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Mikhail Mamonov
Anna Pestova

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Credit Supply Shocks and Household Defaults*

Mikhail Mamonov† Anna Pestova‡

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Abstract

Are disruptions of the mortgage market a consequence of financial imbalances accumulated in the past? In this paper, we study the effects of positive and negative credit supply (CS) shocks on subsequent household defaults on debt over the last four decades in U.S. states. 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 whereas 1989 and 2009 brought systemic *negative* shocks. Further, by employing a difference-in-differences framework, we find that both positive and negative CS shocks lead to greater household defaults in the future if they also increase mortgage-to-income ratios. We show that the CS shock-induced (i) shifts of employment between the tradable and non-tradable sectors, (ii) changes in household income and (iii) in house prices facilitate the accumulation of default risks. Our results indicate that positive CS shocks occurred in 1984 did not raise household defaults by more in more exposed states compared to less exposed states because the shocks increased both future income and mortgage debt, while not affecting mortgage-to-income ratios. In contrast, the 1989, 2004 and 2009 CS shocks increased mortgage-to-income ratios in subsequent years, thereby raising debt delinquencies and household defaults. These results provide further empirical evidence to theories of endogenous credit cycles.

Keywords: Household finance, Banking, Credit supply, Financial instability, Mortgage, Difference-in-differences, VARs, U.S. states, PSID, CEX.

JEL: C34, G21, G33.

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†CERGE-EI and MGIMO-University. CERGE-EI, a joint workplace of Charles University and the Economics Institute of the Czech Academy of Sciences, 111 21 Politickych veznu 7, Prague, Czech Republic. Tel. +420 776 071 490, mikhail.mamonov@cerge-ei.cz.

‡Corresponding author. CERGE-EI and MGIMO-University. CERGE-EI, a joint workplace of Charles University and the Economics Institute of the Czech Academy of Sciences, 111 21 Politickych veznu 7, Prague, Czech Republic. Tel. +420 776 071 490, anna.pestova@cerge-ei.cz.

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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 lead to an accumulation of economic imbalances.[1] We follow these ideas and investigate whether *positive* credit supply (CS) shocks 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.

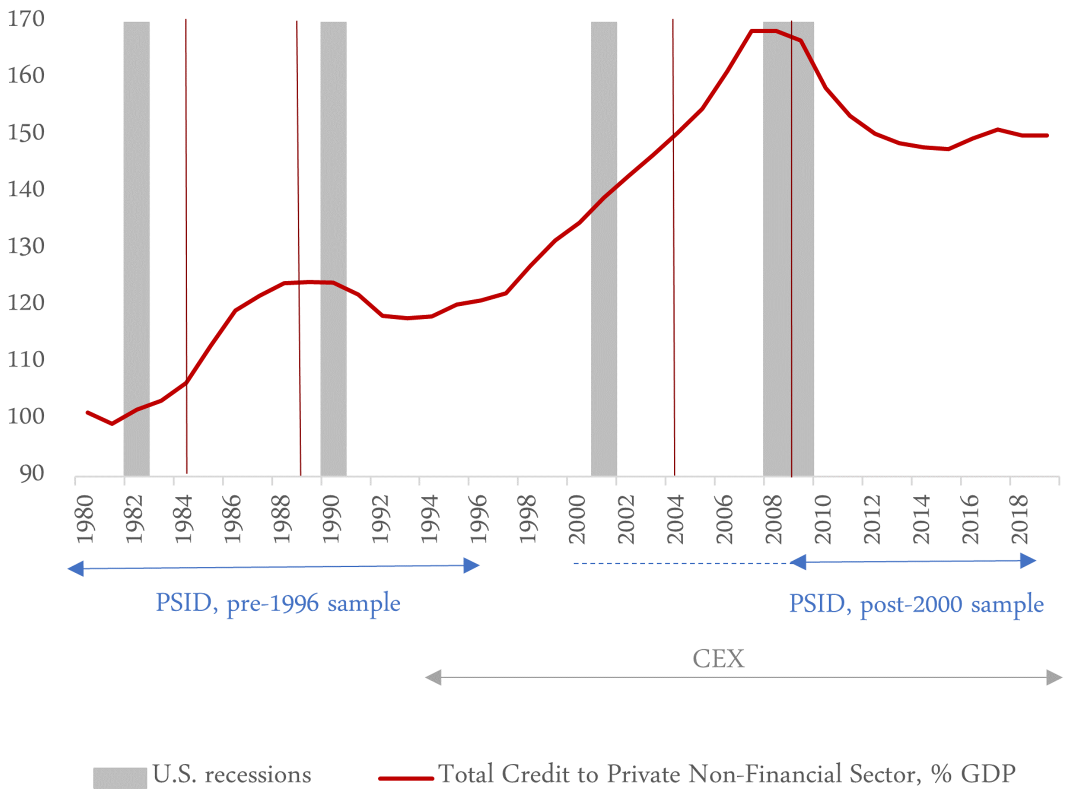
We empirically explore the channels that may rationalize the existence of the effects of CS shocks on household defaults and study time variation of the effects. We also examine possible asymmetries, i.e., we investigate whether *negative* CS shocks affect household outcomes, including defaults, differently than to positive shocks. Our empirical design is based on a differential treatment of U.S. states by CS shocks. In this way, our main source of identification is the variation in CS shocks 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 and systemic negative 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 and systemic credit tightening in 1989 and 2009 based on a careful analysis of the distribution of CS shocks across states in different years. Finally, in the third part of this paper, we estimate the effects of CS shock intensity on the probability of household defaults and other household outcomes on three subsamples of the data: in the 1980s, 1990s, and 2000–2010s. Here, we cross-validate our previous difference-in-differences analysis with a different empirical design and study time variation of the effects of CS shocks.

Let us now explain the timing of the analysis. In the difference-in-differences setting, we focus on the sub-samples of the 1980s and 2000s while we have to exclude the 1990s

[1] 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.

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 1). 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. In the final part of our empirical analysis, in which we estimate the effects of CS shock intensities on the probability of defaults and other outcomes at the household-level, we use data from all four recent decades. Here we are neither restricted by the particular dates of shocks, nor limited by the need to have data on defaults available before and after shocks.



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) which we further use in our difference-in-difference analysis. Detailed data on the fraction of states with positive and negative shocks by years is presented in Figure B.II in the Appendix. Detailed information on PSID and CEX micro-data on household defaults is provided in Section 3.2. Data on total credit to GDP ratio is from the FRED Economic Data portal of St.Louis Fed (source: BIS).

Figure 1: 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 either positive or negative 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, and 1989 and 2009, which brought systemic *negative* CS shocks. It is clear that 1984 and 2004 correspond to pre-crisis periods in the U.S. economy, while 1989 and 2009 mark the crisis periods. 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 four systemic CS shocks on subsequent paths of nine household-level outcomes and show that positive and negative CS shocks have different (asymmetric) effects. We also show, for a given household-level outcome, that the CS effect is likely to be sluggish and 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).

Further, we show that systemic negative CS shocks in both 1989 and 2009 raised household defaults and mortgage delinquencies over the five subsequent years. Our estimates suggest that in both episodes the shocks did lead to a decline in households' real total income and real house values (prices of collateral), which supports the credit supply view of Mian and Sufi (2017). We also show that the 1989 and 2009 shocks reshuffled employment by shifting workers from the non-tradable to the tradable sector, thus restraining local economies. This supports the explanation of CS shocks' transmission through the household demand channel highlighted by Mian et al. (2020). In terms of economic effects, we show that the 1989 negative CS shock raised the probability of household defaults by 0.03 points, which is large since it exceeds the mean probability by a factor of 7 and equals roughly a half of the default probability's standard deviation. We cannot precisely estimate the economic effects of the 2009 systemic CS shock due to the data limitations (noted earlier), and we thus switch from systemic episodes to a horizon of 2009–2017 for which the PSID provides data on mortgage delinquencies. For this recent period we examine both negative and positive state-level CS shocks and show that a one standard deviation increase in positive (negative) CS shocks led to a +6.3 (+8.6) percentage points change in the probability of mortgage delinquencies. Thus, the results suggest that both positive and negative CS shocks may lead to an overhang of financial risks in households' balance sheets.

We also 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, our empirical work considers both positive and negative CS shocks.

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 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, our paper is related to the empirical work on the determinants of household bankruptcies by Fay et al. (2002). We closely follow their model specification and use the same household default data in the third part of our analysis.

Fifth, several theoretical works rationalize household decisions to default using quantitative models (Chatterjee et al., 2007; Livshits et al., 2010, Mitman, 2016; Antunes et al., 2019). 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.

Sixth, 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., 2019; Mian et al., 2020). We 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.

Seventh, there is a vivid discussion in the literature 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.

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[2]. Second, we introduce CS shocks as an exogenous credit market "treatment". Our shocks, by construction, and unlike the treatment variables in other micro-level literature, 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 less evident. Third, we consider asymmetries in the effects of CS shocks. In particular, we evaluate separately the effects of positive and negative shocks. In this direction, we hypothesize that positive CS shocks in the past cause greater household default rates. We also hypothesize that negative CS shocks may also raise the probability of defaults, as suggested by the quantitative model of household credit of Antunes et al. (2019).

The remainder of the paper 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. In Section 4, we switch from considering systemic credit supply shocks in specific

[2]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.

years to analyzing the full distribution of the shocks (across years and states) and tracing their impact on household-level outcomes. We discuss the sensitivity of our results in Section 5. Section 6 concludes the paper.

2 Identification of credit supply shocks

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:

$$A(L)\, y_{s,t} = u_{s,t} \tag{1}$$

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; $A(L)$ 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 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 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 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)[3]. 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 shocks 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 and loan volume goes up, i.e. these two variables move in opposite directions. This implies an outward shift of 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 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, 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.

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

[3] An alternative approach to isolating credit supply shocks could be relying on differences between financial institutions in exposures to financial crisis: 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).

Table 1: 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.

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 overall tightness of the 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 VAR model in equation (1) on a panel data of U.S. states. 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 Antolin-Diaz and Rubio-Ramirez (2018)[4]. The algorithm enables us to draw from a conjugate uniform-normal-inverse-Wishart posterior of VAR coefficients. In particular, for each of 5000 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 B.I in Appendix B. The figure shows median IRFs

[4] A replication kit for Antolin-Diaz and Rubio-Ramirez (2018) is available online.

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 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[5] , which is available from 1977 onward. 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[6]. 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 in the state ("Total Loans & Leases"). We then use interest income on these loans ("Int

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

Inc[7] - 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, households 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 towards lending to all sectors in the economy, not only to households. Second, we consider data on commercial banks insured by the FDIC and do not include savings institutions into 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 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.[8] 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.

2.3 State-level credit supply shocks

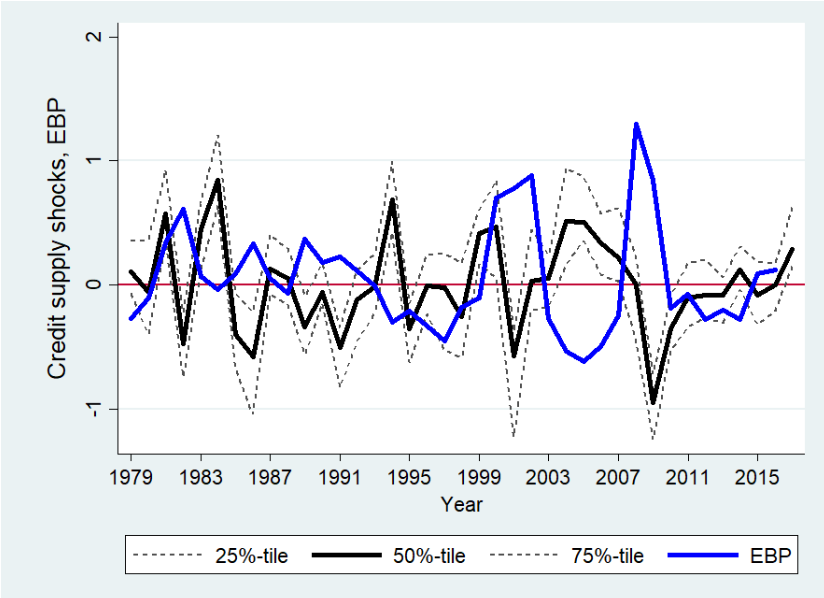
We begin our analysis of estimated state-level credit supply shocks by comparing our estimated shocks with the existing measure of credit market tightness. In particular, we compare the evolution of the median state-level credit supply shock to the excess bond premium (EBP) proposed in the influential 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* 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.

[7]Interest Income.

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

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 2). 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.



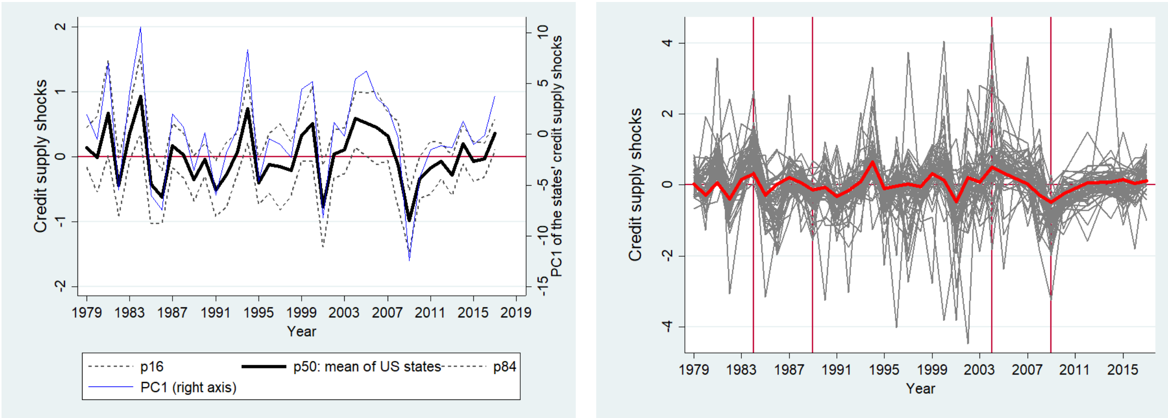
Note: Credit supply shocks are estimated on the panel data of U.S. states with Gambetti and Musso (2017) sign restrictions and Minnesota prior

Figure 2: 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 3a). 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 3b). 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 3: A common component of U.S. state-level credit supply shocks and state heterogeneity in the size of shocks

3 The effects of credit supply shocks on household outcomes: difference-in-differences analysis of shocks in 1984, 1989, 2004, and 2009 shocks

3.1 Methods and empirical strategy

In the first part of our micro-level analysis, we rely on the quasi-experimental design, difference-in-differences approach. 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 group" of 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 the stability of their funding base on the eve of the financial crisis. They compare 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 rate 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 correspond to one element in $ShockYear$ vector $= \{1984, 2004, 1989, 2009\}$:

$$Y_{i,s,t} = \alpha_i + \delta_s + \gamma_t + \sum_{k \neq ShockYear-1} \beta_k \cdot \mathbf{1}_{\{k=t\}} \cdot TREAT_s^{(ShockYear)} + \theta X_{i,s,t} + \epsilon_{i,s,t} \tag{2}$$

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),[9] 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, and negative shocks in 1989 and 2009. 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.,

[9] 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).

2020 and Jensen and Johannesen, 2017):

$$TREAT_s^{(ShockYear)} = \begin{cases} 1 \text{ if } \varepsilon^{CS}_{s,ShockYear} \geq \overline{\varepsilon}^{CS}_{s,ShockYear} \text{ and ShockYear} = \{1984,\, 2004\} \\ 0 \text{ if } \varepsilon^{CS}_{s,ShockYear} < \overline{\varepsilon}^{CS}_{s,ShockYear} \text{ and ShockYear} = \{1984,\, 2004\} \end{cases} \tag{3}$$

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

In contrast, in the years of *negative* shocks, 1989 and 2009, the variable $TREAT_s^{(ShockYear)}$ takes a value of 1 in the states *below* the median, i.e. the states hit by negative shocks that are largest in absolute value, and 0 otherwise. We assign these states into the "treatment" group and compare them with the states that faced less intensive negative shocks.

$$TREAT_s^{(ShockYear)} = \begin{cases} 1 \text{ if } \varepsilon^{CS}_{s,ShockYear} \leq \overline{\varepsilon}^{CS}_{s,ShockYear} \text{ and ShockYear} = \{1989,\, 2009\} \\ 0 \text{ if } \varepsilon^{CS}_{s,ShockYear} > \overline{\varepsilon}^{CS}_{s,ShockYear} \text{ and ShockYear} = \{1989,\, 2009\} \end{cases} \tag{4}$$

Similarly to Auclert et al. (2019) and Jensen and Johannesen (2017) we interpret coefficients β_k in equation (2) as the differences in outcome variables relative to the normalization year, in our case, the year preceding the $ShockYear$.

In the sensitivity analysis in Appendix F, 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":

$$Y_{s,t} = \alpha_s + \gamma_t + \sum_{k \neq ShockYear-1} \beta_k \cdot \mathbf{1}_{\{k=t\}} \cdot TREAT_s + \gamma X_{s,t} + \epsilon_{s,t} \tag{5}$$

Let us now rationalize why we choose 1984, 1989, 2004, and 2009 as the years of credit supply shock "interventions". First, we want to cover *full* credit cycle phases in the U.S. economy. To do so, we take the year in which a positive shock occurred in the beginning of a phase and then the year in which a negative shock occurred in the end of the phase. We show above (see Figure 1) that complete, or full, credit cycle' phases 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 "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 B.II in the Appendix) and conclude that the "systemic" positive shocks within the decades analyzed took place in 1981, 1984, 2004, and 2005, while "systemic" negative shocks occurred in 1986, 1989, 1991, and 2009. 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. We choose 1989 instead of 1986 and 1991 because Mian et al. (2020) argue that the expansionary phase of the credit cycle ended in 1989 (and started in 1982), and the contraction spanned over 1989–1992. Moreover, 1991 was already a recession year, which can contaminate our analysis. The negative shock in 2009 is the only one visible and prevalent across states in the late 2000s. Following Mian et al. (2020) and other literature on bank deregulation, we exclude South Dakota and Delaware from the analysis.

In the sensitivity analysis in Appendix E, 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: $t = ShockYear = \{1984, 2004, 1989, 2009\}$.

$$Y_{i,s,t+h} = \alpha_h + \beta_h \cdot TREAT_{s,t} + \gamma_h \cdot Y_{i,s,t-1} + \theta X_{i,s,t} + \epsilon_{i,s,t+h} \tag{6}$$

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 include into our analysis the following set of outcome variables at the household level, Y:

1. Household defaults: bankruptcy (PSID, 1980 — early 1990s) or mortgage delinquency indicator variable (CEX, 2000s)
2. Employment status, employment in tradable or nontradable 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. Home ownership 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 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 nontradable sector, alongside an outpacing rise of prices in nontradable 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 latter. We thus consider employment outcomes, including employment breakdown by tradable and nontradable 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 an old 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 home ownership rate among the variables through which they test their quantitative model. Following them, we also add this variable to our empirical analysis.

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-onwards ("2000s") (see Figure 1 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 (2), 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.

Two important observations should be noted here. First, 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[10]. Second, the PSID data has annual frequency until 1999, and biannual afterwards, 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-onwards).

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 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.

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

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 a mortgage delinquency of 1- or 3-month duration in a given year if in any quarter of the year there is a corresponding delinquency event.

3.2.1 A closer look at the micro-data on defaults

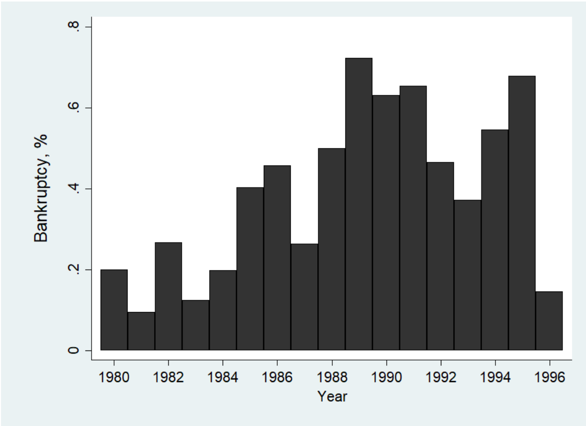
We begin by describing household defaults data in the pre-1996 sample. In 1996, there was a special one-time interview in the PSID in which households were asked about financial distress events. Importantly, households were asked about bankruptcies, not only in the year of interview (1996) but also for *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 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,[11] 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.

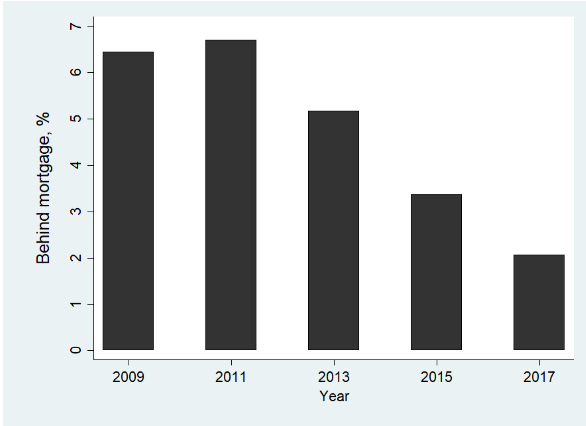
Importantly, we have a reasonable time series variation in our indicator of interest (see Figure 4a). In particular, we have a local peak in the bankruptcy rate in 1982 following the 1981-1982 recession. Then there is another long-lived bankruptcy level peak in the years following the S&L crisis in the 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 bankruptc recall rate is negligible,

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

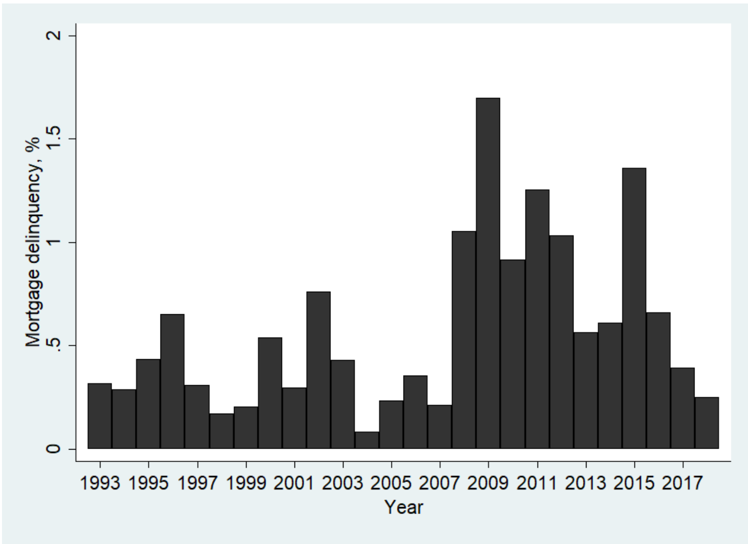
then our identification remains valid.



(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 4: Empirical frequency of household bankruptcies and households mortgage delinquencies according to the PSID and CEX

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 4b. 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. However, in the third part of this paper, in which we are not restricted to a particular timing of shocks, we use this variable in our econometric estimation (see Section 4). A comparison of the frequency of "behind mortgage" events reported in the PSID with the nationwide data on mortgage delinquencies published by FRED[12] 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 estimates (see Figure A.I in Appendix A).

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 4c. 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.

3.2.2 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 nontradable 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, military services. Following Mian et al. (2020), we assign agriculture, mining, and manufacturing into tradable industries; nontradable industries include construction, transportation, trade, and all service industries.

Total family income. We collect information on the 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 have 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 mortgage. 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 on whether a household owns the home or apartment. We assign a value of one if a head reports that he owns or is buying the home, and zero otherwise.

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

3.2.3 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 A.I (see Appendix A).

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 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 year of interview and reported age, and assigning linear growth of age if the recorded age produces an inconsistent year of birth. Based on 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.

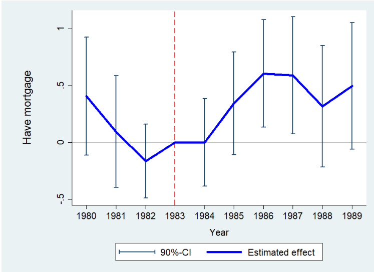
3.3 Estimation results: the effects of systemic positive CS shocks

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 (2) for each of the nine household-level outcomes discussed above.

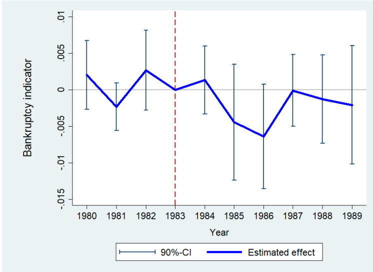
3.3.1 1984 positive CS shock

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. 5 below and depict the time evolution of the effects normalized to the pre-shock year, i.e., 1983.

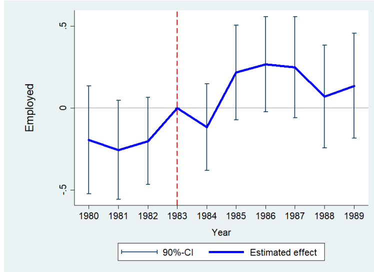
First, from the employment side, we find that the positive CS shock in 1984 had no redistribution effects from the tradable to nontradable sectors in the states that experienced larger CS shocks (see Fig. 5.d). This is in contrast to the findings of Mian et al. (2020). Moreover, the estimated effects on employment in the nontradable sector are negative and significant, both statistically and economically: the peak effect was achieved by 1987 and



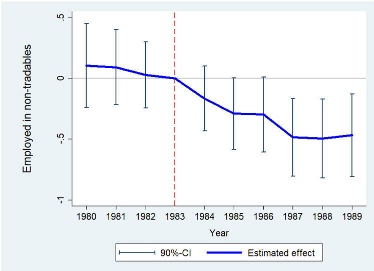
(a) Whether have mortgage



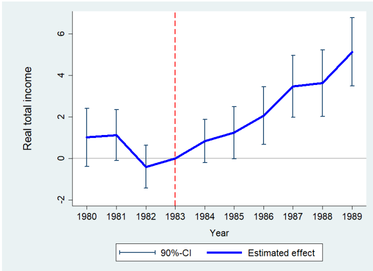
(b) Whether file bankruptcy



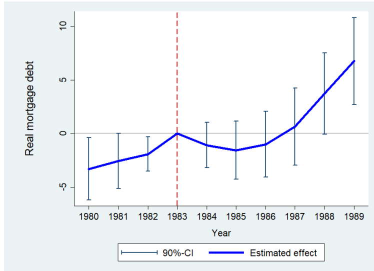
(c) Whether employed



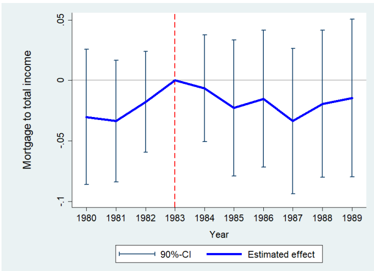
(d) Employed in non-tradables



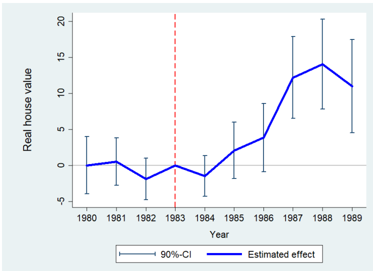
(e) Real total income



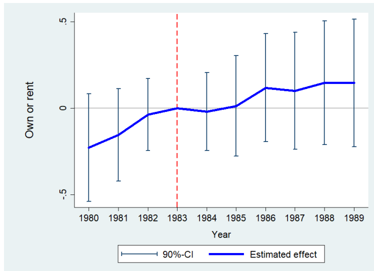
(f) Real mortgage debt



(g) Mortgage to income



(h) Real house value



(i) Whether own or rent

Note: The figure reports the results from estimating equation (2) 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 5: The effects of the positive CS shock of 1984 on household outcomes

equaled a –50 percentage points change in the likelihood of being employed in the nontradable 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. 5.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. 5.e), with the peak reaction being a little less than +5 thousand 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. 5.a): the peak reaction occurred in 1986 and equaled to a more than +50 percentage points 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. 5.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. 5.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. 5.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. C.I in Appendix C). 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.

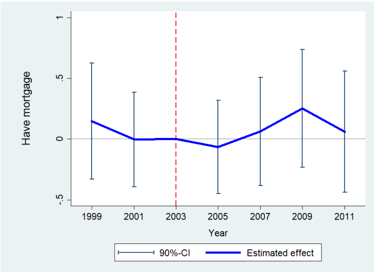
Third, having established that the 1984 CS shock led to a rise in total income and increased mortgage lending, we further find that real house values also went up, and rather substantially, by as much as +15 thousand U.S. dollars by 1987 (see Fig. 5.h). Notably, we reveal no pre-trends here, which means 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 boom and bust 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. 5.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.

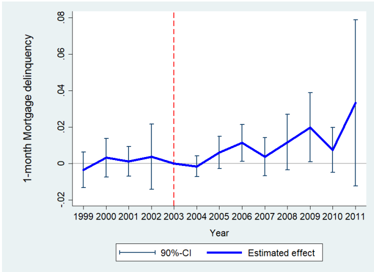
Finally, we repeat the same exercise with the use of a treatment variable constructed based on the early vs. late deregulation dummy variable of Mian et al. (2020) instead of our CS shock measure of 1984. This variable equals one if a state deregulated its inter- or intra-state restrictions prior to 1983 and zero otherwise. The estimation results are reported in Fig. D.I (see Appendix D). 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 originated from early deregulation of U.S. states.

3.3.2 2004 positive CS shock

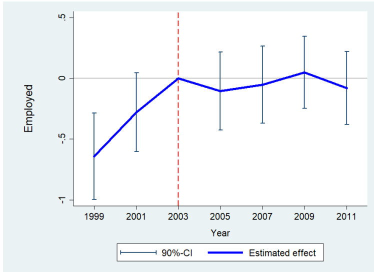
We now turn to the episode of systemic positive CS shock in 2004 and compare its subsequent effects with those discussed above for the 1980s. The estimation results appear in Fig. 6 below.



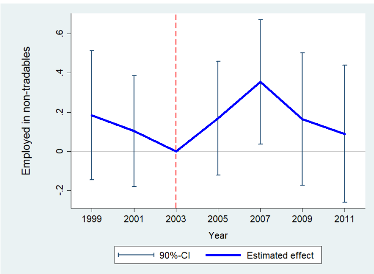
(a) Whether have mortgage



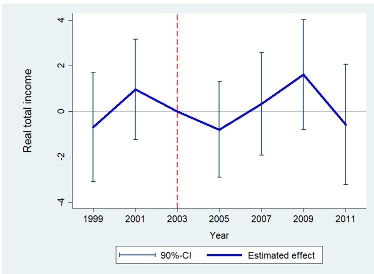
(b) Whether mortgage delinq. (CEX)



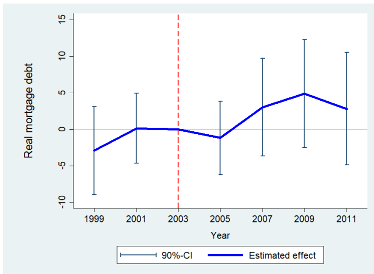
(c) Whether employed



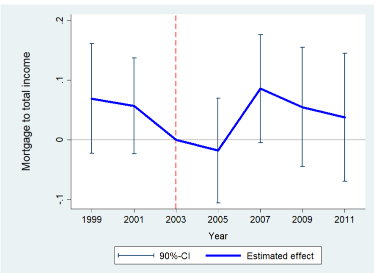
(d) Employed in non-tradables



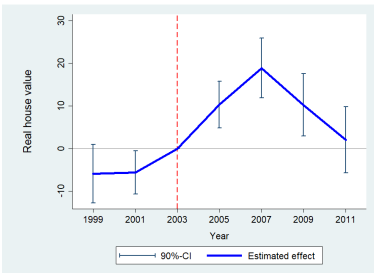
(e) Real total income



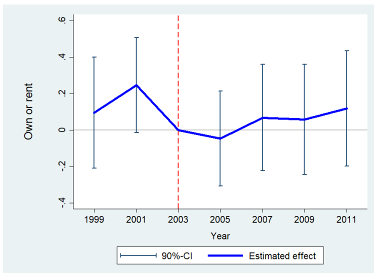
(f) Real mortgage debt



(g) Mortgage to income



(h) Real house value



(i) Whether own or rent

Note: The figure reports the results from estimating equation (2) 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 6: The effects of the positive CS shock of 2004 on household outcomes

First, we find no effects of the positive CS shock of 2004 on employment (see Fig. 6.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 sub-prime 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. 6.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. 6.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. 6.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. E.II.g in Appendix E). 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[13] in more vs. less exposed states (see Fig. 6.a,i).

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. 6.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 prices growth and led 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. 6.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., 2021).

[13]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 sub-prime borrowers were able to access the market.

3.3.3 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 1980s proceeded, "treated" states just catched up with "control" states. One more reason why we do not observe rises of default rates in response to the positive CS shock of 1984 is that we do not find an evidence of the *household demand* channel operating in 1980s. This is reflected in that we find no shifts of employment from tradable to *nontradable* sectors,[14] thus indicating that local household demand and debt-financed consumption was unlikely to disproportionately speed up in response to the positive CS shock. Therefore, the associated financial risks — proxied in 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. (2019) treatment variable.

Second, in contrast to 1980s, in 2000s we do observe *higher* delinquencies on mortgage loans in states with *stronger* CS chocks. The reasons are that (i) real income growth of households was close to zero or even negative in some states[15] and (ii) we do find an evidence of employment shifts from tradable to nontradable sectors, thus supporting operation of the household demand channel (Mian et al., 2020) in 2000s.

Finally, both positive systemic credit shocks of the 1980s and 2000s were accompanied by subsequent house price rises, which is 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 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 2000s.

[14] 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 of our results with those of Mian et al. (2020) could be due to data format: we apply more granular data, i.e., on 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.

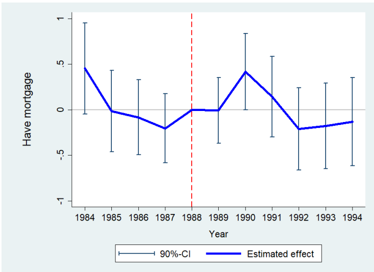
[15] This is, in turn, in line with Mian and Sufi (2009) findings.

3.4 Estimation results: the effects of systemic negative CS shocks

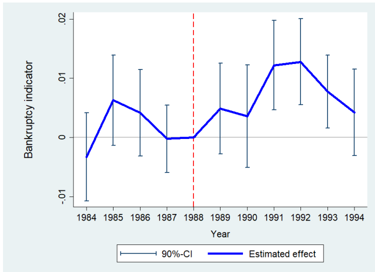
In this section, we turn to the systemic negative CS shocks of 1989 and 2009, as suggested by our empirical approach. We study the effects of these shocks on household-level outcomes.

3.4.1 1989 negative CS shock

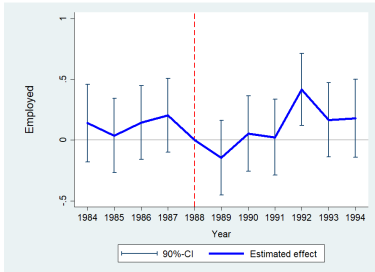
We can make several observations based on the estimation results of the effects of the 1989 negative CS shock (see Fig. 7 below).



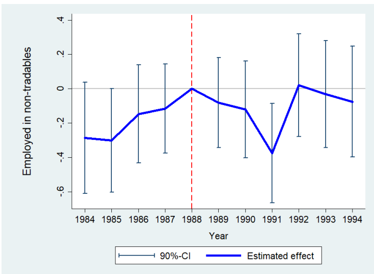
(a) Whether have mortgage



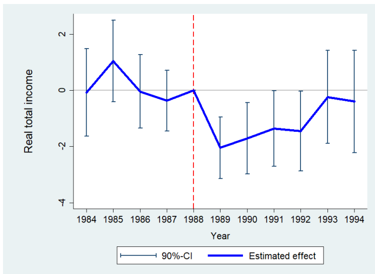
(b) Whether file bankruptcy



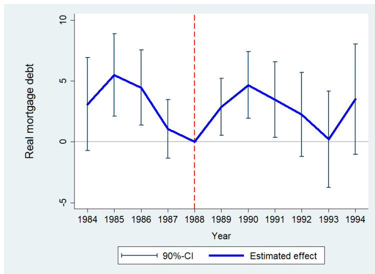
(c) Whether employed



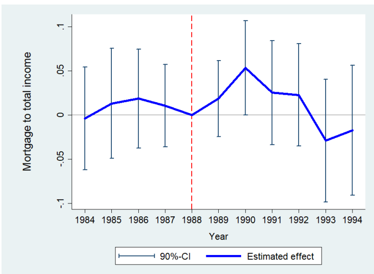
(d) Employed in non-tradables



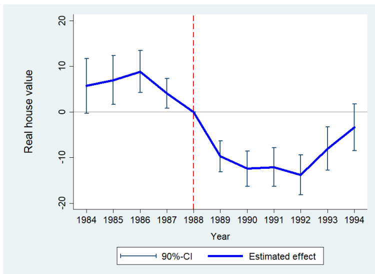
(e) Real total income



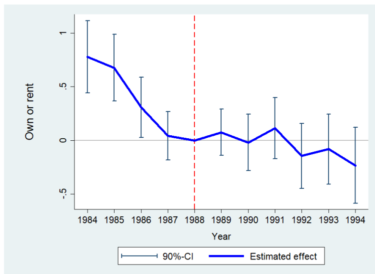
(f) Real mortgage debt



(g) Mortgage to income



(h) Real house value



(i) Whether own or rent

Note: The figure reports the results from estimating Equation (2) for a set of nine outcomes measured at the household level in the 1980s and 1990s and our SVAR-based measure of CS shocks. The pre-shock year is 1988, 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 7: The effects of the negative CS shock of 1989 on household outcomes

First, we do not find any negative effect of the negative CS shock in 1989 on the subsequent path of the employment rate (see Fig. 7.c). We even see a rise in the employment rate in 1992, i.e., in the first year after a recession, which suggests a "cleansing" effect of the shock. At the same time, the estimates point to a decrease in the employment rate in the nontradable sector after the shock (see Fig. 7.d) and a corresponding rise in employment in the tradable sector.[16] This may favor a household demand channel, as suggested by Mian et al. (2020).

Second, the estimates further indicate that real total income declines after the negative shock over the course of the four subsequent years, and that this decline is larger in the states more affected by the shock (see Fig. 7.e). This could be driven by a reduction of local economic activities attributable to decreasing employment in non-tradables, as discussed above. Interestingly, we do not find evidence of declining volumes of mortgage debt after the negative CS shock of 1989, as one could fairly expect. Moreover, the estimates exhibit a positive effect, which is counter-intuitive. However, they also exhibit significant and positive pre-trends in this case, suggesting that the states populated with more indebted households prior to 1989 are also those more likely to be hit by stronger negative shocks in 1989. Given significant pre-trends, we do not interpret causally the subsequent positive effects on the mortgage debt level. We then cross-check this result in Jorda's local projection setting and find insignificant effects there (see Fig. E.III.f in Appendix E). Combining stable mortgage debts with declining total income, we expectedly obtain a rise in mortgage-to-income ratio: a positive and significant (though marginally) effect appeared in 1990, i.e., one year after the shock occurred (see Fig. 7.g).

Third, we find that the negative CS shock in 1989 contributed to a substantial decline in real house value over the next five years (see Fig. 7.h). However, we again cannot interpret this effect as causal because we observe positive and significant pre-trends. These positive pre-trends in turn suggest that the states with more overheated housing markets were affected by the negative credit supply shock more strongly than other states.

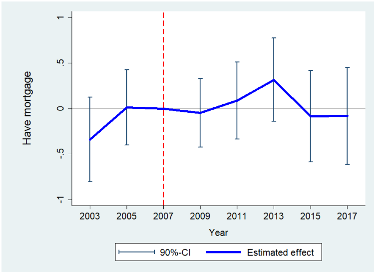
Finally, given that the stronger negative CS shocks of 1989 contributed to a subsequent decline of income and a rise in the debt-to-income ratio, the corresponding rise in household bankruptcies is not surprising (see Fig. 7.b). According to our analysis, the main operating channels through which negative credit supply shocks affected household bankruptcies in the 1980s – early 1990s are the income- and household-demand channels.

3.4.2 2009 negative CS shock

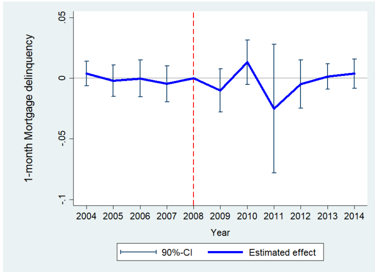
Finally, we report the estimation results on the effects of the 2009 negative CS shock, shown in Fig. 8 below.

First, in contrast to the negative CS shock in 1989, here we observe a persistent decline in employment rate (see Fig. 8.d). Further, and in line with our results for the 1980s, we find that following a stronger negative CS shock employment flows from the nontradable to the tradable sector. Given our finding above that, following the 2004 positive CS shock employment shifted in the opposite direction, this finding can be interpreted as a backward reversal of employment shares in the two sectors.

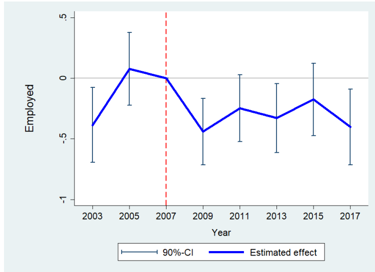
[16] Not presented here for the sake of space. Note that a household in our classification can work either in the tradable or nontradable sector.



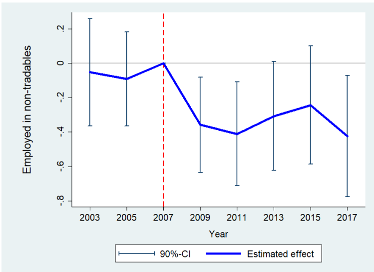
(a) Whether have mortgage



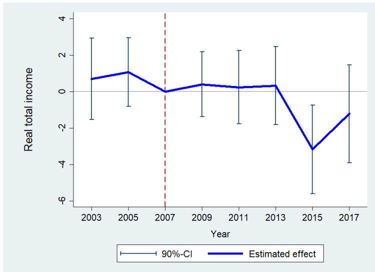
(b) Whether mortgage delinq. (CEX)



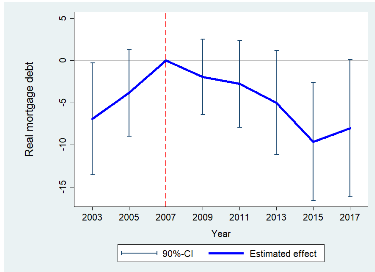
(c) Whether employed



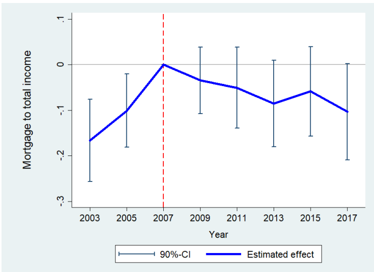
(d) Employed in non-tradables



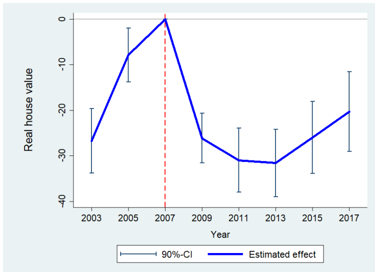
(e) Real total income



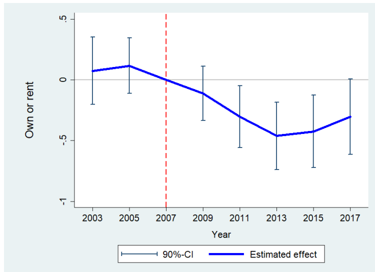
(f) Real mortgage debt



(g) Mortgage to income



(h) Real house value



(i) Whether own or rent

Note: The figure reports the results from estimating equation (2) for a set of nine outcomes measured at the household level in the 2000s and 2010s and our SVAR-based measure of CS shocks. The pre-shock year is 2007, 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 8: The effects of the negative CS shock of 2009 on household outcomes

Second, as in the case of the 1989 negative CS shock, the estimates suggest a negative effect on real total income which, however, appears with a time lag. This could be explained by the persistent effect of the shock on employment levels we noted above. We also find that the effects on the levels of mortgage debt are negative and significant while the effects on the mortgage-to-income ratios are insignificant; however, both effects are not interpreted causally because they contain (negative) pre-trends. In contrast, an interesting result is observed when we analyze the effects on ownership status: a more negative CS shock in 2009 leads to a significantly greater decline in the home ownership rate without any pre-trend, suggesting that the negative shocks may have increased foreclosure rates in the 2010s.

Third, we also find a decline of the real house values after the 2009 negative CS shock. However, there are clear negative pre-trends here, as in the case of mortgages, and we do not interpret the corresponding effect of the shock as causal.

Finally, we do not reveal any statistical differences in mortgage delinquencies between the states more vs. less affected by the 2009 negative CS shock. This is likely due to the absence of the effect on household indebtedness as measured by the mortgage-to-income ratios. As we show above, these ratios remain stable despite declining employment and income in the states with more negative CS shocks.

3.4.3 Summary of results for the negative CS shocks episodes in 1989 and 2009

We find that greater exposure to the systemic negative CS shock of 1989 led to rising household default rates during 1989–1996, for which the PSID provides the data on household bankruptcies. Recall that, unfortunately, the PSID data does not contain information on household bankruptcies after 1996, while the information on household mortgage delinquencies starts only in 2009, which is not enough for our analysis (in particular, the pre-trend assumptions cannot be estimated). Therefore, for our analysis of the effects of the 2009 negative CS shock we switch from household-level to cohort-level data on mortgage delinquencies provided by the CEX from 1994 to 2019. However, even with the CEX data we do not find evidence that the 2009 systemic negative CS shock led to greater delinquencies in the states more affected by the shock. Possibly, the lack of a statistically significant effect is caused by the *time-changing composition* of cohorts, i.e., by the fact that we do not observe the same households across years; instead we observe only the cohorts of households whose heads were born in a particular 5-year time period. Another possible reason is that we do not have actual data on mortgage delinquencies in the CEX. As a substitute, we estimate the timing of an event of mortgage delinquency based on the information of whether the mortgage balance was constant or principal payments on the mortgage were zero over one or three months. We acknowledge that these estimates may be noisy, thus contaminating the quality of our estimated effects.

Overall, we still suspect that the 2009 negative CS shock did cause a rise in problems with mortgage payments — indirect evidence supporting this view is that we also find a decrease in home ownership rates in more exposed states. Loss of home ownership may be an indication of mortgage foreclosures, which resulted from households' inability to pay mortgages. One possible remedy for our analysis is to switch from systemic CS shocks in a given year to the full shock variation across years, because the latter allows us to use the PSID, not the CEX, data on mortgage delinquencies in 2009–2017. We return to this issue in the final section of the paper.

Nonetheless, our results in this section indicate that both the 1989 and 2009 episodes of systemic negative CS shocks led to a subsequent decline in household real total income and real house values, thus supporting the credit supply view of Mian and Sufi (2017). Further, we also find that, in both episodes, employment shifts from the non-tradable to the tradable sector occurred after the shocks, thus restraining local economies and activating the household demand channel highlighted by Mian et al. (2020).

4 The effects of CS shock intensity on household outcomes: subsamples of the 1980s, 1990s, and 2000s

4.1 Model specification

We now turn from considering the periods of *systemic* credit supply shocks (1984, 1989, 2004, and 2009) to analyzing the whole distribution of the shocks across both states and *years*. Our analysis in the previous section revealed the following three regularities in the data which we implement in our regression specifications in this section.

First, the *asymmetric effects of positive and negative CS-shocks.* In order to properly capture the effects of credit supply shocks we need to separate the time-series of the shock into positive and negative parts, and test their own effects rather than attempting to reveal a single effect of the shock. For example, one could follow the latter approach and obtain a positive coefficient on the shock in the regression of household defaults. However, this coefficient would imply that only positive values of the shock are associated with greater household defaults, while negative values would imply lower household defaults. Our results from the previous section indicate that this is not true. That is, both positive and negative credit supply shocks may lead to increased household default rates, through either a greater accumulation of credit risk exposures (economic imbalances) or through a lower opportunity to refinance existing debts. We thus account for this finding by dividing the estimated credit supply shock into positive and negative parts, i.e., $\varepsilon_{s,t}^{CSp}$ and $\varepsilon_{s,t}^{CSn}$, respectively.

Second, the *time variation of the CS-shock effects.* We reveal that the effects in the 1980s differ from those in the 1990s, and together they differ from those in the 2000s. Therefore, we run our subsequent regressions separately for the three sub-periods, which correspond to specific phases of the credit cycle in the U.S. economy, and for which either the PSID or the CEX provide household-level data on defaults. These sub-periods are 1980–1989 (expansion), 1989–1996 (switching to contraction), and 2009–2017 (recovering from the Great Recession).

Third, the *sluggish response of household outcomes on CS-shocks.* The effects of a CS-shock may appear immediately or they may take several years to materialize. For example, the negative credit supply shock that occurred in the majority of the states in 1989 started to have a negative effect on household defaults only three years later, which could imply that indebted households may delay defaults by either selling (some of) their assets or using savings, if any, to repay debts, beyond their labor income. To account for these observations, we include the time lags of positive and negative CS-shocks from 0 (immediate effect) up to 4 years (sluggish effects). This roughly corresponds to a half-year of a typical business cycle and is in line with, e.g., the work by Schularick and Taylor (2012), which applies five-year lags of a credit growth variable to predict financial crises.

We formalize these ideas using the following panel logit regressions of household defaults:

$$\begin{aligned}\Pr\Big[Default_{i,j,s,t} = 1 \,|\, \boldsymbol{X}\Big] = \Lambda\Big(& \sum_{k=0}^{4} \theta_k^p \varepsilon_{s,t-k}^{CSp} + \sum_{k=0}^{4} \theta_k^n \varepsilon_{s,t-k}^{CSn} \\ & + \rho_1 Default_{i,s,t-1} + \rho_2 ADR_{s,t-1} \\ & + \Psi DEMOGRAPHY_{i,s,t} + \alpha_i + \beta_j + \gamma_s + \delta_t \Big)\end{aligned} \tag{7}$$

where i is a household working in industry j and living in a state s at year t. The household has a debt and at each period may either choose to continue paying off the debt or to default: the variable $Default_{i,s,t}$ equals zero in the first case and unity in the second. We model the probability of defaults conditional on $\mathbf{X}$, which encompasses all observable characteristics and various types of fixed effects up to moment t. The probability of default equals unity if the underlying latent process $\boldsymbol{X}\beta + v_{i,j,s,t} > 0$, where $v_{i,j,s,t}$ is the regression error. We assume logistic $\Lambda()$ distribution of $v_{i,s,t}$. Further, the focus explanatory variables are the lagged credit supply shocks, positive $\varepsilon_{s,t-k}^{CSp}$ and negative $\varepsilon_{s,t-k}^{CSn}$ $(k = 0, 1...4)$.

In the regression we also control for the following three groups of characteristics. First, we model households' heterogeneity by including household fixed effects (FEs) α_i. By doing so, we effectively allow households to be different in terms of their consumption–default trade-off and implied degrees of risk-aversion. We then control for household heads' job occupation FEs β_j, which helps us to account for differences between job occupations in terms of cyclicality and labor income which, in turn, may crucially affect a household's decision to default. We also account for unobserved differences across the U.S. states by including state-level FEs γ_s. Finally, since our CS-shocks are measured at the state-level, we want to be sure that they do not absorb other time-specific shocks that could hit all the states at the same time; therefore, it is very important for us to include time FEs and check whether our $\varepsilon_{s,t-k}^{CSp}$ and $\varepsilon_{s,t-k}^{CSn}$ variables have statistically significant effects *beyond* δ_t.

Second, we include a one year lagged dependent variable, $Default_{i,s,t-1}$, to account for possible inertia in a household's decision to default.[17] In addition, we follow Fay et al. (2002) and include the average default rate $ADR_{s,t-1}$ in state s in the previous year to test whether a household is more likely to file for bankruptcy if it lives in a state with greater default rates (negative "information cascades").

Third, we control for the demographic characteristics of households: variables in the $K \times 1$ vector $DEMOGRAPHY_{i,s,t}$ include the household head's sex, age, marital status, and race, as well as the size of the family. We also include a variable reflecting whether a household's head got a college education. Correspondingly, Ψ is the $1 \times K$ vector of unknown coefficients associated with respective demographic variables. In this respect we also closely follow previous research (Fay et al., 2002).

Using regression (7), our three empirical observations on the effects of CS-shocks on house-

[17]Though not directly linked, this approach mirrors that used in the empirical literature on predicting recessions within and across countries. When predicting U.S. recessions, Kauppi and Saikkonen (2008) document that including a lagged dependent variable in logistic regressions substantially improves predictability.

hold defaults discussed above are formalized as $\theta_k^p \neq \theta_k^n$ (asymmetry), $\theta_k^{(1980s)} \neq \theta_k^{(1990s)} \neq \theta_k^{(2000s)}$ (time variation), and $k = 0, 1...4$ years after the respective CS-shock (sluggishness).

Let us now discuss what regression (7) is able to capture and what it omits. Basically, it models an *average* effect of either positive or negative CS-shocks on household defaults. We expect that both positive and negative CS-shocks increase the probability of default in subsequent years: $\sum_{k=1}^{4} \theta_k^p > 0$ (recall that $\varepsilon_{s,t-k}^{CSp} > 0$ by construction) and $\sum_{k=1}^{4} \theta_k^n < 0$ (recall that $\varepsilon_{s,t-k}^{CSn} < 0$ by construction).[18] If this is so, it would mean that neither banks nor households are able to fully internalize the long-run consequences of their current decisions (to supply more credit and to accept it) and would be consistent with the "search for yield" phenomenon during the periods of low interest rates, discovered by Martinez-Miera and Repullo (2017).

However, this logic implies that the shocks always lead to deteriorated outcomes. If we, in turn, think more broadly about the *mechanisms* of CS-shocks' transmission to household defaults we may reveal a more complicated picture.

Consider a positive CS-shock and assume that a household accepts a bank's offer and thus increases its indebtedness. On the one hand, a greater debt-to-income ratio may lead to higher default probabilities in the future, as our baseline expectations suggest. On the other hand, if households use these debts to finance acquisitions of residential property (i.e., use mortgages to buy houses), local housing markets may start growing faster, promoted by increased demand. In this situation, the prices of collateral (houses) should also rise. This could be consistent with the findings of Favara and Imbs (2015) and Mian et al. (2020) that, in the 1980s and 1990s to 2000s, the states that deregulated their local credit markets earlier experienced a greater expansion of their local housing markets. Therefore, if the price of collateral increases, the likelihood of household default may decline. We thus obtain that the same positive CS-shock may raise default rates through a greater *debt-to-income* ratio and, at the same time, decrease default rates through increased *prices of collateral*. Which effect dominates? Regression (7) provides an answer to this question. But it is not suited to analyzing the strength of each channel of transmission. We therefore attempt to modify our empirical approach accordingly below.

Specifically, we turn to a two-stage estimation approach in which the first stage models the effects of positive and negative CS-shocks on either (i) the price of collateral, (ii) the mortgage-to-income ratio, (iii) total income, or (iv) employment status.[19] The second-stage then takes a prediction from the first stage based only on the shocks (i.e., omitting the impact of other control variables, except household FEs) and traces the impact further on the household's default decision. Such a two-stage approach mechanically resembles *IV-*

[18] We acknowledge that there are potentially contemporaneous effects θ_0^p and θ_0^n of the CS-shocks but our empirical results in the previous section have shown that they are not detected in the PSID or CEX data. If anything, we would expect that $\theta_0^p < 0$ and $\theta_0^n < 0$. The first inequality implies a greater ability of a household to repay existing debts, whereas the second inequality reflects a lower such ability.

[19] We add total income and employment status, since Mian et al. (2020) establish that early-= deregulated U.S. states experienced local economic booms, which pushed up labor income in specific job occupations (mainly in non-tradable sectors) and led to employment redistribution (from tradable to non-tradable sectors).

2SLS; however, as noted by Lopez-Salido et al. (2017) who applied a similar approach, it is not as strict as IV-2SLS because all we are interested in is the part of the variation of the first stage dependent variables that is explained by the shocks, and we do not need the exclusion restrictions to hold. The first three of the four dependent variables from the first stage are continuous variables and we thus run standard robust FE regressions in these cases, whereas the fourth dependent variable (employment status) is discrete and we run a logistic regression in that case. The full specification reads as:

$$\begin{aligned}\text{1st stage: } Y_{i,j,s,t} = &\sum_{k=0}^{4} \theta_k^p \varepsilon_{s,t-k}^{CSp} + \sum_{k=0}^{4} \theta_k^n \varepsilon_{s,t-k}^{CSn} \\ &+ \Psi DEMOGRAPHY_{i,s,t} + \alpha_{1,i} + \beta_{1,j} + \gamma_{1,s} + \delta_{1,t} \\ &+ \epsilon_{1,i,j,s,t}\end{aligned} \tag{8}$$

$$\begin{aligned}\text{2nd stage: } &\Pr\Big[Default_{i,j,s,t} = 1 \,|\, \boldsymbol{X}\Big] \\ &= \Lambda\Big(\xi \hat{Y}_{i,j,s,t} + \Psi DEMOGRAPHY_{i,s,t} + \alpha_{2,i} + \beta_{2,j} + \gamma_{2,s} + \delta_{2,t}\Big)\end{aligned} \tag{9}$$

where $Y_{i,j,s,t}$ is one of the four dependent variables at the first stage (in the case of employment status it is the underlying latent process with $\epsilon_{1,i,j,s,t}$ assumed to follow a logistic distribution). $\hat{Y}_{i,j,s,t} = \hat{\alpha}_{1,i} + \sum_{k=0}^{4} \hat{\theta}_k^p \varepsilon_{s,t-k}^{CSp} + \sum_{k=0}^{4} \hat{\theta}_k^n \varepsilon_{s,t-k}^{CSn}$ is the prediction of $Y_{i,j,s,t}$ being further transferred to the second stage instead of the sums of positive and negative shocks, as it was before. In the rest, all the notations remain.

4.2 Estimation results

4.2.1 Direct effects of CS shocks on household defaults

We begin by describing the estimation results on the direct effects of credit supply shocks on household defaults, as suggested by Equation (7). The results appear in Table 2 below. Each pair of columns — (1) and (2), (3) and (4), (5) and (6) — contains the key results from the equation estimated on the sub-period of 1980–1989 (credit expansion), 1989–1996 (contraction and recovery) and 2009–2017 (recovering after the Great Recession), respectively. We report each estimated coefficient reflecting a lagged impact of either positive or negative CS shock (lag $k = 0, 1...4$ years) and we compute the sum of the first to fourth year lags, which reflects a longer-run effect of the respective shock. Each estimated regression includes the full set of individual FEs, industry occupation FEs, state-level FEs, and time FEs, as well as the full set of demographic characteristics.[20] For the first two sub-periods, maximum likelihood failed to converge in the models with lagged dependent variables, and

[20]We omit all these to preserve space. The full estimation results are available from the authors upon request.

we thus were forced to remove these variables.

Table 2: Estimation results: the direct effects of CS shocks on household defaults

Sub-period:	1980-1989		1989-1996		2009-2017	
Positive / Negative CS shock:	$\varepsilon^{CS}_{s,t} > 0$	$\varepsilon^{CS}_{s,t} < 0$	$\varepsilon^{CS}_{s,t} > 0$	$\varepsilon^{CS}_{s,t} < 0$	$\varepsilon^{CS}_{s,t} > 0$	$\varepsilon^{CS}_{s,t} < 0$
	(1)	(2)	(3)	(4)	(5)	(6)
Lagged depvar	N/A		N/A		2.88***	
	(N/A)		(N/A)		(0.24)	
Lagged state frequency of depvar	–53.45*		–44.5**		–6.08**	
	(30.93)		(17.58)		(2.56)	
Positive vs. negative credit supply shocks						
Lag = 0 year	1.25	0.14	0.40	0.37	–0.36	0.59
	(0.87)	(1.05)	(0.61)	(0.47)	(0.57)	(0.49)
Lag = 1 year	–1.01	–0.37	0.30	–0.05		
	(1.09)	(0.98)	(0.48)	(0.54)		
Lag = 2 year	–0.64	–0.30	0.66	–0.98*	1.44**	–0.95**
	(0.89)	(0.75)	(0.50)	(0.51)	(0.65)	(0.38)
Lag = 3 year	–0.58	0.30	1.21*	–1.18***		
	(0.84)	(0.82)	(0.66)	(0.45)		
Lag = 4 year	–0.43	–0.84	1.32*	–0.15	–0.25	–0.70**
	(0.61)	(0.74)	(0.77)	(0.62)	(0.41)	(0.34)
Sum of 1–4 lags	**–2.66**	**–1.22**	**3.49*** **	**–2.36***	**1.18**	**–1.65** **
	(2.26)	**(2.18)**	**(1.19)**	**(1.25)**	**(0.83)**	**(0.68)**
Demography controls	Yes		Yes		Yes	
Household, job, state & Year FEs	Yes		Yes		Yes	
No. obs.	9,133		15,714		5,396	
No. households	1,911		2,822		2,251	
log Likelihood	–255.5		–465.7		–771.7	

Note: The table reports panel logit estimates of the direct CS effects on household defaults, as implied by equation (7).

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

The estimation results are very much consistent with our empirical findings in Section 3 above. Despite we now turn from considering specific years of systemic CS shocks (1984, 1989, 2004, 2009) to considering full sub-periods during which some states could experience positive CS shocks while the others experience negative CS shocks, we still conclude that in the 1980s neither positive nor negative CS shocks affected household defaults, whereas in the subsequent two sub-periods, i.e., the 1990s and 2010s, the U.S. states could face asymmetric and statistically significant effects of the CS shocks.

Specifically, during the 1989–1996 period, we find that the third and fourth year lags of positive CS shocks significantly increase the likelihood of defaults, with the sum of the first to fourth lags also estimated as positive and significant (at 1%). This confirms both the sluggishness of the effects and that the positive CS shocks may lead to an accumulation of economic imbalances for both households and banks, which, in turn, result in greater defaults for the former, and deteriorated quality of assets for the latter. Negative CS shocks, from the other side, begin to raise the likelihood of defaults from the second year after the shocks and continue in the third year. The sum of the first to fourth lags of negative CS shocks is also negative and significant (at 10%). This again supports sluggishness and points to a worsened ability of (some) households to re-finance previous debts (taken, e.g., at higher interest rates) by new debts (which could otherwise be taken at lower interest rates) at a horizon of four years after negative CS shocks. To evaluate the implied economic effects of the shocks, we compute the product of the cumulated marginal effect after logit and one sub-period's standard deviation of respectively positive or negative CS shock (0.33 vs. 0.36). We obtain that a one standard deviation increase of positive CS shock leads to a 10.6 percentage points rise in default probability in the following four years. Analogously, a one standard deviation increase of negative CS shock (in absolute terms) causes the probability of default to rise by 8.3 percentage points. The economic effects are thus comparable and rather large: they exceed the default probability's one standard deviation (in the respective sub-period) by 3.4 and 1.1 percentage points.

Regarding the third sub-period, 2009–2017, we achieve very similar results, despite having to turn to a different dependent variable (whether a household has mortgage delinquency of at least one month) and the PSID data changing its frequency (from annual to biennial). We find that a positive CS shock significantly (at 5%) increases the probability that a household delays its mortgage payment in the subsequent two years. However, the fourth year lag appears insignificant and negative, differing from our findings for the 1989–1996 sub-period. The sum of the second and fourth lags is insignificant, and we thus treat the result with some degree of caution, though we still believe the result is supportive of the general idea that positive CS shocks favor accumulation of economic imbalances.[21] We also find that the effects from the second and fourth year lags of negative CS shocks are both negative and significant (at 5%) and so is their sum. This again indicates that the deeper the negative CS shocks the higher the likelihood of mortgage delinquency in the subsequent four years. In terms of economic effects, we estimate that a one standard deviation of positive CS shock on the respective sub-period (0.35) leads to a 6.3 percentage points increase in the probability of mortgage delinquency in the subsequent four years. A one standard deviation of negative CS shock on the same sub-period (0.67) leads to a 8.6 percentage points rise of the probability of mortgage delinquency over the same horizon in the future. As in the case of the previous sub-period, we conclude that both economic effects are large, even though they are now somewhat lower than a standard deviation of empirical probability of mortgage delinquency (22 percentage points).

[21] Possibly, the insignificant sum of positive CS shocks stems from the fact that the sub-period contains many more episodes of negative than positive shocks, since it covers the Great Recession and the subsequent slow recovery (Kydland and Zarazaga, 2013; Schmitt-Grohe and Uribe, 2017; Gertler et al., 2020).

4.3 Testing for the mechanisms: The effects of CS shocks on household defaults through employment, leverage, collateral, and income channels

Having established that both positive and negative credit supply shocks *on average* increase household default (during the 1980s–1990s) and households' mortgage delinquencies (2009–2017) over a four-year horizon, we now turn to analyzing the mechanisms of such effects, as implied by the two-stage approach formalized in Equations (8)–(9). We test four such mechanisms: the price of collateral, mortgage-to-income, total income, and employment (either in the tradable or nontradable sectors). We report a snapshot with the most important part of the the full estimation results in Table 3 below. The table reports panel-robust FE estimates (in Panels 1–3) or panel-robust logit estimates (in Panels 4–5) of the effects of CS shocks at the first stage, as implied by Equation (8), and panel robust logit estimates at the second stage, as implied by Equation (9) (except Panel 1 for 1980–1989 and 1989–1996, in which the respective maximum likelihood failed to converge, forcing us to apply a linear probability model). In the first stage, we report the sum of coefficients on lags k from 1 to 4 of either positive or negative CS shock. Each regression includes the full set of individual FEs, industry occupation FEs, state-level FEs, and time FEs, as well as the full set of demographic characteristics. We omit all these to preserve space. The full estimation results are available from the authors upon request. Each pair of columns, as in the previous section, — (1) and (2), (3) and (4), and (5) and (6) — contains the estimation results for one of the three sub-periods.

Several outcomes emerge from the estimation results. *Channel 1: the price of collateral.* Our estimates from the first stage indicate that positive CS shocks cause real house values to rise over the four subsequent years, which is very much in line with the findings of Favara and Imbs (2015) and Mian et al. (2020). We find significant (at 1%) effects across all three sub-periods. However, we also discover that negative CS shocks are also in play: they lead to a decline of real house values, and the scope of the decline may be comparable, in absolute terms, to the scope of the effects from positive CS shocks (in the 1990s and 2010s), or even larger than those (in the 1980s), thus implying a certain degree of asymmetry.

Consider first the sub-period of 1980–1989: a one standard deviation of positive CS shock increases real house values at the household-level by 9.4 percentage points in the subsequent four years, while a one standard deviation of negative CS shock raises the prices by as much as 14.7 percentage points over the same horizon, which is 1.5 times larger than the effect of the positive shock. Both effects are economically significant, being roughly equaled $\frac{1}{3}$ and $\frac{1}{5}$ of the standard deviation of the annual growth rates of real house value (the standard deviation equals 39 percentage points; for comparison, the mean annual growth rate equals 1.1%). Further, the estimated effect in the second stage is negative, as expected, but statistically insignificant.[22]

[22]Possibly, the lack of the effect's significance in the 1980s could be explained by the fact that mortgages were a much less developed tool compared to what followed from the 1990s right up to the Great Recession.

Table 3: Estimation results: the mechanisms of CS shocks effects on household defaults

Sub-period:	1980-1989		1989-1996		2009-2017	
Positive / Negative CS shock:	$\varepsilon^{CS}_{s,t} > 0$	$\varepsilon^{CS}_{s,t} < 0$	$\varepsilon^{CS}_{s,t} > 0$	$\varepsilon^{CS}_{s,t} < 0$	$\varepsilon^{CS}_{s,t} > 0$	$\varepsilon^{CS}_{s,t} < 0$
	(1)	(2)	(3)	(4)	(5)	(6)
Panel 1: Transmission through the price of collateral						
1st stage	0.23***	0.46***	0.10***	0.09***	0.08***	0.04***
	(0.04)	(0.06)	(0.03)	(0.03)	(0.02)	(0.01)
2nd stage	–0.01		–0.06**		–0.45**	
	(0.01)		(0.03)		(0.21)	
Panel 2: Transmission through total income						
1st stage	0.10**	0.18***	0.01	0.01	0.02	0.02*
	(0.04)	(0.06)	(0.05)	(0.05)	(0.02)	(0.01)
2nd stage	–0.37***		–0.21		–1.33***	
	(0.14)		(0.17)		(0.21)	
Panel 3: Transmission through mortgage-to-income						
1st stage	–0.03	0.15*	–0.06	–0.10	–0.04	–0.02
	(0.058)	(0.082)	(0.095)	(0.091)	(0.05)	(0.02)
2nd stage	0.85**		1.11***		–0.01	
	(0.35)		(0.27)		(0.01)	
Panel 4: Transmission through employment						
1st stage	0.37	0.46***	1.70	–0.38	0.25	0.04
	(1.06)	(0.06)	(0.03)	(0.97)	(0.20)	(0.09)
2nd stage	–7.30***		–3.22		1.59	
	(2.50)		(3.76)		(3.38)	
Panel 5: Transmission through employment in tradables						
1st stage	–0.19	–0.01	–0.16	0.04	–0.06	0.20*
	(0.28)	(0.30)	(0.29)	(0.24)	(0.35)	(0.12)
2nd stage	–0.86		0.03		0.17	
	(0.78)		(0.66)		(0.46)	
One standard deviation of a shock	0.41	0.32	0.33	0.36	0.35	0.67

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

Let us now turn to the sub-period of 1989–1996. For this sub-period, we again find highly statistically significant effects of both positive and negative CS shocks on real house values in the first stage, as for the previous sub-period. However, from then on, we also find that the second stage coefficient is also significant (at 5%), meaning that CS shocks start affecting household default rates through changes in real house values. The second stage effect is negative, which implies that positive CS shocks *decrease*, not increase (as on average, see the previous section) the default probabilities; correspondingly, negative CS shocks increase

For example, a legal act which permitted local mortgage creditors to grant mortgage credits at an adjustable rate was enacted by Congress only in 1982 (the so-called "*Alternative Mortgage Transactions Parity Act*", AMTPA) while the ratio of mortgage to income was only 0.96 (the mean value during 1980–1989 according to our sample of households in the PSID.

these probabilities. Nonetheless, we find that the potential of this channel is rather limited: a one standard deviation of positive (negative) CS shock raises (lowers) real house values by only 3-3.5 percentage points, which in turn decreases (increases) the probability of household default by no more than 0.2 percentage points. The latter does not seem to be much *per se*, given that the mean household default rate equals 0.5%, i.e., is greater by a factor of 2.5.

Finally, the third sub-period, 2009–2017, delivers qualitatively the same results; quantitatively, though, the results are even more modest. Despite the effects in the first stage being significant once again, positive CS shocks raise real house values while negative CS shocks decrease them — and the effect in the second stage is again negative and significant, the underlying economic effect is much lower than we find for 1989–1996. Of course, we cannot directly (or quantitatively) compare the two sub-periods because the dependent variables differ. Nonetheless, we reveal that an impulse equaled a one standard deviation of positive CS shock computed over 2009-2017 (0.35) leads to a 2.8% increase in real house values,[23] and through that the probability of mortgage delinquency decreases by only 0.1 percentage point. A very similar quantitative effect, i.e., –0.1 percentage points, is obtained for the negative CS shock. Comparing the estimated effects (in absolute terms) with the mean mortgage delinquency ratio, which equals 5% over the 2009–2017 sub-period, we again conclude that the efficacy of the channel of collateral prices is modest at best.

Overall, among the four channels analyzed, we find that the price of collateral is statistically significant within all three sub-periods considered but, at the same time, the magnitude of underlying economic effects on the household decision to default remains very limited. Importantly, this result is consistent with that obtained above using the difference-in-differences approach (see Sections 3.3 and 3.3).

Channel 2: real total income. The estimates indicate that CS shocks significantly affect real total income in the first stage during the earliest sub-period only, i.e., 1980–1989, whereas during the subsequent two sub-periods such effects diminish. This is again very much consistent with what we find above in our difference-in-differences exercise.

Interestingly, for the 1980-1989 sub-period we reveal strong asymmetry: a one standard deviation of positive CS shock raises total income by only 4.1% in the subsequent four years (significant at 5%), whereas a one standard deviation of negative CS shock declines total income by 14.7% (significant at 1%). In other words, total income is disproportionately more sensitive to an unexpected decline of the supply of bank credit than to an unexpected rise in the supply. In the second stage then we find that such the impulses to total income lead to changes of the default probabilities by –1.5 and +2.1 percentage points, respectively. Given the average default rate of 0.4% in the sub-period, we conclude that these economic effects are both powerful. We note that without division of CS shocks into positive and negative we would not be able to achieve such results.

Over the subsequent 1989–1996 sub-period no such strong effects are revealed, although for the 2009–2017 sub-period we find that a one standard deviation of negative CS shock

[23]For comparative reasons, it equals approximately two thirds of the real house value mean.

decreases total income by 1.3 percentage points and through that raises the probability of mortgage delinquencies by 1.8 percentage points. Given the mean value of mortgage delinquencies of 5%, we conclude the economic effect is large, though not as strong as in the 1980s.

Overall, as in the case with collateral prices, we again conclude that positive CS shocks are not always bad: if they lead to an increase of household income, the default probabilities decline, and vice versa. However, the efficacy of the total income channel was high in 1980–1989 but then the channel either diminished fully (in 1989–1996) or started working only through negative CS shocks (in 2009–2017) exhibiting less potential than in the 1980s.

Channel 3: mortgage-to-income. Across the three sub-periods we obtain a significant effect in the first stage only in the 1980–1989 horizon and only for negative CS shocks. Specifically, a one standard deviation of negative CS shock raises the mortgage-to-income ratio by 4.8 percentage points (note that the mean value of the ratio equals 0.96) and through that increases the default probabilities by 4.1 percentage points. The effect thus exceeds the mean default ratio by a factor of 10, and we therefore conclude that the channel was very potent. Note that at first sight the result is counter-intuitive because one would expect negative CS shocks to lower, rather than raise, the mortgage-to-income ratio. However, the result is likely to be driven by the fact that the negative CS shock in the 1980s lowered total income, i.e., the denominator of the mortgage-to-income ratio, by more than it lowered mortgage debts, i.e., the numerator, on average. Hence, the effect is correct. Interestingly, we obtain a very similar result in our difference-in-differences analysis above when we estimate the effect of the negative systemic CS shock in 1989 on the the household-level outcomes over the 1989–1996 sub-period.

Channel 4: employment. As in the case of real total income, we reveal significant effects of CS shocks on employment only in the 1980–1989 and 2009–2017 sub-periods, and only when negative CS shocks are in place. Specifically, in the first sub-period, a one standard deviation of the negative CS shock was able to decline the probability of a household's head being employed by 3.7 percentage points (note the mean probability of employment equals 91%) and through that raise the probability of default on debts by as much as 26.7 percentage points. This is the largest economic effect we find in this section. Further analysis shows that we are not able to attribute this negative effect to decreasing employment neither in the tradable- nor non-tradable sectors. Over the third sub-period we find that a one standard deviation of negative CS shock declines the probability of being employed in only tradable sectors, and the effects equals –3.3 percentage points, which is quantitatively very close to what we obtained earlier for the first sub-period. However, the effect in the second stage is insignificant, meaning that those households whose heads lost their jobs in tradable sectors after these sectors experienced a decline of bank credit supply in 2009–2017 were unlikely to delay their mortgage payments.

Concluding this section, we argue that the three sub-periods considered are very much heterogeneous in terms of the channels through which CS shocks affected household defaults or mortgage delinquencies rates. During the 1990s the most potent channels were (i) whether a household's head loses his or her employment status (the most powerful), (ii) increasing household indebtedness, and (iii) changes in total income. For the subsequent 1989–1996 sub-period, the only potent channel was that of collateral prices, but the underlying economic effect was, though sizeable, not as large as either of the three channels in 1980s. Finally, for the recent sub-period of 2009–2017 we find that two channels were at work: (i) decreasing total income after negative CS shocks and (ii) changes in the price of collateral, with the underlying economic effects of income being comparable with those in the 1980s and the underlying effect of collateral appearing even lower than in the 1990s. Our analysis, therefore, provides an answer to the question why negative CS shocks increase the probability of household defaults. But not to the question of why positive CS shocks do the same. Possibly, for the latter one should slightly modify the first stage variables; in particular, switch from the price of collateral to the volatility of such prices and from total income to the volatility of total income and associated uncertainty (it could be much harder for a household to adapt to volatility than to a decreasing level of, say, income or collateral *per se*). We leave this for future research.

5 Sensitivity analysis

5.1 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) (GM2017) approach with Minnesota priors, to the three available alternatives. The first is the same GM2017 scheme of 5 sign restrictions, but with Minnesota priors replaced by flat priors. One could argue that the Bayesian approach to estimating VAR models, which we use to achieve 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 critique. We re-run our VAR model on the panel of 51 states, assuming flat priors instead of Minnesota. We then plot the time evolution of the new state-level CS shocks and compare it with the baseline, see Fig. B.III.a,c in Appendix B. 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 and systemic negative CS shocks of 1989 and 2009 are still there.

One more possible objection towards our baseline results could be that GM2017 assume too strong reaction of monetary authorities to CS shocks. Following Eickmeier and Ng (2015) (EN2015), we thus remove this assumption and again re-run our panel VAR estimates, both under Minnesota and then flat priors. The results appear at the right panel of the same figure, see Fig. B.III.b,d in Appendix B. We observe in both cases that the median CS shock estimates across years are very similar to those obtained with the GM2017 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 EN2015 and GM2017 are rather close, but the former brings more uncertainty regarding the key periods while the latter brings much less uncertainty.

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 and negative CS shocks. The baseline results survive in each case.[24]

5.2 Different estimation approach

CS shocks are fairly exogenous in 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 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. E.I and E.II for the 1984 and 2004 positive CS shocks and in Figs. E.III and E.IV for the 1989 and 2009 negative CS shocks, respectively (see Appendix E). 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.

5.3 Different level of data aggregation

In the baseline estimations we work with the data at the household level, except 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 a greater comparability with the reference paper but we also reduce the statistical power of our estimates due to a substantial reduction of 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 Figs. F.I–F.IV (see Appendix F). We find that the results are preserved for some episodes but blurred for the 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.

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

However, for the other three episodes we find fewer significant reactions, which is surprising. The exceptions are (i) the case of the 2009 negative CS shock, for which we still observe a negative effect on the ownership rate; (ii) the 2004 positive CS shock, for which we still find a rising reaction of mortgage-to-income, and (iii) the 1989 negative CS shock, for which the estimates suggest a rising effect for mortgage-to-income ratios, declining real house values and greater default rates, though marginally significant.

Overall, we conclude that the state-level results are in line with the household-level results described in Sections 3.3 and 3.4, though they exhibit somewhat lower statistical power.

5.4 Different measures of household defaults

In the baseline estimations our dependent variables are the binary indicator of whether a household defaulted, which is available in the PSID database up to 1996, and the binary indicator of whether a household delayed its mortgage payment on at least one month, which is available from 2009 onwards. For the former sub-period the PSID data provides no alternatives for the default indicator; however, for the latter sub-period there are at least three options. These options are (i) the binary indicator of whether a household delayed its mortgage payment for three months or more, (ii) the binary indicator of whether a household opted to restructure its mortgage(s), and (iii) the number of months during which a household was not able to repay its mortgage(s).

We thus re-run our baseline panel robust logit regressions for the sub-period of 2009–2017. The results appear in Table G.I (see Appendix G). As can be inferred from the table, our baseline measure, i.e., one-month mortgage delinquencies, allows us to better quantify the effects of both positive and negative CS shocks, see columns (1) and (2).

When we switch to longer delinquencies, see columns (3) and (4), we still obtain a very similar, and quantitatively stronger, effect of negative CS shocks, but we no longer observe any significant effects of positive CS shocks. This possibly implies that even during periods of positive CS shocks banks were not ready to re-finance lower quality debts if the quality deteriorated for more than one month. In addition, it could also imply that positive CS shocks are not responsible for an accumulation of long-lasting financial problems (bad mortgage debts) from the liability side of household balance sheets.

Finally, we show that the third and fourth options for the dependent variable, i.e., restructurings and number of months with mortgage delinquencies, are not sensitive to either positive or negative CS shocks; see the pairs of columns (5)–(6) and (7)–(8), respectively. This in turn indicates that households may request a mortgage restructuring not because they took too much credit some years ago on a wave of positive CS shock in the local credit market, but because they have currently lost (a part of) their income and are not able to repay their debts. In addition, this result implies that a household may prefer to just delay the payment for some time than to ask its lender for a full mortgage restructuring.

Overall, this exercise shows that the results exhibit a certain degree of sensitivity to the choice of dependent variable. However, we still suggest that our baseline results are informative because we show that both positive and negative CS shocks increase the likelihood that households with mortgage(s) will experience delinquencies, for either short (one-month) or longer (more than three months) period.

6 Concluding remarks

We estimate time evolution of credit supply (CS) shocks in the 51 U.S. states over the last four decades and document that the positive and negative systemic CS shocks of 1984, 1989, 2004, and 2009 had asymmetric, sluggish, and time-varying effects on household defaults and mortgage delinquencies, household employment and total income, household indebtedness and prices of collateral, and home ownership status.

Our empirical results open an avenue for future research linking bank credit and household balance sheets. In particular, there is a need to theoretically rationalize that households may be ready to accept bank offers of additional (mortgage) credit during the credit cycle expansionary phase, while being unable to fully internalize the long-run consequences of such (too optimistic) decisions and why this leads to greater household defaults. Theoretical approaches of households' optimal default decisions developed by Chatterjee et al. (2007), Livshits et al. (2010), Mitman (2016), and Antunes et al. (2019) seem promising in this direction. In addition, the Bordalo et al. (2018) framework of diagnostic expectations could also be useful since it implies that economic agents may over-react to news by exhibiting over-optimism (-pessimism) in the case of positive (negative) news, which might, according to our results, be related to positive (negative) CS shocks.

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Abstrakt

Jsou nestability hypotečního trhu důsledkem finanční nerovnováhy naakumulované v minulosti? V článku zkoumáme efekt pozitivních a negativních nabídkových šoků na trhu s úvěry na následnou neschopnost domácností splácet dluh za období posledních čtyř dekád ve státech USA. Používáme znaménková omezení ve VAR modelu k izolování nabídkových šoků na státní úrovni a identifikujeme, že 1984 a 2004 byla léta systematických celofederálních pozitivních nabídkových šoků, zatímco léta 1989 a 2009 přinesla negativní šoky. Dále s využitím difference-in-difference odhadu zjišťujeme, že oba pozitivní a negativní šoky na trhu úvěrů vedly k vyššímu výskytu neschopnosti domácností splácet dluhy v následném období, pokud zároveň zvýšily i poměr výše hypoték vůči příjmům. Ukazujeme, že poptávkové šoky vedou k (i) posunu zaměstnanosti mezi obchodními a neobchodními sektory a (ii) změnám v příjmu domácností a (iii) prostřednictvím cen domů usnadňují akumulaci rizika neschopnosti splácet. Naše výsledky naznačují, že pozitivní nabídkový šok v roce 1984 nezvýšil u domácností výskyt neschopnosti splácet více ve státech, které byly vystaveny silnějšímu šoku, než ve státech, které byly vystaveny menšímu šoku. To je dáno skutečností, že šok zvýšil budoucí příjmy i zadlužení, ale neovlivnil poměr mezi příjmy a zadlužením. Naproti tomu v letech 1989, 2004 a 2009 zvýšily poptávkové šoky poměr zadlužení vůči příjmům v následném období a tím zvýšily výskyt neschopnosti splácet. Tyto výsledky nabízejí další empirické důkazy pro teorii endogenních cyklů na úvěrovém trhu.

Klíčová slova: finance domácností, bankovnictví, nabídka úvěrů, finanční nestabilita, hypotéka, difference-in-difference, VAR modely, USA, PSID, CEX.

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