FACULTY OF ECONOMICS AND
BUSINESS ADMINISTRATION

HOUSEHOLD DEBT, HOUSING CYCLES, AND INTERACTIONS WITH MONETARY POLICY

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A dissertation submitted to Ghent University in partial fulfilment of the requirements for the degree of Doctor of Economics

Academic year: 2018 – 2019

GHENT
UNIVERSITY

DOCTORAL JURY

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Prof. Dr. Sarah Zubairy	Texas A&M University

Acknowledgements

I am relieved and happy for reaching the end of this journey; a roller coaster of emotions and new experiences. The PhD is not just about racking your brains to come up with innovative research ideas and finding ways to implement them; you also go through a 'get-to-know-yourself' phase. The PhD tests your drive, motivation, resilience, emotional strength, and health to the limit. Many times I would ask myself how much I wanted this. After all is said and done, a lot, it seems. Looking back, I believe the PhD has contributed substantially to shape my way of thinking and to make me a better economist and researcher.

The idea of doing a PhD started to gain traction in the Summer of 2014. I had a relatively stable and comfortable life. I enjoyed what I did professionally for roughly nine years, but it was not enough. I wanted to develop my research ideas that were hidden away in a neglected shelf in my office. I also wanted to upgrade my skills and move up to a new research level. A full-time PhD was the answer.

I thought I knew what I was getting myself into. Classic beginner's mistake: I did not. I liked the idea of carrying out research with independence and freedom, but there were times when I felt lost and unable to move forward. In addition, the pressure to publish was always lingering in the shadows. But the most challenging part was, by far, the period surrounding the Economics Job Market. Although it was thrilling and exciting, the threat of failure was always looming in the background; it was like being alone in the open ocean without any life jacket, just swimming for the sake of staying afloat. At times I had no clue what I was doing, or if it would work. I am 1.88m tall, but I felt very small.

Fortunately, many good things have come my way during the PhD. I could devote most of my time to research, which was a first for me. It was a great feeling every time my ideas led to findings with important policy implications. I enjoyed presenting my work at conferences, where I interacted with many researchers, from PhD students to renowned scholars. One of the highlights was when I went three months to the Norges Bank to work on a project that would later become my job market paper. It was a period of intense work, and it also reminded me that central banking is where I belong. I enjoyed a lot brainstorming with the economists there, in particular with my co-authors, who always treated me as one of them. It was a decisive turning point to put me back on the right track to finish the PhD.

I would like to thank Gert Peersman for giving me the time to carry out my research projects, and for the opportunity to present my work at several conferences, whether it was in Belgium or Japan. I am also grateful to the PhD committee, Andreas Fuster, Frank Smets, Gerdie Everaert, Sarah Zubairy, and Selien de Schryder, for having devoted their precious time to read my papers and for the insightful and thought-provoking comments.

I want to take a moment to thank those who have made this journey memorable. I thank Marco Bernardini for his friendship, and for our endless talks about Economics and football. I thank Sebastian Ruth for always challenging me to set my bar higher, both at the gym and in research. I thank my office mates Marco Onofri and Frédéric Opitz for their friendship, support and positive energy in the office. I thank Pooya Mongabadi for his friendship, his incredibly refined humour, and for upgrading considerably my social life. I thank my friend Rui Sá Pereira for always being there for me, and for helping me understand that there is so much more to life than just work. I also thank my co-authors and friends, Georgi Krustev, Knut Are Aastveit and André Anundsen, from whom I have learned a great deal, and who understand the power of taking the time to enjoy the small victories.

I thank my parents, sisters, my niece Diana, my nephews Leonardo and Luca, and my brothers-in-law, for their support and love, and for having forgiven me repeatedly for my absences. Although I have been living abroad for already seven years, the *saudade* I feel for not being with them does not diminish with time.

The reason that I finished the PhD is because my partner did not let me fail. She kept me sane over the last years, particularly during the job market period. She saw me at my worse, was incredibly understanding about my never-ending working hours, the lack of holidays, the absences, and the failed promises. She has been my pillar of strength, always giving me positive energy and encouragement. She is the reason I am here writing these lines. She is the reason I am proud of what I have accomplished, and of who I have become. Thank you, Carolien, for all you have given me, for making me feel special every day, for believing in me even when I could not, and for reminding me of who I really am.

Bruno Albuquerque

June 2019

Contents

List of Figures

List of Tables

Introduction

> *"The cycle of manias and panics results from pro-cyclical changes in the supply of credit(...) Money always seems free in manias(...) Real estate bubbles always are a credit phenomenon."*
>
> – Charles P. Kindleberger, *Manias, Panics, and Crashes*, 1978

> *"Economic disasters are almost always preceded by a large increase in household debt. In fact, the correlation is so robust that it is as close to an empirical law as it gets in macro-economics."*
>
> – Atif Mian and Amir Sufi, *House of Debt*, 2014

In his influential 1978 book, *'Manias, Panics, and Crashes: A History of Financial Crises'*, Charles P. Kindleberger argues that financial crises tend to be preceded by a wave of credit bubbles, when indebtedness of a group of borrowers increases at a much faster pace than income for a few years (Kindleberger, 1978). The increase in borrowing during these periods usually does not fund new capital investment, but is rather channelled to the purchase of existing assets, such as real estate, both residential and commercial properties, and also of stocks and bonds. What follows typically is a strong increase in asset prices (equities, commodities, and house prices), which leads to increases in the wealth of the private sector. This period of rapid increases in indebtedness and asset prices is associated with euphoria, i.e. waves of optimism about future economic prospects, giving rise to high growth rates of spending and investment.

But the 'day of reckoning' eventually comes, and the bubble bursts, as debt cannot grow more rapidly than income for an extended period of time; borrowers start to find it increasingly difficult to pay their loans, paving the way to an abrupt adjustment in spending, resulting in large declines in asset prices. In Kindleberger's words, expansions in the supply of credit result in manias and panics, which in turn sow the seeds of damaging and costly financial crises. A credit supply expansion is when lenders increase the quantity of credit or decrease the interest rate on credit for reasons unrelated to changes in income or productivity of the borrowers. Recent work by Mian and Sufi (2018) focuses on a similar mechanism, which they call *'credit-driven household demand channel'*, in which expansions in credit supply that operate mainly through household demand lead to severe macroeconomic adjustments when credit supply ultimately contracts.

The mechanism of financial crises described in Kindleberger (1978) has been present across many parts of the world over the past four centuries, starting with the Dutch tulip bulb bubble in the 17$^{\text{th}}$ century, and ending with the 2000's bubble in credit and in real estate markets in the United States and in many other advanced economies. What is remarkable is that Kindleberger's ideas have survived the test of time, if we bear in mind that the first edition of his book was published more than forty years ago. Since then, several financial bubbles have emerged (surveyed in the seventh edition of his book in 2015): the bubble in real estate and stocks in Japan, Finland, Norway, and Sweden (1985-89), and in several Asian countries (1992-97), the surge in foreign investment in Mexico (1990-99), the bubble in US stocks (1995-2000), and the 2000's bubble in credit and real estate in many advanced economies, which led to the 2007-09 Great Recession. These crises are arguably different in nature, yet they share the same pattern of Kindleberger's original ideas on credit bubbles: excessive credit-driven economic expansions lead eventually to severe financial crises that may last for several years into the future.

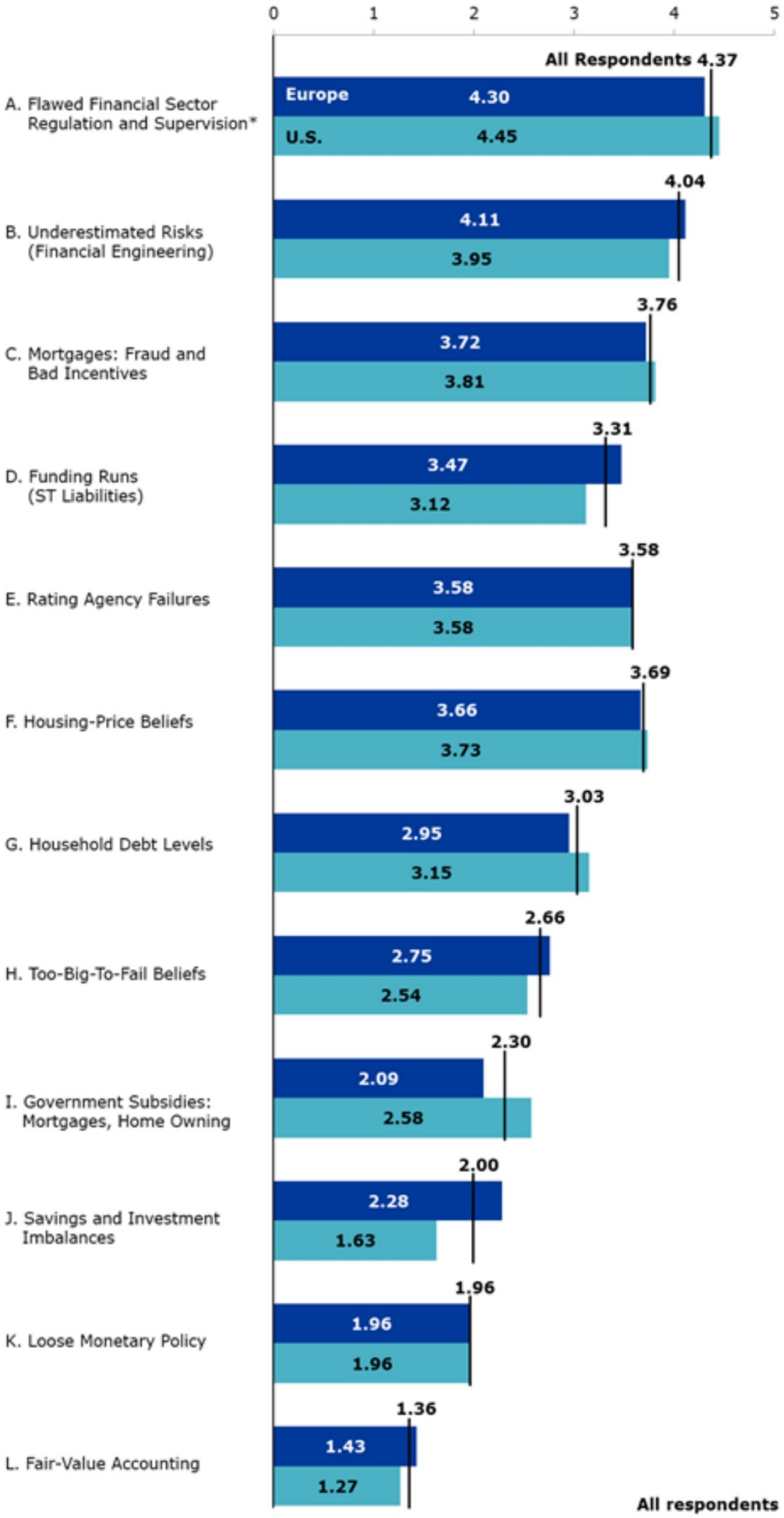
Although we had Kindlberger's early insights to learn from, we were not able to anticipate the devastating effects of the rapid rise in private debt and asset prices, predominantly in household debt and house prices, during the first half of the 2000s. The unprecedented economic downturn that followed the 2007-09 Great Recession has had negative effects that have spanned several dimensions of the world economy, only comparable to the Great Depression of the early 30's. The Great Recession has therefore cast a persistently long shadow on the world economy, with its negative effects still visible in our day-to-day lives, taking the form of a rather sluggish economic recovery translating into stagnant income growth.

Ten years have passed since the onset of the crisis, but we still have not reached a broad consensus on the root causes of the last recession, let alone on how to fix it. Let me give two concrete examples to illustrate that both policy makers and economists alike are still debating the causes of the Great Recession. First, in a review of 21 books on the financial crisis by economists, journalists, and a former US Treasury Secretary, Andrew Lo finds that there is significant disagreement as to what the underlying causes of the crisis were and even less agreement as to what to do about it (Lo, 2012). The silver lining is that there have been significant advances since his study came out, particularly related to the strengthening of the macro prudential frameworks in several countries (Edge and Liang, 2019). These new regulatory and supervisory frameworks aimed at containing risks in the financial systems may help minimise the negative effects of a new crisis. Nevertheless, the lack of consensus among academics has continued in recent years (for different views on the most recent crisis see, for instance, Adelino et al., 2016; Adelino et al., 2018; Albanesi et al., 2017; Cox and Ludvigson, 2018; Gertler and Gilchrist, 2018; Foote et al., 2016; Kotlikoff, 2018).

Second, a survey carried out in October 2017 shows that a panel of leading academic economists believes that several factors have contributed to the Great Recession (Figure 1). Like Aikman et al. (2019b), I group them in two main categories: (i) the fragilities in the financial system associated with excessive leverage and use of short-term funding; and (ii) the unprecedented household debt boom of the early-2000s. The latter topic on household debt touches upon several items listed in Figure 1, namely the bad incentives, fraud, or both in mortgage issuance and securitisation (item C), inflated beliefs about housing prices (item F), elevated levels of US household debt as of 2007 (item G), and government involvement in subsidising mortgages, homeownership, or both (item I). Even if we cannot pin down precisely the main causes of the Great Recession, it seems clear that we need to understand better the role of debt and housing for macroeconomic fluctuations.

Against this background, this dissertation follows in Kindleberger's footsteps in bringing into the fore the fact that household debt, and housing cycles, matter for business cycles fluctuations. It is also highly influenced by Atif Mian's and Amir Sufi's book cited at the beginning of this introduction, *'House of Debt: How They (and You) Caused the Great Recession, and How We Can Prevent It from Happening Again'*, who has revived the concept of household debt build-ups being detrimental to future economic growth (Mian and Sufi, 2014). In particular, this dissertation focuses on household debt and housing cycles in the United States, and their interaction with the real economy and monetary policy. I focus on

Figure 1: Factors Contributing to the 2008 Global Financial Crisis



Source: Chicago Booth Initiative on Global Markets Economic Experts Panel.
Notes: Panel experts rated the importance from none (0) to highest (5) that each item had in contributing to the 2008 global financial crisis.

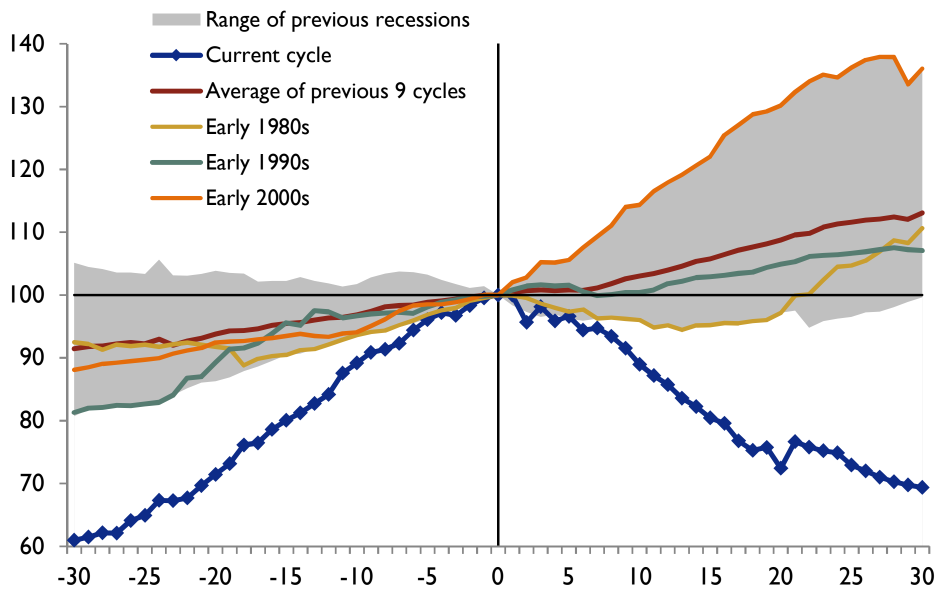
debt in the household sector as the strong rise in private debt in several Western countries in the second half of the 20th century has been driven mainly by credit to households, particularly mortgage debt (Jordà et al., 2015). Apart from the academic interest of the questions I address here, my research also has relevant policy implications which I will discuss in more detail in the overview of the chapters below. Understanding the root of the Great Recession may help design policies to prevent the repetition of such a severe crisis. My results also intend to help better understand the role of debt and housing for macroeconomic fluctuations and financial stability, more generally. Finally, my dissertation sheds light on the importance of household debt to the effectiveness of monetary policy, particularly by stressing the substantial regional heterogeneity within the United States.

Household debt cycles

The balance sheet adjustment in the US household sector has been a prominent feature of the most recent recession and subsequent recovery. The beginning of the US economic downturn in late-2007 coincided with the start of a prolonged reduction in household debt relative to income, which has no parallel in US economic history over the past 70 years (Figure 2). Credit demand is typically procyclical; less appetite for credit during bad times, but more debt accumulation when a recession ends, reflecting looser credit standards, increase in credit availability, rising confidence and more sanguine expectations about future income.

The current business cycle, however, shows a clear deviation from these historical regularities, as households have continued to deleverage several years on after the recession officially ended in June 2009. After the strong debt build-up in the run-up to the Great Recession, which led the household debt-to-income ratio to reach a peak of around 134% in 2007Q4, households started to pay off more debt, take on less new debt, or default on their outstanding stock of debt. This downward trend translated into a cumulative decline in the debt ratio of more than 30 percentage points, to reach a level of around 99% in 2018Q4. The most recent leveraging and deleveraging cycle in the household sector has been accompanied by a boom and bust in house prices – a sizeable increase between mid-90s and mid-2000s, followed by an unprecedented adjustment during the Great Recession, and a recovery since 2012. The strong boom in house prices that ended abruptly in mid-2000s was in part likely to have been driven by unrealistic expectations of future house price developments. As these beliefs eventually turned out to be erroneous, house prices, and mortgage debt that was used to finance house purchases, started to decline rapidly.

FIGURE 2: Household debt-to-income ratio over US business cycles



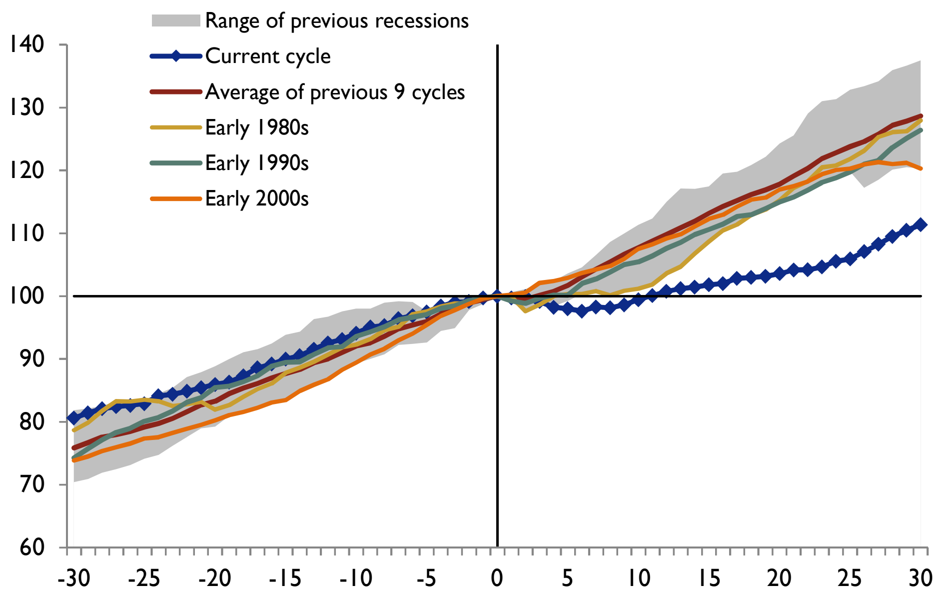
Source: Bureau of Economic Analysis, Federal Reserve Board, and author's calculations.
Notes: Zero marks the start of each recession, with the index scaled to 100. The x-axis refers to quarters. According to the NBER, the last recession started in 2007Q4 and ended in 2009q2.

At the same time, the current business cycle has been characterised by a sluggish economic recovery from the pronounced slump in consumption around the Great Recession (Figure 3). The weakness in consumption should not have come as a surprise amid the substantial balance sheet repair by households, and declining house prices – housing accounts for a substantial fraction of economic activity, and it is the main asset for the majority of households. Given the tight links between household indebtedness, house prices, and consumption, one may question to what extent and through which channels the large swings in household debt over the last two decades might have contributed to depressing economic activity and consumption growth.

The aforementioned substantial swings in credit and housing markets in the United States over the last years create significant new challenges to policy makers. In this context, this dissertation also focuses on the monetary policy transmission mechanism to the real economy in an environment of large debt imbalances in the household sector. Furthermore, I also study how monetary policy transmits to the housing market in the post-crisis period, a period characterised by increasing housing supply constraints, which have limited the expansion in housing supply.

I use disaggregated US data in all four chapters of this dissertation, namely for the states, metropolitan areas, and households. Only by exploring the substantial cross-sectional heterogeneity at those levels of disaggregation can we expect to uncover important economic relationships that otherwise would be washed out with aggregate data.

FIGURE 3: Private consumption expenditures over US business cycles



Source: Bureau of Economic Analysis, and author's calculations.
Notes: Zero marks the start of each recession, with the index scaled to 100. The x-axis refers to quarters. According to the NBER, the last recession started in 2007Q4 and ended in 2009q2.

Overview of the dissertation

In **Chapter 1**, *Debt Overhang and Deleveraging in the US Household Sector: Gauging the Impact on Consumption* (with Georgi Krustev), I investigate the extent to which the rise in household debt in the run-up to the crisis and the subsequent debt deleveraging help explain the weak recovery in consumption from the Great Recession. This chapter adds to the recent strand of literature on household finance, such as Mian and Sufi (2010), Mian et al. (2013), and Dynan (2012), by modelling separately the effects of two distinct concepts of debt on US consumption growth. More precisely, using US state-level data over 1999q1-2012q4, I estimate separately the effect on consumption from deleveraging and debt overhang, respectively the flow and stock concepts of debt. While deleveraging refers to persistent declines in the debt-to-income ratio, I measure debt overhang as the portion of debt that exceeds an estimated equilibrium level. This equilibrium, determined by economic fundamentals, is estimated using an error-correction model in Albuquerque et al. (2015).

One of the contributions of this chapter is methodological: to improve proxies of consumption with disaggregated level data, and to cross-check the baseline results that employ prototype estimates of state-level annual personal consumption expenditures (PCE) from the Bureau of Economic Analysis, I build a measure of retail sales at a quarterly frequency by relying on state tax revenues and tax rates. Specifically, my proxy of retail sales is the result of dividing tax revenues by tax rates for each US state. For this purpose, I do an extensive data collection from the Census Bureau's *Quarterly Summary of State and Local Tax Revenues* and from the Tax Foundation's *Facts & Figures on Government Finances*.

The main findings suggest that although the bulk of the slowdown in consumption around the Great Recession and the early years of the recovery was mainly due to wealth and income effects, excessive indebtedness and the balance-sheet adjustment still exerted a meaningful drag on consumption beyond those traditional determinants of consumption. Furthermore, I find that the drag on consumption growth is driven by a group of states with the largest debt imbalances. Policy makers may take this result as suggestive that the adverse effects of debt on consumption might be felt in a non-linear fashion, whereby indebtedness begins to bite only when misalignments from sustainable debt dynamics become excessive. In fact, although the debt accumulation process can be positive for the economy in the short term, it assumes that debt is not seen as excessive – i.e. that there is no misalignment of debt from fundamentals. The point in case was the rapid leveraging up of households in the run-up to the last recession which led to an arguably large debt overhang and slower consumption growth.

While recent research, including my own above, has found that high household debt, household debt build-ups, or excessive borrowing are detrimental to future economic growth (Mian and Sufi, 2010; Mian and Sufi, 2011; Jordà et al., 2013; Mian et al., 2017), and increase the probability of a financial crisis (Jordà et al., 2015), there are only a few recent papers exploring the non-linear interactions between the monetary transmission mechanism and the level of household indebtedness (Aikman et al., 2019a; Alpanda and Zubairy, 2018).

Moreover, little is known about the role that a common monetary policy might play in exacerbating regional asymmetries between states with different levels of household debt.

Against the background of considerable heterogeneity in household debt across US states, where business and credit cycles are not well synchronised, the question is the extent to which a single monetary policy may amplify regional asymmetries. In **Chapter 2**, *One Size Fits All? Monetary Policy and Asymmetric Household Debt Cycles in U.S. States*, I study the state-dependent effects of monetary policy, by exploring the heterogeneity in household debt imbalances at the state level. The choice of household debt to study the state-dependent effects of monetary policy is underscored by the considerable cross-state heterogeneity in household debt levels and dynamics over the last two decades, coupled with a significant divergence in economic performance between states with high and low household debt over the same period: while states like California and Florida went through a damaging boom–bust cycle, others such as Texas, Indiana, and Ohio, did not observe large swings in household debt (and house prices), and weathered the crisis relatively well.

I first construct a novel indicator of inflation for a sample of 30 US states, by drawing on official Consumer Price Index (CPI) data for several Metropolitan Statistical Areas (MSA) from the Bureau of Labor Statistics. I then make use of the new inflation indicator to compute a measure of monetary policy stance for the states – which I call Monetary Policy Stance Gap (MPSG) – by taking the deviations from a US aggregate Taylor rule.

Using Local Projections on state-level quarterly data over 1999-2017, I find the transmission of monetary policy to the real economy to be curtailed during periods of large imbalances in household debt. In particular, while monetary policy is supportive of borrowing and growth during periods of low household debt gaps (relative to an estimated state-specific trend), this is only the case in the short term during periods of high household debt. My estimates suggest that, over a period of five years, a one-standard deviation increase in the state-specific monetary policy stance leads to lower real GDP of 1.7 percentage points in periods of high debt, compared to periods of low debt. I hypothesise that lower economic growth in periods of high debt after an expansion in the state-specific monetary policy stance appears to be related to the need of households to deleverage from excessive credit. Since households in these periods were already highly indebted to begin with, more borrowing in the short run may place debt at even higher levels relative to income, ‘forcing’ households to deleverage and cut back on consumption expenditures, along the lines of the debt overhang theory of Eggertsson and Krugman (2012). At the same time, I also find that house prices do not increase in these periods of large imbalances in household debt, making it harder for households to take advantage of the home equity loan channel to extract more equity from their homes or to refinance their mortgages.

This chapter also provides evidence that a common monetary policy in the United States does not fit all, in that it may have asymmetric effects on household debt dynamics and economic performance across states in periods of high dispersion in household debt imbalances. These findings have relevant policy implications, raising some concerns about distributional effects and about the effectiveness of housing policy measures that aim at boosting household debt in periods of large household debt imbalances. In particular, my findings suggest that monetary policy during the last recession may have been particularly ineffective, perhaps even counterproductive, in stimulating growth in the US states with the largest debt gaps, which were precisely those states that were going through a severe boom-bust cycle.

The association between excessive household debt and large declines in consumption can be investigated further with microeconomic data. In **Chapter 3**, *Household Heterogeneity and Consumption Dynamics in the Presence of Borrowing and Liquidity Constraints*, I study the non-linear behaviour of consumption to asymmetric changes in income, when households are highly constrained. Highly constrained households devote the highest fraction of income to servicing mortgage debt – a proxy for borrowing constraints – and hold low liquid assets – a proxy for liquidity constraints.

Using data from the Panel Study of Income Dynamics (PSID) over 1999–2013, I find the following. First, consumption of the highly constrained households displays a larger sensitivity to income, which is more than twice as large as the other households, consistent with findings in the literature, such as Johnson and Li (2010). Second, the strong excess sensitivity of consumption to income of highly constrained households is explained by episodes of income increases. I rationalise this finding by the fact that highly constrained households are likely unable to borrow, or cannot do so as much as they would want, and have limited liquid savings. This means that they can only increase consumption expenditures when the income increase materialises. Third, when looking explicitly at the group of households without debt and with different levels of liquidity, which has been largely disregarded by the literature, I find that only households without debt, irrespective of the amount held of liquid assets, cut consumption when they predict their income to fall.

The policy implications of this chapter point to an important role of leverage, liquidity and wealth in affecting consumption; it can shape the way we think about the sources of business cycles, about consumption insurance, and the role played by economic policy in alleviating the negative impact of those economic fluctuations. This is particularly relevant in understanding the dynamics in consumption over the last years, notably around the period of the Great Recession. The careful design of stabilisation policies, such as automatic stabilisers and active fiscal policy, can make all the difference in softening the effects of a recession. In addition, not taking into account the heterogeneity across households might produce undesirable distributional effects. For instance, the complexity of fiscal stimulus measures typically imply that there is a lag between the announcement date and the actual implementation. While it is expected that unconstrained households can adjust consumption immediately when they perceive their income to rise in a sustained fashion, following, for instance, announcements of tax cuts or rises in government spending, borrowing- and liquidity-constrained households cannot do so. This phenomenon may reduce the effectiveness of government measures in the short term, measures which were probably aimed at households with more difficulties in the first place.

Finally, in **Chapter 4**, *Changing Supply Elasticities and Regional Housing Booms* (with Knut Are Aastveit and André Anundsen), I try to understand the implications of the apparent breakdown in the relationship between housing supply and house prices in the United States. While the recovery in construction activity has been rather weak since mid-2012, house prices have increased at a similar pace as during the previous boom that started in the mid-90s and ended in 2006; we have thus two housing boom periods with similar price developments but a much weaker recovery in construction this time around. This suggests that the housing supply elasticity has declined, i.e. that home builders have become less price responsive.

I argue in this chapter that the change in housing supply elasticities is key to understanding how demand shocks transmit to the housing market and to the real economy over time. I first estimate housing supply elasticities for a sample of 254 MSAs, using housing permits as the dependent variable, spanning the previous boom episode (1996–2006) and the recent recovery period (2012–2017). The housing supply elasticity is computed as the coefficient on house prices, controlling for several MSA-specific variables that may affect housing supply. Second I assess how changing supply elasticities affect the transmission of demand shocks.

My estimates show that housing supply elasticities are substantially lower today than during the previous housing boom. Although the decline has been a nation-wide phenomenon, I find the largest declines in areas located in states such as California, Arizona, Florida, Oregon, and New York. The tightening in land-use regulation helps explain this heterogeneity; in fact, local zoning laws have tightened more in these areas, which implies a limited supply response. In addition, these were the same areas that experienced the deepest bust in house prices during the last recession. These results suggest that the Great Recession might have cast a long shadow on builders' expectations, making them more cautious to expand supply in the face of a change in house prices.

In the second part of the chapter, I study the macro implications of having lower supply elasticities. A direct implication of lower supply elasticities is that a given change in demand should have a stronger effect on house prices. I explore the relevance of this conjecture by estimating the effect of a monetary policy shock on house prices and housing supply in the two periods. Following a recent strand of the literature, I use high-frequency data to identify unexpected changes in the Fed policy rate (Gürkaynak et al., 2005; Gertler and Karadi, 2015; Nakamura and Steinsson, 2018). The high-frequency identified (HFI) shocks isolate news about future policy actions that are orthogonal to changes in economic and financial variables. I then use a local projection instrumental variable approach (Jordà et al., 2015; Ramey, 2016; Stock and Watson, 2018) to explore how monetary policy shocks affect house prices in the two booms.

My results offer empirical evidence of a stronger response of house prices to a monetary policy shock in recent years, but a smaller response of supply. More specifically, I find that an exogenous monetary policy shock that lowers the interest rate by one percentage point led during the 1996-2006 boom to an increase in real house prices of about ten percent after four years. For the 2012-2017 recovery, the estimated response is much larger, of around 16 percent. Consistent with this, I find that building permits today increase about two percentage points less in response to the monetary policy shock. My results also point to considerable regional asymmetries, with larger responses of house prices in supply-inelastic markets compared to areas with an elastic supply.

I believe that my findings raise important policy challenges. The first one is about the economic costs and financial stability concerns of tighter housing regulation, since it has resulted in lower supply elasticities, which in turn amplifies the responsiveness of house prices to demand shocks. Second, the decline in housing supply elasticities may explain why recent research finds monetary policy to have become more effective for financial variables (Paul, 2019); an aggregate shock that raises housing demand is absorbed mostly by price adjustments, rather than quantity adjustments. This finding can be important for financial stability considerations, whereby the actions of policy makers aimed at stimulating (housing) demand may have unintended effects by exacerbating the rise in house prices. In the current environment of tighter regulation and declining elasticities, the findings in this chapter cast some doubts about the view that the recent housing market recovery looks 'healthier' and more sustainable compared to the previous boom.

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Chapter 1

Debt Overhang and Deleveraging in the US Household Sector: Gauging the Impact on Consumption

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Abstract*

Using a novel data set for the US states, this paper examines whether household debt and the protracted debt deleveraging help explain the dismal performance of US consumption since 2007 in the aftermath of the housing bubble. By separating the concepts of deleveraging and debt overhang – a flow and a stock effect – we find that excessive indebtedness exerted a meaningful drag on consumption over and beyond wealth and income effects. The overall effect, however, is modest – around one-sixth of the slowdown in consumption between 2000-06 and 2007-12 – and mostly driven by states with particularly large imbalances in their household sector. This might be indicative of non-linearities, whereby indebtedness begins to bite only when misalignments from sustainable debt dynamics become excessive.

Keywords: *Household deleveraging, Debt overhang, Consumption function, Housing wealth*
JEL classification: *C23, D12, H31*

*This chapter has been published in the *Review of Income and Wealth* **64**(2), 459-481, 2018.

Introduction

The onset of the Great Recession broadly coincided with the start of a protracted period of debt reduction in the US household sector (Figure B.1 in Appendix B). This deleveraging process has been commonly cited as a reason for the pronounced slump in consumption and the subsequent sluggish recovery of the US economy. In this context, a growing body of theoretical and empirical studies has focused on explaining to what extent and through which channels the excessive buildup of debt and the deleveraging phase might have contributed to depressing economic activity and consumption growth.

Our study sheds further light on this debate. We use state-level data over a sample that captures most of the leveraging and deleveraging cycle in the United States. Our empirical estimates employ constructed proxies for personal consumption expenditures at the state-level, including the use of a novel data set published for the first time recently by the US Bureau of Economic Analysis. One important innovation of our paper is that it singles out the effect of excessive indebtedness, or the portion of debt that exceeds an estimated equilibrium level, on consumption. We take into account the effects of two distinct concepts of debt on US consumption growth: (1) deleveraging, a flow concept related to the persistent declines in the debt-to-income ratio, and (2) the debt overhang, which refers to the stock of debt in excess of an estimated equilibrium.

Our main finding suggests that the excessive indebtedness of US households and the protracted deleveraging process since 2009 might have exerted a meaningful negative impact on consumption growth over and beyond the traditional effects from wealth and income around the time of the Great Recession and the early years of recovery. The portion of the slowdown in consumption between the two periods (2000-06 and 2007-12) at the national level attributable to household debt dynamics is estimated to be around one-sixth, whereas the other traditional factors account for the bulk of the slowdown. Furthermore, the drag on US consumption growth from the adjustments in household debt appears to be driven by a group of states where debt imbalances in the household sector were the greatest. This suggests that the adverse effects of debt on consumption might be felt in a non-linear fashion and only when misalignments of household debt leverage away from sustainable levels – as justified by economic fundamentals – become excessive.

The remainder of the paper is organised as follows. In the next section, we provide a brief review of the literature on the link between consumption and debt. Section 1.2 contains a description of the data used in the paper, focusing in particular on the construction of our proxy for state-level consumption. In Section 1.3 we present our fixed effects regression results, together with the main findings from several robustness checks, including the study of potential non-linearities. In Section 1.4 we exploit the heterogeneity in the data by carrying out an analysis at the state level. The analysis of the out-of-sample contributions to consumption growth over the 2013-14 period are covered in Section 1.5. Section 1.6 concludes.

1.1 Literature review

From a theoretical standpoint, the relationship between consumption and debt is not clearly defined. In the standard life-cycle permanent income hypothesis framework, individuals smooth consumption over the life cycle by means of a single asset they can borrow or lend freely. Consumption, C, is a function of wealth, W, and permanent income, Y:

$$C = \alpha W + \beta Y \tag{1.1}$$

where α and β are the marginal propensities to consume out of wealth and income. In this model, credit fluctuations have no particular role in explaining consumption dynamics.

Over time, the literature has devoted increasing attention to examining the deviations from, or alternatives to, the standard life-cycle model of consumption. This has opened conceptual channels through which other factors beyond the traditional ones could determine consumption. As demonstrated by Jappelli and Pagano (1989), the presence of liquidity-constrained households implies departures from the life-cycle model of consumption, setting the stage for a link between consumption and credit fluctuations. For example, in the framework described by Hall (2011), liquidity-constrained households always borrow up to the maximum allowed by lenders. Their consumption equals available funds each period, in turn given by current income, I, plus the change in borrowing, $Debt_t$-$Debt_{t-1}$, less interest payments on debt in the previous period, $Interest_t$ *$Debt_{t-1}$:

$$C_t = I_t + \Delta Debt_t - Interest_t * Debt_{t-1} \tag{1.2}$$

This implies that consumption for a large portion of US households may be driven by changes in leverage and the stock of outstanding debt.[1] In a similar vein, Eggertsson and Krugman (2012) and Guerrieri and Lorenzoni (2017) have proposed models in which debt overhang may depress aggregate demand as debt-constrained agents are forced into deleveraging. It is worthwhile to emphasise that the trigger for such deleveraging may come from both the supply-side – for example, as a result of tightening credit restrictions – and the demand side. Eggertsson and Krugman (2012) have also argued that household attitudes towards leverage may change over time, perhaps abruptly. Similarly, Dynan (2012) and Dynan and Edelberg (2013) point out that households may become uncomfortable with their indebtedness relative to some targeted level of leverage or behavioural benchmark. Changes in credit constraints or in the proportion of credit-constrained households, as well as in households' attitudes towards leverage, provide the grounds for a connection between debt and consumption.

Whether household leverage is associated with a positive or negative impact on consumption is debated in the literature, with empirical studies pointing to mixed results. Two alternative hypotheses compete in explaining the nature and sign of this relationship (McCarthy, 1997). On the one side is the 'benign' view on debt, according to which increases in household indebtedness are driven by expectations of higher future incomes, implying that household debt and consumption would tend to rise simultaneously in good times. Along the same lines, if a protracted recession permanently lowers income expectations, households would reduce both consumer spending and leverage. This strand of literature typically focuses on the flow concept of debt, where the main focus is assessing how changes in debt affect consumption growth.

On the other side is the 'alarmist" view on debt. According to this view, high debt burdens constrain households to reduce consumption so they can strengthen balance sheets and correct for past excessive leverage. This would point to a negative relationship between consumption and debt. In contrast with the first view, this literature has focused more on

[1] Defining liquidity-constrained households as those with holdings of net liquid assets being less than two months of income, Hall (2011) reports that 74% of the US households fall into this category, based on the 2007 Survey of Consumer Finances (SCF).

the effect of the debt stock on consumption.

Empirical studies have tested these two competing hypotheses, typically by examining whether debt has any significant effect on consumption once traditional determinants, such as wealth and income, have been taken into account. Table 1.1 summarises the empirical evidence along the lines of whether the findings support the benign or the alarmist view on debt. The studies presented in Table 1.1 did not, however, place a great emphasis on the difference between the two potentially competing concepts of debt, the flow versus the stock effect.

The first group of studies in Table 1.1 support the benign hypothesis, generally reporting a positive relationship between changes in debt and consumption growth. Maki (2002) and McCarthy (1997) found that increases in household debt are significantly and positively associated with consumer spending in the United States, possibly resulting from rising optimism about future income growth. By the same token, Ludvigson (1999) and Bacchetta and Gerlach (1997) show that credit variables help to predict US consumption expenditure growth, while Antzoulatos (1996) finds that periods of rising consumer debt help to signal surges in US consumption, with a tendency of forecasts by the Organisation for Economic Cooperation and Development to underpredict consumption growth during periods of increasing debt-to-income ratio. It is worthwhile noting that these studies focused on aggregate data. Moreover, most of them date back to the second half of the 1990s, so they exclude the period of the strong buildup and ensuing correction of US household indebtedness that occurred with the start of the new millennium.

Empirical studies in the second group support the alarmist hypothesis of household debt, with the stock effect generally being given priority, where typically consumption is regressed on the stock of debt. Some of these cover the more recent period and find supporting evidence that high household debt (and the subsequent deleveraging) was responsible for the large drop in US consumption around the 2007-09 recession. For example, using household-level data, Dynan (2012) and Dynan and Edelberg (2013) report that high leverage contributed in a significantly negative way to weaken consumer spending growth or household spending plans, even after accounting for the traditional explanatory factors, such as negative wealth effects. More specifically, Dynan (2012) finds that an increase of 10% in the household's mortgage leverage ratio is associated with a reduction in annual consumption growth of a few tenths of a percentage point. Using geographic data from the United States, Mian and Sufi (2010) find that high household debt buildup in some US counties during the housing boom led to weaker economic conditions in those counties in the early part of the recovery, and Mian et al. (2013) estimate a larger response of consumption to negative wealth shocks for households with higher leverage. Analysis based on household-level data by Cooper (2012) also points to a negative relationship between leverage and consumption during the Great Recession, even though there is little evidence that this relationship differs from the period that preceded it.

Table 1.1: Empirical studies on the impact of debt on consumption

Benign view on debt (+ impact on C)			**Alarmist view on debt (- impact on C)**		
Study	**Method/model**	**Sample**	**Study**	**Method/model**	**Sample**
Antzoulatos (1996)	C forecast errors regressed on consumer debt	US aggregate data and OECD forecasts (1967-94)	*Mishkin (1976)*	C=f(W, I, debt), IV estimation	US aggregate data (1954-72)
Bacchetta and Gerlach (1997)	C=f(I, debt, controls), IV estimation	Panel for 5 OECD countries, including US (1970-95)	*Ogawa and Wan (2007)*	C=f(W, I, debt, controls), OLS	Japan micro data from NSFIE (1989, 1994, 1999)
Ludvigson (1999)	C=f(I, interest, debt), IV estimation	US aggregate data (1953-93)	*Dynan (2012)*	C=f(W, I, debt, UR), cross section regressions, IV estimation	US micro data from PSID (2005, 2007, 2009)
Maki (2002)	C=f(W, I, interest, debt), ECM	US aggregate data (1962-99)	*Dynan and Edelberg (2013)*	C=f(W, I, debt, controls), probit regressions	US micro data, survey responses from SCF (2007-09)
McCarthy (1997)	VAR model (C, W, debt)	US aggregate data (1960-96)	*Mian and Sufi (2010)*	C=f(W,I, debt, controls), cross section regressions, IV estimation	US county-level data (2002-09)
			Mian, Rao and Sufi (2013)	C=f(W, I, debt, controls), IV estimation	US county and zip-code level data (2006-09)
			Cooper (2012)	C=f(W, I, debt decline as indicator variable), regression analysis	US agg. data (2003-11) and micro data from PSID (2001-09)
Olney (1999)	C=f(W, I, debt), OLS and ML	US aggregate data (1919-41); positive effect over 1938-41	*Olney (1999)*	C=f(W, I, debt), OLS and ML	US aggregate data (1919-41); negative effect over 1919-32

Notes: The estimated impact is based on variables that may differ from one study to another. An attempt is made to group the different proxies used based on the theoretical concepts they represent, so the studies can be summarised succinctly. In the table above, C denotes personal consumption; I, income or personal disposable income; W, household assets or net wealth; debt, household debt or consumer credit; interest, interest rates; UR, unemployment rate. ML stands for maximum likelihood and IV for instrumental variables. PSID is the Panel Study of Income Dynamics, SCF is the the Survey of Consumer Finances and NSFIE is the National Survey of Family Income and Expenditure.

The findings that debt has a negative impact on consumption are not limited to empirical studies analysing the more recent slump in US consumption around the Great Recession. Using aggregate US data, Mishkin (1976) found that increases in consumer liabilities prove to be a deterrent to consumer durable purchases, reporting that US$1 of additional debt held at the beginning of a period reduces purchases of durables by 22 cents in the same period. In a study covering the period around the Great Depression, Olney (1999) reports that debt had a negative effect on consumption from 1919 to 1932 but a positive effect from 1938 to 1941. This phenomenon could be explained by the different treatment of borrowers in case of default, which was affected by legislative changes that were implemented in the aftermath of the Great Depression.[2] Using household-level data for Japan, Ogawa and Wan (2007) report that the excessive debt burden of households had a significantly negative effect on consumption expenditures after the burst of the bubble in the early 1990s, prolonging the economic stagnation in Japan.

To sum up, it can be noted that the second group of studies in Table 1.1, which report that debt has a detrimental effect on consumption, captured periods of pronounced financial imbalances. These periods include the 1920s and early 1930s, the more recent housing bubble and household deleveraging in the United States and the prolonged balance sheet adjustments that took place in Japan's so-called 'lost decade' during the 1990s. In addition, these studies typically used cross-sectional or panel data, in contrast to the first group of studies that focused on aggregate data. This raises the possibility that the adverse effects of indebtedness on consumption may be uncovered only by exploiting the heterogeneity through the use of more granular data, either at the geographical or household level.

1.2 Data

1.2.1 Proxies for consumption at the state level

Our empirical analysis is challenged by the lack of officially published state-level data for US personal consumption expenditures on a quarterly basis. To overcome this, we construct two state-level proxies for consumption. Our first proxy is a quarterly measure of retail sales (RS), obtained by dividing sales tax revenue by the sales tax rate. A similar approach has been used in previous studies by Garrett et al. (2005) and by Zhou and Carroll (2012). More specifically, we compute the following:

[2] While the 1920s were characterised by harsh penalties in the case of default, changes in federal laws had eased default penalties by 1938. These changes significantly reduced the incentive of indebted households to fight default by reductions in their purchases, leading to a positive relationship between consumption and debt.

$$RS_{it} = \frac{Taxrev_{it}}{Taxrate_{it}}$$

where

- *Taxrev* refers to state-level sales tax revenues from the Census Bureau's *Quarterly Summary of State and Local Tax Revenue* at quarterly frequency;

- *Taxrate* is a series for sales tax rates, available at fiscal-year frequency for each state;

- i and t are subscripts denoting the panel (states) and time dimension (quarters) in our data set.

Our main source for the sales tax rates is the Tax Foundation's *Facts & Figures on Government Finances* from which we extract the data for 2000-13. Since we are constrained in going too far back in time by the other variables in our data set – namely, limited time-span of household debt – we need to extend the sales tax rates series backwards only for one more year (1999), which we do by relying on the Zhou and Carroll (2012) data set. We take into account the different fiscal years of each state.[3] Furthermore, we use additional, official state government data to reconstruct the precise dates when historical changes in sales tax rates took place and map these changes into our quarterly data set. As several states collect separate add-on sales taxes on behalf of local governments, we are careful to exclude them since they do not contribute to the reported sales tax revenue used as a numerator in the ratio above.[4]

Our RS proxy is constrained to 46 states (including the District of Columbia) because five states do not collect state-wide sales taxes.[5] We examine in detail our retail sales data at the level of individual states and remove excessive volatility by carefully treating outliers, typically intervening only to smooth jumps in the data that lead to unexplained spikes in annual growth rates. The treatment of outliers is justified by the fact that, as pointed out by Zhou and Carroll (2012), sales tax revenues are occasionally measured with serious

[3] For most states in the United States, the fiscal year begins on 1 July of the previous calendar year and ends on 30 June of the reference calendar year. There are exceptions, however. In Alabama and Michigan, the fiscal year ends on 30 September, while in New York and Texas, it ends on 31 March and on 31 August.

[4] Three states collect a separate, uniform 'local' add-on sales tax: California (1% since 1956, based on the Bradley-Burns Uniform Local Sales and Use Tax Regulations), Utah (1.25%) and Virginia (1%).

[5] Alaska, Delaware, Montana, New Hampshire and Oregon.

errors. As Figure B.2 in Appendix B shows, a bottom-up aggregation of our RS proxy for the states does well in comparison with the official US retail sales data at the national level, with a correlation in the nominal year-on-year growth rate between the two series of 0.88 for 1999-2012. Nevertheless, even after adjusting for outliers, the volatility in the year-on-year nominal growth rate of our RS proxy remains substantial for some states. Finally, we deflate our nominal measure of state-level retail sales with the national personal consumption expenditures deflator, given the unavailability of state-level data.

With respect to our second consumption proxy, we make use of the prototype estimates of state-level personal consumption expenditures (PCE) for 1997-2012, which the Bureau of Economic Analysis published for the first time on 7 August 2014. The data are available only at annual frequency and in nominal terms. We deal with this limitation by interpolating the annual series into quarterly frequency using the Chow-Lin interpolation procedure. For this purpose, we exploit the information from our previously constructed retail sales proxy, using it as an indicator variable in the interpolation procedure, to gain additional insights about the quarterly variation of consumption at the level of particular states.[6] The interpolated PCE resulting from the aggregation of state-level data tracks the officially published quarterly PCE at the national level reasonably well, with a correlation of 0.95 between the two series (see Figure B.3 in Appendix B).[7] Similarly to the case of our RS proxy, we deflate the nominal series with the US national PCE deflator to obtain consumption growth in real terms.

It is worthwhile noting that the rising prominence of e-commerce has eroded the sales tax base for the states and induced sales tax revenue losses, leading to a likely distortion in our retail sales measure of consumption.[8] Nevertheless, since this is a long-term trend, the quarterly variation pattern of retail sales within each year is likely to contain useful information for the interpolation of our annual state-level proxy of PCE. Throughout the empirical analysis that follows, we rely on the PCE measure as the benchmark for our estimates, and we cross-check our results by using the retail sales measure as an alternative dependent variable.

[6] For the five states for which we do not have a retail sales proxy, we use the national US retail sales as the indicator variable.

[7] Interpolating PCE with the RS proxy might create some biases in the data due to the likely seasonality from sales tax revenues. We avoid this issue by interpolating year-on-year changes, which are unaffected by seasonality, rather than the level of PCE.

[8] For example, estimates from the study by Ballard and Lee (2007) are consistent with the hypothesis that US consumers use Internet shopping to avoid sales taxes. For estimates on the sales tax revenue losses resulting from the rising prominence of electronic commerce, see Bruce and Fox (2000).

1.2.2 Explanatory variables

After modelling our two measures of consumption, we use the following explanatory variables available at the state-level (see Table A.1 in Appendix A for the descriptive statistics):

- **Real housing wealth**: The traditional wealth effect implies that increases in housing wealth, through increases in house prices or home ownership, lead to higher spending on services and goods. In the spirit of Case et al. (2013) and Zhou and Carroll (2012), it is computed as follows:

  (Homeownership rate x Occupied housing units) x HPI x Median house price in 2000

  where Homeownership rate is owner-occupied housing units divided by total occupied units, HPI is the Federal Housing Finance Agency (FHFA) House Price Index. Sources: Census Bureau and FHFA.

- **Real income**: Together with housing wealth, personal income also features predominately in a traditional consumption function, where a portion of the income gains translates into higher consumption (the so-called marginal propensity). Source: US Bureau of Economic Analysis.

- **Real interest rate**: Higher interest rates (on conventional mortgages) encourage saving, thus they tend to be associated with lower consumption. Source: Federal Housing Finance Board.

- **Unemployment rate**: The unemployment rate proxies both income expectations and uncertainty, as suggested by the literature (see, for instance, Fernandez-Corugedo and Muellbauer, 2006). For example, expectations of higher future incomes (a lower unemployment rate) are associated with higher consumption growth. Along the same lines, lower uncertainty would imply less need for precautionary saving, and thus would boost consumption. Source: Bureau of Labor Statistics.

- **Loan-to-value ratio (LTV)**: The loan-to-value ratio on conventional mortgages for previously occupied homes (excluding refinancing loans) is a proxy for financial innovation and credit availability. An increase in financial innovation typically leads to an improvement in the access to credit by households, so, in theory, a greater LTV would benefit consumption growth. Source: FHFA.

- **Debt-to-income ratio**: Total household debt – mortgage debt and consumer credit, which includes auto loans, credit cards and student loans – divided by personal income. Source: Federal Reserve Bank of New York/Equifax.

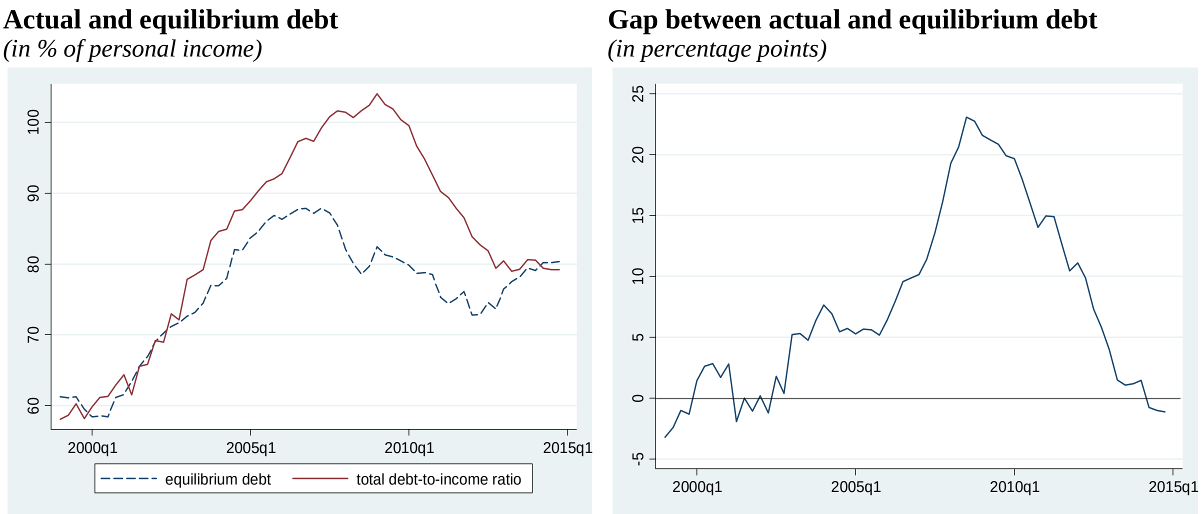
- **Debt gap**: The difference between the actual and the estimated household equilibrium debt-to-income ratio. Source: Albuquerque et al. (2015).

The state-level nominal indicators are deflated with the national personal consumption expenditures deflator. The last two variables will be in the centre of our analysis, as we are primarily interested in studying the role of debt and its misalignment from the estimated equilibrium on consumption growth. In particular, the time-varying debt gap results from an estimated equilibrium household debt-to-income ratio determined by economic fundamentals, resorting to a panel error correction framework for the 51 US states (plus the District of Columbia).[9] As explained in Albuquerque et al. (2015), the model is estimated with the Pooled Mean Group (PMG) estimator, developed by Pesaran et al. (1999), and adjusted for cross-sectional dependence. The original model that was estimated on data from 1999Q1 to 2012Q4 has been updated with the US national data up to 2014Q4.

Figure 1.1 shows that the rise in debt at the US national level resulted in a growing misalignment from the equilibrium level since around 2002-03. This trend has been reinforced since late-2007 by the decline in equilibrium debt, as the economic fundamentals deteriorated. Thereafter, the deleveraging process (a decline in the debt-to-income ratio), which started in 2009, allowed the debt gap to shrink significantly from a peak of around 23 percentage points in 2008Q3. Our updated estimates suggest that the debt gap has been closed since mid-2014, with the recent improvement being supported by an increase in equilibrium debt, reflecting the sustained recovery in the US economy, while actual debt appears to have stopped declining. At the state level, however, and despite the synchronised balance sheet adjustment, deleveraging needs differ. According to our estimates, the adjustment process appears to have been completed in one-third of the states by the end of 2012.

[9] The fundamentals include a measure of house prices, the homeownership rate, the interest rate, and proxies for income uncertainty and credit supply.

FIGURE 1.1: Actual and equilibrium debt-to-income ratio and implied gap



Source: FRBNY/Equifax Consumer Credit Panel and authors' calculations.
Notes: Last observation refers to 2014Q4.

1.3 Estimation results

1.3.1 Fixed effects

In this section we run panel regressions with fixed effects (FE) for the 51 US states (including the District of Columbia) over the period from 1999Q1 to 2012Q4. Not only does our consumption function include the main determinants as used in traditional consumption equations, but it also has a role for debt and its misalignment from equilibrium, including some standard control variables. In particular, we estimate the following equation:

$$\begin{aligned}\Delta_4 C_{it} = \alpha_i + \beta_1 \Delta_4 Wealth_{it} + \beta_2 \Delta_4 Income_{it} + \beta_3 \Delta_4 Debt_{i,t-1} \\ + \beta_4 Debt\_gap_{i,t-1} + \gamma Controls_{it} + \delta d_t + \varepsilon_{it}\end{aligned} \tag{1.3}$$

where C refers to real PCE, *Wealth* is real housing wealth, *Income* is real personal income, *Debt* is the household debt-to-income ratio, and *Debt_gap* is the difference between the actual and the estimated household equilibrium debt-to-income ratio, taken from Albuquerque et al. (2015). *Controls* include the real interest rate (*Interest*), the unemployment rate (*UR*) and the *LTV*. A vector of time dummies d captures time-fixed effects. The subscripts i and t denote the 51 states in the panel and the time dimension (quarters). To minimise the reverse causality issue, we lag the debt ratio and the debt gap by one period. This is in line with other empirical studies in that excessive indebtedness is expected to affect consumption with a lag (Olney, 1999).

After carrying out a set of panel unit-root tests, we find evidence in support of the stationarity of interest rates and the debt gap (see Table A.2 in Appendix A), thus we use them in levels in Equation 1.3. The remaining series are transformed into year-on-year differences. Δ_4 represents year-on-year percentage changes for real PCE, housing wealth, and real income, while it refers to year-on-year percentage point changes for debt-to-income, the unemployment rate and the loan-to-value ratio.

We guard against model misspecification in several ways. We report standard errors that are robust to heteroskedasticity, using the Huber-White sandwich estimator. Based on the results from several model selection tests, we choose to rely on the two-way FE estimation method, which allows for group-specific and time effects. The latter allow to control for the possibility of omitted time-varying factors driving some of the variables at the state level. Finally, in the choice between the FE and the random effects (RE) estimators, we relied on results from an auxiliary regression-based Hausman test.[10]

The issue of cross-sectional dependence deserves a special mention. As pointed out by De Hoyos and Sarafidis (2006), cross-sectional dependence is a common feature in panel data sets and is particularly relevant for units with a high degree of economic and financial integration, such as the states in the United States. Cross-section interdependencies may arise from the presence of common shocks and unobserved components. Given the type of data and period that we are covering, examples of common unobserved factors in our case could be the housing boom and the subsequent bust, the 2007-09 financial crisis or changes in sales tax rates across states that are not captured by our explanatory variables in the model. If ignored in the estimation phase, such cross-sectional interdependencies become part of the error term and are likely to lead to seriously misleading inference due to their correlation with the explanatory variables (Phillips and Sul, 2003). To correct for this problem, we allow for time effects by augmenting our model with time dummies.[11] The rationale and validity

[10]The standard version of the Hausman test becomes invalid when using robust standard errors and time dummies. The issue can be circumvented by using a more general testing procedure based on the use of auxiliary regressions (Mundlak, 1978; Wooldridge, 2010), which is valid in the presence of heteroskedasticity or within autocorrelation.

[11]The use of time dummies assumes that time effects have a homogeneous impact on the cross-sectional units. In Appendix C we focus on dynamic panel models, where we relax this assumption by employing

of this approach are confirmed by the Wald test, which shows the joint significance of the time dummies, and by their efficacy in minimising the problem of cross-sectional dependence in the errors, as revealed by post-estimation results.[12] In particular, we found statistically significant negative time effects around the period of the Great Recession; a sign that time-varying common shocks originating from the financial crisis and the housing slump were driving the dynamics of the variables across panels.

One of the findings from Table 1.2 is that the two traditional variables that have been found in the literature to be the main drivers of consumption – wealth and income – consistently turn out to be highly statistically significant across different specifications. Based on the results in the seven columns of Table 1.2, we determine that the elasticity of consumption to housing wealth is estimated to lie in a range of between 0.09 and 0.11 percentage points, which is in line with the values reported in the literature (Case et al., 2013).[13] With respect to the effect of income, we find that a 1-percentage-point increase in real personal income growth leads to higher consumption growth in the order of 0.3 percentage points, the same order of magnitude as the elasticity reported by Bacchetta and Gerlach (1997) for 1970-95.

We do not find a statistically significant role for interest rates in the standard FE estimation. This feature has been documented elsewhere in the literature (see, among others, Ludvigson, 1999). Changes in the unemployment rate, a plausible proxy for income expectations and uncertainty, are found to exert a highly significant impact on consumption growth with the expected negative sign in line with previous findings (Aron and Muellbauer, 2013). Moreover, our results are not sensitive to the measure used of credit supply; the main results remain unchanged when we replace the LTV ratio with alternative measures of credit supply, such as willingness to lend and credit standards on mortgages from the Senior Loan Officer Opinion Survey (SLOOS).

As for the debt variables, the debt gap is statistically significant and exerts a negative impact on consumption growth. The estimated effect implies that a 10-percentage-point

the common correlated effects approach by Pesaran (2006), which allows for heterogeneous cross-sectional dependence.

[12] We found evidence of severe cross-sectional dependence in the disturbances in a version of Equation 1.3 estimated without time dummies, which allow to filter out time effects.

[13] We have not accounted for financial wealth because of the lack of data at the state level. However, we believe that this is not a major caveat as the recent studies from the literature have reported that financial wealth is not statistically significant in consumption regressions once housing wealth is accounted for (see Zhou and Carroll, 2012). Nevertheless, we cross-checked our results by including financial wealth at the national level as an additional control variable. The results remained broadly similar in qualitative terms.

overhang in the household debt-to-income ratio, interpreted as misalignment from the equilibrium level of leverage, negatively affects consumption growth by around 0.2 percentage points. This would be in line with the alarmist view of debt and similar in magnitude to the estimates of Dynan (2012). At the same time, the estimates yield a statistically significant effect of debt on consumption growth: a 10-percentage point decline in the debt-to-income ratio would lead to lower consumption growth of around 0.2 percentage points. By the same token, deleveraging (a decline in the debt-to-income ratio) tends to depress consumption since it implies the need for higher savings to reduce the outstanding debt balance. The findings support the notion that debt variables have explanatory power for consumption even after accounting for traditional determinants, such as wealth and income.

Table 1.2: Fixed effects estimation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Δ_4Wealth	0.107***	0.105***	0.097***	0.095***	0.095***	0.090***	0.090***
	(0.014)	(0.014)	(0.011)	(0.012)	(0.012)	(0.011)	(0.011)
Δ_4Income	0.314***	0.300***	0.301***	0.293***	0.294***	0.284***	0.285***
	(0.027)	(0.029)	(0.025)	(0.026)	(0.026)	(0.025)	(0.025)
Δ_4Debt$_{t-1}$		0.018		0.014	0.016	0.020*	0.020*
		(0.012)		(0.011)	(0.010)	(0.011)	(0.011)
Debt_gap$_{t-1}$			-0.021**	-0.019*	-0.020*	-0.019*	-0.019*
			(0.010)	(0.011)	(0.011)	(0.011)	(0.011)
Interest					0.567	0.526	0.525
					(0.505)	(0.513)	(0.510)
Δ_4UR						-0.282***	-0.283***
						(0.073)	(0.073)
Δ_4LTV							-0.011
							(0.028)
Observations	2,856	2,601	2,805	2,601	2,601	2,601	2,601
States	51	51	51	51	51	51	51
R-Squared	0.650	0.630	0.653	0.633	0.634	0.638	0.638
Hausman	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Wald t-statistic	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Friedman test	1.000	1.000	1.000	1.000	1.000	1.000	1.000

Notes: Fixed effects regressions with time dummies where the dependent variable is $\Delta_4$real PCE. Δ_4 denotes year-on-year % changes for housing wealth and income, and year-on-year change for debt-to-income, the unemployment rate and the LTV ratio. Robust heteroskedastic and autocorrelation-consistent standard errors shown in parentheses. The Hausman test reports p-values under the null hypothesis that the random effects estimator is both efficient and consistent. The Wald t-statistic is based on a joint test that the coefficients on the time dummies are equal to 0 under the null hypothesis. The Friedman test reports p-values under the null hypothesis of cross-sectional independence of the residuals based on Friedman (1937). Asterisks, *, **, ***, denote statistical significance at the 10, 5 and 1% levels.

Our findings suggest that the assessment of the cumulative effect of debt on consumption should account for both the dynamics of household indebtedness and the degree of debt overhang. To illustrate this point, suppose the impact of debt is symmetric in that an increase in the debt ratio is associated with higher consumption growth. If the debt ratio is not accompanied by a similar increase in equilibrium debt – meaning the economic fundamentals did not support a rise in households' debt capacity – the deviation from equilibrium (the debt gap) would rise by the same amount, offsetting the positive effect from the rise in the debt-to-income ratio. The overall impact of a modest leveraging up of households could even turn negative in the presence of a large debt overhang as, arguably, was the case around the start of the Great Recession.

On the other hand, the negative effects from deleveraging may be reinforced substantially in the event of a large debt overhang that needs to be corrected, as opposed to a scenario where household indebtedness is close to its equilibrium level. In other words, deleveraging matters for consumption, but its importance depends on how far from equilibrium household debt is while the process takes place.

When we employ our RS proxy as explained in Section 1.2.1 as the dependent variable, one difference from the regressions with the PCE is that it is now harder to uncover statistical significance for many of the explanatory variables, with the exception of wealth and income (see Table B.1 in Appendix B). Nevertheless, in most cases, the point estimates of the coefficients maintain their expected signs. The differences in the results are mostly explained by the fact that the regressions with the RS proxy are estimated less precisely, thus yielding larger standard errors. In addition, the R-squared is substantially lower because the RS proxy is more volatile than the PCE measure and, arguably, exhibits larger measurement errors.

The differences in the precision of the estimates might also be the result of a different coverage of goods and services. The RS proxy, which is the result of dividing sales tax revenues by sales tax rates, does not cover goods and services not subject to sales taxes, such as prescription medications, a large fraction of basic food goods and clothing in most states. In addition, the PCE measure also includes consumption of services without market transactions. The largest imputation of these non-market transactions is housing services provided by owner-occupied housing, the so-called imputed rents.[14] To cross-check our results, we drop housing services from the PCE measure, which makes PCE more comparable to the RS proxy, and run again the regressions. When we consider non-housing PCE, our

[14] Imputing rents makes sure that the treatment of owner-occupied housing is comparable to that of tenant-occupied housing, i.e. the rent that homeowners would pay if they rented their own home. The logic behind it is to capture the consumption of housing services, irrespective of being a homeowner or renter.

estimates (not reported) broadly confirm the results from Table 1.2, with the difference that the change in the debt-to-income ratio is estimated less precisely. This suggests that housing services may not be playing a big role in explaining the differences in the estimates between the RS proxy and PCE. Although it would be interesting to investigate deeper the effect of our explanatory variables on the different components of consumption, it is beyond the scope of this paper.

In Appendix C, we investigate the robustness and sensitivity of our main results along several dimensions. For instance, we find that our baseline results remain robust to the potential endogeneity bias, and to employing alternative methods that control for autocorrelation, cross-sectional correlation and heteroskedasticity across panels. We also focus on interaction terms to uncover the existence of specific economic relationships. In particular, our results lend support to the idea that a meaningful channel through which excessive indebtedness interacts with consumption is by soaking up resources away from overly indebted households through debt service payments. In addition, we find tentative evidence of non-linear effects on consumption from leveraging and deleveraging. In a situation where deleveraging is taking place, the larger the pace of debt reduction, the more negative the effect on consumption becomes. In contrast, the support to consumption growth from the debt-accumulating process diminishes as the speed of leveraging picks up. Finally, we also explore further the link between debt and consumption when we introduce a panel-error correction framework to deal with the long-term dynamics, making use of the Common Correlated Effects Pooled Mean Group (CCEPMG) estimator (Pesaran, 2006).

1.3.2 Contributions to the slowdown in consumption

We use our earlier estimates in a simple exercise where we break down the factors behind the observed slowdown in personal consumption expenditures growth between two periods: 2000-06 and 2007-12. These periods are of roughly equal length but are marked by very different characteristics. The first period is characterised by strong consumption growth, significant house price appreciation, low and stable unemployment and a sizable buildup of household leverage, which led afterwards to rising debt overhang. The second period covers the Great Recession and the subsequent subdued recovery. Consumption growth is, on average, less than half compared to the first period and real housing wealth is declining at an unprecedented rate, while the unemployment rate is high and (on average) rising. The overall debt-to-income ratio is also much higher, although deleveraging starts to take hold during the recession. The average debt overhang is larger, reflecting the accumulation of imbalances from the past and weak economic fundamentals, implying a lower level of sustainable/equilibrium debt.

To compute the contributions for the slowdown in consumption growth during the second period, we use the estimated coefficients from the benchmark FE specification in column (7) of Table 1.2 (see Figure B.4 in Appendix B for the in-sample fit). Table 1.3 shows the results based on the average predicted values for all the US states. The main findings could be summarised as follows. First, it appears that the presence of a significant debt overhang and the deleveraging process in the second period reinforced each other in depressing consumption growth. This notwithstanding, the overall direct negative impact from the two debt variables appears to be modest: cumulatively, they account for 15% of the overall slowdown in annual consumption growth since 2007. By contrast, more than two-thirds of the slowdown could be explained by traditional determinants of consumption, namely wealth and income.

Table 1.3: Contribution to the slowdown in PCE growth

Variable	2000-06	2007-12	Change	Contribution	%
Δ_4PCE	3.5	1.4	-2.0	-2.0	100
Δ_4Wealth	7.3	-3.5	-10.8	-1.0	48.1
Δ_4Income	3.1	1.6	-1.4	-0.4	20.0
Debt (Δ_4Debt)	76.6 (4.4)	91.1 (-1.0)	14.5	-0.1	5.3
Debt gap	-1.4	9.2	10.7	-0.2	9.7
UR (Δ_4UR)	4.9 (0.1)	7.1 (0.5)	2.2	-0.1	5.9
Other/unexplained				-0.2	11.0

Authors' calculations based on fixed effects regressions with time dummies where the dependent variable is $\Delta_4$real PCE. The table reports averages for all the US states.

The results need to be seen in the context of the particularly large negative housing wealth shock experienced by US households. As pointed out earlier, our estimates for the elasticity of consumption to traditional determinants are broadly in line with previous empirical studies, some of which exclude the period of the financial crisis. Therefore it is the magnitude of the wealth shock that explains the large negative contribution of wealth effects for the slowdown in consumption over the later period, in line with the findings from Mian et al. (2013). A plausible interpretation is that the large house price declines after 2006 shook the commonly held belief prior to the crisis that housing assets cannot lose their value. This implied a durable reassessment of life-time resources available for consumption; the effect was reinforced by the decline in income and less optimistic future prospects, as well as by the necessity to bring down indebtedness to a new, more sober target level.

One should be cautious, however, to avoid overinterpreting the results. In particular, one caveat is that the FE model implicitly assigns equal weights to the states. But it is possible that the full-sample estimates of the coefficients are driven by developments in a small number of states with particularly severe debt overhang and deleveraging problems – for example, the so-called 'sand states' – which may not be representative of the United States as a whole.[15] We will return to these questions in Section 1.4, when we deal with the state-level heterogeneity.

1.4 Heterogeneity at the state level

We turn our attention to heterogeneity at the state level. The substantial differences in macroeconomic performance across states is documented in Figure B.5 in Appendix B. Against this background, in this section we examine to what extent our main results are driven by developments across particular groups of states. More precisely, we reproduce the results from our baseline specification in column (7) of Table 1.2, distinguishing between those states that experienced the largest deleveraging and those with the smallest deleveraging in the household sector from their respective peaks until the end of 2012. In addition, we check the sensitivity of our results by estimating our consumption function across non-recourse and recourse states, where the difference lies in how borrowers who default are treated. In foreclosure, borrowers in recourse states are liable for the remaining portion of the debt not covered by the sale of the underlying collateral. A pertinent question, then, is whether these borrowers might be facing stronger constraints to honour their debt obligations at the expense of higher savings and lower consumption relative to borrowers in non-recourse states for which default might have less painful implications. We examine these questions by (i) splitting the sample between high deleveraging (HD) and low deleveraging states (LD) as well as between recourse (R) and non-recourse (NR) states; and (ii) by interacting the key variables of interest with dummies for LD states and NR states.

[15] The term 'sand states' refers to Arizona, California, Florida and Nevada. These states experienced the most acute housing downturn in the United States.

The results in Table 1.4 show that the main determinants of consumption – wealth and income – remain highly statistically significant across all specifications. The short-run elasticity of consumption to income falls in the range of 0.21 (LD states) and 0.35 (HD states). Interestingly, the coefficient on housing wealth roughly doubles in size for HD states as opposed to LD states. This might reflect a higher degree of optimism across households in HD states with respect to future house price and/or income dynamics before the crisis, possibly leading to larger swings in borrowing. The result is also consistent with the Mian et al. (2013) finding of a larger response of consumption to negative wealth shocks for households with higher leverage. The effect of uncertainty on consumption growth remains generally highly significant across the various groups. In the case of non-recourse states, the LTV ratio turns out significant and with the expected sign in column (6). This is tentative evidence that easing credit conditions might be more stimulative for consumption in non-recourse states, where households might have had stronger incentives to borrow to capitalise on the housing price boom. The somewhat larger coefficient on housing wealth for non-recourse states should also be noted.[16]

With respect to the debt variables, the coefficient on the change in the debt-to-income ratio remains significant in roughly half of the reported specifications. By contrast, the debt gap turns insignificant in column (2) to (8), even though the point estimates are qualitatively similar to earlier results.[17] One clear take-away from the results, however, is that the effects of leveraging and deleveraging on consumption are driven by the high-deleveraging states in the sample, whereas the impact of debt on consumption appears to be insignificant for the low-deleveraging states. In particular, the coefficient on the debt-to-income ratio doubles in size for the top 10th percentile of the high-deleveraging states relative to the coefficient estimated on the whole sample. In this case, the effect is also significantly different (at the 5% confidence level) from the effect for the remaining 90$^{\text{th}}$ percentile of states with the lowest deleveraging from the peak. This invites caution in drawing strong conclusions from the results with respect to the impact of debt on consumption at the aggregate level.

[16] The average LTV ratio for NR states is 75.7%, almost two percentage points below the average for R states (77.5%). Mortgage rates are essentially identical, suggesting that lenders sought protection from the higher credit risk in NR loans by demanding more collateral (i.e., a lower LTV) instead of charging a higher interest.

[17] This highlights the limitations of our relatively short data sample and the large size of the (robust) standard errors relative to the estimated coefficients on the debt variables: splitting the sample or adding terms to the main specification makes it harder to find statistically significant effects.

At the same time, the effects from the debt variables do not differ in a statistically significant way for the recourse, relative to the non-recourse states (see interaction term with the NR dummy in column (8). Therefore, the results fail to confirm the hypothesis that higher penalties in the case of default result in a stronger impact from excessive indebtedness and/or deleveraging on consumption.

TABLE 1.4: Fixed effects: Examining heterogeneity with split regressions and interaction terms

	(1) Baseline	(2) HD states	(3) LD states	(4) Interact LD50pctl	(5) Interact LD90pctl	(6) Non-Rec. states	(7) Recourse states	(8) Interact NR states
Δ_4Wealth	0.090***	0.098***	0.054***	0.090***	0.092***	0.118***	0.079***	0.090***
	(0.011)	(0.016)	(0.014)	(0.011)	(0.010)	(0.017)	(0.014)	(0.011)
Δ_4Income	0.285***	0.347***	0.212***	0.279***	0.276***	0.299***	0.256***	0.285***
	(0.025)	(0.044)	(0.044)	(0.025)	(0.025)	(0.051)	(0.030)	(0.025)
Δ_4Debt$_{t-1}$	0.020*	0.019*	0.009	0.022*	0.041**	0.039	0.016	0.021*
	(0.011)	(0.011)	(0.026)	(0.011)	(0.018)	(0.037)	(0.010)	(0.012)
Debt_gap$_{t-1}$	-0.019*	-0.007	-0.016	-0.017	-0.014	-0.011	-0.022	-0.017
	(0.011)	(0.011)	(0.019)	(0.011)	(0.013)	(0.011)	(0.015)	(0.014)
Interest	0.525	0.527	0.773	0.604	0.638	1.214	0.010	0.549
	(0.510)	(0.671)	(0.736)	(0.502)	(0.461)	(0.802)	(0.713)	(0.512)
Δ_4UR	-0.283***	-0.471***	-0.080	-0.287***	-0.277***	-0.183*	-0.307***	-0.282***
	(0.073)	(0.145)	(0.113)	(0.073)	(0.074)	(0.088)	(0.078)	(0.072)
Δ_4LTV	-0.011	0.028	-0.023	-0.008	-0.011	0.095*	-0.033	-0.011
	(0.028)	(0.046)	(0.032)	(0.027)	(0.027)	(0.052)	(0.030)	(0.028)
LD50*Δ_4Debt$_{t-1}$				-0.021				
				(0.020)				
LD50*Debt_gap$_{t-1}$				0.018				
				(0.015)				
LD90*Δ_4Debt$_{t-1}$					-0.047**			
					(0.023)			
LD90*Debt_gap$_{t-1}$					0.005			
					(0.015)			
NR*Δ_4Debt$_{t-1}$								-0.010
								(0.019)
NR*Debt_gap$_{t-1}$								-0.007
								(0.018)
Observations	2,601	1,275	1,326	2,601	2,601	612	1,989	2,601
States	51	25	26	51	51	12	39	51
R-Squared	0.638	0.716	0.555	0.639	0.641	0.693	0.635	0.638

Notes: Fixed effects regressions with time dummies where the dependent variable is $\Delta_4$real PCE. High-deleveraging (HD) and low-deleveraging (LD) states in column (2) and column (3) refer to the 50th percentile of states with the largest and smallest declines in their household debt-to-income ratio from their respective peaks up to 2012Q4. LD50 and LD90 in column (4) and column (5) refer to dummy variables that take the value of 1 for the states with the 50$^{\text{th}}$ percentile and 90$^{\text{th}}$ percentile of smallest declines in their household debt-to-income. Non-recourse (NR) states in columns (6) to (8) refer to states where the lender has no recourse against borrowers if the borrowers' house is sold at auction or in a short sale for less than the amount owned by the lender (Alaska, Arizona, California, Connecticut, Idaho, Minnesota, North Carolina, North Dakota, Oregon, Texas, Utah and Washington, D.C.). NR dummy used in column (8) refers to a dummy variable that takes the value of 1 for the non-recourse states.

Table 1.5 decomposes the factors behind the slowdown in PCE growth between 2000-06 and 2007-12 as already seen in Section 1.3.2. This time we split the sample between the top 10$^{\text{th}}$ percentile and the bottom 90$^{\text{th}}$ percentile of states, according to the magnitude of deleveraging they experienced since the balance sheet adjustment process started. The results are based on the specification with interaction terms using a dummy for the LD states as shown in column (5) of Table 1.4. A first glimpse at the table underscores the heterogeneity in economic performance between the two groups. The previous finding of a dominant effect from traditional factors in explaining the slowdown in consumption is confirmed by the results for both samples. Despite the much stronger slowdown in consumption growth for the HD states, wealth and income dynamics appear to explain a similar portion of the slowdown as for LD states. By contrast, the main difference lies in the debt variables. While the contributions from deleveraging and the debt overhang appear to be minimal for the LD states, for HD states the debt variables account for roughly 20% of the slowdown of PCE growth since 2007. As seen before for the results at the national level, the drag from the debt overhang on consumption (the stock of debt in excess of an estimated equilibrium) tended to be larger than the one from household debt deleveraging (the flow concept). Moreover, the prevalence of the effect for those states that appear to have accumulated particularly severe imbalances might be indicative of non-linearities, whereby the adverse impact of excessive indebtedness begins to be felt only at a point when misalignments from sustainable dynamics – as justified by fundamentals – become excessive.

TABLE 1.5: Contribution to the slowdown in PCE growth

Top 10th percentile of states by deleveraging

Variable	2000-06	2007-12	Change	Contribution	%
Δ_4PCE	5.5	0.9	-4.6	-4.6	100
Δ_4Wealth	13.1	-8.1	-21.2	-2.0	42.5
Δ_4Income	4.1	1.1	-3.1	-0.8	18.3
Debt (Δ_4Debt)	95.0 (6.5)	122.3 (-2.8)	27.3	-0.4	8.4
Debt gap	-7.6	23.1	30.7	-0.4	9.4
UR (Δ_4UR)	4.5 (-0.1)	7.7 (0.7)	3.2	-0.2	4.9
Other/unexplained				-0.8	16.5

Bottom 90th percentile of states by deleveraging

Variable	2000-06	2007-12	Change	Contribution	%
Δ_4PCE	3.2	1.5	-1.7	-1.7	100
Δ_4Wealth	6.5	-2.9	-9.5	-0.9	52.0
Δ_4Income	2.9	1.7	-1.2	-0.3	19.8
Debt (Δ_4Debt)	74.2 (4.2)	87.0 (-0.7)	12.8	0.0	-1.5
Debt gap	-0.6	7.4	8.0	-0.1	4.1
UR (Δ_4UR)	4.9 (0.1)	7.0 (0.4)	2.1	-0.1	6.1
Other/unexplained				-0.3	19.5

Authors' calculations based on fixed effects regressions with time dummies where the dependent variable is $\Delta_4$real PCE. The split is between the 10th percentile of states with the largest and the 90th percentile of states with the smallest declines in their household debt-to-income ratio from their respective peaks up to 2012Q4.

1.5 Out-of-sample contributions to consumption over 2013-14

With the ongoing recovery in the United States, the deleveraging process appears to be already over at the US national level. In this context, one might reasonably expect household debt to support consumption growth going forward as long as the increase in debt does not lead to a widening of the debt gap. This is indeed what our out-of-sample results show for 2013-14, where PCE growth picked up to an average of 2.4% compared with an average of 1.4% in the previous six years. Our estimates suggest that the closing of the debt gap, through both deleveraging and an improvement in equilibrium debt (reflecting better economic conditions), accounted for almost one-fifth of the acceleration in PCE growth between the two aforementioned periods (Table 1.6). The upturn in house prices, which led to an important increase in housing wealth, accounted for roughly half of that acceleration. In contrast, income – the other main traditional determinant of consumption – failed to pick up during this period. Finally, the significant improvement in the labour market over the last two years had a prominent role in supporting consumption growth.

TABLE 1.6: Out-of-sample contribution to the pick-up in PCE growth in 2013-14

Variable	2007-12	2013-14	Change	Contribution	%
Δ_4PCE	1.4	2.4	1.0	1.0	100
Δ_4Wealth	-3.5	2.4	5.9	0.5	52.3
Δ_4Income	1.6	1.5	-0.1	0.0	-2.4
Debt (Δ_4Debt)	91.1 (-1.0)	81.0 (-0.9)	-10.2	0.0	0.1
Debt gap	9.2	-1.9	-11.1	0.2	18.6
UR (Δ_4UR)	7.1 (0.5)	6.2 (-0.8)	-0.9	0.4	35.6
Other/unexplained				0.0	4.2

Authors' calculations based on fixed effects regressions with time dummies where the dependent variable is $\Delta_4$real PCE. The table reports averages for all the US states. Because of the lack of data for 2013-14, we construct the state-averages of PCE growth, debt-to-income and the debt gap by relying on data from the US aggregate.

1.6 Concluding remarks

The leveraging and subsequent deleveraging cycle in the US household sector played a significant role in affecting the performance of economic activity in the years around the Great Recession. In this context, our study adds to the recent strand of literature on household finance, such as Mian and Sufi (2010), Mian et al. (2013), and Dynan (2012), by modelling the effects of two distinct concepts of debt on US consumption growth separately:

(1) deleveraging, a flow concept related to the persistent declines in the debt-to-income ratio, and (2) the debt overhang, which refers to the stock of debt in excess of an estimated equilibrium. Our main finding suggests that the excessive indebtedness of US households and the balance-sheet adjustment that followed have had a meaningful negative impact on consumption growth over and beyond the traditional effects from wealth and income around the time of the Great Recession and the early years of the recovery. The prevalence of the effect for those states that appear to have accumulated particularly severe imbalances might be indicative of non-linearities, whereby indebtedness begins to bite only when there is a sizeable misalignment from the debt level dictated by economic fundamentals.

Our main results suggest that the nature of the indebtedness determines what is the ultimate impact of debt on consumption. Against the background of the ongoing recovery in the United States, where the deleveraging process appears to be already over at the US national level, one might expect household debt to support consumption growth going forward as long as the increase in debt does not lead to a widening of the debt gap. This is indeed what our out-of-sample results show for the 2013-14 period, with both deleveraging and an improvement in equilibrium debt (reflecting better economic conditions) accounting for almost one-fifth of the acceleration in PCE growth between this period and the preceding six years. The upturn in house prices, which led to an important increase in housing wealth, accounted for roughly half of that acceleration.

Looking ahead, consumption growth should be supported by the ongoing debt dynamics once again if there are no further shocks to the housing market and households take on more debt in line with the fundamentals, implying that the debt gap remains closed. The significant heterogeneity among US states, however, highlights the possibility that households in some states with unfavourable debt dynamics could still see their consumption growth being held back.

Acknowledgements

We are indebted to the FRBNY and Equifax for providing us with aggregated, state-level summary data on household debt and to Ursel Baumann, Michael Binder, Jean-Philippe Cayen, Alistair Dieppe, Michael Ehrmann, David Lodge, Guido Lorenzoni, participants at the 2015 Money and Finance Workshop (Rimini), the 9th Annual Meeting of the Portuguese Economic Journal (Angra do Heroísmo, Azores), and seminar participants at Bank of Canada and at the ECB for fruitful discussions and suggestions. We would also like to thank the editor and an anonymous referee for their suggestions and comments. Georgi Krustev wishes to thank staff at the Bank of Canada's International Department for their hospitality and support during a three-month visit, where he worked on this project. The opinions expressed in this paper are those of the authors and do not necessarily reflect the views of the Bank of Canada, the ECB or of the Eurosystem. All errors and omissions remain the authors' responsibility.

Appendix

A Data sources and descriptive statistics

Table A.1: Descriptive statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
Δ_4PCE	2856	2.8	2.5	-5.3	14.6
Δ_4Retail sales proxy	2576	1.4	9.1	-45.6	88.1
Δ_4Wealth	2856	2.5	7.6	-24.8	42.0
Δ_4Income	2856	2.5	2.8	-11.2	17.5
Δ_4Debt	2652	1.8	6.2	-30.0	33.7
Debt gap	2856	2.9	10.6	-32.1	98.0
Interest rate	2856	4.0	1.3	1.3	6.7
Δ_4UR	2856	0.2	1.2	-5.4	6.0
Δ_4LTV	2703	-0.2	2.2	-17.5	10.8

Source: Albuquerque et al. (2015), Bureau of Economic Analysis, Bureau of Labor Statistics, Census Bureau, Federal Housing Finance Agency, Federal Housing Finance Board, FRBNY/Equifax Consumer Credit Panel, and authors' calculations.

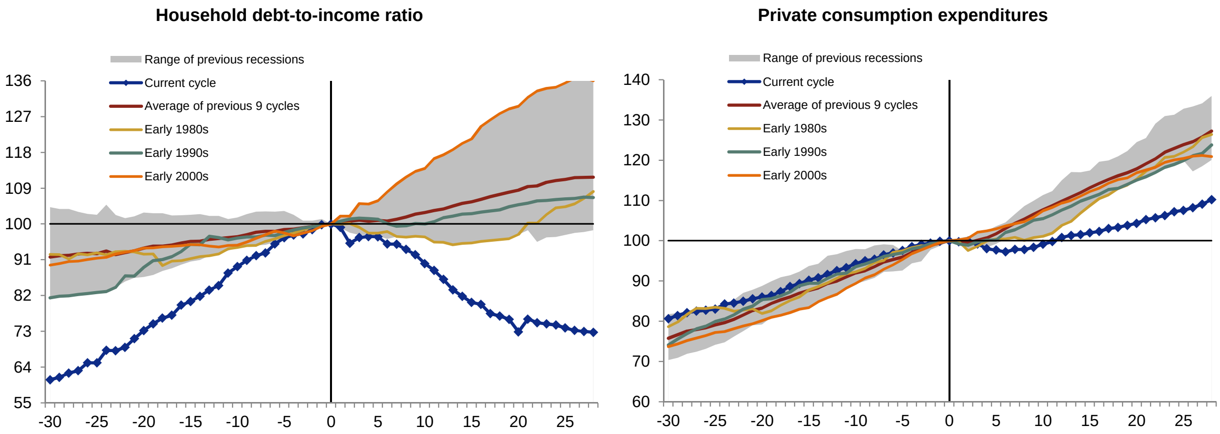
Table A.2: Panel unit-root tests (p-values)

		Retail sales	PCE	Debt	Debt gap	Income	Wealth	Net wealth	Interest	UR	LTV
Levin-Lin-Chu	No constant	0.769	1.000	1.000	0.000	1.000	1.000	0.000	0.000	0.007	0.094
	With constant	0.000	0.000	0.000	0.000	0.000	0.000	0.893	0.000	0.000	0.000
	No means	0.000	0.000	0.215	0.388	0.003	0.023	0.967	0.000	0.000	0.000
Breitung	No constant	0.466	1.000	1.000	0.000	1.000	1.000	0.001	0.000	1.000	0.000
	With constant	0.000	1.000	0.962	0.080	1.000	0.895	0.003	0.011	0.985	0.097
	No means	0.000	0.820	0.001	0.006	1.000	0.622	0.003	0.006	0.820	0.016
	Robust	0.000	0.587	0.554	0.208	1.000	0.470	0.285	0.373	0.477	0.076
Im-Pesaran-Shin	Uncorr. errors	0.000	0.040	0.000	0.000	1.000	0.028	0.918	0.000	1.000	0.763
	No means	0.000	0.000	0.067	0.001	0.999	0.479	1.000	1.000	0.994	0.996
	Correl. errors	0.000	0.005	0.574	0.005	1.000	0.373	0.954	0.000	0.141	0.000
Fisher	ADF	0.000	0.452	0.001	0.004	1.000	0.098	1.000	0.000	0.066	0.000
	PP	0.000	0.318	0.000	0.000	1.000	0.039	0.995	0.000	0.998	0.007
	ADF (no means)	0.000	0.028	0.865	0.107	1.000	0.609	0.992	0.000	0.477	0.000
	PP (no means)	0.000	0.000	0.254	0.001	1.000	0.810	1.000	0.629	0.642	0.122
I(1) at the 1% level		14%	64%	64%	29%	86%	93%	71%	29%	79%	50%

Notes: The tests are based on the null hypothesis that the variables are I(1).

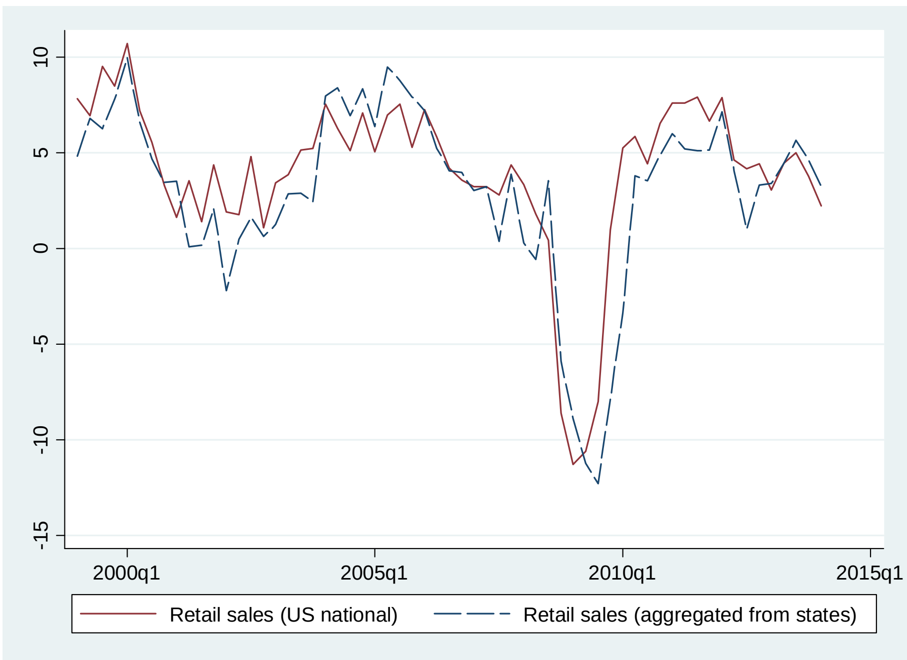
B Additional tables and figures

Figure B.1: Household debt-to-income ratio and private consumption over current and past business cycles



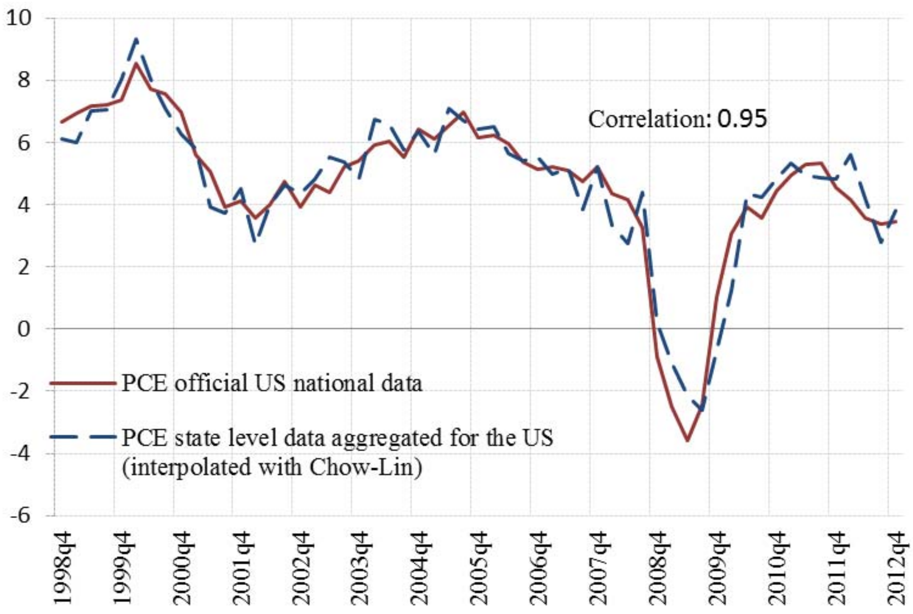
Source: Bureau of Economic Analysis, Federal Reserve Board and authors' calculations.
Notes: Zero marks the start of each recession, where the index assumes the value of 100. The x-axis refers to quarters. According to the NBER, there have been 10 recessions in the US since 1950, with the latest one starting in 2007Q4.

Figure B.2: US official retail sales and aggregated RS proxy (% year-on-year, nominal)



Source: US Census, authors' calculations.

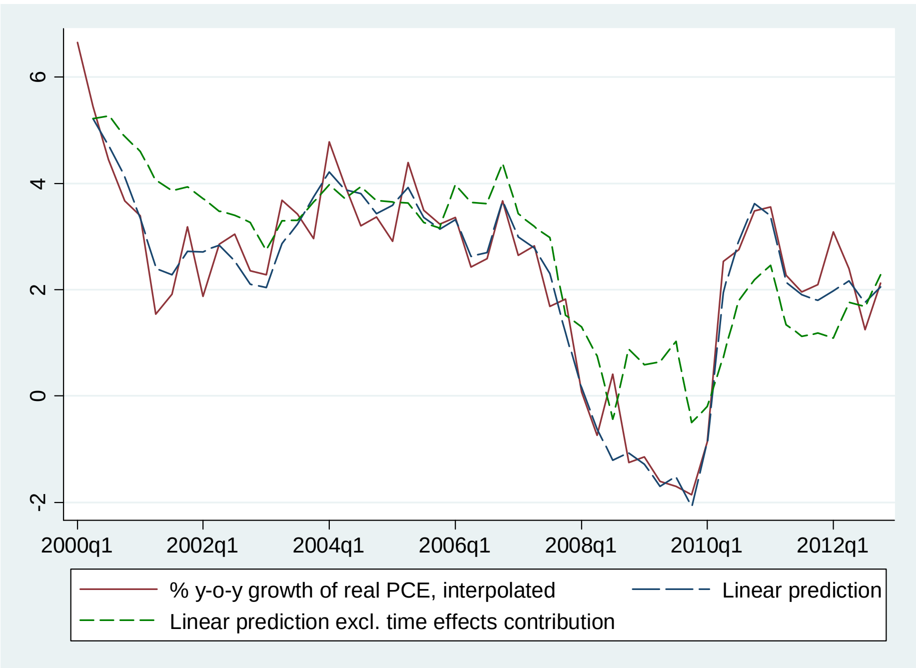
FIGURE B.3: US official PCE and state-aggregated, interpolated PCE (% year-on-year, nominal)



Source: BEA, authors' calculations.

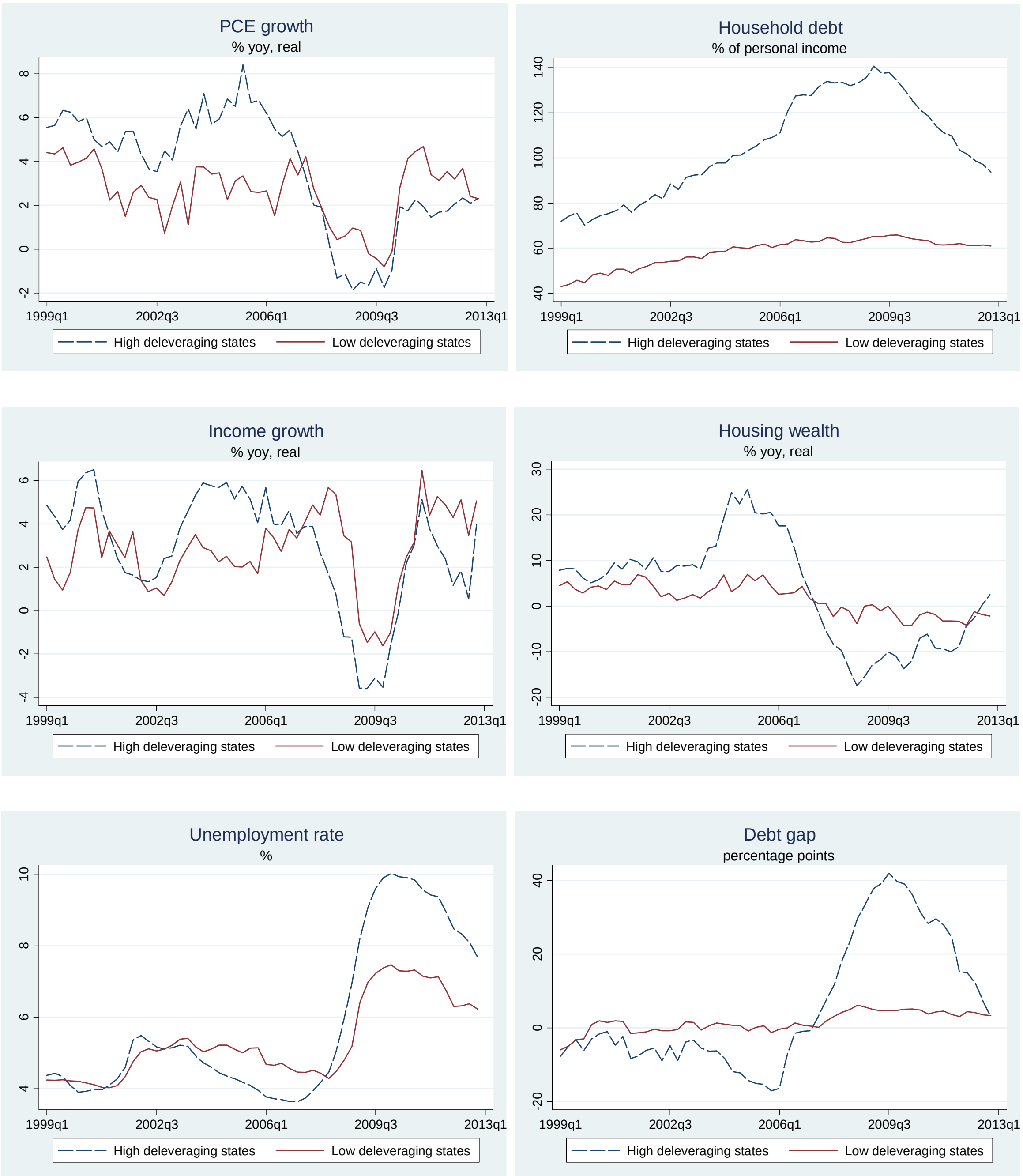
Note: The official national PCE data is the 2013 vintage since the experimental state-level PCE data (used for the interpolation) does not take into account the subsequent July 2014 national revisions to NIPAs.

FIGURE B.4: In-sample fit of US PCE growth from FE estimation (% year-on-year, real)



Source: BEA, authors' calculations.

Figure B.5: Average developments in economic indicators for high versus low deleveraging states



Note: "High deleveraging states" are those states that featured the largest declines in their household debt-to-income ratios between the peak for each state and 2012Q4, defined by the 90th percentile. These include Arizona, California, Florida, Hawaii, Nevada and South Dakota. The "low deleveraging states" are those that featured the smallest declines, defined as the 10th percentile and include Arkansas, Iowa, Kansas, Mississippi, North Dakota and West Virginia.

TABLE B.1: Fixed effects: Retail sales proxy

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Δ_4Wealth	0.103**	0.096**	0.102**	0.091**	0.094**	0.077*	0.075*
	(0.044)	(0.044)	(0.038)	(0.042)	(0.041)	(0.044)	(0.044)
Δ_4Income	0.861***	0.814***	0.840***	0.810***	0.824***	0.796***	0.797***
	(0.188)	(0.222)	(0.193)	(0.219)	(0.220)	(0.220)	(0.221)
Δ_4Debt$_{t-1}$		0.008		0.005	0.023	0.033	0.035
		(0.088)		(0.085)	(0.087)	(0.087)	(0.087)
Debt_gap$_{t-1}$			-0.009	-0.010	-0.017	-0.012	-0.012
			(0.032)	(0.034)	(0.035)	(0.035)	(0.036)
Interest					5.480	5.343	5.349
					(3.781)	(3.639)	(3.632)
Δ_4UR						-0.906	-0.911
						(0.751)	(0.755)
Δ_4LTV							-0.056
							(0.081)
Observations	2,576	2,346	2,530	2,346	2,346	2,346	2,346
States	46	46	46	46	46	46	46
R-Squared	0.220	0.228	0.223	0.228	0.231	0.234	0.234
Hausman	0.959	0.006	0.808	0.015	0.038	0.073	0.052
Wald t-statistic	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Friedman test	0.005	0.010	0.004	0.009	0.014	0.017	0.018

Notes: Fixed effects regressions with time dummies where the dependent variable is $\Delta_4$real retail sales (RS) proxy. Δ_4 denotes year-on-year % changes for housing wealth and income, and year-on-year change for debt-to-income, the unemployment rate and the LTV ratio. Robust heteroskedastic and autocorrelation-consistent standard errors shown in parentheses. The Hausman test reports p-values under the null hypothesis that the random effects estimator is both efficient and consistent. The Wald t-statistic is based on a joint test that the coefficients on the time dummies are equal to 0 under the null hypothesis. The Friedman test reports p-values under the null hypothesis of cross-sectional independence of the residuals based on Friedman (1937). Asterisks, *, **, ***, denote statistical significance at the 10, 5 and 1% levels.

C Online appendix

Robustness checks

We investigate the robustness and sensitivity of our main results along several dimensions. In the first part, we deal with potential econometric issues, whereas in the second part we focus mainly on interaction terms to uncover the existence of specific economic relationships. As a benchmark, we choose the FE specification in column (7) of Table 1.2 in the main text.

Starting with the first part, one issue that we are particularly concerned about is the potential endogeneity bias. We deal with this by resorting to the Instrumental Variables (IV) estimator, where we instrument the key explanatory variables – housing wealth, income, debt-to-income and the debt gap – with two lags of the corresponding variables, as is commonly done in the literature. The IV estimation results shown in column (2) of Table C.1 suggest that endogeneity is not a serious problem in our FE regression since the results remain qualitatively unchanged.

We cross-check our baseline results by (i) employing alternative methods that control for autocorrelation, cross-sectional correlation and heteroskedasticity across panels and (ii) taking into account the dynamics in the dependent variable. On (i), we broadly obtain the same results when we employ alternative methods that allow for autocorrelation within the panels and for cross-sectional correlation and heteroskedasticity across panels, namely the Generalised Least Squares (GLS) and Driscoll-Kraay estimators – columns (3) and (4), respectively. On (ii), by construction, standard dynamic panel-data model estimators are inconsistent because the error terms are correlated with the lagged dependent variable. For this reason, we resort to the Arellano-Bover/Blundell-Bond estimator, using a Generalised Method of Moments (GMM) that corrects for that bias (Arellano and Bover, 1995; Blundell and Bond, 1998). The estimator yields similar results in terms of the long-term impact of the main variables of interest as the standard FE estimates in the main text, which only reports the short-run effects.[18] Furthermore, the debt gap continues to be significant in this type of dynamic model.

Moving to the second part of the analysis, we first make use of net housing wealth (gross housing wealth minus mortgage debt) instead of gross housing wealth. The estimates in

[18] The long-term effect of a variable in this case is its short-run coefficient divided by 1 minus the lagged coefficient on the dependent variable. For housing wealth, in our case 0.043/(1-0.436)=0.076.

column (2) of Table C.2 show that the results are broadly consistent with those shown in the main text. In this case, the positive coefficient on debt roughly doubles in size. This might be related to the fact that we are modelling the effect on consumption from the net worth side and the liability side of (housing) wealth separately.

TABLE C.1: Robustness checks

	(1) Baseline	(2) IV	(3) GLS	(4) XTSCC	(5) AB-BB
Δ_4Wealth	0.090***	0.112***	0.067***	0.090***	0.043***
	(0.011)	(0.011)	(0.001)	(0.017)	(0.012)
Δ_4Income	0.285***	0.336***	0.241***	0.285***	0.184***
	(0.025)	(0.029)	(0.003)	(0.027)	(0.021)
Δ_4Debt$_{t-1}$	0.020*	0.023*	0.014***	0.020**	0.009
	(0.011)	(0.010)	(0.001)	(0.008)	(0.007)
Debt_gap$_{t-1}$	-0.019*	-0.013*	-0.021***	-0.019**	-0.012*
	(0.011)	(0.005)	(0.001)	(0.008)	(0.007)
Interest	0.525	0.976***	-0.057	0.525	0.255
	(0.510)	(0.344)	(0.044)	(0.556)	(0.420)
Δ_4UR	-0.283***	-0.241***	-0.262***	-0.283***	-0.174***
	(0.073)	(0.052)	(0.010)	(0.058)	(0.042)
Δ_4LTV	-0.011	-0.004	-0.015***	-0.011	-0.010
	(0.028)	(0.017)	(0.003)	(0.020)	(0.022)
Lagged PCE					0.436***
					(0.072)
Observations	2,601	2,499	2,601	2,601	2,601
States	51	51	51	51	51
R-Squared	0.638	-	-	0.638	-

Notes: The dependent variable is $\Delta_4$real PCE. Δ_4 denotes year-on-year % changes for housing wealth and income, and year-on-year change for debt-to-income, the unemployment rate and the LTV ratio. Robust heteroskedastic and autocorrelation-consistent standard errors shown in parentheses. The Instrumental Variables (IV) estimation instruments *Wealth*, *Income*, *Debt* and *Debt_gap* with 2 lags of their own variables. The GLS allows estimation in the presence of an heteroskedastic error structure with cross-sectional correlation and AR(1) autocorrelation within panels. XTSCC refers to Driscoll-Kraay standard errors robust to heteroskedasticity and general forms of cross-sectional and temporal-dependence. AB-BB is the the Arellano-Bover/Blundell-Bond estimator. Time dummies are included in all specifications. Asterisks, *, **, ***, denote statistical significance at the 10, 5 and 1% levels.

Furthermore, we explore the role of (excessive) debt service as a potential channel through which (de)leveraging and the debt overhang may suppress consumption – column (3) and column (4) of Table C.2. Indeed, payments to service the stock of outstanding debt divert resources away from disposable income – becoming a form of forced saving – and, as such, act to tighten the budget constraint of households (see Hall, 2011).[19] To proxy for the effects of debt service and excessive debt service we interact the interest rate with the debt-to-income ratio and with the debt gap. While we do not find a statistically significant effect for the

[19]In the conceptual framework described by Hall (2011), a portion of credit-constrained households would choose to borrow up to the maximum limit allowed by lenders, in which case their per-period consumption would equal per-period available funds. These funds will be given by income plus the increased borrowing over the period, less debt service payments on the stock of debt from the previous period.

overall debt service ratio (column 3), it is interesting to note that substituting the debt gap with a measure of the excessive debt service burden (column 4) results in a negative and statistically significant coefficient. A 1-percentage-point excessive debt service burden at the end of the previous period, defined as debt payments as a share of income exceeding the sustainable level, is estimated to reduce annual consumption growth by 0.5 percentage points in the next period. The results support the notion that a meaningful channel through which excessive indebtedness interacts with consumption is by soaking up resources away from overly indebted households through debt service payments.

Table C.2: Sensitivity analysis

	(1) Baseline	(2) Net wealth	(3) Debt serv. ratio (DSR)	(4) Excess DSR	(5) Income uncert.
Δ_4Wealth	0.090***		0.090***	0.090***	0.095***
	(0.011)		(0.011)	(0.012)	(0.011)
Δ_4Net Wealth		0.038***			
		(0.005)			
Δ_4Income	0.285***	0.300***	0.285***	0.285***	0.292***
	(0.025)	(0.031)	(0.025)	(0.025)	(0.025)
$\Delta_4\text{Debt}_{t-1}$	0.020*	0.042***	0.021*	0.022**	0.016
	(0.011)	(0.015)	(0.012)	(0.010)	(0.010)
Debt_gap_{t-1}	-0.019*	-0.020	-0.019*		-0.020*
	(0.011)	(0.012)	(0.011)		(0.011)
Interest	0.525	0.755	0.538	0.500	0.570
	(0.510)	(0.520)	(0.575)	(0.502)	(0.506)
Δ_4UR	-0.283***	-0.314***	-0.282***	-0.267***	
	(0.073)	(0.076)	(0.078)	(0.074)	
Δ_4LTV	-0.011	-0.009	-0.011	-0.010	-0.009
	(0.028)	(0.027)	(0.027)	(0.028)	(0.028)
$\Delta_4(\text{Int*Debt}_{t-1})$			-0.028		
			(0.213)		
$\text{Int*Debt\_gap}_{t-1}$				-0.490**	
				(0.191)	
Income volat.					-0.029
					(0.028)
Observations	2,601	2,499	2,601	2,601	2,601
States	51	49	51	51	51
R-squared	0.638	0.604	0.638	0.639	0.634

Notes: Fixed effects regressions with time dummies where the dependent variable is $\Delta_4$real PCE. The interaction term in column (3) proxies for changes in the debt service ratio and the term in column (4) proxies for the excessive debt service ratio. Column (5) uses an alternative proxy for income uncertainty, income volatility, defined as the absolute value of the discrepancy between the current income growth and its moving average over the previous five years (including current year). Time dummies are included in all specifications. Asterisks, *, **, ***, denote statistical significance at the 10, 5 and 1% levels.

Finally, our baseline estimation results did not change meaningfully when we used an alternative measure of income uncertainty (column 5).[20] Overall, the coefficients of the key

[20] Along the lines of Aron et al. (2012), we measure income volatility as the absolute value of the discrepancy between the current income growth and its moving average over the previous five years (including current year).

variables of interest – wealth, income, debt and the debt gap – remain overall robust to the aforementioned modifications of our baseline specification.

Non-linearities

We check for the possibility that the impact of the key variables of interest – debt and debt gap – might be non-linear (Table C.3). We do this by augmenting the standard regression with quadratic terms of the two variables (column 2 and column 3). In addition, in column (4) and column (5), we shed more light on the potential non-linearities of the impact the leveraging and deleveraging period have on consumption as well as when the debt gap is positive or negative.

TABLE C.3: Non-linearities

	(1) Baseline	(2) Δ_4Debt squared	(3) Debt_gap squared	(4) Lever.	(5) Gap<0
Δ_4Wealth	0.090***	0.091***	0.092***	0.091***	0.092***
	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
Δ_4Income	0.285***	0.282***	0.282***	0.281***	0.283***
	(0.025)	(0.024)	(0.025)	(0.024)	(0.025)
Δ_4Debt$_{t-1}$	0.020*	0.025**	0.018*	0.060**	0.019*
	(0.011)	(0.012)	(0.010)	(0.026)	(0.010)
Debt_gap$_{t-1}$	-0.019*	-0.016	-0.01	-0.016	-0.025***
	(0.011)	(0.011)	(0.014)	(0.011)	(0.010)
Interest	0.525	0.565	0.585	0.575	0.601
	(0.510)	(0.501)	(0.496)	(0.496)	(0.501)
Δ_4UR	-0.283***	-0.291***	-0.268***	-0.297***	-0.273***
	(0.073)	(0.073)	(0.072)	(0.073)	(0.073)
Δ_4LTV	-0.011	-0.012	-0.009	-0.011	-0.010
	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)
$(\Delta_4\text{Debt})^2{}_{t-1}$		-0.001*			
		(0.001)			
Debt_gap$^2{}_{t-1}$			-0.000		
			(0.000)		
Δ_4Debt$_{t-1} > 0$				-0.065**	
				(0.032)	
Debt_gap$_{t-1} < 0$					0.027
					(0.019)
Observations	2,601	2,601	2,601	2,601	2,601
States	51	51	51	51	51
R-squared	0.638	0.639	0.639	0.640	0.639

Notes: Fixed effects regressions with time dummies where the dependent variable is $\Delta_4$real PCE. Columns (2) and (3) test for non-linearities in the effect of the change in the debt-to-income ratio and level of the debt gap by augmenting the regressions with the respective variables squared. Column (4) and column (5) take into account the potential non-linearities of the impact on consumption from the leveraging and deleveraging period, by adding a dummy that assumes the value of 1 for periods where the debt ratio was rising, column (4), and a dummy that assumes the value of 1 when the debt gap was negative, column (5). Time dummies are included in all specifications. Asterisks, *, **, ***, denote statistical significance at the 10, 5 and 1% levels.

Column (2) of Table C.3 reports a negative coefficient on the quadratic term of the debt variable at the 10% significance level. This is tentative evidence that leveraging and deleveraging might have non-linear effects on consumption. While the support to consumption growth from the debt-accumulating process diminishes as the speed of leveraging picks up, when deleveraging occurs, the larger the pace of debt reduction, the more negative the effect on consumption becomes. The regression results from column (4) suggest, however, that the rise in debt is not associated with an increase in consumption expenditures over and beyond the impact through the traditional wealth channel, and the debt gap appears only to matter for consumption growth when actual debt is above equilibrium debt, yielding a positive debt gap (column 5). Nevertheless, these results should be considered with reservation, given that we only have a limited number of observations for the deleveraging period and for when debt gap is below zero.

Error-correction framework

In this section we extend the empirical analysis by placing more focus on the long-term dynamics. Given that the literature on the traditional consumption function assumes that there is a stable long-term relationship between consumption, wealth and income, as in Fernandez-Corugedo et al. (2007), we want to investigate whether our extended consumption function also exhibits a stable relationship in the long run. We do this by testing for cointegration between the variables used in our framework.

The results from the panel cointegration tests by Westerlund (2007) support the case of the existence of a cointegrating relationship between consumption, wealth, income, debt, and the debt gap (Table C.4).

Table C.4: Panel cointegration tests

Variables	Test	Value	p-value[a]	p-value[b]
PCE, Wealth, Income and Debt	G_τ	-18.018	0.000	0.000
	G_α	-6.088	0.000	0.000
	P_τ	-13.432	0.000	0.000
	P_α	-9.551	0.000	0.000
PCE, Wealth, Income, Debt and Debt gap	G_τ	-20.743	0.000	0.000
	G_α	-4.017	0.000	0.000
	P_τ	-15.204	0.000	0.000
	P_α	-6.534	0.000	0.000
PCE, Net wealth, Income and Debt	G_τ	-17.934	0.000	0.000
	G_α	-6.361	0.000	0.000
	P_τ	-13.021	0.000	0.000
	P_α	-9.782	0.000	0.000
PCE, Net wealth, Income, Debt and Debt gap	G_τ	-20.056	0.000	0.000
	G_α	-4.005	0.000	0.000
	P_τ	-14.738	0.000	0.000
	P_α	-6.967	0.000	0.000

Notes: The results are for the four panel cointegration tests developed by Westerlund (2007). The null hypothesis is no cointegration. PCE, Income, Wealth and Net wealth are in logs. All tests are implemented with a constant in the regression. Lags and leads in the error correction test are chosen according to the Akaike criterion. See Persyn and Westerlund (2008) for further details.
[a] p-values are based on the normal distribution.
[b] p-values based on the bootstrapped distribution (500 bootstrap replications used), which are robust to the presence of cross-sectional dependence.

We proceed to estimate an error-correction model by using the PMG estimator developed by Pesaran et al. (1999), which assumes identical long-run coefficients across states but allows for a differentiated response to short-term factors, depending on state-specific characteristics. The standard PMG is based on the following specification:

$$\Delta C_{it} = \mu_i + \phi_i(C_{i,t-1} - \theta X_{it}) + \delta_i \Delta X_{it} + \gamma_i \Delta Z_{it} + u_{it} \tag{1.1}$$

where $X_{it} = \begin{bmatrix} Wealth_{it} \\ Income_{it} \\ Debt_{i,t-1} \\ Debt\_gap_{i,t-1} \end{bmatrix}$, $Z_{it} = \begin{bmatrix} Interest_{it} \\ LTV_{it} \\ UR_{it} \end{bmatrix}$, $\theta = \begin{bmatrix} \theta_1 \\ . \\ . \\ \theta_4 \end{bmatrix}$, $\delta_i = \begin{bmatrix} \delta_{i1} \\ . \\ . \\ \delta_{i4} \end{bmatrix}$ and $\gamma_i = \begin{bmatrix} \gamma_{i1} \\ \gamma_{i2} \\ \gamma_{i3} \end{bmatrix}$

where C is the logarithm of real PCE, X and Z are, respectively, the four main explanatory variables and the three controls used previously in the FE estimation, θ are the long-run coefficients, δ the short-run ones, and ϕ is the speed of adjustment with an expected negative sign. The variables expressed in constant dollar terms – *Wealth* and *Income* – are transformed into logarithms, while the other variables, which are already in percent units, are left unchanged. The delta operator refers to quarter-on-quarter annualised changes.[21]

The standard PMG estimator assumes that the errors u_{it} are independently distributed across states. In reality, however, cross-sectional dependence is often the norm rather than the exception. As discussed in Pesaran (2006), interdependencies at the cross-sectional level might result from the error term u_{it} being affected by unobserved common factors f_t with possibly idiosyncratic factor loadings λ_i:

$$u_{it} = \lambda_i' f_t + \varepsilon_{it} \tag{1.2}$$

When the unobserved common factors are correlated with the explanatory variables, ignoring them would lead to spurious inferences based on the standard PMG estimator. Performing the CD test based on Pesaran (2004) on the residuals from the standard PMG specification in Equation 1.1 suggests that cross-sectional dependence might be an issue.

[21] All variables are in seasonally adjusted terms, but PCE. We minimise the potential issue of seasonality in PCE growth by smoothing it out through 4-quarter moving averages. In addition, we estimate our model on annual data and find that the main results remain qualitatively unchanged (not reported).

Indeed, the null hypothesis of cross-sectional independence is rejected at high levels of significance (Table C.5). Given the type of data and period that we are covering, examples of common unobserved factors in our case could be the housing boom and the subsequent bust, the 2007-09 financial crisis or changes in sales tax rates across states that are not captured by our explanatory variables in the model.

Table C.5: PMG estimation

	(1)	(2)	(3)	(4)
Long-run				
Log Wealth	0.098***		0.065***	
	(0.016)		(0.013)	
Log Net Wealth		0.048***		0.030***
		(0.009)		(0.007)
Log Income	0.768***	0.822***	0.745***	0.782***
	(0.017)	(0.017)	(0.014)	(0.011)
Debt_{t-1}	-0.147***	-0.103***	-0.095***	-0.059***
	(0.027)	(0.026)	(0.018)	(0.013)
Debt_gap_{t-1}	-0.134***	-0.160***		
	(0.027)	(0.031)		
Speed of Adjustment	-0.244***	-0.234***	-0.288***	-0.287***
	(0.004)	(0.003)	(0.028)	(0.028)
Short-run				
ΔDebt_{t-1}	0.000	-0.001	0.006**	0.005**
	(0.006)	(0.006)	(0.002)	(0.003)
$\Delta\text{Debt\_gap}_{t-1}$	0.010	0.011		
	(0.007)	(0.007)		
Debt_gap_{t-1}			-0.016	-0.021
			(0.014)	(0.015)
Constant	0.385***	0.360***	0.634***	0.624***
	(0.021)	(0.018)	(0.057)	(0.057)
Observations	2,754	2,646	2,754	2,646
CD test	0.000	0.000	0.000	0.000
Hausman test	0.030	0.051	0.017	0.054

Notes: Estimates with the PMG estimator. The dependent variable is the logarithm of the four-quarter moving average (4MA) of real PCE. The differenced variables are in quarterly annualised terms and hence the speed of adjustment is reported on an annual basis. The specifications with net housing wealth exclude Nevada and South Dakota. Standard errors shown in parentheses. Asterisks, *, **, ***, denote statistical significance at the 10, 5 and 1% levels. The CD test based on Pesaran (2004) reports p-values under the null hypothesis that the model exhibits cross-sectional independence of the residuals. The Hausman test compares the PMG with the Mean Group (MG) estimator and reports p-values under the null hypothesis that the PMG estimator is both efficient and consistent, i.e., that the long-run homogeneity restriction in the PMG is valid.

To correct for this problem, we make use of the Common Correlated Effects Pooled Mean Group (CCEPMG) estimator. The approach consists of augmenting the standard PMG with cross-sectional averages of the variables as additional regressors, which allows the effects from the unobserved common factors to be filtered out (see, for example, Pesaran, 2006; Chudik and Pesaran, 2015; Albuquerque et al., 2015). More specifically, we estimate the following equation:

$$\Delta C_{it} = \mu_i + \phi_i(C_{i,t-1} - \theta X_{it}) + \delta_i \Delta X_{it} + \gamma_i \Delta Z_{it} + \alpha_i \overline{C}_t + \beta_i \overline{X}_t + \lambda_i \Delta \overline{C}_t + \eta_i \Delta \overline{X}_t + \tau_i \Delta \overline{Z}_t + \varepsilon_{it} \tag{1.3}$$

where $\overline{C}_t$, $\overline{X}_t$ and $\overline{Z}_t$ are averages of the dependent variable and the regressors across states, computed at every time period t.

The four columns of Table C.6 present the estimates from Equation 1.3, with column (1) and column (2) using gross and net housing wealth, respectively, while column (3) and column (4) use the same structure but exclude the debt gap from the long run.[22] The CCEPMG estimates show that all the long-run coefficients are highly statistically significant and have the expected sign.[23] In particular, a 10% increase in gross housing wealth would lead to an increase in real PCE of 0.5%, whereas it is harder to uncover a statistically significant effect from net housing wealth. Furthermore, the long run elasticity of consumption to income is found to be between 0.7 and 0.8. The total debt ratio is highly statistically significant in all specifications. One interpretation is that, in the long run, the misalignments of debt from its equilibrium should be closed, therefore suggesting that this estimate shows the effects of a permanent increase in equilibrium debt. Finally, in column (1) and column (2), the debt gap is highly significant in the long run, exerting a downward force on consumption, which supports our previous results.[24]

If one believes that the debt gap should be closed in the long run, implying that deviations of the debt ratio from an estimated equilibrium level driven by economic fundamentals cannot persist indefinitely, then one can argue that the debt gap should be excluded from the long-run specification. We do that in column (3) and column (4). Overall, the results remain broadly similar, with the short-run negative effect of the debt gap being strongly significant, suggesting that increases in the debt gap weigh on consumption growth in the short run. The debt ratio has a positive sign in the short run, implying a negative effect from deleveraging

[22] Pesaran et al. (1999) show that the PMG estimator remains valid in the presence of regressors with order of integration of both I(0) and I(1).

[23] Moreover, the speed of adjustment is negative and statistically significant in all specifications, which supports the cointegration hypothesis between consumption and the set of long-term determinants included in the model.

[24] The CD test suggests that the null hypothesis of cross-sectional independence in the residuals cannot be firmly rejected in the case of CCEPMG once cross-section averages of the variables are used as additional regressors. In particular, for all specifications from columns (1) to (4) we can no longer reject the null hypothesis at the 1% significance level. This suggests that the CCEPMG augmentation represents a significant improvement over the standard PMG model in terms of dealing with cross-sectional dependence.

on consumption growth.

TABLE C.6: CCEPMG estimation

	(1)	(2)	(3)	(4)
Long-run				
Log Wealth	0.048***		0.048***	
	(0.015)		(0.013)	
Log Net Wealth		0.005		0.010
		(0.007)		(0.006)
Log Income	0.728***	0.806***	0.764***	0.788***
	(0.042)	(0.044)	(0.039)	(0.041)
Debt_{t-1}	0.238***	0.268***	0.232***	0.203***
	(0.033)	(0.031)	(0.029)	(0.027)
Debt_gap_{t-1}	-0.329***	-0.343***		
	(0.036)	(0.035)		
Speed of Adjustment	-0.481***	-0.469***	-0.514***	-0.529***
	(0.072)	(0.068)	(0.079)	(0.073)
Short-run				
ΔDebt_{t-1}	0.019*	0.026**	0.009*	0.009***
	(0.010)	(0.010)	(0.004)	(0.004)
$\Delta\text{Debt\_gap}_{t-1}$	-0.008	-0.014		
	(0.010)	(0.011)		
Debt_gap_{t-1}			-0.134***	-0.110***
			(0.028)	(0.027)
Constant	-0.244	-0.410	-0.187	-0.274
	(0.292)	(0.252)	(0.320)	(0.269)
Observations	2,754	2,646	2,754	2,646
CD test	0.014	0.014	0.013	0.026
Hausman test	0.270	0.516	0.172	0.630

Notes: Estimates with the common correlated effects specification of the PMG estimator – CCEPMG. The dependent variable is the logarithm of the four-quarter moving average (4MA) of real PCE. The differenced variables are in quarterly annualised terms and hence the speed of adjustment is reported on an annual basis. The specifications with net housing wealth exclude Nevada and South Dakota. For the CCEPMG, we include the following cross-section averages: dependent variable, housing wealth, income, debt-to-income ratio, the debt gap and LTV ratio. Standard errors shown in parentheses. Asterisks, *, **, ***, denote statistical significance at the 10, 5 and 1% levels. The CD test based on Pesaran (2004) reports p-values under the null hypothesis that the model exhibits cross-sectional independence of the residuals. The Hausman test compares the PMG with the Mean Group (MG) estimator and reports p-values under the null hypothesis that the PMG estimator is both efficient and consistent, i.e., that the long-run homogeneity restriction in the PMG is valid.

The joint analysis of the coefficient on the debt ratio and the debt gap leads to some interesting results about the impact of household indebtedness on consumption over the long term. In particular, and similarly to what was found previously with the FE estimations, the nature of the indebtedness determines whether debt has a positive or negative impact on consumption. Debt accumulation can support consumption growth over the long run, as long as there is no disequilibrium, in the sense that the level of actual debt is in line with its estimated equilibrium debt. In the same spirit, deleveraging per se is not necessarily harmful for consumption growth over the long run, as long as it serves to correct for excessive levels of debt. To make our case clearer, if one were to assume that an increase in household debt would be the result of improving economic conditions that support an increase in equilibrium debt, then the debt gap would remain unchanged. In this scenario, the impact of debt would be in line with the benign view on debt described in the literature review: a 10-percentage-point increase in the debt-to-income ratio would lead to higher consumption by roughly 2 to 3% in the long term.

If, however, the accumulation of debt is not supported by a rise in the debt capacity of households, but by a corresponding rise in the debt gap – if equilibrium debt were to remain unchanged – then based on column (1) and column (2) the same 10-percentage-point increase in the debt-to-income ratio would lower consumption by a similar amount in the long term, thereby offsetting the positive impetus from the leveraging process.

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Chapter 2

One Size Fits All? Monetary Policy and Asymmetric Household Debt Cycles in U.S. States

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Abstract*

I investigate the non-linear effects of monetary policy through differences in household debt across US states. After constructing a novel indicator of inflation for the states, I compute state-specific monetary policy stances as deviations from an aggregate Taylor rule. I find that the effectiveness of monetary policy is curtailed during periods of large household debt imbalances. Moreover, a common US monetary policy *does not fit all*; it may have asymmetric effects on the economic performance across states, particularly at times of high dispersion in the household debt imbalances, as it may have been the case around the Great Recession.

Keywords: *Monetary policy, Household debt, Regional asymmetries, Local Projections, Taylor rule*
JEL classification: *C33, E32, E52, G21*

Introduction

According to the theory of optimum currency areas by Mundell (1961) and McKinnon (1963), the costs from losing monetary policy autonomy can be particularly large when countries within a monetary union find themselves in a different phase of the business cycle. While the

*This chapter is a forthcoming publication in the *Journal of Money, Credit and Banking.*

euro area often comes to mind when thinking about the adequacy of a single interest rate for its members, the United States is also an interesting case to analyse as economic conditions vary significantly across states.[1] For instance, the cross-state dispersion in unemployment rates, real GDP growth, household debt, and real housing wealth growth is substantial (Figure 2.0.1). The dispersion was remarkably high during the crisis period, particularly for the household debt-to-income ratio, ranging from 62% (West Virginia) to 152% (California) at the peak of the crisis.

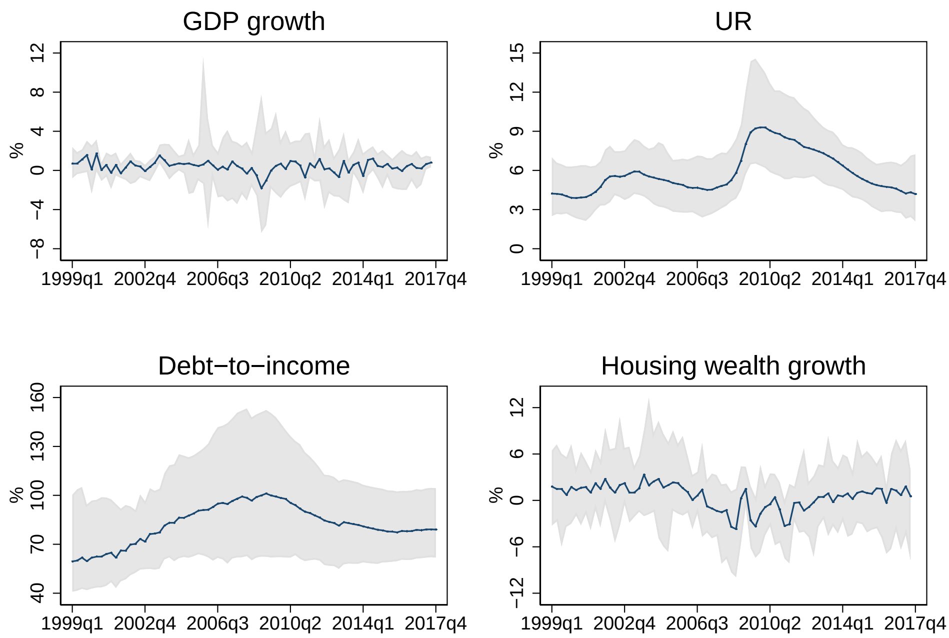
The Federal Reserve (Fed) carries out monetary policy with a dual mandate of price stability and full employment for the country *as a whole* and not for a particular state. But to the extent that business cycles are not perfectly synchronised across states, divergent developments in inflation and economic growth may actually require a differentiated monetary policy stance. Since, by construction, this is not feasible, the question is the extent to which a single monetary policy may amplify on-going trends, and thus accentuate existing regional differences. Along these lines, by relying on a common monetary policy shock, Carlino and DeFina (1998b) and Carlino and DeFina (1999) find that monetary policy has significant asymmetric effects on personal income across US regions and states. By contrast, I do not focus on aggregate monetary policy shocks, but on the implicit stance of monetary policy for each state, given that economic and financial conditions differ widely across regions and states.

One of the candidates to explain how monetary policy may transmit differently to the US states is household debt. The choice of household debt to study the state-dependent effects of monetary policy is underscored by the considerable cross-state heterogeneity in household debt levels and dynamics over the last two decades, coupled with a significant divergence in economic performance between states with high and low household debt over the same period: while states like California and Florida went through a damaging boom-bust cycle, others such as Texas, Indiana and Ohio, did not observe large swings in household debt (and house prices), and weathered the crisis relatively well. While recent research has found that high household debt, household debt build-ups or excessive borrowing are detrimental to future economic growth, such as Albuquerque and Krustev (2018), Jordà et

[1] For example, Carlino and DeFina (1998a), Dornbusch et al. (1998), and Mihov (2001) find significant heterogeneity in GDP and inflation across euro area countries in response to a common monetary policy shock.

al. (2013), Mian and Sufi (2010), Mian and Sufi (2011), and Mian et al. (2017), and increase the probability of a financial crisis (Jordà et al., 2015), there are only a few recent papers exploring the non-linear interactions between the monetary transmission mechanism and the level of household indebtedness (Alpanda and Zubairy, 2018; Aikman et al., 2019). Moreover, little is known about the role that a common monetary policy might play in exacerbating regional asymmetries between states with different levels of household debt.

FIGURE 2.0.1: Cross-state distribution of selected variables



Notes: The grey area captures the range of values at each point in time for each variable across a sample of 30 US states used throughout the paper. The solid blue line represents the mean sample values.

The literature has not, in effect, reached a consensus on the effectiveness of monetary policy conditional on different household debt levels. On the one hand, monetary policy may be *more effective* when household debt is *high* since Bernanke et al. (1999)'s 'financial accelerator' implies that looser monetary policy stimulates house prices and consequently borrowers may increase consumption by extracting more equity from their houses. In addition, monetary policy may also be more powerful when debt is high, as highly-indebted households are typically associated with a high marginal propensity to consume (Hedlund et al., 2016). On the other hand, monetary policy may be *less effective* if households are reluctant to take on more debt when their indebtedness is already high, or if borrowing constrains are binding, along the lines of the debt overhang theory of Eggertsson and Krugman (2012). In addition, the home equity loan channel may be less operational around periods of high debt, which prevents households from borrowing against their homes (Alpanda and Zubairy, 2018).

The notion of 'excessive' or 'high' household debt is a subjective concept, as the level to which debt is deemed to be sustainable is not observed by the policy maker or the econometrician. Albuquerque et al. (2015) try to fill this gap by computing a measure of equilibrium household debt for the US states determined by economic fundamentals, while Aikman et al. (2019) and Alpanda and Zubairy (2018) follow a more conventional practice of using the Hodrick-Prescott (HP) filter to estimate the trend and the gap of household debt for the US aggregate. In this paper I use the concept of a debt gap in the spirit of the latter two papers but, differently from them, I make advantage of the panel dataset by looking at the full distribution of household debt by time and state. In particular, I compute for each US state the deviation of the debt-to-income ratio from its long-term trend derived with the new method of detrending non-stationary data developed by Hamilton (2018). I then define high and low debt periods for each state as those periods belonging to the top or first quintile of their debt-to-income gap distribution. By having state-specific debt periods, I do not impose that all US states should have been in a high debt period in the run-up to, and during, the Great Recession, as suggested by aggregate data in Alpanda and Zubairy (2018), and Aikman et al. (2019).

Against this background, the aim of the paper is twofold. First, I investigate the extent to which the underlying state-specific monetary policy stance may affect the dynamics in economic and financial variables in a non-linear fashion during periods of large imbalances in household debt. Second, I study the interaction between monetary policy and regional asymmetries, conditional on the size of the imbalances in household debt across states. I focus on debt in the household sector as Jordà et al. (2015) document that the strong rise in private debt in several Western countries in the second half of the 20^{th} century has been driven mainly by credit to households, particularly mortgage debt. The paper falls into two different strands of the literature: (i) the interaction between monetary policy, household debt and the macroeconomy, such as Aikman et al. (2019), Alpanda and Zubairy (2018), Bauer and Granziera (2017), Bhutta and Keys (2016), Di Maggio et al. (2017), and Jordà et al. (2015); and (ii) the relationship between monetary policy and regional asymmetries, such as Beraja et al. (2019), and Carlino and DeFina (1998b) and Carlino and DeFina (1999).

Using a novel state-level dataset that combines data on economic activity and debt in the household sector, I apply Jordà (2005)'s Local Projection (LP) method to a panel of 30 US states over 1999-2017 to study the sensitivity of household debt and other macro variables to state-specific monetary policy conditions, placing the focus on the non-linear relationship between monetary policy, household indebtedness and regional asymmetries. Specifically, the measure of monetary policy stance for the states, the Monetary Policy Stance Gap (MPSG), is computed as the difference between the interest rate prescribed by the Taylor rules for each state and the one from the US aggregate. I take the estimated coefficients from an US aggregate Taylor rule estimated on real-time expectations data to generate the Taylor rules for the states, therefore assuming the same central bank's reaction function for all states. To compute the Taylor rules for the states, I construct a novel indicator of consumer prices at the state level, by drawing on official Consumer Price Index (CPI) data for several Metropolitan Statistical Areas (MSA).

The main findings of the paper suggest that: (i) the transmission of monetary policy to the real economy is curtailed during periods of large imbalances in household debt; and (ii) a common monetary policy in the United States *does not fit all*, in that monetary policy may have asymmetric effects on the household debt dynamics and economic performance across states in periods characterised by high dispersion in household debt imbalances.

Regarding the first finding, while a looser state-specific monetary policy stance is supportive of borrowing and growth during periods of low household debt gaps (relative to an estimated state-specific trend), this is only the case in the short term during periods of high household debt. In fact, a loosening in the relative monetary policy stance is associated with a decline in economic growth over the medium term. My estimates suggest that a one-standard deviation increase in the state-specific monetary policy stance leads to lower real GDP of 1.7 p.p. in periods of high debt compared to periods of low debt over five years.

I hypothesise that lower economic growth in periods of high debt after a loosening in the state-specific monetary policy stance appears to be related to the need of households to deleverage from excessive credit. Since households in these periods were already highly indebted to begin with, more borrowing in the short run may place debt at even higher levels relative to income, 'forcing' households to deleverage and cut back on consumption expenditures, along the lines of the debt overhang theory of Eggertsson and Krugman (2012).

At the same time, I also find that house prices do not increase in these periods characterised by large imbalances in household debt, making it harder for households to take advantage of the home equity loan channel to extract more equity from their homes or to refinance their mortgages, as suggested by Alpanda and Zubairy (2018). In contrast, I find that looser monetary policy conditions at the state level are effective in fostering growth and borrowing in periods of low debt. For example, house prices and housing wealth rise consistently over the whole horizon, which may support borrowing through the home equity channel, in line with the findings of Bhutta and Keys (2016) that easier monetary conditions lead households to extract more equity from their homes.

Along the same lines, I find that monetary policy may also have asymmetric effects across states on household debt dynamics and economic performance in periods characterised by high dispersion in the imbalances in household debt. In particular, I find that an increase in the MPSG leads to a decline after five years (relative to the average state in the sample) in the household debt ratio, housing wealth and real GDP for the states that had the largest debt gaps at the start of the Great Recession in 2008q1. In this context, my results indicate that monetary policy during the last recession may have been particularly ineffective, perhaps even counter-productive, in stimulating growth in the states with the largest debt gaps, which were precisely those states that were going through a severe boom-bust cycle.

The main findings remain robust to alternative specifications for the Taylor rule, from which I derive the state monetary policy stances: by using Wu and Xia (2016)'s shadow rate to deal with unconventional monetary policy during the zero lower bound (ZLB) on nominal interest rates; when accounting for the financial cycle; by using the unemployment gap as an alternative slack measure; and by estimating a Taylor rule on actual data.

2.1 State-level CPI

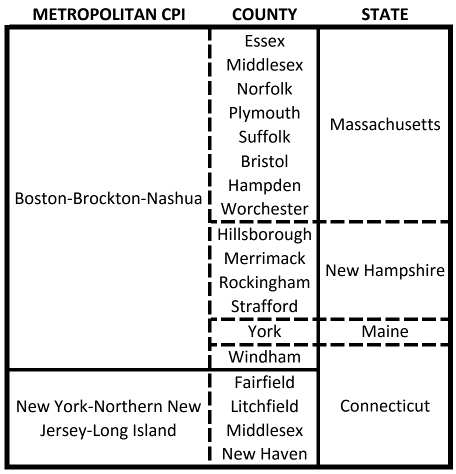
The stance of monetary policy is typically assessed by monetary rules, of which Taylor (1993) and Taylor (1999) rules are the most popular ones. To compute these rules for the US states, I need a measure of consumer prices and slack in the economy of each state. While there is data on unemployment rates and GDP growth to measure the amount of slack in the economy, data on state consumer prices are more limited. Nevertheless, having a measure of consumer inflation at the local level is critical to better capturing differences in local conditions, which likely differ from state to state. The Bureau of Economic Analysis (BEA) has recently made available quarterly data on nominal and real state GDP, from which we can derive the implicit deflator, but the time span is too limited (only since 2005). In addition, the BEA has also made available estimates of regional price deflators, the Implicit Regional Price Deflator (IRPD), but it is only available at a annual frequency, and it covers a short period (2008-2015).

Given the aforementioned data limitations, one of the contributions of the paper is to compute a quarterly measure of consumer price inflation for a sample of 30 US states over 1984-2017 by resorting to CPI data for 26 US MSA from the Bureau of Labor Statistics (BLS).[2] Although these MSA only cover 30 states, the states together are quite representative of the US reality, accounting for around 82% of total US GDP. I compute the state-level CPI by mapping the MSA to the states (for more details, see the online appendix). When doing the mapping, two main challenges arise. For example, Boston-Brockton-Nashua metropolitan area encompasses counties belonging to four different states: Massachusetts, New Hampshire, Maine, and Connecticut (Figure 2.1.1). This MSA will be used in the calculation of each of the latter four states' CPIs, together with any other MSA which may also cover counties belonging to the same state. The second challenge derives from the first, in that a state may include counties from different MSAs. I deal with this issue by taking personal income of the relevant counties as weights. In the case of Connecticut, its CPI is the income-weighted average of the counties (Fairfield, Litchfield, Middlesex, and New Haven counties) belonging to the CPI of New York-Northern New Jersey-Long Island, and of Windham county from Boston-Brockton-Nashua.

In terms of the counties covered by the CPI data for each state, states with lower coverage have, in general, a relatively lower weight in US GDP, whereas larger states tend to be better covered (see the online appendix). Coverage is perfect in District of Columbia and New Jersey and reasonably high in states such as Maryland, Massachusetts, and California. In turn, it

[2] The BLS actually published data for 27 MSAs until end-2017, but I dropped Phoenix-Mesa, Arizona, as it is only available from 2002. Some of these MSAs, however, have been discontinued in January 2018 – which does not affect the construction of the CPI proxy for the period I am covering in the paper – as the BLS introduced a new geographic area sample for the CPI. Specifically, from January 2018, the BLS has started using the 2010 Decennial Census, while also incorporating an updated area sample design. It has also changed the frequency of publication for some areas, and has created new local area and aggregate indexes. All in all, the BLS is currently publishing CPI data for 23 MSA.

Figure 2.1.1: Example of the relationship between metropolitan and state-level CPIs



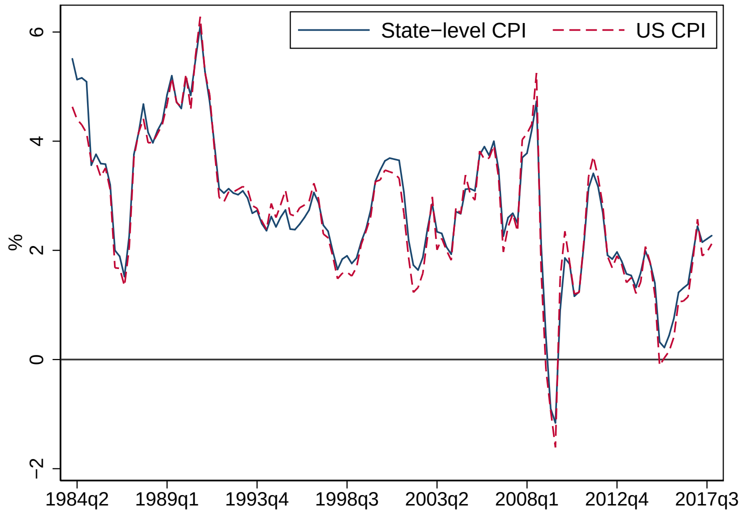
is lowest in West Virginia, Indiana, and Kentucky. One of the concerns is, therefore, that the CPI of these states with low coverage may be biased if the counties not covered by the MSA data exhibit different price dynamics. In the online appendix, I show that there is not any association between how much of the state is covered by MSA data and the 'quality' of the resulting CPI inflation when compared with the BEA's IRPD.

To assess the quality of the state-level CPI proxy, I do a bottom-up income-weighted aggregation and compare its annual inflation rate with that of the official US national CPI. The bottom-up aggregation of the new indicator does a pretty good job at tracking the official CPI, with a correlation of 0.98 over 1984-2017 (Figure 2.1.2). In the online appendix, I also show that the annual inflation rates of my CPI indicator for each state are closely in line with those from the BEA's IRPD.

2.2 Monetary policy rules

This section presents Taylor rules that put the Federal funds rate into perspective since the mid-1980s. Making use of the newly-built state-level CPI proxy, I then construct Taylor rules for each of the 30 US states and look at the cross-state heterogeneity. Finally, I construct an indicator that measures the state-specific monetary policy stance as the difference between the interest rate prescribed by the Taylor rules for each state and the US aggregate.

FIGURE 2.1.2: Bottom-up aggregation of state-level CPI vs US official aggregate – yoy % change



2.2.1 US aggregate

A monetary policy rule describes how the monetary policy stance responds when inflation and economic activity deviate from their targets – also called the *reaction function* of the central bank. The rule should, however, not necessarily be seen as the optimal path for monetary policy, but rather as a rule-of-thumb that a credible central bank tends to follow according to its mandate. In the case of the Fed, the dual mandate refers to price stability and maximum employment. In any case, despite the flexibility to deviate from the rules in the short run, they are nevertheless important to gauge the relative stance of monetary policy, while also allowing the central bank to communicate more easily to the public a specific change in its monetary policy stance.

Since the early 90's, and until a few years ago, the most common and widely used interest rate rules originated from Taylor (1993) and Taylor (1999), who assume that the central bank reacts contemporaneously to deviations in inflation and output. Nevertheless, the estimated contemporaneous response of the central bank to economic fluctuations in these early specifications might be biased on grounds of endogeneity issues. Related to this point, by using actual data, the traditional Taylor rules fail to incorporate the forward-looking aspect of the monetary policy decision process, whereby policy makers set interest rates according to their forecasts on inflation and economic slack. In addition, and although these traditional rules have described particularly well the conduct of US monetary policy during the Great Moderation from the 1980s to the late-2000s, the early 2000s, however, brought about a change in the course of monetary policy, with a standard Taylor rule prescribing a much higher policy rate. This has led some economists to deem monetary policy to have been too accommodative in the run-up to the Great Recession (Belongia and Ireland, 2016; Borio et al., 2017; Leamer, 2015; Taylor, 2011).

Against this background, I depart from the aforementioned traditional Taylor rules in three ways. First, to minimise the endogeneity concerns and to model more adequately the interest-rate decision process in a forward-looking manner, I follow a recent strand of the literature and use real-time expectations data for the central bank, instead of actual data (Coibion and Gorodnichenko, 2011; Coibion and Gorodnichenko, 2012; Orphanides, 2003). In particular, I use the Greenbook forecasts prepared by the Fed Board staff to inform Fed officials prior to each Federal Open Market Committee (FOMC) meeting. These forecasts are made available to the public with a five-year lag. Since the Fed forecasts on inflation and economic activity are made *before* each FOMC meeting, at which the interest rate is set, I can treat the forecasts as exogenous, and estimate the modified Taylor rule by OLS (for a more detailed discussion on why OLS is appropriate to estimate the Taylor rule using Greenbook forecasts, see Coibion and Gorodnichenko, 2011).

Second, I allow for interest-rate smoothing in the Taylor rule, which means that the central bank adjusts the policy rate to changes in economic conditions in a gradually fashion, rather than immediately as implied by the classical Taylor rule. More specifically, I include the lagged dependent variable in the estimated regression, as in Coibion and Gorodnichenko (2012).[3] Taylor rules that take into account the degree of policy inertia in central banks' reaction function have increasingly become more popular, helping to track better the actions of central banks, and therefore closing the gap between the prescribed policy rates and those effectively set by the central bank (Coibion and Gorodnichenko, 2011; Coibion and Gorodnichenko, 2012).

The last modification I make to the classical Taylor rule is to allow the central bank to react also to GDP growth, as advocated by Ireland (2004), and not only to inflation and the amount of slack in the economy (measured with the output gap). Following Coibion and

[3] Coibion and Gorodnichenko (2012) find evidence in favour of interest rate smoothing over serially correlated policy shocks to explain the highly-persistent nature of policy rates set by the Fed.

Gorodnichenko (2012), the Taylor rule used in this paper is as follows:

$$i_t = c + \phi_\pi E_{t-}\pi_{t+h_\pi} + \phi_x E_{t-}x_{t+h_x} + \phi_{\Delta y} E_{t-}\Delta y_{t+h_{\Delta y}} + \rho i_{t-1} + \epsilon_t \tag{2.1}$$

where i is the policy rate (Fed funds rate), E_{t-} refers to the forecast of a given macroeconomic variable made prior to the FOMC meeting, i.e., before setting the interest rate, where h is the forecasting horizon, π is annualised CPI inflation, x is the output gap measured as the deviation of actual output from potential, and Δy is GDP growth.[4] Regarding the key parameters, the coefficient ϕ_π is expected to be positive and above 1 to respect the Taylor principle, and ϕ_x and $\phi_{\Delta y}$ are also expected to be positive as smaller slack in the economy and stronger GDP growth require a higher policy rate. The constant term includes the steady-state level of the interest rate, and the time-invariant target levels of inflation and output growth.

Before I estimate the equation above with quarterly data, and given that there are typically eight FOMC meetings per year, I follow Coibion and Gorodnichenko (2012) and select the meeting dates closest to the middle of the quarter so as to have forecasts from the Greenbook on a quarterly basis. I also follow Coibion and Gorodnichenko (2012) and take the average forecast for inflation over *t+1* and *t+2* as the relevant horizons ($h_\pi = 1,2$), while for the output gap and GDP growth I take the contemporaneous forecast ($h_x = h_{\Delta y} = 0$). I estimate the Taylor rule in Eq. 2.1 for the US aggregate on quarterly data over 1984q1-2007q4, with Newey-West corrected standard errors. I stop the estimation at the end of 2007, thus excluding the ZLB period around and after the Great Recession as it has arguably distorted the relationship between conventional monetary policy and the real economy:

$$\hat{i}_t = \underset{(0.14)}{-0.76^{***}} + \underset{(0.06)}{0.29^{***}} E_{t-}\pi_{t+1,t+2} + \underset{(0.02)}{0.08^{***}} E_{t-}x_t + \underset{(0.02)}{0.22^{***}} E_{t-}\Delta y_t + \underset{(0.04)}{0.89^{***}} i_{t-1} + \epsilon_t \tag{2.2}$$

All the coefficients are estimated with high precision, and are fully in line with those in Coibion and Gorodnichenko (2012), who estimate Taylor rules with Greenbook forecasts over 1987q4-2006q4. The long-term coefficient on inflation is estimated to be 2.54, thus respecting

[4] In Section 2.5 I show that the main results remain robust when I use the unemployment gap as the slack measure. In addition, I get qualitatively the same results when I estimate the aggregate Taylor rule with Fed forecasts for different inflation measures: (i) the Personal Consumption Expenditure (PCE) deflator, the official Fed's target, (ii) core CPI, or (iii) the GDP deflator. Results available upon request.

the Taylor principle, whereby the central bank responds more than one-to-one to changes in expected inflation.[5] The Fed also adjusts the policy rate in a highly statistically significant way to the expected contemporaneous output gap and GDP growth, with the long-term coefficients estimated to be respectively 0.73 and 1.89. In addition, the lagged term on the dependent variable lies in the region of 0.8-0.9 typically found in the literature (Coibion and Gorodnichenko, 2011; Coibion and Gorodnichenko, 2012; Orphanides, 2003).

This type of Taylor rule with interest-rate smoothing, estimated on real-time forecasts from the Fed staff Greenbook, can account for most of the policy changes over the last two and a half decades until the Great Recession, as evidenced by the high R-squared of 98%. The same information can be illustrated by plotting the fitted values from the regression above against the Fed funds rate (Figure 2.2.1). The high degree of policy inertia can also be seen by using the estimated coefficients to extend the Taylor rule over 2008-17: the out-of-sample fitted values continue to track rather closely the actual Fed funds rate.[6] Given the 5-year publication lag in the Greenbook forecasts, I use real-time median expectations from the Survey of Professional Forecasters (SPF) to compute the fitted values from 2012 onwards.[7,8] Although there is evidence that the Greenbook forecasts tend to perform better than the SPF (Romer and Romer, 2000), the argument to complement the Greenbook with the SPF forecasts over 2012-17 is based on more recent research that has found that the gap in the (inflation) forecasting performance between the Greenbook and private sector forecasts has been narrowing considerably since the mid-1980s, and especially after 1994 (Gamber and Smith, 2009).

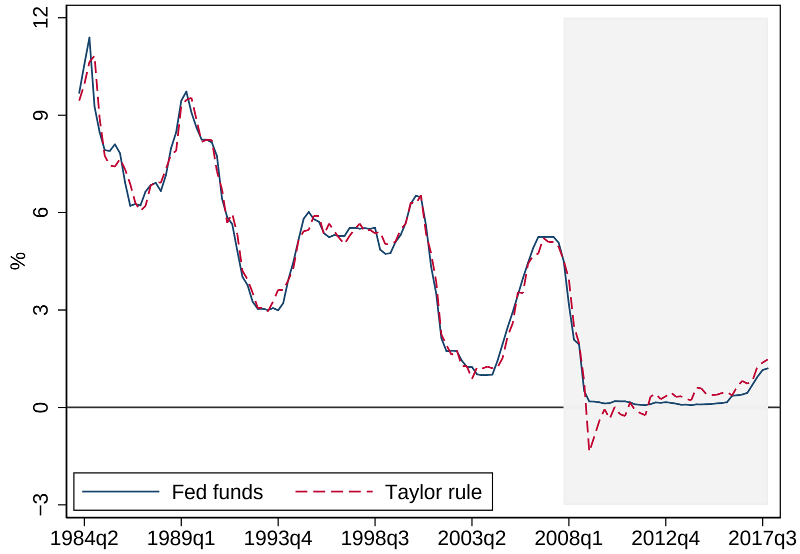
[5] The long-term coefficients result from dividing the short-run coefficients by one minus the coefficient on the lagged dependent variable (1-0.89).

[6] To alleviate potential concerns about the fact that the 'actual' policy rate might have been lower than the estimated and observed ones around the ZLB period, as a result of the use of non-standard monetary policy measures by the Fed, I show in Section 2.5 that the main findings remain robust to using a concept of a shadow rate – measuring the effective policy rate during ZLB periods – from Wu and Xia (2016).

[7] The Survey of Professional Forecasters is a quarterly survey of US macroeconomic forecasts made by a large number of private sector agents, including financial and non-financial. The survey is published around the second week of the second month of a given quarter. It started in 1968, and it is currently maintained, since 1990, by the Federal Reserve Bank of Philadelphia.

[8] I use potential output from the Congressional Budget Office (CBO) to compute the output gap for the SPF, as the survey participants are not asked to forecast potential output; the closest variable they report is the forecast of annual average growth over the following ten years.

FIGURE 2.2.1: The Fed funds rate vs estimated Taylor rule



Notes: The dashed red line refers to the fitted values of the Taylor rule estimated over 1984q1-2007q4 from Eq. 2.2. The grey shaded area represents out-of-sample values since 2008q1.

2.2.2 Heterogeneity across US states in the prescribed policy rates

With the newly-constructed state-level CPI proxy and unemployment gaps, I compute Taylor rules for the 30 US states over 1999q4-2017q4, using the estimated coefficients from Eq. (2.2). To be clear, I do not re-estimate Taylor rules for each state given that I assume the same reaction function for the central bank. Differences in prescribed policy rates across states come from inflation, output gap and GDP growth differentials, not from the coefficients themselves.

I should make a few additional remarks about the Taylor rules with state-level data. Firstly, in contrast to the central bank's real-time expectations data used before for the US aggregate, I use actual data on output and inflation for the US states given the lack of available forecasts at this level of disaggregation. In particular, I use average inflation values over the following two quarters, and the contemporaneous output gap and GDP growth. In a robustness check in Section 2.5, I show, however, that the main results and findings remain relatively similar when I estimate a Taylor rule with actual data, to be consistent with the state-level variables.

Secondly, I estimate the output gap for each state by filtering out the real GDP series from its transitory component with the new method developed by Hamilton (2018). His approach overcomes the typical issues associated with the HP filter, particularly that the latter produces spurious dynamic relations with no basis in the underlying data generating process, and the well-known end-of-sample issue. Hamilton's new method essentially translates into regressing a given non-stationary variable at $t{+}h$ on a constant and on the four most recent values of the dependent variable available at time t. Hamilton (2018) suggests to set the forecasting horizon at $h{=}8$ quarters.

Finally, my sample starts in 1999q4 as a result of data availability on real GDP. Although GDP data are available since 1997, I lose the first 11 quarterly observations in the dataset given that the Hamilton method requires lags 8, 9, 10, and 11 when estimating the cyclical component of real GDP for $h{=}8$.

It is not a surprise that the significant heterogeneity in economic conditions among the states results in different prescribed interest rates. The dispersion of the Taylor rule for the US states, as measured by the 4-quarter moving average of the interquartile range, has indeed been non-negligible, particularly in the run-up to the 2008/09 crisis (Figure 2.2.2). The rise in the cross-state heterogeneity over that period indicates that the divergence in economic performance across US states also called for different interest rates. At the trough of the last recession in early-2009, the prescribed interest rate ranged from around -2.0% to +0.4%, at a time when the estimated Taylor rule for the US aggregate was at -1.4%, while the Fed funds rate was practically at zero. After a decline in the dispersion in the aftermath of the Great Recession, there have been some signs more recently showing again an increase in the dispersion across states.

2.2.3 Monetary Policy Stance Gap

In a next step, I assess the extent to which the prescribed policy rates for the states signal a looser or tighter monetary policy stance. To accomplish that, I take the estimated Taylor rule at the national level as the benchmark, which also allows me to analyse the underlying asymmetries in the relative monetary policy stance at the local level. In this context, I construct an indicator, the Monetary Policy Stance Gap (MPSG), that measures the state-specific monetary policy stance relative to the US national by taking the difference between the interest rate prescribed by the Taylor rules for each state and the one from the US aggregate. The exogeneity assumption is supported by the fact that the Fed does not carry out monetary policy for a particular state, but rather for the country as a whole. As such,

Figure 2.2.2: Dispersion of Taylor rules across US states

Notes: The figure shows the 4-quarter moving average of the interquartile range of the Taylor rules for 30 US states.

there is no single state large enough that can influence US monetary policy alone. For example, California, the largest US state, accounts for a bit above 13% of total US GDP. This new indicator thus captures the variation in state monetary conditions, where the relative stance gap depends on the weighted sum of the differences between the state inflation, output gap, GDP growth and the equivalent variables at the aggregate level (the weights are the parameter estimates from Eq. 2.2):

$$MPSG_{i,t} = \phi_\pi(\pi_{i,t+1,t+2} - \pi^{US}_{t+1,t+2}) + \phi_x(x_{i,t} - x^{US}_t) + \phi_{\Delta y}(\Delta y_{i,t} - \Delta y^{US}_t), \;\; i = 1, ..., 30; \tag{2.3}$$

States for which the MPSG is positive experienced a looser monetary policy stance compared to what the state-specific Taylor rule prescribed, and vice-versa. A positive gap is the result of higher inflation, a more positive or less negative output gap, stronger GDP growth, or a combination of the three, for a given state relative to the US aggregate. To be clear, I compute the deviations from the fitted values of the US Taylor rule, not the actual Fed funds rate. Nevertheless, this is not a critical assumption that affects the results, as we have seen before that the fitted values from a Taylor rule with interest rate smoothing have tracked closely the actual Fed funds rate.

By analysing the MPSG for each state, it appears at first sight that monetary policy was more accommodative in the run-up to the crisis for states that experienced a boom-bust cycle in house prices and debt, and tighter once the crisis broke out (Figure B.1 in Appendix B). For instance, states such as Florida and California, which have undergone a pronounced housing market boom-bust cycle, are among the states with the loosest monetary policy stance before the crisis – an average MPSG over that period of respectively 0.19 and 0.07 – while others, such as Texas, Wisconsin, Indiana, and Ohio, which have not observed large swings in house prices and debt, are at the other end of the spectrum – average MPSG ranging between -0.12 and -0.28. This raises the question of the role of monetary policy in the rise of house prices and household indebtedness and whether monetary policy itself contributed to the widening in economic performance between the states. I test these hypotheses in Section 2.4.

2.3 Econometric framework

I use a novel dataset at the state level that combines data recently made available on economic activity (GDP and PCE) from the BEA, and debt in the household sector from the New York Fed Consumer Credit Panel/Equifax. In particular, PCE encompasses 16 spending categories on non-durable and durable goods, and services. Since the original PCE data are annual, I interpolate into quarterly data with the Chow-Lin method, using the aggregate PCE series as the indicator variable. In turn, household debt comprises data since 1999 on mortgage debt and consumer credit, including auto loans, credit card and student loans (see Appendix A for data definitions and descriptive statistics).[9]

I use the Local Projection (LP) method from Jordà (2005) to compute the sensitivity of household debt and other macro variables to state-specific monetary policy conditions. Compared to Vector Auto Regressive (VAR) models, the Jordà method has the advantage of the impulse responses being less vulnerable to misspecification while being more flexible to capture non-linearities. For instance, the Jordà method estimates local projections at each period of interest instead of extrapolating the impulse responses into increasingly distant horizons where misspecification errors are compounded with the forecast horizon. One of the features of the LP is that it tends to produce larger standard errors than the VARs,

[9] Household debt data are not publicly available between 1999 and 2002.

while there is often some loss of efficiency at longer horizons, resulting in erratic patterns in the dynamic effects.

The LP approach also offers important advantages in modelling non-linearities over alternative methods, such as Markov-switching (MSVAR) or threshold VAR models (TVAR).[10] The main advantage of the LP method over these two alternative techniques is that it neither requires us to take a stance on the duration of a given state nor on the transition dynamics between states (i.e., between high and low debt periods). For instance, Ramey and Zubairy (2018) stress that in the presence of non-linearities the LP method delivers more realistic estimates of the fiscal multiplier, and more consistent with the data generating process; Tenreyro and Thwaites (2016) make similar points on monetary policy. In the LP method, the impulse responses of a given dependent variable at $t{+}h$ is a forecast of how that variable will change at that horizon when another variable changes (MPSG in this paper) at t. The estimated coefficients in the LP framework thus measure the average effect of the MPSG on the dependent variable, as a function of the state dependencies – different debt periods or recessions versus expansions – when the MPSG changes at time t. If the average effect of the MPSG is likely to affect the state-dependencies in the forecasting horizon – say, moving from a high to a low debt period – this will be reflected in the impulse response estimate. The other transitions between regimes that are independent of the MPSG are captured by the state-dependent control variables.[11] In MSVARs or STVAR, however, the impulse responses assume that there is no change in the state of the economy, which may therefore bias the state-dependent impulse responses.

Moreover, the LP method models non-linearities in a more parsimonious way, compared to the highly-parameterised and computationally intensive MSVAR or TVAR models, especially in a panel data context. For instance, the LP method does not require us to estimate or calibrate transition probability functions as in STVARs or MSVARs. Third, LP make use of the full sample to estimate the non-linearities, while in TVARs the state-dependencies/regimes are estimated in separate models, which might complicate the estimation in the presence of a large set of parameters. Along the same lines, the estimation of MSVAR models may

[10]In a TVAR model, the coefficients are allowed to evolve from one regime/state to another, conditional on a pre-specified threshold value, similarly to the LP method in which state-dependencies are defined and imposed *a priori*. When the transition between regimes is allowed to be gradual, we call it smooth-transition VAR (STVAR). In turn, in a MSVAR, the regimes are determined endogenously according to a discrete Markov process, whereby probabilities to the different regimes or states are assigned at each point in time.

[11]For a more detailed explanation, see Ramey and Zubairy (2018).

be unreliable and sometimes become infeasible for large models. In addition, if a regime does not appear very often in the sample, especially relevant when I combine the debt periods with the state of the economy (Section 2.4.4), the lack of enough degrees of freedom in TVAR models makes it challenging to estimate precisely the responses conditional on the debt periods.

Before I present the state-dependent (non-linear) effects of the MPSG, I estimate a linear version of the model. Specifically, for each horizon h=1,2...,20 I estimate the following model with Fixed Effects over 1999q4-2017q4:

$$\Delta_h Y_{i,t+1+h} = \alpha^h + \beta^h MPSG_{i,t} + \lambda^h \Delta \log(X_{i,t-1}) + \eta_i^h + \zeta_t + \epsilon_{i,t+1+h} \tag{2.4}$$

The dependent variables are computed as cumulative changes from quarter *t+1* to *t+1+h*: (i) household debt-to-income ratio (DTI), (ii) logarithm of real GDP, (iii) logarithm of real housing wealth, and (iv) CPI inflation. In a second stage, I also look at PCE consumption and its main components. $MPSG_{i,t}$ is the monetary policy stance gap, $X_{i,t-1}$ is a vector of lagged control variables that help minimise the omitted variable bias and reduce the variance of the error term (Stock and Watson, 2018), which specifically include the lagged dependent variables and lagged MPSG, the change in the unemployment rate, and real income per capita growth.[12] The subscript i refers to the 30 states, t to time (quarters), the Δ operator to first differences expressed in percentage points, η_i^h is the state-specific fixed effect capturing unobserved time-invariant heterogeneity, and ζ_t controls for unobserved time-variant common factors across units in the panel. The inclusion of time dummies is important to absorb the effect of common factors driving the dynamics of the panel, i.e., they take away the national trend which acts as a common source of variation in macro and financial variables across the states.

I use housing wealth to capture the traditional wealth effect from housing assets, with home ownership also affecting housing wealth apart from only house prices. As in Albuquerque and Krustev (2018), housing wealth is as follows, where HPI is the real FHFA

[12] I have run alternative specifications by: (i) adding more state-specific financial variables as controls – available, however, only until end-2015 – which include changes in mortgage interest rates, the loan-to-value (LTV) ratio, and the foreclosure rate; and (ii) controlling for the lagged levels of GDP and the DTI to account for income effects and reversion to the mean. The main results remain broadly insensitive to these alternative specifications. Results available upon request.

House Price Index (all variables available at the state level):

(Homeownership rate x Occupied housing units) x HPI x Median house price in 2000

I deal with the issue of endogeneity and reverse causality potentially running from economic and financial variables in the left-hand side to the relative monetary policy stance by computing the cumulative changes from quarter *t+1* to *t+1+h*, and estimating the model starting only from h=1. With this framework, I assume that the MPSG affects the real economy in each state with a lag of one quarter, as is commonly done in the literature on monetary policy shocks.

In addition, I carry out an exercise to show further that the method above of assuming that the MPSG cannot affect the dependent variables contemporaneously is able to yield impulse responses that are likely not biased by endogeneity and reverse causality. Specifically, I purge the MPSG from state-specific macro and financial variables, by resorting to the method of Bassett et al. (2014), who propose a procedure to purge banks' lending standards from influences of key macro and bank-specific factors. I apply their method by regressing for each state the MPSG on state-specific inflation, GDP growth, debt-to-income and housing wealth growth. I take the residuals of this regression as the new MPSG measure purged from state-specific macro and financial variables. I show in the next section that the resulting impulse responses are broadly in line with the baseline results.

To keep the model parsimonious, I use one lag for all variables as in Tenreyro and Thwaites (2016) in a study of monetary policy shocks and the state of the business cycle, but the main results remain robust to the inclusion of more lags. Finally, I adjust the standard errors with Driscoll and Kraay (1998)'s estimator to account for correlation in the error term across states and time, given that the Jordà method with panel data usually exhibits cross-sectional and temporal dependence.

2.4 Baseline regressions

In this section I investigate the role of state-level monetary policy conditions on household debt and economic activity. After analysing the linear case, I focus on the non-linearities of the transmission of monetary policy. Specifically, I first explore the extent to which the state-specific stance of monetary policy may affect the dynamics in economic and financial variables differently during periods of large imbalances in household debt. Second, I focus on the interaction between monetary policy and regional asymmetries, conditional on the size of the imbalances in household debt across states. In this part, I also distinguish between periods characterised by state-specific recessions versus expansions.

2.4.1 Linear case

When estimating Eq. (2.4), and to better assess the economic relevance of the results, I calibrate the estimates to show the impulse responses to a one-standard deviation increase in the MPSG (0.5 p.p.). I find that an increase in the MPSG (looser monetary policy conditions in a specific state relative to the US aggregate) induces more household debt in a persistent and highly statistically significant way over the whole horizon (Figure 2.4.1).[13] The DTI is higher by roughly 0.8 p.p. after four quarters for those states which stand at a one-standard deviation above the mean of the MPSG, and reach a peak of around 2.4 p.p. after four years. At the same time, house prices also rise when state-specific monetary conditions become less restrictive, reflected in the hump-shaped profile of housing wealth that reaches a peak of 1.9% after three years.

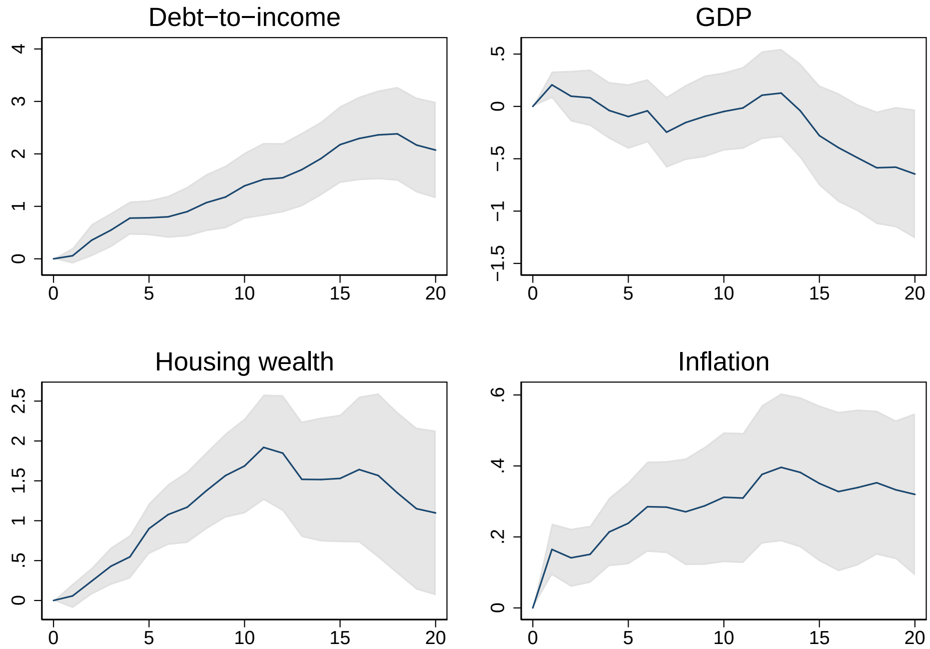
The rise in household debt and housing wealth after an increase in the MPSG is in line with the expected macro effects of looser monetary policy conditions (Alpanda and Zubairy, 2018; Bauer and Granziera, 2017; Jordà et al., 2015; Jordà et al., 2019). Accordingly, expansionary monetary policy lowers the cost of financing and reduces the real value of debt through higher inflation, facilitating the access to credit and thus encourages borrowing. My estimates are also in line with the expected effect stemming from the household balance sheet channel, or the home equity loan channel. This channel plays an important role for homeowners, whereby easier monetary conditions and higher house prices lead to higher housing wealth or home equity, allowing households to borrow more, in line with the findings by Bhutta and Keys (2016).

The response of real GDP displays a hump-shaped profile, with an increase in the very short run of 0.2%, before steadily converging to the baseline. The LP method, however,

[13] I get qualitatively similar impulse responses when I purge the MPSG from state-specific economic and financial variables, as illustrated in Figure B.2 of Appendix B, suggesting that my estimates are likely not plagued by reverse causality and endogeneity issues.

yields point estimates for the GDP response that are a bit erratic, and associated with large standard errors. Moreover, looser monetary policy also lifts consumer prices, which remain statistically above the baseline for the whole projection horizon.

FIGURE 2.4.1: IRF to an expansion in the monetary policy stance gap



Notes: The solid blue line is the cumulative response of the change in the debt-to-income ratio, real GDP, real housing wealth, and CPI inflation, to a one-standard deviation increase in the MPSG for horizons 1 to 20 (β^h from Eq. (2.4)). The grey area refers to the 90% confidence bands.

The increase in economic activity for states that experience a loosening in their monetary conditions relative to the US aggregate is probably connected to the increase in housing wealth and household borrowing that allows households to expand their purchases of goods and services. I find some tentative evidence for this mechanism when I use household consumption and its main components as dependent variables in Eq. (2.4). Although it tends to remain flat over the short run, consumption rises over the medium term when monetary policy conditions become looser (Figure B.3 in Appendix B). The responses are stronger in durable goods, followed by services, while the response of non-durables is more muted. Although Tenreyro and Thwaites (2016) focus on the macro effects of monetary policy shocks while I deal with state-specific monetary policy conditions, my results are in line with their findings that durables, and housing investment, are more sensitive to monetary policy.[14] The

[14]The analysis for the household sector would be more complete by adding residential investment to the picture, but unfortunately residential investment data are not available at the state-level.

estimated responses, however, are surrounded by significant uncertainty, probably related to the statistical noise from interpolating the original annual PCE data into quarterly.

2.4.2 Non-linear transmission of monetary policy: high vs low debt periods

I delve further into the interplay between monetary policy and household debt. The choice of household debt to study the state-dependent effects of monetary policy is underscored by the considerable cross-state heterogeneity in household debt levels over the last 20 years, coupled with a significant divergence in economic performance between states with high and low household debt. Furthermore, while recent research has found that excessive borrowing is detrimental to future economic growth (Albuquerque and Krustev, 2018; Jordà et al., 2013; Mian and Sufi, 2010; Mian and Sufi, 2011; Mian et al., 2017), and increase the probability of a financial crisis (Jordà et al., 2015), there are only a few recent papers exploring the non-linear interactions between the monetary transmission mechanism and the level of household indebtedness (Alpanda and Zubairy, 2018; Aikman et al., 2019). Moreover, little is known about the role that a common monetary policy might play in exacerbating regional asymmetries between states with different levels of household debt.

In the spirit of Bernanke et al. (1999) 'financial accelerator', a collateral constraint dictates the ability of a household to extract equity from housing. This mechanism might therefore amplify the effect of monetary policy when debt is high, since looser monetary policy stimulates house prices and consequently borrowers' home equity levels, which also spurs borrowing. In a similar vein, through a model focused on housing and mortgage debt, Hedlund et al. (2016) argue that monetary policy is more powerful in a high-LTV economy, as a result of more households having a high marginal propensity to consume. But, on the other hand, even if monetary conditions become looser, households might still be reluctant to take on more debt if their indebtedness is already high, or if they are borrowing constrained, which prevents them from increasing debt. Monetary policy in this case might be less effective. This mechanism appears to be reminiscent of the debt overhang theory of Eggertsson and Krugman (2012), in which households are forced into deleveraging when debt is high, and of empirical estimates of Albuquerque and Krustev (2018) who show that US states with higher household debt levels cut consumption by more during the Great Recession.

Furthermore, Alpanda and Zubairy (2018) find that monetary policy is less effective during periods of high household debt, which they argue is probably linked to the weakening of the home equity loan channel around those periods.

By looking at the data over the last years, we know that there has been a significant divergence in economic and financial performance between states with high and low household debt. For instance, the rise and fall in house prices and household debt in the United States over the last 20 years was far from being uniform across states.[15] This phenomenon raises the question about the effectiveness of monetary policy in the face of different levels of household indebtedness, both over time and over the cross-section.

Against this background, I first explore the link between the transmission mechanism of monetary policy in high and low debt periods. This split is in the spirit of Alpanda and Zubairy (2018), who use US aggregate data to define high and low debt periods as debt-to-GDP being above or below its smooth trend. I make advantage of the panel dataset by looking at the full distribution of household debt by time and state. First, I compute the debt-to-income gap for each US state, which I define as the deviation of the debt ratio from its trend derived with the Hamilton (2018) method described in Section 2.2.2. Second, and in contrast with Alpanda and Zubairy (2018), I define three debt periods instead of just two, in order to also allow monetary policy conditions to transmit differently to states with debt gaps at moderate levels. Specifically, I extend Eq. (2.4) with $\Phi^{H}_{i,t-1}$, a pre-determined time-varying dummy where 1 refers to states with a high debt gap, more specifically those belonging to the top quintile of their debt-to-income gap distribution, and with $\Theta^{L}_{i,t-1}$ that takes the value of 1 for states with a low debt gap, those in the first quintile. The remainder states with a moderate debt gap belong to the quintiles in between. The subscripts M, H,

[15] As documented by Albuquerque et al. (2015), household debt and, implicitly, house prices were not aligned with their fundamentals in some states in the run-up to the last recession, particularly in California and Florida, which led to an abrupt correction that deepened the magnitude of the economic downturn. For example, the different dynamics in house prices is quite telling: real house prices in California and Florida increased by around 116% and 97% between 1999 and their respective peaks in 2006, but then suffered a severe adjustment which has left real house prices at the end of 2017 still well below their previous peak. By contrast, house prices in Texas increased by 'only' 15% during the same period, recording a mild decline during the crisis period.

and L indicate the coefficients for moderate, high and low debt:

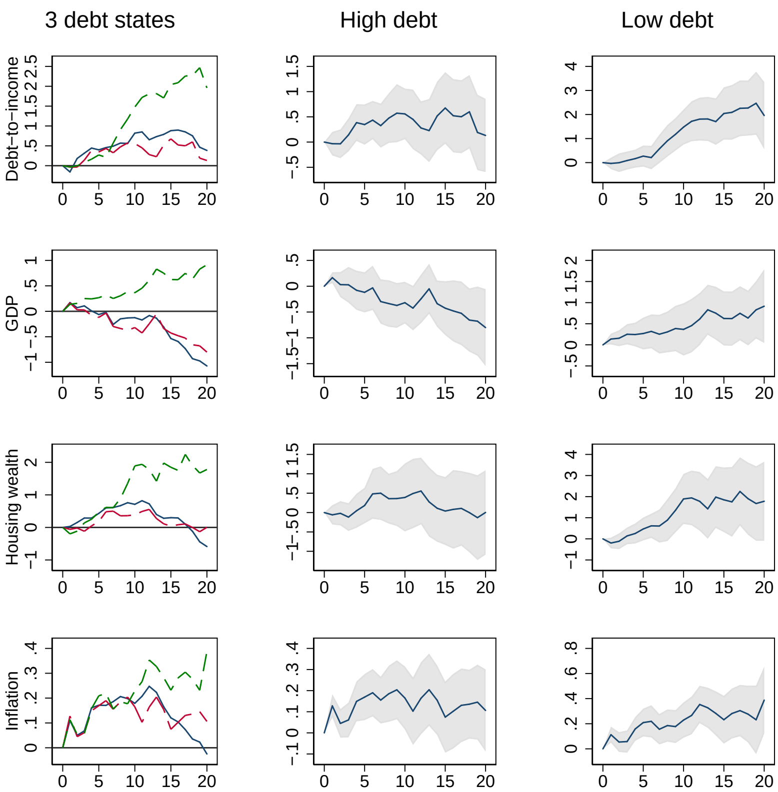
$$\begin{aligned}\Delta_h Y_{i,t+1+h} = & \alpha_M^h + \beta_M^h MPSG_{i,t} + \lambda_M^h \Delta \log(X_{i,t-1}) \\ & + \Phi_{i,t-1}^H \left[\alpha_H^h + \beta_H^h MPSG_{i,t} + \lambda_H^h \Delta \log(X_{i,t-1})\right] \\ & + \Theta_{i,t-1}^L \left[\alpha_L^h + \beta_L^h MPSG_{i,t} + \lambda_L^h \Delta \log(X_{i,t-1})\right] \\ & + \eta_i^h + \zeta_t + \epsilon_{i,t+1+h}\end{aligned} \tag{2.5}$$

By computing the debt gap individually for each state, I allow the US states to have household debt deviating from their trend at different points in time, which means that, in a given quarter, state A can transition from a period of high debt relative to its trend to a period of moderate or low debt in subsequent quarters. In other words, by exploring state-specific debt periods, I do not impose that all US states should have been in a high debt period in the run-up to, and during, the Great Recession, as suggested by aggregate data, such as in Alpanda and Zubairy (2018) and Aikman et al. (2019). According to my approach, the cross-state dispersion in the debt gaps are indeed considerable over the last 20 years, giving support to defining the debt gaps at the state level rather than defining common periods from aggregate data: around 38% of the states had high debt in the two years preceding the Great Recession, which then rose to an average of 50-60% during the crisis period (Figure B.4 in Appendix B). More recently, the substantial household deleveraging has allowed most of the states to transition from high debt to either moderate or low debt. To be clear about the definition, I use high/low debt or high/low debt gaps interchangeably throughout the paper.

The new estimates of a one-standard deviation increase in the MPSG show that the initial increase in household debt in periods of moderate, high and low debt is similar (Figure 2.4.2). But while loose monetary policy is supportive of household debt in low debt periods for the whole horizon, there is an indication that the boost from monetary policy to household debt in periods of high debt is short-lived; the DTI stays relatively flat after one year, and in non-cumulative terms it starts declining after four years, when households likely start adjusting downwards their level of indebtedness relative to income. There is, however, a denominator effect at play in high debt periods that masks the extent of the 'true' decline in household debt over the medium term: lower income (or GDP) attenuates the decline in the debt ratio, as the fall in nominal debt in high debt periods is more marked, and starts early (roughly after one year), than the one the debt ratio is portraying.

Against this background, the short-lived increase in household debt in periods when debt was already high relative to the estimated trend may translate into lower economic activity: after a muted initial response during the first quarters, real GDP starts contracting in periods of high debt. By contrast, I find evidence that looser state-specific monetary policy conditions are expansionary for real GDP over the whole period during low debt periods. The asymmetrical effects of monetary policy show up strongly: real GDP in high debt periods would be roughly 1.7 p.p. lower than in low debt periods after five years.

FIGURE 2.4.2: Household debt and the transmission of state-specific monetary policy conditions



Notes: Cumulative response of each variable to a one-standard deviation increase in the MPSG for h=1 to 20. 1st column: the long-dashed red line refers to periods of high debt gaps; the short-dashed green line to periods of low debt gaps; the solid blue line to periods of moderate debt gaps. 2nd and 3rd columns show the point estimates for periods of high and low debt gaps, with the associated 90% confidence bands.

The interaction of housing wealth with household debt is key to understanding the asymmetric dynamics between periods of high and low debt, following an expansion in state-specific monetary policy conditions relative to the US aggregate. Since, by construction, household debt is already at elevated levels in periods of high debt, more borrowing in the short run may place debt at even higher levels relative to income. This 'excessive' credit may 'force' households to deleverage and cut back on consumption (Figure B.5 in Appendix B).[16] At the same time, house prices, and consequently housing wealth, do not increase in periods of high debt in a statistically significant way, which prevents households from extracting more equity from their homes. The fact that a loosening in monetary policy conditions is not able to stimulate house prices may be related to the household debt dynamics described above: when the level of debt is already high, loose monetary policy may lead eventually to deleveraging over the medium run, which weighs on housing demand and prices, despite supportive monetary policy conditions. Although the reduced-form model prevents me from testing these mechanisms more formally, my findings may be placed in the context of recent work focusing on the role of the household balance-sheet channel for economic activity, particularly that excessive borrowing or household debt build-ups are detrimental to growth in the medium to longer run (Mian and Sufi, 2010; Mian and Sufi, 2011, for the United States; and Jordà et al., 2013; Mian et al., 2017, for a panel of countries), and increase the probability of a financial crisis taking place in the future (Jordà et al., 2015).

My results are similar to those with a financial shock found by Aikman et al. (2019), who use a threshold VAR to study the effects of financial conditions and monetary policy on the US economy during periods of high vs low non-financial sector credit. In particular, they find that in the presence of a high debt gap, a positive shock to financial conditions stimulates economic activity in the short run, but over the medium run it contracts given excessive borrowing. The dynamics above seems also to fit the mechanism described in Alpanda and Zubairy (2018). Particularly, they suggest that monetary policy is less effective in stimulating economic activity in periods of high household debt, arguing that the main mechanism at play may be the home equity loan channel not being operational, as house prices do not increase in these periods, preventing households from borrowing further.

As regards periods of low debt, I find that housing wealth increases consistently over the whole horizon, which contrasts sharply with the responses during periods of high debt. My estimates show that this increase in housing wealth is driven by higher house prices that accumulate as housing equity (intensive margin) rather than by higher homeownership rate (extensive margin) – Figure B.6 in Appendix B. In addition, I find some evidence that

[16] A tightening in credit supply standards might also be playing a role in explaining the weaker dynamics in borrowing during periods of high debt relative to low debt. For instance, the banks and the regulator may consider debt to be excessive relative to some lending criteria, or when it surpasses some pre-specified threshold values of standard macro prudential indicators, such as the loan-to-value ratio or the debt-to-income.

the homeownership rate may actually decline in the short term after a loosening in state monetary conditions, adding further signs that the expansion in household debt may be more the result of existing homeowners taking advantage of the home equity channel when house prices go up, in line with Bhutta and Keys (2016), rather than as a result of potential new homeowners taking on new mortgages.

Although there is a difference in the timing of deleveraging in periods of high debt which, according to my estimates, starts after four years, and the contraction in real GDP growth, which takes place a few quarters earlier, it should be noted that this apparent puzzle has already been observed in the data at the national level in the run-up to the last crisis. In fact, around the Great Recession, the US debt-to-income ratio only started to decline in 2009q1, with the crisis well under way, while real GDP had reached the trough a few quarters earlier (Figure B.7 in Appendix B). In addition, real house prices had been on a downward trend for already two years before the debt ratio started to decline. This difference in timing – which can also be rationalised in the context of a strong rigidity of debt – suggests that it is not only deleveraging *per se* that may affect economic growth, but the on-going debt build-ups, when judged to be excessive, can also exert a toll on economic activity, even before the debt bubble bursts.

2.4.3 Regional asymmetries in the transmission of monetary policy around the Great Recession

In the previous section we have seen that monetary policy has non-linear effects on the real economy, particularly that a loosening in the state-specific monetary policy stance fails to stimulate household debt and housing wealth, and is not associated with higher economic growth in periods when there are large imbalances in household debt. The impulse responses reported before refer to the *average* effect of a change in monetary policy conditions on macro and financial variables within each one of the three debt gap periods (high, low, and moderate). In this context, I have so far remained silent about how individual states are affected by a given change in the MPSG. Since I have found that the debt gap is a key variable to explaining the non-linearities in the transmission of monetary policy, I study in this section how a change in the MPSG affects regional asymmetries by exploiting the cross-sectional heterogeneity in the size of the imbalances in household debt across states.

More specifically, I interact the debt-to-income gap with the MPSG, in order to assess how monetary policy transmits to the states conditional on different debt gap levels. The coefficient γ is expected to be negative, implying that the larger the debt gap, the smaller the impact of an expansion in the MPSG on household debt, housing wealth, and real GDP:

$$\begin{aligned}\Delta_h Y_{i,t+1+h} =& \alpha^h + \beta^h MPSG_{i,t} + \gamma^h MPSG_{i,t} * \text{Debt gap}_{i,t} + \lambda^h \Delta \log(X_{i,t-1}) \\ & + \eta_i^h + \zeta_t + \epsilon_{i,t+1+h}\end{aligned} \tag{2.6}$$

After running the regression above for h=1,...,20 and for each one of the four dependent variables used before, I am particularly interested in investigating the impact of a change in the monetary policy stance at the beginning of the last recession in 2008q1. This was a period when the national monetary policy stance started to ease to fight the recession, but the dispersion in the prescribed Taylor rules diverged the most across states (Figure 2.2.2 in Section 2.2.2), and the dispersion in the estimated debt gaps across states was also the highest (Figure B.8 in Appendix B). Against this background, my findings about the transmission of monetary policy being non-linear in periods of high debt may conceal important regional asymmetries given the large cross-sectional heterogeneity in the debt gaps, especially around the Great Recession.

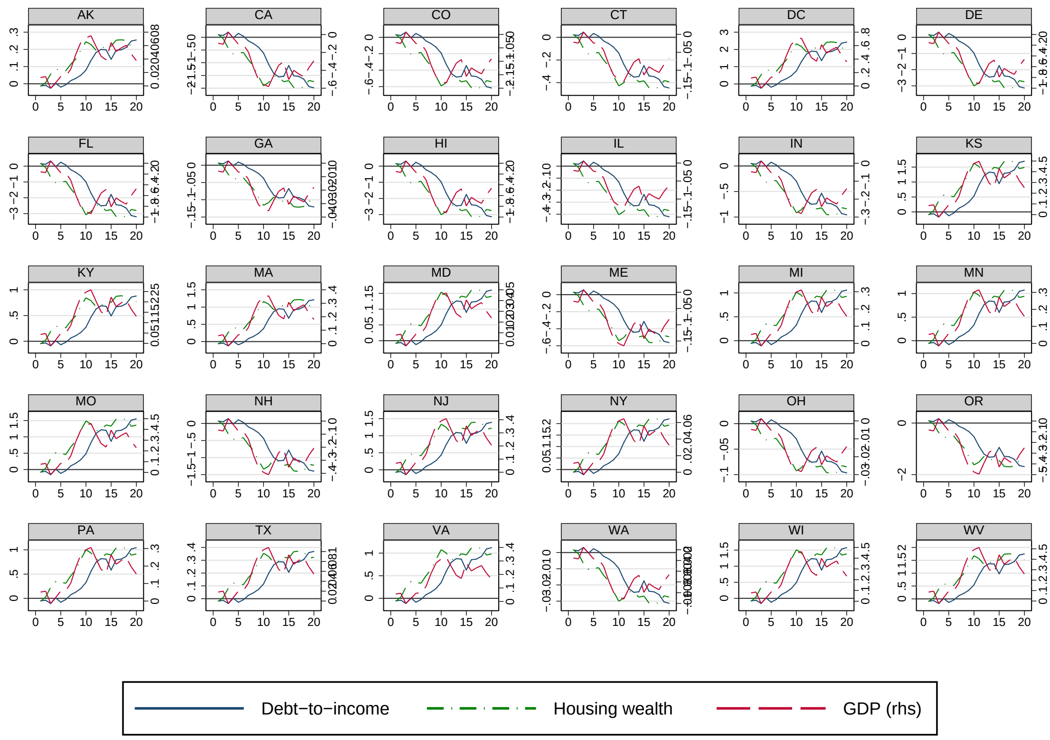
I compute for each state the implied IRF from Eq. 2.6 taking the estimated state-specific debt gaps in 2008q1. I normalise all IRF by the sample average debt gap, so that the response of a given variable in state i should be interpreted relative to the average state. Consistent with the results in the previous section, I find that an increase in the MPSG leads to a decline (relative to the average state in the sample) in the household debt ratio, housing wealth and real GDP after five years for the states that had the largest debt gaps in 2008q1, particularly California, Florida, Delaware, and Oregon (Figure 2.4.3).[17] These states with high debt gaps were precisely those that experienced the most severe boom-bust cycles in the housing market and household debt. In contrast, a loosening in the state-specific monetary policy stance is associated with higher debt, housing wealth and real GDP over the medium term for the states that had small debt gaps relative to the sample average, such as West Virginia, Kansas, and Texas. These findings may be placed in the context of Beraja et al. (2019), who find that an expansion in monetary policy in the wake of the Great Recession was weaker in

[17] The IRFs for CPI are available upon request.

stimulating consumption for US metropolitan areas where house prices dropped by more, as it was more difficult for underwater homeowners to refinance their mortgages and to extract equity from their houses.

This exercise has illustrated that monetary policy makers may face challenges in stimulating the economy not only during periods when the imbalances in household debt are large, but that their actions may also have asymmetric effects on the US states when the heterogeneity in the household debt gaps is high, as it may have probably been the case around the Great Recession.

FIGURE 2.4.3: Implied IRF of individual states relative to the average state in 2008q1



Notes: Cumulative impulse responses to a one-standard deviation increase in the MPSG for each state relative to the average state, conditional on the state-specific debt gaps in 2008q1.

2.4.4 Monetary policy and household debt during recessions and expansions

After finding a relationship between monetary policy, different household debt levels and regional asymmetries, I investigate whether my findings are conditional on the state of the economy. One could think that the reaction of the states to a loosening in their relative monetary policy stance might depend on the stage of the business cycle they find themselves in; macro and financial variables may behave differently to a loosening in monetary conditions in recessions – periods characterised by under-utilisation of resources in the economy – compared to a situation when their economies would be operating in normal circumstances. For instance, and as summarised by Tenreyro and Thwaites (2016), the transmission of monetary policy may depend on the health of the financial system, the degree of price stickiness and, on the household side, on the response of consumption to real interest rates at different stages of the business cycle.

The available empirical evidence on the effectiveness of monetary policy in recessions versus expansions is mixed. On the one hand, Peersman and Smets (2002) show that monetary policy tends to be more effective in recessions, which is in line with Bernanke et al. (1999)'s financial accelerator effect in which the decline in net worth during a recession amplifies the size of the initial shock. But, more recently, Berger and Vavra (2015), and Tenreyro and Thwaites (2016) find that monetary policy is more effective during expansions, with durables and investment responding more strongly in good times. According to Tenreyro and Thwaites (2016), one of the main reasons why monetary policy is more powerful in expansions is related to the pro-cyclicality of fiscal policy during expansions. In turn, Berger and Vavra (2015) show that the presence of adjustment costs leads households to adjust durable goods by much less in recessions.

Surprisingly, little is known about the interplay between monetary policy and different household debt levels during recessions and expansions. One of the exceptions is Alpanda and Zubairy (2018), who find some evidence, by using US aggregate time series data, that the effectiveness of monetary policy is further reduced during periods of high debt that coincide with recessions.

Defining recessions according to the US business cycle would probably not be informative in my dataset, as it does not allow the state of the economy to differ across the US states. Given the substantial heterogeneity in their economic performance, I define state-specific recessions as those periods with the weakest real GDP growth for each state, specifically the first quintile of the lagged 3-quarter moving average of real GDP growth in each state. Using a moving average of GDP growth to compute recessions is in the spirit of Auerbach and Gorodnichenko (2012) for fiscal policy shocks, and of Tenreyro and Thwaites (2016) for monetary policy shocks. I expand Eq. (2.5) with $\Omega^R_{j,t-1}$, a pre-determined time-varying dummy for state-specific recessions, where 1 refers to recessions, and 0 to expansions:[18]

$$\begin{aligned}
\Delta_h Y_{i,t+1+h} =& \alpha^h_M + \beta^h_M MPSG_{i,t} + \lambda^h_M \Delta \log(X_{i,t-1}) \\
&+ \Omega^R_{j,t-1}\left[\alpha^h_{MR} + \beta^h_{MR} MPSG_{i,t} + \lambda^h_{MR} \Delta \log(X_{i,t-1})\right] \\
&+ \Phi^H_{i,t-1}\left[\alpha^h_H + \beta^h_H MPSG_{i,t} + \lambda^h_H \Delta \log(X_{i,t-1})\right] \\
&+ \Phi^H_{i,t-1} * \Omega^R_{j,t-1}\left[\alpha^h_{HR} + \beta^h_{HR} MPSG_{i,t} + \lambda^h_{HR} \Delta \log(X_{i,t-1})\right] \\
&+ \Theta^L_{i,t-1}\left[\alpha^h_L + \beta^h_L MPSG_{i,t} + \lambda^h_L \Delta \log(X_{i,t-1})\right] \\
&+ \Theta^L_{i,t-1} * \Omega^R_{j,t-1}\left[\alpha^h_{LR} + \beta^h_{LR} MPSG_{i,t} + \lambda^h_{LR} \Delta \log(X_{i,t-1})\right] \\
&+ \eta^h_i + \zeta_t + \epsilon_{i,t+1+h}
\end{aligned} \tag{2.7}$$

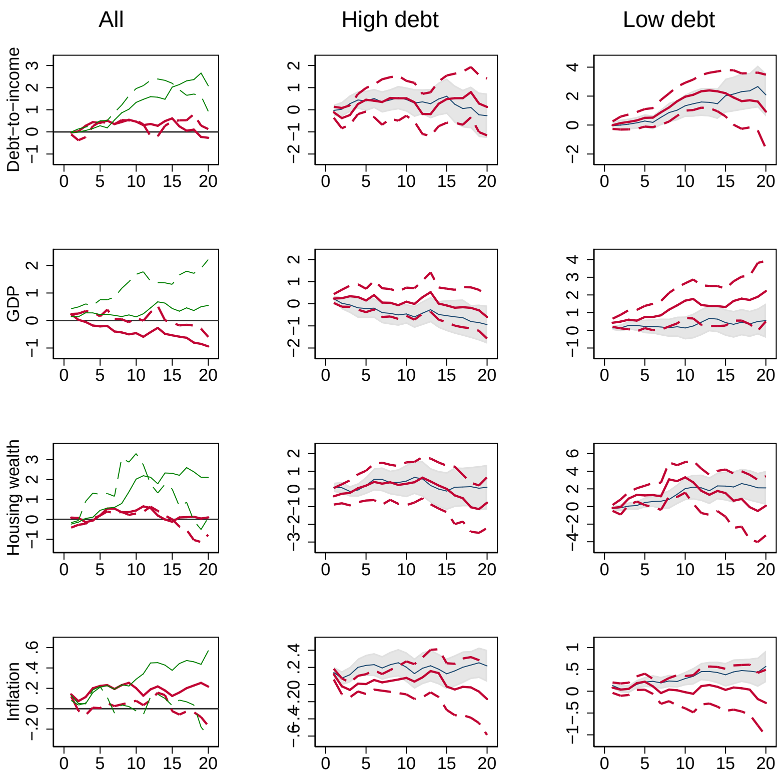
I focus on periods of high versus low debt during recessions and expansions. Following an expansion in the relative monetary stance, I find that during periods of high debt households increase their borrowing in the short run but only during expansions, as debt declines during recessions (Figure 2.4.4). One of the possible explanations for the different debt dynamics in the short run in the two regimes is related to the reluctance of highly-indebted households to take on more debt during bad times, given that borrowing constraints can be more binding as a result of tighter credit conditions during recessionary periods, or due to changes in households' attitudes towards leverage, i.e., households may become uncomfortable with their indebtedness relative to some behavioural benchmarks, as put forward by Dynan (2012). Over the medium term, however, I do not find statistical evidence that the response of household debt in periods of high debt is conditional on the state of the business cycle. In addition, the decline in GDP in high debt periods that coincide with recessions is more muted, although the impulse responses are not estimated with a high degree of precision, given the loss of degrees of freedom when estimating several state-dependencies simultaneously.

As for periods of low debt, a looser monetary policy stance stimulates household debt and economic activity in both times. In addition, economic growth also rises in both recessions and expansions, although the economic magnitude of these increases appears to be somewhat stronger during recessions. Finally, the increase in housing wealth is more short-lived during recessions.

[18] The total coefficient during recessions for low debt states is $\beta^h_L + \beta^h_{LR}$, for high debt states is $\beta^h_H + \beta^h_{HR}$, and for moderate debt states is $\beta^h_M + \beta^h_{MR}$.

Overall, I find that the business cycle appears not to matter materially to uncover asymmetries in the responses to a loosening in monetary conditions between periods of high and low household debt. In other words, I have shown that, apart from some minor differences, the main results of Figure 2.4.2 from the previous section are generally independent of the state of the economy. Accordingly, my findings point to the distinction between high and low debt periods, rather than recessions versus expansions, as being more fundamentally important to uncover differences in the way monetary policy transmits to the economy.[19]

FIGURE 2.4.4: Household debt and the state of the economy



Notes: Cumulative response of each variable to a one-standard deviation increase in the MPSG for h=1 to 20. 1^{st} column: red lines refer to periods of high debt, and green lines to periods of low debt; solid lines to expansions, and dashed lines to recessions. 2^{nd} and 3^{rd} columns show the point estimates for periods of high and low debt, where the solid blue (red) line is the point estimate for expansions (recessions), with the respective 90% confidence bands.

[19] For the sake of completeness, I also provide estimates that rely on a different definition of 'bad' versus 'good' times: slack and non-slack periods. According to Ramey and Zubairy (2018), slack periods are more long-lasting than recessions. Moreover, while recessions indicate periods in which the economy is moving from its peak to its trough, slack periods measure the deviation of the economy from its steady-state or full employment, signalling under-utilisation of resources. Accordingly, I define state-specific slack periods as those when the output gap for each state is below zero, and otherwise as non-slack periods. The estimates in Figure B.9 of Appendix B suggest that monetary policy is more effective in stimulating debt, supporting economic growth and house prices during periods of low debt that coincide with periods of slack. In turn, the responses during high debt periods are broadly in line with those obtained with the recessions/expansions in the baseline framework.

2.5 Robustness checks

I cross-check the sensitivity of the baseline results to alternative Taylor rules, which will then be used to replicate the impulse responses as in Section 2.4.

2.5.1 Alternative Taylor rules

Shadow rate

The first alternative Taylor rule deals with the challenge of the ZLB on nominal interest rates and the use of non-standard monetary policy measures by the Fed after 2008, which may have rendered the Fed funds rate less indicative of the actual monetary accommodation since that period. Lombardi and Zhu (2018) and Wu and Xia (2016) try to translate changes in the Fed's balance sheet into Fed funds rate equivalents by computing a shadow rate that measures the effective policy rate in the economy during ZLB periods. Their methods and estimates differ somewhat, but the main message is that the shadow rate has been significantly below zero after the Fed announced its first round of QE in November 2008 and cut its policy rate to a range of 0-0.25% in December 2008. Against this background, I re-estimate the Taylor rule in Eq. 2.1 using Wu and Xia (2016)'s shadow rate instead of the Fed funds rate as the dependent variable, and over 1984-2017 instead of ending the estimation in 2007. Since the Greenbook forecasts are only available until the end of 2011, I use the SPF forecasts to extend the dataset from that period and until the end of 2017.

Financial cycle

The second alternative specification is an extended Taylor rule with financial indicators. In the standard framework, a central bank only reacts to financial imbalances to the extent that financial indicators, such as credit aggregates or house prices, impact directly on inflation and economic activity. This implies that, for example, in a scenario where inflation is on, or close to, target, and economic activity is also close its potential, the prescribed policy rate may be too low if financial imbalances are building up in the economy. Consequently, the traditional Taylor rule might not capture adequately existing financial stability risks, implicitly creating a downward bias in the prescribed policy rate during financial booms and an upwards bias during financial busts (Borio et al., 2017; Hofmann and Bogdanova, 2012).

The debate on the role of monetary policy in stabilising the business cycle and the financial cycle is, however, far from being settled. Svensson (2017), for instance, defends that, although leaning against the wind with higher interest rates might reduce real debt growth, it comes at a great cost in terms of higher unemployment and lower inflation. The alternative to address financial imbalances, he argues, is to use micro- and macro prudential policy, housing policy, or fiscal policy, and not monetary policy. Furthemore, Coibion and Gorodnichenko (2012) do not find evidence that financial variables matter *per se* in a statistically significant way in Taylor rules with US data. But the challenges on the trade-off between price stability and financial stability, which have been brought to the fore with the Great Recession, have led research to increasingly focus on taking financial vulnerabilities into account in the reaction function of the central bank (see, for instance, Adrian and Duarte, 2018; Disyatat, 2010; Juselius et al., 2017; Woodford, 2012).

The related literature argues that financial cycles tend to be much longer (3 to 4 times) than business cycles, which usually last between 4 to 8 years (Drehmann et al., 2011). The Fed staff do not forecast household debt in the Greenbook, let alone a concept of potential or equilibrium debt. In this context, I capture the dynamics of financial cycles with the debt-to-income gap, computed as the deviation of the debt ratio from its long-term trend derived with the Hamilton (2018) method. Differently from the computation of most macro variables, and taking into account that I have household debt data at the aggregate level from the Flow of Funds that go back as far as 1951q4, I compute the deviation of debt from its long-term trend setting the forecasting horizon at h=20 quarters, instead of 8 used before.[20]

Another approach to compute the debt gap is through the one-sided HP filter, typically done by the Bank of International Settlements. For instance, Drehmann et al. (2011) estimate the debt gap using a smoothing parameter λ of 400,000 over a longer sample starting in the late-50s. The high value for λ assumes that financial cycles could last up to 30 years, as a result of multiplying 1,600, the typical value for business cycles lasting 8 years, by the fourth power of the observation frequency ratio: $\lambda = 4^4.1{,}600 \simeq 400{,}000$. The (backward-looking)

[20] I get qualitatively similar results with the mortgage debt gap or real house prices gap as alternative proxies for capturing the financial cycle.

one-sided filter is more appropriate than the two-sided, as it takes information that was only available at the time the assessment is made, i.e., the actual information set available to policy makers at each point in time.

Figure B.10 in Appendix B shows that the debt gap for the US economy was considerably larger in the run-up to the Great Recession with the Hamilton method than with the HP filter. The cyclical component of household debt with the Hamilton method is also more in line with the debt gap estimates by Albuquerque et al. (2015), who compute a measure of equilibrium household debt for the US states determined by economic fundamentals. The differences between the Hamilton method and the HP filter dissipate to a large extent during the aftermath of the last crisis, a period when substantial deleveraging by households led debt to fall below its trend. More recently, the pick-up in economic activity that has stimulated debt led to a convergence of debt towards its long-run trend, as estimated by the Hamilton method, while the HP filter is still suggesting debt to be considerably below its trend.

I estimate the following extended Taylor rule with the debt gap obtained with the Hamilton method over 1984-2017:

$$i_t = c + \phi_\pi E_{t-}\pi_{t+1,t+2} + \phi_x E_{t-}x_t + \phi_{\Delta y} E_{t-}\Delta y_t + \phi_d E_{t-}d_t + \rho i_{t-1} + \epsilon_t \tag{2.8}$$

The Taylor rule is estimated until 2017 to account for the leverage and subsequent substantial deleveraging in household debt over the last 20 years. Similarly to the previous specification, I use the shadow rate for the ZLB period, and SPF forecasts over 2012-17.

Unemployment gap as the slack measure

In the baseline Taylor rule specification, I have used the output gap to measure the amount of slack in the economy. As a robustness check, I use instead the unemployment gap, which has also been used in the related literature on the estimation of Taylor rules, such as in Coibion and Goldstein (2012), Leduc and Sill (2013), and Rudebusch (2010).[21]

I compute the unemployment gap at the US aggregate level by subtracting the Greenbook forecast for the non-accelerating inflation rate of unemployment (NAIRU) from the forecast for the unemployment rate. Since the NAIRU forecast is available only since 1989, I update

[21] More accurately, Coibion and Goldstein (2012), and Leduc and Sill (2013), use the unemployment rate.

the Greenbook series with the CBO's NAIRU for the preceding five years, 1984q1-1988q4. I estimate the new Taylor rule over 1984q1-2007q4, as in the baseline. As regards the state-level data, I compute the unemployment gap for each state by filtering out the unemployment rate, similarly to what I have done for the state-level output gap in the baseline specification.[22]

Actual data

The fourth and final specification estimates a Taylor rule on actual (final) data, more in the spirit of the classical Taylor rules (Taylor, 1993; Taylor, 1999). Although this Taylor rule is more prone to endogeneity issues – the dynamics in inflation and economic activity might be affected by interest rate decisions by the monetary authority – it may nonetheless be useful to compare the resulting impulse responses to all the other specifications that use real-time expectations.

More specifically, I estimate the original Taylor rule in Eq. 2.1 over 1984-2007, using actual data on inflation and the unemployment gap, but leaving the GDP growth term out of the equation, as it typically does not feature in classical Taylor rules. I use the unemployment gap given that data on the unemployment rate are less revised compared to GDP growth, therefore minimising the risk of having a large discrepancy between the first estimate of the data and the final data.

2.5.2 Robustness of the impulse responses

The estimated coefficients of the alternative Taylor rule specifications are roughly in line with those of the baseline Taylor rule (Table 2.5.1). The long-term coefficients on inflation respect the Taylor principle, although there is some dispersion, with the coefficients ranging from 1.5 in the Taylor rule that uses actual data instead of real-time expectations (*Actual*) to 2.9 in the specification that employs the shadow rate (*Wu-Xia*). It is worth noting that the debt gap is highly statistically significant in the Taylor rule of Column (3), implying that the Fed increases interest rates when household debt goes above its long-term trend. Finally,

[22] An alternative way to compute the NAIRU is to assume that the natural rate of unemployment for each state corresponds to the average of the unemployment rate over the 1990s. Justiniano et al. (2015) consider this period for their model's steady state for the US economy given the relative economic stability, and because the subsequent decade is distorted by the swings in debt and house prices. My main results are broadly robust to using the average unemployment rate over the 90s as the natural rate of unemployment; the only noticeable difference is the GDP response during periods of low debt being more muted.

according to the estimates in Column (4), where I use forecasts for the unemployment gap (*Ugap*), it seems that the Fed responds more strongly to changes in the unemployment gap than to the output gap, even after taking into account an Okun's law coefficient of two to translate changes in unemployment to output.

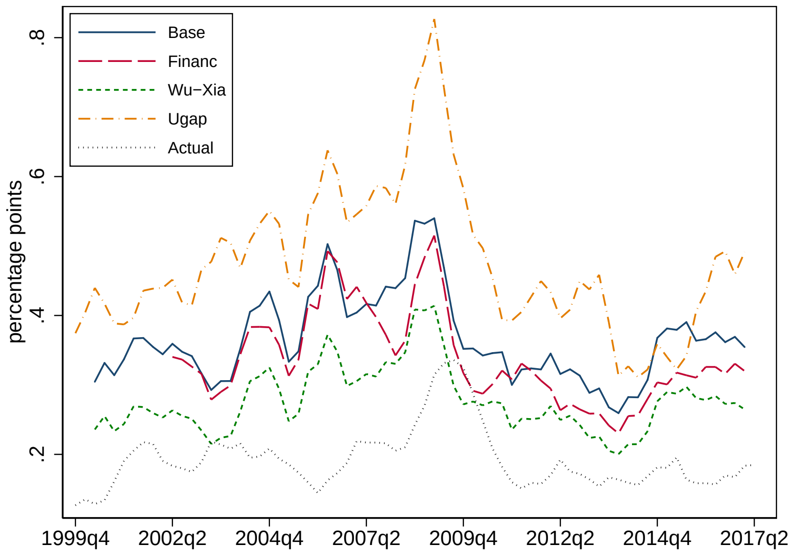
TABLE 2.5.1: Taylor rule regressions

	(1) Base	(2) Wu-Xia	(3) Financ	(4) Ugap	(5) Actual
ϕ_π	0.289***	0.213***	0.286***	0.308***	0.113**
	(0.065)	(0.053)	(0.054)	(0.065)	(0.070)
ϕ_x	0.083***	0.050***	0.048***	-0.262***	-0.166**
	(0.023)	(0.016)	(0.015)	(0.057)	(0.090)
$\phi_{\Delta y}$	0.216***	0.170***	0.167***	0.211***	
	(0.024)	(0.046)	(0.044)	(0.026)	
ϕ_d			0.014***		
			(0.004)		
ρ	0.886***	0.926***	0.900***	0.864***	0.925***
	(0.035)	(0.023)	(0.021)	(0.038)	(0.032)
Constant	-0.762***	-0.687***	-0.823***	-0.710***	0.054
	(0.137)	(0.164)	(0.152)	(0.149)	(0.234)
Period	1984q1-2007q4	1984q1-2017q4		1984q1-2007q4	
Observations	96	136	136	96	96
Adj. R-squared	0.979	0.987	0.988	0.981	0.956

Notes: Regression estimates of Eq. 2.1, with exception of column (3) that refers to Eq. 2.8. The coefficient ϕ_x in Columns (4) and (5) refers to the unemployment gap. Newey-West corrected standard errors in parentheses. Asterisks, *, **, and ***, denote statistical significance at the 10, 5, and 1% levels.

The dispersion of the alternative Taylor rules, as measured by the 4-quarter moving average of the interquartile range, follows the same dynamics over time as the baseline: a steady increase in the dispersion in the run-up to the 2008/09 crisis, followed by a decline in the aftermath of the Great Recession (Figure 2.5.1). There are, however, some differences in the level of the dispersion across specifications, with that of the baseline Taylor rule being generally somewhat higher with respect to the alternative specifications, apart from the one that uses the unemployment gap as the slack measure. In particular, the lower dispersion of the extended Taylor rule with the debt gap *(Finan)* indicates that cross-state differences in the prescribed policy rates can be mitigated somewhat with a central bank that incorporates the financial cycle into its reaction function. The second observation is that the dispersion of the specification estimated on actual data is the lowest. Nevertheless, this is related to the fact that the coefficient on the lagged interest rate increases compared with the baseline, while the coefficient on inflation declines substantially.

FIGURE 2.5.1: Dispersion of alternative Taylor rules across US states

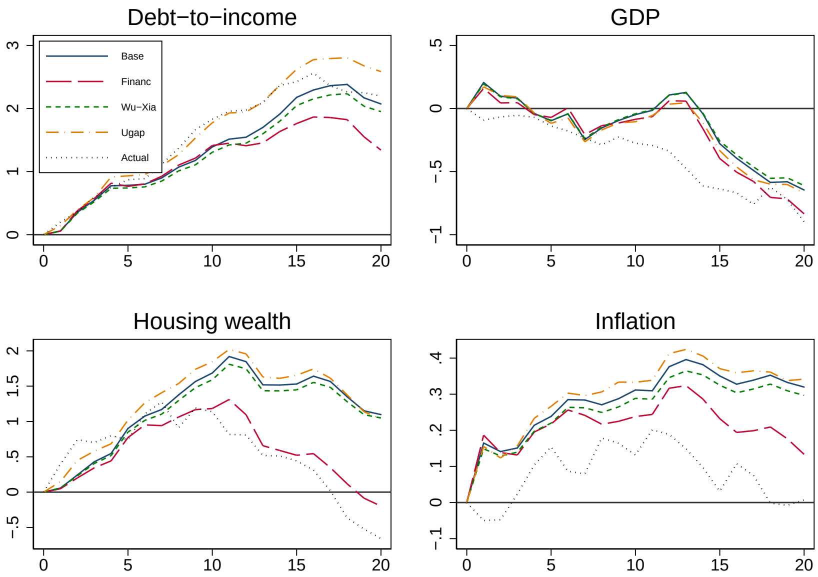


Notes: The figure plots the 4-quarter moving average of the interquartile range of alternative Taylor rules for 30 US states. *Base* is the baseline estimated Taylor rule, *Financ* is the extended rule with the debt-to-income ratio gap, *Wu-Xia* takes Wu and Xia (2016)'s shadow rate, *Ugap* employs forecasts of the unemployment gap as the slack measure, and *Actual* is a Taylor rule estimated on actual data.

To check the robustness of the baseline impulse responses with the alternative Taylor rules, I recompute the MPSG for each state, as in Section 2.2.3, and then replicate Figure 2.4.1 which draws on Eq. (2.4), and Figure 2.4.2 from Eq. (2.5). I find that following a one-standard deviation increase in the state-specific MPSG, the baseline results remain broadly robust to all alternative Taylor rule specifications, both for the linear case (Figure 2.5.2) and when considering non-linearities related to different debt gap periods (Figure 2.5.3).

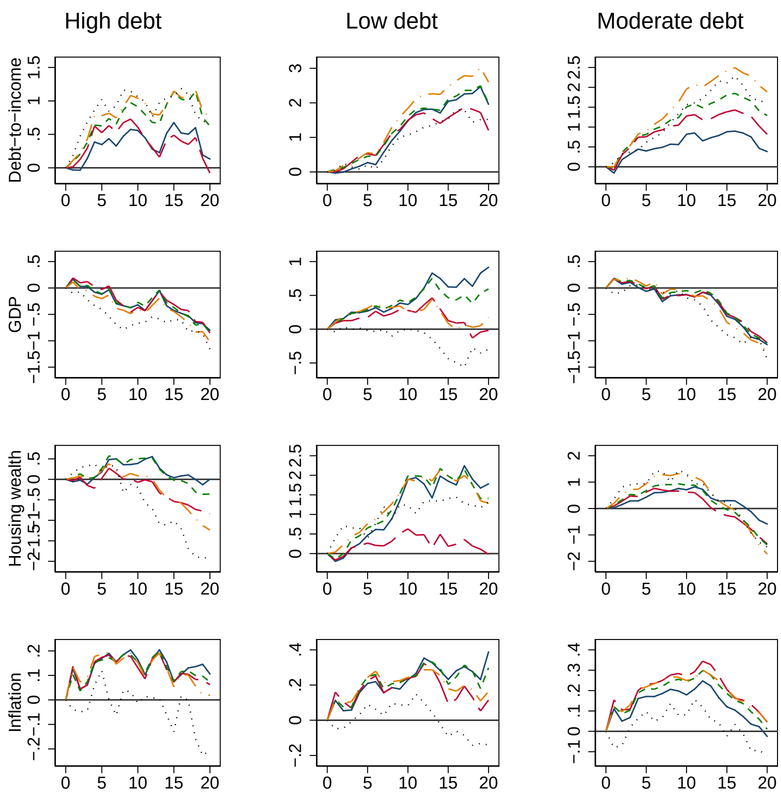
Having said this, there are nevertheless some differences across specifications worth mentioning. Although the dynamics in debt, GDP, housing wealth and inflation are broadly similar, housing wealth converges faster to the baseline for the extended Taylor rule that accounts for the financial cycle in the linear case in Figure 2.5.2. This result is driven mostly by the decline in housing wealth in periods of high debt, and by a muted response in low debt periods (Figure 2.5.3). Accordingly, the debt deleveraging in high debt periods is more pronounced, and starts earlier, than in the baseline, probably also related to the fact that housing wealth during these periods actually declines over the medium run.

FIGURE 2.5.2: IRF to an expansion in the MPSG for alternative Taylor rules



Notes: Cumulative response of each variable to a one-standard deviation increase in the MPSG for h=1 to 20. *Base* is the baseline Taylor rule, *Financ* is the extended rule with the debt-to-income ratio gap, *Wu-Xia* takes Wu and Xia (2016)'s shadow rate, *Ugap* employs forecasts of the unemployment gap as the slack measure, and *Actual* is a Taylor rule estimated on actual data.

FIGURE 2.5.3: Household debt: IRF to an expansion in the MPSG for alternative Taylor rules



Notes: Cumulative response of each variable to a one-standard deviation increase in the MPSG for h=1 to 20. The solid blue line is the baseline Taylor rule, the long-dashed red line the extended rule with the debt-to-income ratio gap, the short-dashed green line the rule with Wu and Xia (2016)'s shadow rate, the dashed-dotted orange line employs forecasts of the unemployment gap as the slack measure, and the dotted black line is a Taylor rule estimated on actual data.

2.6 Final remarks

In this paper I have investigated the extent to which differences in the monetary policy stance across US states may affect the dynamics in economic and financial variables in a non-linear fashion during periods of large debt gaps. In a second step, I have studied the interaction between monetary policy and regional asymmetries, particularly by focusing on the Great Recession, a period characterised by high dispersion in household debt imbalances across states.

The main findings of the paper suggest that the degree of imbalances in household debt affects the transmission of monetary policy to the real economy. More specifically, I have found that a looser state-specific monetary policy stance is supportive of borrowing and growth over the medium term during periods of low household debt, but that this is only the case in the short term during periods of high debt. Economic growth turns negative over the medium to longer run in these periods of large imbalances in household debt, probably linked to household deleveraging from excessive credit growth. In addition, I find that house prices do not increase in high debt periods, making it harder for households to take advantage of the home equity loan channel to extract more equity from their homes to finance consumption, or to refinance their mortgages. Although the reduced-form model prevents me from testing these mechanisms more formally, my findings go in the same direction as recent work focusing on the role of the household balance-sheet channel for economic activity, particularly that excessive borrowing or household debt build-ups are detrimental to growth in the medium to longer run (Jordà et al., 2013; Jordà et al., 2015; Mian and Sufi, 2010; Mian and Sufi, 2011; Mian et al., 2017).

This paper also lends support to the view that a common monetary policy in the United States *does not fit all*, in that monetary policy may have asymmetric effects on the economic performance across states when the household debt cycles are not synchronised across states. In particular, I have found that monetary policy during the last recession may have been particularly ineffective in stimulating growth in the states with the largest debt gaps, which were precisely those states that were going through a severe boom-bust cycle. Against this background, the non-linear interactions between the heterogeneity in the monetary policy stance and household debt across US states play an important role in shedding more light on the distributional effects of monetary policy.

Acknowledgements

I would like to express my gratitude and appreciation to Daniel Carvalho, Georgi Krustev, Gert Peersman, Joris Wauters, Sarah Zubairy, Sebastian Rüth, Selien De Schryder, Wouter Van der Veken, as well as to participants at the Ghent University Economics Seminar, the 4^{th} IAAE 2017 Annual Conference, and the Norges Bank post-event Workshop on Housing and Household Finance. I would also like to thank the editor Sanjay K. Chugh and two anonymous referees for their suggestions and comments that improved the paper substantially. Finally, I thank the FRBNY and Equifax for providing me with aggregated, state-level summary data on household debt. I acknowledge financial support from the UGent Special Research Fund (BOF).

Appendix

A State-level data – 1999q1-2017q4

Debt-to-income: sum of mortgage debt and consumer credit, including auto loans, credit card and student loans, divided by personal income. From 2013 to 2017, I interpolate annual data into quarterly with the Chow-Lin method, using the US aggregate household debt-to-income as the indicator variable. Source: NY Fed/Equifax.

Personal income: income from labour (wages and salaries), from owning a home or business, from the ownership of financial assets, and government transfers. Source: BEA.

Real GDP: Gross Domestic Product computed through the output or value-added approach. Annual data interpolated into quarterly from 1999 to 2004 with the Chow-Lin procedure, and using US real GDP as the indicator variable. Source: BEA.

Housing wealth: estimated total housing wealth owned by home owners, computed as: (Homeownership rate x Occupied housing units) x HPI x Median house price in 2000.

House price index: Weighted, repeat-sales index measuring average price changes in repeat sales or refinancings on the same properties. It tracks the movement of single-family house prices. The raw series has been seasonally adjusted with the X-13ARIMA-SEATS of the US Census Bureau. Source: Federal Housing Finance Agency (FHFA).

Homeownership rate: proportion of housing units that is owner-occupied, defined as the number of housing units that are occupied by owners divided by the total number of occupied housing units. The raw series has been seasonally adjusted with the X-13ARIMA-SEATS of the US Census Bureau. Source: Census Bureau and Haver Analytics.

Occupied housing units: a house, apartment, mobile home or trailer, a group of rooms, or a single room that is occupied. Annual data have been interpolated into quarterly. Source: Census Bureau.

Real PCE: Spending on non-durable and durable goods, and services. Annual data interpolated into quarterly with the Chow-Lin method, using the relevant aggregate PCE series as the indicator variable. Data available until 2016. Source: BEA.

Unemployment rate: the unemployed aged 16 and over in percentage of total labour force, taken from the household survey. Source: BLS.

TABLE A.1: Descriptive statistics over 1999q1-2017q4

In levels

Variable	Obs	Mean	Std. Dev.	Min	Max
Monetary policy stance gap	2130	-0.2	0.5	-1.8	2.5
State-level CPI (% yoy)	2220	2.2	1.3	-3.8	5.7
Debt-to-income ratio	2220	82.4	20.0	40.9	153.3
Real GDP (log)	2220	12.4	1.0	10.5	14.7
Real income per capita (log)	2220	16.8	0.2	16.5	17.7
Real housing wealth (log)	2220	23.3	1.0	21.0	26.0
Real house prices (log)	2220	5.1	0.3	4.6	6.3
Homeownership rate (%)	2220	67.6	7.3	38.1	82.8
Total real PCE (log)	2160	11.3	1.0	9.3	13.4
Real PCE: durables (log)	2160	9.2	1.0	7.1	11.2
Real PCE: non-durables (log)	2160	9.8	1.0	7.8	11.7
Real PCE: services (log)	2160	10.9	1.0	8.8	13.0
Unemployment rate (%)	2220	5.9	1.9	2.1	14.6

In first differences

Variable	Obs	Mean	Std. Dev.	Min	Max
ΔMonetary policy stance gap	2100	0.0	0.5	-4.2	3.1
ΔState-level CPI	2220	0.0	0.7	-4.8	3.7
ΔDebt-to-income ratio	2190	0.3	2.5	-13.0	17.6
ΔReal GDP (%)	2220	0.4	1.2	-6.4	11.6
ΔReal income per capita (%)	2220	0.2	1.2	-8.4	10.4
ΔReal housing wealth (%)	2220	0.5	2.7	-9.9	13.3
ΔReal house prices (%)	2220	0.3	1.7	-10.1	9.2
ΔHomeownership rate	2220	0.0	1.3	-5.3	5.1
ΔTotal real PCE (%)	2160	0.5	0.7	-3.2	2.8
ΔReal PCE: durables (%)	2160	0.2	2.1	-9.5	9.2
ΔReal PCE: non-durables (%)	2160	0.4	1.3	-8.4	4.1
ΔReal PCE: services (%)	2160	0.6	0.7	-1.6	4.1
ΔUnemployment rate	2220	0.0	0.3	-0.9	2.6

Sources: Bureau of Economic Analysis, Bureau of Labor Statistics, Census Bureau, Federal Housing Finance Agency, Federal Housing Finance Board, Mortgage Bankers Association, NY Fed/Equifax, and author's calculations.

B Tables and figures

FIGURE B.1: Monetary policy stance gaps for US states

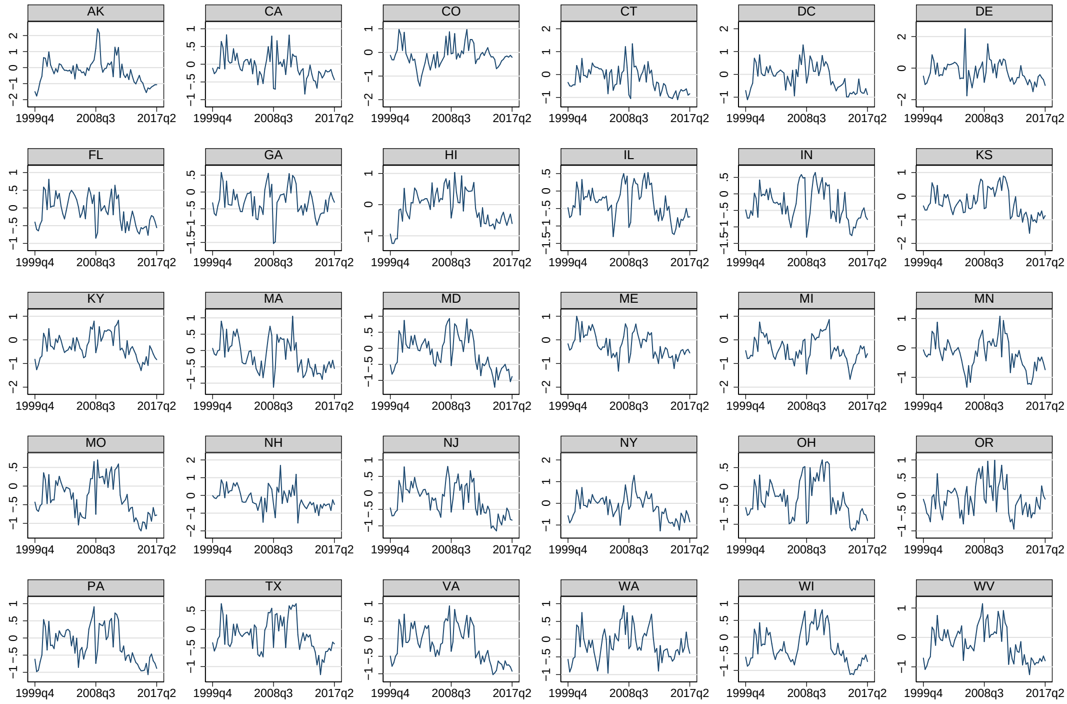
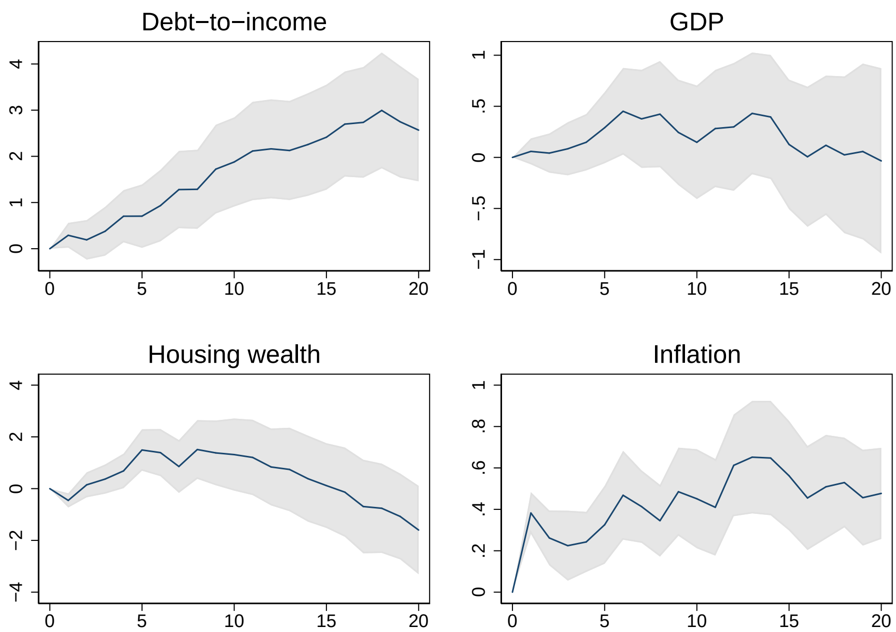
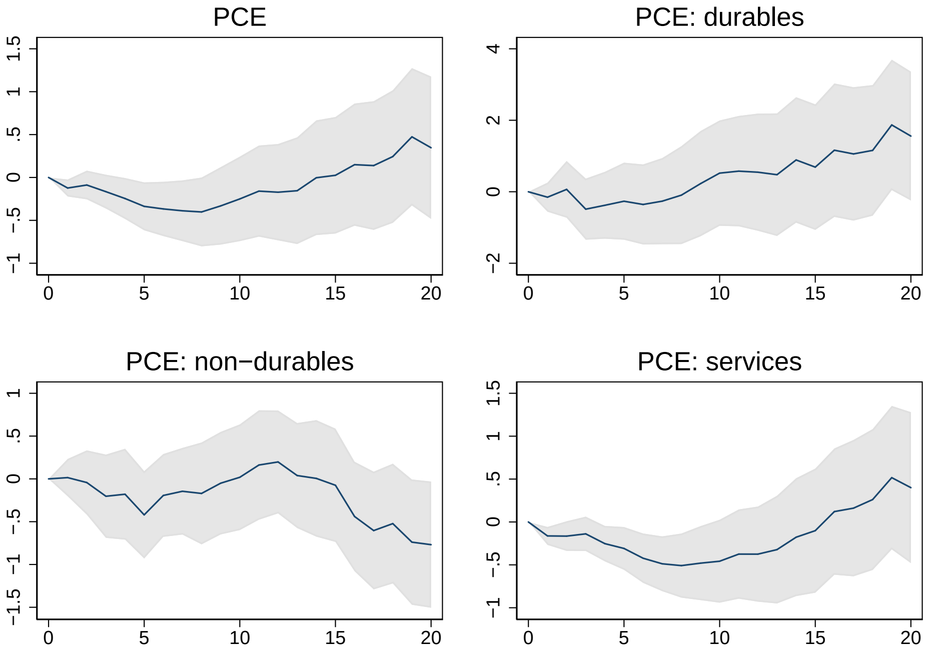


FIGURE B.2: Purged MPSG: IRF to an expansion in the MPSG



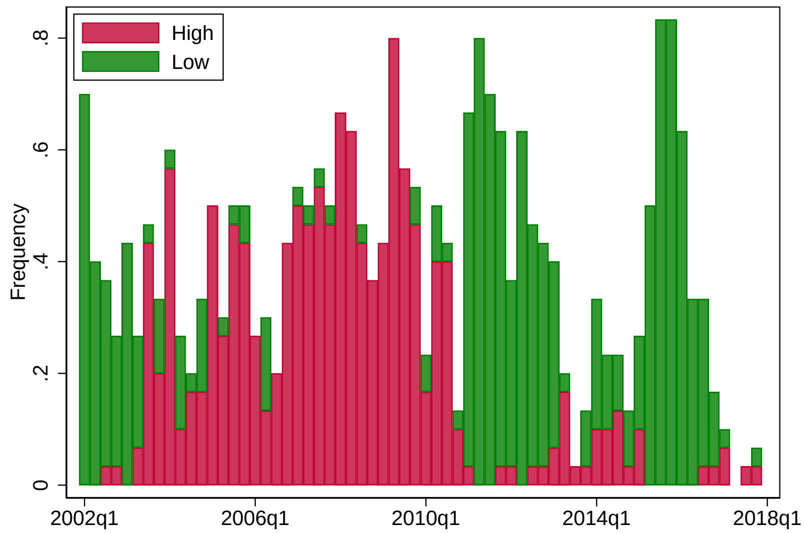
Notes: Cumulative response of the change in the debt-to-income ratio, real GDP, real housing wealth, and CPI inflation, to a one-standard deviation increase in the purged MPSG for horizons 1 to 20 (β^h from Eq. (2.4)). The grey area refers to the 90% confidence bands. The MPSG has been purged from state-specific macro and financial variables. See the main text for details.

FIGURE B.3: IRF of consumption to an expansion in the MPSG



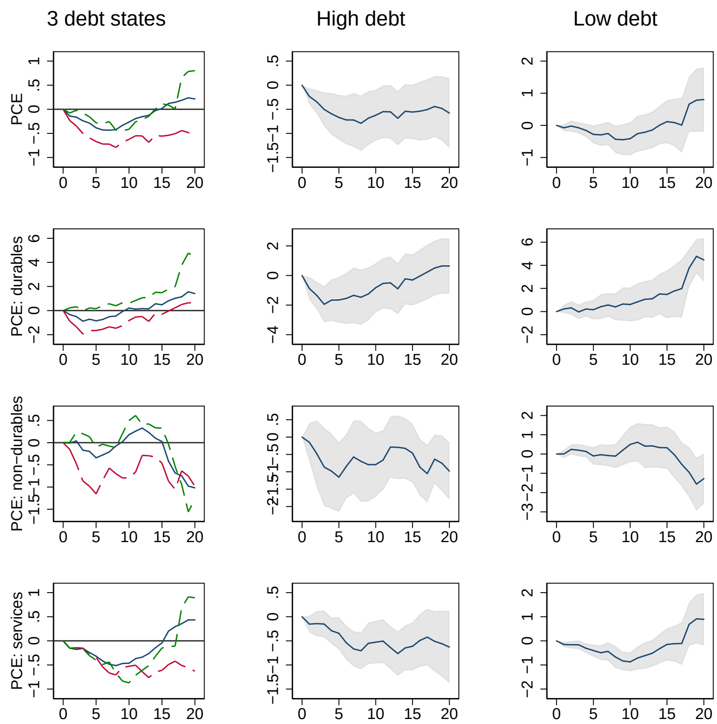
Notes: The solid blue line is the cumulative response of each variable to a one-standard deviation increase in the MPSG for horizons 1 to 20 (β^h from Eq. (2.4)). The grey area refers to the 90% confidence bands.

FIGURE B.4: Distribution of periods of high and low debt across US states



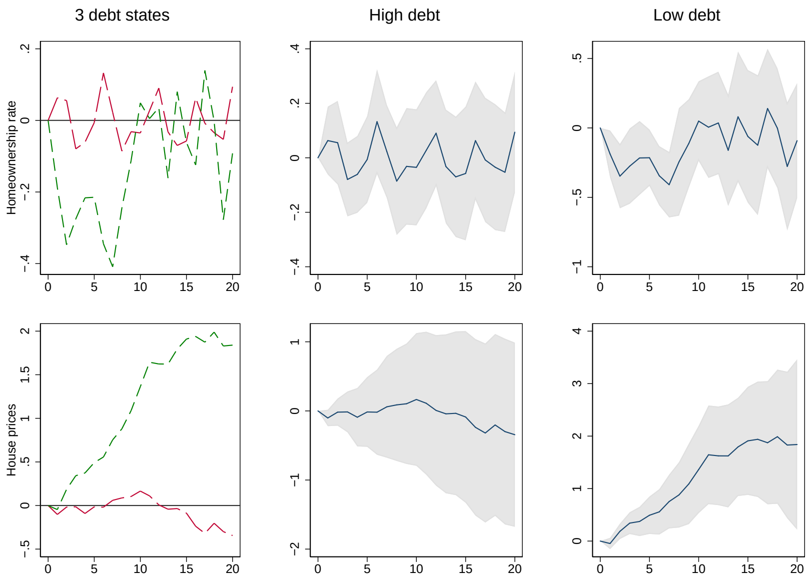
Notes: The figure shows the fraction of states with high and low debt gaps at each point in time.

Figure B.5: Consumption: Household debt and state-specific monetary policy conditions



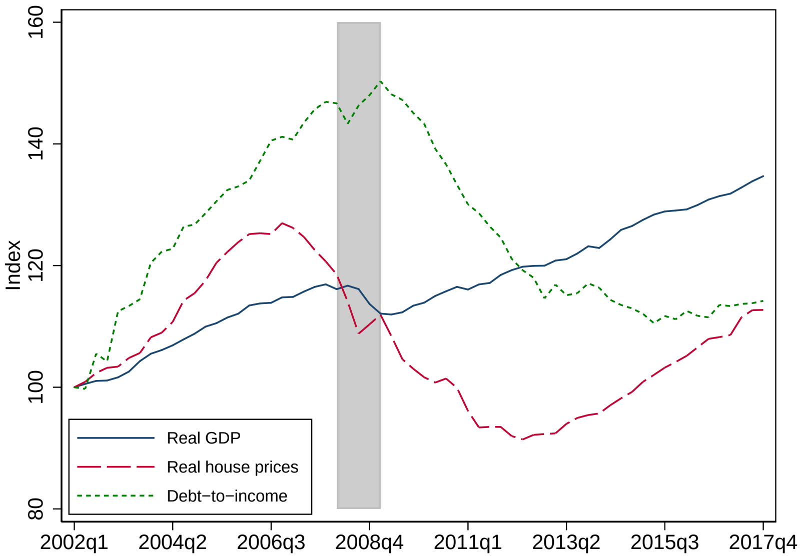
Notes: Cumulative response of each variable to a one-standard deviation increase in the MPSG for h=1 to 20. 1^{st} column: the long-dashed red line refers to periods of high household debt; the short-dashed green line to periods of low debt; the solid blue line to periods of moderate debt. 2^{nd} and 3^{rd} columns show the point estimates for periods of high and low debt, with the associated 90% confidence bands.

Figure B.6: Extensive and intensive margins of the housing market



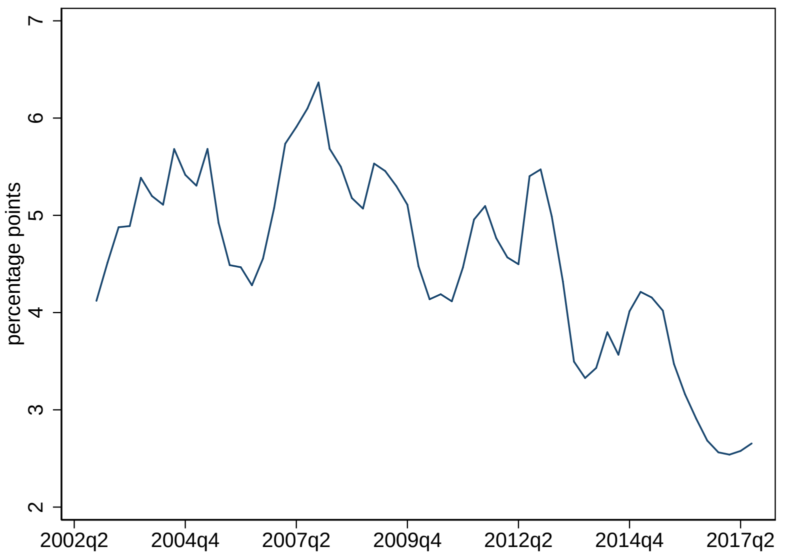
Notes: Cumulative response of each variable to a one-standard deviation increase in the MPSG for h=1 to 20. 1^{st} column: the long-dashed red line refers to periods of high household debt, and the short-dashed green line to periods of low debt. 2^{nd} and 3^{rd} columns show the point estimates for periods of high and low debt, with the associated 90% confidence bands.

FIGURE B.7: Dynamics of GDP, house prices and household debt at the national level



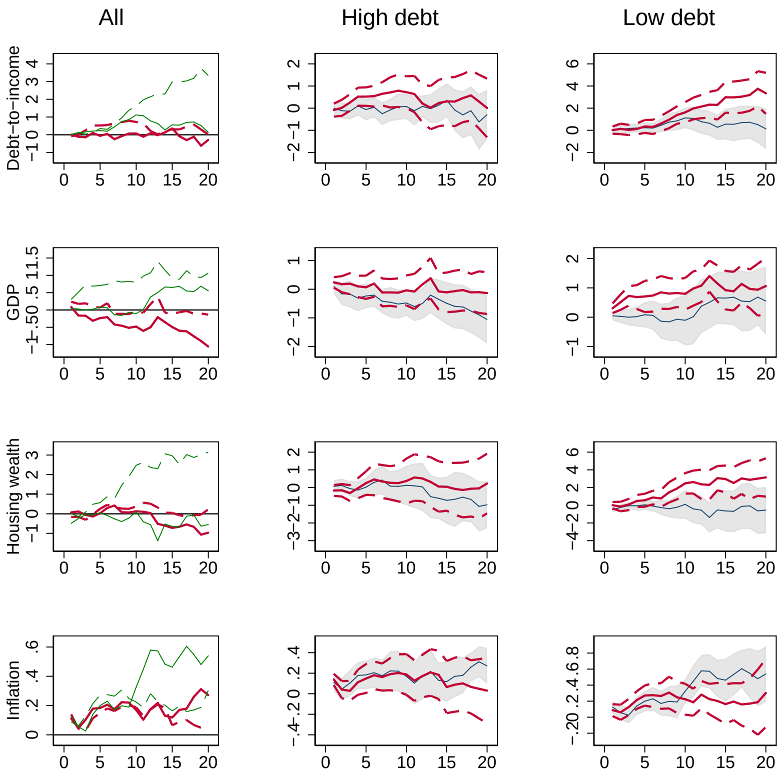
Notes: For the sake of comparison, the variables have been normalised to 2002q1=100. The grey shaded area refers to the official NBER recession over 2007q4-2009q2.

FIGURE B.8: Dispersion of debt gaps across US states



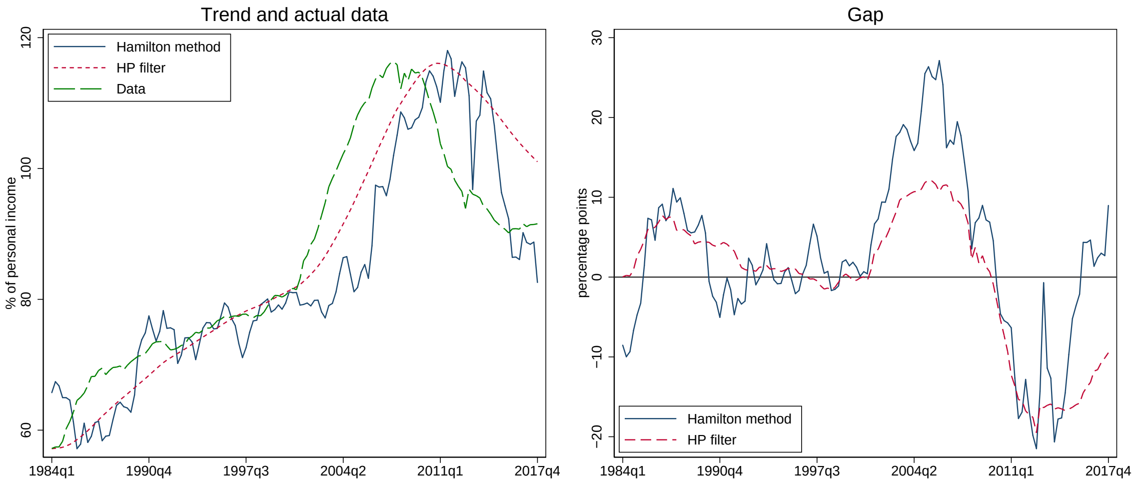
Notes: The figure shows the interquartile range of the debt gaps for the 30 US states.

FIGURE B.9: Household debt and the state of the economy: slack vs non-slack



Notes: Cumulative response of each variable to a one-standard deviation increase in the MPSG for h=1 to 20. 1^{st} column: red lines refer to peridos of high debt, and green lines to periods of low debt; solid lines to non-slack periods, and dashed lines to slack periods. 2^{nd} and 3^{rd} columns show the point estimates for periods of high and low debt, where the solid blue (red) line is the point estimate for non-slack (slack) periods, with the respective 90% confidence bands.

FIGURE B.10: Household debt-to-income: trend and cyclical component



Notes: The left-hand chart shows actual data for the DTI at the aggregate level, plus its trend component based on the Hamilton (2018) method and on a one-sided HP filter with a smoothing parameter of 400,000. The right-hand chart plots the cyclical component, or gap, of the DTI.

C Online appendix

Construction of the state-level CPI

The Consumer Price Index (CPI) published by the Bureau of Labor Statistics (BLS) measures the average change in prices over time in a fixed market basket of goods and services. In particular, I use in the paper the most commonly used index, the CPI for All Urban Consumers (CPI-U) which covers approximately 89% of the US total population. The CPI is based on prices of wide-ranging goods and services, such as food, clothing, shelter, and fuels, transportation fares, charges for doctors' and dentists' services, drugs, and other goods and services that people buy for day-to-day living. These goods and services are grouped into 211 item strata. Each month, the BLS collects prices in 87 urban areas across the country from about 4,000 housing units and approximately 26,000 retail establishments.[23] These 87 urban areas in which pricing is done for the CPI are called primary sampling units (PSU), corresponding to the Office of Management and Budget (OMB) definition of Metropolitan Areas (MA).[24]

Each PSU is first classified according to its size: PSUs with a population larger than 1.5 million are classified as self-representing type A;[25] types B and C refer to the remaining non-self-representing PSUs, metropolitan and non-metropolitan, respectively. A self-representing area represents only its own area definition, while a non-self-representing area stands for multiple area definitions. 31 out of the sampled 87 urban areas are classified as self-representing Type A areas, of which the BLS makes publicly available the CPI for 27 of them. In all of these PSUs, the CPI prices unique items in all of the 211 main item strata on a monthly, bi-monthly or semi-annual basis. The smaller PSUs in the non-self-representing areas are sampled using optimisation procedures, while the larger PSUs are sampled with certainty, and thus are designated self-representing areas.

[23] In January 2018, the BLS introduced a new geographic area sample for the CPI, consisting of 75 urban areas (large, medium, and small) across the country from about 5,000 housing units and approximately 22,000 retail establishments. The 2018 revision uses the 2010 Decennial Census and incorporates changes in the frequency of publication for several local area indexes, establishes new local area and aggregate indexes, and introduces Census division-level indexes. These changes, however, do not affect my CPI indices given that my dataset finishes in 2017.

[24] MA are Metropolitan Statistical Areas (MSA), Primary Metropolitan Statistical Areas (PMSA), or Consolidated Metropolitan Statistical Areas (CMSA).

[25] Anchorage, AK and Honolulu, HI are type A PSUs, although they both have populations smaller than 1.5 million.

In calculating the CPI of the 27 self-representing MSAs, the same procedures and methodologies are adopted as those used for computing the CPI of the US national.[26] In particular, price changes for the various items in each location are averaged together with weights that represent their importance in the spending of the appropriate population group. But due to the smaller sample size of a given MSA, its CPI is subject to more sampling and measurement error than the national index. As a consequence, the CPIs of the MSAs are more volatile than the national index, although their long-term trends are similar.

To construct a quarterly measure of consumer price inflation at the state-level over 1984q1-2017q4, I make use of 26 MSAs, and not 27, as a result of dropping Phoenix-Mesa, Arizona, given that its CPI is only available from 2002. Although these 26 MSAs cover a sub-set of the US states (30), my sample is quite representative of the US national, with the 30 states together accounting for around 82% of total US GDP. Moreover, the states are reasonably well covered by hard data stemming from the MSAs, with larger states, such as California, New York and Illinois, displaying a better coverage – above 75% as a share of personal income or population – while states with lower coverage, such as West Virginia, Indiana, and Kentucky, tend to have a relatively lower weight in US GDP (Figure C.1). Having said this, the next section shows that even states with low coverage tend to capture well the price dynamics within the state.

The original CPI data at the MSA level have different frequencies: monthly, bi-monthly (even or odd months) and semi-annual. I convert the different frequencies into quarterly data: for monthly data I take averages of the 3 months in a given quarter; for bi-monthly data I first interpolate the data to monthly and then calculate 3-month averages for each quarter; for semi-annual data I use the Chow-Lin interpolation method to produce quarterly data points by taking the US aggregate CPI as the indicator variable. Data for all MSAs are available since 1984q1, with the exception of Washington-Baltimore and Tampa-St.Petersburg-Clearwater, which results in the CPIs of Washington D.C., Maryland, Virginia and West Virginia starting in 1996q1, and Florida in 1997q4.

The computation of the state-level CPI requires the mapping of the MSA to the states. Appendix C contains the complete list of the available CPI data for the MSAs with the composition of the counties, and its allocation to the states. When a specific state includes

[26] For the complete details on the construction of the CPI for the US national, see Chapter 17 in the BLS Handbook of Methods, available at https://www.bls.gov/opub/hom/pdf/homch17.pdf.

counties from different MSAs, the state CPI will be the weighted average of the CPI of the relevant MSAs, taking personal income of the respective counties as weights. For example, the CPI of Connecticut is the income-weighted average of the counties (Fairfield, Litchfield, Middlesex, and New Haven counties) belonging to the CPI of New York-Northern New Jersey-Long Island, and of Windham county from Boston-Brockton-Nashua (Figure C.2 plots the state-level CPIs).

State-level CPI vs Implicit Regional Price Deflator

In the main paper I show that my bottom-up-aggregated state-level CPI does a good job at tracking the official CPI, with a correlation of 0.98 over 1984q1-2017q4. In addition, in this section I compare for each individual state the inflation rates between my state-level CPI and the BEA's Implicit Regional Price Deflator (IRPD) over 2009-15.

The regional price deflators published by the BEA on an annual basis, which they call Regional Price Parities (RPP), are price indexes that measure geographic price level differences within the United States.[27] The RPP are calculated using price quotes for a wide range of items from the CPI, which are then aggregated into broader expenditure categories. Since the RPP are expressed as a percentage of the overall national price level, the IRPD are obtained by multiplying the RPP by the national PCE price index.

The annual inflation rates of my state-level CPI are closely in line with those derived from the IRPD (Figure C.3). The differences in average inflation over 2009-15 between the two concepts are relatively small, with exception of a few states, particularly Alaska and Hawaii, which together account for less than 1% of US GDP. Overall, average annual inflation computed from my CPI indicator tends to stand above the one from the IRPD, with an average inflation of 1.46% over 2009-15, compared with 1.30% of the IRPD (Table C.1). This gap, however, may simply reflect differences in the computation of the indicators, as highlighted by the BEA, *'The growth rate of the implicit regional price deflators will not necessarily equal the region or metro area price deflators published by the BLS. This is because the CPI deflators are calculated directly while the IRPDs are indirect estimates, and because of differences in the source data and methodology.'*

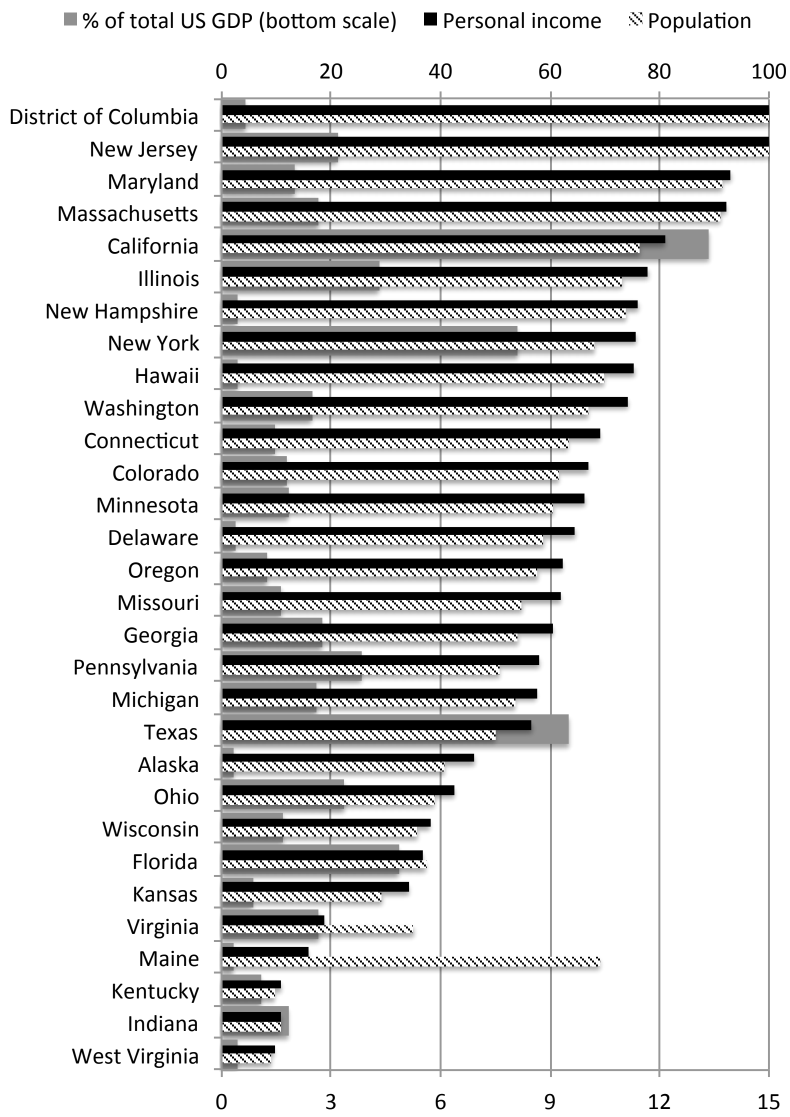
[27] Available at https://www.bea.gov/newsreleases/regional/rpp/2017/pdf/rpp0617.pdf

One of the concerns with the computation of the CPI indicator was related to some states having a low coverage by MSA data, which could bias the state CPI if the counties not covered by the MSA data display completely different price dynamics. The comparison between the CPI and the IRPD, however, attenuate these concerns: the inflation rates derived from the IRPD show that states that have a low coverage by MSA data do not exhibit larger differences in the 2009-15 average inflation between the CPI and the IRPD than the other states (Figure C.4). In fact, the correlation between the two is close to zero, indicating that there is no association between how much of the state is covered by MSA data and the 'quality' of the resulting CPI inflation when compared with the IRPD. For instance, states with a low coverage, such as West Virginia, Indiana, Kentucky, and Maine, have very similar inflation rates over the 2009-15 period. This gives further reassurance that the CPI indicator I constructed for the states captures adequately the overall price dynamics within a given state.

Table C.1: CPI inflation versus Implicit Regional Price Deflator: 2009-15

State	Correlation	CPI inflation	IRPD inflation	Dif
AK	0.58	2.03	1.15	0.89
CA	0.69	1.57	1.35	0.22
CO	0.94	1.99	1.70	0.29
CT	0.81	1.44	1.06	0.38
DC	0.63	1.56	1.49	0.07
DE	0.55	1.21	1.08	0.13
FL	0.99	1.50	1.13	0.37
GA	0.91	1.04	1.17	-0.13
HI	0.78	1.89	1.39	0.50
IL	0.80	1.03	1.26	-0.23
IN	0.93	1.05	1.24	-0.20
KS	0.96	1.49	1.49	0.00
KY	0.85	1.55	1.26	0.29
MA	0.93	1.26	1.13	0.13
MD	0.84	1.55	1.22	0.34
ME	0.67	1.26	1.31	-0.05
MI	0.96	0.97	1.01	-0.04
MN	0.96	1.49	1.32	0.18
MO	0.98	1.48	1.56	-0.09
NH	0.80	1.26	1.02	0.25
NJ	0.84	1.41	1.38	0.03
NY	0.90	1.44	1.34	0.10
OH	0.96	1.33	1.19	0.14
OR	0.82	1.84	1.45	0.39
PA	0.95	1.45	1.27	0.18
TX	0.96	1.36	1.36	0.01
VA	0.96	1.56	1.26	0.30
WA	0.76	1.53	1.53	0.00
WI	0.89	1.62	1.34	0.28
WV	0.83	1.56	1.62	-0.06
Average	0.85	1.46	1.30	0.15

FIGURE C.1: State coverage according to personal income and population - in %



Notes: Sorted by decreasing personal income coverage.

FIGURE C.2: State-level CPIs – yoy % change

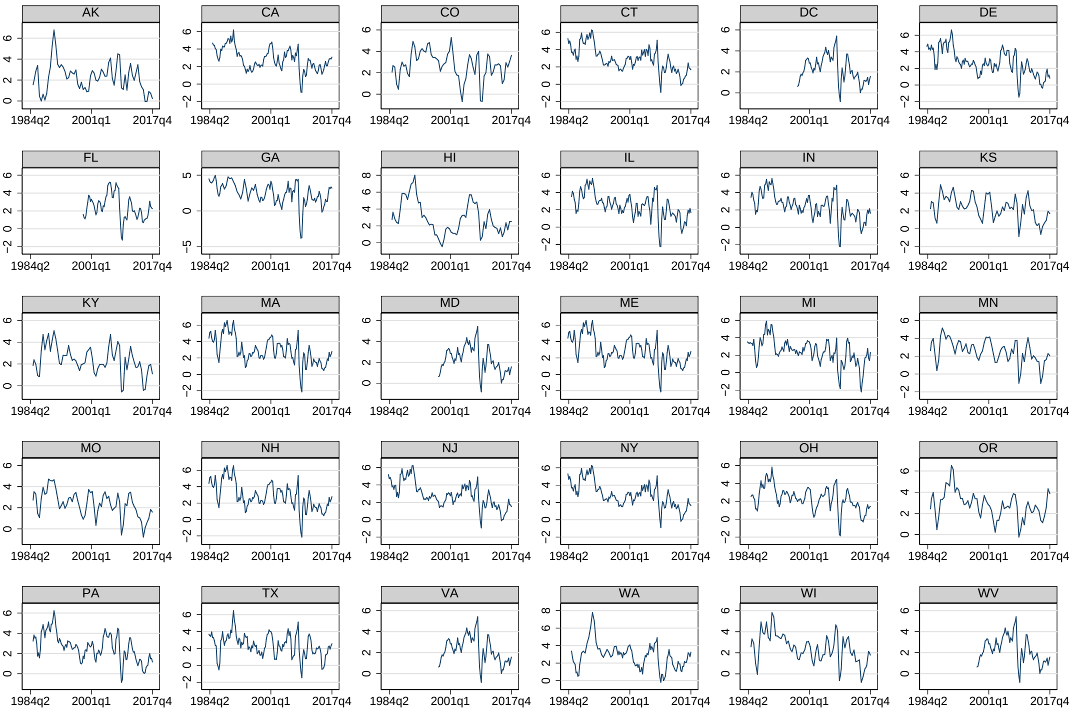


FIGURE C.3: CPI inflation versus Implicit Regional Price Deflator – annual % change

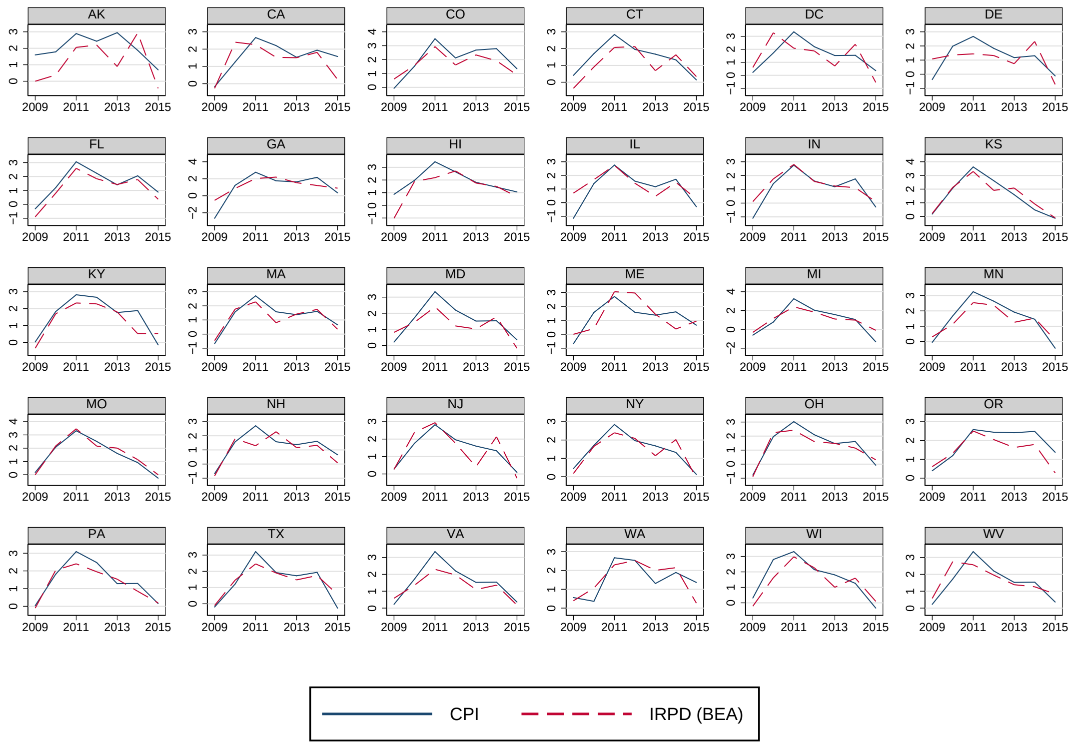
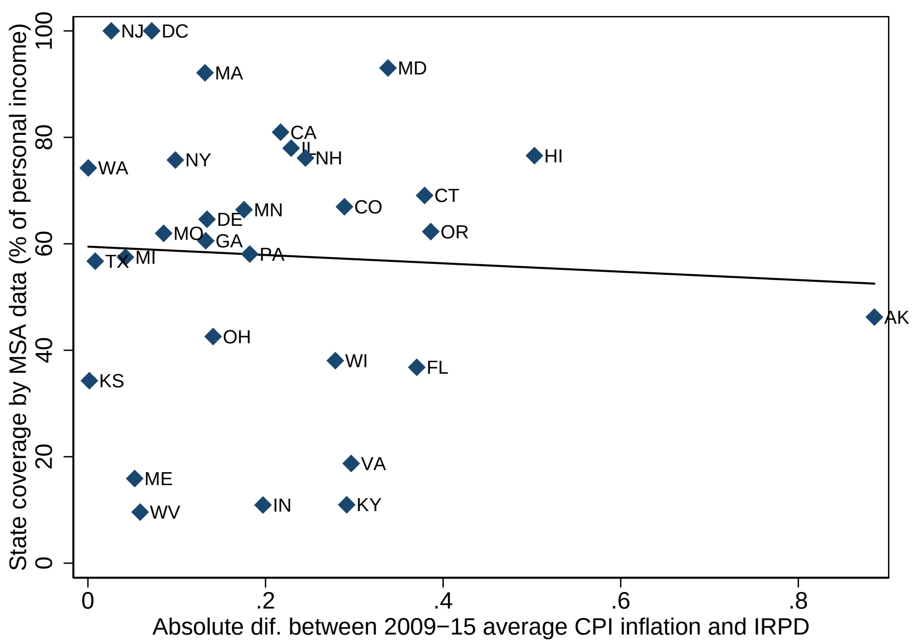


FIGURE C.4: Correlation between state coverage and differences in average CPI inflation and IRPD



List of MSA and respective counties

Alaska

Anchorage: Anchorage Borough.

California

Los Angeles-Riverside-Orange County: Los Angeles, Orange, Riverside, San Bernardino, Ventura.

San Francisco-Oakland-San Jose: Alameda, Contra Costa, Marin, Napa, San Francisco, San Mateo, Santa Clara, Santa Cruz, Sonoma, Solano.

San Diego: San Diego County.

Colorado

Denver-Boulder-Greeley: Adams, Arapahoe, Boulder, Broomfield, Denver, Douglas, Jefferson, Weld.

Connecticut

New York-Northern New Jersey-Long Island: Fairfield, Litchfield, Middlesex, New Haven.

Boston-Brockton-Nashua: Windham.

Delaware

Philadelphia-Wilmington-Atlantic City: New Castle.

District of Columbia

Washington-Baltimore: District of Columbia.

Florida

Miami-Fort Lauderdale: Broward, Miami-Dade.

Tampa-St. Petersburg-Clearwater: Hernando, Hillsborough, Pasco, Pinellas.

Georgia

Atlanta: Barrow, Bartow, Carroll, Cherokee, Clayton, Cobb, Coweta, DeKalb, Douglas, Fayette, Forsyth, Fulton, Gwinnett, Henry, Newton, Paulding, Pickens, Rockdale, Spalding, Walton.

Hawaii

Honolulu: Oahu County.

Illinois

Chicago-Gary-Kenosha: Cook, DeKalb, DuPage, Grundy, Kane, Kankakee, Kendall, Lake, McHenry, Will.

St. Louis: Clinton, Jersey, Madison, Monroe, St. Clair.

Indiana

Chicago-Gary-Kenosha: Lake, Porter.

Cincinnati-Hamilton: Dearborn, Ohio.

Kansas

Kansas City: Johnson, Leavenworth, Miami, Wyandotte.

Kentucky

Cincinnati-Hamilton: Boone, Campbell, Gallatin, Grant, Kenton, Pendleton.

Maine

Boston-Brockton-Nashua: York county.

Maryland

Philadelphia-Wilmington-Atlantic City: Cecil County.

Washington-Baltimore: Baltimore, Anne Arundel, Baltimore City, Calvert, Carroll, Charles, Frederick, Harford, Howard, Montgomery, Prince George's, Queen Anne's, Washington.

Massachussets

Boston-Brockton-Nashua: Essex, Middlesex, Norfolk, Plymouth, Suffolk, Bristol, Hampden, Worcester.

Michigan

Detroit-Ann Arbor-Flint: Genesee, Lapeer, Lenawee, Livingston, Macomb, Monroe, Oakland, St. Clair, Washtenaw, Wayne.

Minnesota

Minneapolis-St. Paul: Anoka, Carver, Chisago, Dakota, Hennepin, Isanti, Ramsey, Scott, Sherburne, Washington, Wright.

Missouri

Kansas City: Cass, Clay, Clinton, Jackson, Lafayette, Platte, Ray.

St. Louis: Crawford, Franklin, Jefferson, Lincoln, St.Charles, St. Louis, Warren, St. Louis City.

New Hampshire

Boston-Brockton-Nashua: Hillsborough, Merrimack, Rockingham, Strafford.

New Jersey

New York-Northern New Jersey-Long Island: Bergen, Essex, Hudson, Hunterdon, Mercer, Middlesex, Monmouth, Morris, Ocean, Passaic, Somerset, Sussex, Union, Warren.

Philadelphia-Wilmington-Atlantic City: Atlantic, Burlington, Camden, Cape May, Cumberland, Gloucester, Salem.

New York

New York-Northern New Jersey-Long Island: Bronx, Dutchess, Kings , Nassau, New York, Orange, Putnam, Queens, Richmond, Rockland, Suffolk, Westchester.

Ohio

Cleveland-Akron: Ashtabula, Cuyahoga, Geauga, Lake, Lorain, Medina, Porage, Summit.

Cleveland-Akron: Brown, Butler, Clermont, Hamilton, Warren.

Oregon

Portland-Salem: Clackamas, Columbia, Marion, Multnomah, Polk, Washington, Yamhill.

Pennsylvania

New York-Northern New Jersey-Long Island: Pike.

Philadelphia-Wilmington-Atlantic City: Bucks, Chester, Delaware, Montgomery, Philadelphia.

Pittsburgh: Allegheny, Armstrong, Beaver, Butler, Fayette, Washington, Westmoreland.

Texas

Dallas-Fort Worth: Collin, Dallas, Denton, Ellis, Henderson, Hood, Hunt, Johnson, Kaufman, Parker, Rockwall, Tarrant.

Houston-Galveston-Brazoria: Brazoria, Chambers, Fort Bend, Galveston, Harris, Liberty, Montgomery, Waller.

Virginia

Washington-Baltimore: Alexandria City, Arlington, Clarke, Culpeper, Fairfax, Fairfax City, Falls Church City, Fauquier, Fredericksburg City, King George, Loudoun, Manassas City, Manassas Park City, Prince William, Spotsylvania, Stafford, Warren.

Washington

Seattle-Tacoma-Bremerton: Island, King, Kitsap, Pierce, Snohomish, Thurston.

Portland-Salem: Clark county.

West Virginia

Washington-Baltimore: Berkeley, Jefferson.

Wisconsin

Chicago-Gary-Kenosha: Kenosha.

Milwaukee-Racine: Milwaukee, Ozaukee, Racine, Washington, Waukesha.

Minneapolis-St. Paul: Pierce, St. Croix.

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Chapter 3

Household Heterogeneity and Consumption Dynamics in the Presence of Borrowing and Liquidity Constraints

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Abstract*

I study the effects of borrowing and liquidity constraints on the response of consumption to anticipated income changes. Using the PSID over 1999-2013, I find that the well-documented strong excess sensitivity of consumption to income of highly-constrained households can be explained by episodes of income increases. In addition, I look into the heterogeneity of households without debt, a group that has been largely disregarded by the literature. My Fixed-Effects estimates show that only those without debt tend to increase their saving in response to anticipated income declines, irrespective of the amount of liquid assets held.

Keywords: *Consumption, Income, Highly-constrained households, Household heterogeneity*
JEL classification: *C8, D12, E21*

*This chapter has been published in *Applied Economics Letters* **26**(6), 454-459, 2019.

Introduction

In this paper, I test the excess sensitivity of consumption to income using the Panel Study of Income Dynamics (PSID) over 1999-2013 to exploit the heterogeneity across households, particularly by placing the focus on borrowing- and liquidity-constrained households. Following Johnson and Li (2010), highly-constrained households are those that devote the highest fraction of their income to servicing mortgage debt – proxy for borrowing constraints – and that hold low liquid assets – proxy for liquidity constraints.[1]

I investigate three main questions. First, I test whether consumption of households who are both borrowing and liquidity constrained react differently to income changes compared to other households. I find that consumption of these highly-constrained households displays a larger sensitivity to income, more than twice as large as the other households, which is consistent with Johnson and Li, 2010 who use the Consumer Expenditure Survey over 1992-2006.

The second question asks whether the consumption response is asymmetric relative to an expected income *increase* versus *decrease*. In fact, most of the related literature has focused on the *average response* of consumption to expected income changes.[2] My estimates suggest that the stronger excess sensitivity of consumption to income of the highly constrained is driven by episodes of income increases; since highly-constrained households are unable to borrow, or cannot do so as much as they would want, and have limited liquid savings, they can only increase consumption expenditures when the income increase materialises.

The third and final question looks explicitly at the group of households without debt and with different levels of liquidity, which, to the best of my knowledge, has been largely disregarded by the literature. I find that only households without debt, regardless of holding small or large liquid assets, cut consumption when they predict their income to fall.

[1] I focus on anticipated income changes and not on unanticipated income shocks. The former has the advantage of not making any assumption about the income process, with income shocks and the error term being modelled jointly. Jappelli and Pistaferri (2010) provide a comprehensive overview about the two different concepts. Nevertheless, in on-going research, I exploit my dataset against the backdrop of unexpected income shocks.

[2] There has been, however, more recent studies on the consumption response to *unanticipated* transitory positive and negative income *shocks*, such as Bunn et al., 2018 and Christelis et al., 2019.

3.1 The data

I use the PSID, a US nationally representative sample, which covers data on employment, income, wealth, expenditures, and a set of household characteristics, such as age, race, marital status, sex, and education. I focus on 1999-2013 (with data every two years), since consumption before 1999 only covers food. Non-housing consumption encompasses spending on food, vehicle-related, utilities, health care, and education. In turn, family income is before taxes, referring to the preceding tax year.

In the spirit of Johnson and Li (2010), I focus on highly-constrained households, who are both borrowing and liquidity constrained, versus unconstrained (the remaining households). Borrowing constraints relate to households not having ready access to credit or not being allowed to borrow as much as they want because they have exhausted their credit limit. The proxy for borrowing constraints is a high Debt Service Ratio (DSR). In turn, liquidity constraints refer to low liquid assets (e.g. money in checking or savings accounts, money market funds, government savings bonds, and treasury bills). I take those households holding small liquid assets relative to their income, the Liquidity-to-Asset Ratio (LAR), as a proxy for liquidity constraints.

One of the novelties of the paper is in modelling households without debt and different liquidity levels, who account for almost 60% of the sample. For instance, Johnson and Li (2010) restrict their analysis to those that hold any form of debt, while Cloyne and Surico (2017), in a study of how households react to monetary policy shocks, split households based on their housing tenure status, regardless of their debt and liquidity levels (mortgagors and outright owners versus renters). In turn, Kaplan et al. (2014) use the PSID to study the reaction of consumption of wealthy Hand-to-Mouth (HtM) and poor HtM (distinguished by their illiquid wealth) to transitory income shocks, although no explicit reference is made to households without debt, let alone the split regarding different liquidity levels.

In this context, I allocate households without debt and with low liquid assets to the highly-constrained group, as they are more likely to be borrowing and liquidity constrained, while the remaining households without debt and with higher liquidity are assigned to the residual (unconstrained) group. The typical household without debt and with low liquidity exhibits several characteristics commonly associated with less-privileged groups and, therefore, with borrowing constraints: low-income, with little or no wealth, hold small amounts of non-mortgage debt, most likely because they are prone to being credit-constrained, devote most of their income to consumption – implying a large MPC – particularly to food, and are the least educated.[3]

3.2 Econometric framework

To test the excess sensitivity of consumption to income, I estimate a model with Fixed Effects over 1999-2013. Φ_i is a 0/1 dummy variable that takes the value of 1 for households who are both borrowing and liquidity constrained (the subscript C stands for *Constrained*):

$$\begin{aligned}\Delta \log(C_{i,t}) =& \beta \Delta \log(Inc_{i,t}) + \lambda \Delta \log(W_{i,t}) + \delta \log(X_{i,t-1}) \\ &+ \Phi_i \left[\beta_C \Delta \log(Inc_{i,t}) + \lambda_C \Delta \log(W_{i,t}) + \delta_C \log(X_{i,t-1})\right] \\ &+ \eta_i + \zeta_t + \epsilon_{i,t}\end{aligned} \tag{3.1}$$

The traditional determinants of consumption are income (*Inc*), and net wealth (W); X includes a set of control variables lagged one period, *Wealth*, *Income*, *DSR*, state-specific variables (*Unemployment rate* and *House prices*), and household-specific characteristics, *Age* of the head, *Family size*, dummies for the *Employment status*, and *College degree*; i refers to households, and t to time (data every two years). η_i is the household-specific fixed effect capturing unobserved time-invariant heterogeneity, and ζ_t captures the unobserved time-variant common factors across units in the panel. With the exception of the ratios, I deflate all nominal variables with the national Consumer Price Index (CPI).

The coefficients of interest are the *betas*, which measure how consumption growth reacts to anticipated income growth: unconstrained (β) versus highly constrained (sum of β and β_C). I follow the same households for at least three consecutive surveys, resulting in an unbalanced panel of N=24,496, of which 6,877 are distinct households.

Regarding econometric concerns, consumption growth can potentially affect income growth – reverse causality – or income growth can be correlated to shocks to consumption growth, $\epsilon_{i,t}$. This potential endogeneity can be minimised by lagging income growth one period. But since income in the PSID dataset refers to income relative to the preceding

[3] Own tabulations of the PSID dataset available upon request.

calendar year, there is no need to lag income growth again. But I also run IV regressions by instrumenting income growth with its own two lags, finding qualitatively similar results (available upon request).

3.3 Results

3.3.1 The excess sensitivity of consumption growth

The estimates from Table 3.3.1 show that consumption of both highly-constrained and unconstrained households is excessively sensitive to income, in line with the findings of Johnson and Li, 2010. Furthermore, highly-constrained households display the highest excess sensitivity, more than twice as large as the other (unconstrained) households. This might be rationalised in constrained households having limited means – restricted or no access to credit and low liquid savings – to expand consumption before the income change takes place. This finding is robust to different groupings, whether considering the top or the top two DSR quintiles, the first or first two LAR quintiles, or thresholds for the latter.

TABLE 3.3.1: Excess sensitivity of consumption

	(1)	(2)	(3)	(4)	(5)	(6)
Unconstrained	0.032*	0.043***	0.032*	0.045***	0.043***	0.037***
	(0.018)	(0.018)	(0.017)	(0.014)	(0.014)	(0.015)
Highly constrained						
LAR Q1-Q2 & DSR Q5	0.083***					
	(0.013)					
LAR Q1 & DSR Q5		0.094***				
		(0.013)				
LAR Q1-Q2 & DSR Q4-Q5			0.084***			
			(0.014)			
LAR Q1 & DSR Q4-Q5				0.094***		
				(0.014)		
LAR <2.5% & DSR Q4-Q5					0.089***	
					(0.013)	
LAR <5% & DSR Q4-Q5						0.083***
						(0.013)
Test (p-value)	0.02	0.00	0.01	0.00	0.00	0.00

Notes: FE regressions with time dummies, where the dependent variable is consumption growth. The control variables are not reported. The coefficients for the highly-constrained households are computed as the sum of β and β_C from Eq. 3.1. P-value of a Wald test of the equality of the coefficients between constrained and unconstrained households. Standard errors clustered by household in parentheses. ***,**,* denote statistical significance at the 1, 5 and 10% levels.

A constrained household might arguably behave differently if income is expected to rise or fall. In fact, consumption may display a stronger excess sensitivity to predictable income increases than declines since constrained households can save when they expect income to fall, but cannot borrow (or not as much as they would want) amid limited liquid savings when income is expected to rise. I now modify slightly the previous model (Eq. 3.1), where β^+ and β^- refer to the response of consumption to anticipated income increases and decreases:

$$\begin{aligned}\Delta\log(C_{i,t}) =&\beta^+\Delta\log(Inc_{i,t})+\beta^-\Delta\log(Inc_{i,t})+\lambda\Delta\log(W_{i,t})+\delta\log(X_{i,t-1})\\ &+\Phi_i\left[\beta_C^+\Delta\log(Inc_{i,t})+\beta_C^-\Delta\log(Inc_{i,t})+\lambda_C\Delta\log(W_{i,t})+\delta_C\log(X_{i,t-1})\right]\\ &+\eta_i+\zeta_t+\epsilon_{i,t}\end{aligned} \tag{3.2}$$

I find that consumption growth of unconstrained households correlates equally with income increases and declines (Table 3.3.2).[4] In contrast, consumption of the highly constrained responds more strongly to income increases: when the income increase materialises, the excess sensitivity of consumption is around three times as large as that of unconstrained households. This difference in the consumption response between the two groups is statistically significant at the 5% level. In contrast, there is no statistical evidence that consumption of constrained households reacts to an anticipated income decrease differently than that of unconstrained households.

These results shed more light on why consumption of highly-constrained households displays a much stronger excessive sensitivity to income than unconstrained households: the phenomenon is driven by *income increases*.

3.3.2 Households without debt

Until now, I have allocated households without debt and with low liquid assets to the *highly-constrained* group, and the remainder households without debt have been considered *unconstrained*. A closer look at the group of households without debt reveals, however, significant heterogeneity along several economic and demographic dimensions, building a case for modelling these four groups separately. For example, compared to the highly-constrained (with

[4] To keep the analysis parsimonious, I proxy highly-constrained households with: the first quintile of the LAR with the top two DSR quintiles (*LAR Q1 & DSR Q4-Q5*); and the LAR smaller than 2.5 % with the top two DSR quintiles (*LAR <2.5% & DSR Q4-Q5*).

Table 3.3.2: Asymmetries in the excess sensitivity of consumption

	(1) LAR Q1 & DSR Q4-Q5	(2) LAR <2.5% & DSR Q4-Q5
Δ Inc$\geq$0		
Unconstrained	0.036***	0.036***
	(0.012)	(0.012)
Constrained	0.115***	0.106***
	(0.029)	(0.026)
Test (p-value)	0.02	0.03
Δ Inc<0		
Unconstrained	0.057***	0.055**
	(0.022)	(0.022)
Constrained	0.067*	0.071**
	(0.038)	(0.035)
Test (p-value)	0.83	0.72

Notes: FE regressions with time dummies, where the dependent variable is consumption growth. Coefficients for unconstrained (β^+ for income increases and β^- for decreases from Eq. 3.2) and constrained households (sum of β and β_C with the plus or minus subscripts). I use a different grouping for highly-constrained households in each column. P-values of a Wald test of the equality of the coefficients between constrained and unconstrained households. Standard errors clustered by household in parentheses. ***,**,* denote statistical significance at the 1, 5 and 10% levels.

high debt and low liquidity), households without debt and low liquidity have lower consumption levels but a higher implicit MPC, are poorer and younger, while concentrating a larger fraction of poor HtM (Table 3.3.3).

Table 3.3.3: Descriptive statistics for selected groups of households (median values)

	Without debt		With debt	
	No liquid assets & LAR Q1	Rest	Unconstrained	Highly-constrained
N	8,998	10,549	6,331	1,667
%	27.0	31.7	19.0	5.0
Real consumption (thous. USD)	8.69	11.71	18.16	13.60
Consumption/income	0.32	0.25	0.18	0.27
Family income (thous. USD)	26.72	46.37	102.05	50.56
LAR	0.00	0.17	0.11	0.00
Illiq. Wealth	0.13	1.67	1.46	0.93
DSR	-	-	0.11	0.23
Age	42	54	45	48
Hand-to Mouth (%)				
Poor HtM	27.1	2.3	0.6	8.8
Wealthy HtM	66.8	21.2	28.2	85.2
Non-HtM	6.1	76.5	71.2	6.1
Housing tenure (%)				
Mortgagors	-	-	100.0	100.0
Outright owners	22.1	53.3	-	-
Renters	69.1	40.7	-	-

Notes: The % of households do not sum up to 100 as I left out those that do not fall into either of the listed categories, i.e. those considered only borrowing constrained but not liquidity constrained (top two DSR quintiles and above the first LAR quintile) and vice-versa (first LAR quintile and below the top two DSR quintiles).

I replicate the previous estimates in Section 3.3.1 with the above four groups of households: *Constrained* encompass households with high debt and low liquid assets; *Unconstrained* are those with low debt and high liquidity; *No debt* & *LQ* refer to households without debt and with low liquid assets; and *No debt* & *not LQ* are those households without debt and high liquid assets.

Overall, I find that the estimates from the new groups yield similar results relative to Table 3.3.1: households without debt and with low liquid assets compared with the constrained group, and households without debt and high liquidity compared with the unconstrained (Table 3.3.4).

TABLE 3.3.4: Excess sensitivity of consumption – households without debt modelled separately

	(1) LAR Q1 & DSR Q4-Q5	(2) LAR <2.5% & DSR Q4-Q5
Unconstrained	0.068***	0.065***
	(0.015)	(0.015)
Constrained	0.055***	0.071***
	(0.020)	(0.026)
Test (p-value)	0.65	0.84
No debt & LQ	0.100***	0.100***
	(0.015)	(0.014)
Test (p-value)	0.11	0.39
No debt & not LQ	0.029	0.030*
	(0.018)	(0.018)
Test (p-value)	0.01	0.01

Notes: FE regressions with time dummies, where the dependent variable is consumption growth. I use a different grouping for highly-constrained households in each column. *No debt* & *LQ* refer to households without mortgage debt and liquidity constrained. P-values of a Wald test of the equality of the coefficients between the different sub-groups. Standard errors clustered by household in parentheses. ***,**,* denote statistical significance at the 1, 5 and 10% levels.

The baseline results are, however, more sensitive to asymmetries in the response of consumption to income increases and declines (Table 3.3.5). First, I find that *Constrained* households display a higher excessive sensitivity to an income increase compared with *No debt* & *LQ* households, those without debt and low liquidity. Second, households without debt and not liquidity constrained are the only group not reacting to income increases. One possibility is that they may resort to their financial liquid cushions to front-load consumption before the income increase materialises. But then an open question is why the unconstrained do not also anticipate spending by drawing down their liquid assets.

Third, I find that only households without debt respond to anticipated income declines, by increasing their saving, irrespective of the amount of liquid assets. This result casts some doubt about whether some of the households without debt and with non-negligible liquid assets – who have been considered unconstrained before – are also not somewhat constrained.

TABLE 3.3.5: Asymmetries in the excess sensitivity – households without debt modelled separately

	(1) LAR Q1 & DSR Q4-Q5		(2) LAR <2.5% & DSR Q4-Q5	
	Δ Inc≥0	**Δ Inc<0**	**Δ Inc≥0**	**Δ Inc<0**
Unconstrained	0.083***	0.039	0.084***	0.028
	(0.021)	(0.048)	(0.017)	(0.051)
Constrained	0.176***	-0.032	0.178***	-0.009
	(0.038)	(0.042)	(0.037)	(0.051)
Test (p-value)	0.09	0.24	0.09	0.58
No debt & LQ	0.109***	0.087**	0.110***	0.087**
	(0.028)	(0.043)	(0.027)	(0.044)
Test (p-value)	0.04	0.04	0.09	0.11
No debt & not LQ	0.005	0.059**	0.010	0.057**
	(0.015)	(0.027)	(0.014)	(0.027)
Test (p-value)	0.01	0.61	0.01	0.57

Notes: FE regressions with time dummies, where the dependent variable is consumption growth. Total coefficients using a different grouping for highly-constrained households in the two columns. *No debt & LQ* refer to households without mortgage debt and liquidity constrained. P-values of a Wald test of the equality of the coefficients between the different sub-groups. Standard errors clustered by household in parentheses. ***,**,* denote statistical significance at the 1, 5 and 10% levels.

3.4 Concluding remarks

The main findings of the paper suggest that consumption of highly-constrained households responds to income in a significantly stronger fashion compared with other households. While this is in line with Johnson and Li (2010), I have gone one step further to show that this finding is the result of episodes of income increases. Constrained households can save when they expect income to fall, but cannot borrow nor use their limited liquid savings when income is expected to rise.

Another contribution of the paper is to treat households without debt and with different levels of liquid assets separately. I have found, in particular, that only households without debt, regardless of having small or large liquid assets, cut consumption when they predict their income to fall. The similar consumption response between households without debt but with different liquidity amounts suggest that there may be additional factors at play, beyond liquidity and borrowing motives, in explaining their identical reaction, such as concerns about lower future income, behavioural causes or different attitude towards risk.

Acknowledgements

I would like to thank Gert Peersman, Sebastian Rüth, and participants at the Macro Research Group meeting, the 21^{st} T2M 2017 Conference, Lisboa, Portugal, the 22^{nd} SMYE 2017 Conference, Halle (Saale), Germany, for their comments and suggestions. I acknowledge financial support from the UGent Special Research Fund (BOF).

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Chapter 4

Changing supply elasticities and regional housing booms

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Abstract

Recent developments in US house prices mirror those of the 1996-2006 boom, but the recovery in construction activity has been weak. Using data for 254 US metropolitan areas, we show that housing supply elasticities have fallen markedly in recent years. Housing supply elasticities have declined more in areas where land-use regulation has tightened the most, and in areas that experienced the sharpest housing busts. A lowering of the housing supply elasticity implies a stronger price responsiveness to demand shocks, whereas quantity reacts less. Consistent with this, we find that an expansionary monetary policy shock has a considerably stronger effect on house prices during the recent recovery than during the previous housing boom. At the same time, building permits respond less.

Keywords: *House prices; Heterogeneity; Housing supply elasticities; Monetary policy*
JEL classification: *C23, E32, E52, R31*

Introduction

At the end of 2017, nominal US house prices were almost ten percent above the pre-recession peak. Despite the strong rise in house prices, construction activity has remained low and is considerably weaker than during the previous housing boom. A similar pattern is evident at the regional level. We document that this is related to a recent decline in housing supply elasticities. Furthermore, we argue that there are large regional differences in the extent of the decline. Against this background, we ask the following questions: (i) How does the decline in housing supply elasticities impact house price volatility and the transmission of housing demand shocks?; and (ii) What factors have contributed to changing housing supply elasticities?

We consider a quarterly panel data set covering 254 US Metropolitan Statistical Areas (MSAs), spanning the previous boom episode (1996–2006) and the recent recovery (2012–2017). Our analysis is confined to the two boom periods. While housing busts are interesting to analyze, there are two main reasons why we focus on boom episodes. First, our main interest is to study the different dynamics across similar housing episodes. Second, the durability of housing entails that housing supply is rigid downwards (Glaeser and Gyourko, 2005), implying that the elasticity should fall towards zero in a bust. Since this should hold in all markets, local-specific factors, such as differences in topography and housing market regulation, should not matter for the responsiveness of housing supply during a bust. For each of the sub-samples, we estimate MSA-specific housing supply elasticities, using building permits as the dependent variable. The housing supply elasticity is computed as the coefficient on house prices, controlling for numerous MSA-specific variables that may affect housing supply. This exercise is non-trivial for at least two reasons. First, there are large regional variations. Second, there is likely reverse causality between construction activity and house prices.

With respect to regional variations, theory suggests that local differences in topography and regulation should impact housing supply elasticities. We take this into account by interacting house prices with the index of topographical constraints calculated by Saiz (2010) and with the index of regulatory restrictions from Gyourko et al. (2008). To deal with reverse causality, we use an instrumental variable (IV) approach. Our identification problem requires separating housing demand from housing supply. We consider two instruments for house prices that we argue lead to shifts in housing demand, but that do not shift housing supply. The first instrument exploits variation in crime rates across MSAs and over time, compiled by the Federal Bureau of Investigation (FBI). Given the negative impact crime can have on society, crime can be viewed as a negative amenity (Pope and Pope, 2012). Crime rates should therefore capture exogenous variations in (negative) amenities that drive house price changes both across and within MSAs over time. The second instrument is real personal disposable income. Income is one of the main determinants of housing and consumption demand in standard macro and housing models (Dougherty and Van Order, 1982; Buckley and Ermisch, 1983; Meen, 1990; Muellbauer and Murphy, 1997; Meen, 2001; Meen, 2002; Duca et al., 2011), but typically does not affect housing supply directly. Thus, from a theoretical point of view, this instrument should satisfy both the relevance and exogeneity conditions.

Our IV-estimates suggest that housing supply elasticities have declined. A direct implication of lower supply elasticities is that a given change in demand should have a stronger effect on house prices. We explore the relevance of this conjecture through the use of exogenous monetary policy shocks. Following a recent strand of the literature, we use high-frequency data to identify unexpected changes in the Fed policy rate (see e.g, Gürkaynak et al., 2005; Gertler and Karadi, 2015; Nakamura and Steinsson, 2018). The high-frequency identified (HFI) shocks isolate news about future policy actions that are orthogonal to changes in economic and financial variables. We then use a local projection instrumental variable approach (Jordà et al., 2015; Ramey, 2016; Stock and Watson, 2018) to explore how monetary policy shocks affect house prices and permits in the two booms.

Our results show considerable heterogeneity in responses across local housing markets. We estimate a substantially greater response in house prices to a monetary policy shock in supply-inelastic markets than in areas with an elastic supply. This holds true for both boom periods. We also document a substantial increase in the responsiveness of house prices to monetary policy shocks in recent years. In particular, our results suggest that for a metro area with a median housing supply elasticity, an exogenous monetary policy shock that lowers the interest rate by one percentage point led to an increase in real house prices of about ten percent after four years during the 1996-2006 boom. For the 2012-2017 recovery, the estimated response is 16 percent. Consistent with this, we find that building permits today increase about three percentage points less in response to the monetary policy shock.

We also find that there are regional differences in how much elasticities have declined. There are several reasons why housing supply elasticities may differ across areas and change over time (Green et al., 2005), including changes in regulation, demographics, and in expectations about future demand and house prices. In a recent study, Herkenhoff et al. (2018) show that there have been substantial changes in residential land-use regulation in most US states over time. Using their measure of time-varying land-use regulation, we find that elasticities have declined the most in areas where regulation has tightened more. Our results also suggest a larger decline in elasticities in areas that experienced the largest decline in house prices at the end of the previous decade. We interpret this as evidence that the fear of a new bust has led developers to be less price-responsive than before.

The results in this paper relate to several strands of the literature. First, a vast number of papers have emphasized local differences in housing supply elasticities as a central driver of cross-sectional variation in US house price developments (see e.g., Green et al., 2005; Gyourko et al., 2008; Saiz, 2010; Huang and Tang, 2012; Glaeser et al., 2014; Anundsen and Heebøll, 2016). This literature has used time-invariant measures of housing supply elasticities to explore cross-sectional variation over the course of a boom-bust cycle, finding that supply-inelastic areas experience stronger house price booms than areas with an elastic housing supply. Our results are consistent with this view, but go a step further by showing that housing supply elasticities may change over time even within the same local market. This contributes to affect local – and possibly aggregate – house price volatility over time.

Second, there is a growing literature looking at the nexus between monetary policy and house prices (see e.g., Iacoviello, 2005; Del Negro and Otrok, 2007; Jarocinski and Smets, 2008; Jordà et al., 2015; Williams, 2011; Williams, 2015). These papers focus on the aggregate effects on house prices, which masks potential heterogeneity across regional housing markets. One exception is Aastveit and Anundsen (2017), who study the asymmetric effects of monetary policy on regional house prices for a sample ending in 2007Q4. We add to this literature by documenting non-trivial heterogeneous responses of regional house prices to a common monetary policy shock for both the 1996-2006 boom and the 2012-2017 boom. Furthermore, we document a sizeable drop in housing supply elasticities over time, which makes house prices even more responsive to monetary policy shocks today. Paul (2019) finds that the transmission of monetary policy to financial variables, such as stock prices and house prices, has become stronger over time. Our work can provide an economic interpretation of these findings: due to the lowering of housing supply elasticities, an aggregate shock that raises housing demand is absorbed mostly by house prices rather than through an increase in quantity.

Herkenhoff et al. (2018) argue that the stronger tightening of residential land-use regulation in highly productive states, particularly California and New York, has restricted the available land for housing and commercial use, raised house prices, reduced capital and labor reallocation, resulting in a substantial decrease in output and productivity. In a similar vein, Ganong and Shoag (2017) find that the decline in income convergence and migration rates across states since the 1980s can – at least partly – be attributed to tight land-use regulation and rising house prices in high-income states. Hsieh and Moretti (2019) document that stringent housing restrictions in highly-productive areas, such as New York and San Francisco Bay Area, result in significant output costs in the form of spatial misallocation of labor across US cities. In addition, Glaeser and Gyourko (2018) posit that highly regulated areas are characterized by higher house prices and smaller population growth relative to the level of demand. Our results relate to this literature by documenting that the tightening of land-use regulation has resulted in a lower supply elasticity, which in turn amplifies the responsiveness of house prices to demand shocks.

Our results are robust along several dimensions. We show that the decline in housing supply elasticities is evident when: (i) employing a Bartik-type instrumental variable approach; (ii) using total crime rates (sum of property crime and violent crime) as the crime variable instrument; (iii) using permit intensity as the dependent variable to allow the dynamics in permits to differ according to the existing stock of houses; (iv) replacing the measures of topographical and regulatory constraints with a summary measure of supply restrictions to account for the possibility that these two indicators might be correlated; and (v) controlling for mortgage originations to assess the impact on the housing supply response of subdued credit developments since the Great Recession. Finally, our results are robust to estimating supply elasticities using 10-year and 15-year rolling windows.

The rest of the paper proceeds as follows. In the next section, we offer a descriptive analysis of the housing boom in the 2000's and the ongoing boom. In Section 4.2, we describe the data and some stylized facts about the US housing cycle over the past 20 years. We discuss our econometric approach and estimate local housing supply elasticities for the two boom periods in Section 4.3. In Section 4.4, we analyze how changing supply elasticities affect housing market dynamics. In Section 4.5, we explore the factors that have led to declining housing supply elasticities. Robustness checks and alternative explanations for the disconnect between house prices and housing supply are discussed in Section 4.6. Section 4.7 concludes the paper.

4.1 The 1996-2006 boom versus the 2012-2017 recovery

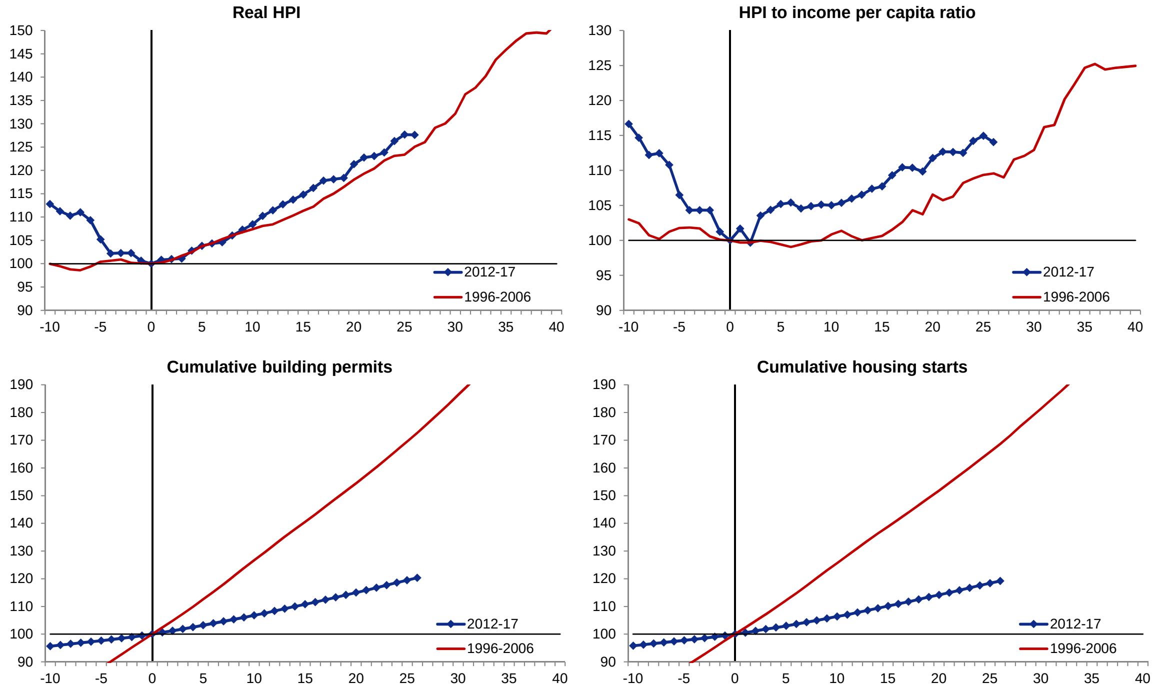
At the national level, real US house prices have increased by more than 26 percent since the beginning of the housing recovery in mid-2012. The dynamics of real house prices during the recovery is similar to that of the previous housing boom. This is illustrated in the upper left panel of Figure 4.1.1, where we plot real house prices for both the 1996-2006 boom (red line) and the 2012-2017 recovery (blue line). We have scaled the price index so that it takes a value of 100 at the beginning of each period. The horizontal axis shows quarters around the beginning of the two booms, while the vertical line at zero is the starting point of both booms. In the upper right panel, we perform the same exercise when deflating house prices by per capita income. Remarkably, the current boom looks far stronger relative to income than the previous boom.[1] Although our house price index is a weighted repeat-sales index, measuring average price changes in repeat sales or refinancings on the same properties, we observe the same pattern in house prices across booms for new homes (Figure D.2 in Appendix D).

Despite similar – or even stronger – developments in house prices, housing supply has grown substantially less during the current boom (lower panel of Figure 4.1.1). While the cumulative increases in total building permits and housing starts were roughly 60 percent over the first five to six years of the previous boom, the cumulative increase between 2012 and 2017 has been around 16 percent. This holds true for both single-family and multi-family units, although the multi-family segment has recovered somewhat faster (Figure D.3

[1] The strong developments in house prices relative to income per capita can be partially attributed to subdued income and consumption growth, as illustrated in Figure D.1 in Appendix D.

in Appendix D). Our measure of housing supply is building permits. Nevertheless, similar developments have been seen for existing homes available for sale (Figure D.4).[2]

FIGURE 4.1.1: House price developments across booms



Sources: Bureau of Economic Analysis, Census Bureau, Federal Housing Finance Agency, and authors' calculations.
Notes: The figure shows developments in real house prices, house prices relative to income per capita, building permits, and housing starts during 1996q4–2006q4 (red solid line) and 2012q3–2017q4 (blue line with markers). The series are scaled such that they take a value of 100 at the beginning of both periods. The horizontal axis shows quarters around the beginning of the two booms, and the vertical line at zero is the starting point of both booms.

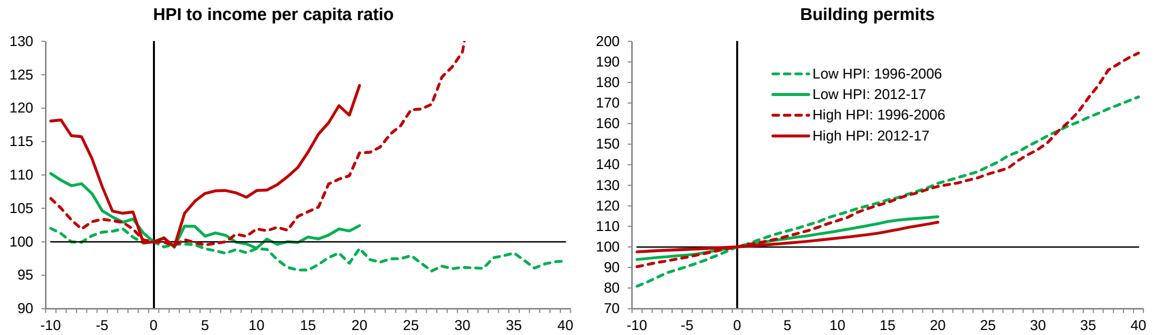
Housing is characterized by important regional heterogeneities (Ferreira and Gyourko, 2012). We use MSA-level data and break the sample into quartiles of the cumulative house price change between 1996 and 2006. We define *Low HPI* MSAs as the areas belonging to the first quartile, while *High HPI* MSAs refers to the fourth quartile. We then compare the evolution of house prices relative to income and permits across the two booms (Figure 4.1.2). The red lines illustrate developments for the High HPI group, and green lines for the Low HPI group. To distinguish between the two periods, we use dotted lines for the 1996-2006 period and solid lines for the 2012-2017 period. Mirroring the aggregate picture, house prices relative to income per capita have increased more during the current boom

[2]In the current housing recovery, there has been a close link between new residential construction and the supply of existing homes listed for sale. With fewer new homes to choose from, many homeowners considering upgrading have chosen to remain in their current homes, and therefore have not listed them for sale. This has prevented other homeowners from upgrading as well, limiting the number of existing homes available for sale even further. Despite rising house prices in both the new and existing home segments, this 'vicious circle' between limited new homes in the market leading to a tight supply of existing homes for sale has been the norm in the current boom (Rappaport, 2018).

for both groups. At the same time, this ratio has increased most for the High HPI MSAs. In contrast, permits have progressed at a sluggish pace during the current recovery, with a slightly weaker expansion in High HPI MSAs.

The marked differences in housing market developments across metropolitan areas highlight the importance of studying regional markets. The use of disaggregated data follows the most recent housing market literature, which tends to look at the housing market as a collection of several markets that differ not only by geography but also by other attributes – see Piazzesi and Schneider (2016) for a survey.

FIGURE 4.1.2: Housing indicators for MSA groups across housing booms



Sources: Bureau of Economic Analysis, Census Bureau, Federal Housing Finance Agency, Moody's Analytics, and authors' calculations.
Notes: The figure shows developments in house prices relative to income per capita and permits for 1996q4–2006q4 (dotted lines) and 2012q3–2017q4 (solid lines). *Low HPI* MSAs (green) are the areas that recorded the smallest cumulative growth in house prices over 1996-2006, as measured by the first quartile, whereas *High HPI* MSAs (red) refers to the fourth quartile. The series are scaled such that they take a value of 100 at the beginning of each period. The horizontal axis shows quarters around the beginning of the two booms, and the vertical line at zero is the starting point of both booms.

4.2 Data and housing market cycles

4.2.1 Data

We use quarterly data for a panel of 254 MSAs between 1996 and 2017. The sample covers more than 80 percent of US income and population. Our MSA definitions follow the new delineations issued by the Office of Management and Budget (OMB), based on the 2010 Census.

The MSA data on housing supply encompass building permits, housing starts, and the housing stock. In addition, we have data on house prices, and controls for macroeconomic, financial and socio-demographic conditions: personal disposable income, unemployment rates, mortgage originations, population, crime rates, dependency ratio (ratio of people younger than 15 or older than 64 relative to those aged 15-64), and the fraction of Blacks and Hispanics relative to the total population. We also use wages and salaries in the construction sector to proxy builders' costs. This series is available only at the state level. We deflate all nominal macroeconomic series with the MSA-level consumer price index (CPI). The MSA data have been provided by Moody's Analytics, with the original sources of the data coming mainly from the Census Bureau, Bureau of Economic Analysis (BEA), Bureau of Labor Statistics (BLS), and Federal Housing Finance Agency (FHFA). The exception is the crimes rates, which we compiled from publicly available reports from the FBI. A full list of variables, sources, and descriptive statistics are provided in Appendix B.

We control for regional differences in supply restrictions with two indices, which vary only at the cross-sectional level. First, we measure topographical supply restrictions with the UNAVAL index by Saiz (2010). UNAVAL measures MSA-level geographical land availability constraints. Saiz (2010) uses GIS and satellite information over 1970-2000 to calculate the share of land in a 50 kilometer radius of the MSA main city center that is covered by water, or where the land has a slope exceeding 15 degrees. These areas are seen as severely constrained for residential construction. Saiz (2010) finds that metropolitan areas that are more inelastic are typically more land constrained. Second, we measure regulatory constraints with the Wharton Regulatory Land Use Index (WRLURI) from Gyourko et al. (2008). WRLURI measures the stringency of local zoning laws, i.e. the time and financial cost of acquiring building permits and constructing a new home. It is based on a nationwide survey in 2005, and on a separate study of state executive, legislative and judicial activity.[3]

4.2.2 Housing market cycles

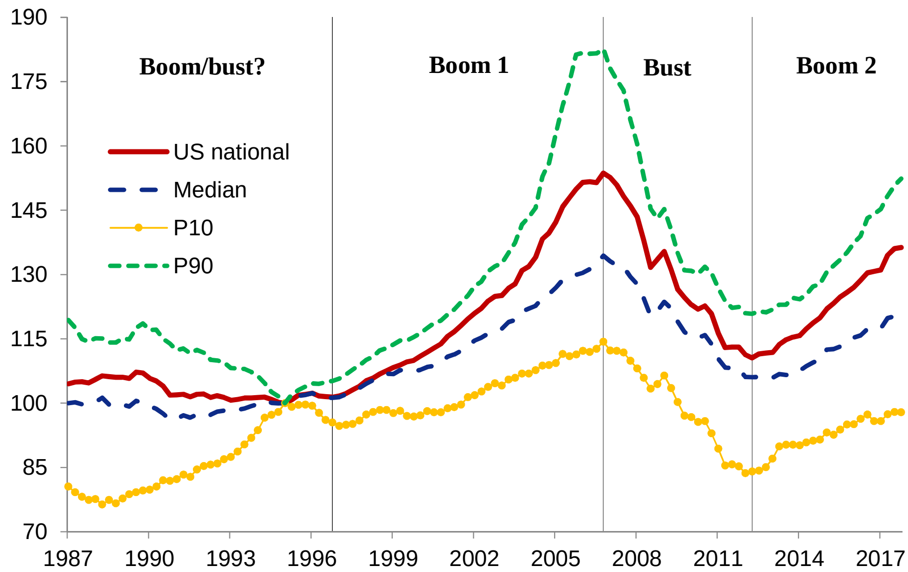
To date booms and busts over the housing cycle, we analyze peaks and troughs in real house prices at the median.[4] For ease of illustration, we plot the national house price index,

[3]This index is based on 11 sub-indices measuring different types of complications and regulations in the process of getting a building permit. WRLURI is available at a town (or city) level, which we have aggregated to the MSA level using the sample probability weights of Gyourko et al. (2008).

[4]Given that our sample of 254 MSAs includes some areas with large variations in prices, we look at the median, instead of the mean as in Glaeser et al. (2008). The median minimizes the effect that outliers have on dating the housing cycles. We track the evolution in the median real house price index over time, which does not mean necessarily that we track the same MSA over time. Alternative approaches to ours of defining a common housing cycle range from the identification of local house price booms and busts (Ferreira and Gyourko, 2011) to clustering MSAs with similar cyclical patterns (Hernández-Murillo et al., 2017).

together with the median, the 10^{th} and 90^{th} percentiles of the house price distribution at the MSA level (Figure 4.2.1). We detect three phases of the housing cycle: a strong boom from 1996 until 2006, followed by a severe bust lasting until 2012.[5] From 2012, a new boom (the ongoing recovery) has started. With our data set, we cannot identify neither a boom nor a bust over 1986-1996. Instead, we observe significant heterogeneity across MSAs over this period; the MSAs at the bottom of the house price distribution recorded a steady increase in house prices, while the MSAs at the top saw the opposite dynamics. At the median real house prices remained relatively stable over that ten-year period.[6]

FIGURE 4.2.1: Real house price cycles



Sources: Bureau of Labor Statistics, Federal Housing Finance Agency, Moody's Analytics, and authors' calculations.
Notes: Real house prices refer to the FHFA house price index, a weighted, repeat-sales index, deflated by CPI. The index assumes the value of 100 in 1995q1. The solid red line represents the US aggregate index, the long-dashed blue line the median for the MSA distribution, the yellow line with markers the 10^{th} percentile, and the dashed green line the 90^{th} percentile. The vertical lines divide the sample period by phases of the housing cycle.

All of the MSAs experienced increasing house prices during the 1996-2006 boom, but dispersion was high; house prices increased by 17 percent, on average, for the MSAs belonging to the first decile, while they increased by 93 percent for the top decile (Table 4.2.1). During

[5]We have also used the Harding and Pagan (2002) algorithm based on local minima and maxima to check the proportion of MSAs that share the same peak and trough as defined by the median. Results are broadly consistent with our approach.
[6]In a sample of 79 MSAs, Glaeser et al. (2008) identify a national boom over 1982-1989, a subsequent bust until 1996, and a strong boom between 1996 and 2006. We get a different picture for 1986-1996, since we cover a substantially larger sample of MSAs.

the 2006-2012 bust, house prices fell in all, but one, MSA. By the end of 2017, house prices have increased in more than 90 percent of the MSAs since the trough of 2012.

Table 4.2.1: Local house price cycles

	US	Median	p10	p25	p75	p90	N	>0
1996-2006	51.5	32.7	16.6	22.0	64.4	93.1	254	254
2006-2012	-28.0	-21.2	-46.0	-31.7	-14.3	-10.0	254	1
2012-2017	23.3	13.3	1.3	6.2	27.4	52.2	254	237

Notes: Cumulative changes in real house prices for different phases of the housing cycle. The first column refers to the national index, and the following columns show points in the distribution for the MSA sample. *N* is the number of MSAs, while *>0* counts the MSAs that recorded cumulative house price increases over each cycle.

4.3 Estimating housing supply elasticities in booms

4.3.1 Main specification

To estimate local housing supply elasticities across the two housing booms, we use a single-equation approach in the spirit of Green et al. (2005). The authors estimate time-invariant housing supply elasticities for a sample of 45 MSA over the 1979-1996 period, by regressing a proxy for the annual growth in the housing stock on lagged house price growth. We use building permits as our housing supply variable to capture the immediate reaction of builders to a change in house prices.[7] Given that building permits do not exhibit stochastic non-stationarities, we adopt a level specification. We follow Glaeser et al. (2008) and assume that permits depend on the price-to-cost ratio (Tobin's Q). Due to data availability, we use wages and salaries in the construction sector as a proxy for total construction costs. We account for geographical (Saiz, 2010) and regulatory constraints (Gyourko et al., 2008) in the response of housing supply to a change in house prices. We estimate the following specification separately for the two boom periods:

$$\begin{aligned}\log(H_{i,t}) =& \beta^j \log(HPI_{i,t}) + \lambda^j \left[\log(HPI_{i,t}) \times UNAVAL_i\right] + \delta^j \left[\log(HPI_{i,t}) \times WRLURI_i\right] \\ &+ \gamma^j X'_{i,t} + \eta_i^j + \zeta_t^j + \epsilon_{i,t}^j\end{aligned} \tag{4.1}$$

[7] The process of building a housing unit first requires builders to apply for a permit to get their construction project approved, which can take a few months. After the approval is granted, the construction works start (housing starts). The process ends when the housing unit is occupied or available for occupancy (housing stock).

where $log(H_{i,t})$ denotes the log of building permits, $\log(HPI_{i,t})$ is the log of the FHFA house price index deflated by CPI, $UNAVAL_i$ is the land unavailability index of Saiz (2010), $WRLURI_i$ is the Wharton Land Use Regulatory Index (Gyourko et al., 2008), and $X'_{i,t}$ is a vector of local economic and socio-demographic variables, which includes the lagged dependent variable, the log of real construction wages, and its interaction with the two supply restriction indices, log of population, the unemployment rate, the inflation rate, the dependency ratio, and the fraction of Blacks and Hispanics in total population. We add η_i^j to account for MSA-fixed effects, and ζ_t^j to capture time-fixed effects. The superscript j indicates that the estimated parameters may differ across the two booms, $j = \{1996 - 2006, 2012 - 2017\}$.

We expect β^j to be positive, as builders apply for more building permits when house prices increase. In addition, the interaction terms in Eq. (4.1) imply that housing supply elasticities may differ across MSAs if there are differences in land availability or regulation. We expect the coefficients λ^j and δ^j to be negative, as tighter geographical or regulatory restrictions should lead to a smaller expansion in building permits. It follows that the implied supply elasticity for a given MSA in housing boom j is found by differentiating Eq. (4.1) with respect to house prices:

$$Elasticity_i^j = \beta^j + \lambda^j \times UNAVAL_i + \delta^j \times WRLURI_i \tag{4.2}$$

4.3.2 IV identification

To deal with reverse causality between house prices and permits, we use an IV approach. An instrument, Z, for house prices in the housing supply equation needs to shift housing demand (and thereby house prices), while at the same time be orthogonal to omitted supply factors. More formally, the traditional IV conditions for all i and t need to be satisfied:

$$Cov(Z_{i,t}, HPI_{i,t}) \neq 0 \tag{4.3}$$

$$Cov(Z_{i,t}, \epsilon_{i,t}) = 0 \tag{4.4}$$

where Eq. (4.3) is the *relevance* condition, stating that the external instrument Z must be contemporaneously correlated with local house prices. The *exogeneity* condition in Eq. (4.4) requires the instrument not to be contemporaneously correlated with the omitted supply factors in Eq. (4.1).

We use two instruments for house prices that we argue lead to shifts in housing demand (relevance), but that does not shift housing supply (exogeneity).[8] The first instrument exploits variation in crime rates across MSAs and over time. We use data on crime rates (per 100,000 inhabitants) from the Uniform Crime Report Offenses Known to Law Enforcement data set, which is compiled by the FBI. These data provide counts of crimes reported to the police for each police agency (cities, towns, and villages), and broken down by two major types: violent crime (murder, forcible rape, robbery, and aggravated assault), and property crime (burglary, larceny theft, and motor vehicle theft). Given the significant negative impact that crime can have on society, either directly through destruction of life and of property, or indirectly through the creation of a sense of insecurity, fear and anxiety as a consequence of criminal acts, crime can be viewed as a negative amenity (Pope and Pope, 2012). Accordingly, crime rates should capture exogenous variation in (negative) amenities that drive house price changes both within and across MSAs.

The relevance condition is supported by findings in the literature that point to high crime rates being strongly and negatively associated with property prices. The seminal paper by Thaler (1978) finds that an increase in property crime per capita reduces house prices in Rochester, New York. More recent papers have found a detrimental effect of crime on property prices, such as Gibbons (2004) for London. In turn, Schwartz et al. (2003) estimates that falling crime rates were responsible for one-third of the increase in property values in New York over 1994-98. Along the same lines, but using zip code-level data, Pope and Pope (2012) estimates the elasticity of property values to the decline in crime rates over 1990-2000 to have been important. We use property crime, which accounts for almost 90 percent of total crime, as our main measure of crime since it is available for a larger sample of MSAs compared with violent crime.[9]

The second instrument we use is the log of real personal disposable income. Income is

[8] We cannot use supply shifters as instruments as they would not satisfy the orthogonality condition. In particular, we cannot resort to one of the most commonly used instruments for house prices, namely the housing supply elasticity calculated by Saiz (2010), see e.g., Mian et al. (2013), and Stroebel and Vavra (2019) – although not free of criticism (Davidoff, 2016). The reason is that it enters the supply equation that we are interested in estimating, and because the housing supply elasticity is our main parameter of interest.

[9] In Section 4.6 we show that none of our results are materially affected by instead using total crime as the instrument.

one of the main determinants of housing and consumption demand in standard macro and housing models, but typically does not affect housing supply directly (Dougherty and Van Order, 1982; Buckley and Ermisch, 1983; Meen, 1990; Muellbauer and Murphy, 1997; Meen, 2001; Meen, 2002; Duca et al., 2011). This instrument should thus satisfy both the relevance and exogeneity conditions.

The validity of the instruments hinges on property crime rates and income affecting housing supply *only* through its impact on house prices, i.e., leading to movements along, but not shifts in, the supply curve. One potential concern is that housing supply conditions may be endogenous to property crime, invalidating the use of our instrument. One could argue that less affordable housing may lead to more property crime, implying a negative association between crime and house prices. One the other hand, one could also argue that high-income neighbourhoods are more prone to property crime, implying a positive association between crime and house prices. While these are admittedly possible concerns when using data at the granular level, they are less likely to be present when using MSA data as neighbourhood (zip code) level effects are washed out in the aggregation.[10]

Although it is impossible to formally test the exclusion restriction, we provide some evidence that it is valid in our context. First, we minimize this bias by adding several local supply controls to the regression. Second, we examine the exclusion restriction along the lines of Mian and Sufi (2011). They use Saiz (2010)'s housing supply elasticities to instrument for house prices, and validate their exclusions restriction by showing that wage growth did not accelerate differentially in elastic and inelastic areas over the 2002-2006 period. Table 4.3.1 shows that crime rates and income are not associated with statistically different wage growth developments in the construction sector in any of the two booms. The same holds true when the dependent variable is the level of construction wages, so as to allow for the possibility that crime rates can also have a permanent level effect on wages.

We have *three* endogenous regressors, as house prices interacted with the supply restrictions $UNAVAL$ and with $WRLURI$ are also endogenous. We therefore have *six* instruments.

[10]Note also that the MSA fixed-effects in our panel model should capture the potential time-invariant endogeneity between supply conditions and MSA-idiosyncratic characteristics.

Table 4.3.1: Validity of the exclusion restriction

Dep. var:	1996-2006		2012-17	
Wage growth	(1)	(2)	(3)	(4)
$\log(Crime)$	-0.079	0.043	-0.045	-0.014
	(0.068)	(0.081)	(0.124)	(0.150)
$\log(Inc)$	-0.014	0.310	-0.022*	-0.412
	(0.013)	(0.285)	(0.011)	(0.247)
Controls	No	Yes	No	Yes
Number of MSA	242	241	254	254
Observations	7,584	7,548	4,866	4,866
Adj. R2	0.439	0.446	0.263	0.263

Notes: OLS estimates with state-fixed effects and time effects, where the dependent variable is the change in the log of construction wages. The constant and control variables are not reported. Robust heteroskedastic standard errors shown in parentheses. Asterisks, *, **, and ***, denote statistical significance at the 10%, 5%, and 1% levels.

For each boom, we estimate the following first- and second-stage regressions:

$$\begin{aligned} W_{i,t} =&\rho_1^j \log(Crime_{i,t}) + \rho_2^j [\log(Crime_{i,t}) \times UNAVAL_i] + \rho_3^j [\log(Crime_{i,t}) \times WRLURI_i] \\ &+ \omega_1^j \log(Inc_{i,t}) + \omega_2^j [\log(Inc_{i,t}) \times UNAVAL_i] + \omega_3^j [\log(Inc_{i,t}) \times WRLURI_i] \\ &+ \phi^j X'_{i,t} + \psi_i^j + \nu_t^j + \mu_{i,t}^j \end{aligned} \tag{4.5}$$

$$\begin{aligned} \log(H_{i,t}) =&\beta^{IV,j} \log(\widehat{HPI_{i,t}}) + \lambda^{IV,j} \widehat{[\log(HPI_{i,t}) \times UNAVAL_i]} + \delta^{IV,j} \widehat{[\log(HPI_{i,t}) \times WRLURI_i]} \\ &+ \gamma^j X'_{i,t} + \eta_i^j + \zeta_t^j + \epsilon_{i,t}^j \end{aligned} \tag{4.6}$$

where j signifies again that all parameters may differ between the two booms. The dependent variable, $W_{i,t} = \{HPI_{i,t}, HPI_{i,t} \times UNAVAL, HPI_{i,t} \times WRLURI\}$ in Eq. (4.5) refers to house prices, and house prices interacted with supply restrictions. To control for possible confounders, we add a set of control variables, listed in Section 4.3.1.

We assess the relevance and strength of the instruments with the weak identification

Cragg-Donald F-statistic test, including a version of the test that is robust to heteroskedasticity (Kleibergen-Paap F-test.) We take Stock and Yogo (2005)'s critical value of 12.2 for the 5 percent relative bias to test for weak instruments. We also compute the Hansen J-statistic test to test for over-identification, given that we have more instruments than endogenous variables.

Results are reported in Table 4.3.2 for both the 1996-2006 boom and the 2012-2017 boom. The first-stage F-test and robust F-test stand between 30 and 50, which is significantly above Stock and Yogo (2005)'s threshold value, suggesting that our instruments are valid and strong.[11] In addition, the Hansen J-test provides strong evidence against rejecting the null hypothesis that the instruments are valid in the first boom. We reach a similar conclusion for the second boom, although the evidence is somewhat weaker.

TABLE 4.3.2: Regression estimates by housing boom

	1996-2006	**2012-2017**
$\log(HPI)$	2.774***	1.794**
	(0.428)	(0.847)
$\log(HPI) \times UNAVAL$	-1.344***	-1.225
	(0.340)	(1.185)
$\log(HPI) \times WRLURI$	-0.718***	-1.086**
	(0.096)	(0.422)
$\log(H_{t-1})$	0.415***	0.203***
	(0.019)	(0.023)
Number of MSA	241	254
Observations	7,548	4,866
Cragg-Donald F-test	39.83	49.66
Kleibergen-Paap (robust) F-test	31.00	29.61
Hansen J-test (p-value)	0.64	0.06

Notes: IV estimates of Eq. (4.6,) where the dependent variable is the log of building permits. The Cragg-Donald F-test and Kleibergen-Paap F-test assume that under the null the excluded instruments are not weakly correlated with the endogenous regressors. The Hansen J-test of overidentifying restrictions reports the p-value under the null hypothesis that the instruments are uncorrelated with the error term, and that the excluded instruments are correctly excluded from the estimated equation. The constant and additional control variables are not reported. Robust heteroskedastic standard errors shown in parentheses. Asterisks, *, **, and ***, denote statistical significance at the 10%, 5%, and 1% levels.

[11] The first-stage coefficients on the instruments are statistically significant for both housing booms: for property crime rates we get coefficients within a range of -0.02 to -0.025 (t-stats above 2), and of around 0.3-0.4 (t-stats above 8) for income.

The coefficient on house prices is statistically significant at conventional levels, and positive, for both housing booms. But there is a considerable decline in the magnitude of the coefficient from the first to the second boom. This implies a weakened response of permits to a given change in house prices. Our estimates indicate that building permits increased by 2.8 percent over the short term (long-term response of 4.7 percent) for every 1 percent increase in house prices during the 1996-2006 boom, which is almost twice as large as during the current housing recovery – a response of roughly 1.8 percent over the short term (long-term response of 2.2 percent).[12]

The interaction of house prices with the supply restriction variables yields the expected negative signs, i.e., the tighter the geographical and regulatory restrictions, the smaller is the expansion in building permits for a given house price increase. The coefficient on the interaction term for *UNAVAL* is, however, not significant in the current boom.

4.3.3 Estimated elasticities

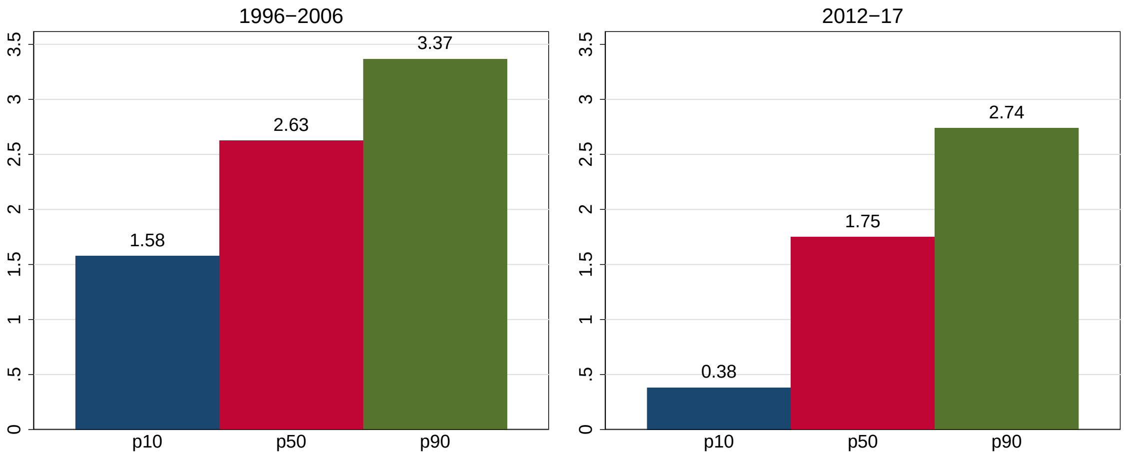
We calculate MSA-specific elasticities for the two booms by inserting the relevant parameters of Eq. 4.6 into the expression of Eq. 4.2. Figure 4.3.1 shows the elasticities at the median, 10^{th} and 90^{th} percentiles for each housing boom. Our results suggest that supply elasticities have fallen across the whole distribution. In addition, the dispersion in supply elasticities has increased during the current cycle, with a particularly strong decline in the lowest part of the distribution.

We shed more light on the heterogeneity between MSAs by looking at the distribution of the elasticities across the two housing booms (Figure 4.3.2). More specifically, we create five groups, where red (blue) colours refer to low (high) elasticity areas. Areas located in states such as California, Arizona, Florida, Oregon, and New York have the lowest elasticities in both booms. This is not surprising, given that geographical idiosyncrasies, such as steep ground and bodies of water, make it harder to build and limit the land available for construction in these areas, compared to the rest of the country (Saiz, 2010). In addition, land-use regulation, which limits the expansion of supply, also tends to be more stringent in these areas (Gyourko et al., 2008). By contrast, we estimate high-elasticity areas to be

[12]The long-term coefficient is the result of dividing its short-run coefficient by 1 minus the lagged coefficient on the dependent variable; for instance, for the 1996-2006 cycle: 4.7=2.774/(1-0.415).

located in the Midwest, where builders face relatively fewer restrictions to expand housing supply.

FIGURE 4.3.1: Estimated elasticities: IV specification



Notes: Estimated elasticities from Eq. 4.6 for the median, 10^{th} and 90^{th} percentiles for each housing boom.

Figure 4.3.2 shows that the rank ordering of the MSAs between the two booms is relatively stable, and Figure 4.3.3 reveals that the largest decline in elasticities between the two booms have taken place in the areas that had the lowest elasticities during the first housing boom.

FIGURE 4.3.2: Estimated elasticities for the two housing booms

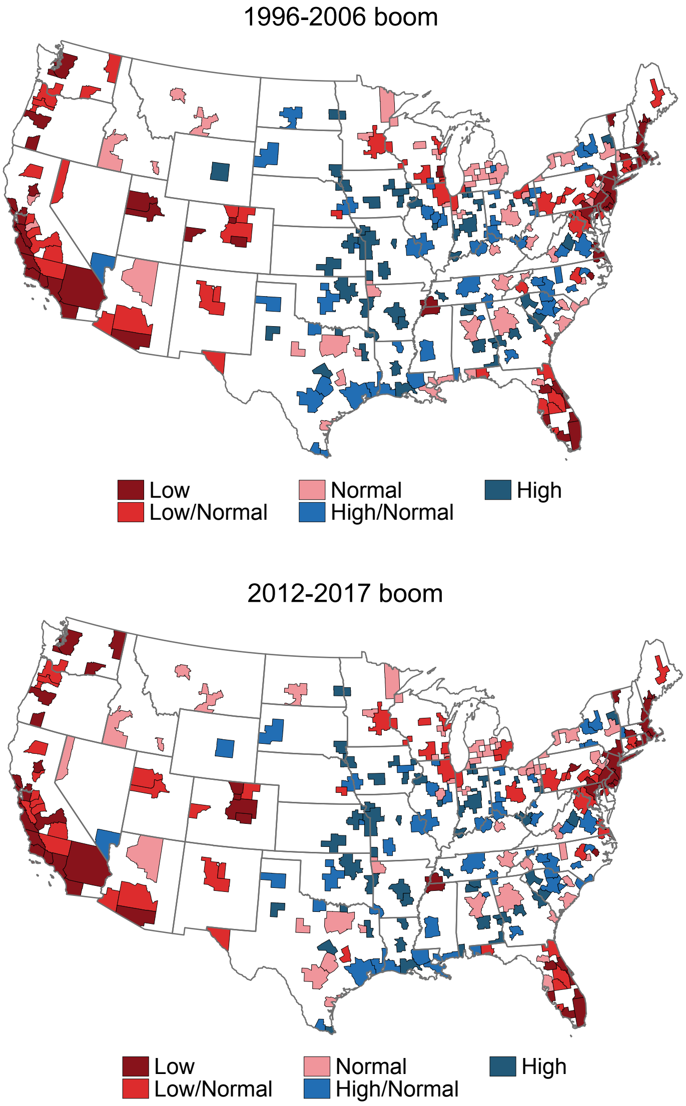
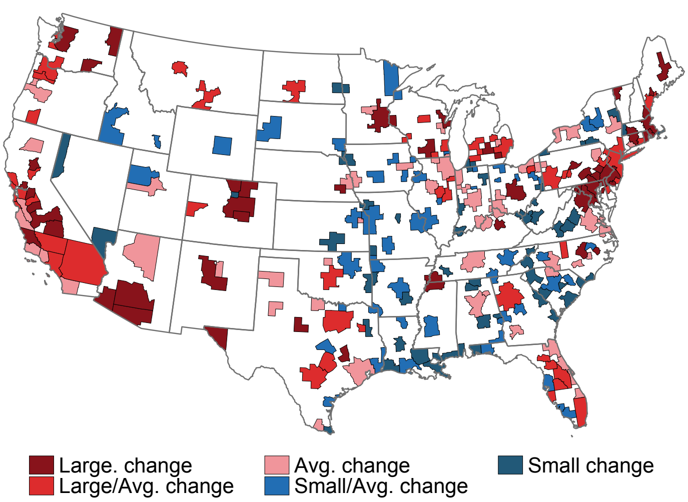


FIGURE 4.3.3: Change in estimated elasticities between booms



4.4 Supply elasticities and demand shocks across booms

Our results point to a nationwide decline in housing supply elasticities. An implication of this is that aggregate demand shocks should have a greater impact on house prices today, whereas quantity should respond less (see Appendix A for the illustration of this point in a simple supply-demand framework). We explore the relevance of this conjecture through the use of exogenous monetary policy shocks.

4.4.1 High-frequency identification of monetary policy shocks

Our measure of monetary policy shocks is computed following a recent strand of the literature that resorts to high-frequency data to identify unexpected changes in the Fed policy rate (see, for instance, Gürkaynak et al., 2005; Gertler and Karadi, 2015; Nakamura and Steinsson, 2018).[13] This high-frequency identified (HFI) approach isolates news about future policy actions that is orthogonal to changes in economic and financial variables. We take the unexpected changes in interest rates for 3-month ahead contracts on Fed funds futures in a 30-minute window surrounding FOMC meetings. In total, we cover 127 meetings over the two housing booms: 83 between 1997q1-2006q4 and 44 between 2012q3-2017q4. The underlying assumption is that changes in the futures rates within that window can only arise from news about monetary policy, given that market participants incorporate all publicly available information at the beginning of that narrow window.

More specifically, let f_{t+j} be the price of a Fed funds future in month t that expires in j months, and S_{t+j} the unanticipated change in the expectation for the Fed funds rate $t+j$ months ahead. The monetary surprise is then constructed as the difference between the price of the $t+j$ month ahead Fed funds future contract 20 minutes after the FOMC announcement and the price of the same contract 10 minutes before the announcement:

$$S_{t+j} = f_{t+j} - f_{t+j,-1}$$

[13] We do not use the standard Romer and Romer (2004)'s narrative shocks given that the Greenbook projections are not available for the period covering the current recovery; they are released to the public with a lag of five years.

We follow standard practice in transforming high frequency data into the quarterly frequency (see, for instance, Ottonello and Winberry, 2018; Wong, 2019). In particular, we first create a daily shock series by cumulating the daily surprises over the past 90 days. We then take quarterly averages of the cumulative daily shocks. Our quarterly shocks are characterised by roughly a 60-40 distribution between expansionary and contractionary shocks over the full sample (Figure D.5 in Appendix D).[14]

HFI shocks may contain measurement errors, thus may capture only part of the 'true' structural shock. For instance, some price changes within the 30-minute window around the policy announcements may reflect trading noise and volatility. In addition, the monthly (and quarterly) series of surprises contains some random zero observations, as a result of calendar months without FOMC meetings. Finally, the monthly (and quarterly) surprise series does not incorporate other monetary policy news released outside of the announcement window, such as speeches by FOMC members. To deal with this, we follow Gertler and Karadi (2015), Ramey (2016), Nakamura and Steinsson (2018), and Stock and Watson (2018) and treat the surprises as instruments for the underlying shock. Following Gertler and Karadi (2015), we choose the one-year Treasury bill yield as the relevant monetary policy indicator. This risk-free asset with a longer maturity than the funds rate has the advantage of also incorporating shocks to forward guidance about the future path of interest rates, instead of just about the current rate.

4.4.2 Empirical results: LP-IV

To study how monetary policy shocks affect house prices and quantity across MSAs over the two booms, we follow Jordà et al. (2015), Ramey (2016), and Stock and Watson (2018) and use an instrumental variable local projection approach. The Jordà (2005) method offers some advantages over Vector Auto Regressive (VAR) models, since impulse responses are less vulnerable to mis-specification (Stock and Watson, 2018). In addition, it easily accommodates non-linearities, allowing us to estimate the dynamic causal effects of monetary policy shocks conditional on our housing supply elasticities.

[14]The time-aggregation bias should not affect the results, as our quarterly shocks exhibit similar moments to the raw high-frequency data (Table D.1 in Appendix D).

We estimate the LP-IV model over one unique sample, the two booms 1997q1-2006q4 and 2012q3-2017q4, by running a series of regressions for each horizon h=1,2...,16 quarters:

$$\Delta_h Y_{i,t+h} = \beta^{Y,h}\Delta MP_t + \gamma^{Y,h}\Delta MP_t \times \widehat{Elast_i^j} + \sum_{j=1}^{4} \lambda_j^{Y,h}\Delta X_{i,t-j} + \eta_i^{Y,h} + \epsilon_{i,t+h}^{Y} \tag{4.7}$$

where the dependent variables, Y, are the cumulative percentage change in real house prices, HPI, or in building permits, H, from period t to t+h.[15] MP_t is the monetary policy indicator (the one-year Treasury bill yield), which is interacted with our estimated supply elasticities $El\hat{a}st_{i,t}$ for each boom, and $X_{i,t-j}$ refers to a vector of lagged control variables (four lags), namely the lagged dependent variables, the external instrument, real disposable income growth, population growth, real construction wage growth, the change in the unemployment rate, and the Gilchrist and Zakrajšek (2012)'s excess bond premium (EBP).[16] This large set of control variables helps minimize the omitted variable bias and reduce the variance of the error term (Stock and Watson, 2018). In addition, Stock and Watson (2018) argue that the nature of the construction of the HFI monetary shocks induces a first-order moving average structure, leading to a correlation between the external instrument and past values of the policy indicator. We follow their suggestion and include lagged values of the external instrument as controls to make our IV valid.

We add MSA-fixed effects $\eta_i^{Y,h}$ to control for time-invariant idiosyncratic MSA characteristics, but we do not include time-fixed effects given that the monetary policy indicator is common across MSAs. The standard errors are MSA-specific cluster-robust, which allow for fully flexible time dependence in the errors within MSAs.[17]

Our parameters of interest are $\beta^{Y,h}$ and $\gamma^{Y,h}$. Following the conjectures from the theoretical model in Appendix A, we expect an expansionary monetary policy shock to boost

[15] Given the high volatility of permits, especially as h increases, we transform the raw series into a four quarter centered moving average.

[16] The EBP is a measure of investor sentiment or risk appetite in the corporate bond market that is not directly attributable to expected default risk. More specifically, Gilchrist and Zakrajšek (2012) define it as the spread between the rate of return on corporate securities and a similar maturity government bond rate that is left after removing the default risk component. We add the EBP as Gertler and Karadi (2015) argue that it has strong forecasting ability for economic activity, thus acting as a summary indicator of the potentially relevant information left out of the model to explain the dependent variable.

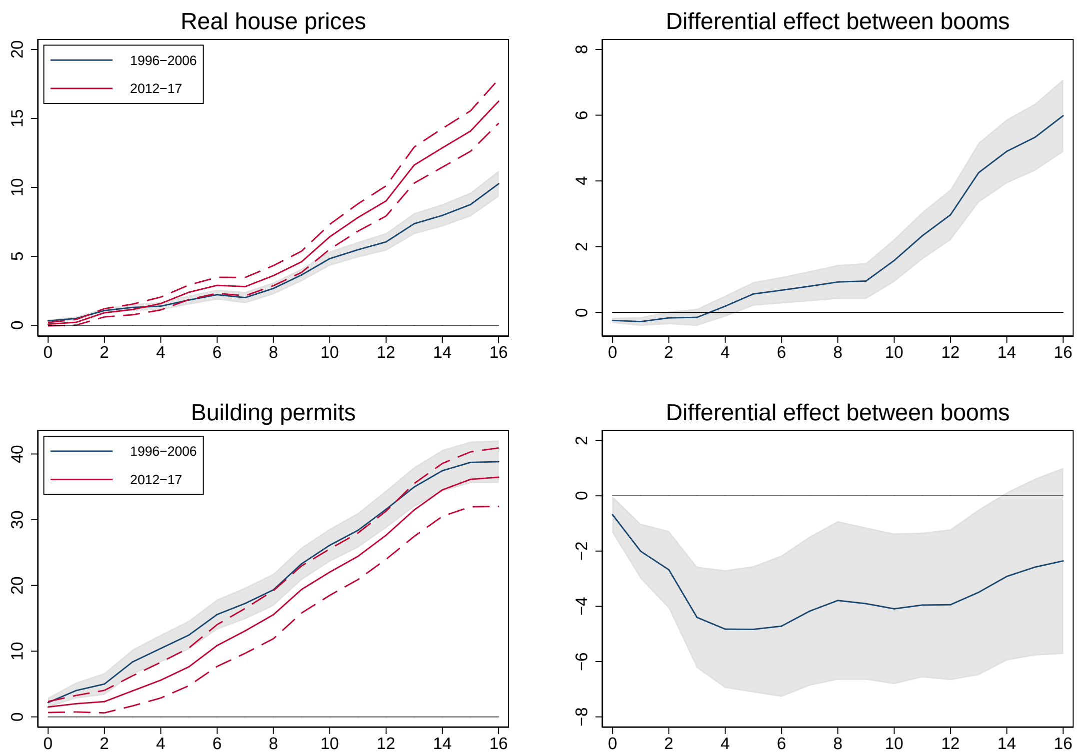
[17] This adjustment tends to produce more conservative standard errors than a standard heteroskedasticity-and-autocorrelation (HAC) estimator (Jordà et al., 2015). Note that the standard errors are not distorted by the generated regressor issues, given that the high-frequency shock is used only as an instrument and not directly included in the model.

house prices ($-\beta^{HPI,h}>$0), but that this effect becomes smaller the higher the housing supply elasticity ($-\gamma^{HPI,h}<$0). Further, we expect an expansionary shock to stimulate more construction activity ($-\beta^{H,h}>$0), and that this effect is reinforced by a higher elasticity ($-\gamma>$0).

We have two endogenous variables and two instruments in Eq. 4.7: the monetary policy indicator and its interaction with the estimated elasticities, instrumented with the HFI surprise series and with its interaction with the elasticities. The first-stage F-test and robust F-test are above the Stock and Yogo (2005)'s threshold, suggesting that our instruments are valid and strong.

We find that an expansionary monetary policy shock that lowers the one-year Treasury bill yield by 100 basis points raises both house prices and quantity over the short to medium run in a statistically significant way for both housing booms (Figure 4.4.1).

FIGURE 4.4.1: Responses to an expansionary monetary policy shock across booms

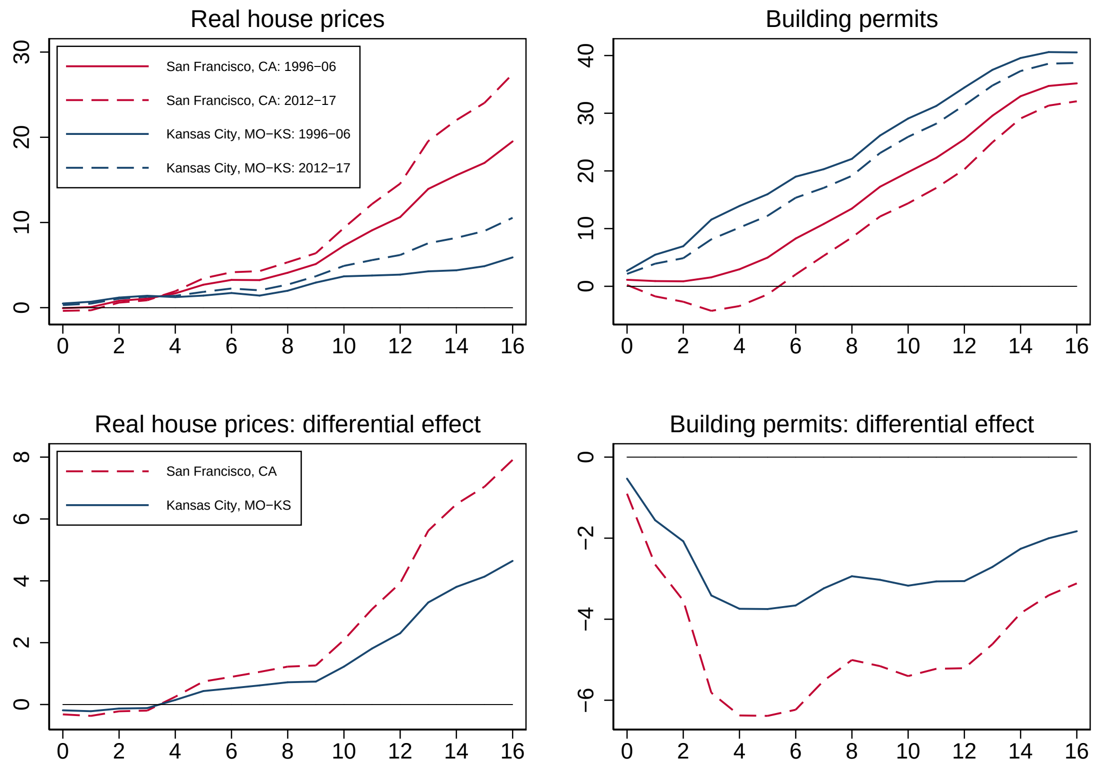


Notes: Cumulative impulse responses to a 100 basis point decline in the one-year Treasury bill yield, assessed at the sample median elasticity for each housing boom period. The right-hand charts depict the difference in the estimated response of house prices and building permits between the 2012-17 and the 1996-2006 booms. The grey area and the dashed red lines refer to 90 percent confidence bands.

Furthermore, we find that house prices rise by considerably more in the 2012-2017 boom compared with the 1996-2006 boom. While price dynamics are similar in the short term, house prices in the current boom start to increase at a statistically significant faster pace after two years. For the same 100 basis points decline in government bond yields, real house prices in the current boom are six percentage points higher after four years (a cumulative 16 percent increase in the 2012-17 boom against ten percent in the previous boom).

We estimate the opposite dynamics for building permits, which reacted more strongly to a monetary policy shock in the 1996-2006 boom. But the difference between the responses is relatively small, given the scale of the increase in permits in both episodes (almost 40 percent after four years). Overall, the differences in the impulse responses are not driven by different magnitudes of the underlying shocks, as illustrated by a similar decline in the response of the policy indicator (Figure D.6 in Appendix D).[18]

FIGURE 4.4.2: Responses to an expansionary monetary policy shock for selected MSAs



Notes: Cumulative impulse responses to a 100 basis point decline in the one-year Treasury bill yield, assessed at the sample median elasticity for selected MSAs and for each housing boom. Kansas City, Missouri, represents a high-supply elasticity MSA, while San Francisco-Oakland-Hayward, California, a low-supply elasticity MSA.

[18] We also check that the statistical difference in the impulse responses between the two booms are robust to adjusting the standard errors for cross-sectional dependence using the Driscoll-Kraay estimator (Figure D.7 in Appendix D). Our results are broadly robust to this.

We show that there is considerable heterogeneity in responses across MSAs within the same period. Figure 4.4.2 shows that house prices in a typical low-elasticity MSA, such as San Francisco-Oakland-Hayward, California respond more strongly to the monetary policy shock than a typical high-elasticity MSA, such as Kansas City, Missouri. While this is in line with Aladangady (2014) and Aastveit and Anundsen (2017), our results also suggest that the differential effect between the two booms may be larger in low-elasticity areas than in high-elasticity areas (lower panel of Figure 4.4.2). Although it is outside the scope of this paper, the time-varying effects of monetary policy also raise concerns about the distributional effects of monetary policy on consumption inequality between MSAs (Beraja et al., 2019).

4.5 Why have elasticities declined?

In theory, several factors might lead to changes in the slope of the housing supply curve, including changes in regulatory conditions, demographics, and in expectations about future demand and house prices.

A recent paper by Herkenhoff et al. (2018) documents a substantial tightening in land-use policy in most US states since 1950. They find that a substantial tightening across states took place between 1990 and 2014, of around 18 percent. The tightening in regulation is particularly marked for high-house price states. Along the same lines, recent research has put forward the notion that the decline in construction productivity may be the result of increased costs stemming from tighter regulation over time (Davis and Palumbo, 2008; Albouy and Ehrlich, 2018) and Glaeser and Gyourko (2018).

A simple correlation analysis between our estimated elasticities and Herkenhoff et al. (2018)'s land-use regulation index suggests that the tightening in regulation between 2000 and 2014 is associated with a decline in our estimated elasticities between the two housing boom episodes (correlation of -0.4).[19] We show that this relationship holds in a multi-variate setting, by estimating the following cross-sectional equation:

$$\Delta \log(\widehat{Elast_i}^{17} - \widehat{Elast_i}^{06}) = \alpha + \beta_1 \Delta \log(X_i^{17} - X_i^{06}) + \beta_2 Z_i + \epsilon_i \tag{4.8}$$

[19] Herkenhoff et al. (2018)'s land-use regulation indicator is available for 48 states, excluding Alaska and Hawaii, and for individual years: 1950, 1960, 1970, 1980, 1990, 2000, and 2014. We take the 2000 and 2014 values of that indicator as the data points relevant for respectively the 1996-2006 and 2012-2017 booms.

where the dependent variable is the log percentage change in estimated elasticities between 2012-2017 ($\widehat{Elast_i}^{17}$) and 1996-2006 ($\widehat{Elast_i}^{06}$). We regress it on the log percentage change for the same period of a set of indicators X_i, namely the state-level Herkenhoff et al. (2018)'s land-use regulation, population density, construction wages, unemployment rate, and on initial conditions Z_i, the levels of house prices to income per capita and of population density. We also include the cumulative change in house price growth during the 2006-2012 bust.

Our results provide statistical evidence that tighter land-use regulation has been associated with a decline in elasticities between the two booms (Table 4.5.1).[20] Our estimates also show that areas with stronger economic performance, as measured by the change in the unemployment rate, and higher initial levels of house prices relative to income and of population density at the beginning of the 2012-2017 boom, tend to be associated with larger declines in elasticities. In contrast, the negative association between faster population density growth and larger declines in elasticities is not statistically significant.

TABLE 4.5.1: ΔElasticity between booms

	(1)	(2)	(3)
$\Delta \log(\text{Land reg.})$	0.273***	0.249***	0.162***
	(0.043)	(0.038)	(0.043)
$\Delta \text{HPI}^{06-12}$		0.886***	0.935***
		(0.095)	(0.150)
$\Delta \log(\text{Pop density})$			-0.008
			(0.138)
$\Delta \log(Wage)$			-0.010
			(0.084)
ΔUR			3.515**
			(1.558)
Hpinc_pc			-0.568***
			(0.139)
Pop density			-0.011**
			(0.004)
Observations	251	251	251
R-squared	0.121	0.379	0.465

Notes: Regression estimates of Eq. 4.8, where the dependent variable is the percentage change in the estimated supply elasticities between 2012-2017 and 1996-2006. Robust heteroskedastic standard errors in parentheses. Asterisks, *, **, and ***, denote statistical significance at the 10%, 5%, and 1% levels.

Finally, we find that the areas that experienced the strongest bust in house prices over the

[20] A decline in the land-use regulation index represents a tightening in regulation.

period 2006-2012 (ΔHPI^{06-12}) also recorded the largest declines in elasticities between the two booms. Our interpretation is that the Great Recession might have cast a long shadow on builders' expectations, making them less price responsive than before. This fear of a new bust may have paved the way for a new housing boom where house prices are more responsive to fluctuations in demand.

4.6 Robustness checks and alternative explanations for declining supply elasticities

4.6.1 Bartik-type instrumental variable approach

We check the robustness of our baseline estimates of housing supply elasticities to employing a Bartik-type instrumental variable approach (Bartik, 1991). More specifically, we follow a similar approach as Guren et al. (2018), and instrument MSA-level house prices with house prices at the Census Division level.[21,22] A detailed description of the approach is provided in Appendix C. Our results are broadly robust to this approach. The estimated elasticities are in line with our baseline results, with a larger decline in the elasticities in the current boom, see Figure D.8 in Appendix D.

4.6.2 Alternative specifications for estimating housing supply elasticities

We carry out additional robustness checks to the housing supply equation (4.6) by: (i) using total crime rates (sum of property crime and violent crime) as the crime variable instrument; (ii) using permit intensity as the dependent variable to allow the dynamics in permits to differ according to the existing stock of houses; (iii) replacing UNAVAL and WRLURI with a summary measure of supply restrictions, essentially the sum of these two variables standardized, to account for the possibility that these indicators might be correlated; and (iv) controlling for mortgage originations (the amount of new mortgage loans) to assess the impact on the

[21] The nine Census Divisions are New England, Middle Atlantic, East North Central, West North Central, South Atlantic, East South Central, West South Central, Mountain, and Pacific.

[22] This is akin to a Bartik-type instrument, as the strategy employed assumes that house prices in a given number of MSAs respond differently to an aggregate shock (regional house price changes) because of pre-existing local differences in the housing market or economic structure. In the original setting, the Bartik instrument involves instrumenting local employment growth with a variable that consists of the interaction between local industry employment shares and national industry employment growth.

housing supply response of subdued credit developments since the Great Recession.[23,24] Our results are robust to these alternative specifications (Table D.3 in Appendix D). Moreover, across all specifications, our findings that supply elasticities have declined between the two housing booms is maintained (Table D.4).

4.6.3 Rolling window estimation of housing supply elasticities

Our approach has been to estimate housing supply elasticities for the two boom periods separately. Another approach is to estimate housing supply elasticities using a rolling window estimation. To explore whether this has any bearing on our findings of a decline in housing supply elasticities, we estimate Eq. 4.6 using 10-year and 15-year rolling windows. For the 10-year window, the first regression covers the period 1997–2006, the second regression spans the period 1998–2007, and so on. Similarly, for the 15-year rolling windows, the first period goes from 1997 to 2011, and the last from 2003 to 2017. We report the rolling window estimates of the median housing supply elasticity in Figure D.9. We find that housing supply elasticities have declined over time, in line with our baseline results. The durability of housing entails that housing supply is rigid downwards (Glaeser and Gyourko, 2005), implying that housing supply elasticities should fall towards zero during severe busts. Consistent with this, our rolling window estimates show a particularly strong decline in housing supply elasticities during the recent housing bust.

[23] Additional robustness checks we have carried out include by: (i) using housing starts and the housing stock as the dependent variables; (ii) using the Arellano-Bond estimator to account for the Nickell (1981) bias in dynamic panels; (iii) adding state-by-time fixed effects to control for time-varying state-specific shocks; and (iv) by estimating the supply equation separately for multi-family building permits. Our baseline regression estimates remain qualitatively unchanged. Furthermore, in a previous version of the paper, we used the mean January temperature instead of crime rates as one of the instruments for house prices, based on the works of Glaeser and Gottlieb (2009), and Glaeser et al. (2012). January temperatures proxy housing demand as they capture the exogenous variation in amenities that lead house prices to change. We find qualitatively similar results as the baseline specification used throughout this paper. The drawback, however, is that the mean January temperature turns out to be a weaker instrument for house prices as it is only able to identify house prices in the cross-section; given the small variability over time, it is defined as monthly average temperatures in January calculated over 1941-1970. Details are available upon request.

[24] Although we would ideally like to control for changes in credit conditions of home builders, which can lead to a shift in the supply curve, data on credit to construction firms are not available at the MSA level. We use instead mortgage originations which should be correlated with the dynamics in credit to construction firms.

4.6.4 Alternative specifications for the impact of monetary policy shocks

We tested the robustness of the local projection regressions of Eq. 4.7 to (i) using surprises in the two-month ahead Fed funds futures to compute the high-frequency monetary shock; (ii) taking the two-year Treasury note rates as the policy indicator; and (iii) running the main model with only one lag. Figure D.10 in Appendix D shows that our main results remain qualitatively robust, with the model that employs the two-year Treasury note rates as the policy indicator (*GS2*) displaying the strongest responses: irrespective of the specification used, house prices rise by considerable more in the 2012-2017 boom, at the expense of a slightly weaker supply response.

4.6.5 Alternative explanations for declining supply elasticities

A first alternative explanation centers on the strong rise in construction activity during the 1996-2006 boom that led to an oversupply of houses in the subsequent period, implying that there may be less need for new homes to be built. We do, however, not find support for the oversupply hypothesis. The housing stock per capita has been trending consistently downwards during the recovery period, whereas it increased over 1996-2006 (Figure D.11 in Appendix D). At the same time, the number of homes available for sale per capita are low. In turn, housing vacancy rates have shown similar developments across the two booms, with the rental vacancy rates actually going somewhat below pre-crisis levels (Figure D.12). Moreover, although the number of new foreclosures were higher at the beginning of the current boom, they started converging steadily to the levels seen before, whilst the months' supply of houses is only slightly above the levels recorded during the previous boom (Figure D.13).[25] Based on these indicators, there seems to be little evidence of a supply overhang in the current recovery, suggesting that it cannot explain the low construction activity in the face of strong price developments.

Second, the weak response of builders during the current recovery could also be explained by difficulties in expanding capacity given shortage of workers in the construction sector. The Joint Center for Housing Studies at Harvard University reports that, a large fraction of

[25] The months' supply of houses measures the ratio of houses for sale to houses sold. It indicates how long the current for-sale inventory would last given the current sales rate if no new houses were built. It is a commonly used indicator to assess the strength of the housing market.

home builders cites the shortage of skilled workers as a significant problem (JCHS, 2018). In addition, job openings and employment growth also appear to remain subdued in this sector (Rappaport, 2018). Nevertheless, we find mixed evidence in favour of this being an explanation for the decline in elasticities. While the share of workers employed in the construction sector remains slightly lower than before the crisis, it has increased since the recovery, and approached pre-crisis levels (left panel of Figure D.14 in Appendix D). From a different perspective, employment in the construction sector is actually above pre-crisis levels; the number of construction workers necessary to build a house is larger than previously, a similar point made by Leamer (2015) – right panel of Figure D.14.

Third, land appears to have become scarcer, which limits the expansion in housing supply. There is some evidence that the number of vacant lots declined between 2008 and 2017, which is particularly more pronounced in the Western metros of San Francisco, San Diego, Seattle, Los Angeles, and Las Vegas (JCHS, 2018). Furthermore, Rappaport (2018) reasons that there is limited availability of undeveloped land in desirable locations, as the outward expansion in supply towards the periphery in many metro areas may have reached its geographical limit, in a context where households are reluctant to take on increasingly long and congested commutes. Related to this, inadequate transportation spending may affect the substitutability between homes in the outskirts and more central locations (Green et al., 2005). The combination of a growing population and inadequate infrastructure spending may have resulted in a lengthening of commute times, leading to a steepening of the land price gradient.

Fourth, following the implementation of Basel III under the Dodd-Frank Act, US regulators have applied more stringent regulatory capital requirements on loans extended to construction and land development. While the Dodd-Frank Act effectively raised capital requirements from 8 percent to about 10-11 percent for C&I loans more generally, it raised required capital to 15 percent for loans to construction and land development. The stricter capital requirements may have contributed to shortages of buildable lots across the country, and consequently to a decline in housing supply elasticities.

A final explanation for the disconnect between house price developments and construction activity in recent years is related to increased market concentration in the home building sector. Haughwout et al. (2012) document that the market share of a few large firms started to increase rapidly in the run-up to the Great Recession. More recently, Cosman and Quintero (2019) also show that there has been a decline in the competitive intensity over the last decade among developers in the United States.[26] In a more concentrated market, firms can time their housing production to maximize profits without fear of pre-emption. Cosman and Quintero (2019) find that this has led to greater price volatility, less production, and fewer vacant unsold units, as firms with market power can decide to build when demand growth is strongest and charge prices higher above their marginal cost of production. This phenomenon is consistent with our finding of a nationwide decline in housing supply elasticities.

4.7 Conclusion

We have provided evidence of a substantial and synchronized decline in local housing supply elasticities from the 1996-2006 housing boom to the ongoing recovery that started in mid-2012. An implication of this finding is that the house price responsiveness to a given demand shock should be higher today, at the expense of a smaller increase in quantity.

When we estimate the effect of an exogenous monetary policy shock on house prices in each of the two booms, we have found that monetary policy has a substantially greater impact on house prices during the current recovery than during the previous boom. In contrast, we have found that the expansion in building permits is slightly smaller today. Furthermore, our results point to significant heterogeneity in the responses across local housing markets. In particular, we estimate a substantially larger response of house prices to a monetary policy shock in supply inelastic markets than in areas with an elastic supply.

Our findings suggest that the decline in supply elasticities has been largest in areas where regulation has tightened the most. We also find that supply elasticities have declined more in areas that experienced the largest bust in house prices during the Great Recession. We interpret this as evidence that the fear of a new bust has led developers to be less price-responsive than before. This behavior may have paved the way for a new housing boom where house prices are more responsive to fluctuations in demand.

[26] Cosman and Quintero (2019) argue that increased market concentration has been the result of three main factors: (i) several construction companies filed for bankruptcy in the wake of the 2007-2009 Great Recession; (ii) a federal legislative stimulus measure in 2009 that increased the ability of home builders to use previous years' losses to reduce their tax payments, which was highly beneficial to the largest companies; and (iii) an increase in the numbers of mergers, leading to a concentration of production in a smaller number of firms.

The lowering of housing supply elasticities may explain why recent research finds that monetary policy has become more effective for financial variables; an aggregate shock that raises housing demand is absorbed mostly by price adjustments, rather than quantity adjustments. This finding can be important for financial stability considerations, whereby the actions of policy makers aimed at stimulating (housing) demand may have unintended effects by exacerbating the rise in house prices. In the current environment of tighter regulation and declining elasticities, our findings cast some doubts about the view that the recent housing market recovery looks 'healthier' and more sustainable compared to the previous boom.

Another implication of our findings relates to wealth inequality, particularly intergenerational inequality. The combination of high house prices and a tight supply of homes makes it difficult for young people and households with little liquid assets to become homeowners. This may have a direct impact on household inequality, by favouring existing homeowners, which tend to be older and wealthier, as their housing equity increases. Despite the recent findings in the literature about the economic costs of regulation, local zoning laws have actually been tightening across the country, and this has reduced supply elasticities. The biggest challenge in relaxing local housing restrictions comes from existing homeowners not wanting more affordable homes, as higher house prices mean that the value of their asset go up. In addition, existing homeowners also want to protect the amenities in their city, as new housing brings in more people, creating a congestion in access to public goods, such as crowded schools and roads (Glaeser and Gyourko, 2018).

Acknowledgements

The views expressed are those of the authors and do not necessarily reflect those of Norges Bank. We gratefully acknowledge comments and suggestions from John Duca, Gerdie Everaert, Andreas Fuster, Kevin J. Lansing, Michael T. Owyang, Gert Peersman, Esteban Prieto, Selien De Schryder, Frank Smets, Egon Zakrajsek, and Sarah Zubairy. We would also like to thank Paulina Souza Tedesco for invaluable research assistance. Finally, we would like to thank participants at the 2018 NBR Spring Institute in Venastul, the 6^{th} Workshop on Empirical Macroeconomics in Ghent, the 24^{th} CEF conference in Milan, the 52^{nd} Annual Conference of the CEA in Montreal, the 5^{th} IAAE Conference in Montreal, the 2018 CEBRA conference in Frankfurt, the 2018 EEA-ESEM in Cologne, the 1^{st} EAYE Workshop on Housing and Macroeconomics in Leipzig, the 27^{th} SNDE conference at the Federal Reserve Bank of Dallas, the RES Annual Conference at the University of Warwick, and seminars at the National Bank of Belgium, the Norges Bank, and at the Bank of England for constructive comments. This paper is part of the research activities at the Centre for Applied Macro and Petroleum economics (CAMP) at the BI Norwegian Business School, and the research activities at Housing Lab – National Center for Housing Market Research at Oslo Metropolitan University.

Appendix

A Theoretical framework

We take as a starting point a simple supply-demand model with durable housing inspired by Glaeser et al. (2008), which is made up of an economy that contains a collection of several local housing markets that exhibit heterogeneity in economic, financial, and social dimensions, including in the supply elasticities. Abstracting from depreciation of the existing stock, the law of motion of capital accumulation for each area i in each period t is given by:

$$H_{i,t} = H_{i,t-1} + I_{i,t} \tag{4.9}$$

where $I_{i,t}$ is new investments in housing capital. We assume that the marginal cost of construction $MC_{i,t}$ is inversely proportional to the existing housing stock $H_{i,t-1}$, implicitly meaning that investment in new construction projects is more attractive in larger housing markets:

$$MC_{i,t} = C_{i,t} \times \left(\frac{I_{i,t}}{H_{i,t-1}} + 1 \right)^{\frac{1}{\varphi_{i,t}}}$$

where $C_{i,t}$ represents housing construction costs (land, labor, and building materials), which rise with investment to reflect the scarcity of the inputs used into housing production, and $\varphi_{i,t}$ is the local-specific housing supply elasticity that is allowed to vary over time. The assumption of a time-varying supply elasticity, consistent with our estimates in the previous section, is a new feature of our model compared with Glaeser et al. (2008). We apply Tobin's Q theory to determine new investments, in that builders adjust supply based on the price of housing relative to the marginal cost of construction. The investment function is obtained by setting the price of housing $PH_{i,t}$ equal to $MC_{i,t}$:

$$I_{i,t} = H_{i,t-1} \times max \left[0, \left(\frac{PH_{i,t}}{C_{i,t}} \right)^{\varphi_{i,t}} - 1 \right] \tag{4.10}$$

Assuming that the supply elasticity is always greater than zero, it follows from Eq. 4.10 that investment will only take place if the price of housing exceeds the costs of construction. By inserting the expression of Eq. 4.10 into Eq. 4.9, and then taking logs, we get the housing supply function $S_{i,t}$:

$$S_{i,t} = \begin{cases} H_{i,t-1} & \text{if } PH_{i,t} \leq C_{i,t} \\ H_{i,t-1} + \varphi_{i,t}(PH_{i,t} - C_{i,t}) & \text{if } PH_{i,t} > C_{i,t} \end{cases} \tag{4.11}$$

The supply curve is piecewise linear and kinked: if the price of housing is smaller or equal to construction costs, the supply of homes is simply equal to the existing housing stock. If the price of housing exceeds construction costs, builders will add a flow of new construction to the existing stock. In this framework, supply is assumed to be rigid downwards, as housing is typically not demolished or dismantled (Glaeser and Gyourko, 2005). Note also that supply increases linearly with the supply elasticity $\varphi_{i,t}$, as we will see below.

We specify housing demand as follows:

$$D_{i,t} = v_0 r_t + v_1 Y'_{i,t} + v_2 PH_{i,t} \tag{4.12}$$

where demand depends linearly on the interest rate r_t, assumed to be common across markets, on local house prices, and on area-specific factors captured by the vector $Y'_{i,t}$, such as household income and crime rates, as a proxy for local amenities – used before in the empirical analysis to identify a demand shift.

Consider a market where construction is greater than zero, with the equilibrium in the housing market being determined by the intersection of supply (Eq. 4.11) and demand (Eq. 4.12). It follows that in equilibrium, house prices and quantity of housing assume the following expressions:

$$D_{i,t} = S_{i,t}$$

$$PH_{i,t} = \frac{1}{\varphi_{i,t} - v_2}(v_0 r_t + v_1 Y'_{i,t} - H_{i,t-1} + \varphi_{i,t} C_{i,t}) \tag{4.13}$$

$$S_{i,t} = \frac{\varphi_{i,t}}{\varphi_{i,t} - v_2}(v_0 r_t + v_1 Y'_{i,t} + v_2 C_{i,t}) - \frac{v_2}{\varphi_{i,t} - v_2} H_{i,t-1} \tag{4.14}$$

We now assume that the economy is in equilibrium at time t=0, and then is hit by a positive demand shock at time t=1, say, an expansionary monetary policy shock in which the central bank reduces the interest rate r_t. The marginal impact of an expansionary monetary policy shock is given by the derivative of Eqs. 4.13 and 4.14 with respect to minus r_t:

$$-\frac{\partial PH_{i,t}}{\partial r_t} = -\frac{v_0}{\varphi_{i,t} - v_2} > 0 \tag{4.15}$$

$$-\frac{\partial S_{i,t}}{\partial r_t} = -\frac{\varphi_{i,t} v_0}{\varphi_{i,t} - v_2} > 0 \tag{4.16}$$

Our model predicts that both house prices and quantity would increase after an interest rate reduction, resulting from the combination of a negative numerator and positive denominator (multiplied by minus 1 as we have a reduction in the interest rate): housing demand is stimulated by declines in the interest rate (negative v_0), while supply elasticities are always equal to or greater than zero, and housing demand declines when house prices increase (negative v_2).

We illustrate the conjectures above in the left panel of Figure A.1. After a reduction in the interest rate, the demand curve shifts from D_0 to D_1, implying a new equilibrium with both higher house prices ph_1 and quantity h_1 (point B). The dotted part of the housing supply curve illustrates that housing supply is rigid downwards, so that the supply curve kinks at A at time t=0 and at B after the shock. This exercise assumes that supply elasticities are constant over time as in Green et al. (2005), Gyourko et al. (2008), Huang and Tang (2012), Glaeser et al. (2014), Anundsen and Heebøll (2016), and Aastveit and Anundsen (2017). The supply elasticities only play a role over the cross-section. For instance, by exploring the variation in supply elasticities across a large sample of MSAs, Aastveit and Anundsen (2017) find that expansionary monetary policy shocks have a substantially greater impact on house prices in markets with an inelastic housing supply.

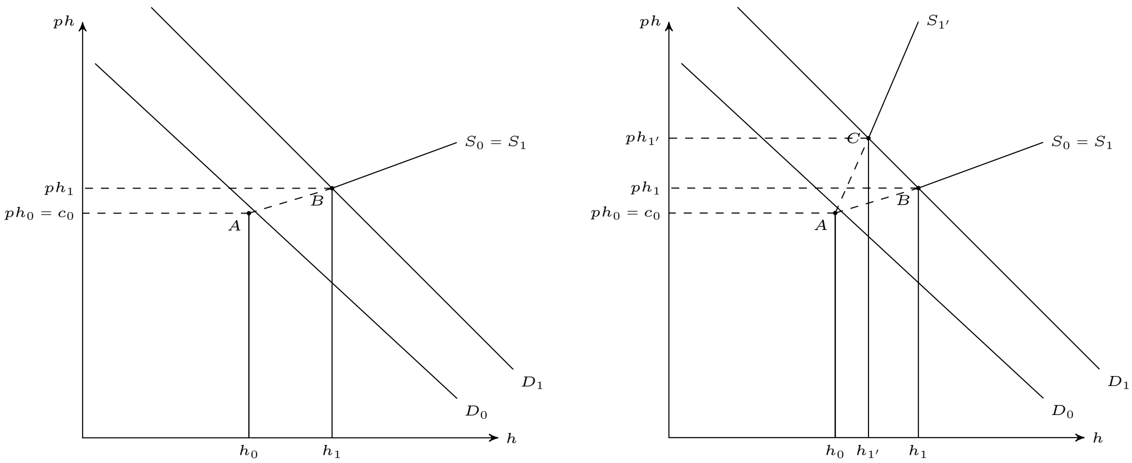
We move one step forward, and show in our model that the same logic applies within the same market: the impact of a given demand shock on house prices in the same area varies over time, if the slope of the supply curve changes between periods. When there is a reduction in the interest rate, the marginal effect of a decline in the supply elasticities on prices and quantities is given by the derivative of Eqs. 4.15 and 4.16 with respect to minus

$\varphi_{i,t}$:

$$
\begin{aligned}
-\frac{\partial\left(-\frac{\partial PH_{i,t}}{\partial r_t}\right)}{\partial \varphi_{i,t}} &= -\frac{v_0}{(\varphi_{i,t}-v_2)^2} > 0 \\
-\frac{\partial\left(-\frac{\partial S_{i,t}}{\partial r_t}\right)}{\partial \varphi_{i,t}} &= -\frac{v_0 v_2}{(\varphi_{i,t}-v_2)^2} < 0
\end{aligned}
$$

Our model suggests that if supply elasticities decline over time for the same area, a lower interest rate would lead house prices to rise by more, and this would be reflected in a smaller expansion in supply.g We illustrate this conjecture in the right panel of Figure A.1. Assuming a decline in the supply elasticity for a given local housing market between period 0 and 1 – akin to what we have found in our empirical estimates – then a steeper supply curve implies that a demand shock moves the equilibrium to higher prices and lower quantity compared to a situation where the supply elasticity is constant (point C versus point B). In this new equilibrium, a steeper supply curve over time ($S_0 = S_1$ to $S_{1'}$) implies that a given demand shock can act as an amplification mechanism for house prices.

FIGURE A.1: Impact of expansionary monetary policy shock on the housing market



Market 1: Constant supply elasticity   Market 2: Steeper elasticity over time

Notes: Left panel: D_0 and S_0 are the original demand and supply curves, and point A is the initial equilibrium with house prices ph_0 and quantity h_0. After an expansionary monetary policy shock, demand shifts to D_1, and the new equilibrium is reached at point B, with both higher house prices ph_1 and quantity h_1, conditional on a time-invariant supply elasticity ($S_0 = S_1$). Right panel: If the supply elasticity declines between periods, i.e. the supply curve steepens from $S_0 = S_1$ to $S_{1'}$, the equilibrium moves to point C, with higher house prices $ph_{1'}$ and lower quantity $h_{1'}$.

B Data description

Building permits: number of permits issued by a local jurisdiction to proceed on a construction project. Source: Census Bureau, and Moody's Analytics.

Housing starts: number of housing units in which construction work has started. The start of construction is when excavation begins for the footings or foundation of a building. Source: Census Bureau, and Moody's Analytics.

Housing stock: a house, apartment, mobile home or trailer, a group of rooms, or a single room that is occupied or available for occupancy. Updated from 2010q3 onwards by accumulating housing completions. Source: Census Bureau, and Moody's Analytics.

FHFA house price index: weighted, repeat-sales index, measuring average price changes in repeat sales or refinancings on the same single-family properties whose mortgages have been purchased or securitized by Fannie Mae or Freddie Mac. Source: FHFA, and Moody's Analytics.

UNAVAL: the land unavailability index captures housing supply geographical constraints. It is constructed using topographic maps measuring the proportion of land in a 50 km radius of the city center that is lost to steep slopes and water bodies, such as oceans, rivers, lakes and wetlands. Source: Saiz (2010).

WRLURI: the Wharton Residential Land Use Regulatory Index captures regulatory restrictions in the housing market, i.e. measures the time and financial cost of acquiring building permits and constructing a new home. It refers to the principal component of 11 survey-based measures which is interpreted as the degree of stringency of local zoning laws. Source: Gyourko et al. (2008).

Crime rates: counts of crimes per 100,000 inhabitants reported to the police for each police agency (cities, towns, and villages). It is broken down into two major types: violent crime, which includes offences of murder, forcible rape, robbery, and aggravated assault, and property crime, which includes offences of burglary, larceny-theft, and motor vehicle theft. Source: Uniform Crime Report Offenses Known to Law Enforcement dataset of the FBI.

Population: resident population in each MSA. Source: Census Bureau, and Moody's Analytics.

Population density: population per square mile. Annual data interpolated into quarterly. Data available since 2000. Source: Census Bureau, and Moody's Analytics.

CPI: consumer price index for all urban consumers. Source: BLS, and Moody's Analytics.

Disposable personal income: The income available to persons for spending or saving. It is equal to personal income less personal current taxes. Source: BEA, and Moody's Analytics.

Construction wages: wages and salaries in the construction sector. Data available at the state level. The original quarterly series has been adjusted for seasonality using X-13-ARIMA from the Census Bureau. Source: BEA.

Unemployment rate: the number of unemployed as a % of total labour force. Source: BLS, and Moody's Analytics.

Mortgage originations: dollar amount of new mortgage loans approved by the mortgage broker or loan officer. Data available until 2016q4. Source: Home Mortgage Disclosure Act, and Moody's Analytics.

Dependency ratio: ratio of people younger than 15 or older than 64 years old to the working age population (those aged 15-64). Source: Census Bureau, and Moody's Analytics.

Black: fraction of black or African American relative to total population. Annual data interpolated into quarterly. Source: Census Bureau, and Moody's Analytics.

Hispanic: fraction of people of Hispanic or Latino origin relative to total population. Annual data interpolated into quarterly. Source: Census Bureau, and Moody's Analytics.

TABLE A.1: Descriptive statistics

	Obs	Mean	Std. Dev.	Min	Max
Real HPI (log)	21,336	4.8	0.2	4.1	5.5
Building permits (log)	21,336	7.3	1.5	2.1	12.1
Housing starts (log)	21,336	7.3	1.4	2.2	11.6
Housing stock (log)	21,336	5.2	1.1	3.3	9.0
UNAVAL	21,336	0.3	0.2	0.0	0.9
WRLURI	21,336	-0.1	0.8	-1.8	4.3
Real personal income (log)	21,336	16.4	1.2	14.2	20.7
Real construction wages (log)	21,336	15.1	1.0	12.2	17.0
CPI (log)	21,336	5.3	0.2	4.5	5.7
Real mortgage originations (log)	19,439	13.7	1.3	8.5	18.3
Unemployment rate (%)	21,336	5.9	2.6	1.2	32.1
Population (log)	21,336	6.0	1.1	4.0	9.9
Population density	18,288	319.3	344.9	6.3	2754.3
Dependency ratio (%)	21,336	50.7	6.2	31.5	85.2
Black ratio (%)	21,272	11.7	11.2	0.1	53.9
Hispanic ratio (%)	21,272	11.3	14.7	0.4	92.2
Total crime rate (%)	17,000	3937.4	1291.2	1128.4	9469.3
Property crime rate (%)	17,360	3492.1	1159.3	3.1	8234.6
ΔReal HPI (%)	21,336	0.3	1.9	-15.7	12.3
ΔReal personal income (%)	21,336	0.6	1.3	-8.9	11.9
ΔReal construction wages (%)	21,336	0.5	3.0	-19.7	17.3
ΔCPI (%)	21,336	0.5	0.6	-3.1	4.0
ΔUnemployment rate	21,336	0.0	0.4	-8.4	6.2
ΔPopulation (%)	21,336	0.2	0.5	-44.3	10.2

Sources: Bureau of Economic Analysis, Bureau of Labor Statistics, Census Bureau, Federal Bureau of Investigation, Federal Housing Finance Agency, Gyourko et al. (2008), Home Mortgage Disclosure Act, Moody's Analytics, and Saiz (2010).

C Details on the Bartik-type instrumental variable approach

The first stage regression when we employ the Bartik-type instrumental variable approach estimates the sensitivity of local house prices to regional house prices for each MSA and for each housing boom j:

$$\Delta \log(HPI_{i,r,t}) = \eta_i^j + \theta_i^j \Delta \log(HPI_{r,t}) + \psi_i^j X'_{i,r,t} + \epsilon_{i,r,t}^j \tag{4.17}$$

where $\Delta log(HPI_{i,r,t})$ denotes the log percentage change in house prices in MSA i of region r, and $\Delta log(HPI_{r,t})$ is the equivalent variable for the nine Census Divisions. In the spirit of the Bartik-shift share approach, our instrument for the house price variables in Eq. 4.6 is given by $\widehat{\theta_i^j} \Delta \log(HPI_{r,t})$. We add a set of controls $X'_{i,t}$ – construction wage growth, income growth, the change in the unemployment rate, population growth, and inflation – to minimise the potential bias arising from the possibility that local permits in our main equation may respond differentially to regional shocks through other channels than local house prices (see the discussion in Guren et al., 2018). When running the regression for MSA i, we exclude the MSA in question from the regional house price index $HPI_{r,t}$, so as to avoid biasing the coefficient θ_i^j, given that the same variable would appear simultaneously on the left and right hand sides.

After running the regression above for each MSA and for each housing boom, we collect the instrument $\widehat{\theta_i^j} \Delta \log(HPI_{r,t})$ for house prices in our supply equation Eq. 4.6. The coefficients are in general estimated less precisely than in the baseline regression, particularly on the interaction terms (Table D.2 in Appendix D). The Bartik-style instruments are, however, relatively weak, as the F-tests suggest that we cannot reject the null hypothesis that our instruments are not weakly correlated with the endogenous variables. One of the reasons for the low power of our instruments may be related to the difficulty of this approach in separating housing demand from supply. In addition, another reason may be related to the lack of enough time variation within each MSA to identify house prices.

D Additional tables and figures

TABLE D.1: Monetary policy shocks

	HF	Q
Mean	-0.011	-0.022
Median	0	-0.011
Std. deviation	0.067	0.067
Min	-0.413	-0.328
Max	0.125	0.128
No. Obs.	127	62

Notes: HF refers to high frequency and Q to quarterly.

TABLE D.2: Regression estimates: Bartik-type instrument

	1996-2006	**2012-2017**
$\log(HPI)$	3.895***	2.668
	(1.071)	(4.091)
$\log(HPI) \times UNAVAL$	-2.033	-6.744
	(1.807)	(5.699)
$\log(HPI) \times WRLURI$	-1.252	1.857
	(1.547)	(2.625)
$\log(H_{t-1})$	0.390***	0.214***
	(0.060)	(0.050)
Number of MSA	241	254
Observations	7,381	4,866
Cragg-Donald F-test	8.13	11.78
Kleibergen-Paap (robust) F-test	0.192	1.275

Notes: IV estimates of Eq. 4.6, where the dependent variable is the log of building permits. House prices have been instrumented by exploring the sensitivity of local house prices to regional house prices (see Section 4.6.1 for more details). The Cragg-Donald F-test and Kleibergen-Paap F-test assume that under the null the excluded instruments are not weakly correlated with the endogenous regressors. The constant and additional control variables are not reported. Robust heteroskedastic standard errors shown in parentheses. Asterisks, *, **, and ***, denote statistical significance at the 10%, 5%, and 1% levels.

TABLE D.3: Robustness regression estimates by housing boom

	1996-2006					2012-17				
	(1) Base	(2) Tot_crime	(3) Perm_int	(4) SRI	(5) Mortg	(6) Base	(7) Tot_crime	(8) Perm_int	(9) SRI	(10) Mortg
$\log(HPI)$	2.774***	2.261***	2.671***	2.431***	2.737***	1.794**	1.730**	1.723**	1.681**	1.065
	(0.428)	(0.376)	(0.425)	(0.367)	(0.417)	(0.847)	(0.855)	(0.847)	(0.741)	(0.889)
$\log(HPI) \times UNAVAL$	-1.344***	-1.182***	-1.307***		-1.380***	-1.225	-1.359	-1.160		-2.038
	(0.340)	(0.316)	(0.336)		(0.325)	(1.185)	(1.232)	(1.184)		(1.259)
$\log(HPI) \times WRLURI$	-0.718***	-0.660***	-0.705***		-0.672***	-1.086**	-1.084**	-1.054**		-0.371
	(0.096)	(0.095)	(0.096)		(0.091)	(0.422)	(0.426)	(0.422)		(0.401)
$\log(HPI) \times SRI$				-0.432***					-0.597***	
				(0.061)					-0.193	
$\log(H_{t-1})$	0.415***	0.421***	0.424***	0.419***	0.408***	0.203***	0.201***	0.207***	0.202***	0.182***
	(0.019)	(0.018)	(0.019)	(0.019)	(0.017)	(0.023)	(0.023)	(0.023)	(0.023)	(0.021)
Number of MSA	241	241	241	241	233	254	254	254	254	242
Observations	7,548	7,464	7,548	7,548	7,442	4,866	4,758	4,866	4,866	3,812
F-test	39.83	47.73	39.99	60.53	37.81	49.66	49.33	49.71	76.53	43.87
F-test (robust)	31.00	35.12	31.19	46.74	29.75	29.61	30.09	29.65	43.20	25.77

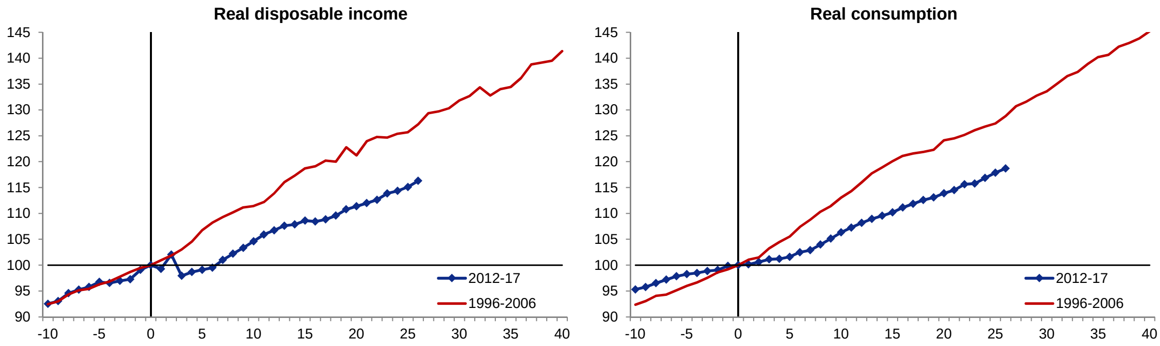
Notes: IV estimates of Eq. 4.6, where the dependent variable is the log of building permits. Each column represents a separate regression: *Base* is the baseline specification, *Tot_crime* uses total crime (property crime plus violent crime) as the instrument, *Perm_int* uses permit intensity as the dependent variable, *SRI* replaces UNAVAL and WRLURI with a supply restrictions index (the sum of these two variables standardized), and *Mortg* controls for mortgage originations. The F-test and robust F-test assume that under the null the excluded instruments are not weakly correlated with the endogenous regressors. The constant and additional control variables are not reported. Robust heteroskedastic standard errors shown in parentheses. Asterisks, *, **, and ***, denote statistical significance at the 10%, 5%, and 1% levels.

TABLE D.4: Estimated elasticities: alternative specifications

	1996-2006			2012-2017		
	p10	p50	p90	p10	p50	p90
Base	1.58	2.63	3.37	0.38	1.75	2.74
Tot_crime	1.19	2.14	2.81	0.28	1.67	2.67
Perm_int	1.50	2.53	3.25	0.36	1.68	2.64
SRI	1.48	2.51	3.26	0.36	1.79	2.83
Mortg	1.54	2.54	3.29	-0.22	0.69	1.30

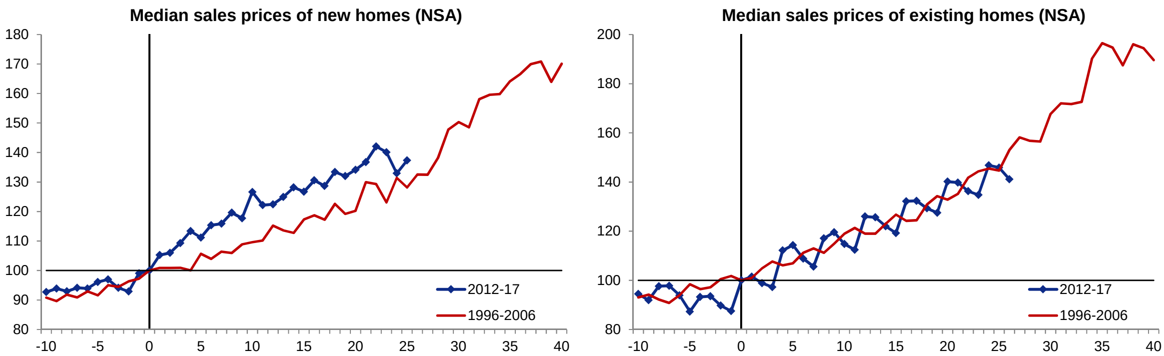
Notes: Estimated elasticities from Eq. 4.6 for the median, 10^{th} and 90^{th} percentiles for each housing boom. *Base* is the baseline specification, *Tot_crime* uses total crime as the instrument, *Perm_int* uses permit intensity as the dependent variable, *SRI* replaces UNAVAL and WRLURI with a supply restrictions index, and *Mortg* controls for mortgage originations.

Figure D.1: Demand fundamentals across booms



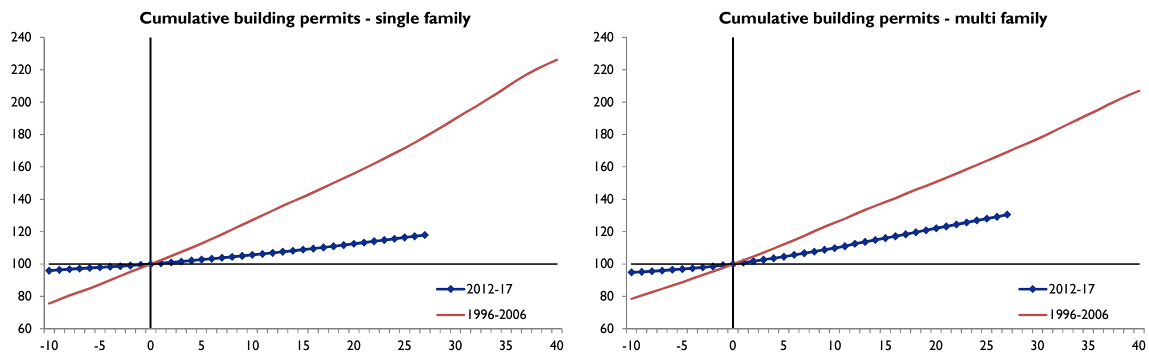
Sources: Bureau of Economic Analysis, and authors' calculations.
Notes: The figure tracks the evolution of real disposable income and real personal consumption at a quarterly frequency during the two house price booms. The zero on the x-axis marks the beginning of each housing boom. The solid line refers to the boom between 1996q4 and 2006q4, while the blue line with markers is from 2012q3 to 2017q4.

Figure D.2: Median sales prices of new and existing homes across booms



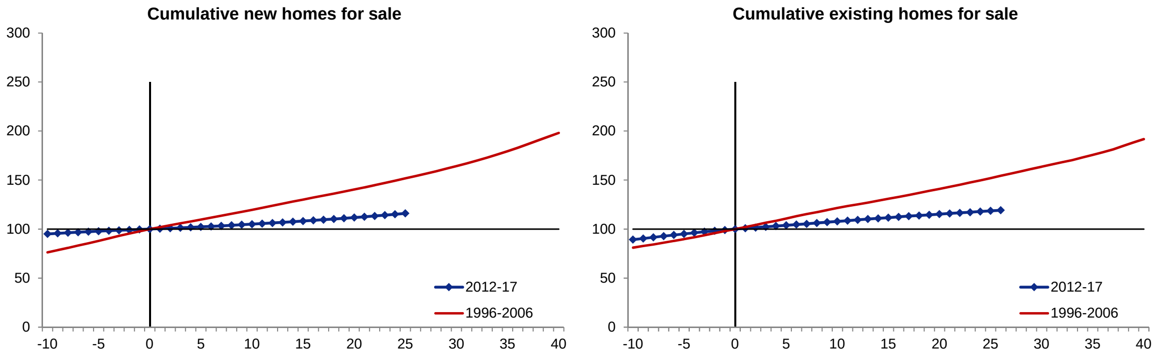
Sources: Census Bureau, National Association of Realtors, and authors' calculations.
Notes: The figure tracks the evolution of non-seasonally adjusted median sales prices of new and existing homes at a quarterly frequency during the two house price booms. The zero on the x-axis marks the beginning of each housing boom. The solid line refers to the boom between 1996q4 and 2006q4, while the blue line with markers is from 2012q3 to 2017q4.

Figure D.3: Building permits by segment across booms



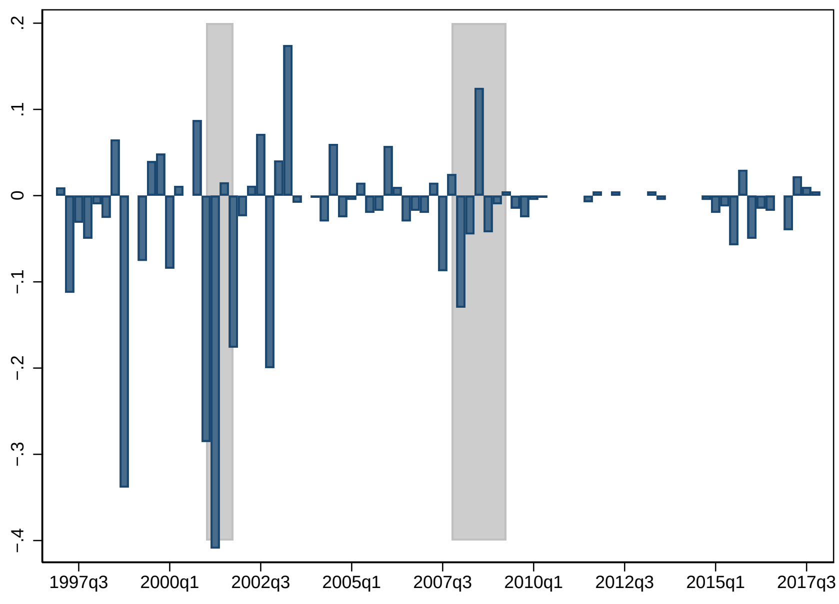
Sources: Census Bureau, and authors' calculations.
Notes: The figure tracks the evolution of single-family and multi-family building permits at a quarterly frequency during the two house price booms. The zero on the x-axis marks the beginning of each housing boom. The solid line refers to the boom between 1996q4 and 2006q4, while the blue line with markers is from 2012q3 to 2017q4.

FIGURE D.4: New and existing homes available for sale across booms



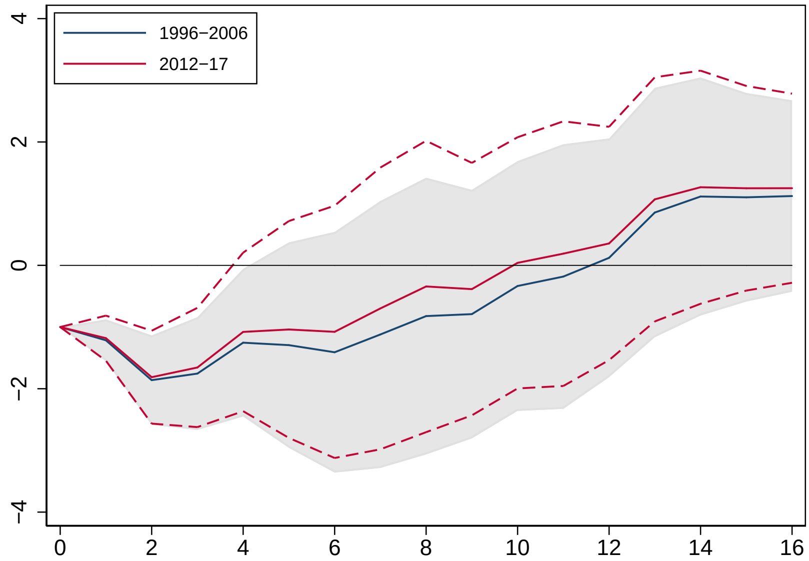
Sources: Census Bureau, National Association of Realtors, and authors' calculations.
Notes: The figure tracks the evolution of new and existing homes available for sale at a quarterly frequency during the two house price booms. The zero on the x-axis marks the beginning of each housing boom. The solid line refers to the boom between 1996q4 and 2006q4, while the blue line with markers is from 2012q3 to 2017q4.

FIGURE D.5: Monetary policy shocks



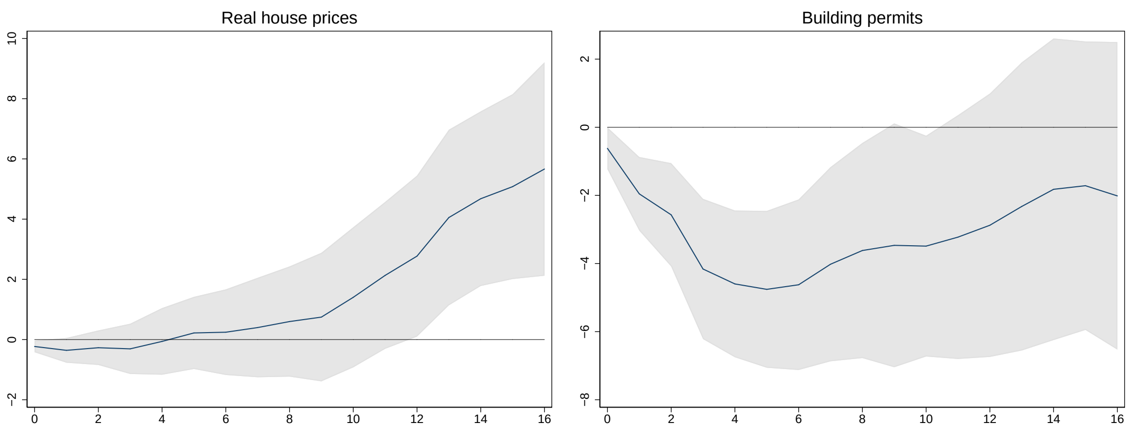
Sources: Bloomberg, and authors' calculations.
Notes: High-frequency monetary policy shocks aggregated to the quarterly frequency. Negative values refer to expansionary shocks. Shaded areas refer to recession periods, as defined by the NBER.

FIGURE D.6: Responses of policy indicator to an expansionary monetary policy shock



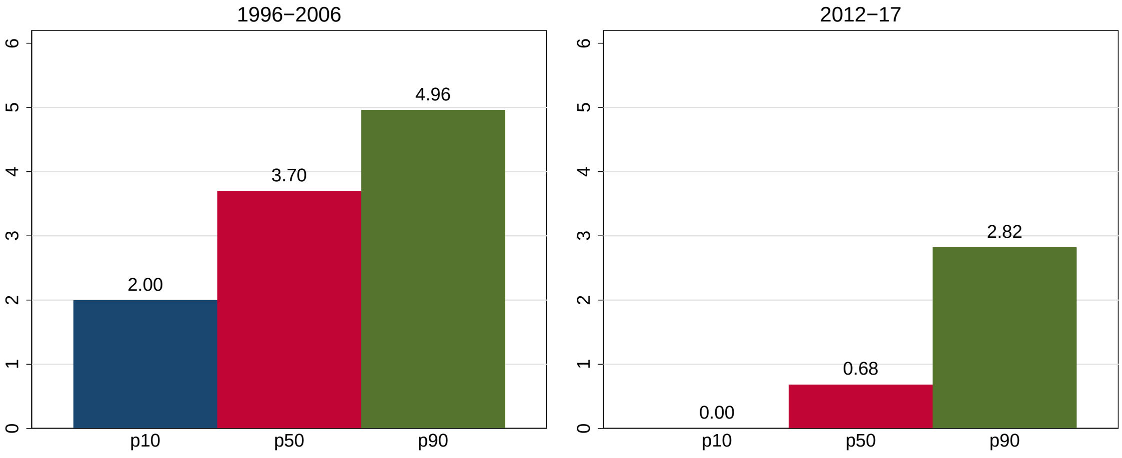
Notes: Cumulative impulse responses to a 100 basis point decline in the one-year Treasury bill yield, assessed at the sample median elasticity for each housing boom period. The grey area and the dashed red lines refer to 90 percent confidence bands.

FIGURE D.7: Differential effect between booms: Driscoll-Kraay estimator



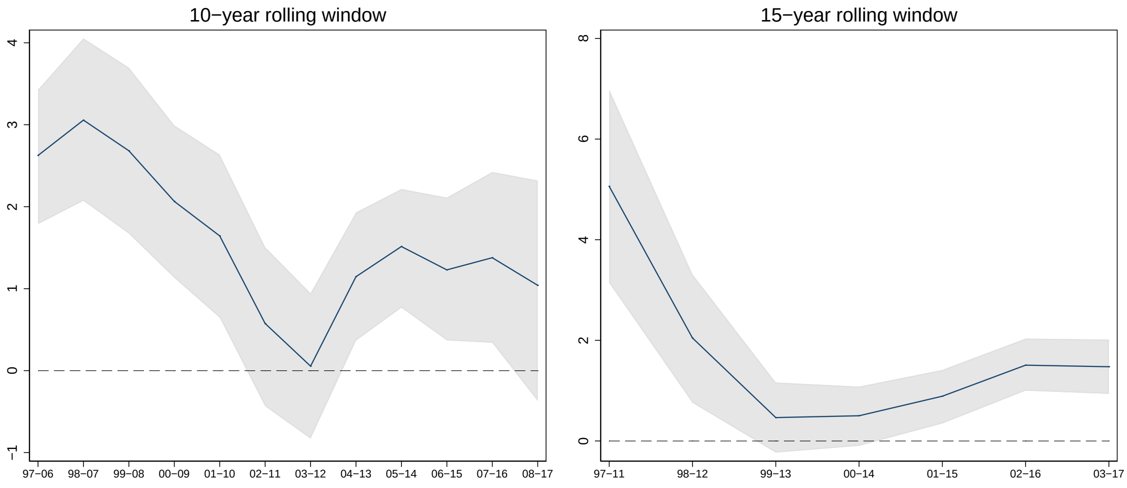
Notes: The figure depicts the difference in the estimated response of house prices and building permits between the 2012-17 and the 1996-2006 booms, with the associated 90 percent confidence bands. Standard errors have been adjusted for cross-sectional dependence in the errors across MSAs with the Driscoll-Kraay estimator.

FIGURE D.8: Estimated elasticities: Bartik-type instrument



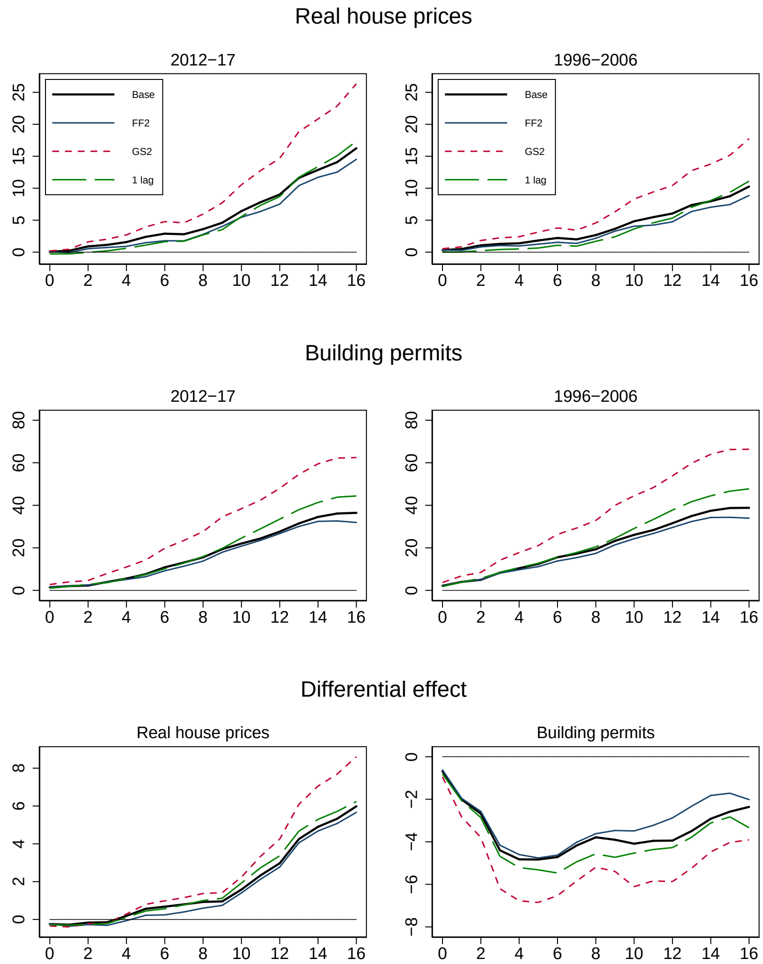
Notes: Estimated elasticities from Eq. 4.6 for the median, 10^{th} and 90^{th} percentiles for each housing boom. House prices have been instrumented by exploring the sensitivity of local house prices to regional house prices (see Section 4.6.1 for more details).

FIGURE D.9: Estimated elasticities with rolling windows



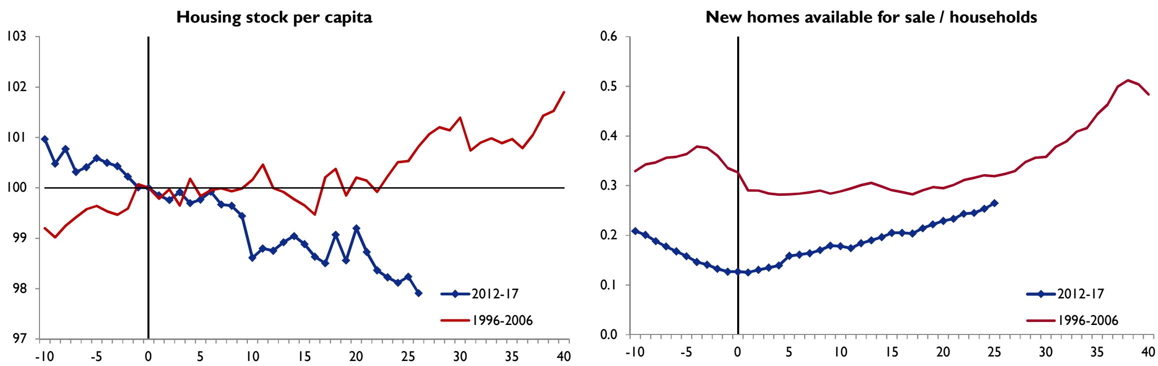
Notes: Estimated elasticities for the median and the associated 90 percent confidence bands from Eq. 4.6, using 10- and 15-year moving rolling windows. The x-axis refers to periods of 10 years (left figure) and 15 years (right figure).

FIGURE D.10: Responses to an expansionary monetary policy shock: alternative specifications



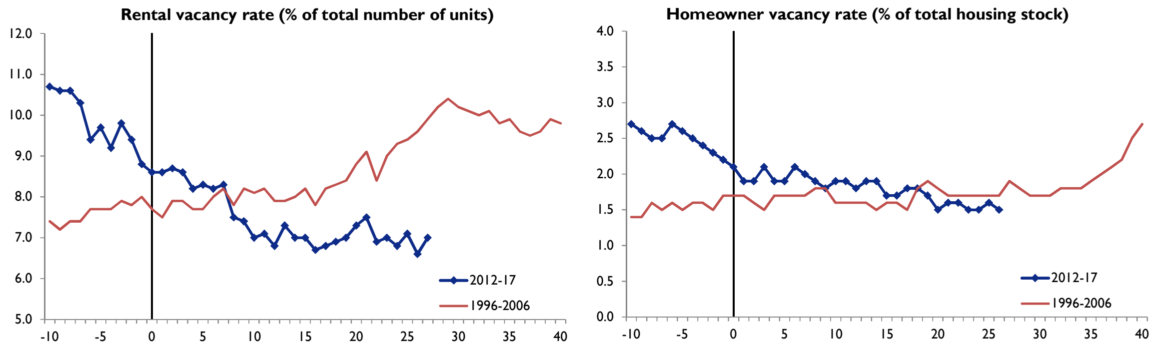
Notes: Cumulative impulse responses to a 100 basis point decline in the one-year Treasury bill yield, assessed at the sample median elasticity for each housing boom. *Base* is the baseline specification, *FF2* uses surprises in the two-month ahead Fed funds futures as the instrument for the monetary policy indicator, *GS2* uses the two-year Treasury note yield as the policy indicator, and *1 lag* is the benchmark model with only 1 lag.

FIGURE D.11: Housing supply indicators across booms



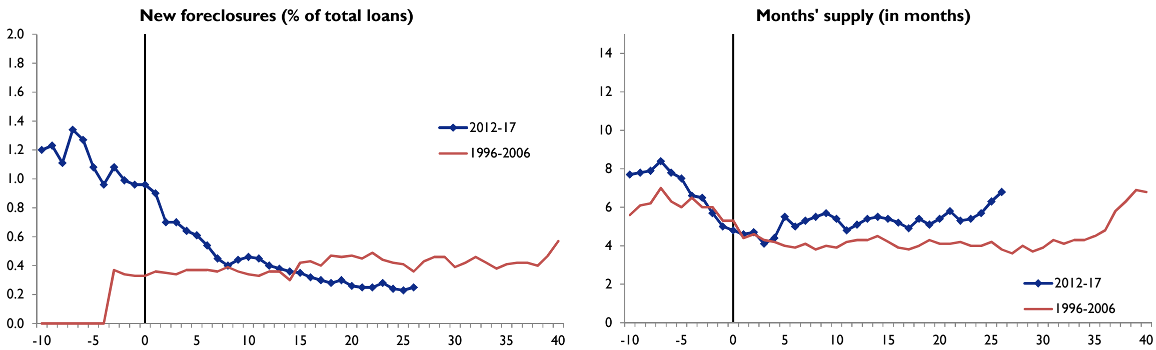
Sources: Census Bureau, and authors' calculations.
Notes: The figure shows developments in housing stock and new homes available for sale divided by the number of households during 1996q4–2006q4 (red solid line) and 2012q3–2017q4 (blue line with markers). The housing stock per capita is scaled such that it takes a value of 100 at the beginning of each period, whereas new homes available for sale per capita is displayed in level terms. The horizontal axis shows quarters around the beginning of the two booms, and the vertical line at zero is the starting point of both booms.

FIGURE D.12: Housing vacancy rates



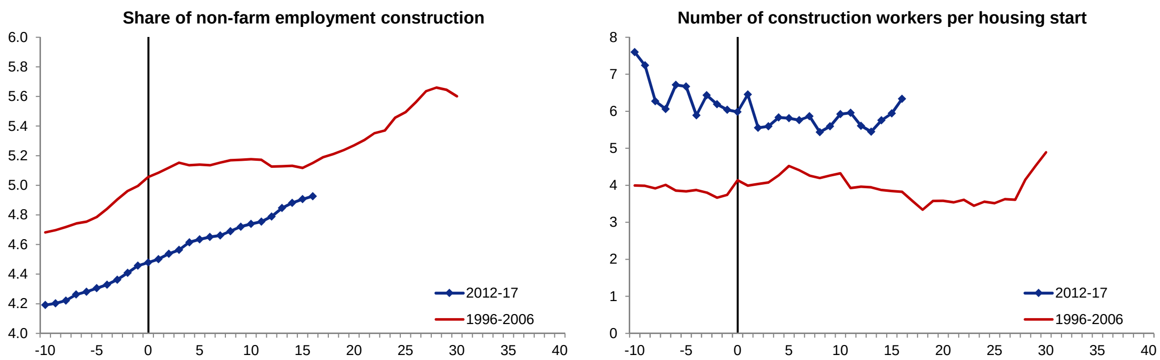
Sources: Census Bureau, and authors' calculations.
Notes: The figure tracks the evolution of housing vacancy rates at a quarterly frequency during the two house price booms. The zero on the x-axis marks the beginning of each housing boom. The solid line refers to the boom between 1996q4 and 2006q4, while the blue line with markers is from 2012q3 to 2017q4.

FIGURE D.13: Foreclosures and months' supply of houses



Sources: Census Bureau, CoreLogic, and authors' calculations.
Notes: The figure tracks the evolution of new foreclosures and months' supply of houses at a quarterly frequency during the two house price booms. The zero on the x-axis marks the beginning of each housing boom. The solid line refers to the boom between 1996q4 and 2006q4, while the blue line with markers is from 2012q3 to 2017q4.

FIGURE D.14: Construction employment across booms



Sources: Census Bureau, Bureau of Labor Statistics, and authors' calculations.
Notes: The figure tracks the evolution of the construction employment share in total employment, and the number of construction workers divided by housign starts at a quarterly frequency during the two house price booms. The zero on the x-axis marks the beginning of each housing boom. The solid line refers to the boom between 1996q4 and 2006q4, while the blue line with markers is from 2012q3 to 2017q4.

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