illicit cyber-enabled activities, electoral interference, and support for Syria. These sanctions were mostly imposed by the US with less support from the European Union (Welt et al., 2020). In August 2017, the US passed the Countering America's Adversaries Through Sanctions Act (CAATS), which included the Countering Russian Influence in Europe and Eurasia Act of 2017 (CRIEEA). The latter, among other measures, strengthened Ukraine-related sanctions and established several new sanctions. In particular, CRIEEA targeted a further reduction of foreign lending to the Russian financial and energy sectors. The new package also introduced mandatory sanctions (previously discretionary) against foreign financial institutions involved in "undesirable" transactions (weapons transfers, oil projects) with Russian entities, thus more strongly reducing Russian access to external financial infrastructure.

[57]VTB Bank, Gazprombank, Rosselkhozbank, VEB, Rosneft, Gazpromneft, Transneft, Novatek, Rostec, Lukoil, Surgutneftegaz, and Gazprom.

The third wave of sanctions appeared in February 2022 as President Putin's troops invaded the territory of Ukraine. As is widely discussed by Berner et al. (2022), the sanctions were of unprecedented size and scope: roughly half of the total international assets of the Central Bank of Russia (CBR) were frozen, private and corporate financial and real assets in Western countries were frozen, state-owned banks that were previously under less strict sectoral sanctions now faced fully blocking sanctions, many banks—including the largest privately-held—faced sanctions and were banned from using the SWIFT international payment system, and Russia's export and import operations were substantially banned. During the first weeks after these 'tsunami' sanctions, the financial sector in Russia seemed paralyzed with massive bank runs and the depreciation of the nominal exchange rate from 75 to roughly 140 rubles per dollar. However, CBR raised the interest rate from 9.5 to 20% and imposed various forms of capital controls. Ultimately, financial stability was restored within a month after the war started. However, as of the date of writing this text, the situation in the real sector of the economy remains highly uncertain due to the massive destruction of supply chains, Western corporate exodus, and concerns about Eastern countries (India, China, etc.) directly substituting Russia's lost imports.

In these circumstances, we need an empirical tool to quantify the effects of the financial and non-financial sanctions and analyze their variation in time, depending on the strength of the shock.

3.3 Methodology and data

3.3.1 A VAR model of the Russian economy

We perform our empirical exercises using vector autoregressive models (VARs). We consider the following (standard) VAR process with n variables and p lags:

$$y_t = A_1 y_{t-1} + ... + A_p y_{t-p} + u_t \tag{13}$$

where $y_t = (y_{1t}, y_{2t}, \ldots, y_{nt})'$ is a column vector containing the values of n variables at time t. Each matrix A_k comprises all unknown coefficients of each variable y_t taken with a lag j ($j = 1 \ldots p$) and thus has $n \times n$ dimension. $u_t = (u_{1t}, u_{2t}, \ldots, u_{nt})'$ is a column vector with reduced-form residuals, which are assumed to be normally distributed with a zero mean and covariance matrix $E\left(u_t u_t'\right) = \Sigma_u$ of $n \times n$ size, $u_t \sim N\left(0, \Sigma_u\right)$.

Following Uribe and Yue (2006), Akinci (2013), and Ben Zeev et al. (2017), we include foreign and domestic variables in our VAR model. We consider three variables in the foreign block: commodity terms of trade (CTOT), the US corporate bond (Baa) spread, and the real US interest rate. *CTOT* captures movements in commodity exports that are crucial for Russia. Oil, gas, and their products account for 63% of total exports, and their exports to GDP ratio is as high as 27% (2010-2016 average). Further, numerous studies find that changes in world financial conditions are important for emerging economies. Early literature focused on the role played by world interest rates (Neumeyer and Perri, 2005; Uribe and Yue, 2006). However, a more recent study by Akinci (2013) finds that the contribution of world interest rates to business cycle fluctuations in emerging economies could be negligible—the major force is global financial shocks. Following these studies, our VAR model includes both the *Baa spread* as a measure of global financial risks[58] and *the real interest rate in the US economy* as a proxy for the world risk-free interest rate.

The composition of the domestic variables block builds upon the real sector variables that have theoretical counterparts in the real business cycle models, e.g., Neumeyer and Perri (2005), Garcia-Cicco et al. (2010), **?**. We include *industrial production* (IP) as a proxy for domestic output, *private consumption* (C), *investment* (I), *trade balance* (TB)—all in constant rubles. We also include JP Morgan's EMBI+ *country spread* for Russia to proxy for the price of international borrowings in Russia (S)[59] and the *outstanding amount of Russia's corporate external debt* to capture the quantity of international borrowings in Russia (D, in US dollars, deflated by US CPI). Both S and D are central for the identification of the sanctions shock (see Section 3.3.3). Following recent studies by Ben Zeev et al. (2017) and Monacelli et al. (2023), we additionally include the *real effective exchange rate* (REER), which transmits the terms of trade shocks to the domestic economy.[60] Finally, we also consider the *regulated interest rate* in Russia (RIR, in real terms) to capture endogenous monetary policy responses to the sanctions shock. Although the inclusion of this variable is not directly dictated by the literature we follow, we argue that this is clearly important for our purposes. As Brunnermeier et al. (2021) show, omitting the regulator's reaction to economic shocks biases substantially the estimated effects of the shock and can thus deliver a misleading conclusion.

Ultimately, the vectors y_t and u_t can be represented as:

$$y_t = \Big[CTOT_t,\, RIR_t^{US},\, Baa_t^{US},\, IP_t,\, C_t,\, I_t,\, TB_t,\, D_t,\, S_t,\, REER_t,\, RIR_t\Big]' \tag{14}$$

[58] Another popular measure, the VIX index provided by CBOE, reflects global financial volatility and is also employed in the literature. We use this variable instead of the Baa spread in the robustness section.

[59] J.P. Morgan Emerging Markets Sovereign Bond Spread, EMBI+.

[60] Domestic production and absorption, and sectoral composition (though we do not consider sectoral outputs, to keep the model short).

$$u_t = \left[u_t^{CTOT},\, u_t^{RIR^{US}},\, u_t^{Baa^{US}},\, u_t^{IP},\, u_t^{C},\, u_t^{I},\, u_t^{TB},\, u_t^{D},\, u_t^{S},\, u_t^{REER},\, u_t^{RIR} \right]' \tag{15}$$

where variables 1–3 reflect external conditions (foreign block) and variables 4–11 internal conditions of the Russian economy (domestic block). To ensure that domestic variables do not affect external conditions, we impose the small open economy restrictions by setting to zero the coefficients on variables 4–11 in the equations in which variables 1–3 are dependent variables.

We estimate the VAR model (13) using the Bayesian methods in a framework suggested by Antolin-Diaz and Rubio-Ramirez (2018). The usage of the Bayesian methods is justified for the following reasons. First, reliable macroeconomic time series on the Russian economy cover at most the last two decades after the transformation and sovereign default crises of the 1990s (Svejnar, 2002) and thus are relatively short—even if we consider monthly frequency. The Bayesian methods are shown to work well in the presence of short time series, by formulating a prior distribution of unknown parameters, and are widely exploited in the literature on macroeconometric forecasting (Koop and Korobilis, 2010; Banbura et al., 2010; Koop, 2013; Carriero et al., 2015). Second, as we discuss below, we employ sign restrictions to isolate the sanctions shocks after estimating the VAR model. As argued by Kilian and Lutkepohl (2017), the sign restrictions perform much better and are thus usually implemented under the Bayesian framework.

Since the Bayesian methods are appropriately designed for the models with nonstationary time series, we specify the VAR model (13) in *levels* instead of deviations from respective HP-trends. In the robustness section, we nonetheless compare the results obtained with the HP-detrended time series.

However, within the Bayesian methods, we apply only the *flat* (i.e., uninformative) prior to escape subjectivity that pertains to other forms of the priors (e.g., Minnesota, inverted-Wishart, etc.). In the baseline estimates, we set $p = 2$ months.[61]

3.3.2 The data

We collect monthly data on each of the 11 variables entering the VAR model (13) and listed in vector y_t (14). We focus on the period from January 2000, i.e., after the sovereign default crisis of the late 1990s, to December 2018, i.e., at least a year and a half after the second wave of sanctions on Russia (see Section 3.2). This gives us 208 observations on each variable

[61] In the sensitivity analysis, we vary the lag structure by considering different values of p.

in total.[62]

External variables. The data on the variables reflecting external conditions for the Russian economy (i.e., the variables 1–3 in the VAR model) comes from the following sources. CTOT data is retrieved from the IMF Commodity Terms of Trade Database, where it is readily available on a monthly basis. Note that Ben Zeev et al. (2017) constructed the commodity terms-of-trade index for each country themselves based on the IMF Primary Commodity Price data set and the country-specific weights of commodities in their exports. CTOT is a net export price index of Russia's commodity bundle, in which individual commodities are weighted by the ratio of net exports to GDP.[63] Further, the real interest rate in the US economy is calculated as the US CPI-adjusted nominal 3-month Treasury Bill rate (both series come from the IMF's International Financial Statistics database). The Baa spread for the US economy is retrieved from the St. Louis FRED database.

Domestic variables. Domestic real sector variables are constructed based on the datasets of the Federal State Statistics Service of the Russian Federation (Rosstat). Financial data, in turn, is obtained through the website of the Central Bank of Russia. Industrial production, consumption, and investment are constructed based on chain indices and the nominal values and re-expressed in constant 2010 prices.[64] Trade balance is calculated as the difference between the dollar value of Russia's exports and imports and deflated by US CPI (the data is taken from the IMF's International Financial Statistics database).

Data on corporate external debt in Russia is obtained from the website of the Central Bank of Russia.[65] We sum the banks' and other sectors' external debt and subtract debt owed by these sectors to direct investors.[66] We then linearly interpolate quarterly series to obtain monthly data and deflate it by the US CPI.

Following Uribe and Yue (2006), we compute the real interest rate as the sum of the US

[62]We also experimented with adding the data for each of the 12 months of 2019 and revealed no added value in terms of identification of the credit supply shocks related to the two waves of sanctions. The data from 2020 is ignored due to COVID-19 concerns.

[63]The weighting scheme transforms the series into constant prices because import prices stand in the denominator. We also consider a deflated series: we divide the commodity export price index by the US import price index of manufactured goods from industrialized countries, similarly to Ben Zeev et al. (2017). The results did not change.

[64]Data source: Short-term economic indicators, see https://rosstat.gov.ru/compendium/document/50802.

[65]External Sector Statistics, see http://cbr.ru/eng/statistics/.

[66]A sizeable amount of Russia's corporate external debt falls into a category of debt to direct investors and direct investment enterprises. As of the end of 2013, the share of this type of corporate external debt amounted to 2% for Russian banks and 35% for Russian non-financial firms. This portion of debt is characterized by non-market behavior, as the creditors are tightly connected to the borrowers through a common ownership structure such as a group or consortium. Thus, these creditors are likely to extend debt repayment deadlines even under sanctions. We address this issue by excluding the debt to direct investors from the total stock of corporate external debt.

real interest rate and JP Morgan's EMBI country spread for Russia (J.P. Morgan Emerging Markets Sovereign Bond Spread, EMBI+). We obtain the REER variable from the Bank of International Settlement (BIS) website. Following Ben Zeev et al. (2017), we re-express this series as an inverse of that reported by BIS to interpret a decrease in this variable as REER appreciation and an increase in it as depreciation.

We apply the seasonal adjustment procedure X13 to industrial production IP_t, consumption C_t, investment I_t, and trade balance TB_t. All variables are further transformed into logs. In the robustness section, we also apply an alternative approach to data transformation: we use HP-filter to compute deviations from the filtered ("long-run") values for each of the variables employed in the VAR model.

3.3.3 Identification of the financial sanction shock

Sign restriction scheme: Sanctions as an international credit supply shock

Because the financial sanctions induce an increase in the country spread and a decrease in the amount of foreign debt simultaneously, we suggest treating them as realizations of negative international credit supply shocks (Cesa-Bianchi et al., 2018; Ben Zeev, 2019; di Giovanni et al., 2021). It is thus natural to use a proper sign restrictions scheme that allows the separation of credit supply shocks from credit demand and other shocks (Eickmeier and Ng, 2015; Gambetti and Musso, 2017).

Formally, we first rewrite the reduced-form VAR model (13) in the companion form $Y_t = AY_{t-1} + u_t$ and then premultiply both sides by a matrix B_0 that is aimed at isolating the necessary shocks. This yields a structural representation of the VAR model:

$$B_0 Y_t = B_1 Y_{t-1} + \varepsilon_t \tag{16}$$

where ε_t is a vector of orthogonal structural shocks that are related to the original reduced-form residuals via $u_t = B_0^{-1} \varepsilon_t$.

Since an international credit supply shock is a movement of the quantity and price of foreign debt along the demand curve, whereas an international credit demand shock pushes the two along the supply curve, we thus impose the following sign restrictions to identify B_0^{-1}:

$$\begin{pmatrix} \vdots \\ u_t^{IP} \\ u_t^{C} \\ u_t^{I} \\ u_t^{TB} \\ u_t^{D} \\ u_t^{S} \\ u_t^{RIR} \\ u_t^{REER} \end{pmatrix} = \begin{pmatrix} \vdots & \vdots & \vdots & 0 & 0 & 0 & 0 & 0 & 0 & 0 \\ . & . & . & a. & . & . & . & . & . & . \\ . & . & . & . & . & . & . & . & . & . \\ . & . & . & . & . & . & . & . & . & . \\ . & . & . & . & . & . & . & . & . & . \\ . & . & . & . & . & . & . & + & - & . \\ . & . & . & . & . & . & . & + & + & . \\ . & . & . & . & . & . & . & . & . & . \\ . & . & . & . & . & . & . & . & . & .b \end{pmatrix} \begin{pmatrix} \vdots \\ \varepsilon_t^{4} \\ \varepsilon_t^{5} \\ \varepsilon_t^{6} \\ \varepsilon_t^{7} \\ \varepsilon_t^{Credit\ Demand} \\ \varepsilon_t^{Credit\ Supply} \\ \varepsilon_t^{10} \\ \varepsilon_t^{11} \end{pmatrix} \tag{17}$$

where "+" and "−" are the imposed signs that guarantee that D and S move in the same direction when a credit demand shock hits and in the opposite direction when a credit supply shock occurs. Further, "$\vdots$" are the cells that correspond to the three exogenous variables: they may affect each other, but they are not affected by the domestic variables (the small open economy restrictions; each 0 has a 3×1 dimension, for convenience reasons). Finally, "." means a non-empty (unrestricted) element.

Using the framework of Antolin-Diaz and Rubio-Ramirez (2018), we rotate candidates for the B_0^{-1} matrix until we obtain at least 10,000 successful draws from the posterior distribution that satisfy the imposed sign restrictions. For each successful draw, we compute the time series of the international credit supply shock $\hat{\varepsilon}_t^{Credit\,Supply}$ and the impulse responses (IRFs) of the domestic macroeconomic variables to this shock h periods ahead ($h = 1, 2...60$ months). The IRFs are normalized across all variables such that the shock is equivalent to a 1 pp increase in the country spread variable. We first plot the time evolution of the resultant empirical distribution of the international credit supply shock to analyze whether we identify significant spikes around the first and second sanction waves in 2014-2015 and 2017-2018, respectively.[67] If we do identify these, we then relate them to the financial sanctions and we eventually compute the average effects of the sanctions on the i-th domestic variable as the product of the peak magnitude of respective IRF and the size of the shock in the *50th %-tile* of the shock's distribution. For the first two waves of sanctions, we do it *in-sample*:

$$\Delta^{(J)}\hat{y}_i = \max_{t \in J}\left(\hat{\varepsilon}_t^{Credit\,Supply}\right) \times \max_h \left(\frac{\partial \hat{y}_{i,\tau+h}}{\partial \hat{\varepsilon}_\tau^{Credit\,Supply}}\right), \tag{18}$$

[67]Similar procedures of relating the identified shocks to specific events that are generally attributed to the episodes of particular shocks are performed in, e.g., Antolin-Diaz and Rubio-Ramirez (2018) and Brunnermeier et al. (2021) to ensure credibility.

where $J = [Mar.2014 \ldots Dec.2015]$ marks the first wave and $J = [Jun.2017 \ldots Dec.2018]$ marks the second wave of sanctions. $\max_{t \in J}$ implies obtaining maximum value over the J-th wave of financial sanctions and $\max_h$ implies searching for such h at which respective IRF reaches its maximum. Therefore, $\Delta^{(J)} \hat{y}_i$ means the maximum predicted change of y_i caused by the international credit supply shock over the J-th wave of financial sanctions.

For the third wave—the 2022 war-related full-scale sanctions—we compute the *out-of-sample* predictions of the effects of sanctions. We assume that the (peak) IRFs did not change in time and the size of the shock is fully captured by the observed dramatic increase in the country spread during the first months of the war (recall Fig. 20):

$$\Delta^{(J)} \hat{y}_i = \max_{t \in J} \left(Spread_t \right) \Bigg|_{J=[Feb.22 \ldots Apr.22]} \times \max_h \left(\frac{\partial \hat{y}_{i,\tau+h}}{\partial \hat{\varepsilon}_{\tau}^{Credit\,Supply}} \right) \Bigg|_{\tau \in [Jan.00 \ldots Dec.18]}, \quad (19)$$

Overall, the sign restriction approach allows us to isolate international credit supply shocks while controlling for commodities terms-of-trade (first confounder) and domestic monetary policy responses to rising prices (second confounder).

Microeconomic justification of the aggregate credit supply shock: Evidence from syndicated loan data

We now provide evidence supporting our sign restrictions scheme at a more granular level. Specifically, we employ the data on syndicated loans in Russia that were issued between January 2011, i.e., three years before the sanctions, and December 2017, i.e., three years after. By matching banks and their corporate borrowers and employing the combinations of borrower*month fixed effects, this data enables us to separate supply from demand on loans. Of course, a typical drawback is that syndicated loans cover only a small portion of firms compared to all firms borrowing within a given country. However, these are typically very large firms that operate not only within the country but also abroad and attract loans from the syndicates of local and foreign banks. When we explore the effects of international sanctions, a decline in the supply of loans can stem from the foreign banks' decreased willingness to continue lending in the sanctioned country (Efing et al., 2019).

We obtain syndicated loan data from an international financial IT-company Cbonds.[68] We reveal 294 loans granted by the syndicates of Russian and foreign banks to non-financial firms and banks operated in Russia from January 2011 to December 2017. We observe that 148 loans were issued to firms and the other 146 were issued to banks. We also witness a decline in the number of loans as the economy switches from non- to the sanctions regime: 177 loans

[68] See `https://cbonds.com/`.

were granted before and only 117 loans after the Crimean sanctions. We also observe in the data that the average amount of loans declines (in real US dollars) whereas the average interest rate on those loans rises when we compare 'before' and 'after' the sanctions—a pattern that is already consistent with the supply-side effects (Table 4). We see that the share of the ever-sanctioned firms and banks in the total number of borrowers in the market declines by 10 pp in the sanctions regime. We finally reveal that the total amount of the 294 syndicated loans is equivalent to roughly 30% of the total banking system's credit to firms in Russia.

Table 4. Descriptive statistics of the Russian syndicated loan market

Note: The table reports descriptive statistics for the variables employed in Equation (20). Real loans, the interest rate on loans, and the maturity of the loans match the syndicate of lending banks s, borrowing firm f (either a non-financial entity or the bank itself), and month t when the contract is signed. *Whether credit goes to ever-sanctioned firm*$_f$ is a binary variable equal to 1 if borrowing firm f ever faces sanctions after March 2014 and until the end of the sample period in 2020. Analogously, *Whether sanctioned banks in syndicate*$_{b,s}$ is a binary variable equal to 1 if bank b participating in syndicate s ever faces sanctions.

	Obs	Mean	SD	Min	Max
	(1)	(1)	(2)	(3)	(4)
Before the sanctions (Jan.2011–Feb.2014)					
Real Loan$_{s,f,t}$, USD bln 2015	177	0.762	1.450	0.006	13.152
Interest Rate$_{s,f,t}$, % annum	95	3.4	2.2	1.7	12.8
Whether credit goes to ever-sanctioned firm$_f$	177	0.2	0.4	0.0	1.0
Whether sanctioned banks in syndicate$_{b,s}$	177	0.2	0.4	0.0	1.0
Loan Maturity$_{s,f,t}$, months	177	53.9	38.4	6.0	240.0
After the sanctions (Mar.2014–Dec.2017)					
Real Loan$_{s,f,t}$, USD bln 2015	117	0.630	1.140	0.001	10.515
Interest Rate$_{s,f,t}$, % annum	34	3.6	2.4	1.2	12.8
Whether credit goes to ever-sanctioned firm$_f$	117	0.1	0.3	0.0	1.0
Whether sanctioned banks in syndicate$_{b,s}$	117	0.3	0.5	0.0	1.0
Loan Maturity$_{s,f,t}$, months	117	68.5	46.3	6.0	192.0

With this data at hand, we run the following difference-in-differences regression:

$$\begin{aligned} Y_{s,f,t} = \alpha_{i,t} &+ \beta_1\Big(SANCTIONED_f \times POST.March2014_t\Big) \\ &+ \beta_2\Big(SANCTIONED_f \times POST.Date_{f,t}\Big) + Controls + \varepsilon_{s,f,t} \end{aligned} \tag{20}$$

where $Y_{s,f,t}$ is the dependent variable—either the log of real loans issued by syndicate s to borrowing firm f in month t or the interest rate on this loan. $\alpha_{i,t}$ is a product of the firm's f industry fixed effects and year fixed effects.[69] This combination of fixed effects is intended to capture demand on loans of the firms from the same industries, in the spirit of Degryse et al. (2019). $SANCTIONED_f$ is a binary variable that equals 1 during each month within 2011–2017 if firm f ever faces sanctions after March 2014 until the end of the sample period in 2020 and 0 if else. $POST.March2014_t$ and $POST.Date_{f,t}$ are the binary variables that mark 'before' and 'after': i.e., before and after the first sanction announcement that occurred in March 2014 (the first variable) and before and after each and every further sanction on Russian firms that appeared after March 2014 (the second variable). These two variables are inspired by the work of Mamonov et al. (2021) that reveals a strong information effect of sanctions after 2014: even if not-yet-sanctioned, potentially targeted firms (banks) adapted their international operations in advance. Thus, in Equation (20) we also separate the information and direct effects of sanctions. $Controls$ include the components of the two products, maturity of loans, and whether ever-sanctioned Russian banks participate in syndicate s.

We argue that, if a negative (international) credit supply shock leads to a declining amount and a rising price of syndicated loans, then we will obtain $\beta_k < 0$ in the regression of real loans and $\beta_k > 0$ in the regression of interest rates on those loans ($k = 1, 2$). The estimation results appear in Table 5.

As the results show, this is indeed the case: we obtain a negative and significant coefficient on the $SANCTIONED_f \times POST.March2014_t$ variable when the dependent variable is the log of real loans (column 1), whereas the coefficient turns positive and significant when we switch the dependent variable to the interest rate on those loans (column 2). This means that after March 2014, the syndicates of banks started to reduce the volume of new loans and raise the interest rates on those loans for the firms that were potentially targeted by the sanctions—state-owned or controlled corporates and banks—as compared to other firms. Economically, the effects are large: the average amount of loans was reduced by 72% ($e^{-1.354} - 1$) while the interest rate was raised by 1.4 pp. Strikingly, no such effects are obtained for any other sanction announcements once we control for the one associated with March 2014.

We therefore obtain microeconomic evidence that the sanctions led to a decrease in borrowings and an increase in the price of borrowed funds for the firms targeted by the sanctions. This evidence backs up our sign restrictions scheme introduced above (Section 3.3.3) and favors our usage of the concept of credit supply shocks at the aggregate level in the rest of this

[69] Since we are rather restricted in the number of observations, we are not able to include firm*month fixed effects (Khwaja and Mian, 2008). We instead have to aggregate the firms at their respective industry level and then multiply the industry dummies with the year, not month, indicator variables. In total, we have 11 industries and 7 years.

Table 5. Difference-in-differences estimation results: Supply-side effects of sanctions at the syndicated loan level

Note: The table reports the estimates of Equation (20) with the dependent variable being either the log of the real amount of loan issued by syndicate s to borrowing firm f in month t (column 1) or the interest rate on this loan (column 2). $SANCTIONED_f$ is a binary variable that equals 1 during each month within 2011–2017 if firm f ever faces sanctions after March 2014 until the end of the sample period in 2020 and 0 if else. $POST.March2014_t$ and $POST.Date_{f,t}$ are the binary variables that mark 'before' and 'after': i.e., before and after the first sanction announcement that occurred in March 2014 (the first variable) and before and after each and every further sanction on Russian firms that appeared after March 2014 (the second variable). $Loan.Maturity_{s,f,t}$ is loan maturity, in months. *Whether sanctioned Russian banks in syndicate* is a binary variable equal to 1 if syndicate s contains Russian bank(s) under sanctions. *Industry* is a set of 11 binary variables equal to 1 if firm f belongs to the respective industry and 0 if else.

Dependent variable, $Y_{s,f,t}$:	$\ln(\text{Real.Loan})_{s,f,t}$	$\text{Interest.Rate}_{s,f,t}$
	(1)	(2)
$\text{SANCTIONED}_f \times \text{POST.March2014}_t$	–1.354***	+1.380***
	(0.548)	(0.397)
$\text{SANCTIONED}_f \times \text{POST.Date}_{f,t}$	0.516	–5.331
	(0.976)	(4.135)
SANCTIONED_f	1.612***	–0.095
	(0.277)	(0.397)
POST.March2014_t	0.830	2.959
	(0.569)	(3.720)
$\ln(\text{Loan.Maturity}_{s,f,t})$	0.078	–0.337
	(0.136)	(0.503)
Whether sanctioned Russian banks in syndicate	0.579***	2.711***
	(0.213)	(0.765)
Industry × Year FE	Yes	Yes
N obs	294	129
R^2	0.569	0.745

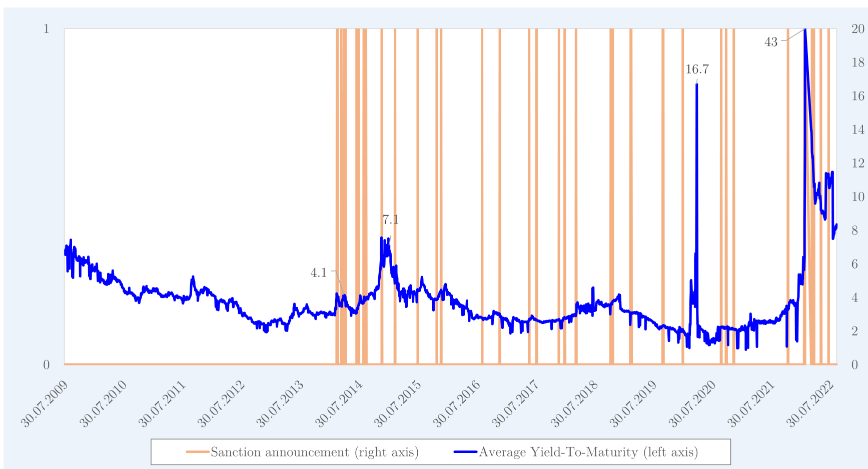
***, **, * indicate that a coefficient is significant at the 1%, 5%, 10% level, respectively. Standard errors are clustered at the loan level and appear in the brackets under the estimated coefficients.

chapter.

3.3.4 Identification of the overall sanction shock

We now employ the high-frequency identification (HFI) approach to uncover the effects of all sanctions, not only financial sanctions. HFI has been widely used to capture monetary policy shocks using the Fed's announcements on the interest rate (Gertler and Karadi, 2015) and then, more recently, oil news shock using OPEC's announcements on oil extraction quotas (Kanzig, 2021a), climate policy shocks using the EU's announcements on future CO_2 emission quotas (Kanzig, 2021b), and policy shocks using the UK's Brexit announcements (Geiger and Guntner, 2022).

We adopt HFI to identify the *sanctions news shocks* using daily dates on the OFAC/EU announcements of sanctions against Russia's politicians, state-connected businessmen, and corporations (both firms and banks) that fell on either the SDN or SSI lists. The idea is that we observe substantial spikes in the yield-to-maturity of Russia's US dollar-denominated sovereign bonds around sanction announcements because investors are likely to re-evaluate risks and start selling bonds once the bad news arrives. This is indeed what we can observe in Fig. 22, which presents the daily evolution of the yield-to-maturity averaged across 15 different (partly overlapping) issues of Russia's Ministry of Finance US dollar-denominated bonds on the background of more than 30 OFAC sanction announcement dates that occurred between 20 March 2014 and 21 July 2022.[70]



Note: The figure reports the average daily yield-to-maturity across 15 issues of Russia's US dollar-denominated sovereign bonds over 2001 to 2022 (*blue line*) and 31 OFAC daily announcements of sanctions against Russia's individuals and firms between 2014 and 2021 (SSI and SDN).

Figure 22. Average yield-to-maturity of Russia's US dollar-denominated sovereign bonds and the OFAC sanction announcements

Therefore, we can attribute (some of the) daily changes in the yield-to-maturity (YTM) to the announcements of sanctions, or anticipation of these announcements, and apply these sanction-driven changes as an instrument to isolate exogenous variation in the reduced-form residuals u of the country spread S regression at the first stage:

$$u_t^{(S)} = \alpha_k + \beta_k \cdot \Delta\overline{YTM}_{k,t} + \xi_{k,t}, \tag{21}$$

[70]Recall, however, that the macroeconomic data available for our VAR analysis is limited by the year 2019.

where $u_t^{(S)}$ is obtained from the VAR model (13), $\Delta\overline{YTM}_{k,t}$ is a cumulative within-month t sum of one-day changes in the average yield-to-maturity $\overline{YTM}$ of Russia's US dollar-denominated sovereign bonds around sanction announcement days, which is defined as:

$$\Delta\overline{YTM}_{k,t} = \sum_{\tau(t)=1}^{R_t} \Delta_1\overline{YTM}_{\tau(t)+k}, \tag{22}$$

where $\tau(t)$ is a day of sanction announcement within a month t and R_t is the total number of sanction announcements that occur within that month. $\Delta_1\overline{YTM}_{\tau(t)+k}$ is a one-day change in the average daily YTM that occurs $\tau(t)+k$ days before (if $k<0$) or after (if $k>0$) the sanction announcement. The k parameter governs potential *leakage* of the information on upcoming sanctions that may appear shortly before the announcements (e.g., $-5 \leq k < 0$ days) or traces potential delays in the reaction of financial markets to the news on already announced sanctions (e.g., $0 \leq k \leq 5$). International media sources provide direct evidence on such leakages.[71] In turn, delays may take place because global investment funds may not be able (or not allowed) to sell all the bonds within one day, which is aimed at restricting the negative systemic effects on the financial markets that such sales could entail.

If our instrument works well in the first stage, we then proceed to the second stage of the HFI approach. Specifically, we apply Jorda (2005) local projection (LP) approach to build impulse responses of domestic macroeconomic variables to the sanctions shock, as measured with the fitted values $\widehat{u}_t^{(S)} = \widehat{\beta}_k \cdot \Delta_1\overline{YTM}_{k,t}$ from the first stage. As discussed, e.g., in Mian et al. (2017), Jorda's LP is more flexible in terms of control variables than VARs and is thus more robust to functional misspecification. We use the following regression form:

$$y_{i,t+h} = \omega_{i,h} + \gamma_{i,h} \cdot \widehat{u}_t^{(S)} + \delta'_{i,h}\mathbf{X}_t + \mu_{i,t+h} \tag{23}$$

where $y_{i,t}$ is ith ($i = 1, 2...8$) domestic macroeconomic variable considered in the VAR model (13) above, t is month from January 2000 to December 2018 and $h = 1, 2...36$ is prediction step ahead of the sanction shock. $\mathbf{X}_t$ contains control variables: all monthly lags of $\widehat{u}_t^{(S)}$ from 1*st* until 12*th*, thus covering the whole previous year, and the current values and 12th

[71] We run a series of Google searches of the following form: "[*Name of the media*] Russia sanctions" in a five-day time interval $[\tau-5, \tau)$ across such medias as *The Guardian*, *Wall Street Journal*, *New-York Post*, *BBC*, *Bloomberg*, etc. In all cases, we find that the sanction announcements were highly expected one to five days in advance. Essentially, this is not surprising because an adverse action—another episode of Putin's aggression—and the response of the West to it—economic sanctions—are clearly separated in time. After the action and before the sanction announcement, the sanction's preparation stage takes place during which leakage may occur, see Appendix no. 22 for examples of media reports on expected sanctions on the eve of the most important announcements in 2014: on March 17th (politicians responsible for the annexation of Crimea and the *Rossiya Bank*, the so-called "Putin's wallet"), July 16th (most of the largest state-owned banks, excluding the top-1, Sberbank), and September 12th, Sberbank).

month lagged values of each of the eleven variables in y_t.[72] Born et al. (2020) apply a similar procedure of unfolding the effects of spread shocks on macroeconomic variables.

3.4 Results: Macroeconomic effects of sanctions

In this section, we present the macroeconomic estimates of the sanctions shock and its impact on the system of domestic macroeconomic variables. We begin with the effects of *financial sanctions* that we capture using the sign restrictions (SR) approach and the concept of international credit supply slump. We then turn to the effects of *all sanctions*, which include not only financial sanctions, but also restrictions on trade, politicians, and technology. For this purpose, we employ the high-frequency identification approach (HFI). We then summarize the effects we obtain under SR and HFI attributing the difference between them to the other (non-financial) sanctions.

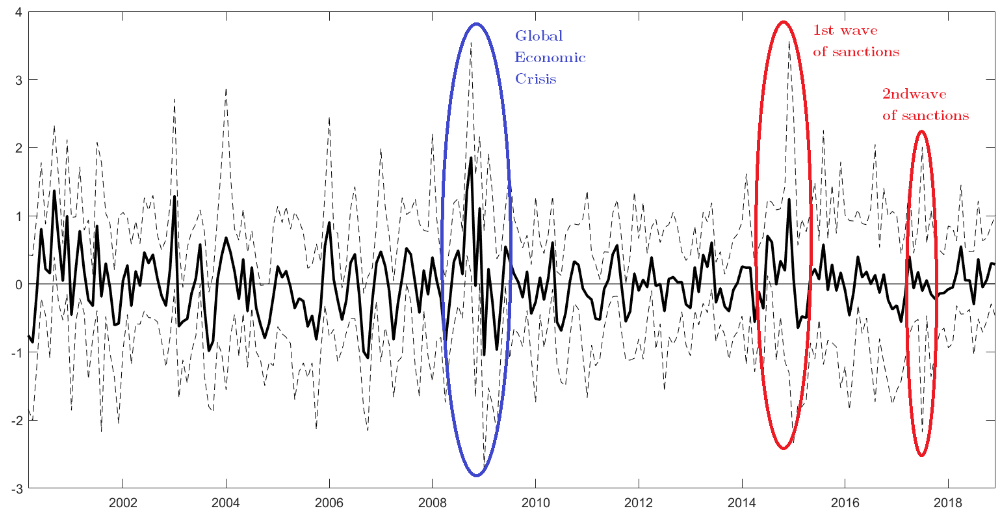
3.4.1 Sign restrictions: The effects of financial sanctions

We start with the preliminary results that we obtain from a version of the VAR model (13) with the domestic interest rate being dropped from the list of endogenous variables. Domestic monetary policy is typically ignored in the VAR models of EMEs because the literature assumes that local financial regulators simply follow world interest rate cycles determined by global central banks, see, e.g., Uribe and Yue (2006). Recall that we have the real interest rate in the US economy among the external variables in our models. In the next section, we add the domestic interest rate to close the model and reveal whether there is an added value in terms of the estimated effects of sanctions.

Using the sign restriction scheme (17), we first isolate a *negative* international credit supply (ICS) shock from the residuals of the VAR model (13) and we then analyze the time evolution of the isolated shock (Fig. 23). By construction, positive values of the ICS shock correspond to unexpected declines in the supply of external borrowings, and negative values—to unexpected rises. We plot the median extraction from the posterior distribution of the estimated ICS shock and the conventional bands formed by the 16th and 84th %-tiles of the same distribution. We infer that the resultant time series contain substantial spikes around the first wave of the financial sanctions in 2014. The peak of these spikes is the

[72] Recall that all variables in y_t are taken in levels so that it is enough to consider their $0th$ and $12th$ lags to cover the previous year. Our results remain the same if we include each lag from $1st$ until $12th$ of each of the eleven variables in y_t. We do not consider it a baseline because it is much less parsimonious than what is implied by Equation (23), given the relatively short time span that we have. We also stress that the results remain the same if we drop the $1st$ to $12th$ lags of $\widehat{u}_t^{(S)}$ from $\mathbf{X}_t$.

largest one in the 2010s and is comparable to the maximum value of the estimated ICS shock—the one that corresponds to the global financial crisis of 2008–2009. Conversely, we observe no jumps around the second wave of the financial sanctions in 2017–2018. These results are in line with our expectations and the findings of Mamonov et al. (2021), which show that sanctioned banks in Russia adapted their international operations after 2014 but in advance of actually facing the restrictions.

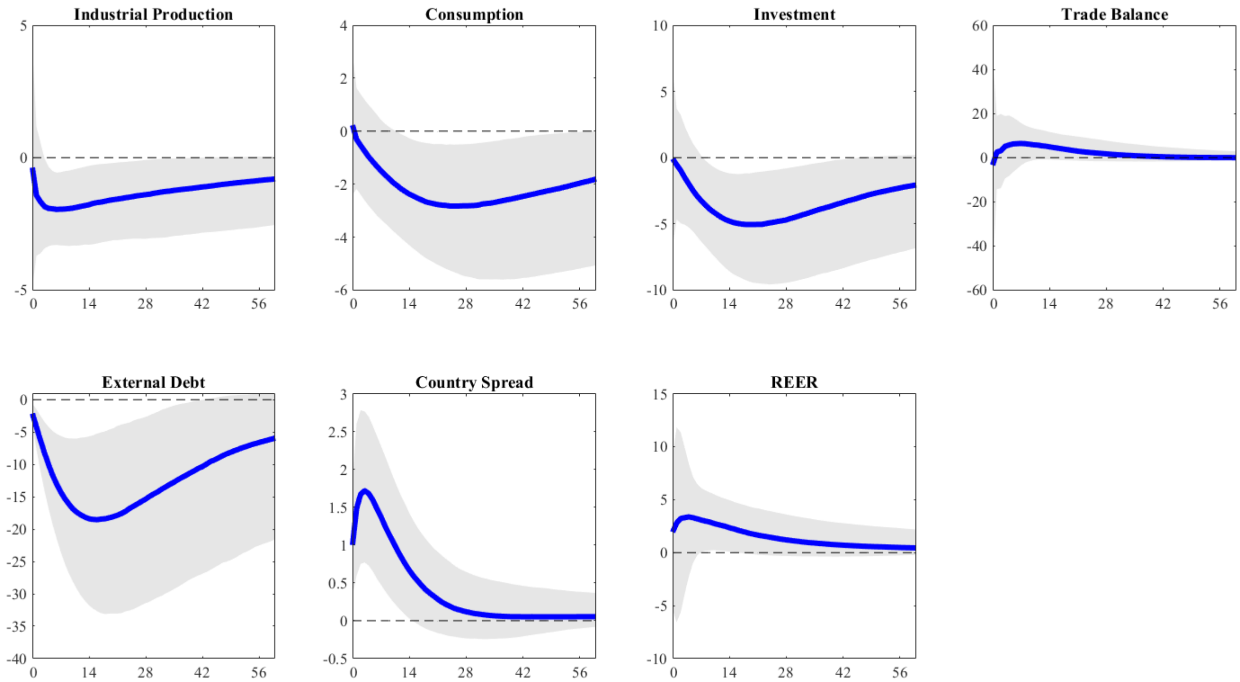


Note: The figure reports the time evolution of the sanctions shock estimated with the BVAR model containing 10 variables. The Bayesian estimates are obtained with the flat (uninformative) prior. We set 10,000 draws from the posterior distribution and we discard the first 5,000 draws. Conventional credible bands comprised of the 16^{th} and 84^{th} percentiles of the post-burned-in estimated IRFs are reported. Substantial spikes in the time series of the estimated shock are identified for the first but not for the second wave of sanctions at the end of 2014 and 2017, respectively. One more is identified for the period of the 2008–2009 global economic crisis and is reported for comparative reasons.

Figure 23. Time evolution of a negative shock to the international credit supply identified under the sign restrictions scheme

With a plausible estimate of the ISC shock, we now turn to analyze the responses of the domestic endogenous variables. The estimation results appear in Fig. 24 below. We report the estimated impulse responses over five years of the domestic variables to a *negative* ISC shock defined above. For the sake of representation, the shock is re-scaled to a +1 pp increase in the country spread on impact, and the responses are re-scaled accordingly.

First, we find that after the initial impulse, the country spread's response peaks at +1.7 pp half a year after the ICS shock and then it attenuates towards zero in the following three years. We also find that corporate external debt, i.e., our second restricted variable, declines by 18 pp one year after the shock. Second, we obtain significantly negative and persistent reactions of the real economy to the ICS shock: industrial production declines by 1.95 pp within half a year after the shock, private consumption falls by 3 pp two years after the shock,



Note: The figure reports the estimated IRFs of domestic macroeconomic variables to the sanction shock identified using the sign restrictions scheme as an international credit supply shock. The IRFs are re-scaled so that the shock is equivalent to a +1 pp rise of $Country.Spread_t$. The BVAR model contains 10 variables: external characteristics—commodity terms-of-trade ($CTOT_t$), the Baa corporate bond spread ($Baa.Spread_t$), the real interest rate in the US economy ($US.Real.Interest.Rate_t$); domestic indicators—industrial production (IP_t), private consumption ($Consum_t$), investments ($Invest_t$), trade balance (TB_t), corporate external debt ($ExtDebt_t$), Russia's country spread ($Country.Spread_t$), the real effective exchange rate ($REER_t$). Monetary policy reaction to the sanctions shock is ignored in this version of the model. The $Country.Spread_t$ variable is ordered second last. The Bayesian estimates are obtained with the flat (uninformative) prior. We set 10,000 draws from the posterior distribution and we discard the first 5,000 draws. Conventional credible bands comprised of the 16^{th} and 84^{th} percentiles of the post-burned-in estimated IRFs are reported (grey shaded area).

Figure 24. Impulse response functions to the international credit supply shock identified under the sign restriction scheme

and investments slump by 5 pp in the second year after the shock.[73] Third, the results show that the international trade balance, in contrast, reacts positively to the ICS shock, which may imply that imports decline by more than exports; however, the estimated reaction is barely significant. Finally, our estimates indicate that REER also rises in response to the ISC shock, peaking at +4 pp in a quarter after the shock. REER depreciates because Russia's economic agents are forced to repay their external debts, which means a greater demand for foreign currencies in Russia and their outflows abroad. Trade balance rises because agents need to earn enough income in foreign currencies to be able to repay their external debts.

Accounting for endogenous monetary policy responses

[73] The more strong reaction of private consumption as compared to industrial production is consistent with the lack of consumption smoothing over the business cycle typically observed in EMEs Neumeyer and Perri (2005); Uribe and Schmitt-Grohe (2017).

As argued by Brunnermeier et al. (2021), we can be sure that we capture the real effects of credit supply shocks only if we properly account for the monetary policy changes in response to such shocks. In our setting, the idea is that negative shocks to international credit supply can provoke rises of credit supply by domestic financial institutions, holding the demand on loans at the same level (substitution channel), which in turn can create upward pressure on domestic prices. Clearly, domestic financial regulators may step in and raise the interest rate to curb inflation. A well-known side effect of this policy is the depression of economic activity. Therefore, we eventually could have a double negative effect on the macroeconomy—one stemming from the international credit supply shock and the other from the monetary contraction. It is a-priori unclear which of the two negative effects dominates and how they relate to each other. To address these concerns, we add the domestic regulated interest rate (in real terms) to the list of endogenous variables employed in the VAR model (13) and re-run the same exercises as in the previous section.

As can be inferred from Fig. 1 in Appendix no. 23, the time evolution of the re-estimated ICS shock remains very close to the baseline. The re-estimated impulse responses show that the Central Bank of Russia indeed tends to raise the key domestic interest rate in response to negative ICS shocks (Fig. 2). The peak increase reaches +1.4 pp half a year after the ICS shock. However, this has only a minor quantitative impact on our previous results: we find that the estimated responses of the other domestic variables remain almost the same as before. For instance, industrial production declines by 1.78 pp at most, which is only 0.17 lower in magnitude than the respective estimate in the previous section, where the domestic regulated interest rate was ignored.

Overall, accounting for endogenous monetary policy reactions to negative ICS shocks leads to only a small reduction of the estimated responses of domestic macroeconomic variables to these shocks. As an alternative and more conventional approach, we also employ *recursive scheme* (Cholesky ordering) and analyze the effects of financial sanctions by isolating innovations to country spread (S) instead of international credit supply (ICS). The results are largely in line with what we obtain with the ICS shock and are reported in Appendix no. 24.

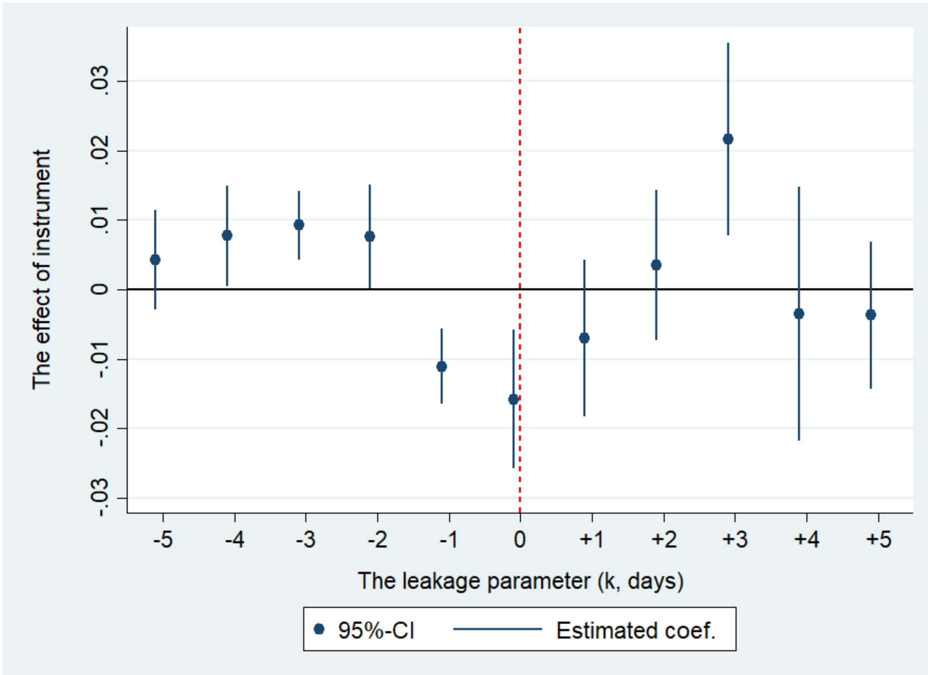
3.4.2 High-frequency identification (HFI) approach: The effects of all sanction packages

We report the first-stage estimation results in Fig. 25, as implied by Equation (21). Strikingly, we obtain positive and highly significant β_k estimate when the leakage parameter is set at three days *before* the sanction announcement ($k = -3$). Moreover, this is the only case when the associated first-stage F-statistic exceeds the threshold of 10 (13.5), meaning that the underlying instrument is not weak. For deeper values of the leakage parameter k we either obtain an insignificant estimate ($k = -5$) or still significant but the corresponding F-statistic falls largely below 10 ($k = -4$). For smaller lags, we either obtain an insignificant positive estimate ($k = -2$) or even a negative and highly significant one ($k = 0, 1$). The negative estimates may indicate a reversal from the (over)selling of bonds at deeper k's to buying those at smaller k's. Apparently, this implies that the financial markets expect harsher sanctions than they ultimately are.

With regard to the *after the announcement* days, we find that YTMs start rising during the first three days, and the associated effect that pertains to the third day ($k = 3$) becomes positive and highly significant. This effect is the highest across all days before and after the announcement, exceeding its counterpart that we find significant at $k = -3$ by a factor of 2. Interestingly, if one is willing to consider the average effect across all $k \in [-3, 3]$ to balance the different forces that take place before and after sanction announcements, then the resultant sum (0.0082) is surprisingly similar to the single effect at $k = -3$ (0.0092). Effectively, this means that the overall inference at the second stage would be the same. We thus stick to the β_{-3} case.

As for the second-stage results, we present the estimated impulse responses to the HFI-based sanction shock in Fig. 26(a)–(h), as implied by the local projection Equation (23). Each subfigure plots the time evolution of the estimated impulse responses $\gamma_{i,h}$ of a given variable $y_{i,t}$ to the HFI shock $\widehat{u}_t^{(S)}$ against the background of the recursively identified SVAR-based shock $\varepsilon_t^{(S)}$. The 95% confidence intervals are computed with bootstrap (500 draws, with replications) to account for the estimated nature of the shocks. The responses are re-scaled to a +1 pp rise in Russia's country spread variable.

We find that industrial production declines faster and two times more intensively in response to the HFI shock than to the country spread shock (a). The peak reaction to the HFI shock reaches –4 pp by the end of the first year after the shock hits (significant at 1%), whereas the maximal response to the country spread shock is only –2 pp that is reached by the end of the second year after the shock. A similar pattern holds for private consumption (b) and investment (c) whose declines reach 3.2 and 5 pp within a year after the shock. For the trade balance, we do not obtain significant results (d) under either the HFI or recursive identification. For external debt (e) we find that the peak reaction is comparable to what we get with the recursive identification (around –10 pp), but again this happens much faster—within the first year (HFI), not the second year (recursive). For the REER (g), we also obtain that Russia's ruble depreciates following the HFI-based sanction shock, as we get under the recursive scheme; however, the peak depreciation is larger, +10 pp,



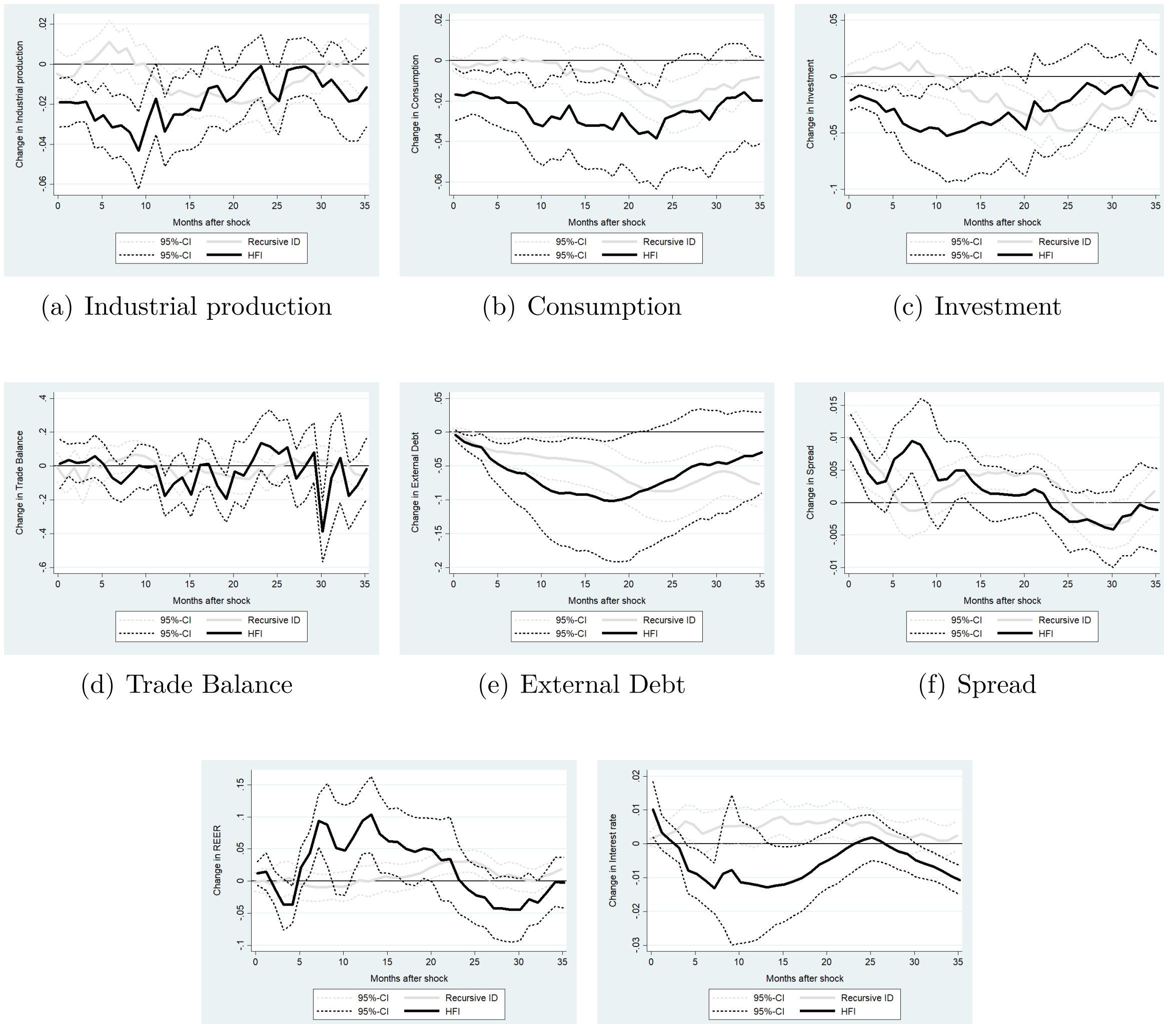
Note: The figure reports the estimation results from the first stage, as implied by Equation (21). A sanction announcement takes place on day 0 (*red line*). The estimated coefficients (*blue dots*) show the effect of sanction announcements on Russia's country spread at *monthly* frequency that runs through the changes in the average yield-to-maturity of Russia's US dollar-denominated sovereign bonds that occur k *days* prior to the sanction announcements ($-5 \leq k < 0$, *leakage*) or after it ($0 \leq k \leq 5$, *delay*).

Figure 25. High frequency identification of the effects of sanctions: 1st stage estimation results

and this happens faster, within a year after the shock. Finally, we estimate that domestic monetary policy accommodates the sanction shock on impact—by raising the key interest rate by 1 pp—but then turns to easing, by 1.2 pp within half a year after the shock (HFI). Overall, under the HFI approach, we find that the reaction of macroeconomic variables to the sanction shock is much deeper and it materializes faster than under the SVAR-based approach.

3.4.3 Summary of macroeconomic estimates

We have so far isolated the trajectories of the shocks to country spread (S) and international credit supply (ICS) using the VAR model (13) and the sanctions news shock using the HFI approach (21)–(23). With these trajectories at hand, we then estimated the peak responses of domestic macroeconomic variables to these shocks and established the spikes in the shocks' trajectories around 2014–2015 (the first wave of sanctions), 2017–2018 (the second wave), and 2022 (the third wave). By exploiting the *peak responses* and the *sizes of the shocks*, we now



(a) Industrial production (b) Consumption (c) Investment

(d) Trade Balance (e) External Debt (f) Spread

(g) REER (h) Interest rate

Note: The figure reports impulse responses to a positive country spread shock identified with the high-frequency approach (*HFI*) and recursive identification scheme (*Recursive ID*). The responses are obtained under Jorda's LP approach, as implied by $\beta_{j,h}$ in Equation (23). The 95% confidence intervals are computed with bootstrap (500 draws, with replications).

Figure 26. Impulse responses to the country spread shock identified under the high-frequency approach

compute the resultant macroeconomic effects of the financial sanctions using Expressions (18) for the first two waves of sanctions (in-sample) and (19) for the third wave (out-of-sample). We report the computation results in Table 6. The table compares the effects obtained under the sign restrictions (*Sign*) in an 11-variable VAR model and under the Jorda (2005) Local Projection (*LP*) and the HFI approach. For comparison, we also report the effects obtained under the recursive identification (*Recurs*). We treat the HFI approach as the one capturing the overall effects of sanctions, whereas *Sign* captures only the effects of financial sanctions.

Table 6. Macroeconomic effects of sanctions on Russia: Estimation summary

Note: The table contains the (median) estimates of the macroeconomic effects across three waves of financial sanctions, as implied by Expressions (18) and (19). The estimates are obtained with the use of either a structural VAR model or Jorda (2005) local projection (LP) and the HFI approach. Under the VAR model, the identification methods are: recursive ordering (*Recurs*) or sign restrictions (*Sign*). *Recurs* identifies a positive shock to Russia's country spread and *Sign* isolates a negative shock to the international credit supply. *HFI* identifies a sanctions news shock that pushes investors to sell Russia's sovereign bonds. D_t is Corporate external debt, IP_t is Industrial production, C_t is Final consumption, I_t is Investment, TB_t is Trade balance, $REER_t$ is Real effective exchange rate.

1st and 2nd wave estimates: *in-sample* predictions (Jan.2000–Dec.2018). 3rd wave estimates: *out-of-sample* predictions (2022) based on (*i*) the realized shock to Russia's country spread during the first weeks of Russia's war over Ukraine and (*ii*) the impulse responses estimated for the period of Jan.2000–Dec.2018.

Sanction wave:	First (2014–2015)			Second (2017–2018)		Third (2022)		
Approach:	SVAR		HFI + Jorda LP	SVAR	HFI + Jorda LP	SVAR		HFI + Jorda LP
ID scheme:	Recurs	Sign		*both*		Recurs	Sign	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
D_t	–20.0	–11.9	–11.2	0	–1.7	$\sim$ –100	$\sim$ –100	–38.5
$REER_t$	+7.0	+2.0	+11.2	0	1.7	+61.3	+17.9	+38.5
TB_t	+4.0	+5.1	0	0	0	+35.0	+44.6	0
IP_t	–3.8	–1.2	–4.8	0	–0.7	–34.1	–11.7	–17.6
GDP_t	–2.5	–0.8	–3.2	0	–0.5	–22.9	–7.9	–11.8
C_t	–4.5	–1.5	–4.5	0	–0.7	–23.9	–8.2	–12.3
I_t	–5.4	–3.4	–5.6	0	–0.9	–51.2	–17.6	–26.5

Corporate external debt. We start the description of our results with the effect of sanctions on the targeted variable—(corporate) external debt (D_t) of the Russian economy. Our estimations indicate that this characteristic is the most responsive variable across all of Russia's macroeconomic variables. With the HFI approach, we estimate that the corporate external debt declines by 11% in response to the first wave of sanctions, as cumulative during 2014–2015.[74] This accounts for roughly half of the overall decline of corporate external debt during that period. Given that the strength of the second wave of sanctions is much lower, we get that its effect on the corporate external debt is just –1.7%.[75] During the third wave of sanctions, the shock is so large that our model predicts a 40% decline of corporate external debt in 2022 in response to the 'tsunami' sanctions. Notably, the estimates that we

[74]The effect is computed as the product of the first stage coefficient (0.93), the cumulative sanctions news shock over the period (1.20), and the peak response estimated at the second stage (-10).

[75]The effect is computed similarly to the previous one, with the size of the sanctions news shock being replaced from 1.20 by 0.18.

obtain under the *Recurs* and *Sign* approaches predict a complete shutdown of international borrowings for Russia's economic agents. This clearly speaks in favor of the HFI approach whose results are more realistic, given that Russian firms may still (though partly at best) substitute Western financial funds with those attracted from Asian financial markets.

International trade and exchange rate. Clearly, sanctions force Russia's economic agents to accelerate payments on their external debts. To be able to repay, the agents have to earn relatively more income from international trade—or the government has to support them directly—and then service the external debts. This must cause the outflows of foreign currencies from Russia and, eventually, lead to a depreciation of the ruble.

As our computations under the HFI approach show, the real effective exchange rate ($REER_t$) of the ruble depreciated by 11% in response to the first wave of sanctions in 2014–2015, by another 2% due to the second wave, and by roughly 40% in response to the sanctions news shock in February–March 2022. These estimates mirror those for the corporate external debt that we have just described above. As for the trade balance (TB_t), our HFI estimates produce a zero reaction to the sanctions news across all three waves, meaning that the associated declines in exports could be as large as the declines in imports. If so, then in order to repay external debts, the agents have to appeal for government support. As has been recently shown by Nigmatulina (2022), the government support channel was indeed strong over the above periods. We note, however, that the two other approaches we use, *Recurs* and *Sign*, deliver different results that are consistent with the agents' abilities to repay the debts using growing income from international trade. That is, under these two approaches, Russia's trade balance increased in response to the first and third waves of sanctions. We assume both channels were at work.

Industrial production and GDP growth. Given the depreciation of the ruble and the decline in international borrowings by Russian firms in response to the sanctions, we can anticipate real negative effects on the Russian economy.[76] Indeed, with our HFI approach, we estimate that industrial production in Russia could have lost nearly 5% in response to the first wave of sanctions cumulatively in 2014–2015.[77] This accounts for 63% of the overall decline in industrial production during that period (–7.6%). The effect of the second wave is much (six times) smaller and equals just 0.7% of lost industrial production in 2017–2018. This explains 32% of the overall decline in industrial production over the respective period (–2.3%). Conversely, when it comes to the third wave, the estimated effect turns dramatically

[76] The firms heavily relied on international borrowings as a source of funds: corporate external debt was equivalent to 30% of GDP in Russia on the eve of the first wave of sanctions.

[77] The effect is computed as the product of the first stage coefficient (0.93), the cumulative sanctions news shock over the period (1.20), and the peak response estimated at the second stage (-4.3).

high: minus 17.6% of losses in terms of industrial production dynamics in 2022, which is roughly four times larger than during the first wave and twenty-five times larger than in the second wave.[78] We stress that, by the construction of our local projection equation (23), these estimated effects go beyond the effects of CTOT movements and monetary policy responses to changing prices that occurred during the three waves of sanctions.[79]

Given that monthly data on GDP does not exist, we uncover the effects of the three waves of sanctions on Russia's final output by using a simple linear mapping from industrial production to GDP estimated at the quarterly frequency (0.67, significant at 1%, see Appendix no. 29). With this mapping, we find that real GDP in Russia could have lost 3.2% during the first wave of sanctions in 2014–2015, 0.5% in 2017–2018, and that in could lose nearly 12% in 2022. Importantly, one should not confuse these estimates with the overall forecast of GDP dynamics in the respective years. Instead, these estimates capture the potential of initial sanctions shock: a pure sanction effect originating from the size of the sanctions news shock to Russia's US dollar-denominated sovereign bonds that had occurred on the eve of the sanction announcements. These estimates thus do not take into account responses to sanctions by the Russian government, the Central Bank of Russia, and the international partners across the world that help Russia to evade the universe of global restrictions.[80]

In almost all cases, our estimates exceed those in the literature (between 0% in Kholodilin and Netsunajev (2019) and –1.5% in Barseghyan, 2019), analyst reports (–0.2% by the IMF, 2015), and our own estimates obtained with the use of the VAR models under *Recurs* or *Sign*. For instance, under the *Sign* approach, we estimate that the effect of the ICS shock on GDP is 2.4 pp less strong during the first wave of sanctions and 3.9 pp less strong during the third wave.[81] We argue that these discrepancies in the estimated effects arise exactly

[78] The estimate is built up similarly to the two previous ones, with the sanctions news shock being replaced by 4.14. The computation is $100\% \cdot \left(\left(1 + \frac{1}{100} \cdot 0.0093 \cdot 4.14 \cdot (-4.3)\right) \cdot \left((1-0.007)(1-0.006)\right) - 1 \right) = -17.6\%$. Here, we have also accounted for the monthly growth rates of industrial production that we observe for the pre-war January and February 2022 (−0.7% and −0.6%, respectively) before the data was closed by the Russian government as the war raged.

[79] As an example of relative contribution during the first wave of sanctions, we find that the oil price slump and monetary contraction explain jointly 4.4 pp of the decline of industrial production. This means that together with the sanctions news effects (4.8 pp), the three shocks accommodate a 9.2 pp decline in industrial production over those times. Recall that the actual decline equals 7.6%. This implies that a conservative estimate of the strength of the government support channel (when the Russian government was supporting sanctioned firms, see Nigmatulina, 2022) can be equivalent to at least 1.6 pp.

[80] For the overall forecasts of Russia's GDP, one could be directed to the IMF predictions produced, e.g., in August 2022, according to which Russia's GDP could lose around 6% in 2022. At the moment of this text writing in mid-2023, the final figure for 2022 is just –2%. Therefore, one can compare our estimate of the sanction potential, –12%, and this final figure, –2%, and think of the difference, i.e., 10 pp, as of the effect of sanction evasion in 2022.

[81] Note that the size of the ICS shock is not observed in 2022 because we do not have the data on the amount

because we use an innovative measure of the sanctions shock stemming from the news on upcoming sanctions which we accommodate with the HFI approach (*external instrument*) rather than using the ICS concept originating from the residuals of (VAR) regressions based on macroeconomic time series themselves.

Consumption and investment. With the HFI approach, we find that the sanctions news shock could have led to a decline in private consumption of 4.5% and investment of 5.6% during the first wave of sanctions, as cumulative over 2014–2015. This implies much stronger negative reactions than that of GDP which we described above—by 1.3 and 2.4 pp on magnitude, respectively. We then obtain the negative effects on consumption and investment turn substantially milder (five to six times) during the second wave in 2017–2018, being bounded by –1% and still exceeding the effect on GDP. Our computations then indicate that the sanctions news shock at the beginning of the third wave in February–March 2022 is able to trigger a slump in consumption of 12% and investment of more than 25%, which are comparable only to the effect of the USSR collapse in the early 1990s.[82]

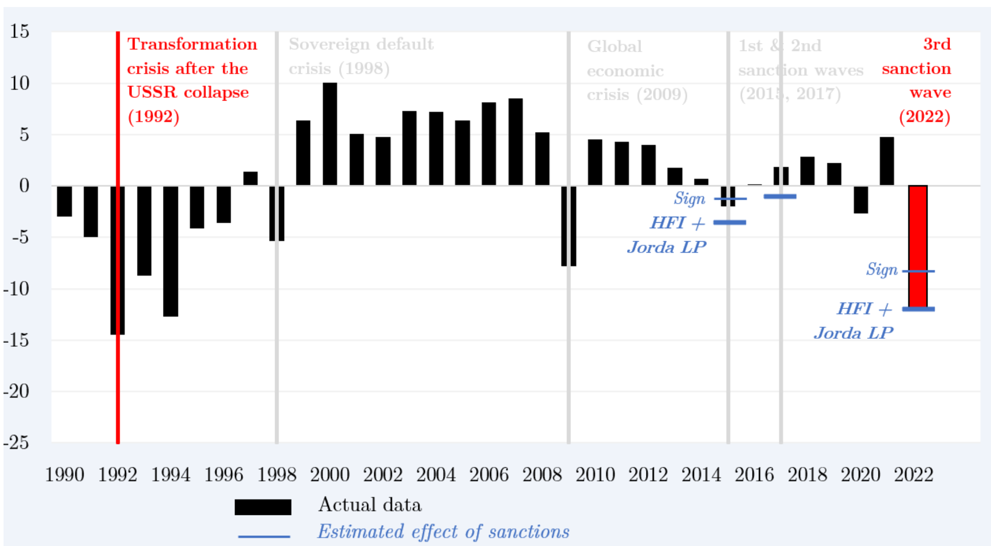
Again, as was the case with GDP, our estimates obtained with the *Sign* approach indicate a less strong reaction of both consumption and investment than those obtained with HFI. Clearly, this highlights the key difference between the two approaches: while *Sign* captures only the reduced *supply* of international funds, *HFI* encompasses both the reduced supply of these funds and the depressed *aggregate demand* in the economy due to negative feedback loops. Put differently, sanctions first shrink the supply of international finance—this then raises the likelihood that firms' and households' borrowing constraints become binding—this, in turn, forces consumption and investment to shrink, and thus the agents demand less in the economy than before the sanctions hit. We argue that the expectations of this chain of events by financial markets are included in the prices of Russia's sovereign bonds and thus are fully captured by our HFI approach, whereas *Sign*, by construction, ignores the demand side of the story.

Our results are partially consistent with the cross-country event-study estimates of Gutmann et al. (2021), who find that consumption falls by 2.2% during the first year after sanctions while investment decreases by 24% in two years after the sanctions. Our results point to more equal reactions of consumption and investment to the international financial sanctions.

of external debt decline. To overcome this issue, we assume that the ratio between the peak magnitudes of the country spread and ICS shocks remains constant in time—in 2014 when both are observed, and in 2022 when only the country spread is observed. This allows us to uncover the assumed size of the ICS shock in 2022 and compute the effect on industrial production and other macroeconomic variables.

[82]These out-of-sample computations exploit a linear mapping between private consumption and industrial production (0.69, see Appendix no. 29) and between investment and industrial production (1.5, see also Appendix no. 29), respectively.

Overall, we document that financial sanctions have multiple effects: they not only change the flows of international borrowing funds but also have significant real effects on the domestic (sanctioned) economy. These effects clearly depend on the size of the sanction shock and on whether and how much other shocks affect the economy at the same time. But even in 2014–2015, when the Russian economy encountered a deep negative oil price shock and similarly deep restrictive monetary response, we show that the sanctions were still responsible for at least 50% of the total decline in industrial production and GDP. In 2022, by contrast, external conditions were more than favorable but the sanction shock was unprecedentedly high causing the largest decline in the economy since the collapse of the USSR (Fig. 27). Of course, the latter estimate should be perceived as a pure effect of the sanctions *prior to* the Russian government's response to the shock, including the imposition of capital controls by the Central Bank of Russia in early March 2022.



Note: The figure reports the time evolution of real GDP growth rates over the last 30 years in Russia and marks the episodes of economic crises. *Sign*, *Jorda LP* and *Recurs* are the methods we apply to obtain the estimates of the effects of sanctions: sign restrictions (17), Jorda's local projection (23), and recursive identification (40).

Figure 27. Sanctions and the history of business cycles from the collapse of the USSR until the war in Ukraine, 1990–2022

3.4.4 Other robustness checks

The rest of the sensitivity analysis is devoted to understanding how much our estimated impulse responses depend on the modelling assumptions and data transformation.

First, instead of imposing the sign restrictions (17) on impact, we assume a wider time period during which the restrictions must hold. We consider 1, 2, and 3 months when estimating the VAR models using the approach of Antolin-Diaz and Rubio-Ramirez (2018). In all cases, we obtain virtually the same time series of the estimated ICS and country spread shocks and the patterns of impulse responses. The quantitative differences with respect to the baseline results are negligible (available upon request).

Second, the empirical macroeconomic literature that relies on frequentist (i.e., non-Bayesian) estimation methods typically exploits detrended time series to ensure stationarity and comparability with theoretical literature (Akinci, 2013). Though we apply the Bayesian methods that are robust to non-stationarity in the data, we also perform a portion of VAR estimates with HP-detrended time series. The results obtained under the recursive identification scheme appear in Appendix no. 25 and those under the sign restriction in Appendix no. 26. Qualitatively, we obtain the same results as in the baseline: real variables—industrial production, private consumption, investment—contract, trade balance improves, REER appreciates, and external debt and the domestic regulated interest rate rise. Only one exception is the response of investment in the recursive case, which turns positive but remains insignificant during the whole prediction horizon. Quantitatively, the recursive case delivers significant responses, whereas the sign restrictions produce mostly insignificant responses when the data is HP-detrended. Under the recursive case, interestingly, the estimated responses are 2 to 3 times lower in magnitude as compared with the baseline (Fig. 1), and the size of the shock in 2014 is also lower by 1 pp than in the baseline estimates (Fig. 2).

Third, we run a more parsimonious model—a five-variable VAR from Uribe and Yue (2006)—and perform the recursive identification of the country spread shock. We report the results in Appendix no. 27, which indicate that output falls by slightly more (–1.1 pp) than in our 11-variable VAR specification. Investment, by contrast, falls slightly less (–1.1 pp) than in the baseline. In this regard, the results are very much robust. However, with regard to the trade balance, we encounter a wedge in the results: in the five-variable VAR, we find that the trade balance reacts negatively, not positively, to a positive country-spread shock. This contradicts the theory that we use (Uribe and Yue, 2006; **?**; Uribe and Schmitt-Grohe, 2017). Clearly, for an export-oriented economy like Russia, being strongly dependent on the export prices of fuel goods, omitting commodity terms-of-trade as well as REER may pose a serious challenge for recovering the full space of shocks. Nonetheless, even in this case the estimated time evolution of the identified shock to country spread still allows us to recognize a substantial spike in 2014 (the first wave of sanctions) and no significant shocks in 2017 (the second wave of sanctions).

Fourth, we use Jorda's LP approach to re-estimate the impulse responses obtained with our

VAR model (13). The estimation results are reported in Fig. 1.(a)–(h) (see Appendix no. 28). Each subfigure plots the time evolution of the estimated impulse responses of a given variable $y_{i,t}$ to the shock $\hat{\varepsilon}_t^{(j)}$ that is computed either with the recursive or sign restriction schemes. We find that in most cases (except for industrial production), the results are quantitatively larger under the VAR than Jorda's LP methods but remain qualitatively the same. Therefore, we conclude that our baseline results are supported by Jorda's LP approach and are thus robust to misspecification.

3.5 Results: Cross-sectional effects of sanctions

Having established significant macroeconomic implications of the financial sanctions for the Russian economy in 2014–2015 and 2022, we now ask how the aggregated sanctions shock affects the cross-sections of households and firms. We are specifically interested in the heterogeneity of the effects of sanctions. We may expect that the current sanctions have larger negative effects on the economic agents that are less likely to support the political regime in Russia: richer households in large cities, as they may have international assets and are more competitive in international labor markets, and more productive firms, as they are more likely to be well-integrated into the world economy. Conversely, the sanctions are less likely to hit the regime's proponents: poorer households in rural areas and local firms with lower levels of productivity.

3.5.1 Sanctions and the cross-section of firms

We collect firm-level data from the SPARK-Interfax database over the period from 2012 to 2018.[83] We require firms to simultaneously have non-missing non-negative values on total assets, total revenue, value added, number of employees and wages, capital and intermediate inputs (materials), and bank and non-bank borrowed funds. We also require the firms to operate for at least three consecutive years. The final sample consists of 7,460 large and small firms resulting in 40,381 firm–year observations over the period of 2012–2018.[84] The firms operate in as many as 16 different sectors of the Russian economy (two-digit classification)

[83]See `https://spark-interfax.com/`.

[84]Initial sample consists of roughly 300,000 firms. The substantial decline in the number of firms is caused by many missing values on employee and wage data in the firms' balance sheets and the requirement to work for at least three years in a row. We cannot remove the condition imposed on employees and wages because this data is essential for estimating TFP. If we remove the condition on at least three years of operations, then the number of firms rises to 32,790, i.e., by a factor of four, and the number of firm–year observations increases to 81,004. The results on the cross-sectional effects of sanctions do not change in this case (see below). We prefer to keep this condition to relax the 'survivorship bias' problem.

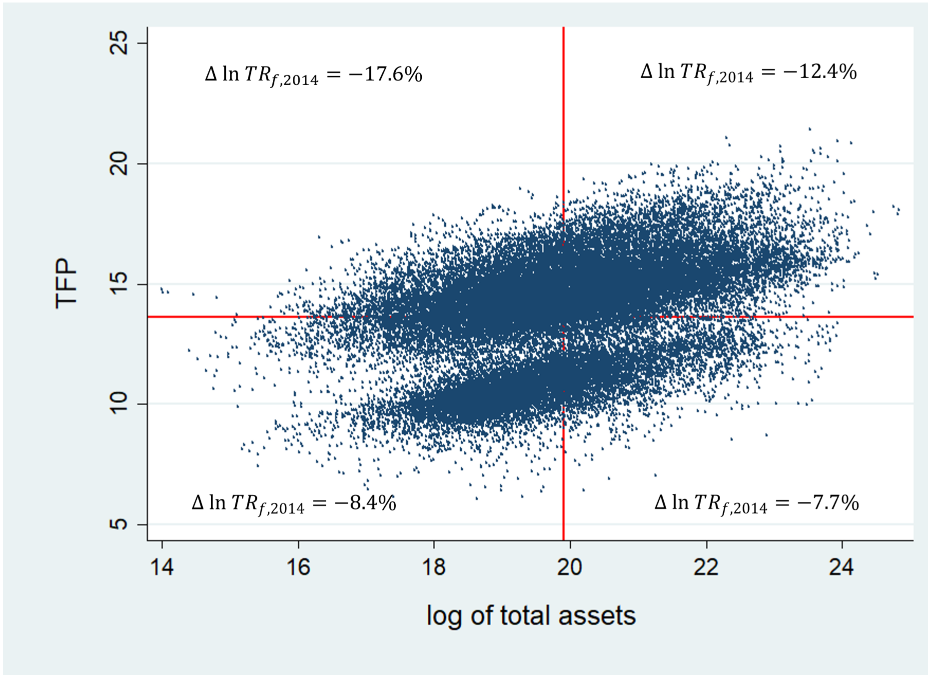
ranging from natural resources extraction to IT.

With this data at hand, we estimate the firms' TFPs by applying a popular methodology proposed by Wooldridge (2009) and Petrin and Levinsohn (2012). We assume a Cobb-Douglas production function with the real value added as the dependent variable and labor, capital, and materials as the explanatory variables. We also impose constant returns to scale. The summary statistics on the variables employed in the estimation and the estimates of firm productivity $TFP_{f,t}$ appear in Table 1 (see Appendix no. 30). The estimates show that the magnitude of productivity averaged across all firms and years equals 13.6, being bounded between 6.1 and 21.4 and thus indicating a large variation in firms' $TFP_{f,t}$ (note that the mean magnitude of the real value added is 18.5). Plotting the time evolution of the firms' distribution by $TFP_{f,t}$ and size, as proxied with the log of the firms' total assets (in constant prices) $\ln TA_{f,t}$, we observe a slightly positive trend in the firms' productivities, despite the sanctions shock in 2014, and a visible negative trend in the firms' size, especially during 2014 (see Fig. 1.(a) and (b) in Appendix no. 30). In both cases, the observed variation across firms remains large and stable over time.

Given the estimated firms' $TFP_{f,t}$ and sizes $\ln TA_{f,t}$, we divide our sample into four parts: (*i*) large firms with high TFP ($N\,obs = 14,126$), (*ii*) large firms with low TFP ($N\,obs = 5,198$), (*iii*) small firms with high TFP ($N\,obs = 9,684$), and (*iv*) small firms with low TFP ($N\,obs = 11,373$). We use the mean value of $\ln TA_{f,t}$ to separate 'large' and 'small' firms. 'High' and 'low' productivities are defined accordingly using the mean value of $TFP_{f,t}$. Fig. 28 visualizes the resultant four cells of firm–year observations and reports the growth rate of firms' total revenue (in constant prices) during the first year of the financial sanctions in each of the four cells. In line with the anecdotal evidence discussed above, we indeed observe that holding the firms' size constant, more productive firms faced larger declines in real revenues than less productive firms. Regarding large firms, more productive firms experienced a 12.4% decline in real revenues in 2014 while less productive firms encountered only a 7.7% drop during the same year. Concerning small firms, more productive firms reported a 17.6% slump in real revenues in 2014 while less productive firms experienced only an 8.4% reduction over the same period. These figures also imply that larger firms were able to better support their revenues than smaller firms, and more so for more productive firms.[85]

Clearly, the raw data shows that all firms in Russia experienced a deterioration of their real revenues in the first year of the Crimea-related sanctions. However, given the negative

[85] An interesting side outcome from Fig. 28 is a clustering of the scatter-plot: there are two clusters of firms—more productive and less productive, given the same firm size. Probably, this could be related to the exporting statuses of the firms. Our data, however, does not allow us to elaborate more on this topic. We leave it for future research.



Note: The figure reports the scatter-plot of 40,381 firm–year observations (7,460 firms over the 2012–2018 period) on the log of total assets (in constant prices, X axis) and firms' TFPs, as estimated using the Wooldridge (2009) and Petrin and Levinsohn (2012) approach (Y axis). The horizontal and vertical red lines mark the mean levels of the firms' TFPs and total assets, respectively. For each of the four resultant cells, the figure also reports the growth rate of real total revenue $\Delta \ln TR_{f,t}$ that the firms reported in their balance sheets by the end of 2014, i.e., the first year of the Crimea-related sanctions.

Figure 28. Firm size, TFP, and decline in firms' real income during the first year of sanctions

CTOT shock and restrictive monetary policy stance in 2014, we ask what part of the deterioration could be attributed to the sanctions. From the previous literature, we only know that the firms directly targeted by the sanctions encountered a more pronounced decline in employment and sales compared to non-targeted firms (Ahn and Ludema, 2020; Crozet et al., 2021). We take a broader perspective and apply the Jorda (2005) LP approach to reveal the effects of sanctions in the four cells of firms outlined above.

For the purpose of estimation, we employ the same regression (23) as before but adapted to the firm level. The dependent variable $y_{f,t+h}$ is the log of total revenue (in constant prices). The key explanatory variable remains the same—either the *ICS* or *country spread* shocks isolated from our VAR models. The monthly estimates of the shocks are aggregated to the annual level by means of summation within each year. We also adjust the control variables $\mathbf{X}_{f,t}$ so that they contain the current values of the total revenue, number of employees, investment, and, importantly, the interest expenses on the firms' loans from domestic banks and the CTOT variable. The two last variables are intended to capture the restrictive monetary policy of the Central Bank of Russia in 2014 and the tumble in world oil prices during the same period.

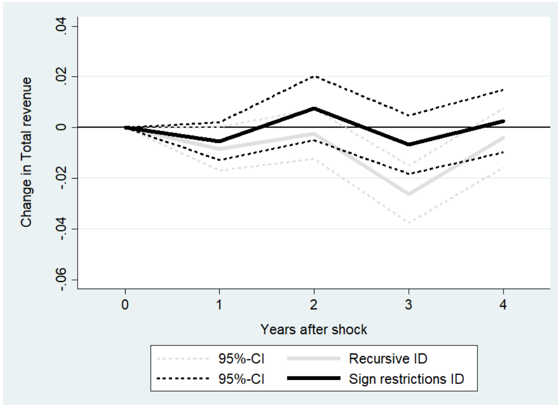
The estimation results appear in Fig. 29. The figure contains the same four cells of firms and in the same order as in Fig. 28 above. First, the estimates suggest that during the first year of the financial sanctions (*ICS*) shock, the real total revenue of the *large firms with high TFP* and *large firms with low TFP* both decline by 2.0–2.2 pp. These are equivalent to 16% and 29% of the total decline in real revenues of these firms, respectively.[86] This means the sanctions had economically significant effects on the performance of large firms in Russia beyond the effects of the oil price collapse and monetary contraction back in 2014. Interestingly, for the large firms with high TFPs, the effect of the sanctions turns to declining from the 2nd year, though still significant, whereas the same effect for the large firms with low TFPs continues to expand, reaching almost -4%. This implies that productivity matters for the absorption of the effect of the sanctions. Starting from the 3rd year, the effect on both types of firms attenuates to zero. We also note that the results remain the same, and even stronger quantitatively, if we consider *country spread* instead of ICS shock.

Second, the estimates indicate that, during the first year after the ICS shock, the real revenue of the *small firms with high TFP* decreases by 0.5 pp. This decrease, however, is insignificant. During the 2nd to 4th years after the ICS shock, the estimated response remains close to zero and insignificant. (Only in the case of the country spread shock does the response turn significant during the 3rd year, but we treat it with caution). Qualitatively, almost the same results pertain to the group of *small firms with low TFP*: during the first year after the ICS shock their real revenue declines by 1 pp (or 12% of the overall decline in 2014),[87] but this effect is much lower than for the large firms and it turns virtually zero from the 2nd year onwards. Strikingly, the small firms experienced much larger total declines in their real revenue than did the large firms (recall Fig. 28) but, as our estimates suggest, these declines are barely explained by the sanctions.

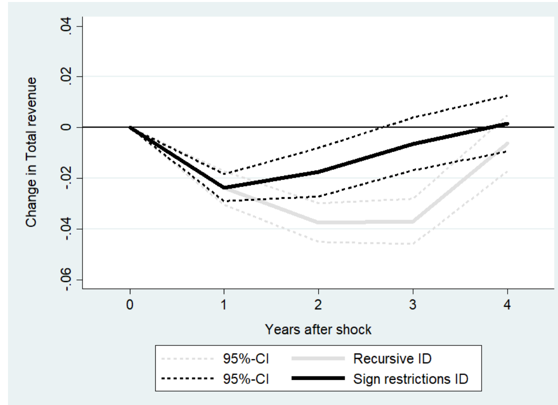
Finally, we argue that the results remain the same if we drop our condition that the firms in the sample must operate for at least three years. Indeed, as can be inferred from Fig. 2, the sanctions shock negatively affects large firms, and less so if TFP is higher, and the shock has virtually no effect on smaller firms, regardless of their TFP (see Appendix no. 30).

[86] The shares are computed as $-\frac{2.0}{12.4} = -0.16$ and $-\frac{2.2}{7.7} = -0.29$.

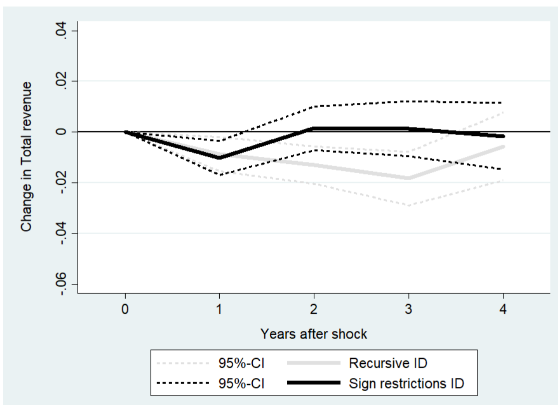
[87] The share is computed as $-\frac{1.0}{8.4} = -0.12$.



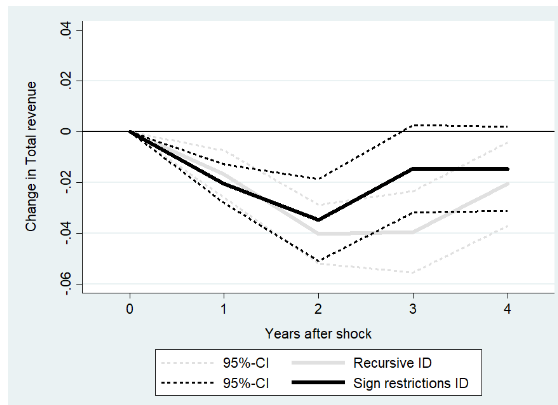
(a) Small firms with high TFP



(b) Large firms with high TFP



(c) Small firms with low TFP



(d) Large firms with low TFP

Note: The figure reports the impulse responses of the firms' total revenues (in constant prices) to the imposition of sanctions, as measured with the ICS (*Sign restrictions ID*) and country spread (*Recursive ID*) shocks. The responses are obtained using Jorda (2005) local projection approach. The sample contains 40,381 firm–year observations for 7,460 firms over the period of 2012–2018. The condition that the firms must operate for at least three consecutive years is imposed. The monthly estimates of the ICS and country spread shocks, as measures of the financial sanctions, are aggregated to the annual level by summation of the monthly magnitudes within a given year.

Figure 29. The effects of the sanctions shock on the real total revenue in a cross-section of firms

3.5.2 Sanctions and the cross-section of households

To test the hypothesis that richer households in larger cities were more adversely affected by the sanctions than poorer households in rural areas we need appropriate survey data. This data comes from the RLMS-HSE database, a rich survey of 5,000 Russian households that the National Research University "Higher School of Economics" has been conducting across

Russia since 1994.[88] We extract the data on income and consumption for the period from 2006 to 2018 and winsorize the data below 1 and above 99%-tiles, which resulted in 21,813 individuals from different households and 74,356 observations in total.

The data allows us to trace the place of living and total income of each individual, among other things. The breakdown of the 74,356 observations that we have for the analysis is as follows: 31,266 pertain to a region's capital city (*Region's capital*), 20,836 belong to large towns other than the capital (*Large town*), 4,460 are in smaller towns (*Small town*), and 17,794 are attributed to rural areas within a region (*Rural*). Mean annual income across the four locations is, respectively, 483.6, 416.7, 422.4, and 371.9 thousand rubles (in constant 2014 prices).[89]

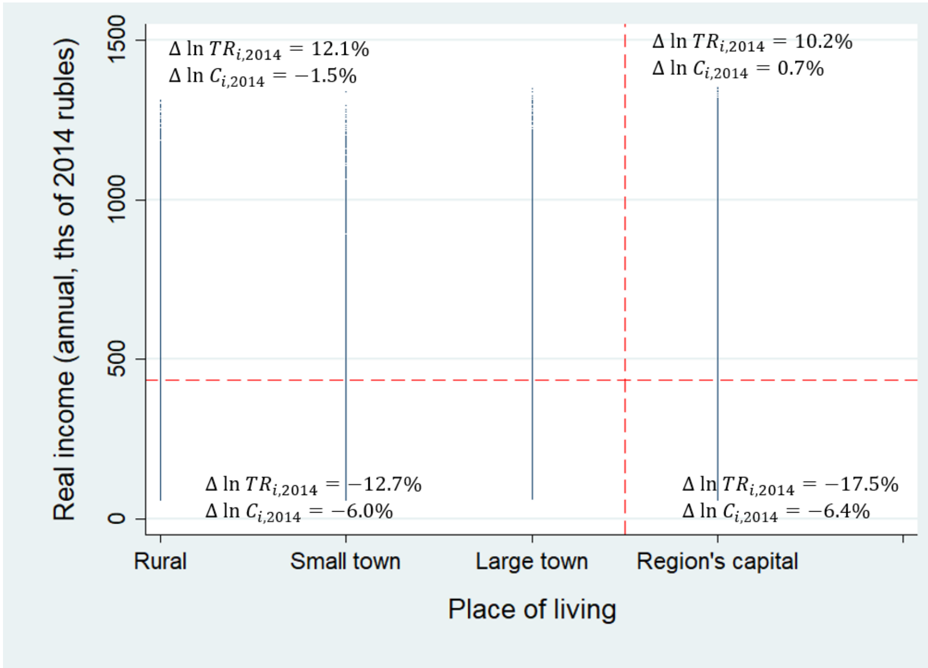
With these preliminaries at hand, we divide all observations into four cells: (*i*) richer individuals residing in regions' capital city ($N\,obs = 31,266$), (*ii*) poorer individuals residing in regions' capital city ($N\,obs = 20,836$), (*iii*) richer individuals residing in regions' other locations ($N\,obs = 4,460$), and (*iv*) poorer individuals residing in regions' other locations ($N\,obs = 17,794$). Within these four cells, the raw data shows that richer households experienced growing, not declining, income during the first year of sanctions in 2014, whereas poorer households suffered from a substantial decline in income (Fig. 30). Indeed, the annual growth rate of real income equaled 10.2% and 12.1% for richer households residing in regions' capital cities and other places, respectively. Conversely, the same figures for the poorer households were –17.5% and –12.7%, respectively. These numbers imply that the overall variation in real income rose dramatically during the first wave of the sanctions.[90] However, as was the case with firms in the previous section, the question arises as to whether we can fully attribute this increased variation in households' income to the effects of sanctions, given the other important adverse shocks hitting the households during the same period (oil price slump and monetary tightening).

Interestingly, the rise in real income of the richer households in 2014 was apparently not enough to sustain their consumption—the annual growth rates of real total consumption were either positive but low or even negative. For the poorer households, the growth rates of real total consumption were even much more negative, implying a substantial decline in

[88] The data is representative and has been already used in many different areas of economics research, see, e.g., Yakovlev (2018).

[89] These numbers are equivalent to 11.6, 10.0, 10.2, and 8.9 thousand US dollars, assuming the exchange rate of 41.57 rubles per US dollar, as an average over 2006–2018.

[90] Though it is out of the scope of this chapter and our data does not allow us to explore this issue, we can cautiously assume that richer households possess substantially higher savings denominated in foreign currencies than poorer households back in 2014. The ruble lost 90% of its value against the US dollar during that year.



Note: The figure reports the scatter-plot of 74,356 individual–year observations (21,813 individuals over 2006–2018) on the place of living (X axis) and annual real income (Y axis). The horizontal dashed red line marks the mean levels of the individuals' income. The vertical dashed red line separates observations on the individuals residing in a region's capital city from the others living in either rural areas, small or large towns different from the capital. For each of the four resultant cells, the figure also reports the growth rate of real income and total consumption by the end of 2014, i.e., the first year of the Crimea-related sanctions.

Figure 30. Individual income, consumption, and the place of living in a cross-section of households

their standards of living during the first year of sanctions (Fig. 30).[91] It is also important to understand how the financial sanctions impacted total consumption and its components, consumption of durables and non-durables. We report the descriptive statistics on these variables in Table 1 (see Appendix no. 31).

To answer the question as to how the financial sanctions affect different parts of the population, we again explot the *ICS* and *country spread* shocks and apply the Jorda (2005) LP approach, as we did for firms in the previous section. The estimation results appear in Fig. 31. The figure contains the same four cells of households and in the same order as in Fig. 30 above.

Richer households. The estimates suggest that the real income of richer households does not respond to the sanctions (*ICS*) shock during the first year after the shock occurs. However,

[91] Again, though our data does not contain this information, we can assume that all households had to substantially increase the interest payments on their loans, given that the key interest rate had been raised by the Central Bank of Russia from 5.5 to 17% during 2014. This dramatic rise in the price of money had negatively affected the households' consumption at that time, as the literature on consumption and monetary policy predicts (Cloyne et al., 2019).

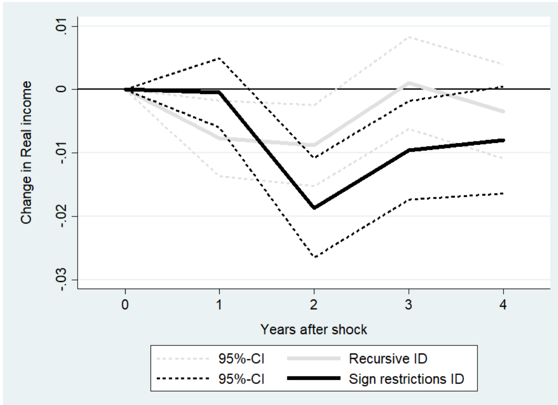
during the second year after the shock, the real income declines by 1.5 pp if the households live in regions' capital cities, and by 2.0 pp if they live everywhere else (all estimates are significant at 5%). Interestingly, in regions' capital cities, the effect on real income persists in time, remaining negative and significant even during the 3rd and 4th years after the shock. For the other places of living, by contrast, the effect on real income weakens starting from the 3rd year. If we switch to the country spread shock, we get an even more pronounced contraction of income during already the 1st year after the shock. A further disaggregation analysis reveals that the effects on total consumption of the richer households are driven by the reduction in the consumption of non-durables, while the consumption of durables was barely affected by the sanctions (Fig. 1, see Appendix no. 31).

Poorer households. Strikingly, the estimates further indicate that the real income of poorer households responds positively, not negatively, to the ICS shock during the first year. The positive reactions are 1.2 pp for the poorer households residing in regions' capital cities and 1.1 pp for the poorer households everywhere else (all estimates are significant at 5%). However, during the second year after the ICS shock, the reactions flip the sign negative, reaching –1.5 and –2.1 pp, respectively (all estimates are significant at 5%). The 3rd and 4th years' reactions vanish and are insignificant. As is the case with richer households, our further disaggregation analysis shows that the positive effect of sanctions during the first year is triggered by rising consumption of non-durables (Fig. 1, see Appendix no. 31).

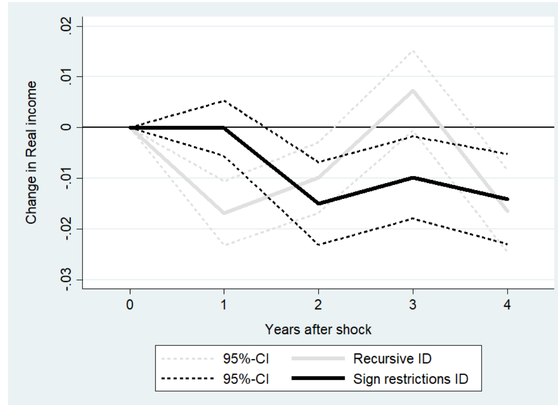
By pooling the results for richer and poorer households together, we argue that the financial sanctions could have the unintended effect of reducing income inequality. This is because the sanctions could have (partly) closed the doors for the international businesses of richer households while forcing the Russian government to support poorer households through the redistribution of income and taxes. The government support channel is established by the micro evidence from Mamonov et al. (2021) and Nigmatulina (2022).

Indeed, recall from our description of the raw data above that richer households enjoyed growing real income in 2014, whereas poorer households suffered from a slump in their income. As Ananyev and Guriev (2018) show, a decline in income causes the destruction of trust in the government in Russia. Moreover, as the findings of Simonov and Rao (2022) suggest, an average consumer of (state-owned) media news in Russia—at least back in the 2010s—has a distaste for pro-governmental ideology. This, when coupled with the declining income of poorer households, may have produced a large negative unintended effect on the Russian government, which is clearly not what the Kremlin's policy aims to achieve.

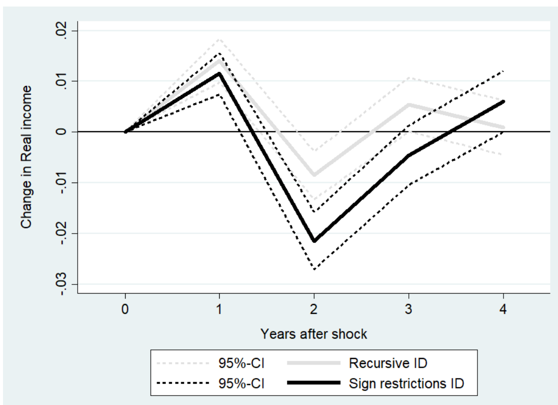
Our findings contrast with those of Neuenkirch and Neumeier (2016), who reveal that US sanctions typically led to a rising poverty gap in the sanctioned countries in the past, i.e.,



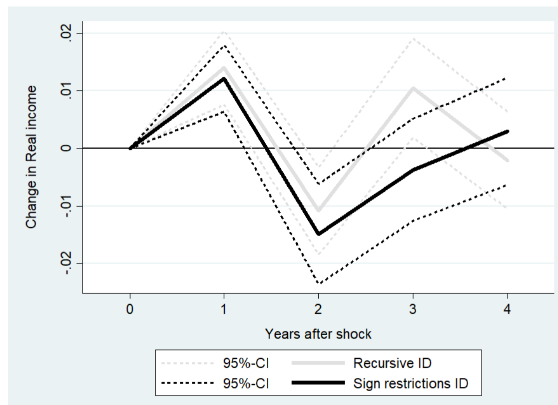
(a) High income, region's other places of living



(b) High income, region's capital city



(c) Low income, region's other places of living



(d) Low income, region's capital city

Note: The figure reports the impulse responses of the individuals' income (in constant prices) to the imposition of sanctions, as measured with the ICS (*Sign restrictions ID*) and country spread (*Recursive ID*) shocks. The responses are obtained using Jorda (2005) LP approach. The sample contains 74,356 individual–year observations for 21,813 individuals over the period of 2006–2018. The monthly estimates of the ICS and country spread shocks, as measures of the financial sanctions, are aggregated to the annual level by summation of the monthly magnitudes within a given year.

Figure 31. The effects of the sanction shock on the real income in a cross-section of households

prior to the Crimea-related restrictions. The authors did not account for the potential support for the poorer population by the sanctioned government. The unintended effect of reducing income inequality that we find is, however, unlikely to persist over time, since our estimates show that the positive effect on the poorer households lasted for only one year.

4 Country spread shocks, sudden stops, and business cycle fluctuations in emerging economies

4.1 Introduction

In this chapter I study the role of country spread shocks in business cycle fluctuations in emerging economies. Country spread shocks are defined as unexpected movements in an interest rate paid by domestic lenders for foreign borrowings relative to the world rate of interest. Below I use the terms *country spread* and *country interest rate* as synonyms, because the world interest rate is exogenous with respect to any emerging economy, and shocks to the first perfectly transmit to the second.

Emerging economies are characterized by excess volatility of the business cycle and higher consumption volatility relative to output, as compared to advanced countries (Uribe and Schmitt-Grohe, 2017; see Figure 7), and procyclical capital flows (Frankel, 2011). In emerging economies, a rise in interest rates on foreign borrowings is associated with capital reversals and economic downturns (Neumeyer and Perri, 2005; Monacelli et al., 2023; see Figure 32). Interestingly, the countercyclicality of interest rates is a distinguishing feature of business cycles in emerging economies, with no similar behavior recorded in advanced economies (see right panel of Figure 32). Therefore, studying the transmission of interest rate shocks and the amplification mechanism associated with country interest rate fluctuations is crucial to understanding the nature of business cycles in emerging economies.

The literature that aims to explain business cycle facts in emerging economies has moved in two main directions. The first approach, pioneered by Aguiar and Gopinath (2007), highlights nonstationary productivity shocks as a driving force of excess volatility of the business cycle and trade balance countercyclicality. The second approach introduces country interest rates coupled with financial frictions as a source of shocks and an amplification mechanism of standard (stationary) productivity shocks (Neumeyer and Perri, 2005). In a series of subsequent papers (Garcia-Cicco et al., 2010) and Chang and Fernandez (2013), the authors allow nonstationary productivity shocks to compete with interest rate shocks in explaining business cycle fluctuations in emerging economies. Both papers reach the same conclusion: the role of nonstationary productivity shocks is negligible (they explain less than 3% of variance), and a country's interest rate is an important propagation mechanism of standard stationary productivity shocks.

The question of the quantitative importance of interest rate shocks as an exogenous source

Table 7. Business cycle moments in emerging and advanced economies

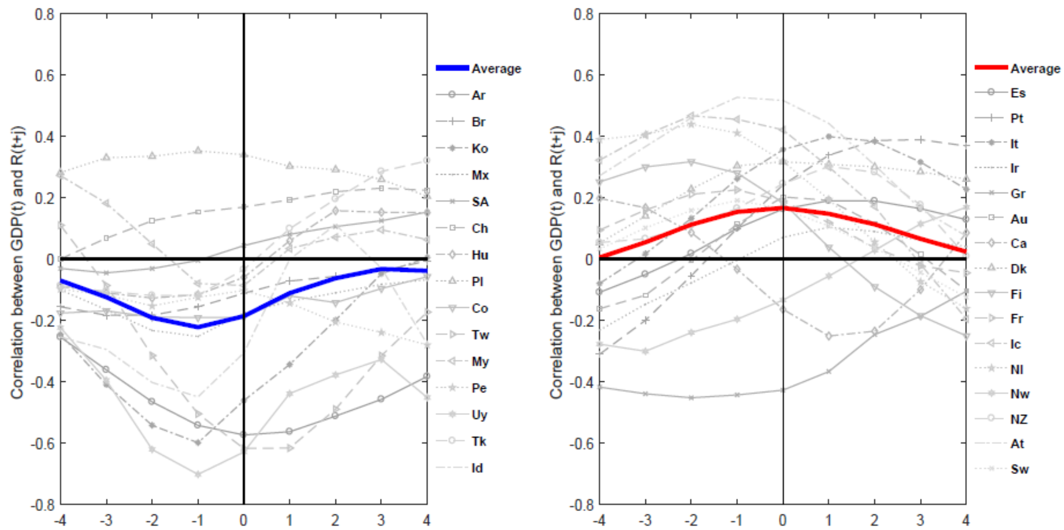
	Log-Quadratic Time Trend			HP Filter		
Statistic	All	Emerging	Rich	All	Emerging	Rich
Standard Deviations						
σ_y	3.26	4.27	2.74	1.80	2.60	1.38
σ_c/σ_y	0.99	1.23	0.87	1.01	1.32	0.85
σ_g/σ_y	1.46	2.07	1.15	1.30	2.02	0.93
σ_i/σ_y	3.44	3.67	3.31	3.73	3.88	3.65
σ_x/σ_y	3.77	3.97	3.67	4.01	3.80	4.11
σ_m/σ_y	3.52	3.55	3.51	4.44	3.65	4.84
$\sigma_{tb/y}$	1.80	2.93	1.21	1.09	1.95	0.64
Correlations with y						
y	1.00	1.00	1.00	1.00	1.00	1.00
c	0.83	0.72	0.88	0.78	0.78	0.78
g/y	-0.43	-0.11	-0.59	-0.58	-0.22	-0.78
i	0.86	0.82	0.88	0.84	0.77	0.87
x	0.17	-0.00	0.26	0.43	-0.05	0.67
m	0.60	0.48	0.66	0.68	0.52	0.76
tb/y	-0.44	-0.52	-0.41	-0.39	-0.56	-0.31
tb	-0.44	-0.51	-0.40	-0.39	-0.56	-0.31
Means						
tb/y	-0.1	0.2	-0.2			
$(x+m)/y$	43.8	45.7	42.8			

Note: This table presents key business cycle statistics for emerging and advanced economies (marked as "rich"). y stands for output, c denotes total private consumption, g is government spending,, i is investment, x is exports, m is imports, and tb is trade balance. All variables are real and per capita. The long-term trend in the data is removed either with log-quadratic detrending or using HP filtering. For detrending, all variables except trade balance are detrended in logs and expressed in percent deviations from the trend. Trade balance to output is detrended in levels. The sample contains 11 emerging and 17 rich countries. Moments are averaged across countries using population weights. The sets of emerging and rich countries are defined as all countries with average PPP converted GDP per capita in U.S. dollars of 2005 over the period 1990-2009 within the ranges 3,000-25,000 and 25,000-1, respectively. Rich Countries: Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Hong Kong, Italy, Japan, Netherlands, Norway, Sweden, Switzerland, United Kingdom, and the United States. Emerging Countries: Argentina, Israel, South Korea, Mexico, New Zealand, Peru, Portugal, South Africa, Spain, Turkey, and Uruguay. All statistics are calculated based on quarterly data over 1980Q1–2012Q4.

Source: Uribe and Schmitt-Grohe (2017).

of economic fluctuations and an amplification mechanism of productivity shocks remains open. On the one hand, in the empirical paper by Uribe and Yue (2006), the authors study the effects of country spread shocks (a part of a country interest rate on foreign borrowing unrelated to the foreign interest rate) in a vector autoregression (VAR) framework. They rely on recursive identification, in which they assume that macroeconomic variables such as output and investment do not respond contemporaneously to country spread shocks.

Figure 32. Cross-correlations between GDP and interest rates in emerging and advanced economies



(a) Emerging Markets (b) Advanced Economies

Source: Monacelli et al. (2023).

However, this assumption is at odds with the basic theoretical mechanism built into most emerging economies' business cycle models: the existence of working capital constraint. In particular, most theoretical papers assume contemporaneous responses of macroeconomic allocation to wedges generated by this type of financial friction. Moreover Uribe and Yue (2006)'s identification assumptions are not fully supported by Neumeyer and Perri (2005)'s business cycle facts, according to which a contemporaneous correlation between output and country interest rate is almost as strong as if the interest rate would be taken with a one-quarter lag.

On the other hand, when studying exogenous country spread shocks versus endogenous propagation mechanisms through the spread, the quantitative business cycle literature relies on strong prior assumptions about deep parameter values. In particular, two most important parameters responsible for the effect of productivity shocks on spreads and of spreads on real quantities: the share of production costs paid in advance using working capital and the elasticity of the spread to productivity shocks in a reduced-form equation[92] are either fixed or estimated with a tight prior. For example, Neumeyer and Perri (2005) assume that the full value of the wage bill is paid in advance, whereas the literature has shown that the

[92]See Arellano (2008), Mendoza and Yue (2012), and Fernandez and Gulan (2015) for a justification of this relation.

empirical importance of working capital constraint is much lower: in the range of 6-10% of GDP according to Mendoza and Yue (2012). Another example of the sensitivity of the results to the choice of parameter is a Bayesian estimation of an open economy business cycle model with a tight prior on the elasticity of country spread to productivity shocks. In Chang and Fernandez (2013), the posterior estimate mostly reproduces the prior, thus making the conclusions sensitive to changes in the prior.

The main contribution of this chapter is a new country spread shock identification procedure that is validated using a quantitative emerging economy business cycle model. My identification procedure introduces the possibility to study the relative importance of country spread shocks and other shocks (e.g., productivity, terms-of-trade) using a data-driven VAR model with a minimum set of restrictions. In particular, I extend the analysis of Uribe and Yue (2006) using structural identification of shocks with sign restrictions in a VAR. In contrast to Uribe and Yue (2006), I do not rely on dubious timing restrictions. To back up identifying sign restrictions, I rely on a standard emerging markets' business cycle model with financial frictions. I check if the model's parameter values yield distinctive signs of responses of key macroeconomic variables to the shocks. If this is the case, I can use them in a structural VAR model. Using VARs, I compute impulse responses of macroeconomic variables to the shocks in several major emerging economies and compare sign-identified impulse responses to conventional ordering restrictions.

Potential applications of my proposed identification procedure include studying the determinants of recessions and financial crises in Latin America and Southeastern Asia in the 1980s and the 1990s, cross-border capital flow reversals in peripheral Europe in the late 2000s and early 2010s, and the effects of financial sanctions—bans on new foreign borrowings—imposed on Russia in 2014 and subsequently strengthened. In the next section, I provide justification for why these episodes can be classified as sudden stops and how it matters for the trade balance dynamics around the sudden stops, and how it is used for deriving identification assumptions.

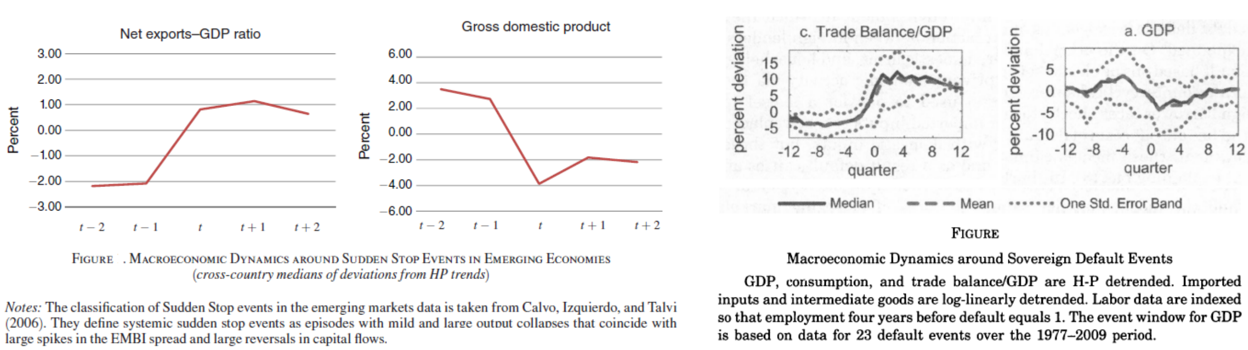
The chapter is structured as follows. Section 4.2 delivers examples of country spread shocks and sudden stops and discusses their implications for trade balance. Section 4.3 describes existing and newly proposed identification of country spread shocks in open economies. Section 4.4 presents the main results. Section 4.5 describes a quantitative model used to justify the sign restriction scheme I propose. Section **??** concludes.

4.2 Country Spread Shocks and Sudden Stops: Country Examples and Implications for Trade Balance

Sudden stops are a major issue in emerging open economies: an abrupt fall in capital inflows is frequently associated with large output decline and financial disruptions (Calvo et al., 2004, 2006). The observed procyclicality of capital flows in emerging markets (Frankel, 2010) may explain the excess volatility of business cycles in emerging countries, as compared to advanced countries documented in Uribe and Schmitt-Grohe (2017). According to previous studies, sudden stops materialize as a result of a combination of supply-side factors (global capital market turmoil) and demand-side factors (domestic vulnerabilities, financial imbalances[93]). The entangled combination of the supply and demand-side factors complicates empirical research on the macroeconomic effects of external financial shocks.

According to Calvo et al. (2004), a sudden stop reflects "*large and unexpected falls in capital inflows that have costly consequences in terms of disruptions in economic activity*" (p. 14). Quantitatively, the authors define a sudden stop as a year-on-year fall in capital flows that is below two standard deviations from its long-term mean. Several authors document substantial trade balance reversals—around 3 p.p. of GDP—around sudden stops and sovereign defaults (Mendoza, 2010; Mendoza and Yue, 2012). Sudden stops are also associated with large output losses. These trends are presented in Figure 33.

Figure 33. Sudden stops and sovereign defaults



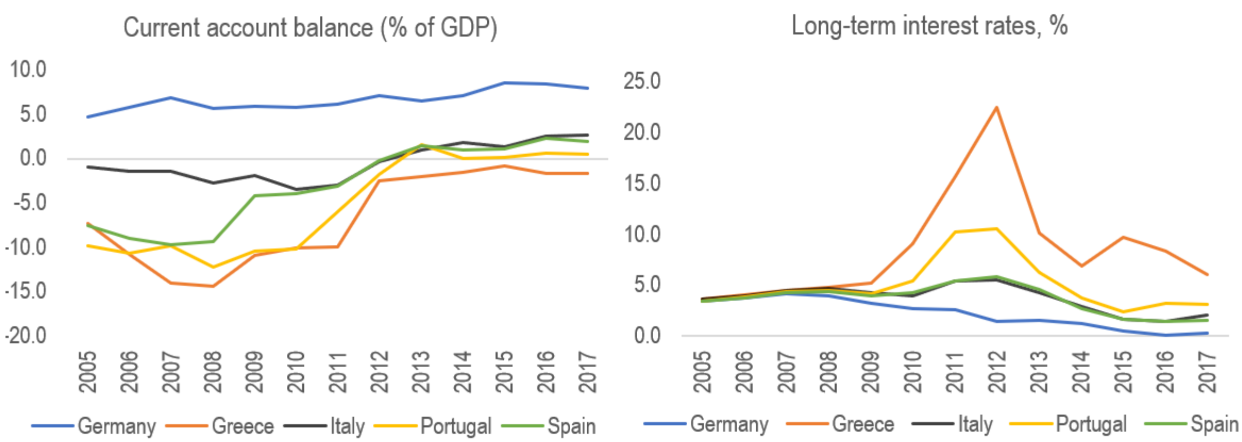
Source: Mendoza (2010) and Mendoza and Yue (2012)

Large current account balance reversals were documented in several southern European countries: Greece, Italy, Portugal, and Spain around the European debt crisis of the early 2010s (see Figure 34). Importantly, the fall in capital inflow (which mirrors current account

[93] According to Calvo et al. (2004), balance sheet dollarization is a key one.

balance reversals) took place together with a spike in long-term lending rates. These taken together suggest an important role of the supply-side factors behind the tightening of financial conditions. Note that, at the same time, Germany's current account balance and interest rates were relatively stable during the European debt crisis period, suggesting that this country did not encounter a similar episode.

Figure 34. Balance of payments crisis in the Euro area



Source: World Development Indicators.

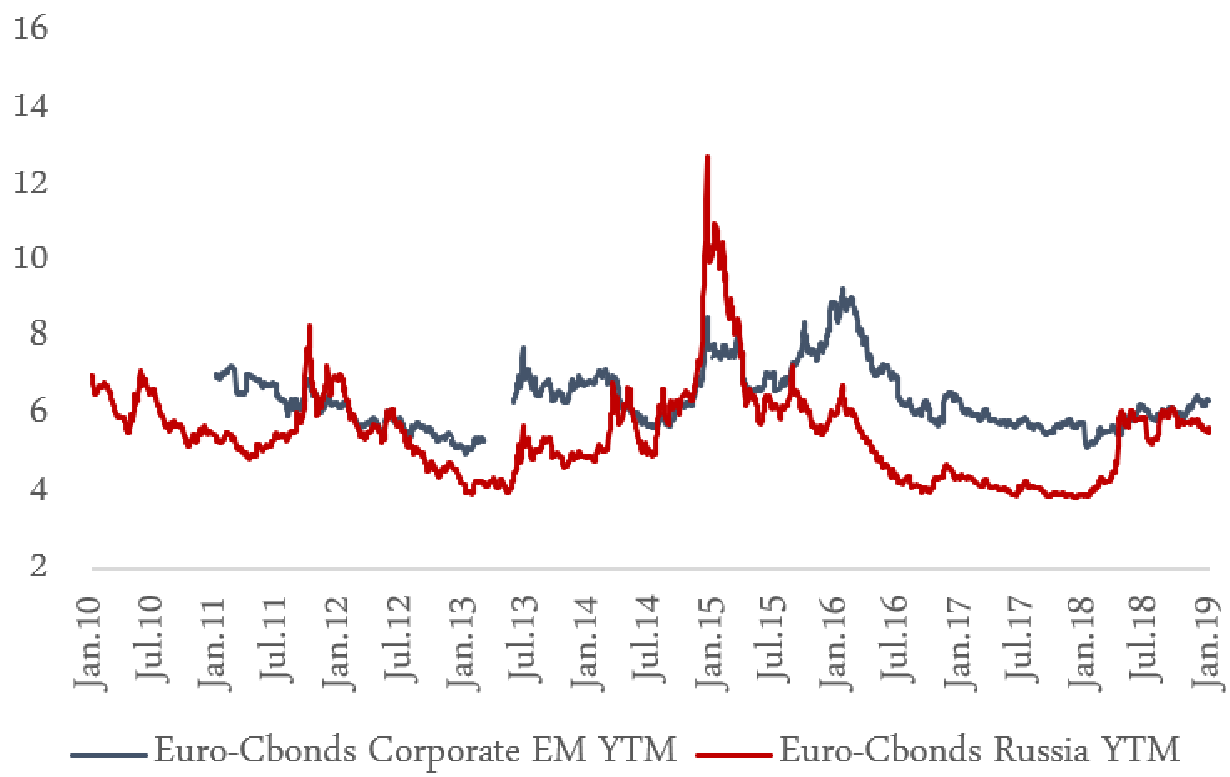
The recent episode of financial sanctions imposed on Russia by Western countries provides another example of a sudden stop in an emerging open economy. The financial sanctions prohibit Russian corporate establishments under sanctions to place new debt of a certain maturity at the European and US financial markets. Under a de-facto closed primary market and following the scheduled debt repayment, Russian corporate external debt shrunk by 25% in 2014-2015.[94] A distinctive feature of this episode is the large ex-ante role of the supply-side financial shock. The financial sanctions were imposed in response to political tensions in Ukraine, and both their scope and timing were orthogonal to Russian macroeconomic conditions (a more detailed description of the sanctions is provided in Chapter 3). Another feature of this episode suggesting a supply-side nature of the shock is the negative correlation between country spread and the amount of external debt in 2014 (also see details in Chapter 3).

Using the same methodology to obtain monthly data on capital flows as in Calvo et al. (2004), I argue that December 2014 in Russia (5-6 months after the imposition of the first package

[94]End-2015 to end-2013 change of the external debt of banks and other sectors excluding debt liabilities to direct investors and to direct investment enterprises. *Source*: The Bank of Russia, External Sector Statistics, own calculations.

of financial sanctions) can be classified as an episode of sudden stop according to Calvo's criterion. Therefore, I conclude that the Russian economic and financial crisis of 2014-2015 is a sudden stop episode. In a subsequent paper (Calvo et al., 2006), the authors impose an additional requirement on a sudden stop: surprisingly low capital flows should coincide with peaking bond spread. I check this requirement on the Russian data and find that in the first half of 2015 (again, 5-6 months after the imposition of the financial sanctions), the average yield to maturity of Russian corporate borrowers was significantly higher than in other emerging countries (see Figure 35).

Figure 35. Yield to maturity, Russian corporate Eurobonds and emerging markets (EM) foreign bonds average, %



Source: Cbonds.info, own calculations.

I further compare major macroeconomic trends around sudden stop episodes documented in Mendoza (2010) with those observed in Russia in 2014-2015. I center the Russian sudden stop episode around the year 2015 because, according to the capital flows definition, the episode started in December 2014; therefore, its macroeconomic consequences were realized only in 2015. Several observations arise from this analysis (see Figure 1 in Appendix no. 32). Qualitatively, economic fluctuations in Russia are similar to those observed in other emerging countries; however, the magnitude of the Russian sudden stop is relatively lower. Increased absorption and production was observed (relatively to trend) before the sudden stop episode, with a trade balance below the trend and high asset prices. Around the sudden stop, international capital flows reversed with net exports rising above trend to finance debt repayment, and production and absorption declined. Additionally, asset prices went down. The observations presented suggest that such dynamics around sudden stops would be difficult to reconcile with a frictionless business cycle model because if financial markets were frictionless, foreign borrowers would extend debt financing to allow domestic consumers to smooth consumption. Therefore, there should be a supply-side shock in the willingness of foreign creditors to lend or a shock to the country's price of borrowing—country spread—to explain this evidence.

Overall, I conclude that sudden stop episodes are large capital outflows that are costly in terms of associated GDP decline. Sudden stops are typically accompanied by trade balance and current account reversals, and a spike in borrowing costs. Several discussed sudden stop episodes have supply-side shock features: a combination of debt repayment and increased borrowing cost. This rationalizes my proposed identification of country spread shocks, which I discuss in the next subsection. In particular, I assume that a (supply-side) country spread shock—an exogenous increase in the country's borrowing costs unrelated to the borrower's fundamentals—is associated with an improvement in the trade balance.

4.3 Shocks identification in structural VARs: existing and new approaches

In this section, I describe an existing and the newly proposed identification of country spread shocks in open economies.

4.3.1 Recursive identification

To illustrate the recursive identification approach, consider the standard open economy vector autoregression (VAR) in a reduced form (Uribe and Yue, 2006):

$$y_t = Ay_{t-1} + u_t$$

where

$$y_t = \begin{pmatrix} \hat{y}_t \\ \hat{i}_t \\ tby_t \\ \hat{R}^*_t \\ \hat{S}_t \end{pmatrix}$$

is a vector of endogenous variables: output, investment, trade balance to GDP ratio, foreign interest rate, and domestic country spread. A hat on output and investment denotes log deviations from a trend, and a hat on interest rates denotes the log. u_t's are reduced-form residuals estimated from the data, which are correlated across equations.

To identify the model, one needs to establish a link between reduced-form residuals and structural shocks determined by matrix B_0:

$$u_t = B_0^{-1}\varepsilon_t$$

Rewrite a model in a structural form:

$$B_0 y_t = B_1 y_{t-1} + \varepsilon_t$$

$$B_0 \begin{pmatrix} \hat{y}_t \\ \hat{i}_t \\ tby_t \\ \hat{R}^*_t \\ \hat{S}_t \end{pmatrix} = B_1 \begin{pmatrix} \hat{y}_{t-1} \\ \hat{i}_{t-1} \\ tby_{t-1} \\ \hat{R}^*_{t-1} \\ \hat{S}_{t-1} \end{pmatrix} + \begin{pmatrix} \varepsilon^y_t \\ \varepsilon^i_t \\ \varepsilon^{tby}_t \\ \varepsilon^{R*}_t \\ \varepsilon^S_t \end{pmatrix}$$

Uribe and Yue (2006) assume B_0 to be lower triangular, i.e. that $\hat{y}_t, \hat{i}_t$, and tby_t do not respond to ε^{R*}_t and ε^S_t contemporaneously:

$$B_0 = \begin{pmatrix} 1 & 0 & 0 & 0 & 0 \\ . & 1 & 0 & 0 & 0 \\ . & . & 1 & 0 & 0 \\ . & . & . & 1 & 0 \\ . & . & . & . & 1 \end{pmatrix}$$

Consider how reduced-form residuals and structural shocks are linked in the Uribe and Yue

(2006) identification (note that an inverse of the lower triangular matrix is lower triangular):

$$\begin{pmatrix} u_t^y \\ u_t^i \\ u_t^{tby} \\ u_t^{R*} \\ u_t^S \end{pmatrix} = u_t = \underbrace{B_0^{-1}}_{\text{C}} \varepsilon_t = \begin{pmatrix} 1 & 0 & 0 & 0 & 0 \\ . & 1 & 0 & 0 & 0 \\ . & . & 1 & 0 & 0 \\ . & . & . & 1 & 0 \\ c_{51} & c_{52} & c_{53} & c_{54} & 1 \end{pmatrix} \begin{pmatrix} \varepsilon_t^y \\ \varepsilon_t^i \\ \varepsilon_t^{tby} \\ \varepsilon_t^{R*} \\ \varepsilon_t^S \end{pmatrix}$$

Suppose that output shock coincides with the structural productivity shock, $\varepsilon_t^y = \varepsilon_t^A$, then I can write the following system of contemporaneous equations:

$$\begin{aligned} u_t^y &= \quad \varepsilon_t^A \\ &\ldots \\ u_t^S &= \quad c_{51}\varepsilon_t^A + \ldots + \varepsilon_t^S \end{aligned}$$

With these assumptions (Uribe and Yue, 2006) interpret all contemporaneous correlation between y_t and S_t as driven solely by productivity shock, ε_t^A. However, this is a rather strong assumption. First, as demonstrated in Figure 32, output and interest rates in emerging economies are negatively correlated both contemporaneously and when interest rate leads output. Next, it relies on a strong assumption in the quantitative model: if there is working capital friction and working capital loans are granted by the current period interest rate (not by the previous period rate, as it is assumed in Neumeyer and Perri, 2005), then output, investment, and trade balance would respond contemporaneously to country spread or world interest rate shocks. Therefore, below, I relax these strong timing assumptions and propose an alternative identification scheme based on sign restrictions.

4.3.2 The proposed sign restrictions

I propose the following identification in which I do not restrict any element of B_0^{-1} to be zero:

$$\begin{pmatrix} u_t^y \\ u_t^i \\ u_t^{tby} \\ u_t^{R*} \\ u_t^S \end{pmatrix} = \begin{pmatrix} d_{11} & . & . & . & d_{15} \\ . & . & . & . & . \\ . & . & . & . & . \\ . & . & . & . & . \\ d_{51} & . & . & . & d_{55} \end{pmatrix} \begin{pmatrix} \varepsilon_t^A \\ . \\ . \\ . \\ \varepsilon_t^S \end{pmatrix}$$

$$
\begin{aligned}
u_t^y = & \qquad d_{11}\varepsilon_t^A + ... + d_{15}\varepsilon_t^S \\
... & \\
u_t^S = & \qquad d_{51}\varepsilon_t^A + ... + d_{55}\varepsilon_t^S
\end{aligned}
$$

In my identification, a contemporaneous correlation between output y_t and country spread S_t may be driven by both shocks to productivity and country spread shocks, ε_t^A and ε_t^S. This is in line with a generic quantitative model if the following frictions are in place: working capital loans are paid by the current period interest rate, and country spread is linked to productivity, This is also in line with the data, given that contemporaneous correlation between country spread and output is almost the same as if taken with one period lag, see Figure 32.

To disentangle productivity and country spread shocks, I propose to use the sign restrictions approach which, to the best of my knowledge, was never been used in this setting. In particular, I apply sign restrictions presented below in the columns of B_0^{-1} matrix. I assume that a (negative) productivity shock decreases output and trade balance to output, and increases country spread. I also assume that a (positive) country spread shock increases country spread, decreases output, and increases the trade balance to output ratio. The assumed restrictions are presented as the signs of elements of the matrix B_0 below.

$$
\begin{pmatrix} u_t^y \\ u_t^i \\ u_t^{tby} \\ u_t^{R*} \\ u_t^S \end{pmatrix} = \begin{pmatrix} - & . & . & . & - \\ . & . & . & . & . \\ - & . & . & . & + \\ . & . & . & . & . \\ + & . & . & . & + \end{pmatrix} \begin{pmatrix} \varepsilon_t^A \\ . \\ . \\ . \\ \varepsilon_t^S \end{pmatrix}
$$

$$
u_t = B_0^{-1}\varepsilon_t
$$

My identification relies on distinct sign responses of trade balance to productivity and country spread shocks. In the following subsections, I first present empirical evidence on the effects of country spread shocks in major emerging economies and then I check if these restrictions can be justified with a quantitative open economy business cycle model.

4.4 Empirical evidence on the effects of country spread shocks

In this subsection I describe the implementation of proposed sign restrictions and their application to the empirical analysis of the effects of country spread shocks. To do so, I take Uribe and Yue (2006) empirical model and variable construction and extend their dataset up to the early 2020s. My country dataset covers Argentina, Brazil, Ecuador, Peru, Philippines—which follow the sample of countries in Uribe and Yue (2006)—and Russia, which I add to maintain comparability with Chapter 2 of this Thesis. There are data issues for Mexico and South Africa—other countries that are covered in Uribe and Yue (2006))—so I have to remove them from my sample. The original time period in Uribe and Yue (2006) spans 1994Q1-2001Q4, whereas my data extract covers 1997Q4-2019Q3. I stop before the COVID-19 pandemic to avoid the specificity of this crisis. My sample starts 3 years later than in Uribe and Yue (2006), because the current data vintages do not have older data.

I estimate the following 5-variable VAR model closely following Uribe and Yue (2006):

$$y_t = Ay_{t-1} + u_t$$

where

$$y_t = \begin{pmatrix} \hat{y}_t \\ \hat{i}_t \\ \hat{tby}_t \\ R_t^{US} \\ R_t \end{pmatrix} = \begin{pmatrix} \textit{log-deviation of output from trend} \\ \textit{log-deviation of investment from trend} \\ \textit{log-deviation of trade balance to output from trend} \\ \textit{log US real interest rate} \\ \textit{log country real interest rate} \end{pmatrix}$$

I develop a Matlab script that downloads IMF International Financial Statistics (IFS) data for each country in my sample and makes all necessary data transformations. I take the following raw variables from IFS (all in national currency): nominal GDP, GDP deflator, nominal investment, nominal export, and import[95]. I collect data on US short-term interest rates and inflation rate to construct real US interest rates[96] I use data on J.P. Morgan Emerging Markets Bond Spread (EMBI+) from World Bank's Global Economic Monitor to measure country spread, in line with Uribe and Yue (2006).

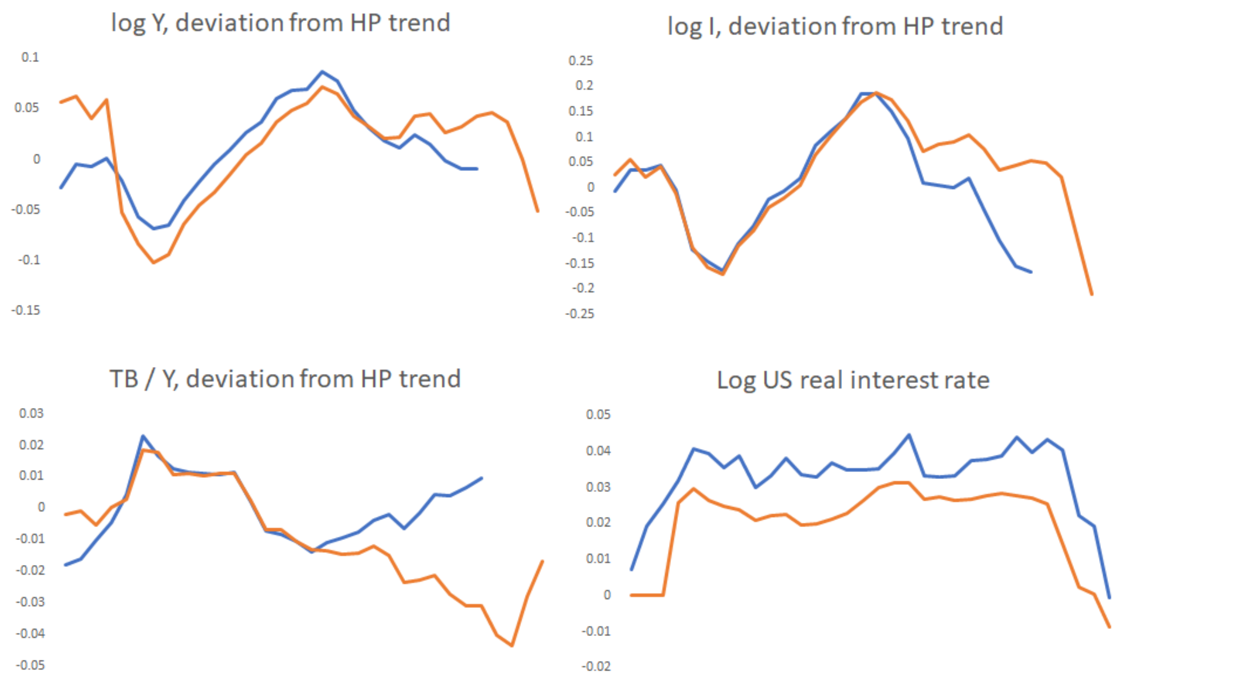
In Figure 36, I present the evolution of key detrended macroeconomic variables in Uribe and Yue (2006)'s data and in my replication with the more recent vintage of the data for

[95]The corresponding variable codes in the IMF IFS dataset are: $NGDP\_XDC$, $NGDP\_D\_IX$, NFI_XDC, NX_XDC, NM_XDC.

[96]The corresponding variable codes in the IMF IFS dataset are: $FITB\_PA$, $PCPI\_PC\_CP\_A\_PT$.

Argentina, the first country in my sample. During the period of dataset intersection, both lines evolve closely. There are some differences closer to the end of Uribe and Yue (2006)'s sample that can be explained by the updated trends in the data in my vintage.

Figure 36. Detrended data in Uribe and Yue (2006)'s model: Argentina



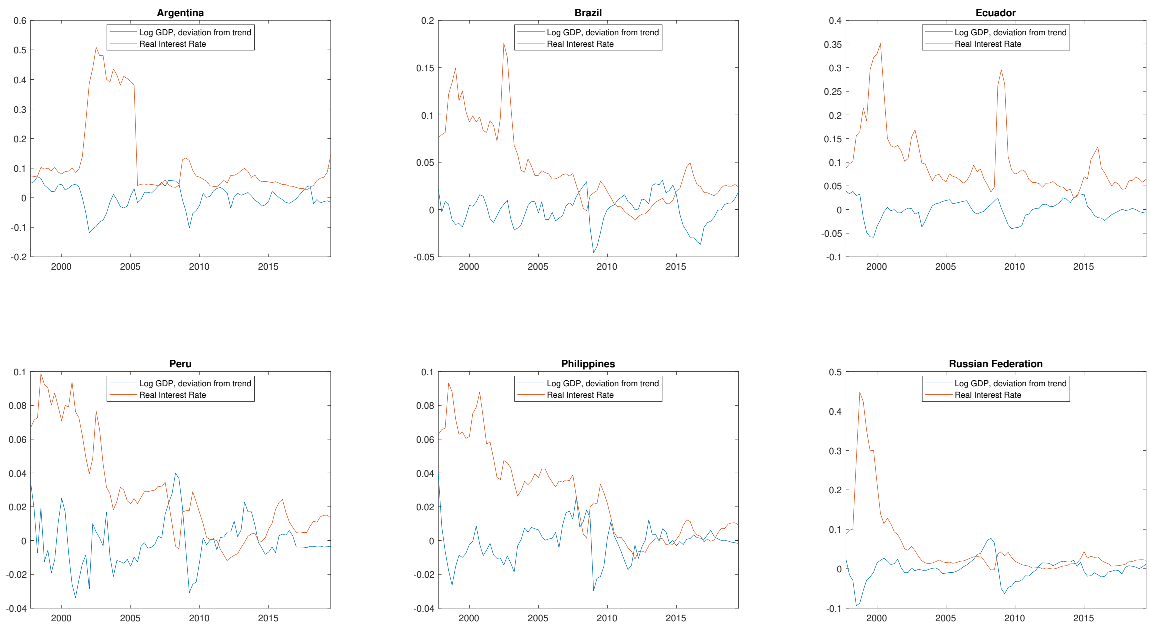
Note: The blue line denotes Uribe and Yue (2006)'s data (1994Q1-2001Q4), the brown line denotes my replication with the more recent vintage of the data (1997Q4-2019Q3).

Importantly, in my updated dataset, the key business cycle fact of emerging economies is preserved. In particular, there is a strong countercyclicality of country interest rate (and country spread) which is the case for all emerging countries in my sample (see Figure 37) and in line with reported facts by Neumeyer and Perri (2005) and Monacelli et al. (2023). Furthermore, as was shown in the previous research, country spreads and interest rates are acyclical in advanced economies, which is the case in my data too for two advanced open economies: New Zealand and Sweden, see Figure 38.

To estimate the effects of the country spread shocks, I estimate the country-by-country Bayesian VAR model with recursive identification and sign restrictions. I apply the Bayesian estimation of the model because the frequentist approach to sign restrictions is very rare and uninformative (Kilian and Lutkepohl, 2017). I use a flat (i.e., uninformative) prior so that coefficient estimates are not affected directly by the prior's choice.[97] I impose a Normal-

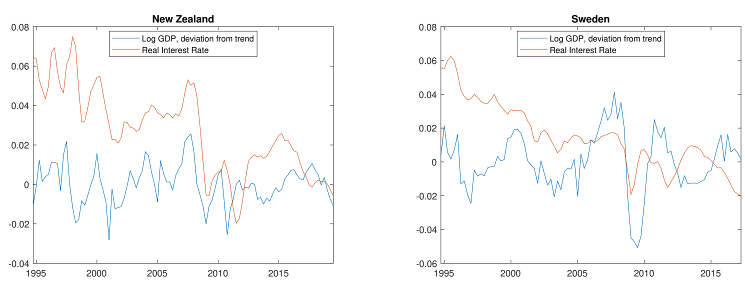
[97]There may still exist some non-trivial influence of the flat prior on posterior inference under certain conditions (Baumeister and Hamilton, 2015).

Figure 37. Evolution of country real interest rates and detrended GDP in emerging economies



Note: GDP is in deviation from its quasi-real-time HP trend. The country's real interest rate is the sum of the US real interest rates and the country spread.

Figure 38. Evolution of country real interest rates and detrended GDP in selected advanced open economies



Note: GDP is in deviation from its quasi-real-time HP trend. The country's real interest rate is the sum of the US real interest rates and the country spread.

inverse-Wishart (conjugate) prior to coefficients and employ an algorithm that draws from a conjugate uniform-normal-inverse-Wishart posterior (Antolin-Diaz and Rubio-Ramirez, 2018).

The details of the implementation of sign restrictions are summarized in Appendix 1.F above so I omit them here.

4.4.1 Impulse response functions: Recursive identification

Below I present impulse response functions to a country interest rate shock and output shock identified recursively, as in Uribe and Yue (2006). Recall that in this model, output is placed first in the VAR system and the country's real interest rate last, thus assuming that the country's real interest rate reacts to output innovations contemporaneously while shocks to the country's real interest rate do not affect real variables within the same period.

The estimated impulse response functions for Argentina and Brazil are similar in signs of responses to negative output shock and positive country spread shock: output and investment decline in both countries, trade balance improves, and country interest rate rises (see Figures 39, 40). Similar results are obtained in Uribe and Yue (2006): if output shock is flipped to be negative, then both responses look similar in terms of the signs (see Figure 41). This is worrisome given that my purpose is to disentangle two shocks empirically. Recursive identification does not provide sound empirical ground to separate shocks if I used sign restrictions because, on impact, signs of responses of variables are identical to both shocks. But recall that recursive identification relies strongly on timing assumptions, which are relaxed in the sign restrictions approach. Therefore below I present the impulse response functions of key macroeconomic variables to both shocks, if shocks are identified using sign restrictions.

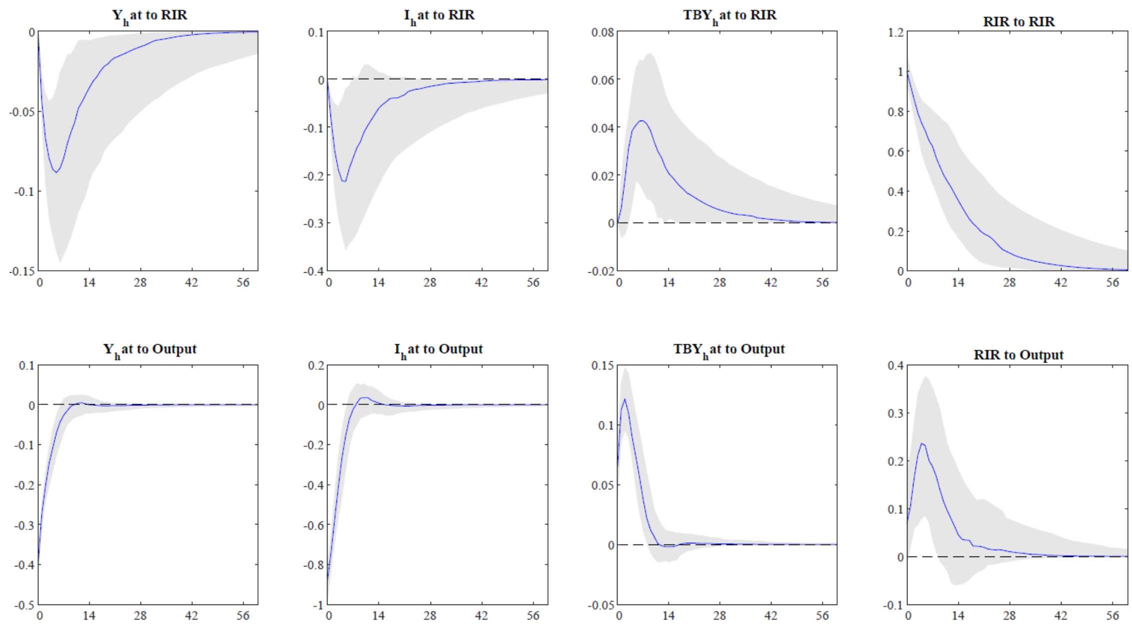
4.4.2 Impulse response functions: Sign restrictions

Imposing on-impact differential sign restrictions on the responses of the trade balance to the country's real interest rate shock and to productivity/output shock yields a qualitatively similar picture in both countries considered, see Figures 42, 43. In both cases, on-impact differential restriction on trade balance response vanishes very quickly and becomes the same in its sign on the unrestricted horizon. This means that the data does not provide supporting evidence for proposed restrictions. In the next subsection, I check if the proposed restrictions can be backed up by a quantitative emerging economy business cycle model.

4.5 Quantitative emerging markets' business cycle model as a source of identification restrictions

In this section, I set up a quantitative model and check if my VAR identification assumptions are satisfied under some plausible parameter values. I follow Neumeyer and Perri (2005) and specify a simple model in which an economy is subject to productivity and interest rate

Figure 39. Impulse response functions to real interest rate and output shocks identified under recursive identification, Argentina



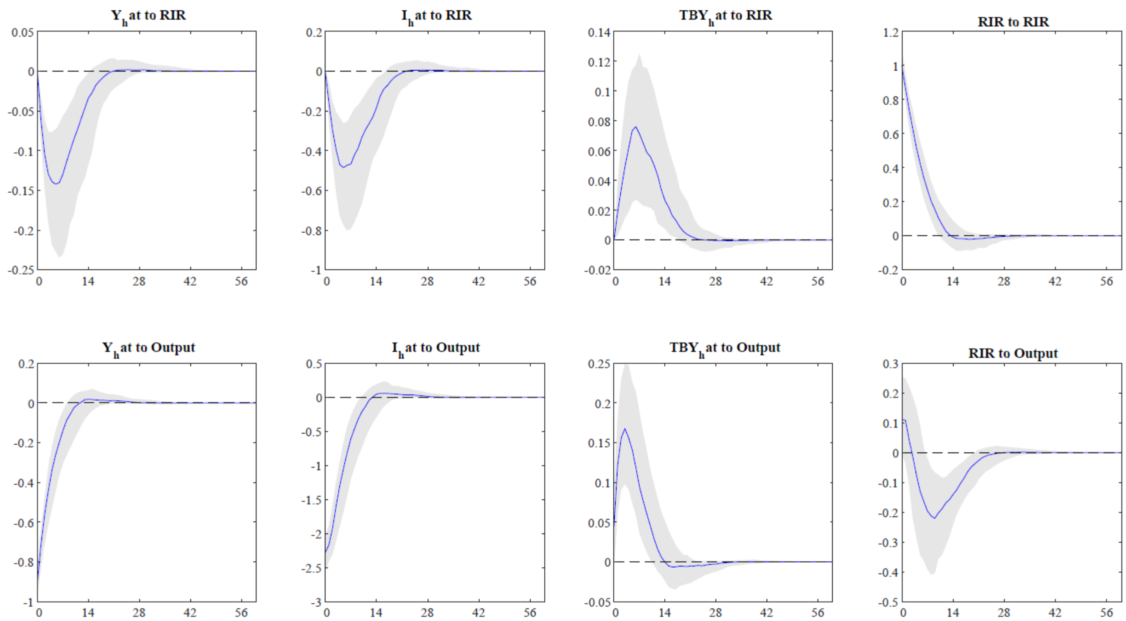
Note. The figure reports the estimated IRFs of domestic macroeconomic variables to shocks identified recursively. The BVAR model contains 5 variables: domestic GDP (Y), domestic investment (I), trade balance to GDP ratio (TBY), the real interest rate in the U.S. economy (exogenous, not presented), and the country's real interest rate. The Bayesian estimates are obtained with the flat (uninformative) prior. I set 5,000 draws from the posterior distribution. Conventional credible bands comprise the 16th and 84th percentiles of the post-burned-in estimated IRFs and are reported as grey-shaded areas.

shocks.[98] Two crucial features of this model are the following: an introduction of a working capital constraint and assuming labor supply decisions be independent of consumption. The former allows macroeconomic aggregates to respond to country interest rate fluctuations while the latter produces procyclical hours worked, which is in line with business cycle moments in emerging economies.

The considered model is an infinite-horizon representative firm, representative household model. There are three goods: capital, labor, and final good. Domestic agents have access to the international credit market. In particular, a representative firm issues within-period working capital loans to pay in advance for a fraction of the wage bill. Working capital loans are granted at the previous period interest rate. Moreover, representative household issues one-period intertemporal discount bonds. An interest rate on both loans is a sum of an exogenously given world interest rate and a country spread. Country spread is assumed to be linked to domestic productivity. There are three shocks in the model: productivity,

[98] An alternative modeling framework includes the models of Garcia-Cicco et al. (2010) and Chang and Fernandez (2013).

Figure 40. Impulse response functions to real interest rate and output shocks identified under recursive identification, Brazil



Note. The figure reports the estimated IRFs of domestic macroeconomic variables to shocks identified recursively. The BVAR model contains 5 variables: domestic GDP (Y), domestic investment (I), trade balance to GDP ratio (TBY), the real interest rate in the U.S. economy (exogenous, not presented), and the country's real interest rate. The Bayesian estimates are obtained with the flat (uninformative) prior. I set 5,000 draws from the posterior distribution. Conventional credible bands comprise the 16th and 84th percentiles of the post-burned-in estimated IRFs and are reported as grey-shaded areas.

world interest rate, and country spread shocks.

A representative firm hires labor and rents capital from households and issues within-period working capital loans at the previous period interest rate. A representative firm maximizes its profit subject to production constraint:

$$\max_{L_t, k_{t-1}} Y_t - W_t L_t - Q_t K_{t-1} - (R_{t-1} - 1)\theta W_t L_t \tag{24}$$

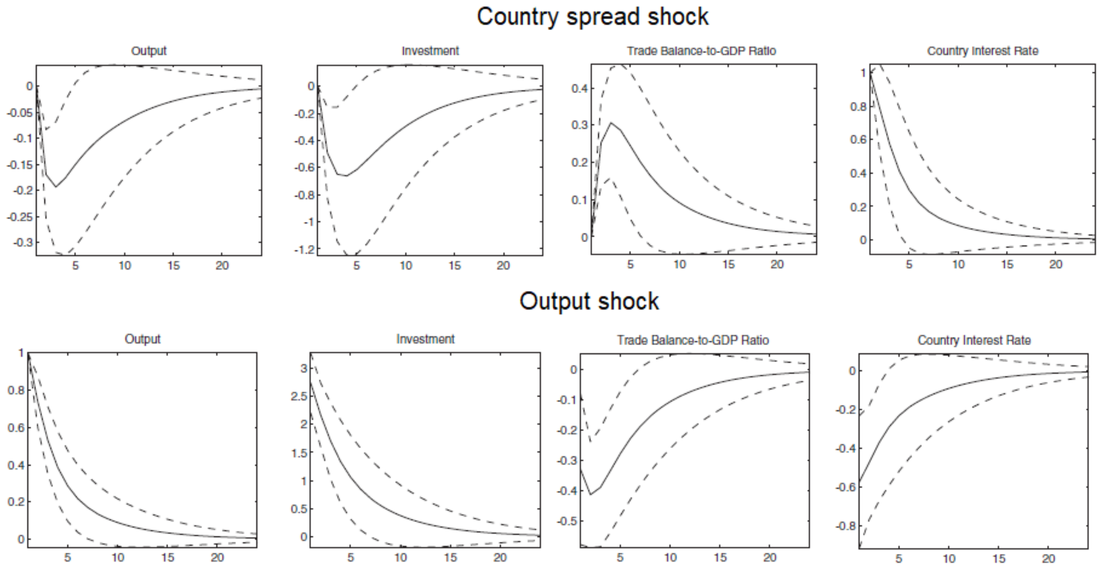
$$\text{s.t. } Y_t = A_t k_{t-1}^{\alpha} [(1+\gamma)^t L_t]^{1-\alpha} \tag{25}$$

where θ is a fraction of wage bill paid in advance, γ is exogenous labor-augmenting technological progress.

A representative household decides how much to work, invest, borrow internationally, and consume subject to budget constraint and the law of motion for capital:

$$\max_{C_t, L_t, B_t, X_t{}_{t=0}^{\infty}} E_0 \left[\sum_{t=0}^{\infty} \beta^t u\Big(C_t - g(L_t)\Big) \right] \tag{26}$$

Figure 41. Impulse response functions to real interest rate and output shocks identified under recursive identification (Uribe and Yue, 2006).



Note. The figure reports the estimated IRFs of domestic macroeconomic variables to shocks identified recursively. *Source*: Uribe and Yue (2006). Country sample: Argentina, Brazil, Ecuador, Mexico, Peru, Philippines, and South Africa. The VAR model contains 5 variables: domestic GDP, investment, trade balance to GDP ratio, the real interest rate in the U.S. economy, and the country's real interest rate. Dashed lines represent two-standard-error bands computed using the delta method.

$$\text{s.t. } C_t + X_t + B_t + \kappa(B_t) \leq W_t L_t + Q_t K_{t-1} + B_{t-1} R_{t-1} \tag{27}$$

$$X_t = K_t - (1-\delta)K_{t-1} + \Phi(K_{t-1}, K_t) \tag{28}$$

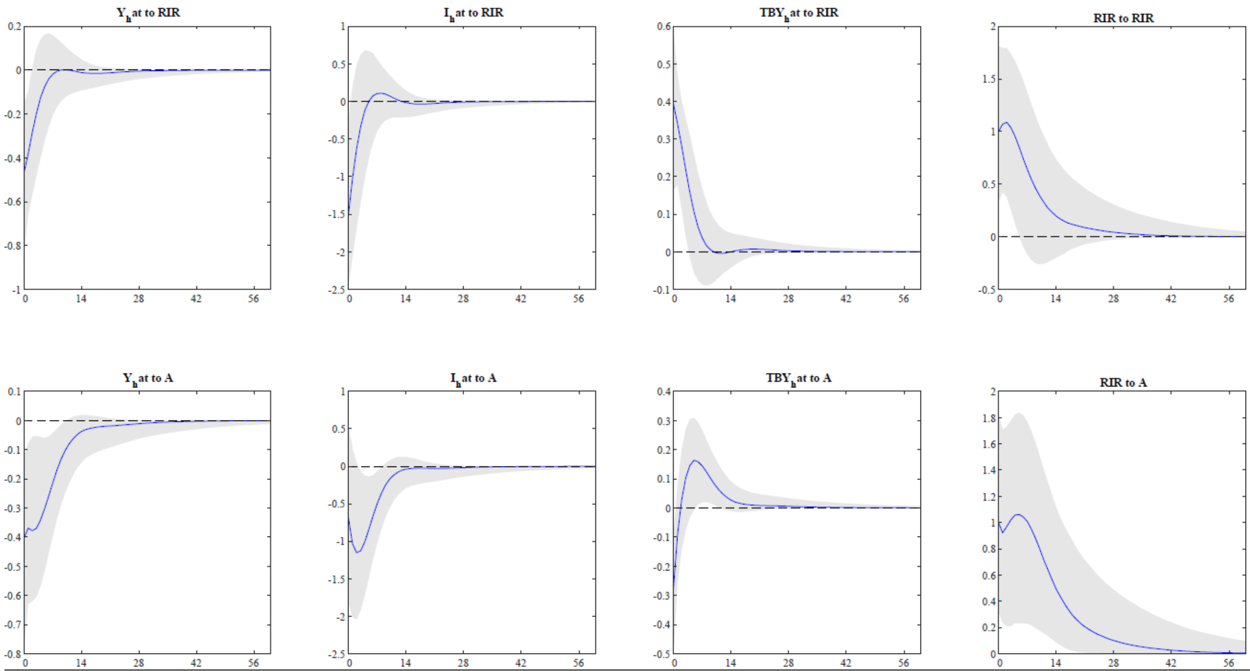
where x_t is fixed capital investments, b_t is foreign bond holdings, Φ are capital adjustment costs, $\kappa(B_t)$ costs of adjusting bond holdings portfolio (both quadratic). GHH preferences (Greenwood et al., 1988) have typically been employed in open economy models since the work by Mendoza (1991).

The domestic interest rate is a sum of an exogenously given world interest rate and a country spread (all in logs): $\hat{R}_t = \hat{R}^*_t + \hat{S}_t$. The world interest rate is exogenous and follows an AR(1) law of motion: $\hat{R}^*_t = \rho_R \hat{R}^*_{t-1} + \varepsilon^{R*}_t$.

Following Neumeyer and Perri (2005), I begin exploration with the first case in which country risk is assumed to be independent of productivity: $\hat{S}_t = \rho_S \hat{S}_{t-1} + \varepsilon^S_t$. I later substitute this *independent country risk* assumption with the *induced country risk*. In the case of induced country risk, I assume that country risk is negatively related to future productivity:[99]

[99] for a theoretical explanation for this reduced form link through a default risk see Arellano (2008) and Mendoza and Yue (2012).

Figure 42. Impulse response functions to real interest rate and output shocks identified with sign restrictions, Argentina



Note. The figure reports the estimated IRFs of domestic macroeconomic variables to shocks identified with sign restrictions. A (negative) output/productivity shock depresses the trade balance to output ratio and increases country spread. A positive country spread shock increases country spread and increases trade balance to output. The BVAR model contains 5 variables: domestic GDP (Y), domestic investment (I), trade balance to GDP ratio (TBY), the real interest rate in the U.S. economy (exogenous, not presented), and the country's real interest rate. The Bayesian estimates are obtained with the flat (uninformative) prior. I set 5,000 draws from the posterior distribution. Conventional credible bands comprise the 16th and 84th percentiles of the post-burned-in estimated IRFs and are reported as grey-shaded areas.

$\hat{S}_t = -\eta E_t[\hat{A}_{t+1}] + \varepsilon_t^S$. In both cases, domestic productivity is persistent and subject to exogenous productivity shocks: $\hat{A}_t = \rho_A \hat{A}_{t-1} + \varepsilon_t^A$.

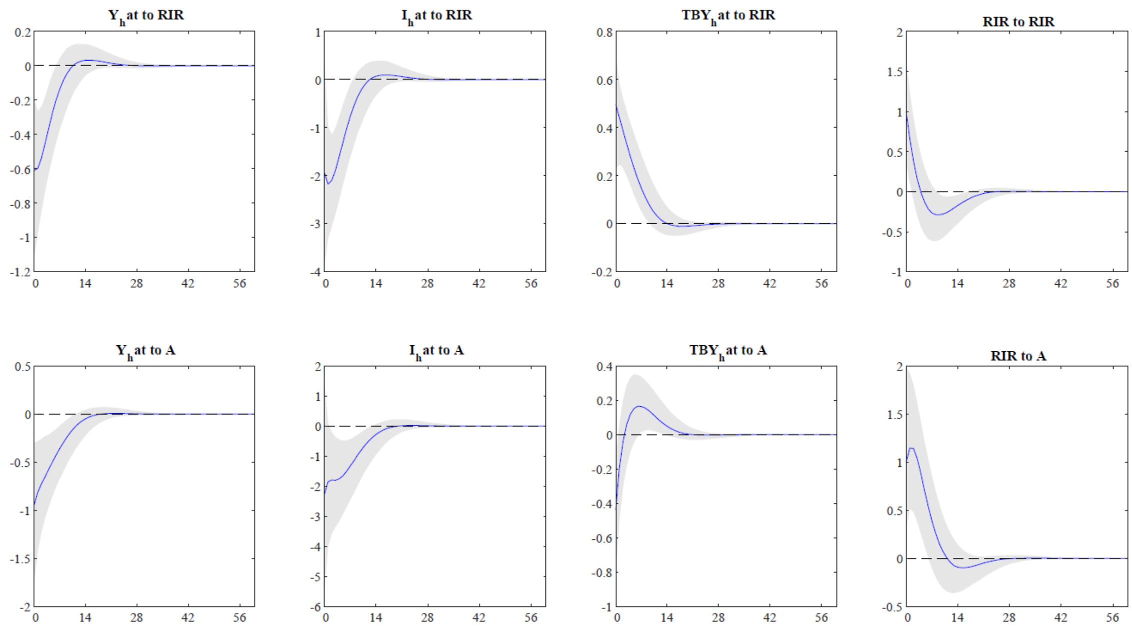
I derive the system of equilibrium conditions. To do so, I transform all variables denoted by X into their detrended analogs: $\tilde{X}_t = X_t/(1+\gamma)^t$. An exception is capital and bonds for which transformation is the following: for $X_t = (K_t, B_t)$, I set $\tilde{X}_{t-1} = X_{t-1}/(1+\gamma)^t$. The list of equilibrium conditions:

$$\tilde{W}_t = \psi \nu L_t^{\nu-1} \tag{29}$$

$$\lambda_t = (\tilde{C}_t - \psi L_t^{\nu})^{-\sigma} \tag{30}$$

$$\lambda_t \Big[1 + \kappa\Big(\frac{\tilde{B}_t(1+\gamma)}{Y_t} - \Big(\frac{B}{Y}\Big)^{BGP}\Big)\Big] = \beta \frac{\lambda_{t+1}}{(1+\gamma)^{\sigma}} R_t \tag{31}$$

Figure 43. Impulse response functions to real interest rate and output shocks identified with sign restrictions, Brazil



Note. The figure reports the estimated IRFs of domestic macroeconomic variables to shocks identified with sign restrictions. A (negative) output/productivity shock depresses the trade balance to output ratio and increases country spread. A positive country spread shock increases country spread and increases trade balance to output. The BVAR model contains 5 variables: domestic GDP (Y), domestic investment (I), trade balance to GDP ratio (TBY), the real interest rate in the U.S. economy (exogenous, not presented), and the country's real interest rate. The Bayesian estimates are obtained with the flat (uninformative) prior. I set 5,000 draws from the posterior distribution. Conventional credible bands comprise the 16th and 84th percentiles of the post-burned-in estimated IRFs and are reported as grey-shaded areas.

$$\lambda_t\Big[1+\phi-\Big(\frac{\tilde{K}_t(1+\gamma)}{\tilde{K}_{t-1}}-(1+\gamma)\Big)\Big] = \beta\frac{\lambda_{t+1}}{(1+\gamma)^{\sigma}}\Bigg[Q_{t+1}+1-\delta + \frac{\phi}{2}\Bigg(\Big(\frac{\tilde{K}_{t+1}(1+\gamma)}{\tilde{K}_t}\Big)^2-(1+\gamma)^2\Bigg)\Bigg] \tag{32}$$

$$\tilde{W}_t = (1-\alpha)\frac{\tilde{Y}_t}{L_t}\frac{1}{1+(R_{t-1}-1)\theta} \tag{33}$$

These conditions are combined with the firm's FOC on K, production function, budget constraint, investment, and trade balance to get the full system of equations.

In the calibration strategy I follow Neumeyer and Perri (2005), see Table 8. I use Argentinian *quarterly* data over 1983-2001. I take ν and σ from Neumeyer and Perri (2005). I set $\alpha, \beta, \gamma, \delta, \psi$ to match long-run growth averages in the data. I set the bond holdings to income ratio to match the historical ratio of net foreign assets to GDP. $\left(\frac{B}{Y}\right)^{BGP} : \left(\frac{B}{Y}\right)^{SS} - \frac{\theta W^{SS} L^{SS}}{Y^{SS}} =$

-0.42 (historical average of NFA/GDP). The fraction of working capital paid in advanced θ is assumed to be 1 in Neumeyer and Perri (2005), but I vary it between 0.25 and 1 to check the sensitivity of the results. In the original paper, ϕ was set to 25.1 to match the volatility of the investment. I also vary this parameter because it guides the sensitivity of investment, and therefore output, to shocks. I discuss η parameter below when I present the induced country risk results.

Table 8. Calibration of parameters of the Neumeyer and Perri (2005) model

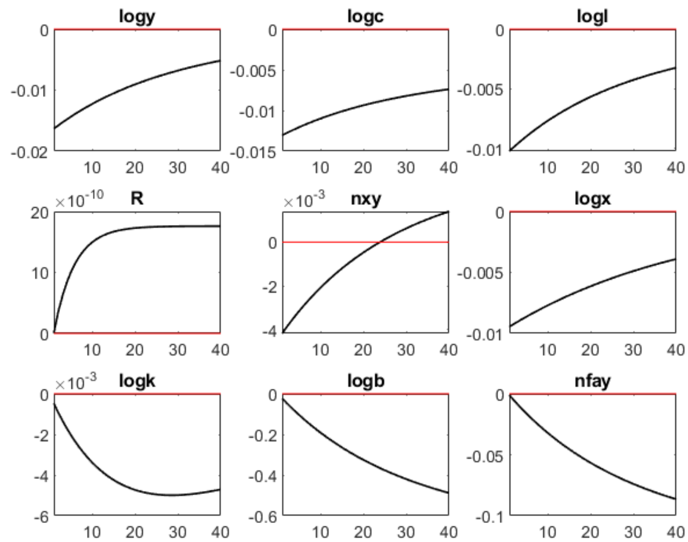
Name	Symbol	Value
Preference parameters		
Discount factor	β	0.9965
Utility curvature	σ	5
Labor curvature	ν	1.6
Labor weight	ψ	2.48
Technology parameters		
Technological progress growth	γ	0.62%
Capital exponent (production)	α	0.38
Depreciation rate	δ	4.4%
% Labor income paid in advance	θ	{0.25, 0.5, 0.75, 1}
Bond holding cost	κ	10^{-5}
Capital adjustment costs	ϕ	{8, 25.1, 40}
Shocks		
Productivity	ρ_A	0.95
International rate	ρ_{R^*}	0.81
Country risk	ρ_S	0.78

Note: baseline calibration from Neumeyer and Perri (2005) is highlighted in blue.

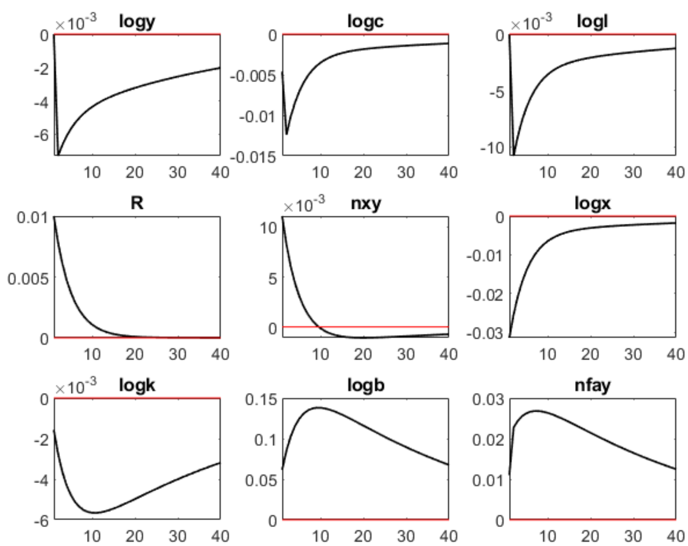
Case I: Independent country risk

Figure 44 reports the impulse response functions to contractionary productivity and country spread shocks. I find that both shocks decrease output, consumption, and investment. In the case of independent country risk, I also find that negative productivity shocks do not affect the country's interest rate. I further establish that negative productivity shocks decrease the trade balance-to-output ratio, whereas positive spread shocks increase it. Overall, in the case of independent county risk, I obtain distinct sign responses of the trade balance-to-output ratio to both shocks, which is in line with my hypothesis, at the cost of losing the on-impact sensitivity of the country spread variable to productivity shocks. The latter contradicts the idea of imposing distinct sign restrictions on the on-impact response of trade balance to productivity and country spread shocks. Moreover, this is also an important feature of both data and (most) quantitative models, that is why I next turn to the case of the induced country spread. But before moving to the next case, in Figure 45 I present robustness checks of my main results—on the signs of responses of the trade balance-to-output ratio to shocks—to reasonable changes in key parameters of the model.

Figure 44. Impulse response functions to shocks (*independent country risk*)



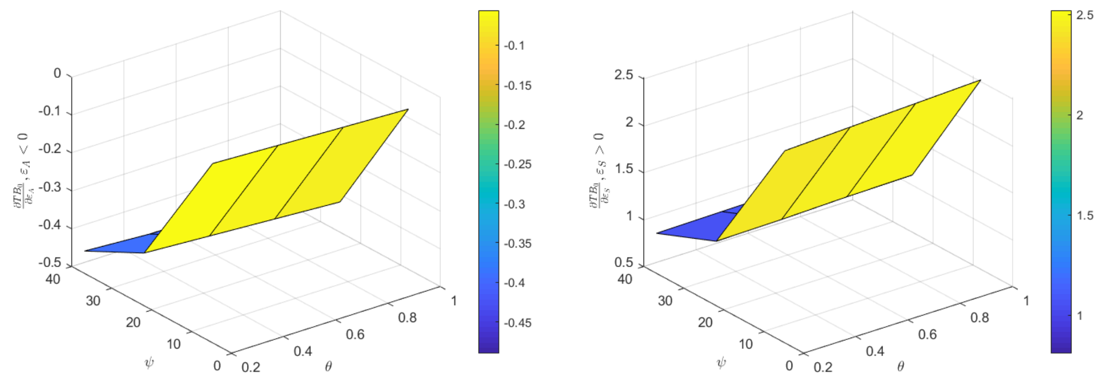
(a) Impulse response functions to a productivity shock (–1 p.p.)



(b) Impulse response functions to a country spread shock (+1 p.p.)

Note. The figure reports IRFs to contractionary productivity and country spread shocks in an economy calibrated to Argentinian data in case productivity and county spread processes are independent. log y stands for output, log c is consumption, log l is labor, R is country real interest rate, nxy is trade-balance-to-output ratio, log x -isinvestment, log k is capital, log b is borrowings, $nfay$ is net foreign asset position to GDP.

Figure 45. The on-impact responses of the trade balance-to-output ratio to shocks: Robustness to parameter changes (*independent country risk*)



(a) Impact response of TB to a productivity shock (-1 p.p.)

(b) Impact response of TB to a country spread shock (+1 p.p.)

Note. The figure reports a sensitivity of impact response of the trade balance-to-output ratio to shocks to changes in the following parameters: the share of labor income paid in advance, θ which I vary in the range $\{0.25, 0.5, 0.75, 1\}$, the parameter governing capital adjustment costs ϕ which I vary in the range $\{8, 25.1, 40\}$. In both cases, blue denotes the baseline value.

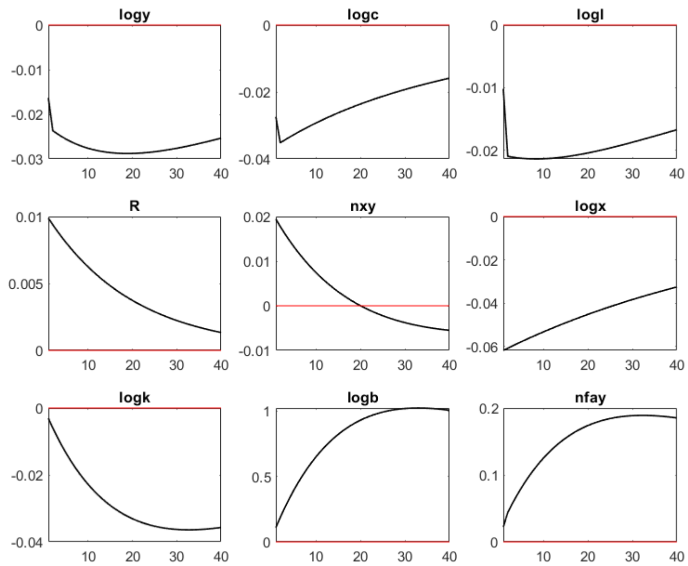
Case 2: Induced country risk

I continue with the case of the induced country spread in which country spread is assumed to react negatively to productivity innovations: $\hat{S}_t = -\eta E_t[\hat{A}_{t+1}] + \varepsilon_t^S$. Figure 46 reports the impulse response functions to contractionary productivity and country spread shocks. In the case of induced country spread, the effects of productivity shocks become amplified (due to spread) while the effects of spread shocks become very short-lived - because I turned off any persistence in the latter. In principle, one would want to combine both inverse reactions to productivity and persistence to make the picture more realistic and estimate the degree of persistence and the degree of sensitivity to productivity using Bayesian methods.

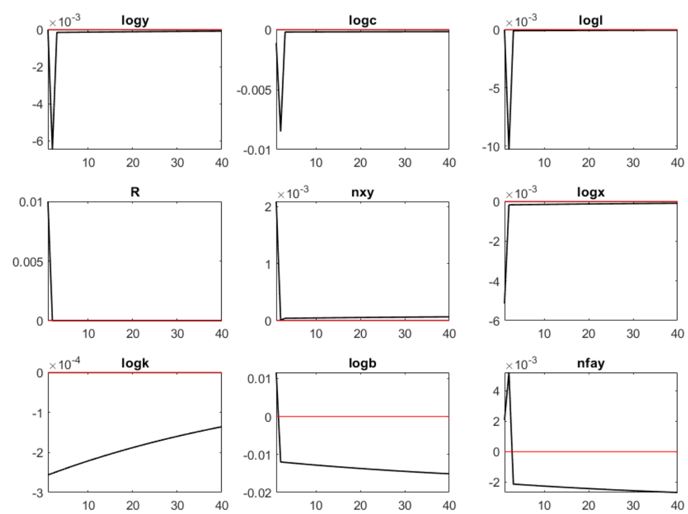
Coming back to the current analysis, in the case of the induced country risk, the response of the trade-balance-to-output becomes positive to both shocks which makes these shocks indistinguishable through the lens of sign restrictions. The reason - consumption reacts to spread changes more compared to output when productivity shocks are amplified by spread changes, and this generates an increase in domestic savings.

Evaluating the sensitivity of the on-impact trade-balance-to-output ratio to a productivity shock (see Figure 47), it can be seen that there is some variation in the sign of the on-impact responses of *tby* to shock. However, for most of the values of η: between 0.2 and 1, there is

Figure 46. Impulse response functions to shocks (*induced country risk*)



(a) Impulse response functions to a productivity shock (–1 p.p.)

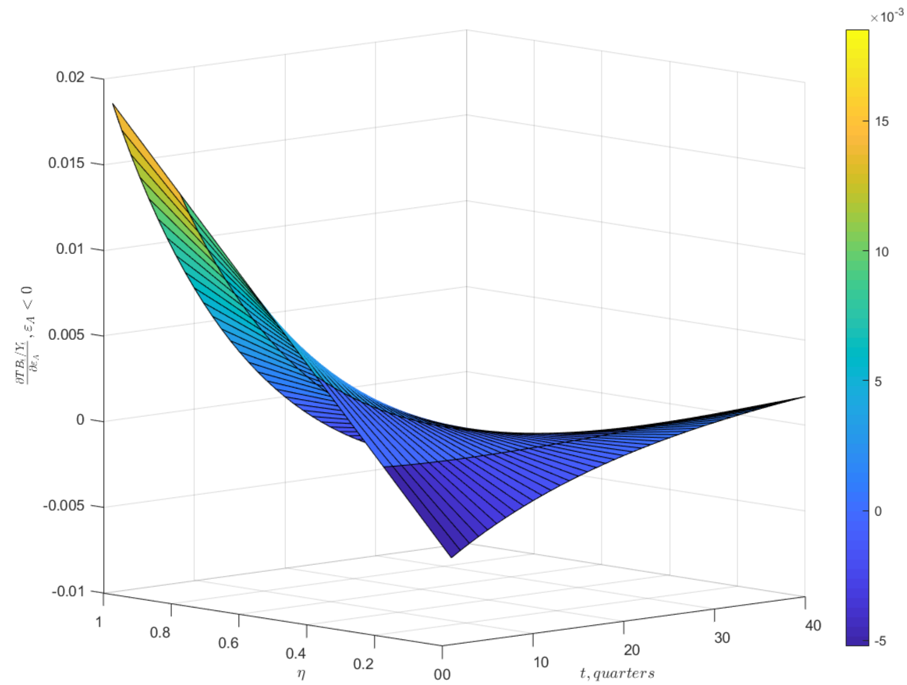


(b) Impulse response functions to a country spread shock (+1 p.p.)

Note. The figure reports IRFs to contractionary productivity and country spread shocks in an economy calibrated to Argentinian data in case the county spread process is linked inversely to productivity. $\log y$ stands for output, $\log c$ is consumption, $\log l$ is labor, R is country real interest rate, nxy is trade-balance-to-output ratio, $\log x$ is investment, $\log k$ is capital, $\log b$ is borrowings, $nfay$ is net foreign asset position to GDP.

an increase in the trade-balance-to-output ratio in response to negative productivity shock, which contradicts my sign-restrictions-based identification idea. Recall that in Neumeyer and Perri (2005), η was calibrated and set to 1.04, in Chang and Fernandez (2013), prior on η was centered around 1 with quite narrow uncertainty around it which forced the posterior to be centered around 0.73.

Figure 47. Impulse response function of the trade-balance-to-output ratio to a negative productivity shock (–1 p.p.; *induced country spread*)



Note. The figure reports IRFs to a contractionary productivity shock in an economy calibrated to Argentinian data in case the county spread process is linked inversely to productivity. The figure reports the variation in the IRF depending on the parameter η is the sensitivity of country spread to productivity. The higher η is, the larger the reaction of the country spread to a productivity shock, and the larger the positive response of the trade-balance-to-output ratio.

Overall, my analysis shows that even if the value of η were significantly decreased, up to 0.2 in the Neumeyer and Perri (2005) model, the sign responses of the trade-balance-to-output ratio to shocks are still similar: trade-balance-to-output increases in response to both shocks. There is a narrow parameter space of η, 0 to 0.2 (see Figure 47), under which there is support to my proposed identification restrictions and for which there will be distinctive sign responses of *tby* to shocks. However, it is unlikely that η falls into this region in the estimation. Therefore, I do not impose them in the latter analysis. I summarize my proposed sign restrictions below.

Both country spread and trade balance are functions of model parameters and shocks. Initially, I assumed that trade balance responds to negative productivity shock and positive country spread shock differently, which is likely not to be the case. This is because the sign of the latter response turned out to be positive under most of the model parameter values as I have shown above. This does not encourage the use of proposed sign restriction in shock identification, because shocks would "masquerade" (Wolf, 2020): see Table 1.

Table 9. Expected and actual sign restrictions on responses to shocks

		Country Spread	Trade Balance
$\varepsilon^A < 0$	Negative productivity shock	$+$	$(-)\ +$
$\varepsilon^S > 0$	Positive country spread shock	$+$	$+$

What could be done if one still wants to empirically estimate the effects of the country spread shocks? One possible solution could be to continue exploring quantitative model properties under a broader model and parameter space and derive identification restrictions from the models. In particular, one may consider a set of models—for example Neumeyer and Perri (2005) and Chang and Fernandez (2013)[100]—and employ full Bayesian estimation of key parameters of models, $theta$, ϕ, and η, to check if a broader set of models and alternative parameter estimation can yield plausible identification restrictions. Alternatively, one may stick to traditional recursive identification, which is still used in empirical research (Monacelli et al., 2023). Another approach to identifying country spread shocks I propose in this thesis—high-frequency identification—is described in Chapter 3. Overall, there is space for further research on new approaches to identify country spread shocks and their validation with general equilibrium models.

Overall, this chapter explores two empirical approaches to the identification of country spread shocks in emerging economies: the popular recursive identification which relies on timing restrictions, and a new one that I propose, which is based on sign restrictions. I first present some background information on sudden stops and country spread fluctuations around large financial crises. I further document necessary identification restrictions that are used to identify two key shocks driving the bulk of business cycle fluctuations in emerging economies: productivity shocks and county spread shocks. I next set up a simple quantitative open economy model to check if my proposed identification is in line with it. I calibrate

[100] One may also vary the timing of the working capital constraint: in particular, in the baseline model (Neumeyer and Perri, 2005), it is assumed that working capital is financed by a previous period interest rate, which is restrictive. One may assume that working capital is financed by a current period interest rate.

parameters of the model and check if the model delivers differential responses of the key variables to shocks. My key finding is that the parameter space under which the model yields identification is too narrow to rely on. My finding lays out an avenue for future research.

Conclusion

This Thesis studies the role of credit expansions in the economy. In a cross-country setting, we first show that bank credit growth acts as a double-edged sword: in the short run, it reduces the likelihood of a recession, but in the medium run the effect reverts and the recession risk turns to rising, reaching its peak in three years. We then reveal that this boom-bust type of recession response to bank credit is explained by expansionary *aggregate demand* shocks fueling *household credit*. Other business cycle shocks—monetary policy, credit supply, and others—do not cause similar recession responses; instead, they only increase the probability of recession in around three years. Firm credit expansion, in contrast to household credit, only increases the risk of recession. We also demonstrate that the recession response is severely amplified under collateral booms (especially, housing booms), in advanced economies, and is larger under the fixed exchange rate regime. In addition, this Thesis explores credit supply shocks in the US economy (bank deregulation in the 1980s and pre-crisis period of the early 2000s) and in Russia (financial sanctions in the 2010s punishing for Crimea's annexation) and other emerging economies (sudden stops).

More broadly, this Thesis provides new insights into why some credit booms end in recessions while others do not. Previous research highlights the role of technological growth as an important distinguishing factor between "good" and "bad" credit booms (Gorton and Ordonez, 2019). Through the lens of our empirical findings, the defining feature lies in the type of shocks driving them. All expansionary shocks, when transmitted through bank credit, eventually increase recession risks but some shocks may postpone it for the future—aggregate demand. Therefore, the final recession outcome depends on the mixture of shocks and their timing.

From the policy perspective, the findings of this Thesis suggest that macroprudential policy tightened to the shock- and credit type may be beneficial if the regulator targets smoothing the business cycle. The current macroprudential policy regulating loan-to-value (LTV) ratio may not be sufficient because if both credit and housing prices increase, LTV limits may be still satisfied while recession risks have soared.

Future research may explore differences in household and firm credit (mis)allocation during credit expansions. Our results suggest that firm credit is riskier, especially when accompanied by collateral price booms, implying that firm credit misallocation is more widespread compared to household credit. Differences in riskiness may arise, for example, from less liquid and more volatile collateral offered by firms (company receivables and cash flow, Lian and Ma, 2021) as compared to household credit (pledging housing). Further research is needed to assess the contribution of financial easing to firm credit misallocation and to develop mitigation measures.

Summary

In the first chapter, we study the role of credit supply and other business cycle shocks in shaping the effect of credit growth on future recession risk. In the second chapter, we estimate the propagation of credit supply shocks on household balance sheets and labor market outcomes. In the third chapter, we show that several episodes of imposition and strengthening of financial sanctions against Russia can be interpreted as external credit supply shocks. We propose a novel identification of such shocks, quantify their contribution to macroeconomic dynamics in Russia, and estimate their heterogeneous effects on domestic firms and households. In the last chapter, I explore the identification of country spread shocks – a manifestation of credit supply shocks in emerging economies – through the lens of the calibrated general equilibrium open economy model. Overall, this dissertation provides a complex assessment of the role of credit supply shocks in the accumulation of macro-financial risks at both aggregate and microeconomic levels.

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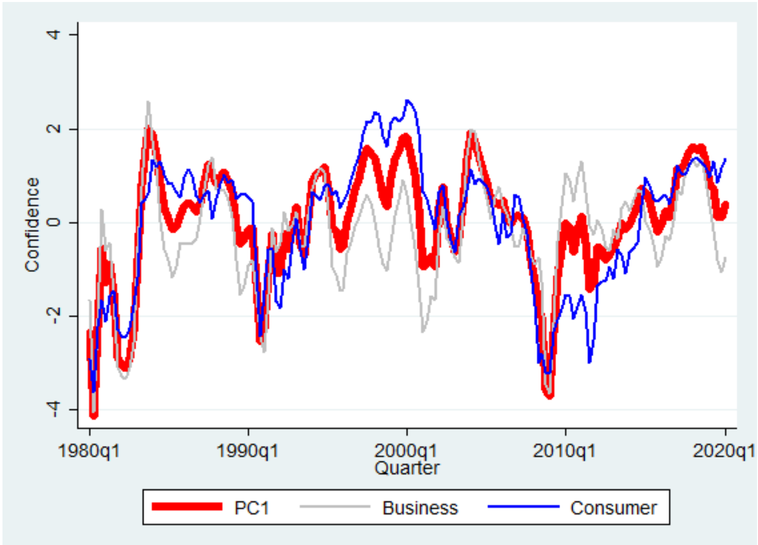
Appendix no. 1: Choice of recession predictors

We employ classical recession predictors as control variables in all our recession models. These indicators usually operate at short, i.e. less than a year, forecasting horizons. However, to obtain conservative estimates of the effect of credit on recession in dynamic recession prediction logit models, we include classical predictors with both short- and medium-run lags. We do so to avoid any potential omitted variable bias. In the robustness check, we consider other financial overheating and trade imbalance indicators as recession predictors. Below we explain our choice of recession predictors based on the existing literature and provide details on how we construct our indicators.

The first classical predictor is the *stock market index*. Stock prices are forward-looking and reflect future prospects of the corporate sector of the economy. In this respect, a decline in stock prices can signal an approaching recession. A different interpretation arises if considering the wealth effect. A decline in asset prices can induce economic agents to perceive themselves as relatively poorer and thus willing to consume less. Here recession arises due to demand deficiency. Several papers have exploited the informational content of stock market prices in recession forecasting (Estrella and Mishkin, 1998; Christiansen et al., 2014; Liu and Moench, 2016; Bluedorn et al., 2016; Aastveit et al., 2019). We use the annual log growth of the real stock market index. We deflate the stock market index by seasonally adjusted (X13) CPI to make data comparable across countries with different rates of inflation.

Second, Christiansen et al. (2014) highlight the importance of *sentiment indicators* and their predictive power beyond the standard recession predictors (term spread, stock market growth) on the U.S. data. As a measure of sentiment, they use both the index of consumer sentiment, which measures consumers' estimate of business conditions and the purchasing managers index, which measures the managers' assessment of the current level of economic activity relative to previous periods. A deterioration of either consumer or business expectations (or both) signals gloomy economic perspectives and, eventually, predicts recessions. Other papers employing various sentiment indicators to predict recessions include Liu and Moench (2016) and Aastveit et al. (2019). To measure expectations in the economy, we extract the first static principal component from the two series: consumer and business confidence indicators (see Fig. 1). We do so to avoid potential multicollinearity between the two series, which can decrease estimation precision. Moreover, we would like to focus on those signals coming from changes in confidence which feature synchronous changes in both indicators.

Third, several papers focused on financial crisis prediction conclude that *global economic indicators* have an added value and perform well when forecasting domestic vulnerabilities



Note: The grey line represents the business confidence indicator, and the blue line stands for the consumer confidence indicator. The thick red line depicts the first principal component extracted from these two series. All series are averaged across countries.

Figure 1. Confidence indicators, average across countries

(Alessi and Detken, 2011; Lo Duca and Peltonen, 2013; Babecky et al., 2014). We follow these lines and include expectations about the state of the world economy into the list of our short-term predictors. We use a leading indicator of GDP growth in OECD countries as a proxy for expectations about the future growth of the global economy. OECD countries represent a large part of the world economy and include the United States, European Union, the United Kingdom, and several large emerging economies.

The fourth indicator, *term spread*, represents the difference between long- and short-term interest rates and is confirmed to be among the most powerful predictors of recessions (Ahrens, 2002; Kauppi and Saikkonen, 2008; Nyberg, 2010), with an added value beyond other recession predictors (Estrella and Mishkin, 1998; Christiansen et al., 2014; Liu and Moench, 2016; Bluedorn et al., 2016; Aastveit et al., 2019). The narrowing of the term spread predicts the onset of a recession. The first channel explaining this negative link is that a temporary monetary tightening affects short-term rates stronger than the long-term ones[101] and thus leads to a flattening, or even an inversion, of the yield curve. At the same time, monetary tightening is usually accompanied by a slowdown of economic activity, though with a lag, and, therefore, a decrease in the term spread predicts a recession. The second channel, in turn, comes from the observation that the long-term interest rate conveys important information about corresponding inflation expectations (Ahrens, 2002). Flattening of the yield curve thus indicates a reduction of inflation expectations. Note that

[101] The argument is based on the expectations theory of the term structure (Ahrens, 2002).

recessions are usually accompanied by a decline in inflation. This explains the observed negative relationship between the term spread and the probability of recession. We calculate term spread as the difference between long- and short-term interest rates. We use the interest rate on ten-year government bonds as the long-term rate and use three-month money market or treasury bills rates as the short-term interest rate.

Our final classical recession predictor is the *short-term interest rate*. This usually measures the cost of interbank lending and thus reflects both credit risk, i.e., the risk of default of counterparty banks, and liquidity risk. An increase in the short-term interest rate reflects a rise of perceived default risk on interbank loans, and hence induces lenders to demand a higher risk premium on loans. In addition, if there is a drying up of liquid funds on the interbank market due to, for example, depositors' panic, the cost of interbank lending also rises. As shown in Gertler et al. (2019), this liquidity shortage and associated banking panics have disruptive macroeconomic consequences. Note that under an inflation-targeting regime, the central bank controls short- and long-term interest rates through market operations. Therefore we use the short-term interest rate as a proxy for monetary conditions throughout the paper. Other papers that exploit the predictive ability of the short-term interest rate for predicting recessions include Estrella and Mishkin (1998), Christiansen et al. (2014), and Liu and Moench (2016). We construct the real short-term interest rate by subtracting annual past CPI inflation from the nominal short-term rate.

Financial overheating and external imbalance indicators.

The first indicator is the rate of housing price growth. We construct annual log growth of the residential property price index (RPI) from BIS. The next two indicators exploit the open economy features and measure the intensity of external imbalances: net foreign capital inflow and changes in the real price of domestic currency. As a proxy for the changes in foreign liabilities, we use the level of the current account balance (CAB) to GDP ratio, in which both the numerator and denominator are summed over four previous quarters. In the estimation, we take negative CAB to GDP to treat an increase in current account balance as a decrease in foreign liabilities, and a decrease in CAB to GDP as an increase in net foreign capital inflow to fund the raising current account deficit. Further, we use annual log growth of the real effective exchange rate (REER) as the final external imbalance indicator. The data is retrieved from BIS and from the IFS database.

Appendix no. 2: Data sources and variable transformation

Table 1. Data sources and transformations *(beginning)*

Variable name	Raw data	Raw data transformation	Raw data sources
Panel 1: Recession			
I {Recession = 1}	Gross Domestic Product, Volume, Index	Bry Boshan Quarterly (BBQ) busines cycle dating algorithm Harding and Pagan (2002) applied to seasonally adjusted (X13) GDP index. Indicator variable, takes value 1 if an economy is in a recession, 0—otherwise	IFS
Panel 2: Classical recession predictors			
Stock market	Share prices index, common shares of companies traded on national or foreign stock exchanges	Deflated by CPI, annual log growth	IFS
Confidence	OECD standardised business and consumer confidence indicators, amplitude adjusted (long term average=100), seasonally adjusted	First principal component of business and consumer confidence	OECD
World economy	Composite leading indicator (CLI) for the group of OECD countries (OECD total). Leading indicator of the annual growth rate. 12-month rate of change of the trend restored CLI	-	OECD
Term spread	Long- and short-term interest rates	Difference between long- and short-term interest rates. Approximates the shape of the yield curve	OECD
Short-term interest rate	The rate on short-term lending between financial institutions or the rate at which short-term government bonds are issued or traded in the market	-	OECD
Panel 3: Bank credit: various transformations			
Bank Credit / GDP	Bank credit to the private non-financial sector to GDP ratio	Annual log growth	BIS
Real Bank Credit	Bank credit to the private non-financial sector (expressed in domestic currency)	Deflated by CPI, annual log growth	BIS (credit), IFS (CPI)
(Bank Credit / GDP)—HP trend	Bank credit to the private non-financial sector to GDP ratio	Hodrick-Prescott filter with smoothing parameter λ (varies), log-deviation from from its quasi-real-time HP-trend	BIS

Table 2. Data sources and transformations *(continuing)*

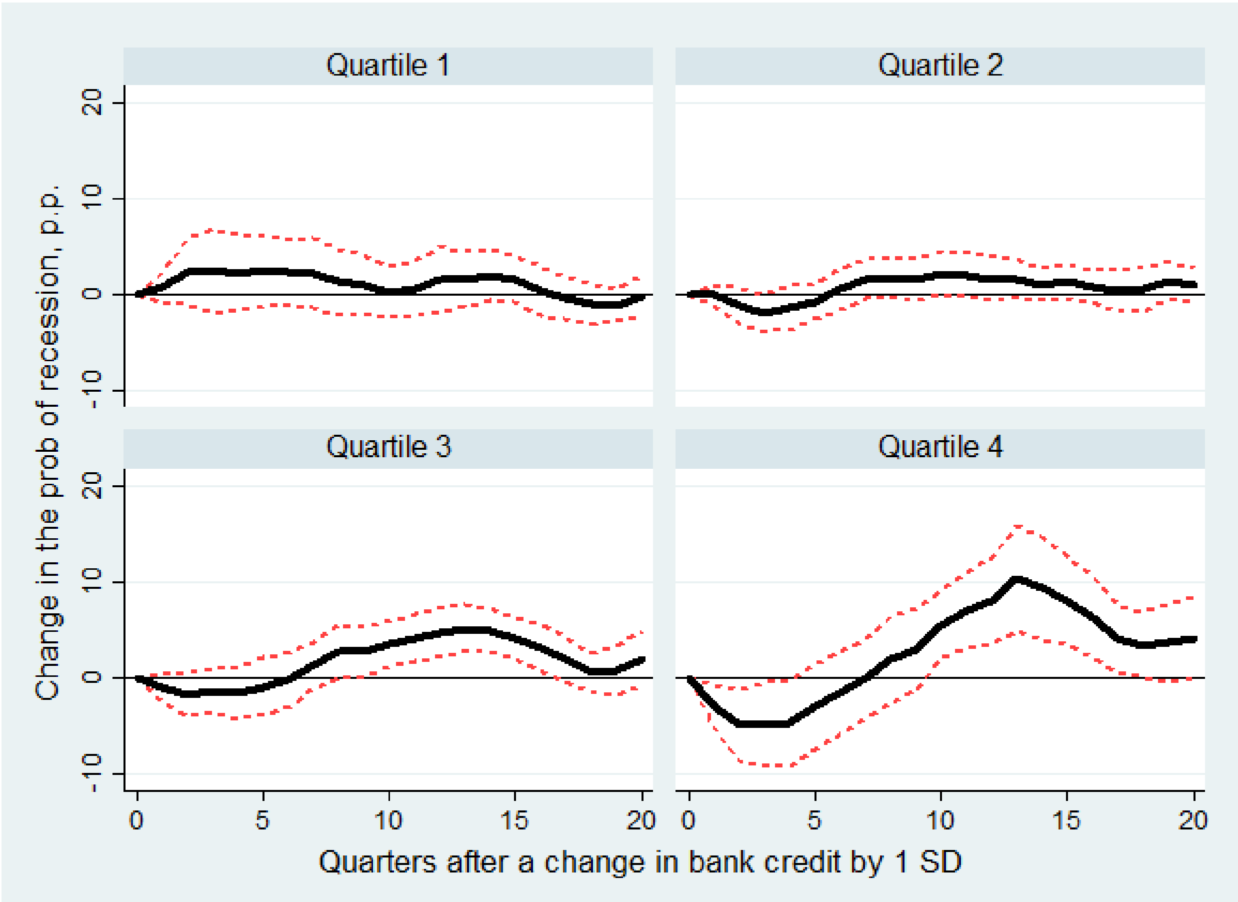
Variable name	Raw data	Raw data transformation	Raw data sources
Panel 4: Alternative measures of credit			
Total credit / GDP	Total credit to the private non-financial sector to GDP ratio	Annual log growth	BIS
Household credit / GDP	Total credit to households to GDP ratio	Annual log growth	BIS
Firm credit / GDP	Total credit to non-financial corporation to GDP ratio	Annual log growth	BIS
Panel 5: Financial overheating and trade imbalance indicators			
RPI	Real Residential Property Price Index	Annual log growth	BIS
CAB / GDP	Current account balance (U.S. dollars); Nominal GDP (domestic currency); Domestic currency per U.S. dollar exchange rates (period average)	Quarterly CAB summed over past four quarters divided by the annual sum of nominal quarterly GDP expressed in U.S. dollars	IFS (CAB, GDP), BIS (exchange rates)
Panel 6: Data for the 5-variable VAR			
GDP volume index	Gross Domestic Product, Real, Domestic Currency, 2010 = 100	Logarithm, seasonally adjusted (X13)	IFS
CPI	Prices, Consumer Price Index, All items, Index, 2010=100	Logarithm, seasonally adjusted (X13)	IFS
Short-term interest rate	same as in *Panel 2*		
Lending rate	Financial, Interest Rates, Lending Rate, Percent per annum		IFS
Bank credit	Bank credit to the private non-financial sector, national currency	Logarithm	BIS

Appendix no. 3: Descriptive statistics

Table 1. Descriptive statistics, 25 countries over 1981 Q1—2019 Q4 (with gaps)

	Obs	Mean	SD	Min	Max
	(1)	(2)	(3)	(4)	(5)
Panel 1: Dependent variable $Y_{i,t} = I\{recession = 1\}$					
Binary indicator variable of recession	1,932	0.15	0.36	0.00	1.00
Panel 2: Classical recession predictors $= X_{i,t}$					
Δ log (Real stock market index), four quarter change	1,932	5.51	23.00	–86.52	156.41
Composite confidence indicator (first principal component of business & consumer confidence)	1,932	1.83	120.29	–443.92	335.41
Composite leading indicator, OECD countries	1,932	2.19	1.63	–4.31	5.46
Term spread	1,932	1,26	1,93	–10.93	24.70
Real short-term interest rate	1,932	1.33	2.45	–10.00	12.36
Panel 3: Bank Credit: various transformations $= Credit_{i,t}$					
Δ log (Loans/GDP), four quarter change	1,932	1.42	5.37	–42.04	24.30
Δ log (Real loans), four quarter change	1,932	3.88	6.55	–22.91	41.32
Log (Loans/GDP)—HP trend, $\lambda = 1,600$	1,932	–0.41	3.47	–23.40	15.03
Log (Loans/GDP)—HP trend, $\lambda = 26,000$	1,932	–1.39	7.67	–48.80	19.29
Log (Loans/GDP)—HP trend, $\lambda = 400,000$	1,932	–0.41	3.47	–23.40	15.03
Panel 4: External imbalance indicators					
Δ log (Real residential property price index), four quarter change	1,932	2.30	7.07	–28.56	51.31
Current account balance/GDP, four quarter	1,748	0.73	4.38	–10.02	15.12
Δ log(REER), four quarter change	1,718	–0.05	6.15	–27.64	35.33

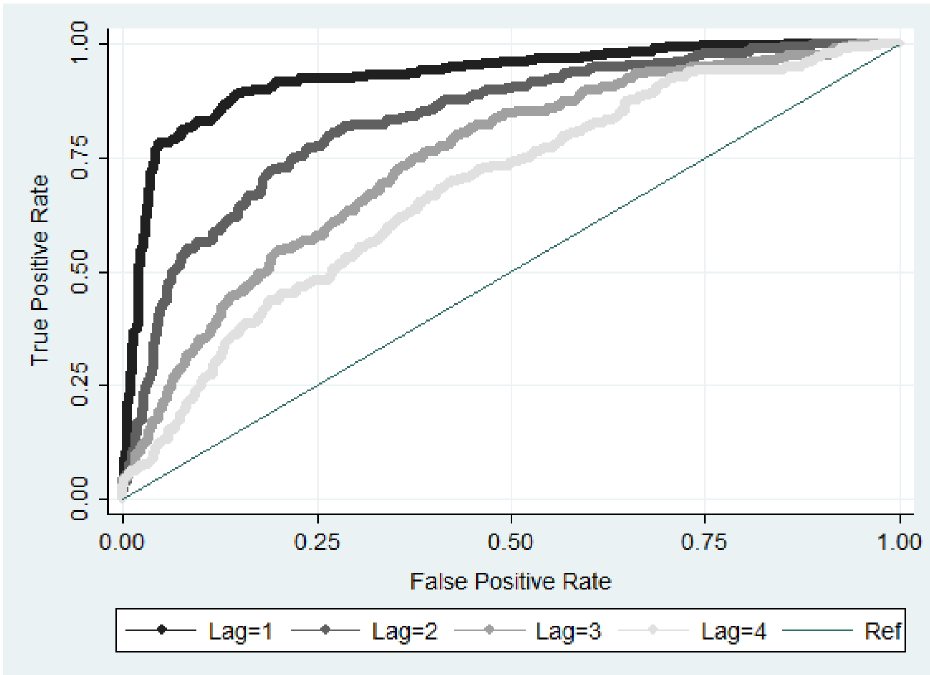
Appendix no. 4: Local projections using quartiles of bank credit



Note: The figure reports Jorda LP estimation results, as implied by equation (1), with the only exception that here we estimate the sequences of coefficients $\beta^{(h)}$ $(h = 1, 2, ..., 20)$ for each of the four quartiles of bank credit distribution simultaneously in one equation. Initial impact of bank credit is normalized to its one standard deviation: +3.0 pp in Q_1, +0.6 pp in Q_2, +1.7 pp in Q_3, +4.5 pp in Q_4. The bank credit variable is the annual log growth of domestic credit to GDP ratio. Standard errors are clustered on country and year level.

Figure 1. Impulse responses of the probability of recession to a credit expansion

Appendix no. 5: Evaluation of binary events forecasting models: ROC-analysis



Note: True and false positive rates are defined as in the classification table below.

Figure 1. ROC curves after dynamic logit models of recession with classical recession predictors, lags $k = 1...4$ quarters

Table 1. Classification of "true" and "false" recessionary events

	True state	
	Recession	Expansion
Predicted state		
Recession	TP	FP
Expansion	FN	TN

Note: TP is true positive outcome, FP is false positive outcome, FN is false negative outcome, and TN is true negative outcome.

Appendix no. 6: Structural vector autoregression with sign restrictions

A typical reduced-form VAR(p) process reads as (exposition follows Kilian and Lutkepohl, 2017):

$$y_t = A_1 y_{t-1} + ... + A_p y_{t-p} + u_t \tag{34}$$

where y_t is a $K \times 1$ vector of endogenous variables, A_h is h^{th} $K \times K$ matrix of coefficients ($h = 1...p$), u_t is the reduced-form residuals with variance-covariance matrix $E(u_t u_t') = \Sigma_u$, p is the VAR lag.

Rewriting model (34) in the VAR(1) representation, we obtain:

$$Y_t = AY_{t-1} + U_t, \tag{35}$$

where $\mathrm{Y}_t = \begin{pmatrix} y_t \\ y_{t-1} \\ ... \\ ... \\ y_{t-p+1} \end{pmatrix}$, $A = \begin{pmatrix} A_1 & A_2 & ... & A_{p-1} & A_p \\ I_k & 0 & ... & & 0 \\ 0 & I_k & ... & & 0 \\ & & ... & & \\ 0 & 0 & ... & I_k & 0 \end{pmatrix}$, $U_t = \begin{pmatrix} u_t \\ 0 \\ ... \\ ... \\ 0 \end{pmatrix}$

Further, we are interested in the identification of orthogonal shocks w_t, such that $E(w_t w_t') = I_k$ and which have an economic interpretation. To do so, we first establish the link between the reduced-form residuals u_t and structural shocks w_t in the following way:

$$u_t = B_0^{-1} w_t \tag{36}$$

To move from nonstructural residuals to structural shocks and estimate impulse responses, one has to find an appropriate B_0^{-1} matrix. Usually, B_0^{-1} summarizes identification assumptions.

Once B_0 is defined, impulse response functions (IRFs) of variables in y to structural shocks are obtained as follows:

$$IRF(h) = \frac{\partial y_{t+h}}{\partial w_t'} = \Theta_h = JA^h J' B_0^{-1}, \tag{37}$$

where $J = [I_K, \quad 0_{K \times K(p-1)}]$ and A is defined above.

We estimate the VAR model (34) using the Bayesian methods[102] with flat prior on its

[102] We turn to the Bayesian methods because the sign restriction approach is most commonly implemented in this way (Kilian and Lutkepohl, 2017). Only a few empirical applications construct frequentist confidence sets for sign-identified structural impulse responses. If the frequentist inference is applied, impulse responses

coefficients. We use the recent algorithm from Arias et al. (2018) and Antolin-Diaz and Rubio-Ramirez (2018), which draws from a conjugate uniform-normal-inverse-Wishart posterior distribution[103].

The Bayesian approach to sign restrictions involves the following steps (Kilian and Lutkepohl, 2017):

Step 1. Take a random rth draw from the posterior of VAR coefficients and covariance matrix (A^r, Σ_u^r). Compute the lower-triangular Cholesky decomposition of Σ^r, $P = chol(\Sigma_u^r)$.

We know that $u_t = P\eta_t$, and by construction, η_t are mutually uncorrelated and have unit variance. η_t, however, does not necessarily have economic interpretation. To overcome this, one can search for candidate solutions w_t^* for the unknown structural shocks w_t as $w_t^* = Q'\eta_t$, where Q' is a square orthogonal rotation matrix with $Q'Q = QQ' = I_k$ and $u = PQQ'\eta_t = PQw_t^*$. Therefore, the next step of the procedure reads as:

Step 2. For each (A^r, Σ_u^r), consider K random draws of the rotation matrix Q (such that $QQ' = I$), and for each triple (A^r, Σ^r, Q) compute implied structural impulse responses Θ^r, see equation (37).

Note that for each r-th draw, and for each rotation matrix Q, we know B_0^{-1} because $B_0^{-1} = PQ = chol(\Sigma_u^r)Q$.

Step 3. If Θ^r satisfies sign restrictions, store them; otherwise, discard them.

Step 4. Repeat steps 1–3 until the appropriate amount of draws is stored.

For each successful draw of parameters and rotation matrix, we store the time evolution of the structural shocks w_t. We then use medians of each shock in the baseline regression analysis under our two-stage approach and we also use all percentiles of the shocks in the sensitivity analysis.

tend to be wide and uninformative (Kilian and Lutkepohl, 2017).

[103] We use Matlab code provided in Antolin-Diaz and Rubio-Ramirez (2018), https://www.openicpsr.org/openicpsr/project/113168/version/V1/view.

Appendix no. 7: Data and estimation of country-level SVAR-models

We follow Gambetti and Musso (2017) and isolate four expansionary shocks: aggregate demand (AD), aggregate supply (AS), monetary policy (MP), and credit supply (CS) shocks, for each of the 25 countries in our sample. To do so, we impose the set of restrictions on the contemporaneous impact of shocks on variables, as presented in Table 1.[104]

Table 1. Sign restrictions

Shock	Real GDP	Inflation	Short-term interest rate	Lending rate	Loans
Aggregate supply	+	–	No restriction	No restriction	No restriction
Aggregate demand	+	+	+	+	No restriction
Monetary policy	+	+	–	No restriction	No restriction
Credit Supply	+	+	+	—	+

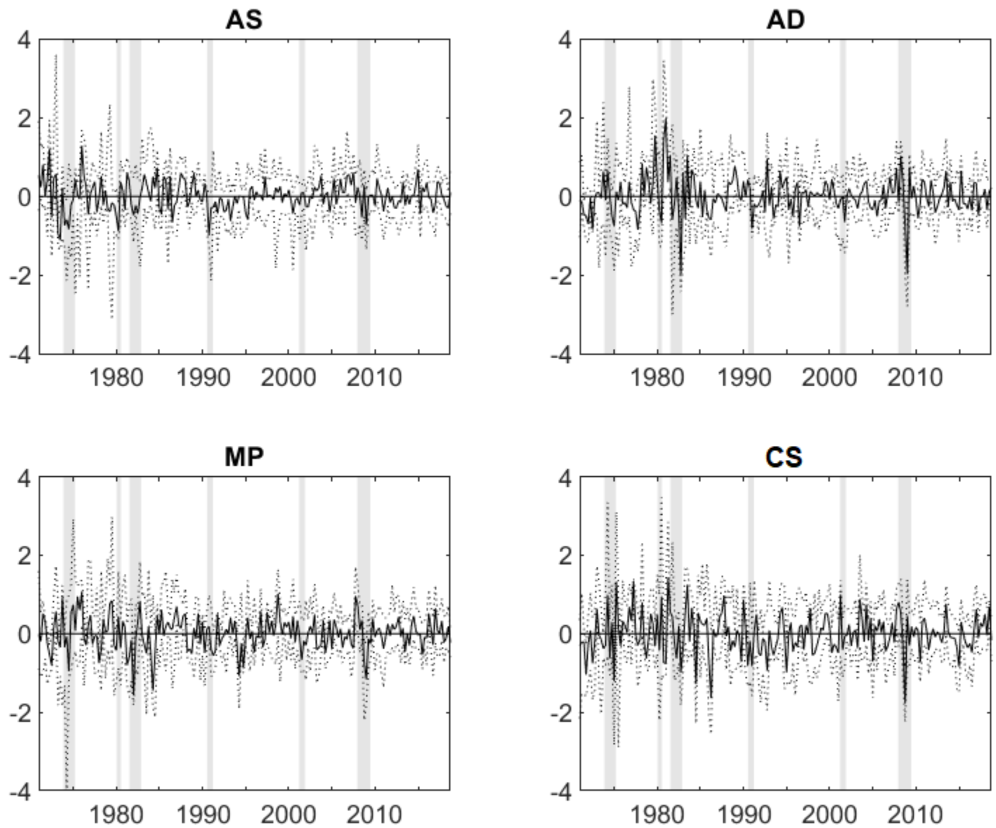
Note: Sign restrictions are imposed on impact of variables. Source: Gambetti and Musso (2017).

Data. To estimate the Gambetti and Musso (2017) five-variables SVAR-model, we use the quarterly data from several sources and compute the logarithm of the seasonally adjusted GDP index (X13), the logarithm of the seasonally adjusted consumer prices index (X13), short-term interest rate, lending rate, and the logarithm of the nominal amount of loans extended by domestic banks to the private non-financial sector. Output, prices, and lending rate are taken from the IMF IFS database, short-term interest rate comes from the OECD, and the amount of loans is retrieved from the BIS. Details on variable construction and data sources are also provided in 4.5.

Estimated business cycle shocks. Using this data, we estimate 25 country-level SVAR models, assuming that each country has its own coefficients, which we believe is a more flexible alternative than a panel SVAR model. To implement the Gambetti and Musso (2017) sign restrictions scheme, we apply Arias et al. (2018)'s sampling approach. All the technical details on our SVAR-model implementation are reported in 4.5.

We now discuss shocks we obtained from our country-level SVAR model. This is important because we build our further analysis on these shocks. We first average the identified shocks across all countries at each quarter and then plot the resultant time evolution. Figure 1 depicts the result of this exercise. All shocks are represented such that the positive values reflect expansionary shocks or easing (in the case of the MP shocks) and the negative values imply contractionary shocks (or tightening of MP). In addition, we also plot the dates of U.S. recessions, as defined by NBER.

[104]We also ran an estimation in which the restrictions were imposed 'on impact' plus one quarter after the shocks. The results did not change.



Note: Each subfigure presents the country distribution of four identified macroeconomic shocks. "AS" denotes aggregate supply, "AD"—aggregate demand, "MP"—monetary policy, "CS"—credit supply. Positive values mean expansions or easing (in the case of MP shocks) and negative values reflect contractions or tightening. Each plot contains median (solid line) and 16^{th} and 84^{th} percentiles. Shaded areas correspond to recessions in the U.S. economy, as dated by NBER.

Figure 1. Time evolution of the SVAR-estimated macroeconomic shocks (averaged across countries)

As can be inferred from the figure, we identify several episodes of sizable negative AS shocks in the 1970s and early 1980s—the periods corresponding to the world oil price shocks (Kilian, 2009; Antolin-Diaz and Rubio-Ramirez, 2018). We then identify large positive spikes in the estimated AD shocks in the early 1980s, which are related to the period of high inflation. These positive AD shocks are then quickly followed by negative AD shocks, which can be attributed to recessions in many advanced economies. We identify another large negative AD shock around the global economic crisis of 2007-2009.

Regarding monetary policy, we identify negative MP shocks in the early 1980s, which can be related to the anti-inflationary policies adopted by central banks to curb inflation. Somewhat less sizable negative MP shocks, i.e., rising interest rates, are identified by our model for the periods of the mid-1990s and the end-2000s. For the latter period, the negative MP shocks correspond to a zero lower bound constraint, which was binding in many advanced countries during those times (Guerrieri and Lorenzoni, 2017). Put differently, the interest rates were actually higher than the model predicts, conditional on poor macroeconomic conditions during the crisis times.

Finally, we identify positive CS shocks for the periods of the 1970s and early 1980s and we obtain negative CS shocks in the second half of the 1980s. We reveal another large negative CS shock during the period of the global economic crisis of 2007-2009. The latter corresponds to the sharp contraction of loan supply by banks in developed countries Gambetti and Musso (2017) and around the globe Mian et al. (2017) in those times.

Overall, our analysis identifies large shocks prior to the mid-1980s, which were followed by a period of small shocks in the 1990s and early-2000s ("Great moderation", Stock and Watson, 2003). The era of small shocks had been terminated by the global economic crisis which saw many large negative demand-type shocks (Mian et al., 2021).

We further check if the estimated shocks have well-defined distribution, e.g., do not contain more than one hump, or do not have fat tails, which might impose problems on further regression estimates. We plot the empirical densities of our shocks and show that the densities are statistically close to the Normal distribution (see Fig. 2). Based on the analysis, we conclude that our shocks are reliable and could be used in subsequent regression analysis.

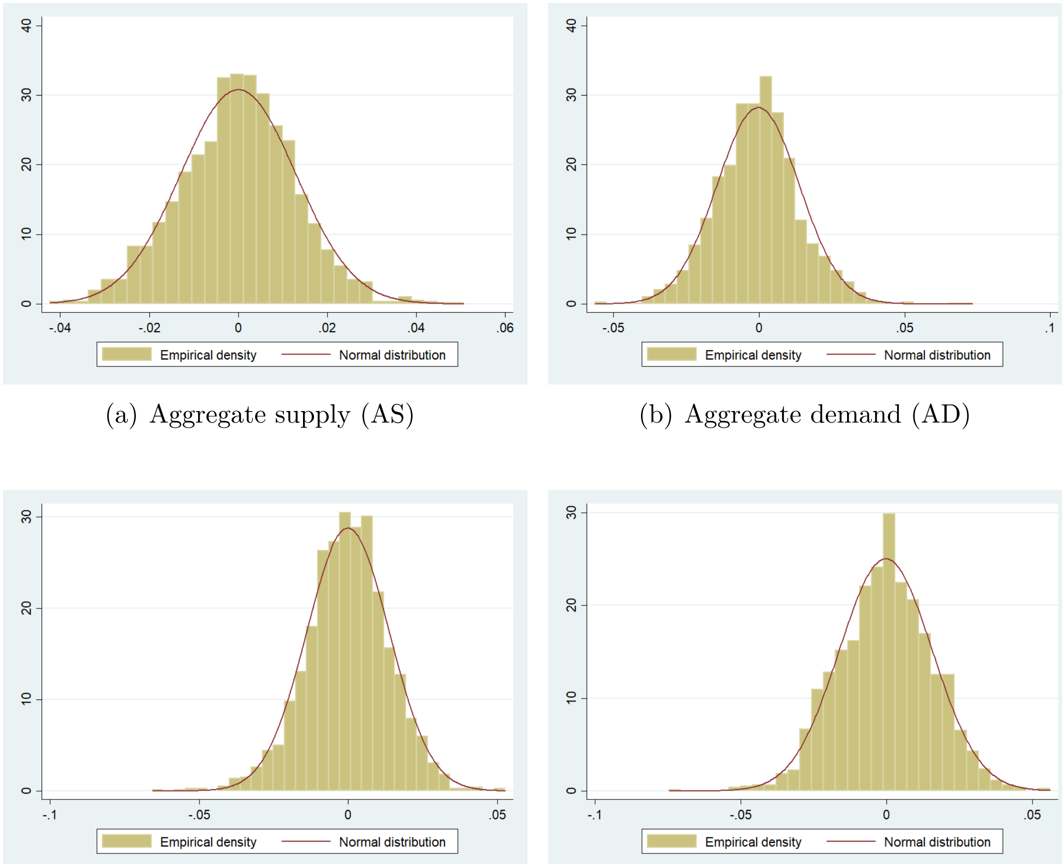
Impulse responses. We average the estimated country-level IRFs across the countries. Estimated IRFs are reported in Figure 3. As can be verified from the figure, the estimated signs of IRFs correspond to those imposed by the sign restrictions. Since we are not interested in the IRFs themselves, we no longer pay attention to them.

Historical decomposition of bank credit

As shown in Kilian and Lutkepohl (2017), each suitably demeaned and detrended variable in the VAR can be decomposed on structural shocks:

$$\hat{y}_t = \sum_{s=0}^{t-1} \Theta_s w_{t-s} \tag{38}$$

where Θ_s are structural MA coefficient matrices and w_t is a vector of structural shocks. Historical contribution $\hat{y}_{kt}^{(j)}$, $j = 1...K$, of shock j to a k-th variable in vector y_t can be interpreted as a cumulative effect of structural shock j to y_{kt} up to time t. In other words, the historical decomposition of a variable on shock j shows how this variable would have evolved if all other than j-th shock would be turned off. Historical decomposition naturally provides counterfactual dynamics of variables driven by a shock of interest.



(a) Aggregate supply (AS)

(b) Aggregate demand (AD)

(c) Monetary policy (MP)

(d) Credit supply (CS)

Note: The figure reports empirical densities (*bars*) of the four shocks estimated with the country-level SVAR models against the background of corresponding Normal densities (*lines*). The SVAR models for each country contain five variables (GDP, CPI, short-term interest rate, interest rate on bank credit, and the amount of bank credit). The four shocks are: aggregate supply (AS), aggregate demand (AD), monetary policy (MP), and credit supply (CS). All shocks are set as expansionary.

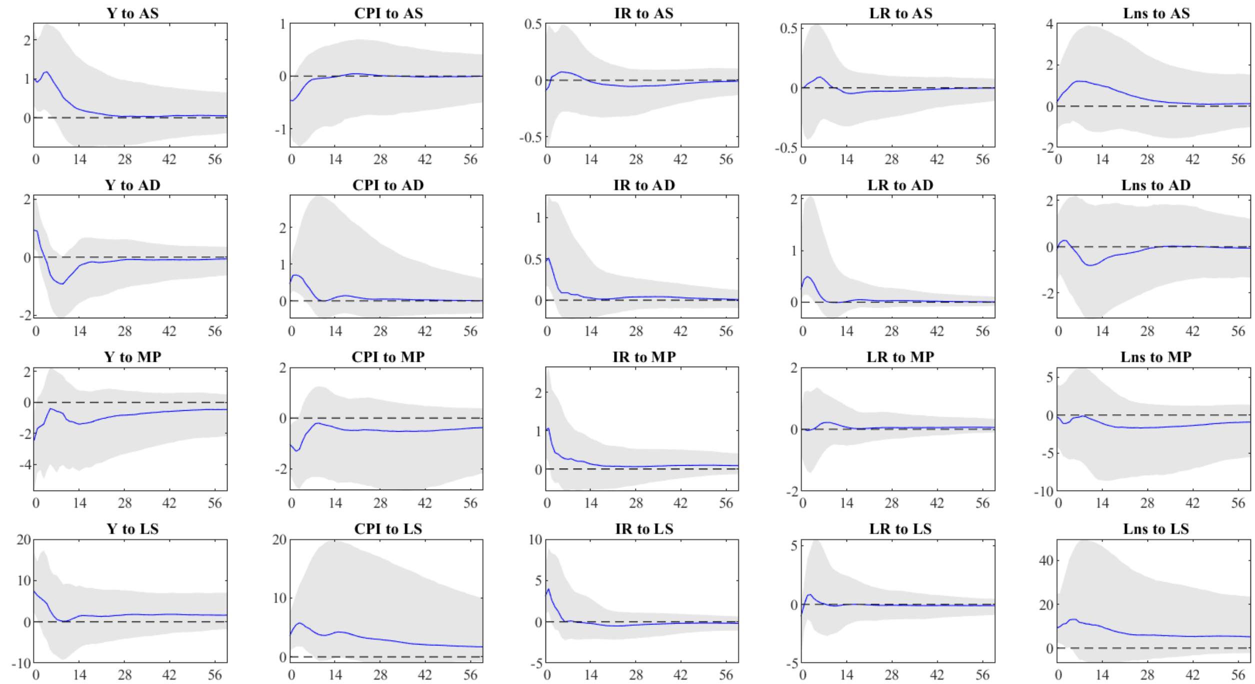
Figure 2. Empirical densities of the SVAR-estimated shocks

Recall that our five-variable VAR contains the following endogenous variables:

$$y = (\ln \underbrace{GDP}_{\text{real GDP}}, \ln \underbrace{P}_{\text{CPI}}, \underbrace{i^{SR}}_{\text{Short-term interest rate}}, \underbrace{i^{Lns}}_{\text{Lending rate}}, \ln \underbrace{Lns}_{\text{nominal loans}})$$

In our case, historical decomposition based on the VAR model provides $\hat{y}_{kt}^{(j)}$—a contribution of each of the four identified structural shocks $j = 1...4$, to each of our five variables of interest.

Using the VAR model, we construct counterfactual dynamics of the credit growth variable $Credit_{i,t}$, which is the annual log growth of the bank credit to GDP ratio. Denote $y_1 =$



(a) All countries, *unscaled responses*

Note: Each row represents responses of the five variables to respective shock identified with sign restrictions (see the scheme in Table 1): AS is aggregate supply, AD is aggregate demand, MP is monetary policy, LS is loan supply. Y is the logarithm of the real GDP index, seasonally adjusted, CPI is the logarithm of the consumer price index, seasonally adjusted, IR is the short-term interest rate, LR is lending rate, and $Loans$ are the logarithm of nominal loans issued by domestic banks to the private non-financial sector.

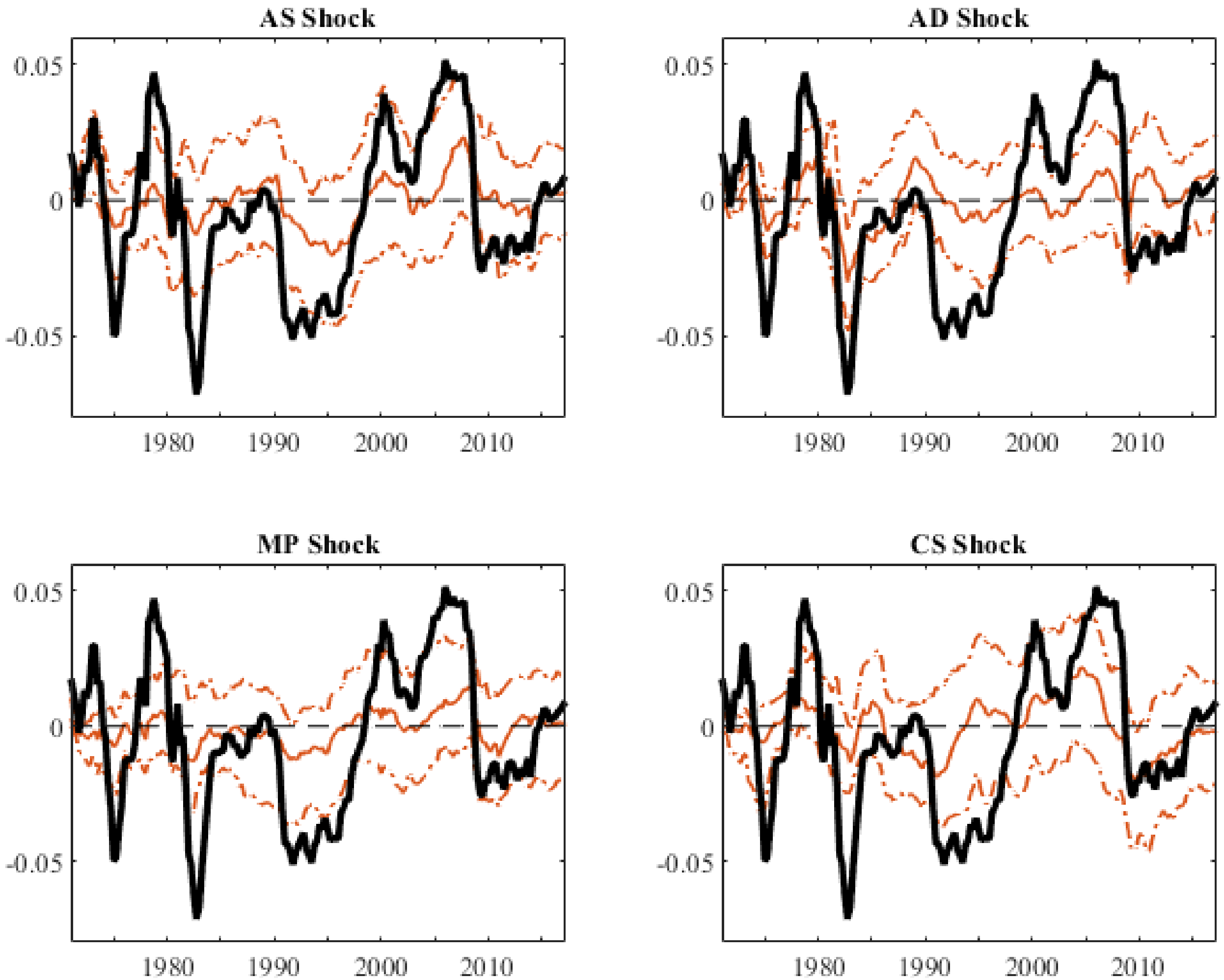
Figure 3. Impulse response functions on identified macroeconomic shocks, average across countries

$\ln GDP$, $y_2 = \ln P$, $y_3 = i^{SR}$, $y_4 = i^{Lns}$, $y_5 = \ln Lns$. Then, we can reconstruct counterfactual dynamics of annual log growth of the bank credit to GDP ratio driven by a shock j, $\widehat{Credit}_t^{(j)}$ as follows:

$$\widehat{Credit}_t^{(j)} = \left(\hat{y}_{5,t}^{(j)} - \hat{y}_{1,t}^{(j)} - \hat{y}_{2,t}^{(j)}\right) - \left(\hat{y}_{5,t-4}^{(j)} - \hat{y}_{1,t-4}^{(j)} - \hat{y}_{2,t-4}^{(j)}\right) \tag{39}$$

This yields counterfactual dynamics of $\widehat{Credit}$ driven by each shock of interest, which we further include in Jorda's local projections.

Figure 4 presents an example of how identified business cycle shocks contribute to the first variable in the VAR model, y_1, output in one of the countries in our sample—the U.S. The estimated historical decomposition provides insight into which shocks were important for output variation in different time periods. As can be inferred from the figure, there was a large negative contribution of aggregate demand shock in the early 1980s coupled with



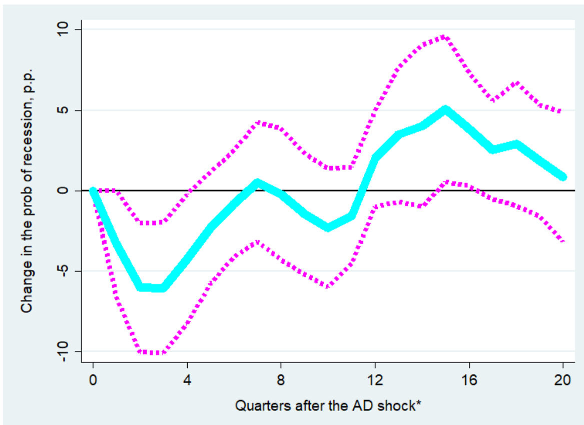
Note: The figure reports real GDP in deviation from its deterministic trend (solid black line) and the estimated median contribution of four business cycle shocks to GDP (solid red line), along with the 16th and 84th percentiles (dashed red lines), in the U.S.

Figure 4. Historical decomposition of output using aggregate shocks for the US economy

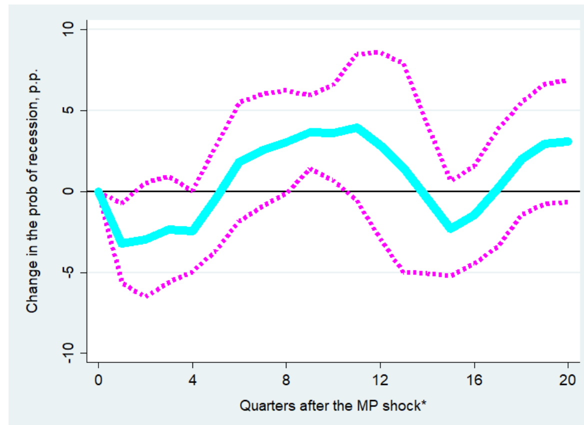
some negative pressure from monetary tightening during the same time. Further, there was a technological slowdown in the 1990s with a subsequent positive contribution of the AS shock to the U.S. GDP in the early 2000s (dot-com growth). Finally, we obtain a growing positive contribution of CS shocks to the U.S. GDP over the 2000s, which then turned into a substantial negative contribution in 2009 (mortgage boom and bust).

This analysis suggests that the estimated historical decomposition of variables to shocks using our VAR model has an economic interpretation and corresponds to recognized events and crises.

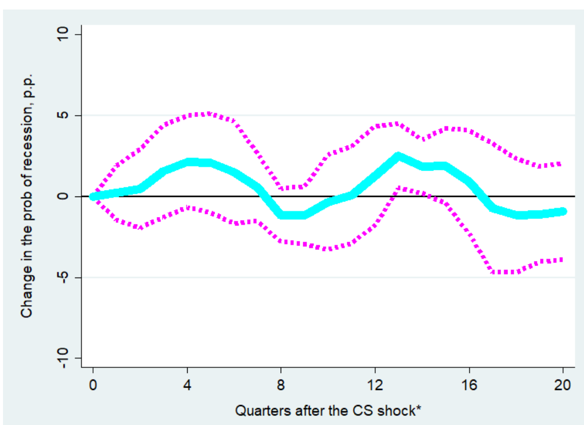
Appendix no. 8: Household credit, business cycle shocks, and the recession response



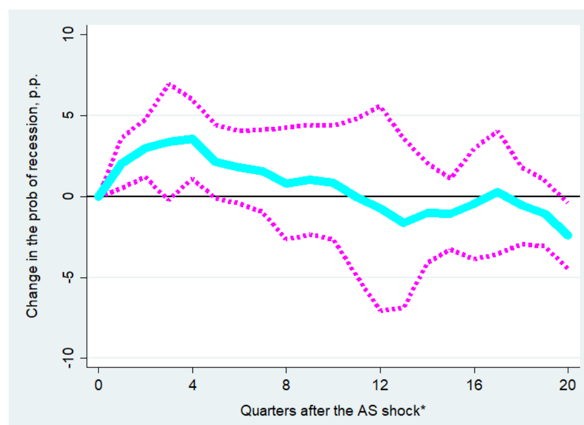
(a) Aggregate demand (AD)



(b) Monetary policy (MP)



(c) Credit supply (CS)

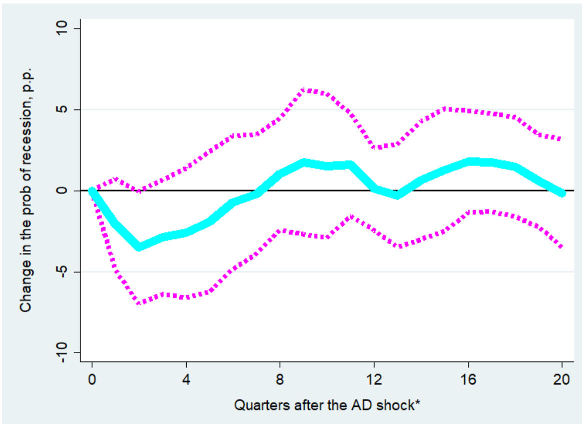


(d) Aggregate supply (AS)

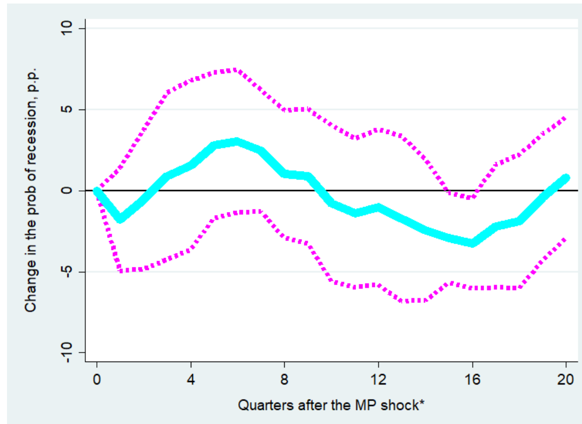
Note: The figure reports Jorda LP estimation results, as implied by a modified version of equation (1). The modification is that we replace the key explanatory variable—four-quarter log difference of household credit to GDP ratio—by a part of it driven by j^{th} aggregate shock ($j = 1, ..., 4$), as implied by our historical decomposition exercise in country-level SVAR models. All shocks are expansionary. In subfigure (a), we estimate the response of the probability of recession to AD-driven expansion of household credit; in subfigure (b) to MP-driven expansion; in subfigure (c) to CS-driven expansion; in subfigure (c) to AS-driven expansion. Each subfigure reports a response to a one standard deviation increase in the corresponding shock-driven credit. Standard errors are clustered on country and year levels.

Figure 1. Impulse responses of the probability of recession to expansions of different types of credit

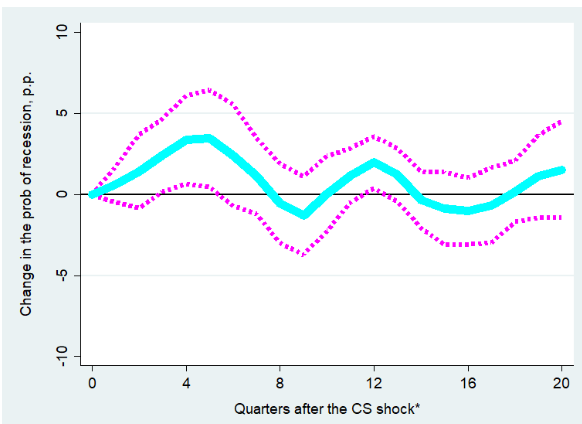
Appendix no. 9: Firm credit, business cycle shocks, and the recession response



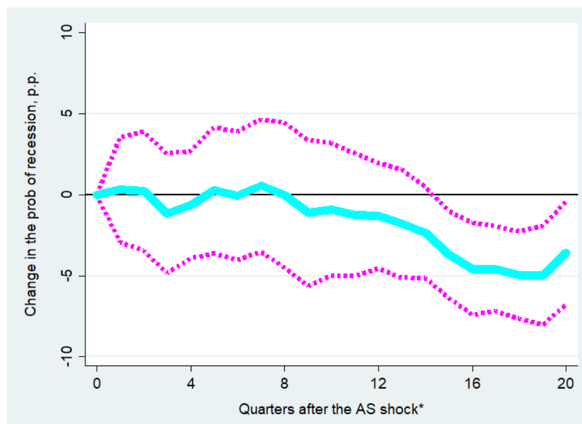
(a) Aggregate demand (AD)



(b) Monetary policy (MP)



(c) Credit supply (CS)



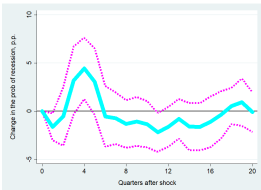
(d) Aggregate supply (AS)

Note: The figure reports Jorda LP estimation results, as implied by a modified version of equation (1). The modification is that we replace the key explanatory variable—four-quarter log difference of firm credit to GDP ratio—by a part of it driven by j^{th} aggregate shock ($j = 1, ..., 4$), as implied by our historical decomposition exercise in country-level SVAR models. All shocks are expansionary. In subfigure (a), we estimate the response of the probability of recession to AD-driven expansion of household credit; in subfigure (b) to MP-driven expansion; in subfigure (c) to CS-driven expansion; in subfigure (c) to AS-driven expansion. Each subfigure reports a response to a one standard deviation increase in the corresponding shock-driven credit. Standard errors are clustered on country and year levels.

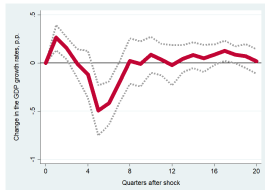
Figure 1. Impulse responses of the probability of recession to expansions of different types of credit

Appendix no. 10: Shocks, credit, and recessions

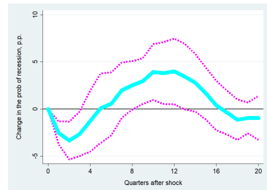
Reduced-form regressions | **'Second-stage' regressions**



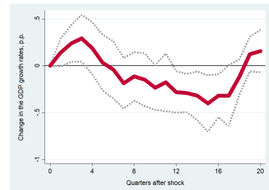
(a) Aggregate demand (AD) → Prob of recession



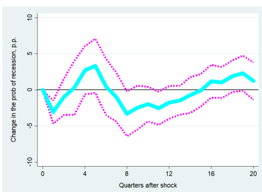
(b) Aggregate demand (AD) → GDP growth



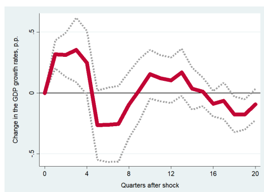
(c) AD-driven credit → Prob of recession



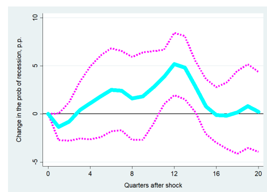
(d) AD-driven credit → GDP growth



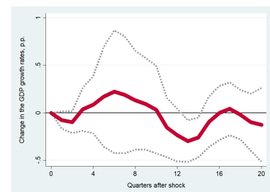
(e) Monetary policy (MP) → Prob of recession



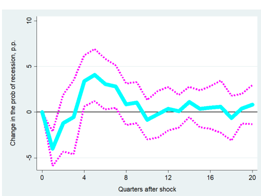
(f) Monetary policy (MP) → GDP growth



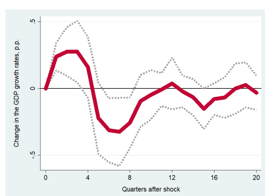
(g) MP-driven credit → Prob of recession



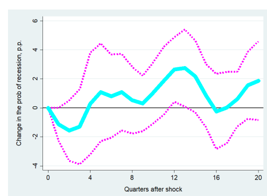
(h) MP-driven credit → GDP growth



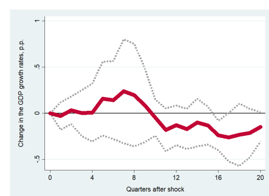
(i) Credit supply (CS) → Prob of recession



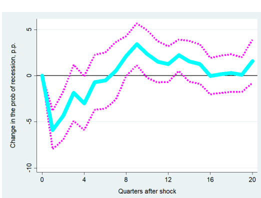
(j) Credit supply (CS) → GDP growth



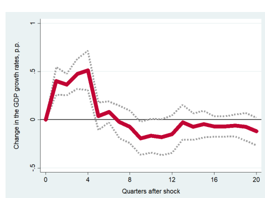
(k) CS-driven credit → Prob of recession



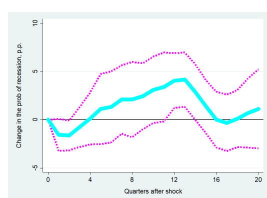
(l) CS-driven credit → GDP growth



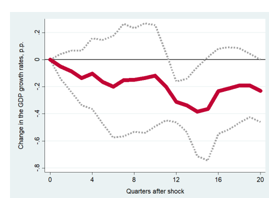
(m) Aggregate supply (AS) → Prob of recession



(n) Aggregate supply (AS) → GDP growth



(o) AS-driven credit → Prob of recession



(p) AS-driven credit → GDP growth

Note: The figure reports Jorda LP estimation results, as implied by modified versions of equation (1). In subfigures (a,e,i,m), the modification is that the key explanatory variable—four-quarter log difference of domestic bank credit to GDP ratio—has been switched to j^{th} aggregate shock ($j = 1, ..., 4$, all expansionary by construction). In subfigures (b,f,j,n), the dependent variable is the GDP growth rates. In subfigures (c,g,k,o), the key explanatory variable has been switched from bank credit to a part of it driven by j^{th} aggregate shock ($j = 1, ..., 4$), as implied by our historical decomposition exercise in country-level SVAR models. Finally, In subfigures (d,h,l,p), the dependent variable has been replaced by GDP growth rates. Each subfigure reports a response to a one standard deviation increase in the respective explanatory variable. Standard errors are clustered on country and year levels.

Figure 1. Aggregate shocks and the economy

Appendix no. 11: Choice of the measure of bank credit

Table 1. Dynamic logit estimation results: different measures of bank credit

$Credit_{i,t}$:=	$\Delta\ln\left(\frac{Credit}{GDP}\right)$	$\Delta\ln(Credit)$	$Credit - \mathrm{HP}_\lambda(Credit)$		
			$\lambda = 1,600$	$\lambda = 26,000$	$\lambda = 400,000$
	(1)	(2)	(3)	(4)	(5)
Panel 1: Domestic bank credit ($Credit_{i,t}$)					
Short lag: $Credit_{i,t-1}$	–9.419***	–8.954***	–17.210***	–13.300***	–17.233***
	(2.152)	(2.191)	(3.308)	(2.728)	(3.312)
Longer lags: $\sum_{j=4}^{20} Credit_{i,t-j}$	18.651***	12.541***	11.464***	14.274***	11.475***
	(3.288)	(2.701)	(4.189)	(2.769)	(4.185)
N Obs.	2,391	2,391	2,407	2,407	2,407
N Countries	25	25	25	25	25
Pseudo R^2	0.542	0.535	0.538	0.540	0.538
Δ AUROC w.r.t. no $Credit_{i,t}$	0.007**	0.006**	0.004**	0.005**	0.004**
Standard deviation of $Credit_{i,t}$, σ_L	0.059	0.075	0.041	0.016	0.041
Δ $\Pr(Y_{i,t} = 1 \mid \Delta Credit_{i,t-1} = \sigma_L)$	–0.031***	–0.037***	–0.038***	–0.068***	–0.038***
Δ $\Pr(Y_{i,t} = 1 \mid \Delta \sum_{j=4}^{20} Credit_{i,t-j} = \sigma_L)$	0.061***	0.052***	0.025***	0.073***	0.026***

Note: The table reports dynamic logit estimates of equation (1) from the main text with different measures of bank credit. The measures of bank credit are as follows: annual log growth of the loans to GDP ratio (1), annual log growth of the real loans (2), deviation of the log of loans to GDP ratio from its HP trend with $\lambda = 1,600$ (3), deviation of the log of loans to GDP ratio from its HP trend with $\lambda = 26,000$ (4), a deviation of the log of loans to GDP ratio from its HP trend with $\lambda = 400,000$ (5). The economic effects are computed as the product of a bank credit variable's one standard deviation and the marginal effect of the respective coefficient in the logit model.

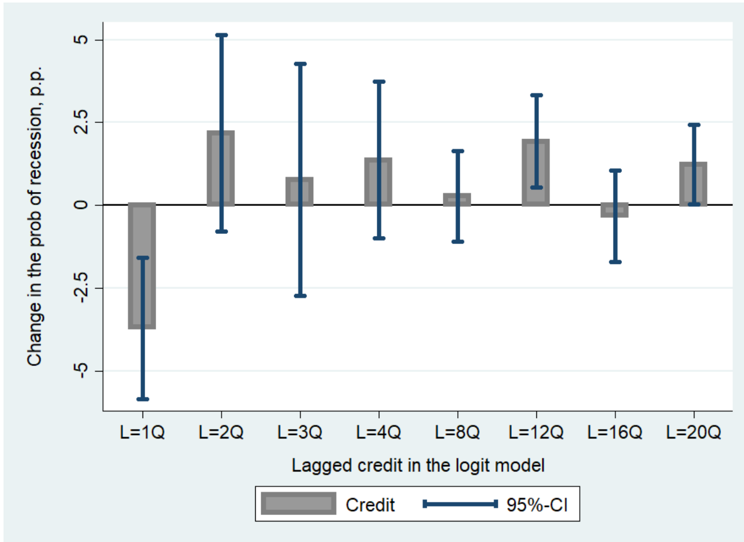
***, **, * indicate that a coefficient is significant at the 1%, 5%, 10% level, respectively. Robust standard errors appear in the brackets under the estimated coefficients.

Appendix no. 12: Varying short lags of bank credit growth

Table 1. Dynamic logit estimation results: varying the short lags of the bank credit variable

The short lag:	$k = 1$	$k = 2$	$k = 3$	$k = 4$
	(1)	(2)	(3)	(4)
Short lag: $\text{Credit}_{i,t-k}$	–9.419***	–8.389***	–8.109***	–0.630
	(2.152)	(2.144)	(2.813)	(1.575)
Longer lags: $\sum_{j=4(4)}^{20} Credit_{i,t-j}$	18.651***	22.253***	25.916***	20.468***
	(3.288)	(3.242)	(3.386)	(2.167)
N Obs.	2,391	2,383	2,374	2,365
N Countries	25	25	25	25
Pseudo R^2	0.542	0.316	0.222	0.167
Δ $\Pr(Y_{i,t} = 1 \mid \Delta Credit_{i,t-1} = \sigma_L)$	–0.031***	–0.044***	–0.049***	–0.004
Δ $\Pr(Y_{i,t} = 1 \mid \Delta \sum_{j=4(4)}^{20} Credit_{i,t-j} = \sigma_L)$	0.061***	0.118***	0.158***	0.134***

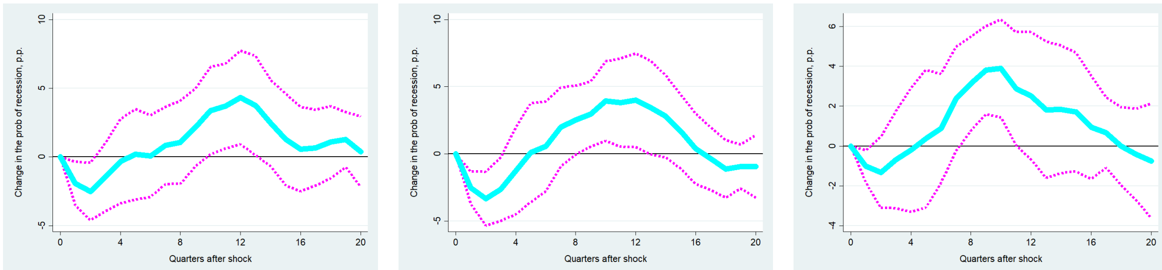
Note: The table reports key estimated coefficients from the dynamic logit model of the risk of recessions, as implied by equation (2). Across the columns of the table, we vary the short lag of each explanatory variable ($k = 1, \ldots, 4$) while fixing the longer lags of bank credit growth at $4, 8, 12, 16, 20$ quarters. All regressions contain the country fixed effects, the full set of classical recession predictors taken with the k^{th} lag, and the k^{th} lag of the dependent variable.
***, **, * indicate that a coefficient is significant at the 1%, 5%, 10% level, respectively. Robust standard errors appear in the brackets under the estimated coefficients.



Note: The figure reports the estimated economic effects based on the results from a modified version of the dynamic logit model of recessions, see equation (1), where we include all short lags—from the 1^{st} to 4^{th}—of the bank credit variable. In the main text, we were using only the 1^{st} lag. The economic effects are computed as the product of the bank credit variable's one standard deviation (+5.9 pp) and the marginal effect of the respective coefficient in the logit model. The bank credit variable is the four-quarter log difference of the domestic credit to GDP ratio.

Figure 1. Economic effects of bank credit on the risk of recession at different horizons

Appendix no. 13: Local projections of the probability of recessions across the percentiles of the AD shock distribution

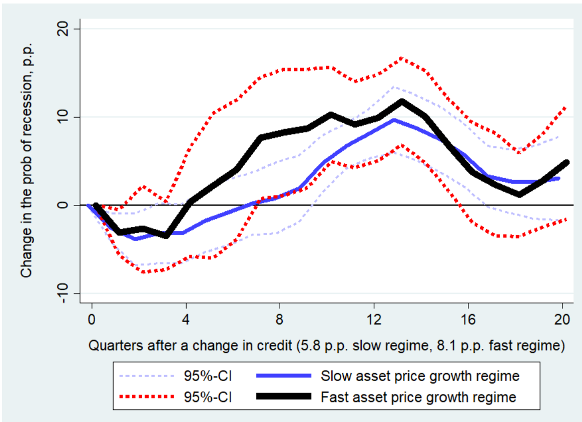


(a) 16^{th} %-tile (b) 50^{th} %-tile (*baseline*) (c) 84^{th} %-tile (*baseline*)

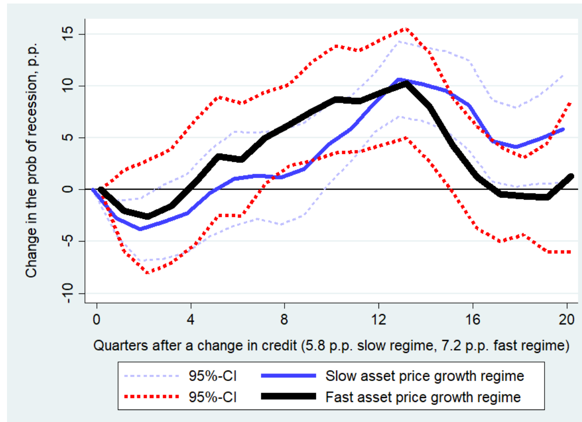
Note: The figure reports Jorda LP estimation results, as implied by modified versions of equation (1). The modification is that the key explanatory variable—four-quarter log difference of domestic bank credit to GDP ratio—has been switched to a part of it driven by an expansionary aggregate demand (AD) shock, as implied by our historical decomposition exercise in country-level SVAR models. In subfigure (a), we draw the 16^{th} percentile of the AD-shock distribution, run the historical decomposition of bank credit with this draw, and then perform Jorda LP estimations. In subfigure (b), we do the same with the 50^{th} percentile of the AD-shock distribution, as in the main text. And in subfigure (c), we move to the 84^{th} percentile of the AD-shock distribution. The figure, therefore, answers the question about the differences in the responses of the probability of recession to credit expansion if the credit expansion is driven by a relatively *small*, *moderate*, or *large* positive shocks to aggregated demand. Each subfigure reports a response to a one standard deviation increase in the respective explanatory variable. Standard errors are clustered on country and year levels.

Figure 1. Estimates of the first and second stages across the percentiles of the estimated aggregate demand (AD) shock distribution

Appendix no. 14: High versus low growth rate of asset prices with Greenwood et al. (2022)'s threshold



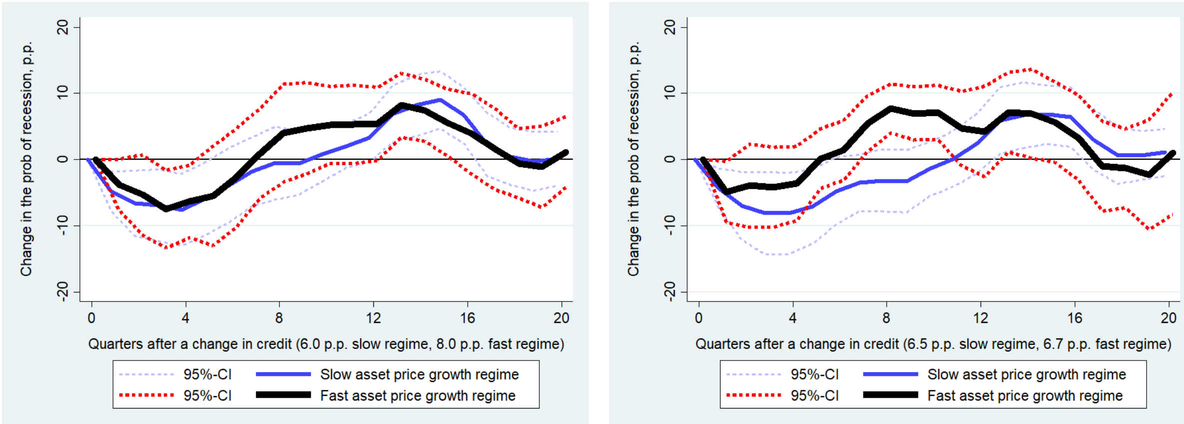
(a) Stock market prices



(b) Residential property prices

Note: Each subfigure reports a k-step ahead prediction of the probability of a recession in quarters $t+k$, $k = 1...20$, following a one standard deviation change of bank credit to GDP growth in quarter t. On both subfigures, we split the sample into two sub-samples of country-quarter observations: those in which a country experiences a high growth rate of its domestic stock market (a) or residential property prices (b) (above the 67^{th} percentile across all countries, in line with Greenwood et al. (2022) and all the rest.

Figure 1. Impulse responses of the probability of recession to bank credit during the periods of *high* and *low growth of domestic stock market*, $k = 1...20$ quarters ahead, threshold $= 67^{th}$ percentile

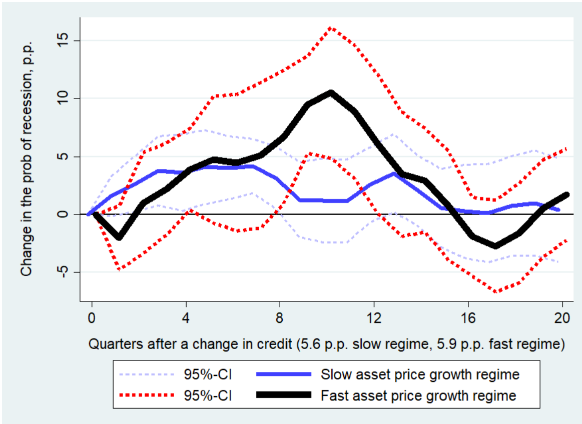


(a) Stock market prices

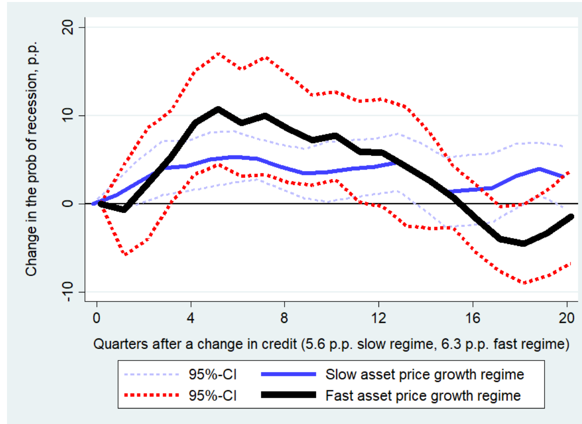
(b) Residential property prices

Note: Each subfigure reports a k-step ahead prediction of the probability of a recession in quarters $t+k$, $k = 1...20$, following a one standard deviation change of household credit to GDP growth in quarter t. On both subfigures, we split the sample into the two sub-samples of country-quarter observations: those in which countries experience extremely high growth rates of the domestic stock market (a) or residential property prices (b) (above the 67^{th} percentile across all countries).

Figure 2. Impulse responses of the probability of recession to *household* credit during the periods of *high* and *low asset price growth*, $k = 1...20$ quarters ahead, threshold $= 67^{th}$ percentile



(a) Stock market prices



(b) Residential property prices

Note: Each subfigure reports a k-step ahead prediction of the probability of a recession in quarters $t + k$, $k = 1...20$, following a one standard deviation change of firm credit to GDP growth in quarter t. On both subfigures, we split the sample into the two sub-samples of country-quarter observations: those in which countries experience extremely high growth rates of the domestic stock market (a) or residential property prices (b) (above the 67^{th} percentile across all countries).

Figure 3. Impulse responses of the probability of recession to *firm* credit during the periods of *high* and *low asset price growth*, $k = 1...20$ quarters ahead, threshold $= 67^{th}$ percentile

Appendix no. 15: Description of the PSID data

Table 1. Data description *(beginning)*

Variable name	Formula	PSID variables	PSID codes [year]code
Bankrupcy indicator	gen bkrpt_indicator1 = (bkrpt_yr1 == year) if bkrpt_yr1≠. and year≤1996	Year of first most recent bankrupcy	[96]ER8917
	gen bkrpt_indicator2 = (bkrpt_yr2 == year) if bkrpt_yr2≠. and year≤1996	Year of second most recent bankrupcy	[96]ER8943
	gen bkrpt_indicator = bkrpt_indicator1 if year≤1996		
	replace bkrpt_indicator = bkrpt_indicator2 if bkrpt_indicator2==1 and year≤1996		
Behind on mortgage indicator	gen bhnd_mtg = bhnd_mtg1	Whether behind on mortgage 1	[09]ER42052 [11]ER47359 [13]ER53059 [15]ER60060 [17]ER66062
	replace bhnd_mtg = 1 if bhnd_mtg2 == 1	Whether behind on mortgage 2	[09]ER42071 [11]ER47380 [13]ER53080 [15]ER60081 [17]ER66083
3 months or more behind on mortgage	gen mths_bhnd_mtg = mths_bhnd_mtg1	Months behind on mortgage 1	[09]ER42053 [11]ER47360 [13]ER53060 [15]ER60061 [17]ER66063
	replace mths_bhnd_mtg = mths_bhnd_mtg1 + mths_bhnd_mtg2 if mths_bhnd_mtg1≠. and mths_bhnd_mtg2≠.	Months behind on mortgage 2	[09]ER42072 [11]ER47381 [13]ER53081 [15]ER60082 [17]ER66084
	rgen npl_mtg = mths_bhnd_mtg≥3		
	replace npl_mtg =. if mths_bhnd_mtg ==.		
Mortgage restructuring	gen restruct = restruct1	Whether worked with lender to restructure mortgage/loan 1	[09]ER42057 [11]ER47364 [13]ER53064 [15]ER60065 [17]ER66067
	replace restruct = 1 if restruct2 == 1	Whether worked with lender to restructure mortgage/loan 2	[09]ER42076 [11]ER47385 [13]ER53085 [15]ER60086 [17]ER66088

Table 1. Data description *(continuing)*

Variable name	Formula	PSID variables	PSID codes [year]code
Employment status	gen employed = (emp==1)		
	replace emp = emp_first if year≥1994	Employment status, head	[68]V196 [69]V639 [70]V1278 [71]V1983 [72]V2581 [73]V3114 [74]V3528 [75]V3967 [76]V4458 [77]V5373 [78]V5872 [79]V6492 [80]V7095 [81]V7706 [82]V8374 [83]V9005 [84]V10453 [85]V11637 [86]V13046 [87]V14146 [88]V15154 [89]V16655 [90]V18093 [91]V19393 [92]V20693 [93]V22448
		Employment status, head, first mention	[94]ER2069 [95]ER5068 [96]ER7164 [97]ER10081 [99]ER13205 [01]ER17216 [03]ER21123 [05]ER25104 [07]ER36109 [09]ER42140 [11]ER47448 [13]ER53148 [15]ER60163 [17]ER66164
Race	replace race = 3 if race>2	race≠.	[68]V181 [69]V801 [70]V1490 [71]V2202 [72]V2828 [73]V3300 [74]V3720 [75]V4204 [76]V5096 [77]V5662 [78]V6209 [79]V6802 [80]V7447 [81]V8099 [82]V8723 [83]V9408 [84]V11055 [85]V11938 [86]V13565 [87]V14612 [88]V16086 [89]V17483 [90]V18814 [91]V20114 [92]V21420 [93]V23276 [94]ER3944 [95]ER6814 [96]ER9060 [97]ER11848 [99]ER15928 [01]ER19989 [03]ER23426 [05]ER27393 [07]ER40565 [09]ER46543 [11]ER51904 [13]ER57659 [15]ER64810 [17]ER70882
	gen white = race==1		
Home ownership status		Own / rent	[68]V103 [69]V593 [70]V1264 [71]V1967 [72]V2566 [73]V3108 [74]V3522 [75]V3939 [76]V4450 [77]V5364 [78]V5864 [79]V6479 [80]V7084 [81]V7675 [82]V8364 [83]V8974 [84]V10437 [85]V11618 [86]V13023 [87]V14126 [88]V15140 [89]V16641 [90]V18072 [91]V19372 [92]V20672 [93]V22427 [94]ER2032 [95]ER5031 [96]ER7031 [97]ER10035 [99]ER13040 [01]ER17043 [03]ER21042 [05]ER25028 [07]ER36028 [09]ER42029 [11]ER47329 [13]ER53029 [15]ER60030 [17]ER66030

Table 1. Data description *(continuing)*

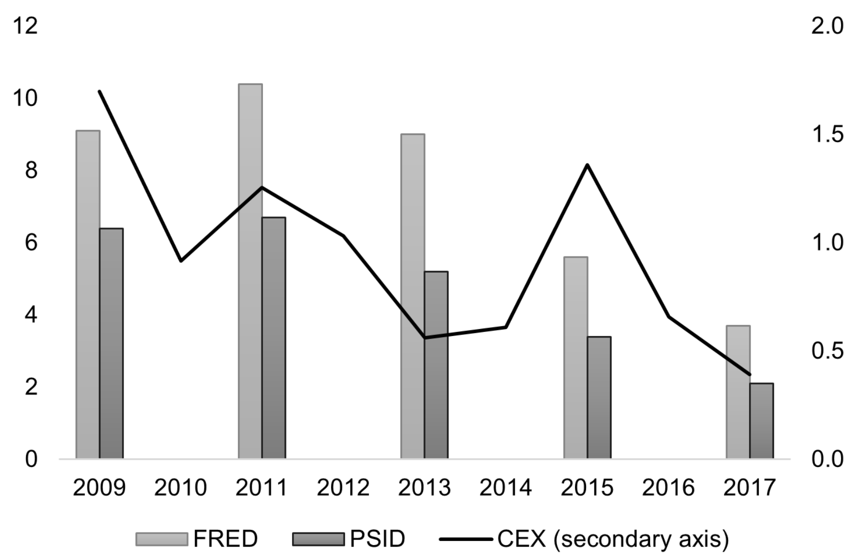
Variable name	Formula	PSID variables	PSID codes [year]code
Education	gen highschool = educ$\leq$ 12	Education (years of completed schooling)	[68]ER30010 [70]ER30052 [71]ER30076 [72]ER30100 [73]ER30126 [74]ER30147 [75]ER30169 [76]ER30197 [77]ER30226 [78]ER30255 [79]ER30296 [80]ER30326 [81]ER30356 [82]ER30384 [83]ER30413 [84]ER30443 [85]ER30478 [86]ER30513 [87]ER30549 [88]ER30584 [89]ER30620 [90]ER30657 [91]ER30703 [92]ER30748 [93]ER30820 [94]ER33115 [95]ER33215 [96]ER33315 [97]ER33415 [99]ER33516 [01]ER33616 [03]ER33716 [05]ER33817 [07]ER33917 09]ER34020 [11]ER34119 [13]ER34230 [15]ER34349 [17]ER34548
	gen somecollege = educgeq13	educ<16	
	gen college = educ$\geq$16		
House value conditional on being home owner	gen real_house_value = house_value/CPI*100	CPI	State-level CPI, see state-level data description
	gen real_$_o$$wner_house_value$ = own_or_rent $*$ $log(real\_house\_value)$	House value	[68]V5 [69]V449 [70]V1122 [71]V1823 [72]V2423 [73]V3021 [74]V3417 [75]V3817 [76]V4318 [77]V5217 [78]V5717 [79]V6319 [80]V6917 [81]V7517 [82]V8217 [83]V8817 [84]V10018 [85]V11125 [86]V12524 [87]V13724 [88]V14824 [89]V16324 [90]V17724 [91]V19024 [92]V20324 [93]V21610 [94]ER2033 [95]ER5032 [96]ER7032 [97]ER10036 [99]ER13041 [01]ER17044 [03]ER21043 [05]ER25029 [07]ER36029 [09]ER42030 [11]ER47330 [13]ER53030 [15]ER60031 [17]ER66031
Debt to income	gen mortgage_debt = rem_principal_mtg1	Remaining principal, mortgage 1	[69]V451 [70]V1124 [71]V1825 [72]V2425 [76]V4320 [77]V5219 [78]V5719 [79]V6321 [80]V6919 [81]V7519 [83]V8819 [84]V10020 [85]V11127 [86]V12526 [87]V13726 [88]V14826 [89]V16326 [90]V17726 [91]V19026 [92]V20326 [93]V21612 [94]ER2037 [95]ER5036 [96]ER7042 [97]ER10044 [99]ER13047 [01]ER17052 [03]ER21051 [05]ER25042 [07]ER36042 [09]ER42043 [11]ER47348 [13]ER53048 [15]ER60049 [17]ER66051

Table 1. Data description *(continuing)*

Variable name	Formula	PSID variables	PSID codes [year]code
	replace mortgage_debt = rem_principal_mtg1 + rem_principal_mtg2 if rem_principal_mtg1≠. and rem_principal_mtg2≠.	Remaining principal, mortgage 2	[94]ER2038 [95]ER5037 [96]ER7043 [97]ER10045 [99]ER13056 [01]ER17063 [03]ER21062 [05]ER25053 [07]ER36054[09]ER42062 [11]ER47369 [13]ER53069 [15]ER60070 [17]ER66072
	gen mortgage_to_income = mortgage_debt/total_income if total_income>0	Total income	[68]V81 [69]V529 [70]V1514 [71]V2226 [72]V2852 [73]V3256 [74]V3676 [75]V4154 [76]V5029 [77]V5626 [78]V6173 [79]V6766 [80]V7412 [81]V8065 [82]V8689 [83]V9375 [84]V11022 [85]V12371 [86]V13623 [87]V14670 [88]V16144 [89]V17533 [90]V18875 [91]V20175 [92]V21481 [93]V23322 [94]ER4153 [95]ER6993 [96]ER9244 [97]ER12079 [99]ER16462 [01]ER20456 [03]ER24099 [05]ER28037 [07]ER41027 [09]ER46935 [11]ER52343 [13]ER58152 [15]ER65349 [17]ER71426
Industry classification of main job	tabulate ind14, generate(ind14_)		
Age	replace age_fam = age_ind if age_fam==.	Age of head, family file	[68]V117 [69]V1008 [70]V1239 [71]V1942 [72]V2542 [73]V3095 [74]V3508 [75]V3921 [76]V4436 [77]V5350 [78]V5850 [79]V6462 [80]V7067 [81]V7658 [82]V8352 [83]V8961 [84]V10419 [85]V11606 [86]V13011 [87]V14114 [88]V15130 [89]V16631 [90]V18049 [91]V19349 [92]V20651 [93]V22406 [94]ER2007 [95]ER5006 [96]ER7006 [97]ER10009 [99]ER13010 [01]ER17013 [03]ER21017 [05]ER25017 [07]ER36017 [09]ER42017 [11]ER47317 [13]ER53017 [15]ER60017 [17]ER66017
		Age of individual, individual file	[68]ER30004 [69]ER30023 [70]ER30046 [71]ER30070 [72]ER30094 [73]ER30120 [74]ER30141 [75]ER30163 [76]ER30191 [77]ER30220 [78]ER30249 [79]ER30286 [80]ER30316 [81]ER30346 [82]ER30376 [83]ER30402 [84]ER30432 [85]ER30466 [86]ER30501 [87]ER30538 [88]ER30573 [89]ER30609 [90]ER30645 [91]ER30692 [92]ER30736 [93]ER30809 [94]ER33104 [95]ER33204 [96]ER33304 [97]ER33404 [99]ER33504 [01]ER33604 [03]ER33704 [05]ER33804 [07]ER33904 [09]ER34004 [11]ER34104 [13]ER34204 [15]ER34305 [17]ER34504

Table 1. Data description *(ending)*

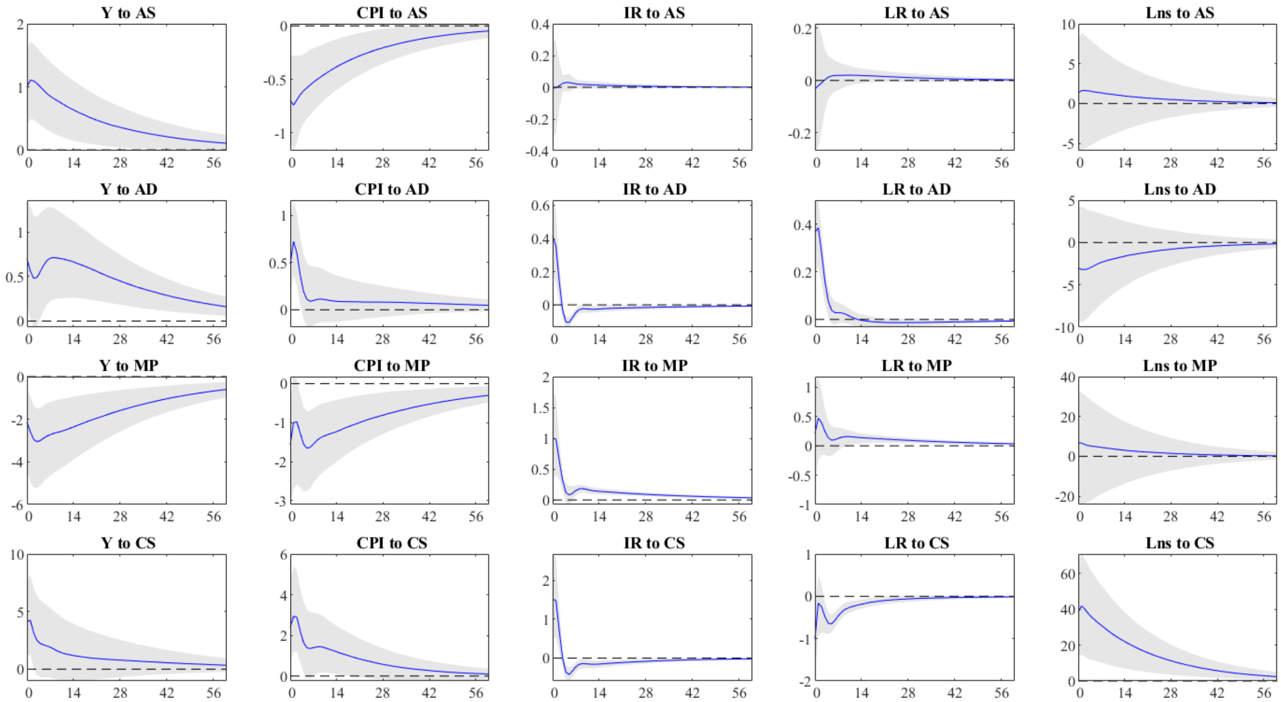
Variable name	Formula	PSID variables	PSID codes [year]code
	gen aged1 = age_corr<30.		
	gen aged2 = age_corr≥30 and age_corr<45		
	gen aged3 = age_corr≥45 and age_corr<60		
	gen aged4 = age_corr≥60		
Sex	gen male = gender==1	Gender	[68]ER32000
Family status	gen married = (marital==1)	Marital status	[68]V239 [69]V607 [70]V1365 [71]V2072 [72]V2670 [73]V3181 [74]V3598 [75]V4053 [76]V4603 [77]V5650 [78]V6197 [79]V6790 [80]V7435 [81]V8087 [82]V8711 [83]V9419 [84]V11065 [85]V12426 [86]V13665 [87]V14712 [88]V16187 [89]V17565 [90]V18916 [91]V20216 [92]V21522 [93]V23336 [94]ER4159A [95]ER6999A [96]ER9250A [97]ER12223A [99]ER16423 [01]ER20369 [03]ER24150 [05]ER28049 [07]ER41039 [09]ER46983 [11]ER52407 [13]ER58225 [15]ER65461 [17]ER71540



Note: This graph presents mortgage delinquency rates according to different data sources. FRED is the actual St. Louis FED data on delinquency rate on single-family residential mortgages, booked in domestic offices, all commercial banks, indicator's code: DRSFRMACBS. PSID stands for the frequency of positive responses to the question of whether a household is behind on mortgage payments in the PSID database. The CEX denotes our estimate of mortgage delinquency rate in the CEX database according to the information on either zero principal payments or stable mortgage balance in any month of a year.

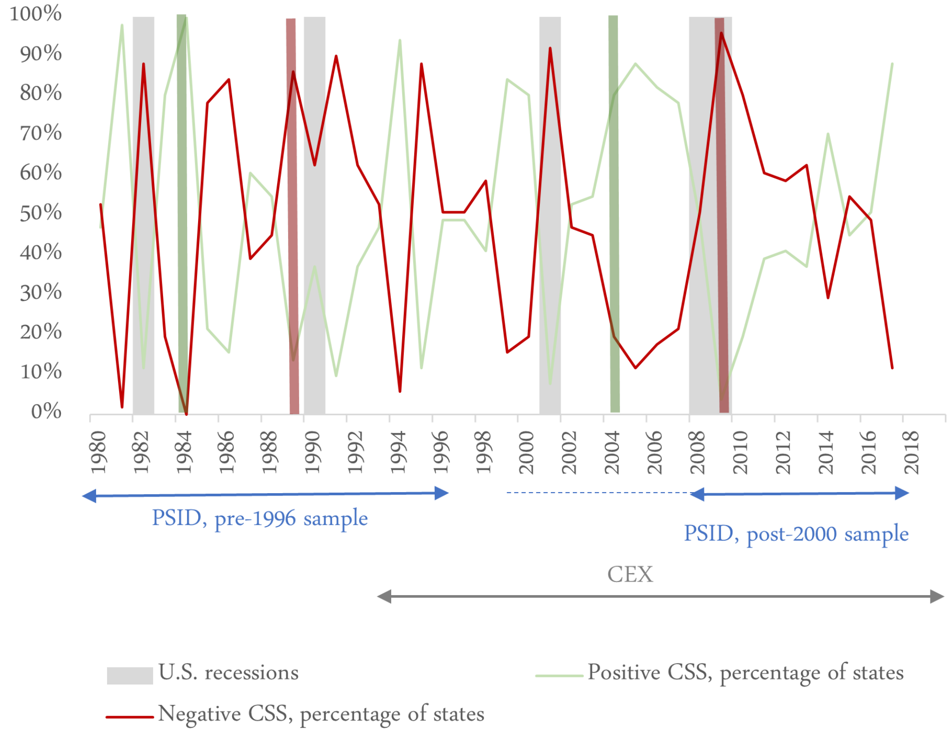
Figure 1. Mortgage delinquency rate, %

Appendix no. 16: Credit supply shocks at the US-state level: additional results



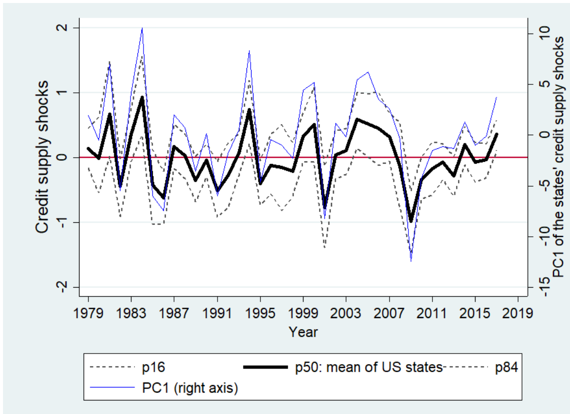
Note: Each row represents responses of the five variables to one shock identified with sign restrictions (see Table 1): AS is aggregate supply, AD is aggregate demand, MP is monetary policy, CS is credit supply. Y is the logarithm of the real GDP index, CPI is the logarithm of the consumer price index, IR is the short-term interest rate, LR is the lending rate, and $Loans$ is the logarithm of nominal loans issued by commercial banks in a particular state. All variables in logarithms are additionally multiplied by 100, i.e. their impulse responses are in percentages. The SVAR model is estimated on the panel data on 51 US states over 1977–2017 using the sign restrictions of Gambetti and Musso (2017) with Minnesota priors on VAR coefficients.

Figure 1. Impulse response functions on identified macroeconomic shocks, the panel of all states

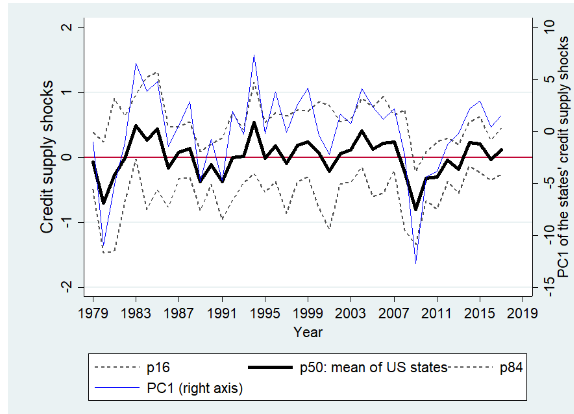


Note: This graph explains our choice of the years of "systemic" credit supply shocks (CS) – i.e. positive or negative shocks that hit most of the states in particular years (1984, 1989, 2004, 2009) – in our difference-in-differences analysis. Green bars denote the years of "systemic" positive CS, and red bars denote the years of "systemic" negative CS. We focus on the 1980s and 2000s in this analysis because first, there is no complete credit cycle in the 1990s (see Figure 13), and second, we do not have continuous micro-level data for the 1990s. We choose 1984 instead of 1981 as the year of "systemic" positive CSS in the 1980s because 1982 is a recession year, and we want to focus on positive credit supply shocks corresponding to the expansionary phase of both credit and business cycles. We choose 2004 instead of 2005 as the year of "systemic" positive CSS in the 2000s because this is the first year of prevalent positive CSS in the 2000s.

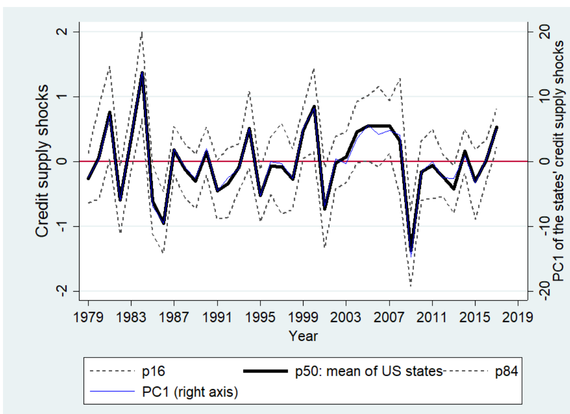
Figure 2. Share of states with positive and negative credit supply shocks, and the dates of U.S. recessions



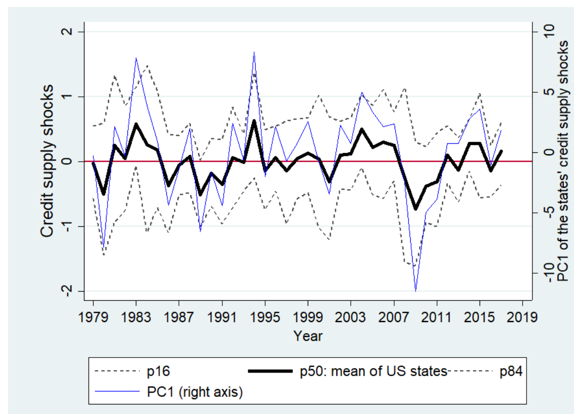
(a) (*baseline*) GM2017, Minnesota priors



(b) EN2015, Minnesota priors



(c) GM2017, flat priors

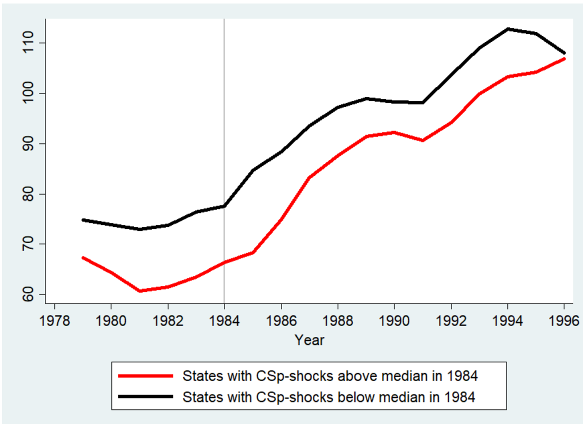


(d) EN2015, flat priors

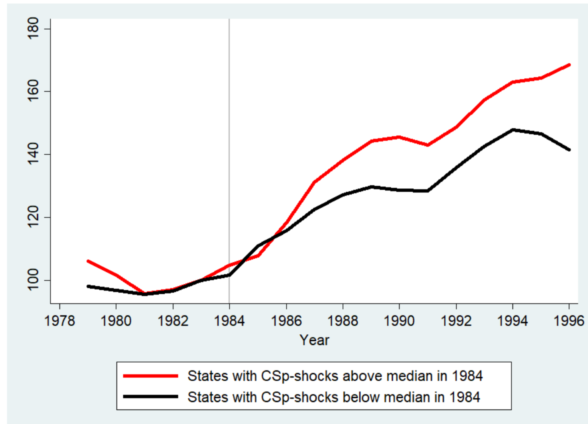
Note: GM2017 denotes the approach of Gambetti and Musso (2017), EN2015 stands for Eickmeier and Ng (2015). See the main text for details (Section 2.2.1).

Figure 3. Alternative identifications of credit supply shocks

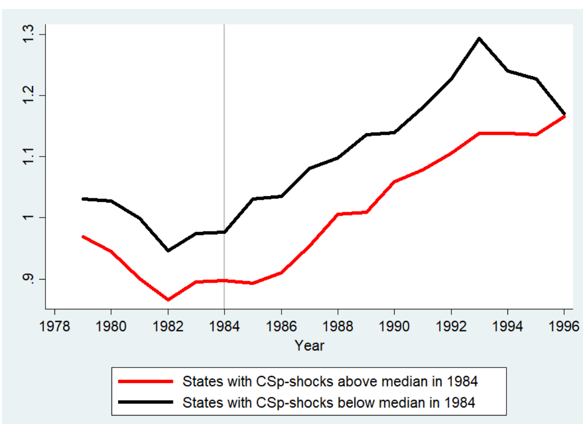
Appendix no. 17: Data analysis at the US-state level: household outcomes in the treated and control groups of states



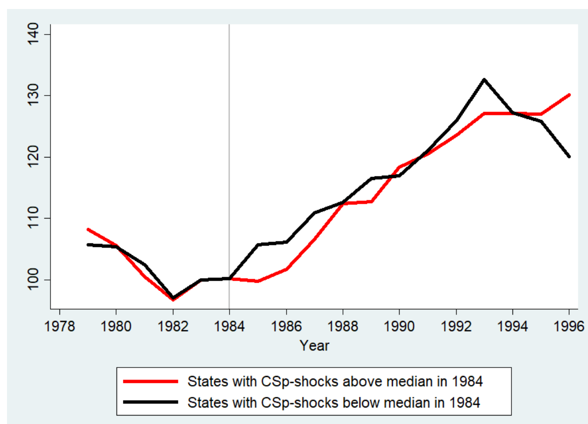
(a) Real mortgage



(b) Real mortgage, 1983=100



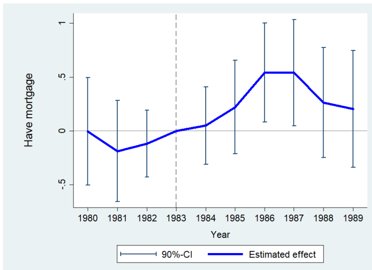
(c) Mortgage-to-income



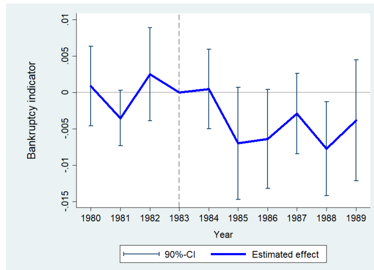
(d) Mortgage-to-income, 1983=100

Figure 1. Time evolution of selected outcome variables in the states with stronger (treated) and less strong (control) CS shocks

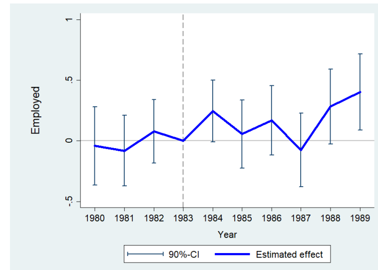
Appendix no. 18: Validation of the 1984 CS shock with credit market reforms



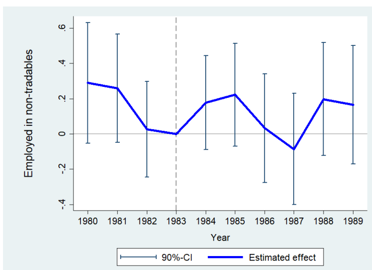
(a) Whether have mortgage



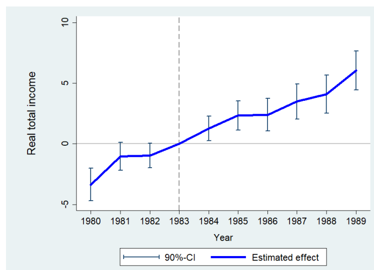
(b) Whether file bankruptcy



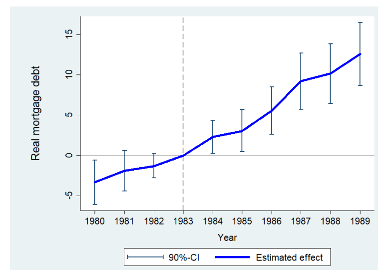
(c) Whether employed



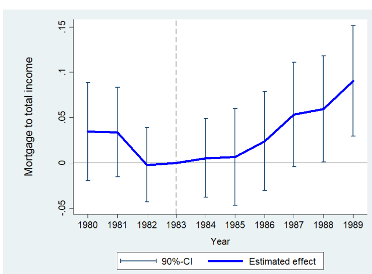
(d) Employed in non-tradables



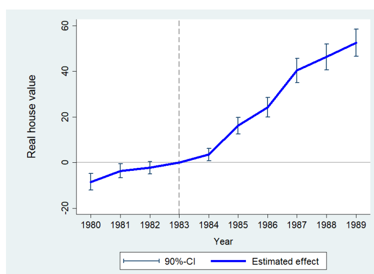
(e) Real total income



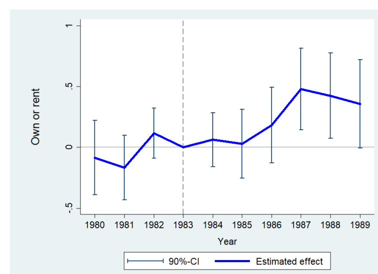
(f) Real mortgage debt



(g) Mortgage to income



(h) Real house value

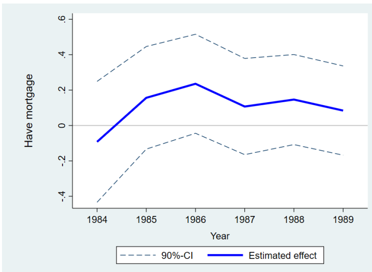


(i) Whether own or rent

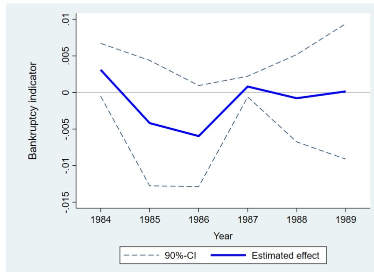
Note: The figure reports the results from estimating equation (9) for a set of nine outcomes measured at the household level in the 1980s and the Mian et al. (2020) early vs. late deregulated states. The pre-shock year is 1983, and we normalize the effect in this year to be equal to zero so that all the coefficients in the years prior or after reflect changes with respect to the pre-shock year.

Figure 1. The effects of the positive CS shock of 1984 on household outcomes: cross-validation

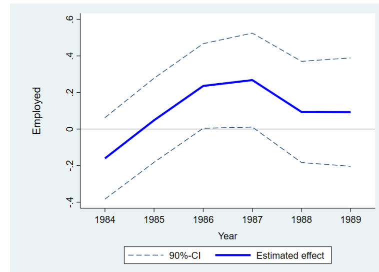
Appendix no. 19: Jorda's local projection estimation results



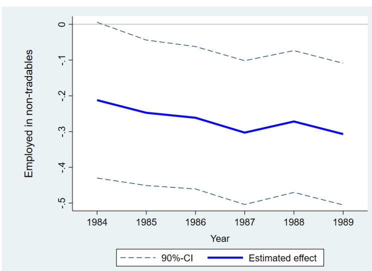
(a) Whether have mortgage



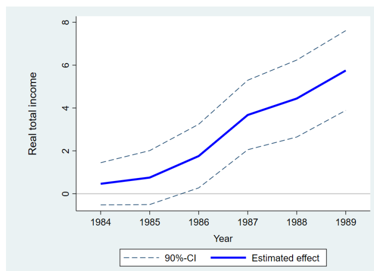
(b) Whether file bankruptcy



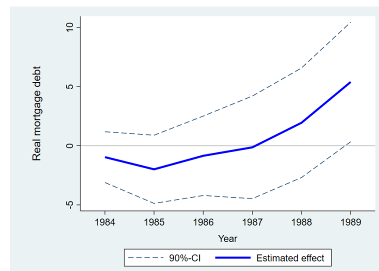
(c) Whether employed



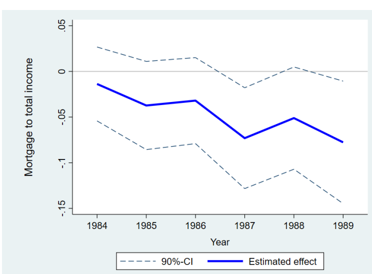
(d) Employed in non-tradables



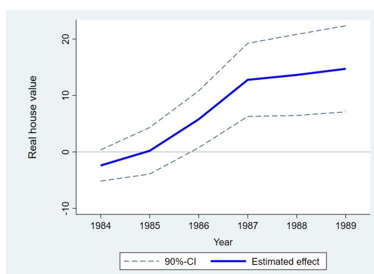
(e) Real total income



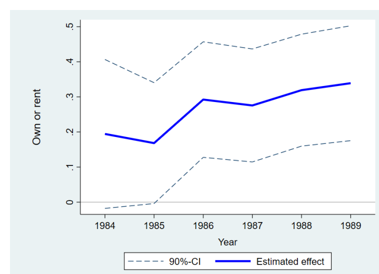
(f) Real mortgage debt



(g) Mortgage to income



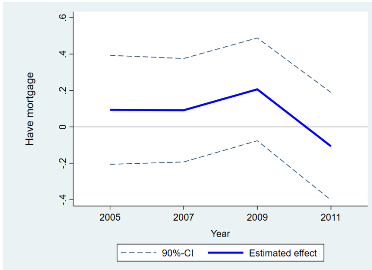
(h) Real house value



(i) Whether own or rent

Note: The figure reports the results from estimating equation (12) for a set of nine outcomes measured at the household level in the 1980s and our SVAR-based measure of CS shocks. The pre-shock year is 1983, and we normalize the effect in this year to be equal to zero so that all the coefficients in the years prior or after reflect changes with respect to the pre-shock year.

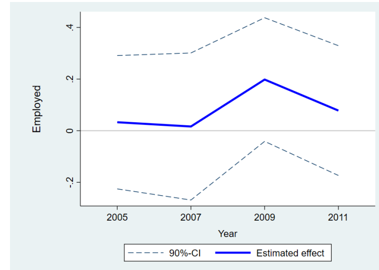
Figure 1. The effects of the positive CS shock of 1984 on household outcomes: Jorda's local projection estimation results



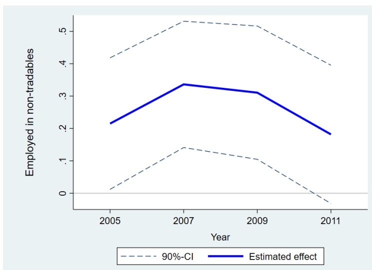
(a) Whether have mortgage

N / A

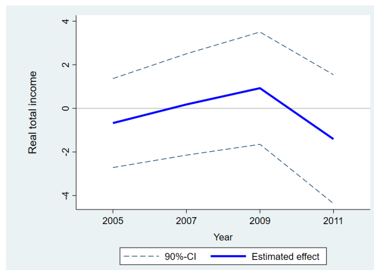
(b) Whether mortgage delinq.



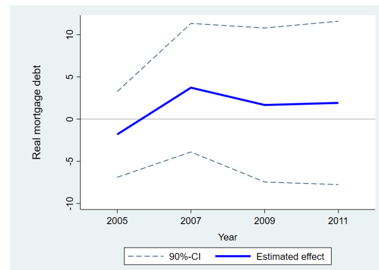
(c) Whether employed



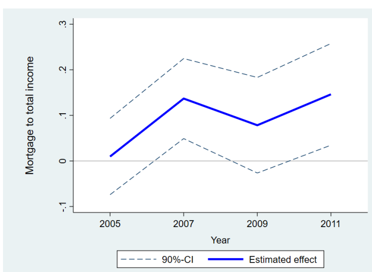
(d) Employed in non-tradables



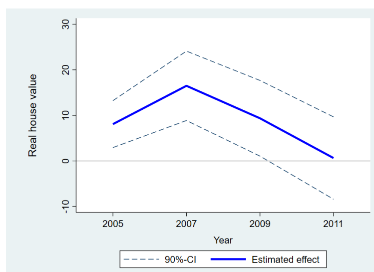
(e) Real total income



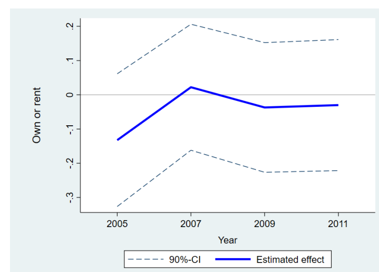
(f) Real mortgage debt



(g) Mortgage to income



(h) Real house value

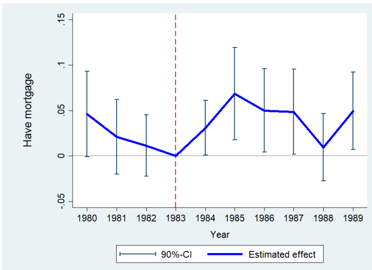


(i) Whether own or rent

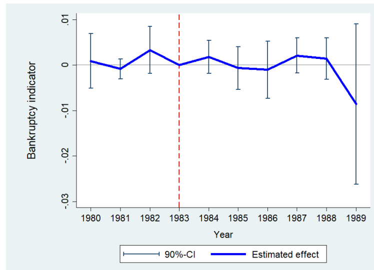
Note: The figure reports the results from estimating equation (12) for a set of nine outcomes measured at the household level in the 2000s and our SVAR-based measure of CS shocks. The pre-shock year is 2003, and we normalize the effect in this year to be equal to zero so that all the coefficients in the years prior or after reflect changes with respect to the pre-shock year.

Figure 2. The effects of the positive CS shock of 2004 on household outcomes: Jorda's local projection estimation results

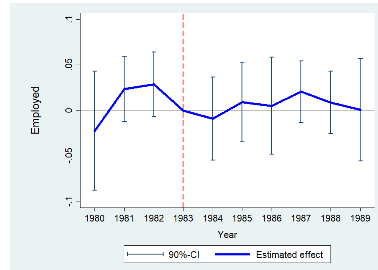
Appendix no. 20: Difference-in-differences: estimation results at the US-state level



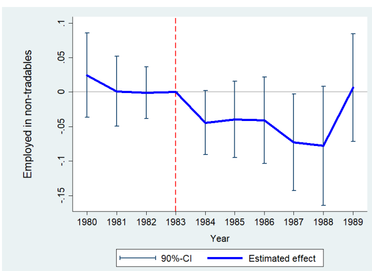
(a) Whether have mortgage



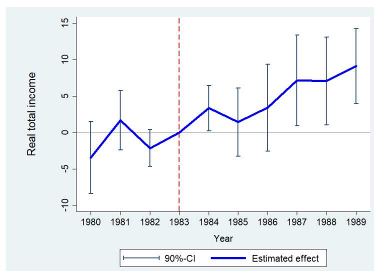
(b) Whether file bankruptcy



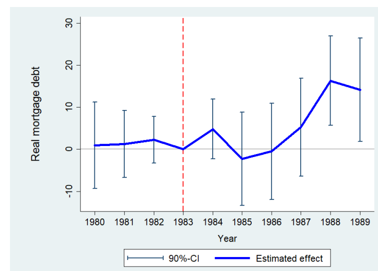
(c) Whether employed



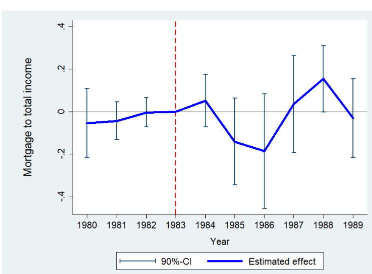
(d) Employed in non-tradables



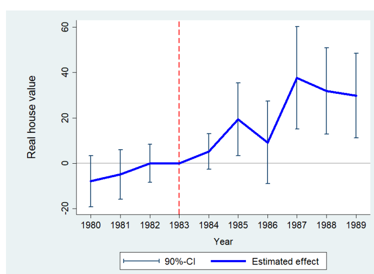
(e) Real total income



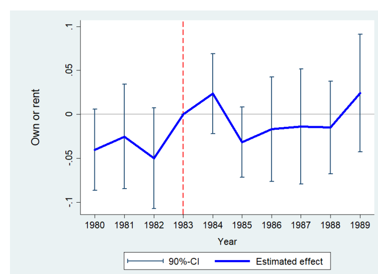
(f) Real mortgage debt



(g) Mortgage to income



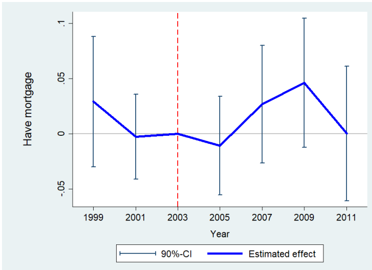
(h) Real house value



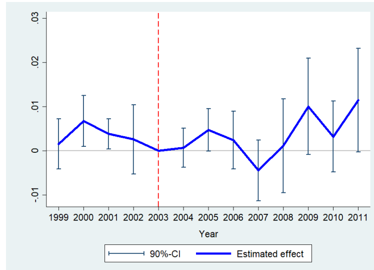
(i) Whether own or rent

Note: The figure reports the results from estimating equation (9) for a set of nine outcomes measured at the state level in the 1980s and our SVAR-based measure of CS shocks. The pre-shock year is 1983, and we normalize the effect in this year to be equal to zero so that all the coefficients in the years prior or after reflect changes with respect to the pre-shock year.

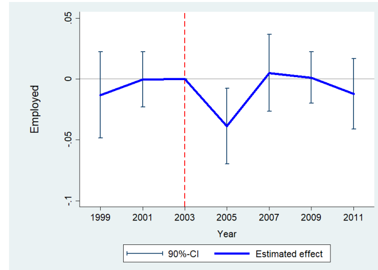
Figure 1. The effects of the positive CS shock of 1984 on state-level outcomes



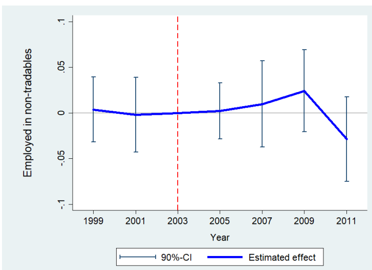
(a) Whether have mortgage



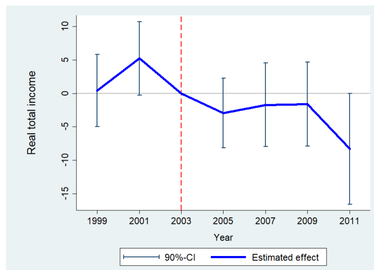
(b) Whether mortgage delinq. (CEX)



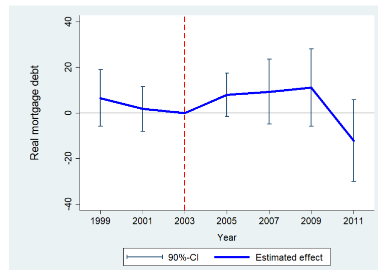
(c) Whether employed



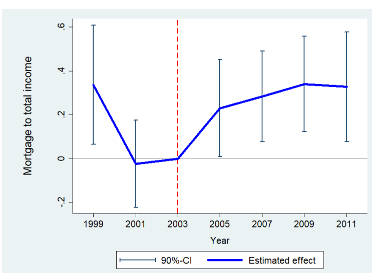
(d) Employed in non-tradables



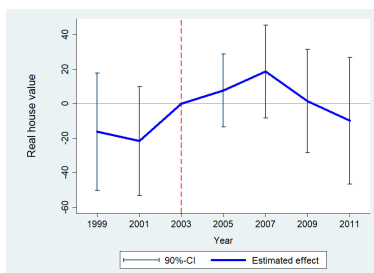
(e) Real total income



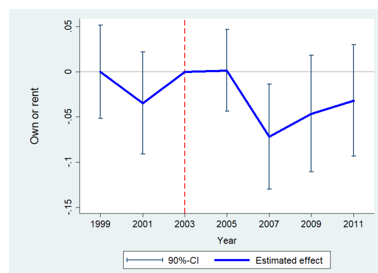
(f) Real mortgage debt



(g) Mortgage to income



(h) Real house value

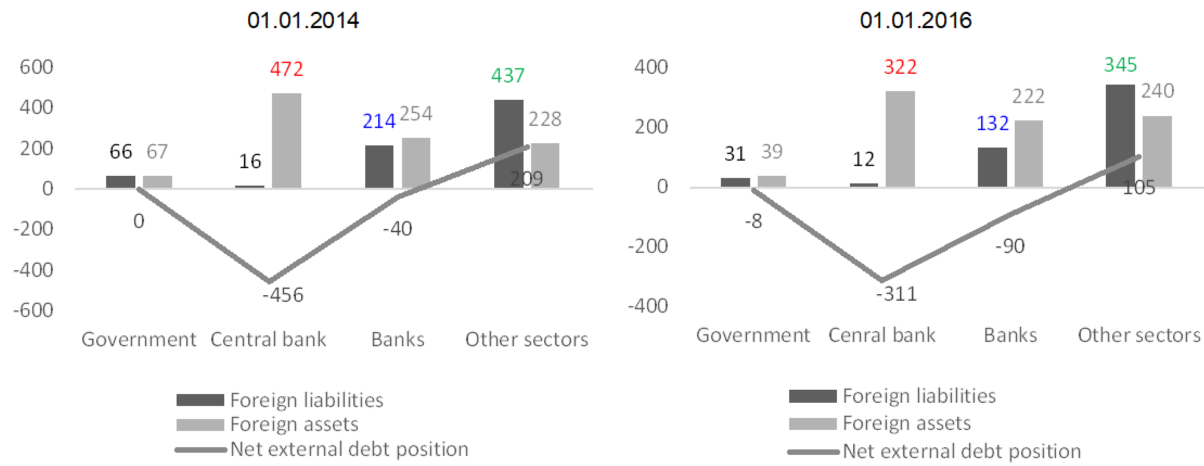


(i) Whether own or rent

Note: The figure reports the results from estimating equation (9) for a set of nine outcomes measured at the state level in the 2000s and our SVAR-based measure of CS shocks. The pre-shock year is 2003, and we normalize the effect in this year to be equal to zero so that all the coefficients in the years prior or after reflect changes with respect to the pre-shock year.

Figure 2. The effects of the positive CS shock of 2004 on state-level outcomes

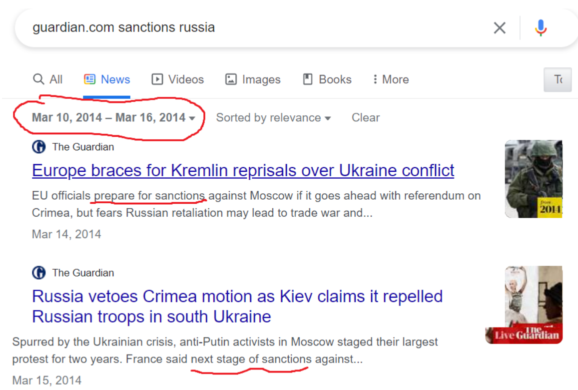
Appendix no. 21: Net foreign debt positions



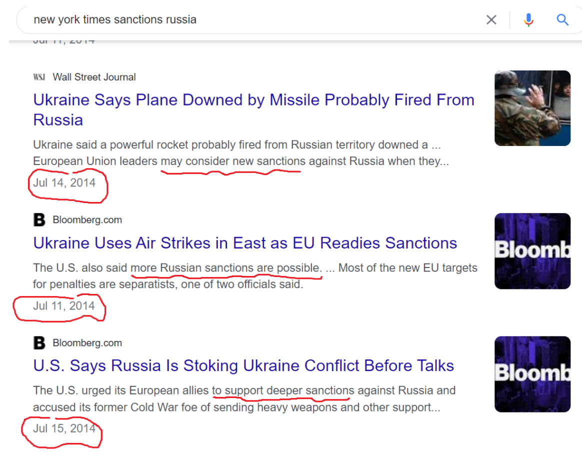
Source: The Central Bank of Russia

Figure 1. Net foreign debt position of different sectors of the Russian economy before and after the 2014 sanctions

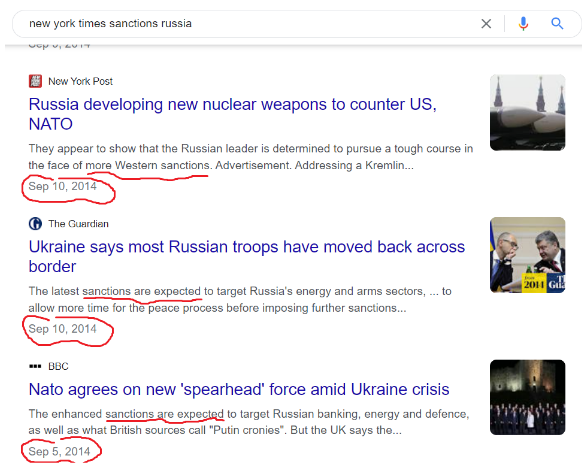
Appendix no. 22: News on upcoming sanctions



(a) Before 20 March 2014



(b) Before 16 July 2014

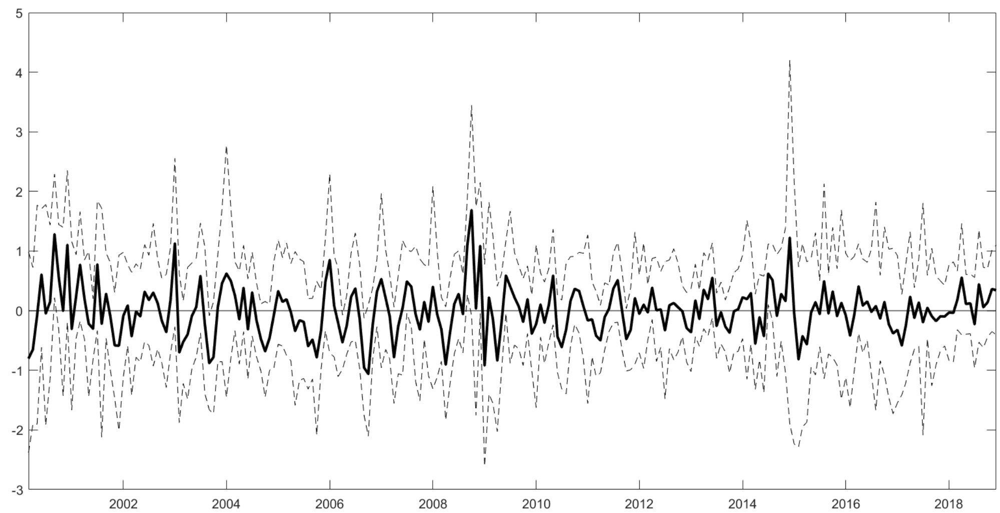


(c) Before 12 September 2014

Note: The figure reports news search results of the following form "[*Name of the media* Russia sanctions] in a five-day time interval before the sanction announcements by OFAC on 20 March 2014 (*a*, 16 July 2014 (*b*), and 12 September 2014 (*c*).

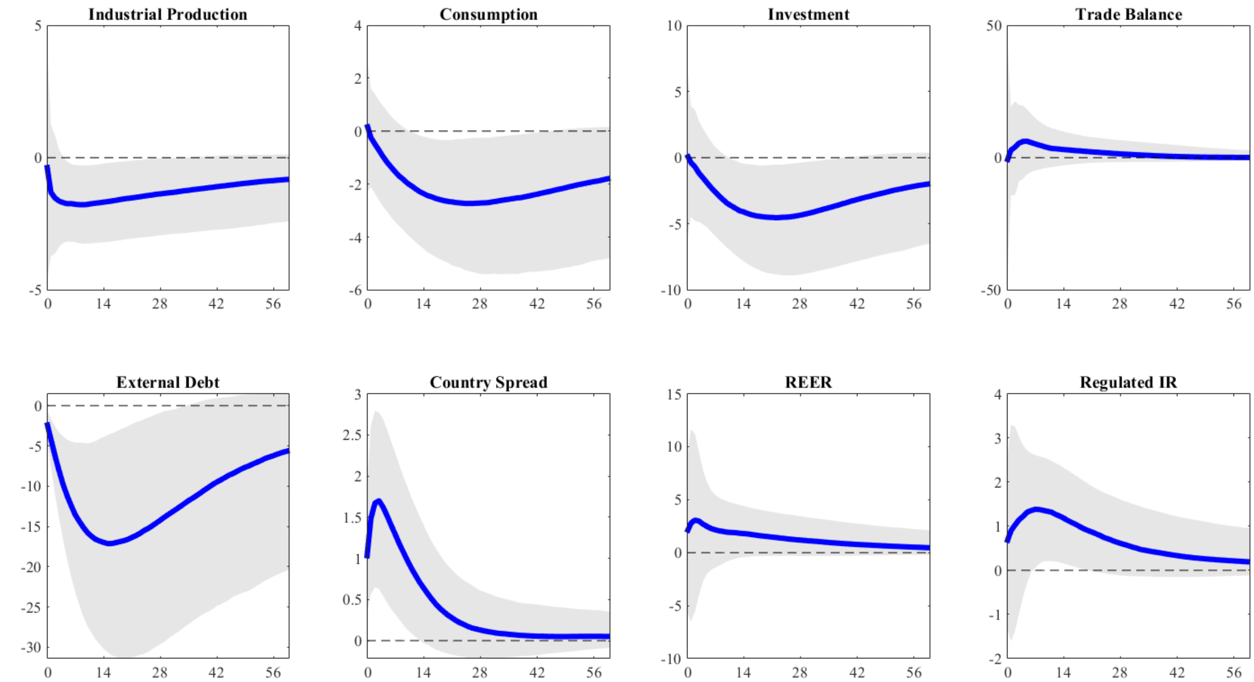
Figure 1. Anticipation of sanctions (informational leakage) on the eve of sanction announcements

Appendix no. 23: International credit supply shock in a VAR model with monetary policy



Note: The figure reports the time evolution of the estimated negative shock to the international credit supply (ICS) shock isolated with the use of 11-variable VAR model. Positive values of the shock variable reflect unexpected declines of ICS, and vice versa.

Figure 1. Time evolution of the international credit supply shock identified under the sign restrictions



Note: The figure reports the estimated IRFs of domestic macroeconomic variables to the sanction shock identified using the sign restrictions scheme as an international credit supply shock. The IRFs are re-scaled so that the shock is equivalent to a +1 percentage point rise of $Country.Spread_t$. The BVAR model contains 11 variables: external characteristics—commodity terms-of-trade ($CTOT_t$), the Baa corporate bond spread ($Baa.Spread_t$), the real interest rate in the U.S. economy ($US.Real.Interest.Rate_t$); domestic indicators—industrial production (IP_t), private consumption ($Consum_t$), investments ($Invest_t$), trade balance (TB_t), corporate external debt ($ExtDebt_t$), Russia's country spread ($Country.Spread_t$), real effective exchange rate ($REER_t$), and the regulated interest rate (real, $Regulated.IR_t$). The last variable captures the monetary policy reaction to the sanctions shock. The $Country.Spread_t$ variable is ordered third last. The Bayesian estimates are obtained with the flat (uninformative) prior. We set 10,000 draws from the posterior distribution and we discard the first 5,000 draws. Conventional credible bands comprised of the 16^{th} and 84^{th} percentiles of the post-burned-in estimated IRFs are reported (grey shaded area).

Figure 2. Impulse response functions to the international credit supply shock identified under the sign restriction scheme that accounts for endogenous monetary policy reactions

Appendix no. 24: Back-up for the sign restrictions: Recursive identification

We now discuss an alternative approach: instead of capturing a negative ICS shock using the sign restriction scheme (17) we isolate a positive shock to country spread—an unexpected rise in the country risk premium—by applying a more conventional recursive identification. First, the sign restriction scheme applied in the main text lacks identification of other important shocks (CTOT, monetary policy, etc.), which could also affect the economy during the sanctions shock (as it was in 2014). Second, there is ample literature arguing that country spread shocks account for a non-negligible part of business cycle fluctuations in EMEs (Uribe and Yue, 2006; Garcia-Cicco et al., 2010; **?**). Moreover, there is an established procedure for the identification of these shocks, which we follow to ensure comparability with the literature and support our baseline results from the previous section.[105]

To isolate a country spread shock using the recursive identification, the literature typically assumes that the *country spread* variable reacts to the shocks to other variables immediately, whereas a *shock* to the country spread affects other (domestic) variables only with a time lag. Put differently, the country spread variable is usually ordered last (Uribe and Yue, 2006; Akinci, 2013; Born et al., 2020; Monacelli et al., 2023).

Recall, however, that we study a larger VAR model than in prior studies: we include REER in the set of domestic variables as one of the channels through which the sanctions transmit to the economy. Monacelli et al. (2023) mention that there is a potential problem if the country interest rate (or spread) is ordered *after* REER: this would imply that REER does *not* react to innovations in domestic interest rate, which is clearly dubious. Therefore, in our recursive identification, we place REER *last*, the domestic regulated interest rate *second last*, and the country spread *third last*.[106] The matrix B_0^{-1} is thus assumed to have the following structure, being lower triangular with unit elements on the main diagonal:

[105] In addition, credible bands for the estimated impulse responses are likely to be more narrow under the recursive scheme (RS) as compared to the sign restrictions (SR). This is because RS uses just one structural model to identify shocks and impulse responses to them; conversely, SR effectively uses multiple models to produce generalized impulse responses.

[106] In the sensitivity analysis, we also consider the five-variable VAR of Uribe and Yue (2006) which does not contain REER or a foreign block, and in which we order country interest rate last.

$$
\begin{pmatrix} \vdots \\ u_t^{IP} \\ u_t^{C} \\ u_t^{I} \\ u_t^{TB} \\ u_t^{D} \\ u_t^{S} \\ u_t^{RIR} \\ u_t^{REER} \end{pmatrix} = \begin{pmatrix} \ddots & & & & & & & & \\ \dots & 1 & & & & & & & \\ \dots & . & 1 & & & & & & \\ \dots & . & . & 1 & & & & & \\ \dots & . & . & . & 1 & & & & \\ \dots & . & . & . & . & 1 & & & \\ \dots & . & . & . & . & . & 1 & & \\ \dots & . & . & . & . & . & . & 1 & \\ \dots & . & . & . & . & . & . & . & 1 \end{pmatrix} \begin{pmatrix} \vdots \\ \varepsilon_t^{IP} \\ \varepsilon_t^{C} \\ \varepsilon_t^{I} \\ \varepsilon_t^{TB} \\ \varepsilon_t^{D} \\ \varepsilon_t^{S} \\ \varepsilon_t^{RIR} \\ \varepsilon_t^{REER} \end{pmatrix} \tag{40}
$$

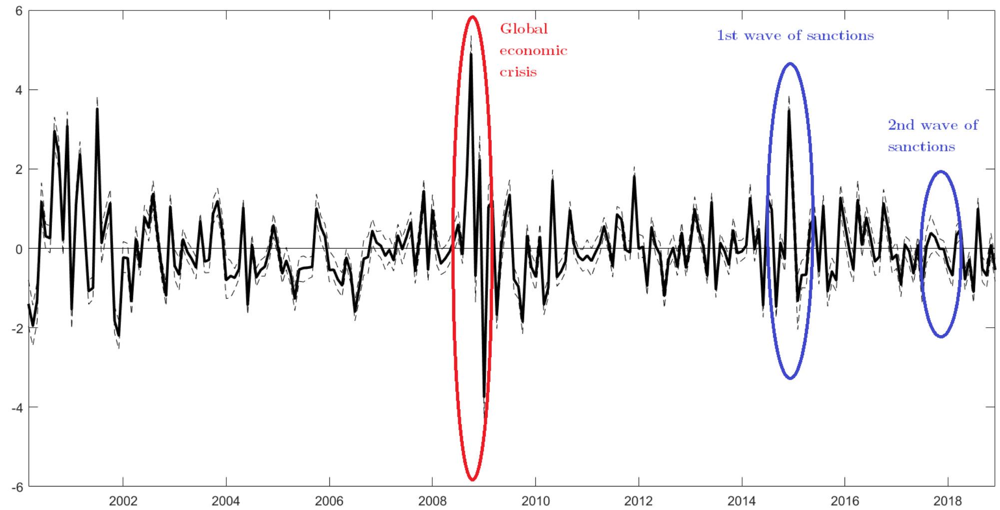
where "$\vdots$" and "..." correspond to the three exogenous variables, "." implies a non-empty element, and the empty cells, by the construction of the lower triangular, contain zeros.

Differently from the sign restrictions scheme (17), we now have a shock to the country spread ε_t^S instead of a shock to ICS $\varepsilon_t^{Credit\,Supply}$. We also now obtain shocks to CTOT ε_t^{CTOT} and the domestic regulated interest rate ε_t^{RIR}. The latter allows us to be sure that we do not confuse the sanctions shock of 2014 with a slump in the world oil price and the dramatic rise of the domestic regulated interest rate that both occurred during the same time. However, the drawback of (40) is that it does not distinguish demand and supply-driven forces in the dynamics of external debt.

Let us now turn to the estimation results that we obtain under the recursive identification (40). As can be inferred from Fig. 1, the time evolution of the estimated country spread shock ε_t^S is remarkable. First, we observe a sharp spike at the end of 2014 which can clearly be attributed to the first wave of the financial sanctions. The size of the shock equals +4 p.p. which, according to our estimates, is the second strongest shock over the last two decades after the shock associated with the global economic crisis of 2007–2009 (+5 p.p., at the beginning of 2009). Further, when we turn to the second wave of sanctions, we recognize a credible positive shock in the second half of 2017, but its size is at least three times lower than during the first wave. This again implies that the second wave could have much less harmful macroeconomic effects. We also note that ε_t^S shocks of a size comparable to that of the second wave of sanctions occur very often, according to our estimates. Overall, we obtain qualitatively the same result for the dynamics of the country spread shock as the one under the sign restriction scheme presented in the main text (see Fig. 23).

The estimated impulse responses to the identified country spread shock appear in Fig. 2.[107] As before, all impulses were re-scaled to a +1 percentage point shock to the country spread. First, we obtain a negative and significant reaction of the volume of external debt, with the

[107]Estimated responses from a recursive identification scheme that ignores monetary policy imply the same results and are reported in Appendix **??**.



Note: The figure reports the time evolution of the sanctions shock estimated with the BVAR model containing 11 variables. The Bayesian estimates are obtained with the flat (uninformative) prior. We set 10,000 draws from the posterior distribution and we discard the first 5,000 draws. Conventional credible bands comprised of the 16^{th} and 84^{th} percentiles of the post-burned-in estimated IRFs are reported. Substantial spikes in the time series of the estimated shock are identified for the first but not for the second wave of sanctions at the end of 2014 and 2017, respectively. One more is identified for the period of 2008–2009 global economic crisis and is reported for comparative reasons.

Figure 1. Time evolution of a positive shock to the country spread identified under the recursive identification scheme

peak response being equal –5 p.p. On one hand, it implies that supply-driven forces dominate over demand for external debt. On the other hand, this peak reaction is at least three times lower in magnitude than the analogous estimate under the sign restriction scheme. This indirectly implies that demand also plays a large role in determining the inflows of external debt to Russia.

Second, the real economy also reacts negatively to the country spread shock: industrial production declines by almost 1 percentage point, private consumption by slightly more than 1 percentage point, and investment by 1.4 p.p. Strikingly, these estimates are two to three times lower in magnitude as compared to their analogs obtained under the sign restrictions in the previous section. However, the credible bands indeed become much more narrow than before. What is also remarkable is that the estimated responses are now much less persistent than before. Overall, the results obtained with the recursive identification are qualitatively the same as those achieved with the sign restrictions, thus supporting our baseline findings.

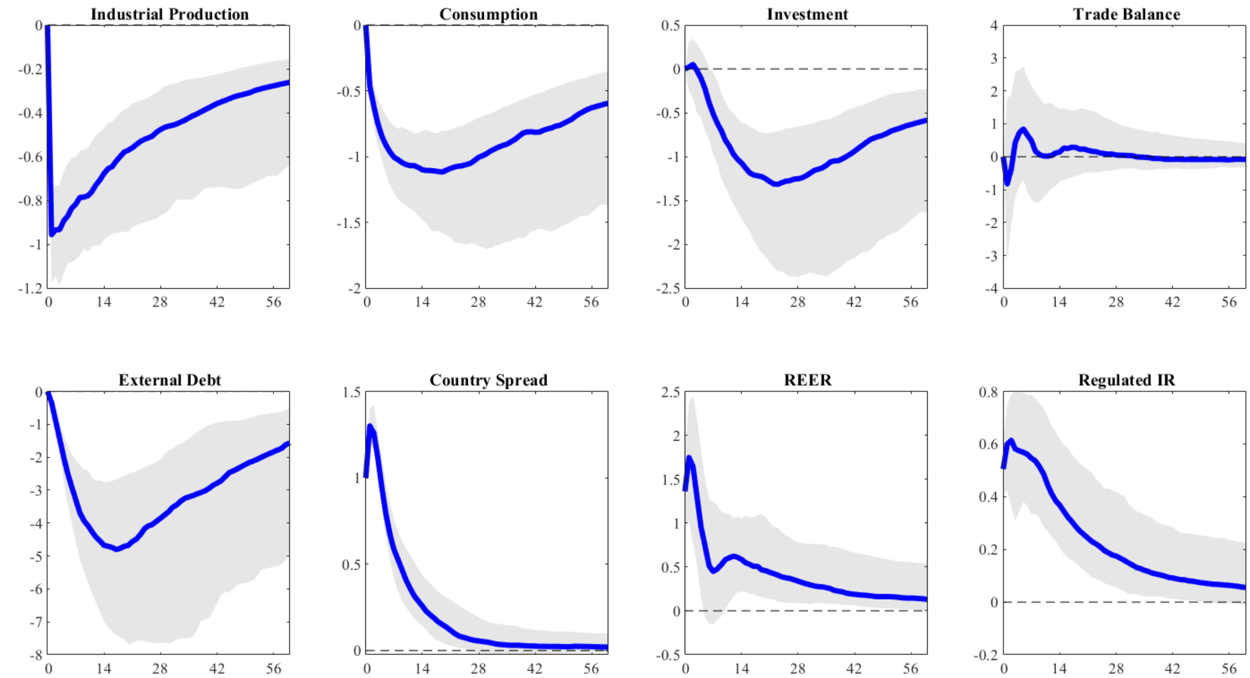
Third, trade balance tends to respond positively to the positive country spread shock, however, the response is again insignificant, as it was before. REER also reacts positively and, in turn, significantly, with a response peaking at +1.8 p.p. This is again lower than in the baseline, by a factor of two. As before, the depreciation of the Ruble in response to the positive country spread shock is justified by the necessity to repay external debts (i.e., the magnitude of the external debt's decline), which, in turn, becomes possible via (marginal) improvement of the trade balance.

Finally, the domestic regulated interest rate also reacts positively to the country's spread shock, thus accommodating the increased price of foreign borrowings. The estimated peak reaction equals +0.6 p.p., which is less than one and is lower than in the baseline by a factor of three.

Therefore, the results obtained under the recursive identification fully support our baseline results, although the estimated impulse responses differ quantitatively. Recall, however, that the estimated size of the ICS shock in 2014, i.e., during the first wave of the financial sanctions, is lower by a factor of three as compared to the size of the country's spread shock during the same time. This means that the resulting estimates of the effects of sanctions are expected to be comparable across the two SVAR-based identification schemes (see Section 3.4.3 in the main text).

One more issue which we elaborate on in this section is how important for the economy are the shocks to the country spread in comparison with the shocks to CTOT and domestic monetary policy (MP), as identified through the recursive scheme. Impulse responses of the domestic macroeconomic variables to the (positive) CTOT and (restrictive) MP shocks are reported in Appendices **??** and **??**, respectively. Recall that the country spread shock was set at +1 percentage point when we were computing the effects of this shock above. If we now re-scale both responses to the CTOT and MP shocks accordingly so that they are equivalent to +1 percentage point rises of the country spread, then we obtain the following result. Industrial production reacts negatively and significantly to both (negative) CTOT and (restrictive) MP shocks, with the peaks reaching –2 p.p. $(+0.4 \times (-5))$ and –2.4 p.p. (-0.47×5), respectively.[108] This result means that oil price drops, as captured by negative CTOT shocks, and rises in domestic interest rate, as captured by restrictive MP shocks, both force the Russian economy to decline deeper than the shocks to the country spread. This argument exhibits its importance in Section 3.4.3 of the main text where we compute the final effects of the financial sanctions.

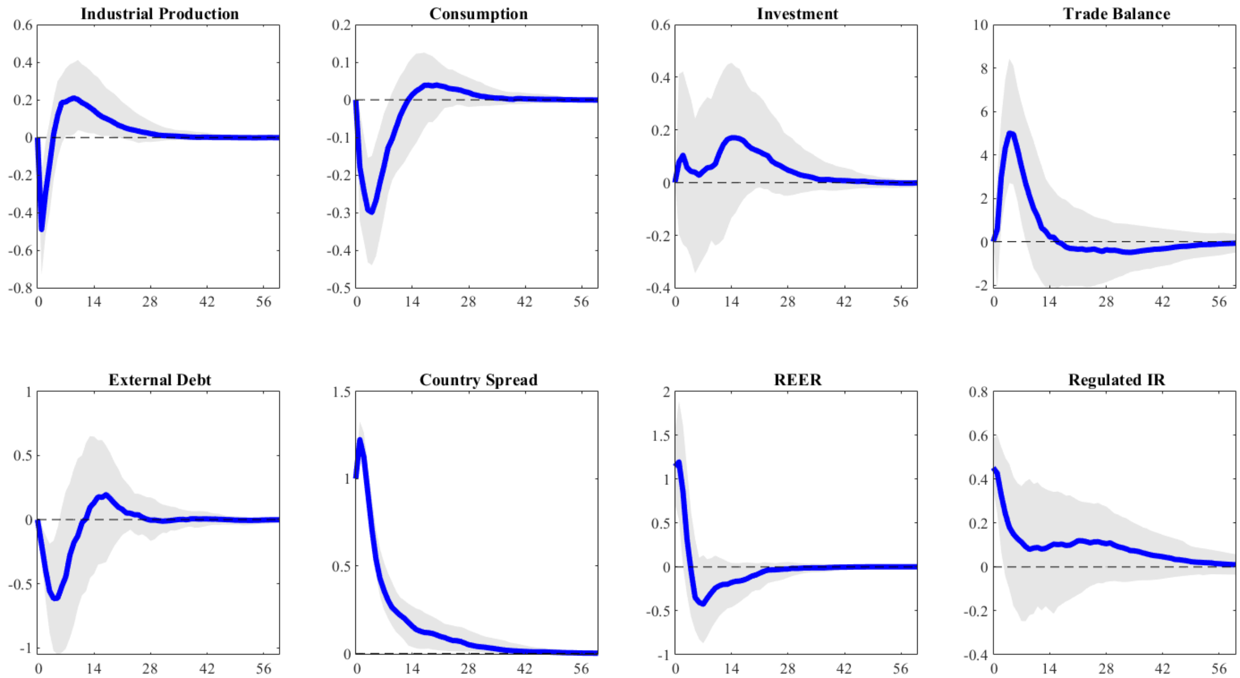
[108] In here, -5 and 5 are the re-scaling factors that force the country spread to reach a +1 percentage point rise in response to CTOT and MP shocks.



Note: The figure reports the estimated IRFs of domestic macroeconomic variables to the sanction shock identified using the recursive scheme as a shock to country spread. The IRFs are re-scaled so that the shock is equivalent to a +1 percentage point rise of $Country.Spread_t$. The BVAR model contains 11 variables: external characteristics—commodity terms-of-trade ($CTOT_t$), the Baa corporate bond spread ($Baa.Spread_t$), the real interest rate in the U.S. economy ($US.Real.Interest.Rate_t$); domestic indicators—industrial production (IP_t), private consumption ($Consum_t$), investments ($Invest_t$), trade balance (TB_t), corporate external debt ($ExtDebt_t$), Russia's country spread ($Country.Spread_t$), the real effective exchange rate ($REER_t$), and the regulated interest rate (real, $Regulated.IR_t$). The last variable captures the monetary policy reaction to the sanctions shock. The $Country.Spread_t$ variable is ordered third last. The Bayesian estimates are obtained with the flat (uninformative) prior. We set 10,000 draws from the posterior distribution and we discard the first 5,000 draws. Conventional credible bands comprised of the 16^{th} and 84^{th} percentiles of the post-burned-in estimated IRFs are reported (grey shaded area).

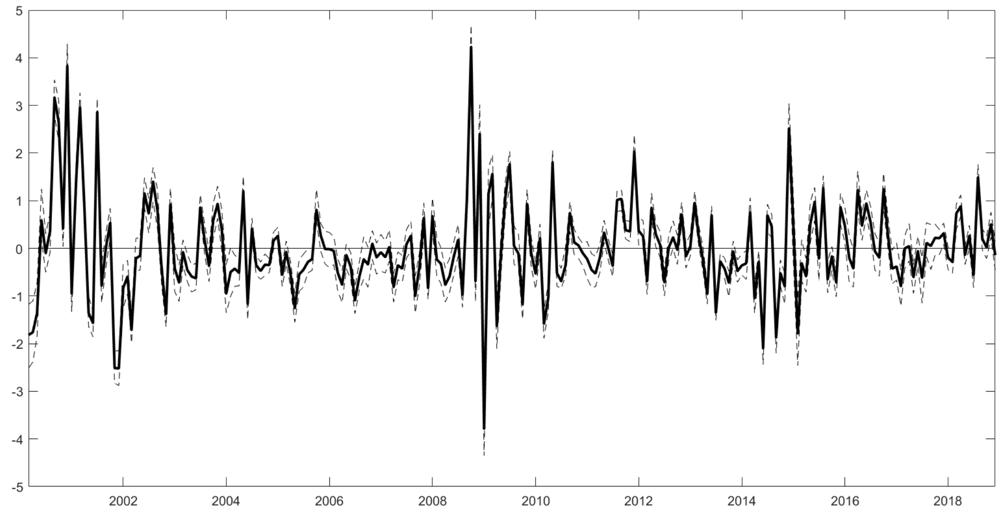
Figure 2. Impulse response functions to the country spread shock identified under the recursive scheme that accounts for endogenous monetary policy reactions

Appendix no. 25: Recursive identification with the HP-filtered time series



Note: The figure reports estimated impulse responses of domestic macroeconomic variables to a +1 percentage point shock in the country spread variable. The VAR model contains 11 variables, and the country spread variable is ordered third last, i.e., before the REER and domestic regulated interest rate variables. The Bayesian estimates are obtained with the flat (uninformative) prior. We set 10,000 draws from the posterior distribution and discard the first 5,000 draws. Conventional credible bands comprised of the 16^{th} and 84^{th} percentiles of the post-burned-in estimated responses are reported (grey shaded area).

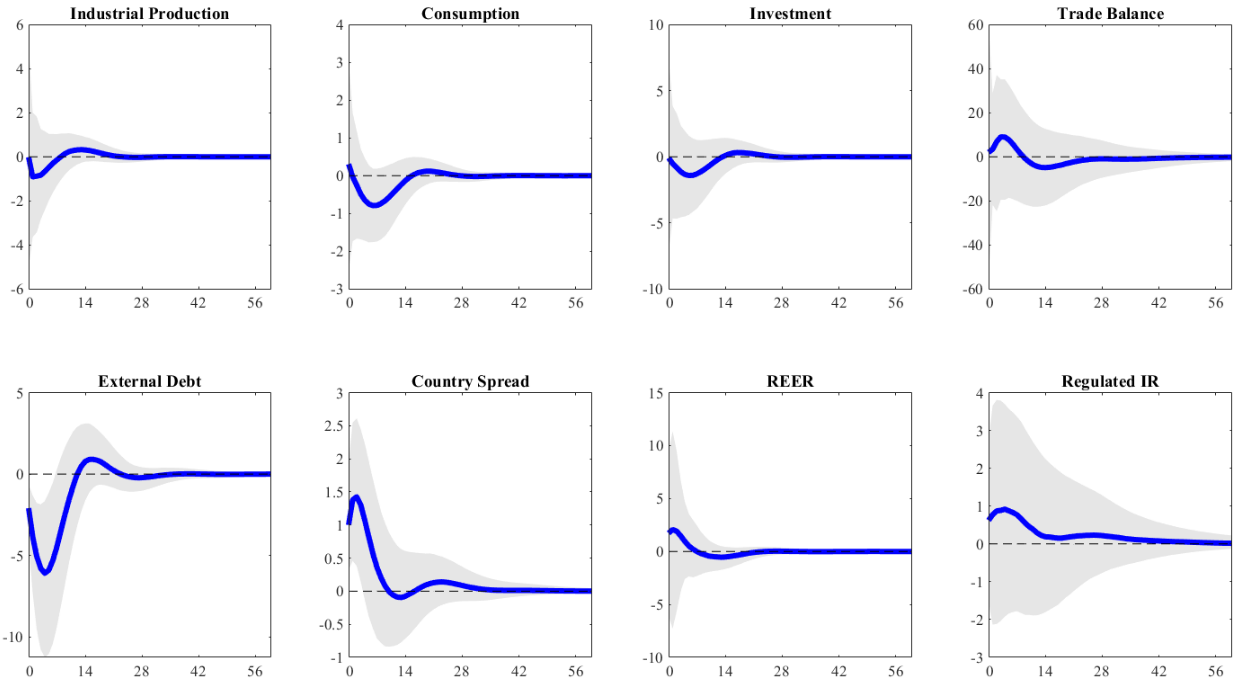
Figure 1. Impulse responses to the country spread shock identified under the recursive scheme



Note: The figure reports the time evolution of the positive shock to the country spread estimated with the 11-variable VAR model. Positive values of the shock variable reflect unexpected rises of Russia's country spread, and vice versa.

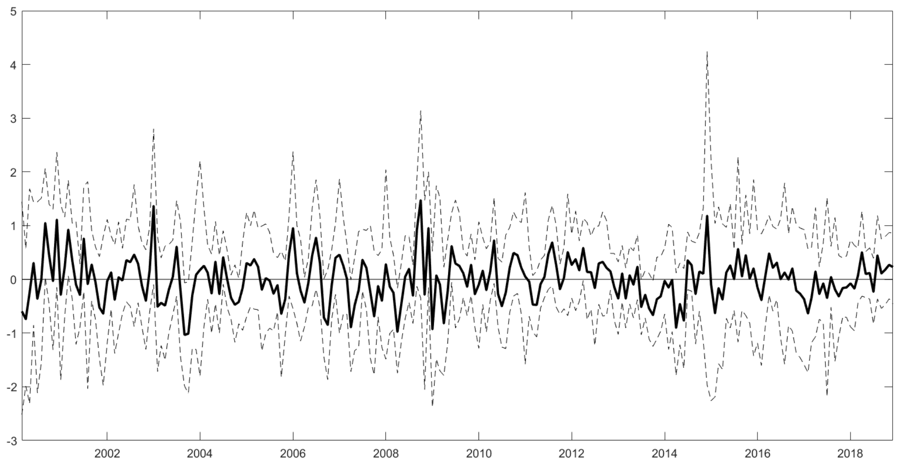
Figure 2. Time evolution of the country spread shock identified under the recursive scheme

Appendix no. 26: Sign restrictions with the HP-filtered time series



Note: The figure reports estimated impulse responses of domestic macroeconomic variables to a negative international credit supply (ICS) shock. The shock is re-scaled so that it is equivalent to a +1 percentage point rise in the country spread variable. The VAR model contains 11 variables. The Bayesian estimates are obtained with the flat (uninformative) prior. We set 10,000 draws from the posterior distribution and discard the first 5,000 draws. Conventional credible bands comprised of the 16^{th} and 84^{th} percentiles of the post-burned-in estimated responses are reported (grey shaded area).

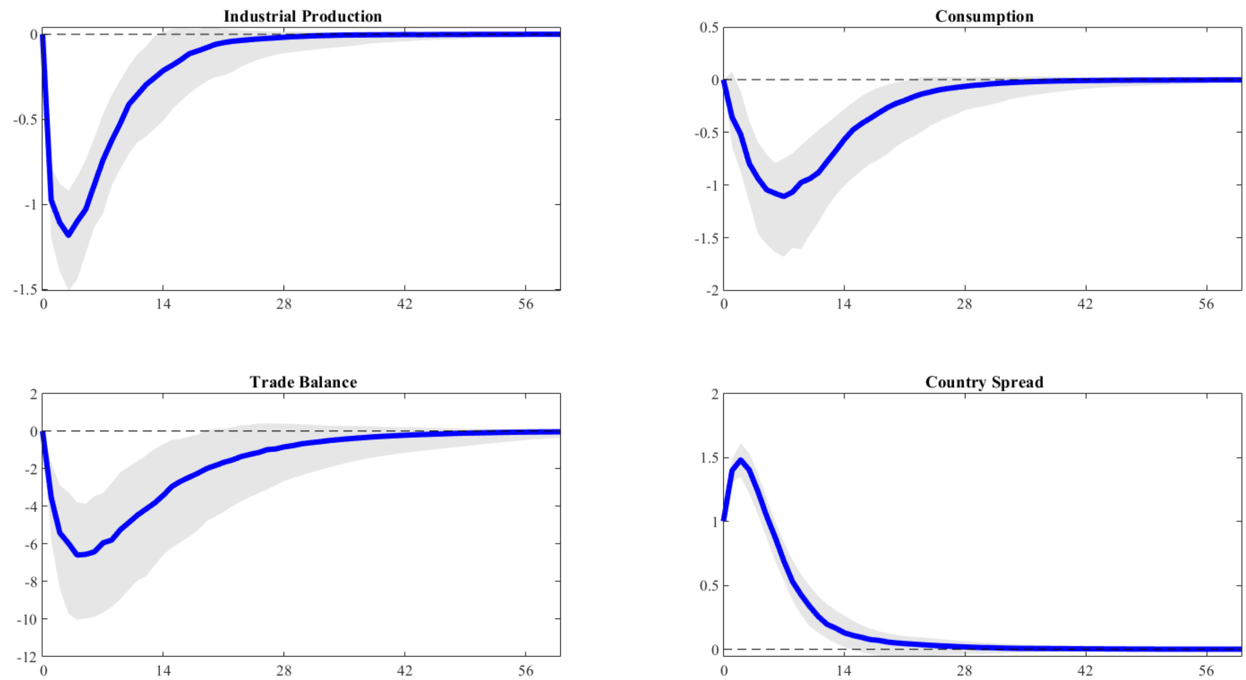
Figure 1. Impulse responses to the international credit supply shock identified under the sign restrictions



Note: The figure reports the time evolution of the estimated negative shock to the international credit supply (ICS) shock isolated with the use of the 11-variable VAR model. Positive values of the shock variable reflect unexpected declines in ICS and vice versa.

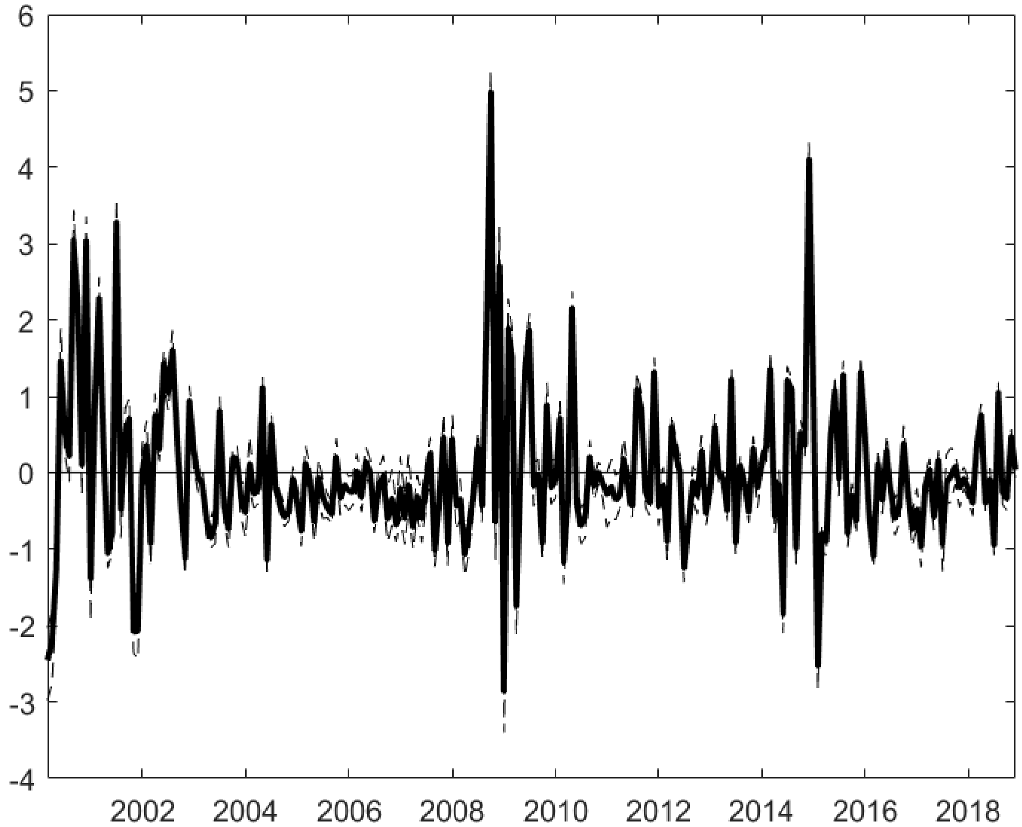
Figure 2. Time evolution of the international credit supply shock identified under the sign restrictions

Appendix no. 27: Recursive identification in the five-variable VAR model of Uribe and Yue (2006)



Note: The figure reports estimated impulse responses of domestic macroeconomic variables to a +1 percentage point shock in the country spread variable. The VAR model contains the five variables, as in Uribe and Yue (2006). The country spread variable is ordered last. The Bayesian estimates are obtained with the flat (uninformative) prior. We set 10,000 draws from the posterior distribution and discard the first 5,000 draws. Conventional credible bands comprised of the 16^{th} and 84^{th} percentiles of the post-burned-in estimated IRFs are reported (grey shaded area).

Figure 1. Impulse responses to the country spread shock identified under the recursive scheme in a five-variable VAR model



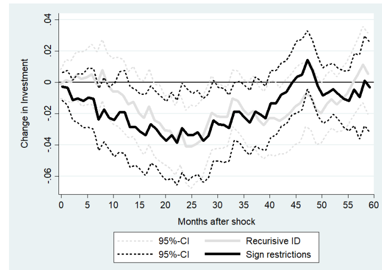
Note: The figure reports the time evolution of the positive shock to the country spread estimated with the five-variable VAR model. Positive values of the shock variable reflect unexpected rises of Russia's country spread, and vice versa.

Figure 2. Time evolution of the country spread shock identified under the recursive scheme in a five-variable VAR model

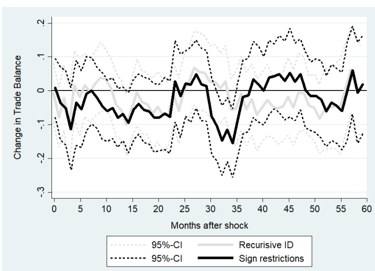
Appendix no. 28: Jorda's local projection of Russia's main macroeconomic indicators

(a) Industrial production

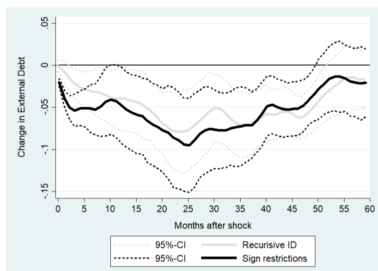
(b) Consumption



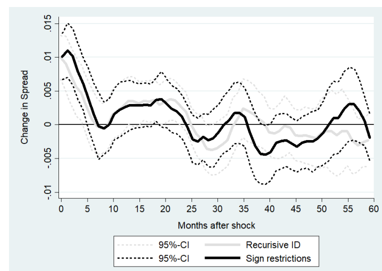
(c) Investment



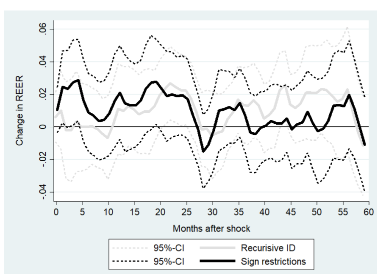
(d) Trade Balance



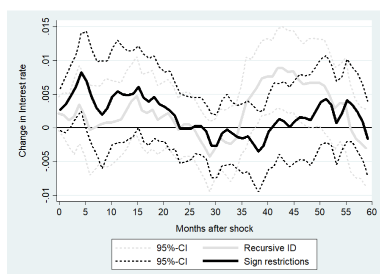
(e) External Debt



(f) Spread



(g) REER

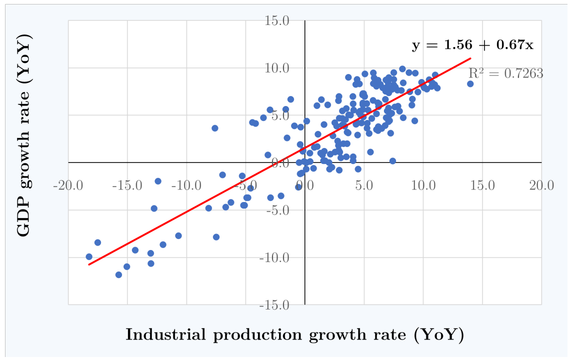


(h) Interest rate

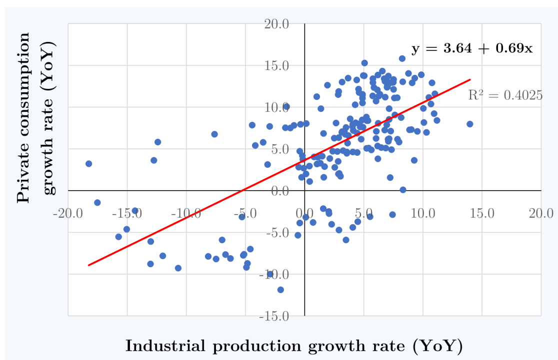
Note: The figure reports impulse responses to a positive country spread shock (*Recursive ID*) and a negative ICS shock (*Sign restrictions*). The responses are obtained under the Jorda's LP approach, as implied by $\beta_{j,h}$ in the following equation: $y_{i,t+h} = \alpha_{i,j,h} + \beta_{j,h} \cdot \hat{\varepsilon}_t^{(j)} + \gamma'_{i,j,h}\mathbf{X}_t + \mu_{i,j,t+h}$, where $y_{i,t}$ is ith domestic macroeconomic variable considered above ($i = 1, 2...8$), t is month from January 2000 to December 2018 and $h = 1, 2...60$ is prediction step ahead of the shock $\hat{\varepsilon}_t^{(j)}$, where $j = 1$ stands for the recursive identification (*country spread shock*) and $j = 2$ the sign restrictions scheme (*ICS shock*). $\mathbf{X}_t$ contains control variables: all monthly lags of $\hat{\varepsilon}_t^{(j)}$ from $1st$ till $12th$, thus covering the whole previous year, and the current values and 12th month lags of each of the eleven variables in y_t.. The 95% confidence intervals are computed with bootstrap (500 draws, with replications).

Figure 1. Impulse responses to the international credit supply shock and country spread shock estimated with the Jorda's local projection

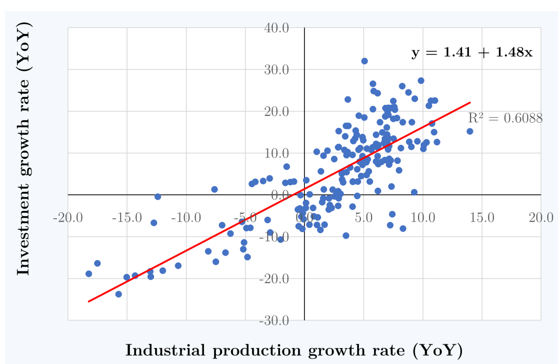
Appendix no. 29: Russia's industrial production and GDP components



(a) GDP



(b) Private consumption



(c) Investment

Note: The figure reports empirical relationships between industrial production and various macroeconomic characteristics of the Russian economy. The data covers the period of January 2000 to December 2018.

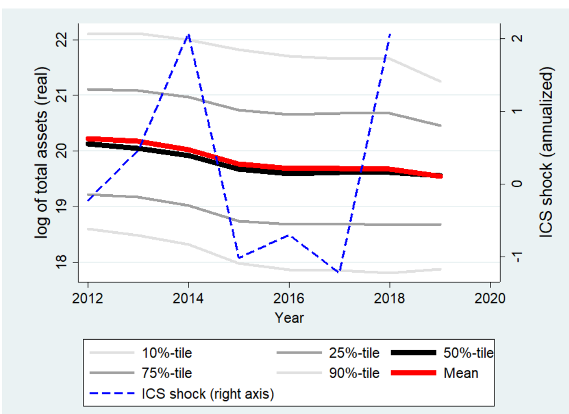
Figure 1. Relationship between industrial production and GDP components

Appendix no. 30: Details on the cross-section of firms in Russia

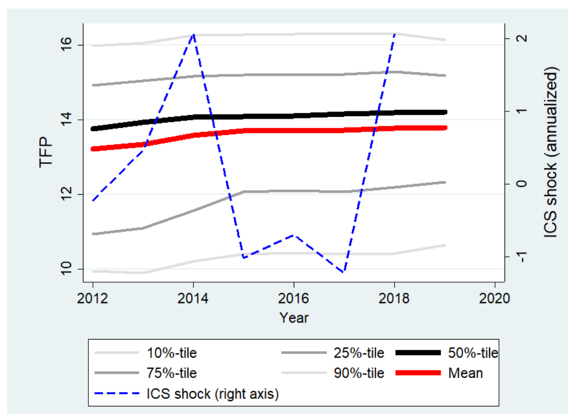
Table 1. Summary statistics for production function estimates

Note: The table reports the estimates of firms' total factor productivity, $TFP_{f,t}$, and the summary statistics for the variables employed in its estimation. We apply the methodology of Wooldridge (2009) and Petrin and Levinsohn (2012) to estimate a production function with the real value added $Y_{f,t}$ as a dependent variable and the number of employees $L_{f,t}$, capital (as proxied with fixed assets) $K_{f,t}$, and intermediate inputs (materials, as proxied with payments to suppliers) $M_{f,t}$. The estimation period is 2012–2019.

	Obs	Mean	SD	Min	Max
	(1)	(2)	(3)	(4)	(5)
$TFP_{f,t}$	40,381	13.6	2.2	6.1	21.4
$\ln Y_{f,t}$	40,381	18.5	1.5	12.2	23.2
$\ln L_{f,t}$	40,381	5.1	1.6	0.0	8.8
$\ln K_{f,t}$	40,381	18.3	2.0	8.9	23.8
$\ln M_{f,t}$	40,381	19.3	1.8	11.0	24.4

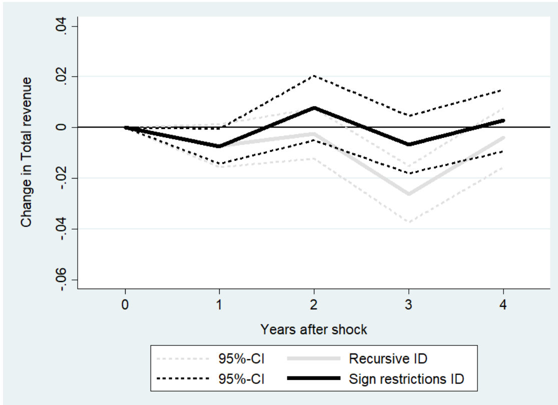


(a) Firm size $lnTA_{f,t}$

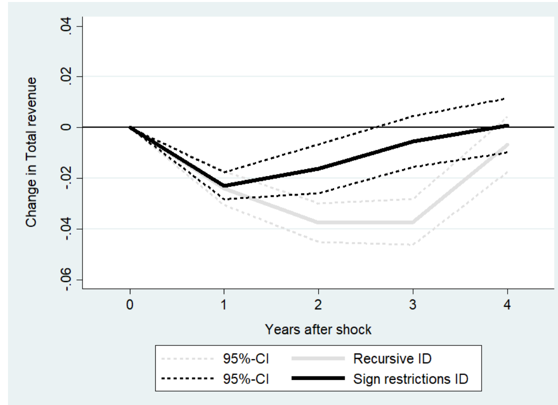


(b) Firm productivity $TFP_{f,t}$

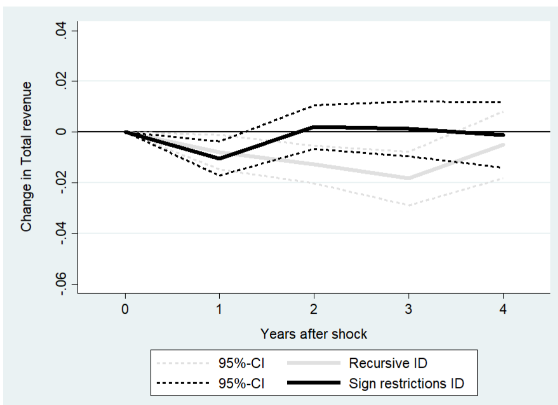
Figure 1. Firm size and productivity



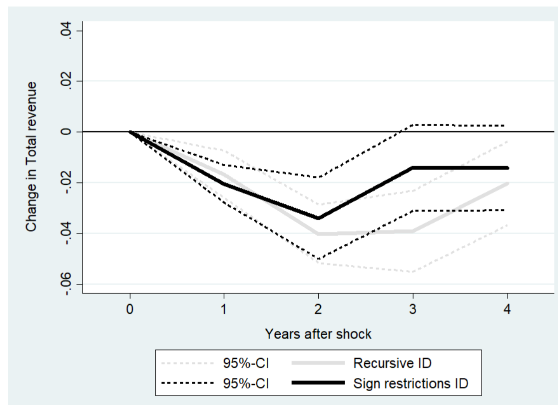
(a) Small firms with high TFP



(b) Large firms with high TFP



(c) Small firms with low TFP



(d) Large firms with low TFP

Note: The figure reports the impulse responses of the firms' total revenue (in constant prices) to the imposition of sanctions, as measured with the ICS (*Sign restrictions ID*) and country spread (*Recursive ID*) shocks. The responses are obtained using the Jorda (2005)'s LP approach. The sample contains 81,004 firm–year observations for 32,790 firms over the period of 2012–2018. The condition that the firms must operate for at least three consecutive years is not imposed. The monthly estimates of the ICS and country spread shocks, as measures of the financial sanctions, are aggregated to the annual level by summation of the monthly magnitudes within a given year.

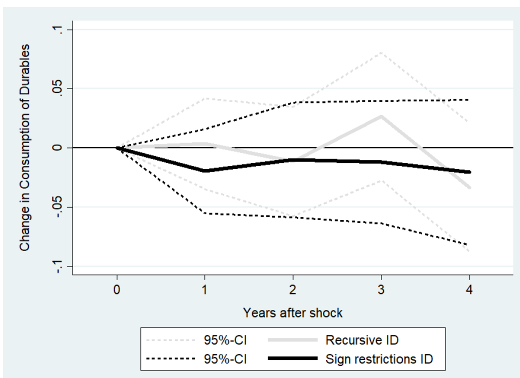
Figure 2. The effects of the sanctions shock on the real total revenue in a cross-section of firms

Appendix no. 31: Details on the cross-section of households in Russia

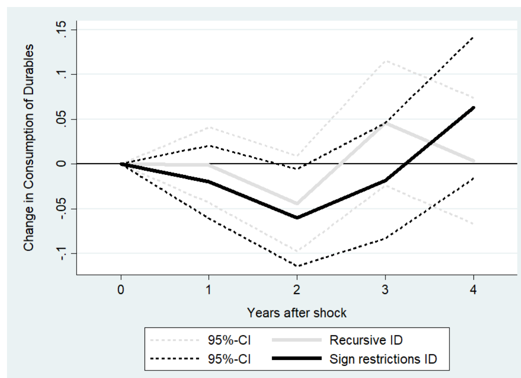
Table 1. Summary statistics for the sample of households

Note: The table reports the summary statistics on individuals' total income $Y_{i,t}$, total consumption $C_{i,t}$, consumption of durables $C_{i,t}^{D}$, and consumption of non-durables $C_{i,t}^{N}$, all in constant 2014 prices. The sample contains 21,813 individuals over the period of 2006–2018.

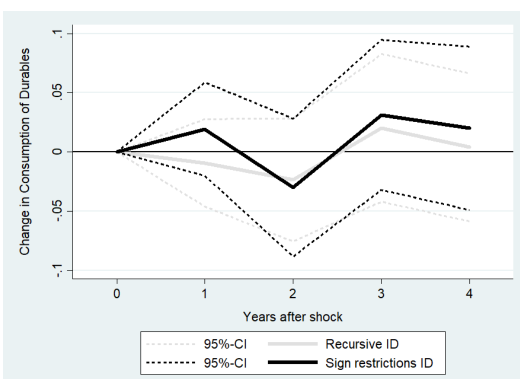
	Obs	Mean	SD	Min	Max
	(1)	(2)	(3)	(4)	(5)
$\ln Y_{h,t}$	74,356	5.9	0.6	4.1	7.2
$\ln C_{h,t}$	74,356	5.5	0.6	3.6	7.0
$\ln C_{h,t}^{D}$	74,356	3.1	1.7	–3.4	8.9
$\ln C_{h,t}^{N}$	74,356	5.3	0.7	–1.0	10.2



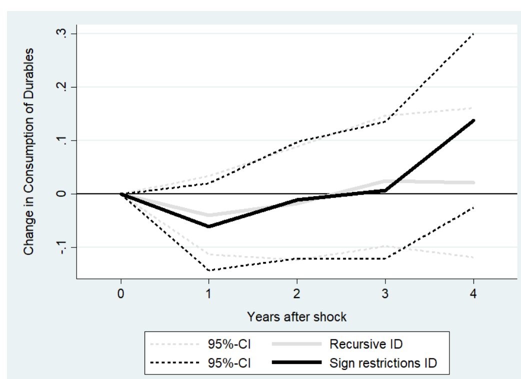
(a) High income, region's other places of living



(b) High income, region's capital city

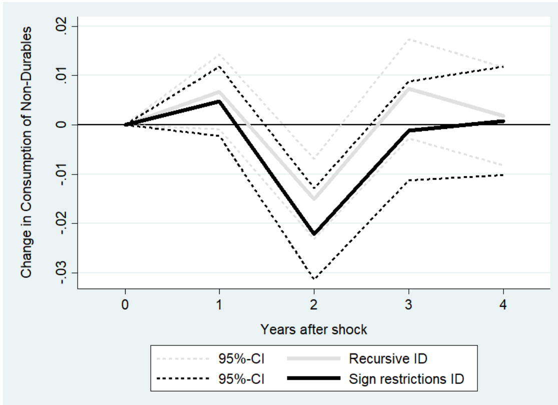


(c) Low income, region's other places of living

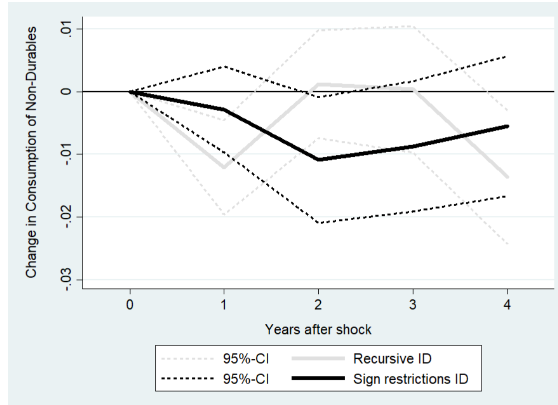


(d) Low income, region's capital city

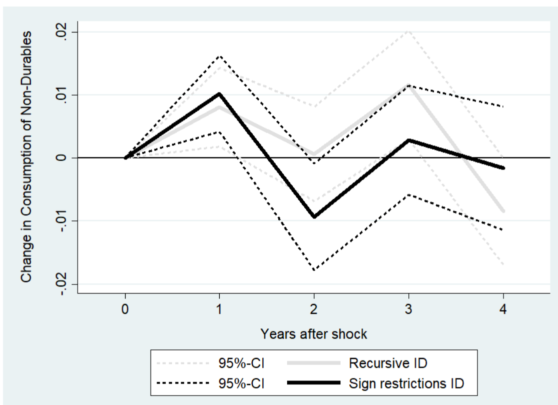
Figure 1. The effects of the sanctions shock on consumption of durables in a cross-section of households (*beginning*)



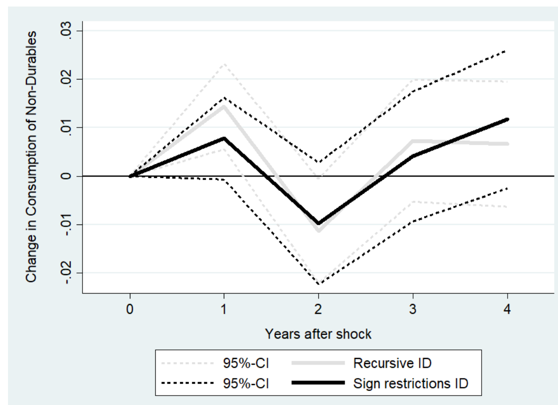
(e) High income, region's other places of living



(f) High income, region's capital city



(g) Low income, region's other places of living



(h) Low income, region's capital city

Note: The figure reports the impulse responses of the individuals' income (in constant prices) to the imposition of sanctions, as measured with the ICS (*Sign restrictions ID*) and country spread (*Recursive ID*) shocks. The responses are obtained using the Jorda (2005)'s LP approach. The sample contains 74,356 individual–year observations for 21,813 individuals over 2006–2018. The monthly estimates of the ICS and country spread shocks, as measures of the financial sanctions, are aggregated to the annual level by summation of the monthly magnitudes within a given year.

Figure 1. The effects of the sanctions shock on consumption of durables in a cross-section of households (*ending*)

Appendix no. 32: Sudden stop dynamics in emerging economies and Russia

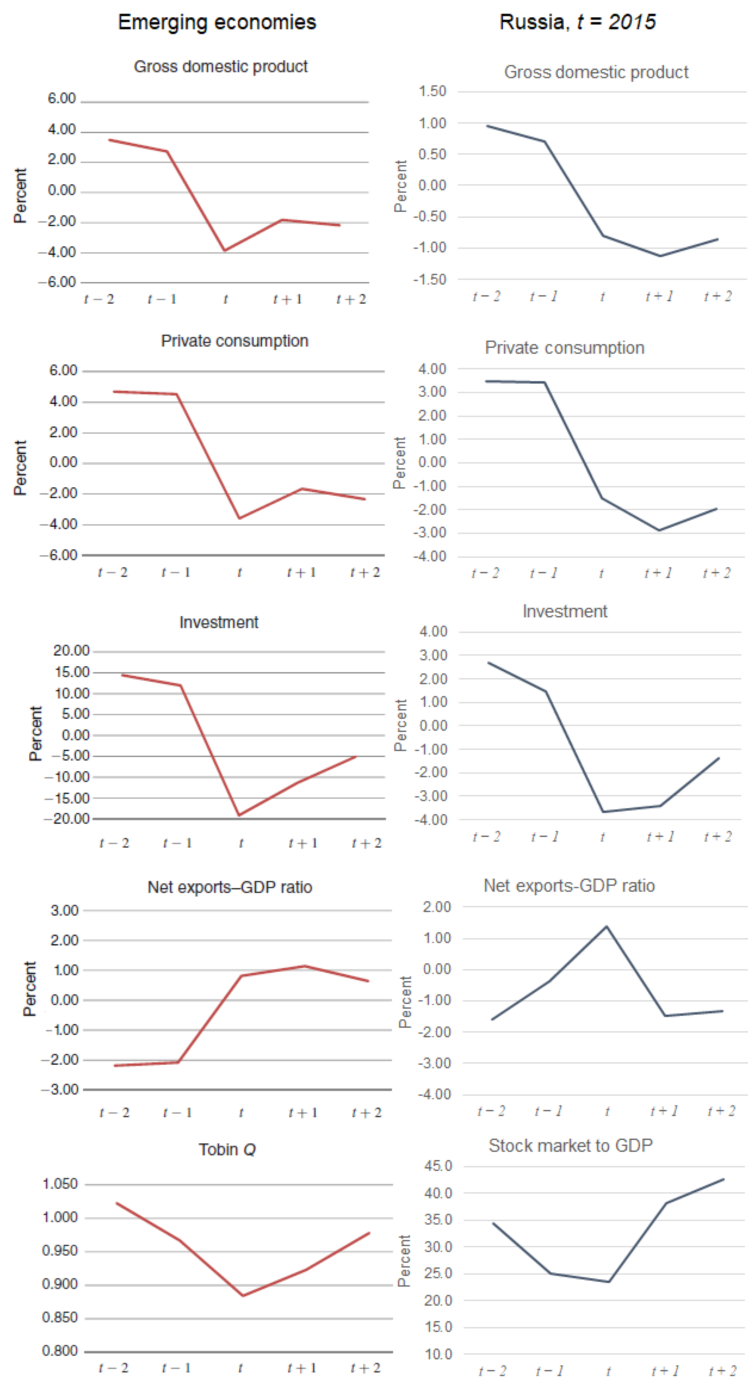


Figure 1. Macroeconomic dynamics around sudden stop events in emerging economies (cross-country medians of deviations from HP trends, left) and Russia, t=2015 (right)

Source: Mendoza (2010), Federal State Statistical Service, own calculations.