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by

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Abstract

Using a novel dataset 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 stock effect—we find that excessive indebtedness exerted a meaningful drag on consumption over and beyond income and wealth effects. The overall impact, however, is modest, around one-sixth of the slowdown in consumption between 2000–06 and between 2007–12—and mostly driven by states with particularly large imbalances in the household sector. This might be indicative of non-linearities, whereby indebtedness begins to bite only when misalignments from sustainable debt dynamics become more excessive.

JEL classification: C13, C23, C52, D12, H31

Bank classification: Credit and credit aggregates; Econometric and statistical methods; International topics

Résumé

Dans cette étude, les auteurs cherchent à déterminer, à partir d'un nouvel ensemble de données sur les États américains, si la dette des ménages et le long processus d'assainissement de leurs bilans comptent parmi les facteurs explicatifs de la croissance anémique de la consommation observée aux États-Unis depuis 2007, à la suite de l'éclatement de la bulle immobilière. Ils opèrent une distinction entre la réduction du levier d'endettement – un effet de flux – et le surendettement – un effet de stock – et constatent que le poids excessif de la dette des ménages a exercé un effet modérateur significatif sur la consommation, en plus du frein exercé par les effets de revenu et de richesse. L'incidence globale est cependant légère – elle équivaut à environ un sixième du ralentissement de la consommation enregistré sur les périodes de 2000-2006 et 2007-2012 – et est surtout attribuable aux États américains où les déséquilibres dans le secteur de ménages étaient particulièrement importants. Ce résultat pourrait signaler l'existence de non-linéarités, à savoir que la consommation ne commence à pâtir de l'endettement des ménages qu'à partir du moment où les écarts par rapport à une dynamique soutenable de la dette deviennent trop grands.

Classification JEL : C13, C23, C52, D12, H31

Classification de la Banque : Crédit et agrégats du crédit; Méthodes économétriques et statistiques; Questions internationales

Non-technical summary

The leveraging and subsequent deleveraging cycle in the US household sector had a significant impact on the performance of economic activity in the years around the Great Recession of 2007-09. A growing body of theoretical and empirical studies has therefore 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. 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.

We use panel regression techniques applied to a novel data set with prototype estimates of personal consumption expenditures at the state-level 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 functions, but it also has a role for debt and its misalignment from 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 effect at the national level, however, seems to be of a modest size—around one-sixth of the slowdown in consumption between 2000-06 and 2007-12—and mostly driven by the states with particularly large imbalances in their household sector. This might be indicative of non-linearities, whereby indebtedness begins to bite only when there is a sizable misalignment from the debt level dictated by economic fundamentals.

We argue that the nature of the indebtedness determines 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 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.

1 Introduction and literature review

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. 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.

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 :

$$C = \alpha W + \beta Y \tag{1}$$

where α and β are the marginal propensities to consume out of wealth and income, respectively. 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, $Interest_t * Debt_{t-1}$:

$$C_t = I_t + \Delta Debt_t - Interest_t * Debt_{t-1} \tag{2}$$

This implies that consumption for a large portion of US households may be driven both by changes in leverage and by the stock of outstanding debt.¹ In a similar vein, Guerrieri and Lorenzoni (2011) and Eggertsson and Krugman (2012) 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

¹Defining 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).

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

Against this background, our study uses state-level data over a sample that captures most of the leveraging and deleveraging cycle in the United States. We use a novel data set with prototype estimates of personal consumption expenditures at the state-level, published for the first time recently by the US Bureau of Economic Analysis. One important innovation consists of taking into account the effects of two distinct concepts of debt on US consumption growth separately: (1) deleveraging, which is 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 describe the data used in the paper, focusing in particular on the construction of our proxy for state-level consumption. In Section 3 we present our fixed effects regression results, together with several robustness checks, including the study of potential non-linearities. In Section 4 we exploit the heterogeneity in the data by carrying out an analysis at the state level. In Section 5 we extend the baseline model by placing more focus on the long-term dynamics, through the use of an error-correction model. The analysis of the out-of-sample contributions to consumption growth over the 2013-14 period are covered in Section 6. Section 7 concludes.

²While 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.

2 Data

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:

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

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 errors. As Figure B.2 in Appendix B shows, a bottom-up aggregation of our RS proxy for the states does well in

³For 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, respectively.

⁴Three 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%).

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

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). 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.⁷ 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.

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:

$$(\text{Homeownership rate} \times \text{Occupied housing units}) \times \text{HPI} \times \text{Median house price in 2000}$$

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.

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

⁷For 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).

- **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).

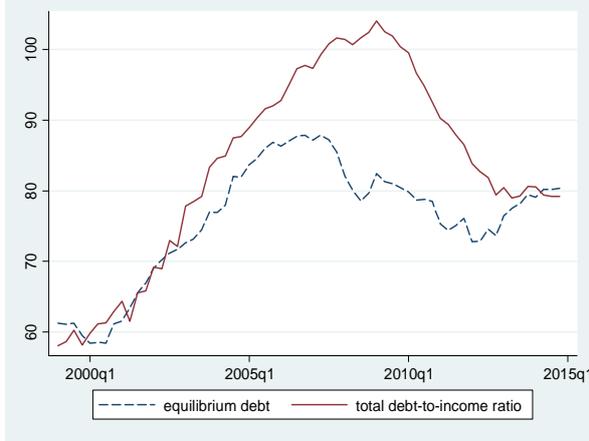
The state-level nominal indicators are deflated with the national personal consumption expenditures deflator. The last two variables will be in the centre of our analysis, as we are primarily interested in studying the role of debt and its misalignment from the estimated equilibrium on consumption growth. 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).⁸ 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 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.

⁸The fundamentals include a measure of house prices, the homeownership rate, the interest rate, and proxies for income uncertainty and credit supply.

Figure 1: Actual and equilibrium debt-to-income ratio and implied gap

Actual and equilibrium debt
(in % of personal income)



Gap between actual and equilibrium debt
(in percentage points)



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

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.

3 Estimation results

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:

$$\begin{aligned} \Delta_4 C_{it} = & \alpha_i + \beta_1 \Delta_4 Wealth_{it} + \beta_2 \Delta_4 Income_{it} + \beta_3 \Delta_4 Debt_{i,t-1} \\ & + \beta_4 Debt_gap_{i,t-1} + \gamma Controls_{it} + \delta_t + \varepsilon_{it} \end{aligned} \quad (3)$$

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,

respectively, 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 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. As for time effects, we augment our model with time dummies to control for the possibility of omitted time-varying factors driving some of the variables at the state level.⁹ The rationale and validity 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.¹⁰ Finally, in the choice between the FE and the random effects (RE) estimators, we relied on results from an auxiliary regression-based Hausman test.¹¹

One of the findings from Table 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. If we focus on the first seven columns, 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).¹² With respect to the effect of income,¹² 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

⁹The use of time dummies assumes that time effects have a homogeneous impact on the cross-sectional units. In Section 5 we focus on dynamic panel models, where we will relax this assumption by employing the common correlated effects approach by Pesaran (2006), which allows for heterogeneous cross-sectional dependence.

¹⁰As pointed out by 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 that become part of the error term and are likely to lead to seriously misleading inference if ignored in the estimation phase (Phillips and Sul 2003). We found evidence of severe cross-sectional dependence in the disturbances in a version of Equation 3 estimated without time dummies.

¹¹The 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 2002), which is valid in the presence of heteroskedasticity or within autocorrelation

¹²We 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.

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 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 2: Fixed effects estimation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Δ_4 Wealth	0.107*** (0.014)	0.105*** (0.014)	0.097*** (0.011)	0.095*** (0.012)	0.095*** (0.012)	0.090*** (0.011)	0.090*** (0.011)
Δ_4 Income	0.314*** (0.027)	0.300*** (0.029)	0.301*** (0.025)	0.293*** (0.026)	0.294*** (0.026)	0.284*** (0.025)	0.285*** (0.025)
Δ_4 Debt $_{t-1}$		0.018 (0.012)		0.014 (0.011)	0.016 (0.010)	0.020* (0.011)	0.020* (0.011)
Debt_gap $_{t-1}$			-0.021** (0.010)	-0.019* (0.011)	-0.020* (0.011)	-0.019* (0.011)	-0.019* (0.011)
Interest					0.567 (0.505)	0.526 (0.513)	0.525 (0.510)
Δ_4 UR						-0.282*** (0.073)	-0.283*** (0.073)
Δ_4 LTV							-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 the year-on-year change in real personal consumption expenditures (PCE). Δ_4 denotes year-on-year percent changes for housing wealth and income, while it refers to the year-on-year change for debt-to-income, the unemployment rate and the LTV ratio. Robust heteroskedastic and autocorrelation-consistent standard errors are 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, respectively, 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. We will further explore the link between debt and consumption in Section 5 when we introduce a panel-error correction framework to deal with the long-term dynamics.

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 2 (see Figure B.4 in Appendix B for the in-sample fit). Table 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. This needs to be seen in the context of the particularly large negative housing wealth shock experienced by US households.

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

Table 3: Contribution to the slowdown in PCE growth

Variable	2000-06	2007-12	Change	Contribution	%
Δ_4 PCE	3.5	1.4	-2.0	-2.0	100
Δ_4 Wealth	7.3	-3.5	-10.8	-1.0	48.1
Δ_4 Income	3.1	1.6	-1.4	-0.4	20.0
Debt (Δ_4 Debt)	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_4 UR)	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 the year-on-year change in real PCE. The table reports averages for all the US states.

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”¹³—which may not be representative of the United States as a whole. We will return to these questions in the final section of the paper when we deal with the state-level heterogeneity.

3.3 Robustness checks

We investigate the robustness and sensitivity of our 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 2.

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 4 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 column (7) from Table 2, which only reports the short-run effects.¹⁴

¹³The term “sand states” refers to Arizona, California, Florida and Nevada. These states experienced the most acute housing downturn in the United States.

¹⁴The 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 $0.043/(1-0.436)=0.076$.

Furthermore, the debt gap continues to be significant in this type of dynamic model.

Moreover, we also cross-check the baseline results by using our alternative measure of consumption at the state level (see Table B.1 in Appendix B). When we employ our RS proxy as explained in Section 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. 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.

Table 4: Fixed effects: Robustness checks

	(1) Baseline	(2) IV	(3) GLS	(4) XTSCC	(5) AB-BB
Δ_4 Wealth	0.090*** (0.011)	0.112*** (0.011)	0.067*** (0.001)	0.090*** (0.017)	0.043*** (0.012)
Δ_4 Income	0.285*** (0.025)	0.336*** (0.029)	0.241*** (0.003)	0.285*** (0.027)	0.184*** (0.021)
Δ_4 Debt _{t-1}	0.020* (0.011)	0.023* (0.010)	0.014*** (0.001)	0.020** (0.008)	0.009 (0.007)
Debt_gap _{t-1}	-0.019* (0.011)	-0.013* (0.005)	-0.021*** (0.001)	-0.019** (0.008)	-0.012* (0.007)
Interest	0.525 (0.510)	0.976*** (0.344)	-0.057 (0.044)	0.525 (0.556)	0.255 (0.420)
Δ_4 UR	-0.283*** (0.073)	-0.241*** (0.052)	-0.262*** (0.010)	-0.283*** (0.058)	-0.174*** (0.042)
Δ_4 LTV	-0.011 (0.028)	-0.004 (0.017)	-0.015*** (0.003)	-0.011 (0.020)	-0.010 (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 the year-on-year change in real PCE. Δ_4 denotes year-on-year percent changes for housing wealth and income, while it refers to the year-on-year change for debt-to-income, the unemployment rate and the LTV ratio. Robust heteroskedastic and autocorrelation-consistent standard errors are 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 shows the results from a FE regression with 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, respectively, statistical significance at the 10, 5 and 1% levels.

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 column (2) of Table 5 show that the results are broadly consistent with those in Table 2. 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.

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 5. 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).¹⁵ To proxy for the effects of debt service and excessive debt service, respectively, we interact the interest rate with the debt-to-income ratio and with the debt gap. Finally, we check whether our results are sensitive to the use of an alternative measure of income uncertainty (column 5).¹⁶

Table 5: Fixed effects: Sensitivity analysis

	(1) Baseline	(2) Net wealth	(3) Debt serv. ratio (DSR)	(4) Excess DSR	(5) Income uncert.
Δ_4 Wealth	0.090*** (0.011)		0.090*** (0.011)	0.090*** (0.012)	0.095*** (0.011)
Δ_4 Net Wealth		0.038*** (0.005)			
Δ_4 Income	0.285*** (0.025)	0.300*** (0.031)	0.285*** (0.025)	0.285*** (0.025)	0.292*** (0.025)
Δ_4 Debt _{t-1}	0.020* (0.011)	0.042*** (0.015)	0.021* (0.012)	0.022** (0.010)	0.016 (0.010)
Debt_gap _{t-1}	-0.019* (0.011)	-0.020 (0.012)	-0.019* (0.011)		-0.020* (0.011)
Interest	0.525 (0.510)	0.755 (0.520)	0.538 (0.575)	0.500 (0.502)	0.570 (0.506)
Δ_4 UR	-0.283*** (0.073)	-0.314*** (0.076)	-0.282*** (0.078)	-0.267*** (0.074)	
Δ_4 LTV	-0.011 (0.028)	-0.009 (0.027)	-0.011 (0.027)	-0.010 (0.028)	-0.009 (0.028)
Δ_4 (Int*Debt _{t-1})			-0.028 (0.213)		
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 the year-on-year change in real PCE. The interaction term in column (3) proxies for changes in the debt service ratio. The interaction 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, respectively, statistical significance at the 10, 5 and 1% levels.

Table 5 shows the estimation results. The coefficients of the key variables of interest—wealth, income, debt and the debt gap—remain overall robust to the modifications of our baseline specification. While we do not find a statistically significant effect for the 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

¹⁵In 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.

¹⁶Along 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).

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. Finally, our baseline estimation results did not change meaningfully when we used an alternative measure of income uncertainty (column 5).

3.4 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 6). 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 6: Fixed effects: Non-linearities

	(1) Baseline	(2) Δ_4 Debt squared	(3) Debt_gap squared	(4) Lever.	(5) Gap < 0
Δ_4 Wealth	0.090*** (0.011)	0.091*** (0.011)	0.092*** (0.011)	0.091*** (0.011)	0.092*** (0.011)
Δ_4 Income	0.285*** (0.025)	0.282*** (0.024)	0.282*** (0.025)	0.281*** (0.024)	0.283*** (0.025)
Δ_4 Debt $_{t-1}$	0.020* (0.011)	0.025** (0.012)	0.018* (0.010)	0.060** (0.026)	0.019* (0.010)
Debt_gap $_{t-1}$	-0.019* (0.011)	-0.016 (0.011)	-0.01 (0.014)	-0.016 (0.011)	-0.025*** (0.010)
Interest	0.525 (0.510)	0.565 (0.501)	0.585 (0.496)	0.575 (0.496)	0.601 (0.501)
Δ_4 UR	-0.283*** (0.073)	-0.291*** (0.073)	-0.268*** (0.072)	-0.297*** (0.073)	-0.273*** (0.073)
Δ_4 LTV	-0.011 (0.028)	-0.012 (0.028)	-0.009 (0.028)	-0.011 (0.028)	-0.010 (0.028)
$(\Delta_4$ Debt) $^2_{t-1}$		-0.001* (0.001)			
Debt_gap $^2_{t-1}$			-0.000 (0.000)		
Δ_4 Debt $_{t-1} > 0$				-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 the year-on-year change in 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, respectively, statistical significance at the 10, 5 and 1% levels.

Column (2) of Table 6 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.

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 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.

The results in Table 7 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.¹⁷

With respect to the debt variables, the coefficient on the change in the debt-to-income ratio

¹⁷The 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.

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.¹⁸ 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 90th 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.

Table 7: 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_4 Wealth	0.090*** (0.011)	0.098*** (0.016)	0.054*** (0.014)	0.090*** (0.011)	0.092*** (0.010)	0.118*** (0.017)	0.079*** (0.014)	0.090*** (0.011)
Δ_4 Income	0.285*** (0.025)	0.347*** (0.044)	0.212*** (0.044)	0.279*** (0.025)	0.276*** (0.025)	0.299*** (0.051)	0.256*** (0.030)	0.285*** (0.025)
Δ_4 Debt _{t-1}	0.020* (0.011)	0.019* (0.011)	0.009 (0.026)	0.022* (0.011)	0.041** (0.018)	0.039 (0.037)	0.016 (0.010)	0.021* (0.012)
Debt_gap _{t-1}	-0.019* (0.011)	-0.007 (0.011)	-0.016 (0.019)	-0.017 (0.011)	-0.014 (0.013)	-0.011 (0.011)	-0.022 (0.015)	-0.017 (0.014)
Interest	0.525 (0.510)	0.527 (0.671)	0.773 (0.736)	0.604 (0.502)	0.638 (0.461)	1.214 (0.802)	0.010 (0.713)	0.549 (0.512)
Δ_4 UR	-0.283*** (0.073)	-0.471*** (0.145)	-0.080 (0.113)	-0.287*** (0.073)	-0.277*** (0.074)	-0.183* (0.088)	-0.307*** (0.078)	-0.282*** (0.072)
Δ_4 LTV	-0.011 (0.028)	0.028 (0.046)	-0.023 (0.032)	-0.008 (0.027)	-0.011 (0.027)	0.095* (0.052)	-0.033 (0.030)	-0.011 (0.028)
LD50* Δ_4 Debt _{t-1}				-0.021 (0.020)				
LD50*Debt_gap _{t-1}				0.018 (0.015)				
LD90* Δ_4 Debt _{t-1}					-0.047** (0.023)			
LD90*Debt_gap _{t-1}					0.005 (0.015)			
NR* Δ_4 Debt _{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 the year-on-year change in 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 50th percentile and respectively 90th percentile of smallest declines in their household debt-to-income as defined above. Non-recourse (NR) states in columns (6) to (8) refer to those 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.

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

¹⁸This 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.

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 8 decomposes the factors behind the slowdown in PCE growth between 2000-06 and 2007-12 as already seen in Section 3.2. This time we split the sample between the top 10th percentile and the bottom 90th 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 7. 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 8: Contribution to the slowdown in PCE growth

Top 10th percentile of states by deleveraging					
Variable	2000-06	2007-12	Change	Contribution	%
Δ_4 PCE	5.5	0.9	-4.6	-4.6	100
Δ_4 Wealth	13.1	-8.1	-21.2	-2.0	42.5
Δ_4 Income	4.1	1.1	-3.1	-0.8	18.3
Debt (Δ_4 Debt)	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_4 UR)	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_4 PCE	3.2	1.5	-1.7	-1.7	100
Δ_4 Wealth	6.5	-2.9	-9.5	-0.9	52.0
Δ_4 Income	2.9	1.7	-1.2	-0.3	19.8
Debt (Δ_4 Debt)	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_4 UR)	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 the year-on-year change in 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.

5 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 (see Table A.3 in Appendix A). 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:

$$\Delta C_{it} = \mu_i + \phi_i(C_{i,t-1} - \theta X_{it}) + \delta_i \Delta X_{it} + \gamma_i \Delta Z_{it} + u_{it} \quad (4)$$

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

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.

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 :

$$u_{it} = \lambda_i' f_t + \varepsilon_{it} \quad (5)$$

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 4 suggests that cross-sectional dependence might be an issue. Indeed, the null hypothesis of cross-sectional independence is rejected at high levels of significance (see results in Table B.2 in Appendix B). 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.

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 2013, Albuquerque et al. 2015). More specifically, we estimate the following equation:

$$\Delta C_{it} = \mu_i + \phi_i(C_{i,t-1} - \theta X_{it}) + \delta_i \Delta X_{it} + \gamma_i \Delta Z_{it} + \alpha_i \bar{C}_t + \beta_i \bar{X}_t + \lambda_i \Delta \bar{C}_t + \eta_i \Delta \bar{X}_t + \tau_i \Delta \bar{Z}_t + \varepsilon_{it} \quad (6)$$

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

The four columns of Table 9 present the estimates from Equation 6, 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.¹⁹ The CCEPMG estimates show that all the long-run coefficients are highly statistically significant and have the expected sign.²⁰ 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.²¹

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 on consumption growth.

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

¹⁹Pesaran 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).

²⁰Moreover, 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.

²¹The 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.

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.

Table 9: CCEPMG estimation

	(1)	(2)	(3)	(4)
<i>Long-run</i>				
Log Wealth	0.048*** (0.015)		0.048*** (0.013)	
Log Net Wealth		0.005 (0.007)		0.010 (0.006)
Log Income	0.728*** (0.042)	0.806*** (0.044)	0.764*** (0.039)	0.788*** (0.041)
Debt _{t-1}	0.238*** (0.033)	0.268*** (0.031)	0.232*** (0.029)	0.203*** (0.027)
Debt_gap _{t-1}	-0.329*** (0.036)	-0.343*** (0.035)		
Speed of Adjustment	-0.481*** (0.072)	-0.469*** (0.068)	-0.514*** (0.079)	-0.529*** (0.073)
<i>Short-run</i>				
Δ Debt _{t-1}	0.019* (0.010)	0.026** (0.010)	0.009* (0.004)	0.009*** (0.004)
Δ Debt_gap _{t-1}	-0.008 (0.010)	-0.014 (0.011)		
Debt_gap _{t-1}			-0.134*** (0.028)	-0.110*** (0.027)
Constant	-0.244 (0.292)	-0.410 (0.252)	-0.187 (0.320)	-0.274 (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 are shown in parentheses. Asterisks, *, **, ***, denote, respectively, 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.

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.

6 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 10). 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 10: Out-of-sample contribution to the pick-up in PCE growth in 2013-14

Variable	2007-12	2013-14	Change	Contribution	%
Δ_4 PCE	1.4	2.4	1.0	1.0	100
Δ_4 Wealth	-3.5	2.4	5.9	0.5	52.3
Δ_4 Income	1.6	1.5	-0.1	0.0	-2.4
Debt (Δ_4 Debt)	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_4 UR)	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 the year-on-year change in 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.

7 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 sizable 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.

Appendix

A Data sources and descriptive statistics

Table A.1: Descriptive statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
Δ_4 PCE	2856	2.8	2.5	-5.3	14.6
Δ_4 Retail sales proxy	2576	1.4	9.1	-45.6	88.1
Δ_4 Wealth	2856	2.5	7.6	-24.8	42.0
Δ_4 Income	2856	2.5	2.8	-11.2	17.5
Δ_4 Debt	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_4 UR	2856	0.2	1.2	-5.4	6.0
Δ_4 LTV	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).

Table A.3: Panel cointegration tests based on Westerlund (2007)

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
	G_τ	-20.743	0.000	0.000
PCE, Wealth, Income, Debt and Debt gap	G_α	-4.017	0.000	0.000
	P_τ	-15.204	0.000	0.000
	P_α	-6.534	0.000	0.000
	G_τ	-17.934	0.000	0.000
	G_α	-6.361	0.000	0.000
PCE, Net wealth, Income and Debt	P_τ	-13.021	0.000	0.000
	P_α	-9.782	0.000	0.000
	G_τ	-20.056	0.000	0.000
	G_α	-4.005	0.000	0.000
	P_τ	-14.738	0.000	0.000
PCE, Net wealth, Income, Debt and Debt gap	P_α	-6.967	0.000	0.000

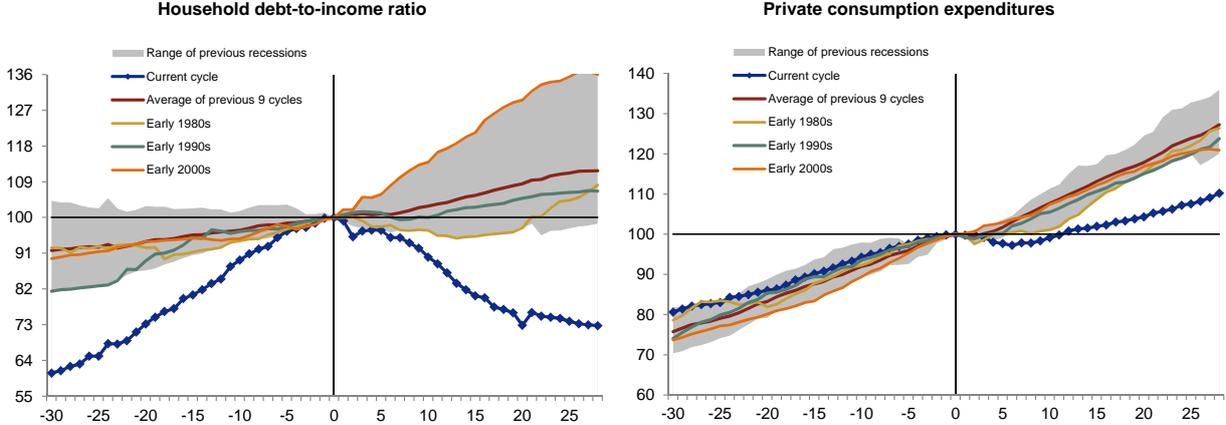
Notes: The results are for the four panel cointegration tests developed by Westerlund (2007). The null hypothesis is no cointegration. PCE, Income, Wealth and Net wealth are in logs. All tests are implemented with a constant in the regression. Lags and leads in the error correction test are chosen according 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.

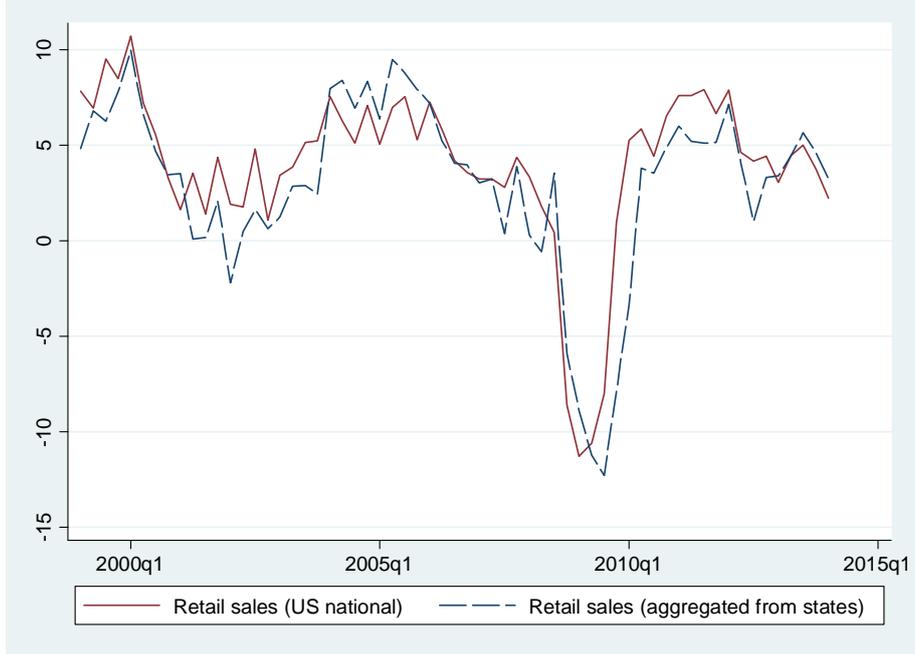
B Additional tables and figures

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



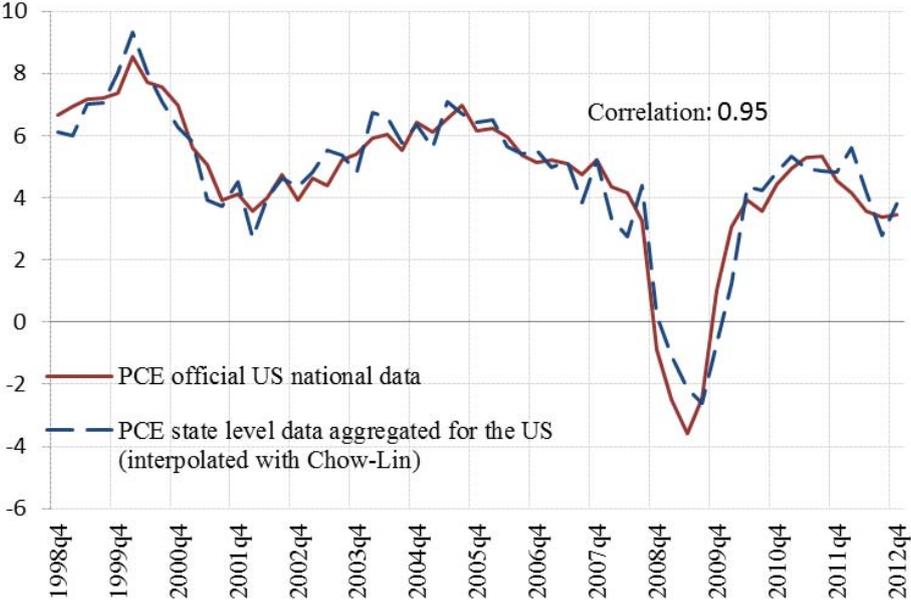
Source: Bureau of Economic Analysis, Federal Reserve Board and authors' calculations.
 Notes: Zero marks the start of each recession, where the index assumes the value of 100. The x-axis refers to quarters. According to the NBER, there have been 10 recessions in the US since 1950, with the latest one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

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



Source: US Census, authors' calculations.

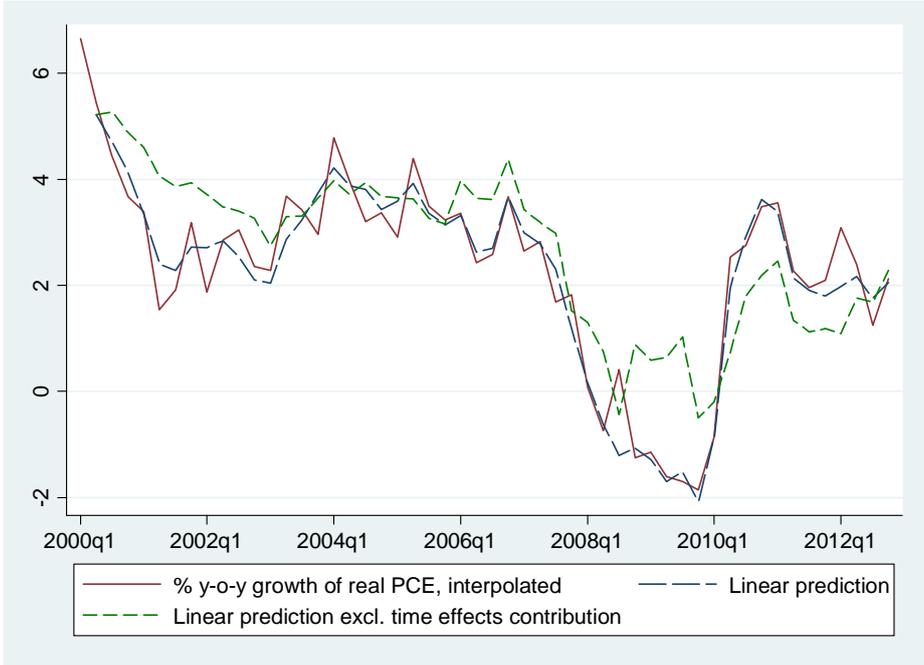
Figure B.3: US official PCE and state-aggregated, interpolated PCE (% year-on-year, nominal)



Source: BEA, authors' calculations.

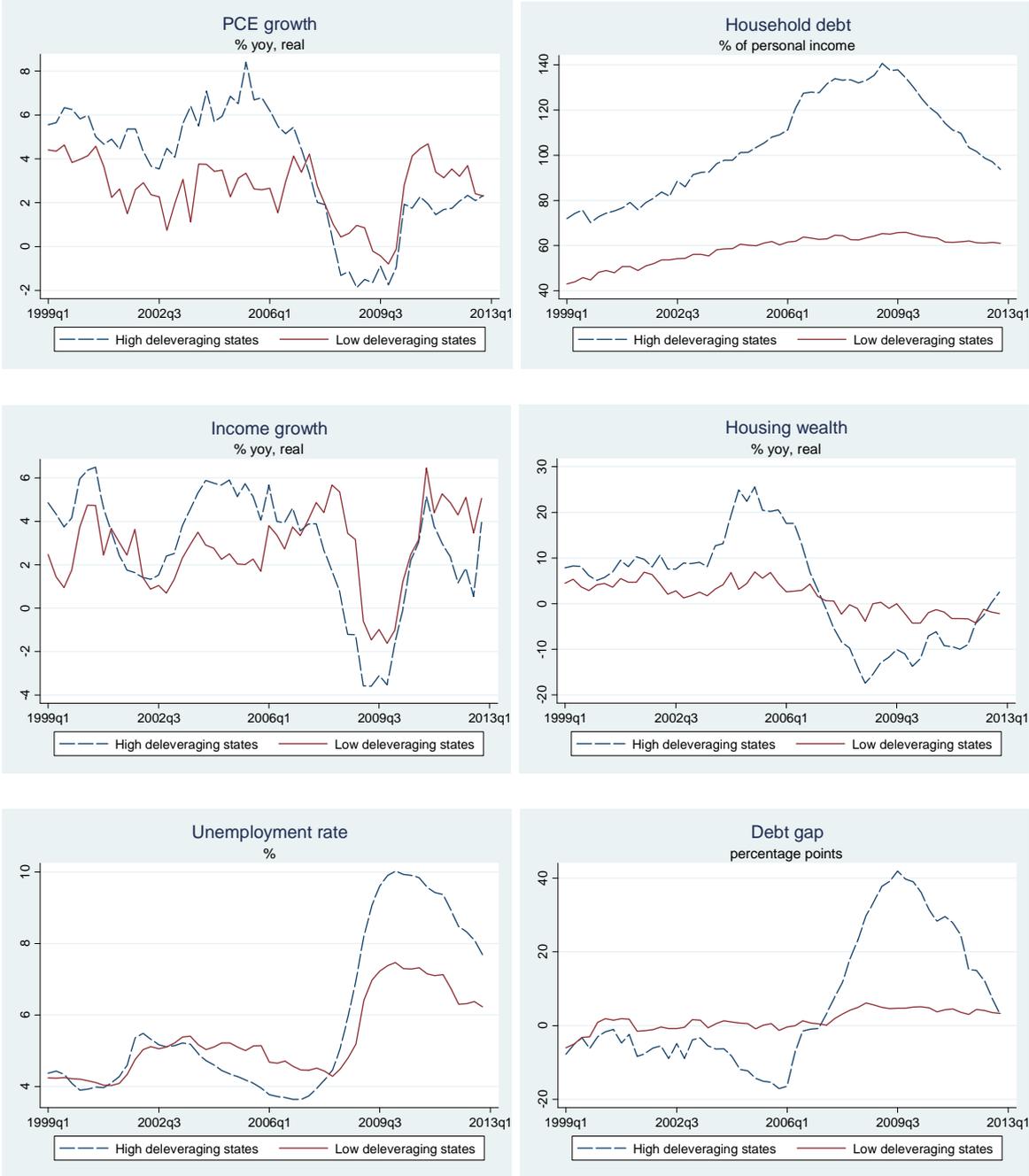
Note: The official national PCE data is the 2013 vintage since the experimental state-level PCE data (used for the interpolation) does not take into account the subsequent July 2014 national revisions to NIPAs.

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_4 Wealth	0.103** (0.044)	0.096** (0.044)	0.102** (0.038)	0.091** (0.042)	0.094** (0.041)	0.077* (0.044)	0.075* (0.044)
Δ_4 Income	0.861*** (0.188)	0.814*** (0.222)	0.840*** (0.193)	0.810*** (0.219)	0.824*** (0.220)	0.796*** (0.220)	0.797*** (0.221)
Δ_4 Debt $_{t-1}$		0.008 (0.088)		0.005 (0.085)	0.023 (0.087)	0.033 (0.087)	0.035 (0.087)
Debt_gap $_{t-1}$			-0.009 (0.032)	-0.010 (0.034)	-0.017 (0.035)	-0.012 (0.035)	-0.012 (0.036)
Interest					5.480 (3.781)	5.343 (3.639)	5.349 (3.632)
Δ_4 UR						-0.906 (0.751)	-0.911 (0.755)
Δ_4 LTV							-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 the year-on-year change in retail sales (RS) proxy. Δ_4 denotes year-on-year percent changes for housing wealth and income, while it refers to the year-on-year change for debt-to-income, the unemployment rate and the LTV ratio. Robust heteroskedastic and autocorrelation-consistent standard errors are 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, respectively, statistical significance at the 10, 5 and 1% levels.

Table B.2: PMG estimation

	(1)	(2)	(3)	(4)
<i>Long-run</i>				
Log Wealth	0.098*** (0.016)		0.065*** (0.013)	
Log Net Wealth		0.048*** (0.009)		0.030*** (0.007)
Log Income	0.768*** (0.017)	0.822*** (0.017)	0.745*** (0.014)	0.782*** (0.011)
Debt _{t-1}	-0.147*** (0.027)	-0.103*** (0.026)	-0.095*** (0.018)	-0.059*** (0.013)
Debt-gap _{t-1}	-0.134*** (0.027)	-0.160*** (0.031)		
Speed of Adjustment	-0.244*** (0.004)	-0.234*** (0.003)	-0.288*** (0.028)	-0.287*** (0.028)
<i>Short-run</i>				
ΔDebt _{t-1}	0.000 (0.006)	-0.001 (0.006)	0.006** (0.002)	0.005** (0.003)
ΔDebt-gap _{t-1}	0.010 (0.007)	0.011 (0.007)		
Debt-gap _{t-1}			-0.016 (0.014)	-0.021 (0.015)
Constant	0.385*** (0.021)	0.360*** (0.018)	0.634*** (0.057)	0.624*** (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 are shown in parentheses. Asterisks, *, **, ***, denote, respectively, 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.

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