the review of income and wealth

Review of Income and Wealth
Series 64, Number 2, June 2018
DOI: 10.1111/roiw.12265

DEBT OVERHANG AND DELEVERAGING IN THE US HOUSEHOLD SECTOR: GAUGING THE IMPACT ON CONSUMPTION

by Bruno Albuquerque*

Ghent University, Ghent, Belgium

and

Georgi Krustev

European Central Bank, Frankfurt am Main, Germany

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

JEL Codes: C23, D12, H31

Keywords: household deleveraging, debt overhang, consumption function, housing wealth

1. Introduction

The onset of the Great Recession broadly coincided with the start of a protracted period of debt reduction in the U.S. household sector (Figure B.1 in Appendix B, in the Online Supporting Information). This deleveraging process has been commonly cited as a reason for the pronounced slump in consumption and the subsequent sluggish recovery of the U.S. 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

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

*Correspondence to: Bruno Albuquerque, Ghent University, Sint-Pietersplein 5, 9000 Ghent, Belgium (bruno.albuquerque@ugent.be).

deleveraging phase might have contributed to depressing economic activity and consumption growth.

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

Our main finding suggests that the excessive indebtedness of U.S. 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 U.S. 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 organized as follows. In the next section, we provide a brief review of the literature on the link between consumption and debt. Section 3 contains a description of the data used in the paper, focusing in particular on the construction of our proxy for state-level consumption. In Section 4 we present our fixed-effects regression results, together with the main findings from several robustness checks, including the study of potential non-linearities. In Section 5 we exploit the heterogeneity in the data by carrying out an analysis at the state level. The analysis of the out-of-sample contributions to consumption growth over the 2013–14 period are covered in Section 6. Section 7 concludes.

2. Literature Review

From a theoretical standpoint, the relationship between consumption and debt is not clearly defined. In the standard life-cycle permanent income hypothesis framework, individuals smooth consumption over the life cycle by means of a single asset they can borrow or lend freely. Consumption, C, is a linear 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. 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 U.S. households may be driven by changes in leverage and 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 emphasize that the trigger for such deleveraging may come from both the supply-side—for example, as a result of tightening credit restrictions—and the demand side. Eggertsson and Krugman (2012) have also argued that household attitudes towards leverage may change over time, perhaps abruptly. Similarly, Dynan (2012) and Dynan and Edelberg (2013) point out that households may become uncomfortable with their indebtedness relative to some targeted level of leverage or behavioural benchmark. Changes in credit constraints or in the proportion of credit-constrained households, as well as in households' attitudes toward 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 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

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

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 summarizes 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 U.S., 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 U.S. consumption expenditure growth, while Antzoulatos (1996) finds that periods of rising consumer debt help to signal surges in U.S. 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 worth 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 U.S. 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 U.S. 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 percent 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 U.S., Mian and Sufi (2010) find that high household debt buildup in some U.S. 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.

The findings that debt has a negative impact on consumption are not limited to empirical studies analysing the more recent slump in U.S. consumption around the Great Recession. Using aggregate U.S. data, Mishkin (1976) found that increases in

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

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 U.S., 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 geographic or household level.

3. Data

3.1. *Proxies for Consumption at the State Level*

Our empirical analysis is challenged by the lack of officially published state-level data for U.S. 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; and
- i and t are subscripts denoting the panel (states) and time dimension (quarters) in our dataset.

[2]While the 1920s were characterized 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.

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 dataset—namely, the 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) dataset. 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 dataset. 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 (in the Online Supporting Information) shows, a bottom-up aggregation of our RS proxy for the states does well in comparison with the official U.S. 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 August 7, 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,

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

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

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

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

with a correlation of 0.95 between the two series (see Figure B.3 in Appendix B, in the Online Supporting Information).[7] Similarly to the case of our RS proxy, we deflate the nominal series with the U.S. national PCE deflator to obtain consumption growth in real terms.

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

3.2. *Explanatory Variables*

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

- **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, and HPI is the Federal Housing Finance Agency (FHFA) House Price Index (*sources*: Census Bureau and FHFA).
- **Real income**: Together with housing wealth, personal income also features predominantly in a traditional consumption function, where a portion of the income gains translates into higher consumption (the so-called marginal propensity) (*source*: U.S. 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

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

[8]For example, estimates from the study by Ballard and Lee (2007) are consistent with the hypothesis that U.S. 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).

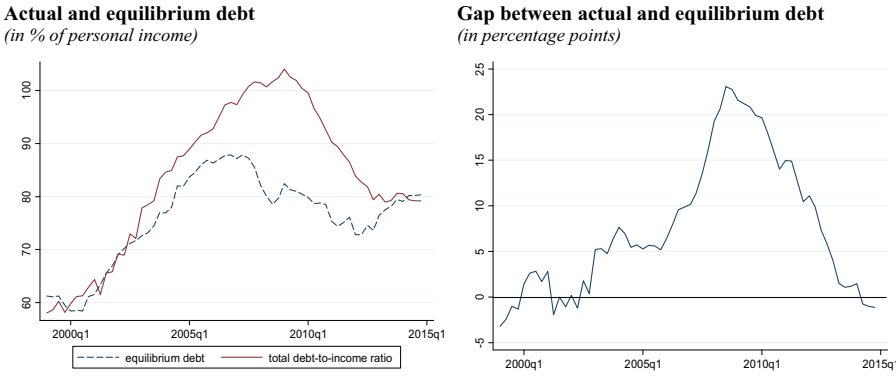

Figure 1. Actual and Equilibrium Debt-to-Income Ratio and Implied Gap [Color figure can be viewed at wileyonlinelibrary.com]

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

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 center 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 U.S. states (plus the District of Columbia).[9] As explained in Albuquerque *et al*. (2015), the model is estimated with the Pooled Mean Group (PMG) estimator, developed by Pesaran *et al*. (1999), and adjusted for cross-sectional dependence. The original model that was estimated on data from 1999Q1 to 2012Q4 has been updated with the U.S. national data up to 2014Q4.

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

Figure 1 shows that the rise in debt at the U.S. national level resulted in a growing misalignment from the equilibrium level since around 2002–03. This trend has been reinforced since late 2007 by the decline in equilibrium debt, as the economic fundamentals deteriorated. Thereafter, the deleveraging process (a decline in the debt-to-income ratio), which started in 2009, allowed the debt gap to shrink significantly from a peak of around 23 percentage points in 2008Q3. Our updated estimates suggest that the debt gap has been closed since mid-2014, with the recent improvement being supported by an increase in equilibrium debt, reflecting the sustained recovery in the U.S. economy, while actual debt appears to have stopped declining. At the state level, however, and despite the synchronized 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.

4. Estimation Results

4.1. *Fixed Effects*

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

$$\begin{aligned}\Delta_4 C_{it} = {} & \alpha_i + \beta_1 \Delta_4 Wealth_{it} + \beta_2 \Delta_4 Income_{it} + \beta_3 \Delta_4 Debt_{i,t-1} \\ & + \beta_4 Debt\_gap_{i,t-1} + \gamma Controls_{it} + \delta d_t + \varepsilon_{it},\end{aligned} \tag{3}$$

where C refers to real PCE, *Wealth* is real housing wealth, *Income* is real personal income, *Debt* is the household debt-to-income ratio, and *Debt_ gap* is the difference between the actual and the estimated household equilibrium debt-to-income ratio, taken from Albuquerque *et al*. (2015). *Controls* include the real interest rate (*Interest*), the unemployment rate (*UR*), and the *LTV*. A vector of time dummies d captures time-fixed effects. The subscripts i and t denote the 51 states in the panel and the time dimension (quarters). To minimize 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, in the Online Supporting Information); thus we use them in levels in Equation (1). The remaining series are transformed into year-on-year differences. Δ_4 represents year-on-year percentage changes for real PCE, housing wealth, and real income, while it refers to year-on-year percentage point changes for debt-to-income, the unemployment rate, and the loan-to-value ratio.

We guard against model misspecification in several ways. We report standard errors that are robust to heteroskedasticity, using the Huber–White sandwich estimator. Based on the results from several model selection tests, we choose to rely on the

two-way FE estimation method, which allows for group-specific and time effects. The latter allow to control for the possibility of omitted time-varying factors driving some of the variables at the state level. Finally, in the choice between the FE and the random effects (RE) estimators, we relied on results from an auxiliary regression-based Hausman test.[10]

The issue of cross-sectional dependence deserves a special mention. As pointed out by Hoyos and Sarafidis (2006), cross-sectional dependence is a common feature in panel datasets and is particularly relevant for units with a high degree of economic and financial integration, such as the states in the U.S. Cross-section interdependencies may arise from the presence of common shocks and unobserved components. Given the type of data and period that we are covering, examples of common unobserved factors in our case could be the housing boom and the subsequent bust, the 2007–09 financial crisis or changes in sales tax rates across states that are not captured by our explanatory variables in the model. If ignored in the estimation phase, such cross-sectional interdependencies become part of the error term and are likely to lead to seriously misleading inference due to their correlation with the explanatory variables (Phillips and Sul 2003). To correct for this problem, we allow for time effects by augmenting our model with time dummies.[11] The rationale and validity of this approach are confirmed by the Wald test, which shows the joint significance of the time dummies, and by their efficacy in minimizing the problem of cross-sectional dependence in the errors, as revealed by post-estimation results.[12] In particular, we found statistically significant negative time effects around the period of the Great Recession; a sign that time-varying common shocks originating from the financial crisis and the housing slump were driving the dynamics of the variables across panels.

One of the findings from Table 2 is that the two traditional variables that have been found in the literature to be the main drivers of consumption—wealth and income—consistently turn out to be highly statistically significant across different specifications. Based on the results in the seven columns of Table 2, we determine that the elasticity of consumption to housing wealth is estimated to lie in a range of between 0.09 and 0.11 percentage points, which is in line with the values reported in the literature (Case *et al*. 2013).[13] With respect to the effect of income, we find that a 1-percentage-point increase in real personal income growth leads to higher consumption growth in the order of 0.3 percentage points, the

[10]The standard version of the Hausman test becomes invalid when using robust standard errors and time dummies. The issue can be circumvented by using a more general testing procedure based on the use of auxiliary regressions (Mundlak 1978; Wooldridge 2002), which is valid in the presence of heteroskedasticity or within autocorrelation.

[11]The use of time dummies assumes that time effects have a homogeneous impact on the cross-sectional units. In supplementary material on the first-named author's website (available at https://sites.google.com/site/brunoalbuquerque19), we focus on dynamic panel models, where we relax this assumption by employing the common correlated effects approach by Pesaran (2006), which allows for heterogeneous cross-sectional dependence.

[12]We found evidence of severe cross-sectional dependence in the disturbances in a version of Equation 3 estimated without time dummies, which allow to filter out time effects.

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

TABLE 2
Estimation of Fixed Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\Delta_4 Wealth$	0.107***	0.105***	0.097***	0.095***	0.095***	0.090***	0.090***
	(0.014)	(0.014)	(0.011)	(0.012)	(0.012)	(0.011)	(0.011)
$\Delta_4 Income$	0.314***	0.300***	0.301***	0.293***	0.294***	0.284***	0.285***
	(0.027)	(0.029)	(0.025)	(0.026)	(0.026)	(0.025)	(0.025)
$\Delta_4 Debt_{t-1}$		0.018		0.014	0.016	0.020*	0.020*
		(0.012)		(0.011)	(0.010)	(0.011)	(0.011)
$Debt\_gap_{t-1}$			−0.021**	−0.019*	−0.020*	−0.019*	−0.019*
			(0.010)	(0.011)	(0.011)	(0.011)	(0.011)
Interest					0.567	0.526	0.525
					(0.505)	(0.513)	(0.510)
$\Delta_4 UR$						−0.282***	−0.283***
						(0.073)	(0.073)
$\Delta_4 LTV$							−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 $\Delta_4$real PCE. Δ_4 denotes year-on-year percentage changes for housing wealth and income, and year-on-year change for debt-to-income, the unemployment rate, and the LTV ratio. Robust heteroskedastic and autocorrelation-consistent standard errors 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, *, **, and ***, denote statistical significance at the 10, 5, and 1% levels.

same order of magnitude as the elasticity reported by Bacchetta and Gerlach (1997) for 1970–95.

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

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

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 that 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 in which household indebtedness is close to its equilibrium level. In other words, deleveraging matters for consumption, but its importance depends on how far from equilibrium household debt is while the process takes place.

When we employ our RS proxy as explained in Section 3.1 as the dependent variable, one difference from the regressions with the PCE is that it is now harder to uncover statistical significance for many of the explanatory variables, with the exception of wealth and income (see Table B.1 in Appendix B, in the Online Supporting Information). Nevertheless, in most cases, the point estimates of the coefficients maintain their expected signs. The differences in the results are mostly explained by the fact that the regressions with the RS proxy are estimated less precisely, thus yielding larger standard errors. In addition, the *R*-squared is substantially lower because the RS proxy is more volatile than the PCE measure and, arguably, exhibits larger measurement errors.

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

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

drop housing services from the PCE measure, which makes PCE more comparable to the RS proxy, and run the regressions again. When we consider non-housing PCE, our estimates (not reported) broadly confirm the results from Table 2, with the difference that the coefficient on the change in the debt-to-income ratio is estimated less precisely. This suggests that housing services may not be playing a big role in explaining the differences in the estimates between the RS proxy and PCE. Although it would be interesting to investigate deeper the effect of our explanatory variables on the different components of consumption, it is beyond the scope of this paper.

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

4.2. *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 characterized 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 (for the in-sample fit, see Figure B.4 in

TABLE 3
CONTRIBUTION TO THE SLOWDOWN IN PCE GROWTH

Variable	2000–06	2007–12	Change	Contribution	%
$\Delta_4 PCE$	3.5	1.4	−2.0	−2.0	100
$\Delta_4 Wealth$	7.3	−3.5	−10.8	−1.0	48.1
$\Delta_4 Income$	3.1	1.6	−1.4	−0.4	20.0
Debt ($\Delta_4 Debt$)	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 ($\Delta_4 UR$)	4.9 (0.1)	7.1 (0.5)	2.2	−0.1	5.9
Other/unexplained				−0.2	11.0

Notes: Authors' calculations, based on fixed-effects regressions with time dummies, where the dependent variable is Δ_4*real PCE*. The table reports averages for all the U.S. states.

Appendix B, in the Online Supporting Information). Table 3 shows the results based on the average predicted values for all the U.S. states. The main findings could be summarized 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 percent 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.

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

One should be cautious, however, to avoid overinterpreting the results. In particular, one caveat is that the FE model implicitly assigns equal weights to the states. But it is possible that the full-sample estimates of the coefficients are driven by developments in a small number of states with particularly severe debt overhang and deleveraging problems—for example, the so-called "sand states"[15]—which may not be representative of the U.S. as a whole. We will return to these questions in Section 5, when we deal with the state-level heterogeneity.

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

5. 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 (in the Online Supporting Information). 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 honor 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 4 show that the main determinants of consumption—wealth and income—remain highly statistically significant across all specifications. The short-run elasticity of consumption to income falls in the range of 0.21 (LD states) and 0.35 (HD states). Interestingly, the coefficient on housing wealth roughly doubles in size for HD states as opposed to LD states. This might reflect a higher degree of optimism across households in HD states with respect to future house price and/or income dynamics before the crisis, possibly leading to larger swings in borrowing. The result is also consistent with the Mian *et al*. (2013) finding of a larger response of consumption to negative wealth shocks for households with higher leverage. The effect of uncertainty on consumption growth remains generally highly significant across the various groups. In the case of non-recourse states, the LTV ratio turns out significant and with the expected sign in column (6). This is tentative evidence that easing credit conditions might be more stimulative for consumption in non-recourse states, where households might have had stronger incentives to borrow to capitalize on the housing price boom. The somewhat larger coefficient on housing wealth for non-recourse states should also be noted.[16]

With respect to the debt variables, the coefficient on the change in the debt-to-income ratio remains significant in roughly half of the reported specifications.

[16]The average LTV ratio for NR states is 75.7 percent, almost two percentage points below the average for R states (77.5 percent). 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.

TABLE 4

FIXED EFFECTS: EXAMINING HETEROGENEITY WITH SPLIT REGRESSIONS AND INTERACTION TERMS

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Baseline	HD states	LD states	Interact LD50pctl	Interact LD90pctl	Non-recourse states	Recourse states	Interact NR states
$\Delta_4 Wealth$	0.090***	0.098***	0.054***	0.090***	0.092***	0.118***	0.079***	0.090***
	(0.011)	(0.016)	(0.014)	(0.011)	(0.010)	(0.017)	(0.014)	(0.011)
$\Delta_4 Income$	0.285***	0.347***	0.212***	0.279***	0.276***	0.299***	0.256***	0.285***
	(0.025)	(0.044)	(0.044)	(0.025)	(0.025)	(0.051)	(0.030)	(0.025)
$\Delta_4 Debt_{t-1}$	0.020*	0.019*	0.009	0.022*	0.041**	0.039	0.016	0.021*
	(0.011)	(0.011)	(0.026)	(0.011)	(0.018)	(0.037)	(0.010)	(0.012)
$Debt\_gap_{t-1}$	−0.019*	−0.007	−0.016	−0.017	−0.014	−0.011	−0.022	−0.017
	(0.011)	(0.011)	(0.019)	(0.011)	(0.013)	(0.011)	(0.015)	(0.014)
Interest	0.525	0.527	0.773	0.604	0.638	1.214	0.010	0.549
	(0.510)	(0.671)	(0.736)	(0.502)	(0.461)	(0.802)	(0.713)	(0.512)
$\Delta_4 UR$	−0.283***	−0.471***	−0.080	−0.287***	−0.277***	−0.183*	−0.307***	−0.282***
	(0.073)	(0.145)	(0.113)	(0.073)	(0.074)	(0.088)	(0.078)	(0.072)
$\Delta_4 LTV$	−0.011	0.028	−0.023	−0.008	−0.011	0.095*	−0.033	−0.011
	(0.028)	(0.046)	(0.032)	(0.027)	(0.027)	(0.052)	(0.030)	(0.028)
LD50*$\Delta_4 Debt_{t-1}$				−0.021				
				(0.020)				
LD50*$Debt\_gap_{t-1}$				0.018				
				(0.015)				
LD90*$\Delta_4 Debt_{t-1}$					−0.047**			
					(0.023)			
LD90*$Debt\_gap_{t-1}$					0.005			
					(0.015)			
NR*$\Delta_4 Debt_{t-1}$								−0.010
								(0.019)

TABLE 4 *Continued*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Baseline	HD states	LD states	Interact LD50pctl	Interact LD90pctl	Non-recourse states	Recourse states	Interact NR states
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 Δ_4*real PCE*. High-deleveraging (HD) and low-deleveraging (LD) states in columns (2) and (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 columns (4) and (5) refer to dummy variables that take the value of 1 for the states with the 50th percentile and 90th percentile of smallest declines in their household debt-to-income. Non-recourse (NR) states in columns (6)–(8) refer to states where the lender has no recourse against borrowers if the borrowers' house is sold at auction or in a short sale for less than the amount owned by the lender (Alaska, Arizona, California, Connecticut, Idaho, Minnesota, North Carolina, North Dakota, Oregon, Texas, Utah, and Washington, D.C.). NR dummy used in column (8) refers to a dummy variable that takes the value of 1 for the non-recourse states.

TABLE 5
CONTRIBUTION TO THE SLOWDOWN IN PCE GROWTH

Top 10th percentile of states by deleveraging					
Variable	2000–06	2007–12	Change	Contribution	%
$\Delta_4 PCE$	5.5	0.9	−4.6	−4.6	100
$\Delta_4 Wealth$	13.1	−8.1	−21.2	−2.0	42.5
$\Delta_4 Income$	4.1	1.1	−3.1	−0.8	18.3
Debt ($\Delta_4 Debt$)	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 ($\Delta_4 UR$)	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	%
$\Delta_4 PCE$	3.2	1.5	−1.7	−1.7	100
$\Delta_4 Wealth$	6.5	−2.9	−9.5	−0.9	52.0
$\Delta_4 Income$	2.9	1.7	−1.2	−0.3	19.8
Debt ($\Delta_4 Debt$)	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 ($\Delta_4 UR$)	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 Δ_4*real PCE*. The split is between the 10th percentile of states with the largest and the 90th percentile of states with the smallest declines in their household debt-to-income ratio from their respective peaks up to 2012Q4.

By contrast, the debt gap turns insignificant in columns (2)–(8), even though the point estimates are qualitatively similar to earlier results.[17] One clear takeaway 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 percent 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.

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 the interaction term with the NR dummy in column (8). Therefore, the results fail to confirm the hypothesis that higher penalties in the case of default result in a stronger impact from excessive indebtedness and/or deleveraging on consumption.

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

Table 5 decomposes the factors behind the slowdown in PCE growth between 2000–06 and 2007–12, as already seen in Section 4.2. This time, we split the sample between the top 10th percentile and the bottom 90th percentile of states, according to the magnitude of deleveraging that 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 4. A first glimpse at the table underscores the heterogeneity in economic performance between the two groups. The previous finding of a dominant effect from traditional factors in explaining the slowdown in consumption is confirmed by the results for both samples. Despite the much stronger slowdown in consumption growth for the HD states, the 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 percent 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.

6. Out-of-Sample Contributions to Consumption over 2013–14

With the ongoing recovery in the U.S., the deleveraging process appears to be already over at the U.S. 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 percent compared with an average of 1.4 percent in the

TABLE 6

Out-of-Sample Contribution to the Pick-Up in PCE Growth in 2013−14

Variable	2007–12	2013–14	Change	Contribution	%
$\Delta_4 PCE$	1.4	2.4	1.0	1.0	100
$\Delta_4 Wealth$	−3.5	2.4	5.9	0.5	52.3
$\Delta_4 Income$	1.6	1.5	−0.1	0.0	−2.4
Debt ($\Delta_4 Debt$)	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 ($\Delta_4 UR$)	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 Δ_4*real PCE*. The table reports averages for all the U.S. 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 U.S. aggregate.

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 6). The upturn in house prices, which led to an important increase in housing wealth, accounted for roughly half of that acceleration. In contrast, income—the other main traditional determinant of consumption—failed to pick up during this period. Finally, the significant improvement in the labour market over the past two years had a prominent role in supporting consumption growth.

7. Concluding Remarks

The leveraging and subsequent deleveraging cycle in the U.S. 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 modeling the effects of two distinct concepts of debt on U.S. 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 U.S. 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 nonlinearities, 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 U.S., where the deleveraging process appears to be already over at the U.S. 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 U.S. states, however, highlights the possibility that households in some states with unfavorable debt dynamics could still see their consumption growth being held back.

References

Albuquerque, B., U. Baumann, and G. Krustev, "US Household Deleveraging Following the Great Recession–A Model-Based Estimate of Equilibrium Debt," *The B.E. Journal of Macroeconomics*, 15 (1), 255–307, 2015.

Antzoulatos, A., "Consumer Credit and Consumption Forecasts," *International Journal of Forecasting*, 12 (4), 439–53, 1996.

Aron, J. and J. Muellbauer, "Wealth, Credit Conditions and Consumption: Evidence from South Africa," *Review of Income and Wealth*, 59, S161–96, 2013.

Bacchetta, P. and S. Gerlach, "Consumption and Credit Constraints: International Evidence," *Journal of Monetary Economics*, 40 (2), 207–38, 1997.

Ballard, C. and J. Lee, "Internet Purchases, Cross-Border Shopping, and Sales Taxes," *National Tax Journal*, 60 (4), 711–25, 2007.

Bruce, D. and W. Fox, "E-Commerce in the Context of Declining State Sales Tax Bases," *National Tax Journal*, 53 (4), 1373–90, 2000.

Case, K., J. Quigley, and R. Shiller, "Wealth Effects Revisited: 1975–2012," *Critical Finance Review*, 2 (1), 101–28, 2013.

Cooper, D., *U.S. Household Deleveraging: What Do the Aggregate and Household-Level Data Tell Us?* Federal Reserve Bank of Boston, Public Policy Brief No. 12-2, 2012.

Dynan, K., "Is a Household Debt Overhang Holding Back Consumption?" *Brookings Papers on Economic Activity, Spring*, 2012.

Dynan, K. and W. Edelberg, "The Relationship between Leverage and Household Spending Behavior: Evidence from the 2007–2009 Survey of Consumer Finances," *Federal Reserve Bank of St. Louis*, 95 (5), 425–48, 2013.

Eggertsson, G. and P. Krugman, "Debt, Deleveraging, and the Liquidity Trap: A Fisher–Minsky–Koo Approach," *The Quarterly Journal of Economics*, 127 (3), 1469–513, 2012.

Fernandez-Corugedo, E. and J. Muellbauer, *Consumer Credit Conditions in the United Kingdom*, Bank of England Working Paper 314, 2006.

Friedman, M., "The Use of Ranks to Avoid the Assumption of Normality Implicit in the Analysis of Variance," *Journal of the American Statistical Association*, 32 (200), 675–701, 1937.

Garrett, T., R. Hernández-Murillo, and M. Owyang, "Does Consumer Sentiment Predict Regional Consumption?" *Federal Reserve Bank of St. Louis Review*, 87 (2), 123–35, 2005.

Guerrieri, V. and G. Lorenzoni, *Credit Crises, Precautionary Savings, and the Liquidity Trap*, NBER Working Paper No. 17583, 2011.

Hall, R., "The Long Slump," *American Economic Review*, 101 (2), 431–69, 2011.

De Hoyos, R. E. and V. Sarafidis, "Testing for Cross-Sectional Dependence in Panel-Data Models," *Stata Journal*, 6 (4), 482–96, 2006.

Jappelli, T. and M. Pagano, "Consumption and Capital Market Imperfections: An International Comparison," *American Economic Review*, 79 (5), 1088–105, 1989.

Ludvigson, S., "Consumption and Credit: A Model of Time-Varying Liquidity Constraints," *The Review of Economics and Statistics*, 81(3), 434–47, 1999.

Maki, D., "The Growth of Consumer Credit and the Household Debt Service Burden," in T. Durkin and M. Staten (eds), *The Impact of Public Policy on Consumer Credit*, Springer Science+Business Media, New York, 43–68, 2002.

McCarthy, J., "Debt, Delinquencies, and Consumer Spending," *Current Issues in Economics and Finance*, 3 (3), 1–6, 1997.

Mian, A. and A. Sufi, "Household Leverage and the Recession of 2007–09," *IMF Economic Review*, 58 (1), 74–117, 2010.

Mian, A. and A. Sufi, K. Rao, and A. Sufi, "Household Balance Sheets, Consumption, and the Economic Slump," *The Quarterly Journal of Economics*, 128 (4), 1687–726, 2013.

Mishkin, F., "Illiquidity, Consumer Durable Expenditure, and Monetary Policy," *American Economic Review*, 66 (4), 642–54, 1976.

Mundlak, Y., "On the Pooling of Time Series and Cross Section Data," *Econometrica*, 46 (1), 69–85, 1978.

Ogawa, K. and J, Wan, "Household Debt and Consumption: A Quantitative Analysis Based on Household Micro Data for Japan," *Journal of Housing Economics*, 16 (2), 127–42, 2007.

Olney, M., "Avoiding Default: The Role of Credit in the Consumption Collapse of 1930," *The Quarterly Journal of Economics*, 114 (1), 319–35, 1999.

Pesaran, M., "Estimation and Inference in Large Heterogeneous Panels with a Multifactor Error Structure," *Econometrica*, 74 (4), 967–1012, 2006.

Pesaran, M., Y. Shin, and R. Smith, "Pooled Mean Group Estimation of Dynamic Heterogeneous Panels," *Journal of the American Statistical Association*, 94 (446), 621–34, 1999.

Phillips, P. and D. Sul, "Dynamic Panel Estimation and Homogeneity Testing under Cross Section Dependence," *Econometrics Journal*, 6 (1), 217–59, 2003.
Wooldridge, J., *Econometric Analysis of Cross-Section and Panel Data*, The MIT Press, Cambridge, MA, 2002.
Zhou, X. and C. Carroll, "Dynamics of Wealth and Consumption: New and Improved Measures for U.S. States," *The B.E. Journal of Macroeconomics*, 12 (2), 1–44, 2012.

Supporting Information

Additional Supporting Information may be found in the online version of this article at the publishers website:

Appendix:

A Data sources and descriptive statistics

Table A.1: Descriptive statistics
Table A.2: Panel unit-root tests (p-values)

B Additional tables and figures

Figure B.1: Household debt-to-income ratio and private consumption over current and past business cycles
Figure B.2: US official retail sales and aggregated RS proxy (% year-on-year, nominal)
Figure B.3: US official PCE and state-aggregated, interpolated PCE (% year-on-year, nominal)
Figure B.4: In-sample fit of US PCE growth from FE estimation (% year-on-year, real)
Figure B.5: Average developments in economic indicators for high versus low deleveraging states
Table B.1: Fixed effects: Retail sales proxy