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Private debt overhang and the government spending multiplier: Evidence for the United States

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Summary

Using state-dependent local projections and historical US data, we find that government spending multipliers are considerably larger in periods of private debt overhang. In particular, while multipliers are below or close to one in low private debt states, we find significant crowding-in of private spending in periods of debt overhang, resulting in multipliers that are much larger than one. In high private debt episodes, more government purchases even reduce the ratio of government debt to gross domestic product. These results are robust for the type of shocks, and when we control for the business cycle, financial crises, deleveraging episodes, government debt overhang, and the zero-lower-bound.

1 | INTRODUCTION

The Great Recession and the European sovereign debt crisis have reignited the academic and political debate on the role of fiscal stimulus packages for business cycle stabilization, as well as the macroeconomic consequences of austerity policies. Whereas the debate previously focused mainly on the average size of the so-called government spending multiplier—that is, the dollar change in output caused by an exogenous dollar increase or decrease in government purchases—the current debate centers more on the question of whether government spending multipliers differ according to the state of the economy. In particular, government spending multipliers are not structural constants, and may depend on a number of conditions that vary across countries and time (Hall, 2009).

From a theoretical perspective, multipliers depend, for instance, on monetary policy and the amount of slack in the economy. Eggertsson (2011), Woodford (2011), and Christiano, Eichenbaum, and Rebelo (2011) show that a deficit-financed increase in government purchases has a much stronger impact on economic activity when the nominal interest rate hits the zero lower bound. Michaillat (2014) demonstrates that the effect of government policies may be stronger in recessions, even when the zero lower bound does not bind. The empirical support for both hypotheses is, however, mixed. For example, Auerbach and Gorodnichenko (2012, 2013a) find innovations to government purchases to be much more effective in recessions than expansions, but this finding cannot be confirmed by Owyang, Ramey, and Zubairy (2013) or Ramey and Zubairy (2018) for military spending news shocks over a longer sample period. The latter study also finds no robust evidence that multipliers are greater when interest rates are near the zero lower bound.

In this paper, we approach the state-dependence of government spending multipliers in another way; that is, we examine whether aggregate spending multipliers in the USA have historically been greater in periods of private debt overhang. It is surprising that the role of private debt has so far been ignored in the empirical literature on state-dependent fiscal multipliers, despite a rising number of studies highlighting its relevance in affecting the macroeconomic propagation of shocks. On the theoretical side, recent contributions by Eggertsson and Krugman (2012), Kaplan and Violante (2014), and Andrés, Boscá, and Ferri (2015) have shown that debtors, differently from lenders, are more sensitive to exogenous income changes. On the empirical side, instead, several studies have shown that private indebtedness amplifies business cycles. Specifically, it is widely believed that a rapid increase in household debt between 2000 and 2008 set the stage for the Great Recession, and that tightening household liquidity constraints have been essential for understanding the macroeconomic consequences of the crisis (e.g., Hall, 2011). For example, Mian and Sufi (2010) find that differences in the debt overhang of households before the crisis can explain the postcrisis recovery at the county level within the USA, while Jordà, Schularick, and Taylor (2013) show that more credit-intensive booms tend to be followed by deeper recessions and slower recoveries for a panel of 14 advanced countries between 1870 and 2008. In a recent study, Cloyne and Surico (2017) find for the UK that households with mortgage debt increase their consumption after a decline in income taxes, while outright homeowners hardly adjust their expenditures. It is an open question whether private debt also matters for the aggregate effects of fiscal policy in the USA, in particular the evolution of government spending multipliers over time. This paper is the first study trying to fill this gap in the literature.

In the spirit of Auerbach and Gorodnichenko (2013a), Owyang et al. (2013) and Ramey and Zubairy (2018), we estimate state-dependent government purchases multipliers for the USA using Jordà's (2005) local projection method, and allow the state of the economy to vary according to the presence of debt overhang in the private sector. High and low private debt states are identified as periods when ratios of private debt to gross domestic product (GDP) were respectively above and below trend. We use historical US data from Gordon and Krenn (2010) and Carter et al. (2006) to have sufficient episodes of substantial variation in government spending and private debt states; that is, the sample period is 1919:Q1–2013:Q4. For example, the fluctuations in government purchases and private debt during the Great Depression and World War II were huge, providing a rich source of information to analyze the role of debt overhang as a driver of the spending multiplier. We examine the effects of two very different types of government spending shocks that have been proposed in the literature. In particular, we consider innovations to government purchases in the spirit of Blanchard and Perotti (2002) and Ramey's (2011) narratively identified defense news shocks.

We find that government spending multipliers are considerably larger in periods of private debt overhang. The results reveal a crowding-out effect on real personal consumption and investment in low private debt states, resulting in cumulative multipliers that are below one, that is, ranging between 0.8 and 0.9 in the medium to long run. Conversely, in high private debt states, both consumption and investment increase in response to an expansionary government spending shock, whereas multipliers turn out to be much larger than one. In the long run (after 3 years), we find cumulative multipliers around 1.5 in periods of private debt overhang. Furthermore, in the identified episodes of ample private debt over the past century, more (less) government purchases have on average even reduced (increased) the government debt-to-GDP ratio as a result of the strong effects on real GDP.

The results are robust for alternative specifications of the model, definition of debt overhang, use of historical data, and the type of government spending shocks. Moreover, the results prove to be robust when we control for alternative state variables that could influence the multiplier. Specifically, we still find much higher multipliers when we control for periods of banking crises, stock market crashes, deleveraging, recessions (slack), the presence of the zero lower bound on the interest rate, as well as government debt overhang.

These stylized facts have some important (policy) implications. First, private indebtedness seems to be an important indicator for the repercussions of fiscal consolidations and stimulus packages. In low private debt periods, increases in government purchases may not be effective in stimulating private sector activity, while the consequences of fiscal consolidations are probably not very harmful. In contrast, at times of debt overhang in the private sector, deficit-financed government spending is probably able to support the economy. In other words, as argued by Eggertsson and Krugman (2012), more public debt can be a solution to a problem caused by too much private debt. A fiscal expansion could sustain output and employment while private balance sheets are repaired, and the government can successfully pay down its own debt after the high private debt period has come to an end. Second, given the excessive private debt levels at the onset of the Great Recession, our findings align with the postcrisis perception that government spending multipliers were much larger than in normal times. Finally, theoretical macroeconomic models that analyze fiscal policy issues should take into account debt overhang in the private sector to properly capture the interaction with the real economy. More generally, private debt seems to matter for macroeconomic fluctuations.

The rest of the paper is organized as follows. In the next section, we describe the state-dependent local projection methodology. The measurement of private debt states and different types of government spending shocks that we consider in the empirical analysis are discussed in Section 3. The baseline estimation results are reported in Section 4, the impact on some other relevant variables in Section 5, while Section 6 addresses some extensions of the model to control for the influence of a set of financial, business cycle, and policy states. Section 7 concludes.

2 | METHODOLOGY

To investigate government spending multipliers depending on the state of the economy, we follow Auerbach and Gorodnichenko (2013a), Owyang et al. (2013), and Ramey and Zubairy (2018), and estimate state-dependent impulse responses of exogenous innovations in government purchases using Jordà's (2005) local projections. This method has become very popular to estimate fiscal multipliers. The advantages compared to vector autoregressions (VARs) are that it is more robust to misspecification because it does not impose implicit dynamic restrictions on the shape of the impulse responses, while also a more parsimonious specification can be used since not all variables are required to be included in all equations. Moreover, it can easily accommodate state-dependence and avoids a potential bias when elasticities are converted to multipliers.[1]

To examine the evolution of government spending and GDP after the shocks, and assess whether there are crowding-in or crowding-out effects of the fiscal impulse on personal consumption and investment, we first estimate the following linear regression model:

$$\begin{aligned} z_{t+h} &= I_{t-1}\left[\alpha^z_{A,h} + \psi^z_{A,h}(L)\, ctr_{t-1} + \beta^z_{A,h} shock_t\right] \\ &\quad + (1 - I_{t-1})\left[\alpha^z_{B,h} + \psi^z_{B,h}(L)\, ctr_{t-1} + \beta^z_{B,h} shock_t\right] + \epsilon_{t+h}, \end{aligned} \tag{1}$$

for $h = 0, \ldots, H$, where z_{t+h} is the variable of interest at horizon $t + h$, L represents the lag operator, I_{t-1} is a predetermined dummy variable that indicates the state $\{A, B\}$ of the economy in the period just before the government spending shock $shock_t$, and ctr_{t-1} is a vector of predetermined control variables.[2] The collection of the $\beta^z_{A,h}$ and $\beta^z_{B,h}$ coefficients in Equation 1 measure the state-dependent responses of variable z at time $t + h$ to the shock at time t.[3]

For the definition of the government spending shocks (*shock*) and the state of the economy (I), we refer to the next section. The variables z that we consider in the baseline estimations are real per capita GDP, personal consumption expenditures, and fixed investment.[4] From 1947:Q1 onwards, we use the National Income and Product Accounts (NIPA) tables from the Bureau of Economic Analysis. Before this period (i.e., 1919:Q1–1946:Q4) we use the Gordon and Krenn (2010) historical quarterly dataset. Following Hall (2009) and Barro and Redlick (2011), we convert each variable prior to the estimations as follows:

$$z_{t+h} = \frac{Z_{t+h} - Z_{t-1}}{Y_{t-1}}, \tag{2}$$

where Z_t is respectively GDP or one of its components, while the responses are scaled by lagged GDP, that is, Y_{t-1}. All coefficients are hence in the same unit, which is needed for the construction of the multipliers. The control variables *ctr* are four lags of GDP, government purchases, personal consumption, investment, nominal interest rate, GDP deflator, average marginal tax rate, and private and public debt-to-GDP ratios.[5]

In addition to the impulse responses of Equation 1, the analysis in this paper will mainly be based on so-called *cumulative government spending multipliers*. These indicators relate the cumulative change in a variable of interest (e.g., GDP) to the cumulative change in the amount of government purchases, both induced over time by an initial government spending impulse. In contrast to peak and impact multipliers, for example, which can directly be measured from the above impulse responses, cumulative multipliers have the advantage that they can be used to assess the short-run as well as the medium-run macroeconomic effects of a fiscal shock. In addition, they allow for a proper comparison of the effects of government spending shocks across states and shock definitions. Following Ramey and Zubairy (2018), we compute

[1]Note that this method also has some disadvantages to calculate impulse responses, in particular a more erratic pattern at longer horizons because of a loss of efficiency. For a discussion, we refer to Ramey and Zubairy (2018).

[2]A one-period-lagged state indicator I_{t-1} is used to avoid a simultaneity problem between the effects of the shock and the state of the economy at the moment of the shock.

[3]To allow for a comparison of the estimates across horizons h, we hold the sample constant by using the longest horizon H. Standard errors are adjusted for the presence of residual heteroskedasticity and autocorrelation by using the STATA command *ivreg2* followed by the options *robust* and *bw(auto).*

[4]Similar to Perotti (2008), we consider personal consumption expenditures related to nondurable goods and services and fixed investments. Durable goods and changes in private inventories are excluded. For more details on the construction of all data used in this paper, we refer to the data Appendix.

[5]In line with Barro and Redlick (2011), the control variables are transformed in first differences, and those expressed in dollars also scaled by lagged GDP. The conclusions reported in this paper are, however, not affected when we use dependent and control variables in levels. Following Ramey and Zubairy (2018), we also include lagged values of the defense news shocks to control for serial correlation. As will become clear in the next section, since the set of control variables already contains lagged government spending, this is not necessary for the Blanchard–Perotti shocks.

state-dependent cumulative multipliers by estimating a local projection instrumental variable (LP-IV) model for each variable z and each horizon h:

$$\sum_{l=0}^{h} z_{it+l} = I_{t-1}\left[\alpha_{A,h}^{mz} + \psi_{A,h}^{mz}(L)\,ctr_{t-1} + \beta_{A,h}^{mz}\sum_{l=0}^{h} g_{it+l}\right] + (1-I_{t-1})\left[\alpha_{B,h}^{mz} + \psi_{B,h}^{mz}(L)\,ctr_{t-1} + \beta_{B,h}^{mz}\sum_{l=0}^{h} g_{it+l}\right] + \epsilon_{t+h}, \tag{3}$$

where the notations are essentially the same as in Equation 1. However, $\sum_{l=0}^{h} z_{it+l}$ and $\sum_{l=0}^{h} g_{it+l}$ are now respectively the cumulative paths of a variable of interest and government purchases between times t and $t+h$, while the two endogenous variables $(1-I_{t-1})\sum_{l=0}^{h} g_{it+l}$ and $I_{t-1}\sum_{l=0}^{h} g_{it+l}$ are instrumented by $I_{t-1}shock_t$ and $(1-I_{t-1})\,shock_t$, where $shock_t$ is the government spending shock hitting at time t. Accordingly, the $\beta_{A,h}^{mz}$ and $\beta_{B,h}^{mz}$ coefficients provide directly the state-dependent cumulative multipliers of variable z at horizon h in the two states of the economy.[6]

3 | PRIVATE DEBT STATES AND GOVERNMENT SPENDING SHOCKS

To analyze whether government spending multipliers depend on the presence of private debt overhang in the economy, we need to identify high and low private debt states. Furthermore, for the estimation of fiscal multipliers, it is crucial to identify exogenous innovations to government purchases. In this section, we describe how we disentangle both states of the economy and how we derive autonomous shifts in government purchases.

3.1 | Private debt states

3.1.1 | Baseline identification

The identification of episodes of private debt overhang is not trivial. It essentially requires two choices: the selection of an indicator that is available for the whole sample period, and a threshold criterion to disentangle high and low private debt periods. For the baseline estimations, we use the domestic nonfinancial private debt-to-GDP ratio as the debt indicator; that is, domestic debt net of government and financial sector debt divided by national income. A similar indicator is used by Schularick and Taylor (2012) to identify (bank) credit booms. The advantage of using a debt-to-GDP ratio is that we control for inflation, population, and economic activity. The quarterly series is constructed by splicing the most recent Fed flow of funds data to the historical records provided in Carter et al. (2006).[7] To disentangle high and low private debt states, we define the former as the periods when there was a positive deviation of the debt-to-GDP ratio from a very smooth Hodrick–Prescott trend (i.e., $\lambda = 10^6$) for at least two consecutive quarters.

The debt-to-GDP ratio, the smooth trend, and the resulting dummy (state) variable are shown in Figure 1. This procedure identifies four periods of private debt overhang: 1927:Q3–1940:Q3, 1957:Q2–1975:Q3, 1985:Q3–1992:Q1, and 2001:Q3–2010:Q3. Overall, the US economy has been about half of the time in each state, which is convenient for an accurate estimation of the parameters. The identified states are consistent with historical evidence. The first and the last episodes capture the high private debt burdens that have characterized the run-up and the aftermath of, respectively, the Great Depression and the Great Recession. In particular, the analysis in Persons (1930), Mishkin (1978), and Mian and Sufi (2014), among others, suggests that the increasing weakness of borrowers' balance sheets has contributed to the cause and the severity of both crises. Furthermore, the second episode is consistent with Chen, Kim, Otte, Wiseman, and Zdzienicka (2015) and Mishkin (1977), who identify an era of household debt deleveraging starting in the mid-1960s which, in turn, signals a preexisting condition of high private indebtedness. Finally, the third episode can be linked to the boom and bust phases of bank lending around the so-called Savings & Loan Crisis; that is, a large-scale failure of financial institutions specialized in the collection of deposits in order to provide loans to households and firms.

[6]When each regressor in the model is modeled as state-dependent, the states of the economy are mutually exclusive, and the sample is fixed across horizons, there is an analytical relation between the impulse responses estimated in Equation 1 and the cumulative multiplier estimated in Equation 3. In particular, $\beta_{A,h}^{mz} = \sum_{l=0}^{h}\beta_{A,l}^{z} / \sum_{l=0}^{h}\beta_{A,l}^{g}$ and $\beta_{B,h}^{mz} = \sum_{l=0}^{h}\beta_{B,l}^{z} / \sum_{l=0}^{h}\beta_{B,l}^{g}$.

[7]In the Appendix, we provide the data transformations and the official sources.

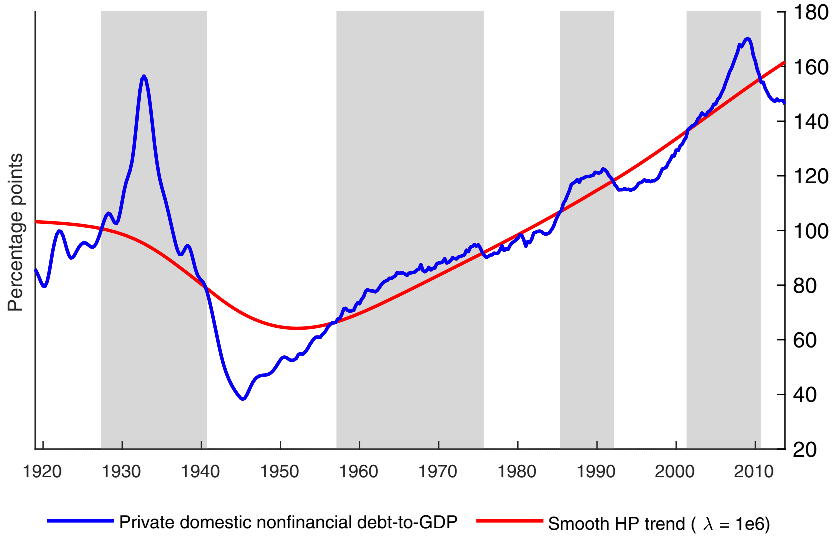

FIGURE 1 Baseline periods of private debt overhang. Gray bars are the identified periods of high private debt [Colour figure can be viewed at wileyonlinelibrary.com]

3.1.2 | Why detrending the debt-to-GDP ratio?

Standard theoretical models usually assume constant equilibrium debt-to-GDP levels over time (see, e.g., Eggertsson & Krugman, 2012). Why do we then detrend the debt-to-GDP ratio rather than defining high and low private debt states based on the level of the private debt-to-GDP ratio itself? The reason is that those types of models typically abstract from an important empirical stylized fact: the presence of financial progress. Specifically, financial progress or financial deepening can be seen as a set of slow-moving factors that allow the debt-to-GDP ratio to increase over time along a long-run trend without necessarily raising the cost of servicing it, thus potentially breaking the direct link between debt and its burden. Examples are securitization, which promotes risk sharing, the creation of new financial instruments, which improves diversification, or the adoption of innovative technologies, which increase efficiency. Such innovations tend to affect the average interest rate or the remaining maturity on outstanding debt—two key factors that determine the effective burden of debt. Intuitively, for a given debt-to-GDP level, a decrease in the average interest rate on loans or an increase in the average remaining maturity tends to alleviate the debt burden. Note that such factors are related to the entire stock of outstanding debt and thus evolve very smoothly over time. For example, a change in the interest rate on new loans has only a marginal impact on the average interest payments on the outstanding stock of debt.

The presence of financial progress is a robust empirical finding, which, especially after World War II, has characterized the long-run trending behavior of the credit-to-GDP ratios in advanced countries (Schularick & Taylor, 2012). This phenomenon has thus induced most of the empirical literature to focus on deviations of private debt-to-GDP ratios from very smooth trends (see, among others, Dell'Ariccia, Igan, Laeven, & Tong, 2016; Gourinchas & Obstfeld, 2012). This approach has also been widely accepted by policymakers, who regularly use a similar credit gap indicator for macroprudential policy (see Basel Committee on Banking Supervision, 2010). While the available methods differ slightly in the computation of the smooth trend, they all recognize the importance of taking care of it.[8] In other words, higher-than-trend private indebtedness is a better proxy for periods of relatively high debt levels, rather than absolute debt levels, as an elementary interpretation of the theory would suggest.[9]

3.1.3 | Comparison with alternative measures

The debt-to-GDP ratio is a key proxy for the debt service ratio, which is the ultimate determinant of debt affordability (Buttiglione, Lane, Reichlin, & Reinhart, 2014). Hence a reasonable alternative would involve using direct measures of debt burden. Unfortunately, such indicators are only available over a much shorter sample period. However, the identified

[8] Dell'Ariccia et al. (2016) show that different methodologies tend to identify the same list of episodes.

[9] An intuitive analogy is provided by an alternative form of innovation, the more popular "technological" progress. Smets and Wouters (2007) show that the inclusion of a deterministic steady-state output growth rate in a DSGE setup is a simple way to reconcile the data with the model, which otherwise would predict a constant, unrealistic, output steady-state level.

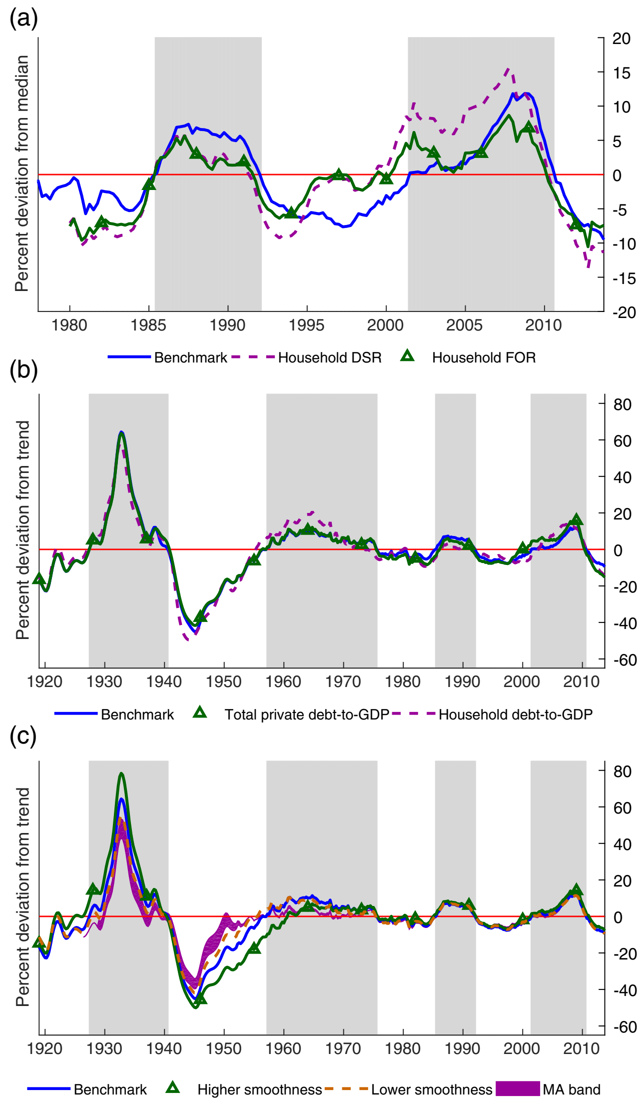

FIGURE 2 Alternative measures of private debt states: (a) benchmark versus debt service ratios; (b) benchmark versus alternative variables; (c) benchmark versus alternative trends. Gray bars are the identified periods of high private debt. Panel (a) compares the benchmark deviation with measures of household debt service and financial obligations ratios from the Federal Reserve Board (percent deviation from median). The other panels compare the benchmark percent deviation from trend with alternative measures obtained using different reference variables (b) and trends (c) [Colour figure can be viewed at wileyonlinelibrary.com]

periods of private debt overhang correspond very well with periods of high debt service. More specifically, Figure 2(a) shows the deviation from trend of the baseline domestic nonfinancial private debt-to-GDP ratio, as well as two direct measures of household debt burden provided by the Federal Reserve Board, which are available from 1980 onwards. The latter are shown in percentage point deviations from their (constant) median. The debt service ratio (DSR) takes into account mortgage and consumer debt, whereas the financial obligations ratio (FOR) is a more comprehensive measure that also includes additional forms of debt payment.[10] In essence, these measures are debt-to-income ratios, adjusted for the average interest rate and remaining maturity on outstanding debt. These considerations signify once again that "debt overhang" is a relative concept, because it tends to vary over time at low frequency, which requires detrending using very smooth filters. It is thus not surprising that direct measures of debt burden unveil very similar episodes of private debt overhang as our baseline (detrended) debt measure.

[10]These are "rent payments on tenant-occupied property, auto lease payments, homeowners' insurance, and property tax payments." See the website of the Federal Reserve Board for additional information.

The identification of episodes of private debt overhang is also very similar when we use alternative private debt measures. Specifically, Figure 2(b) shows the deviations from trend of the baseline domestic nonfinancial private debt measure, total private debt-to-GDP (including financial sector debt) and household debt-to-GDP ratio. The latter, for instance, is used by Krugman (2013) as a proxy for the debt burden of US households. The same Hodrick–Prescott (HP) filter ($\lambda = 10^6$) has been applied to all three debt measures. As can be seen in the figure, the deviation of total private debt-to-GDP from trend is almost indistinguishable from the baseline measure. Also, the deviation of household debt-to-GDP from trend has been very similar over time. A notable difference was a somewhat larger positive deviation from trend during the mid-1950s and the 1960s, which confirms that this was indeed an episode of high private debt burden.

To identify periods of private debt overhang, we have used the two-sided HP filter to detrend the debt-to-GDP ratio. In contrast to one-sided HP filters, a two-sided filter uses the full amount of information in the sample, which should more accurately extract the trend of a series. Since we are using a very high smoothing parameter in the HP filter ($\lambda = 10^6$), changes in the trend are very small relative to quarterly variation in real GDP or government spending. Accordingly, possible endogeneity issues (i.e., fiscal policy affecting GDP, which, in turn, affects the estimated trend of the debt-to-GDP ratio) should not be quantitatively important in a setting where the debt states are identified based on timing.[11] To check the robustness of the identified debt states with respect to detrending, Figure 2(c) shows the evolution of detrended private debt over time when we apply HP trends characterized by higher and lower smoothness, that is, respectively a λ parameter equal to $10^7$and $4 \cdot 10^5$. The figure reveals that both alternative measures differ in the magnitude of the deviation from trend in the first part of the sample period, while the magnitudes tend to be very similar from the 1970s onwards. However, despite the differences in the early part of the sample, the identified periods of private debt overhang are remarkably consistent throughout the sample.

Hamilton (2017) criticizes the use of the HP filter for detrending.[12] As an alternative way to gauge the trend, we therefore also consider a band of simple long-term moving averages. The band of moving averages is constructed using windows ranging between 15 and 20 years, in order to capture the low frequency that characterizes financial cycles (Borio, 2014). As can be seen in Figure 2(c), we again obtain differences in the magnitude of the deviations from trend, but very similar periods of relatively high and low private debt.

In sum, since we use a discrete (dummy) indicator to identify private debt states, all debt measures or detrending methods more or less identify the same periods as high- and low-debt states, which makes us confident that we are capturing a general evolution that does not strongly depend on the selection of the debt indicator. Notwithstanding, in Section 4 , we also show the estimation results based on the alternative debt measures reported in Figure 2(b, c). Finally, note that we do not distinguish between periods of rising (detrended) private debt and deleveraging in the baseline estimations. In the following sections, however, we will also analyze in more detail whether there are differences between episodes of rising and falling debt-to-GDP ratios.

3.2 | Government spending shocks

There is also not a unique way to identify exogenous changes in government purchases. Numerous studies have been conducted to isolate such components in government spending, and none of them is immune to identification problems. In this paper, we do not take a stance on the best way to identify shocks to government purchases, and therefore consider the two most popular approaches that have been used in the literature.

3.2.1 | Blanchard–Perotti shocks

Since the seminal paper of Blanchard and Perotti (2002), several studies have used VAR models to identify government spending shocks. The key identifying assumption that is usually made in this literature is that it typically takes longer

[11]See also Auerbach and Gorodnichenko (2013b, footnote 2) for a similar reasoning in the case of spending multipliers in recessions versus expansions. Note also that the theories that we are testing do not rely on the expectations that agents form on the debt cycle, but on the debt cycle itself. It is hence better to use a filter based on past, present, and future observations to identify the debt cycle, rather than a (one-sided) filter that only relies on past observations. In fact, when we use a one-sided HP filter to derive the trend, a major part of the 1930s turns out to be a low-debt period—that is, from 1933 onwards—which is at odds with common wisdom.The reason is that the one-sided HP filter identifies the deleveraging during the Great Depression as a low-debt state in real time, while debt levels were historically (ex post) relatively high. See also Edge and Meisenzahl (2011) and Alessandri, Bologna, Fiori, and Sette (2015) for evidence on the unreliability of credit-to-GDP ratios to measure debt overhang based on information in real time.

[12]Note that his critiques mainly apply to the estimation of higher-frequency business cycles (e.g., the output gap). In the case of lower-frequency cycles, the predictive power of the alternative method that he proposes is very small at longer horizons.

than a quarter for government purchases to respond to changes in the economy, due to the presence of decision lags and the absence of automatic stabilizers affecting government purchases. In other words, government purchases follow a backward-looking policy rule of the type

$$g_t = \psi(L)\, ctr_{t-1} + shock_t, \tag{4}$$

where government purchases of goods and services depend on a set of lagged variables ctr_{t-1} and an orthogonal $shock_t$ capturing autonomous shifts in government spending.

It is convenient to implement the identification assumption of Blanchard and Perotti (2002) in a local projection framework. In particular, when the set of control variables ctr_t in the local projection model (Equation 3) contains the (lagged) variables of the policy rule (Equation 4), the Blanchard and Perotti (2002) identification assumption can be implemented by simply setting $shock_t$ (henceforth BP shocks) equal to g_t.[13]

3.2.2 | Ramey's defense news shocks

A drawback of the BP shocks is that there is evidence that these shocks are predictable, and hence not fully unanticipated.[14] For this reason, we also consider an alternative measure of exogenous innovations to government purchases. More precisely, we use Ramey's narrative defense news variable (henceforth DN shocks) reflecting changes in the expected present value of government spending that are linked to political and military events, a series which has been updated and extended by Owyang et al. (2013) and Ramey and Zubairy (2018). The variable has been constructed using *Business Week* and several other newspaper sources. These changes are likely to be independent of the state of the economy, and can be considered as exogenous shocks to government spending. Differently from the BP shocks, this measure tackles the fiscal foresight problem by directly focusing on news instead of observable changes in government spending.

A first drawback of the DN shocks is that it cannot be excluded that the political and military events could have had an impact on the economy beyond the changes in government purchases, and that other fiscal shocks might have occurred at the same time, distorting the estimation results. Second, Perotti (2013) shows that the estimated average multipliers based on the DN shocks are sensitive to the presence of some extreme military events in the sample period, and that multipliers differ between defense and nondefense government spending on goods and services. Finally, the limited amount of news in the sample (around 25% of the observations t) could distort the estimation of state-dependent effects. It is thus useful to consider both alternative measures of government spending shocks to assess the robustness of the results before drawing conclusions.

4 | ARE GOVERNMENT SPENDING MULTIPLIERS GREATER IN PERIODS OF PRIVATE DEBT OVERHANG?

4.1 | Baseline results

The baseline results are presented in Figures 3 and 4 and Table 1. The panels of Figure 3 show the estimated responses of real government spending, GDP, personal consumption and investment in both states for the first 12 quarters after respectively the BP and DN shocks, which are the $\beta^z_{A,h}$ and $\beta^z_{B,h}$ coefficients of Equation 1, together with 90% confidence bands that are based on heteroskedasticity and autocorrelation (HAC) consistent standard errors. Figure 4 shows the state-dependent cumulative spending multipliers of GDP, consumption and investment obtained from the local projection instrumental variable model as specified in Equation 3. At the bottom of each panel in Figure 4, we show the estimated difference in the size of the multiplier between high and low private debt states.

The cumulative output multipliers on impact and in the fourth, eighth, and twelfth quarters are also reported in Table 1, as well as the estimated F-statistics across states, which assess whether the shock is a good instrument for government spending in each state.[15] Due to a delayed actual rise in government spending, the F-statistics for the DN shocks are very low in the short run and only become apparent at longer horizons. Accordingly, as also argued by Ramey and Zubairy (2018), DN multipliers are not informative and cannot be interpreted at short horizons. The F-statistics also indicate a

[13]Note that the use of a state-dependent policy rule in Equation 4 delivers the same identity since the baseline model specifies mutually exclusive private debt states.

[14]Ramey (2011) finds that professional forecasts and her narrative measure Granger-cause the VAR shocks. However, Mertens and Ravn (2010) and Perotti (2014) show that the predictability of the VAR innovations does not significantly affect the results.

[15]The F-statistics in Table 1 test the null hypothesis that the TSLS bias exceeds 10% of the OLS bias. In the presence of heteroskedasticity and autocorrelation, the threshold is 19.7 for the 10% critical value and 23.1 for the 5% critical value (Montiel Olea & Pflueger, 2013).

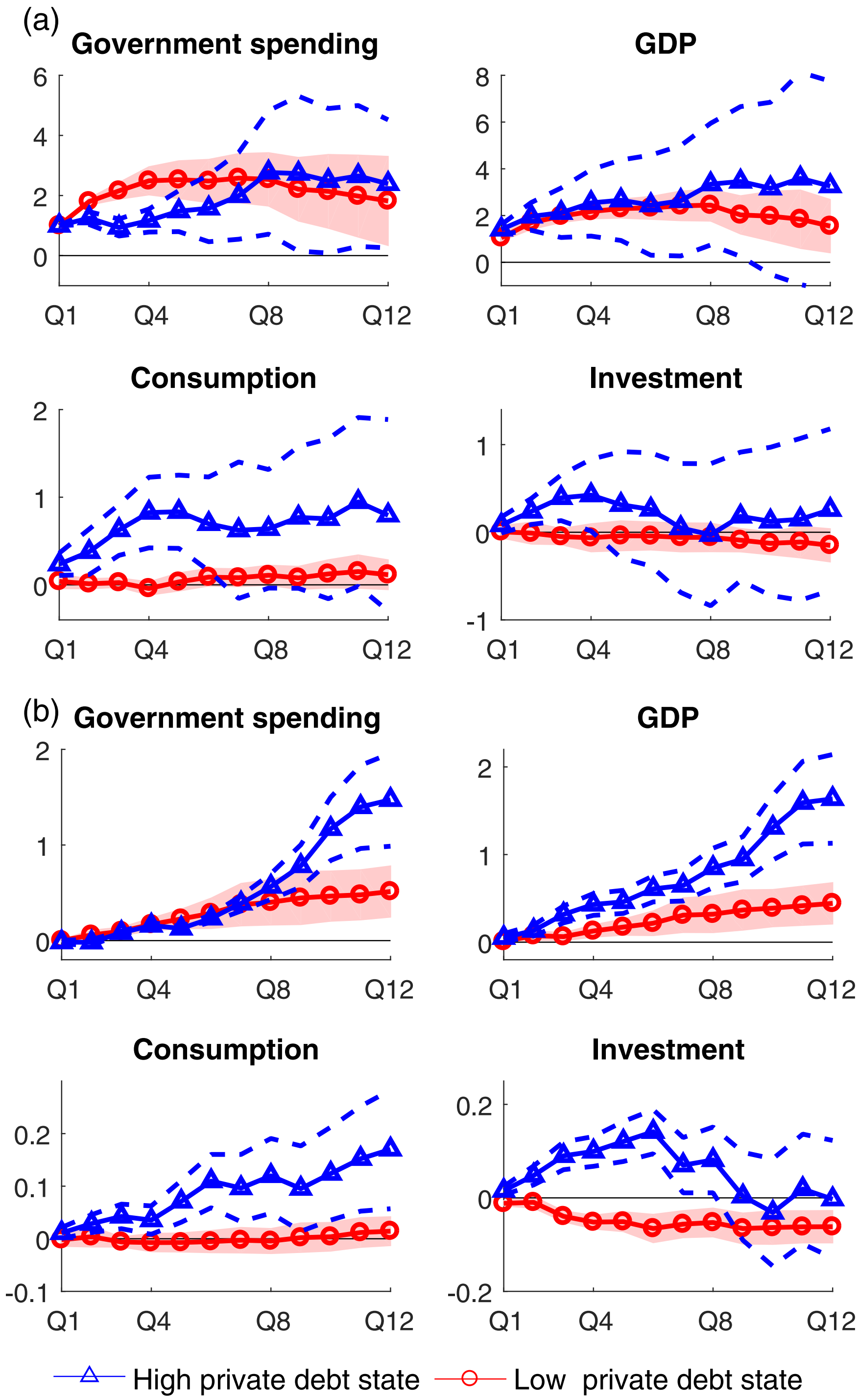

FIGURE 3 Effects of spending shocks in high and low private debt states: (a) BP shocks; (b) DN shocks. Panels show the effects of Blanchard–Perotti shocks (BP) and Ramey's Defense News (DN) on government spending, GDP, consumption, and investment in high and low private debt states. The horizontal axes measure the analyzed horizon (expressed in quarters) and the vertical axes measure the dollar change. The bands show the 90% confidence interval, based on heteroskedasticity and autocorrelation (HAC) consistent standard errors [Colour figure can be viewed at wileyonlinelibrary.com]

possible problem with instrument relevance at longer horizons in the low-debt state for DN shocks, and for the BP shocks at longer horizons in the high-debt state. In the table, we therefore also report Anderson–Rubin (AR) *p*-values for the difference test between both states, which are robust to weak instruments.[16]

[16]We constructed the AR test conditional on the assumption that there was no instrument relevance problem for the linear term in government spending, and then tested the state-dependent term. See Ramey and Zubairy (2018) for a similar approach.

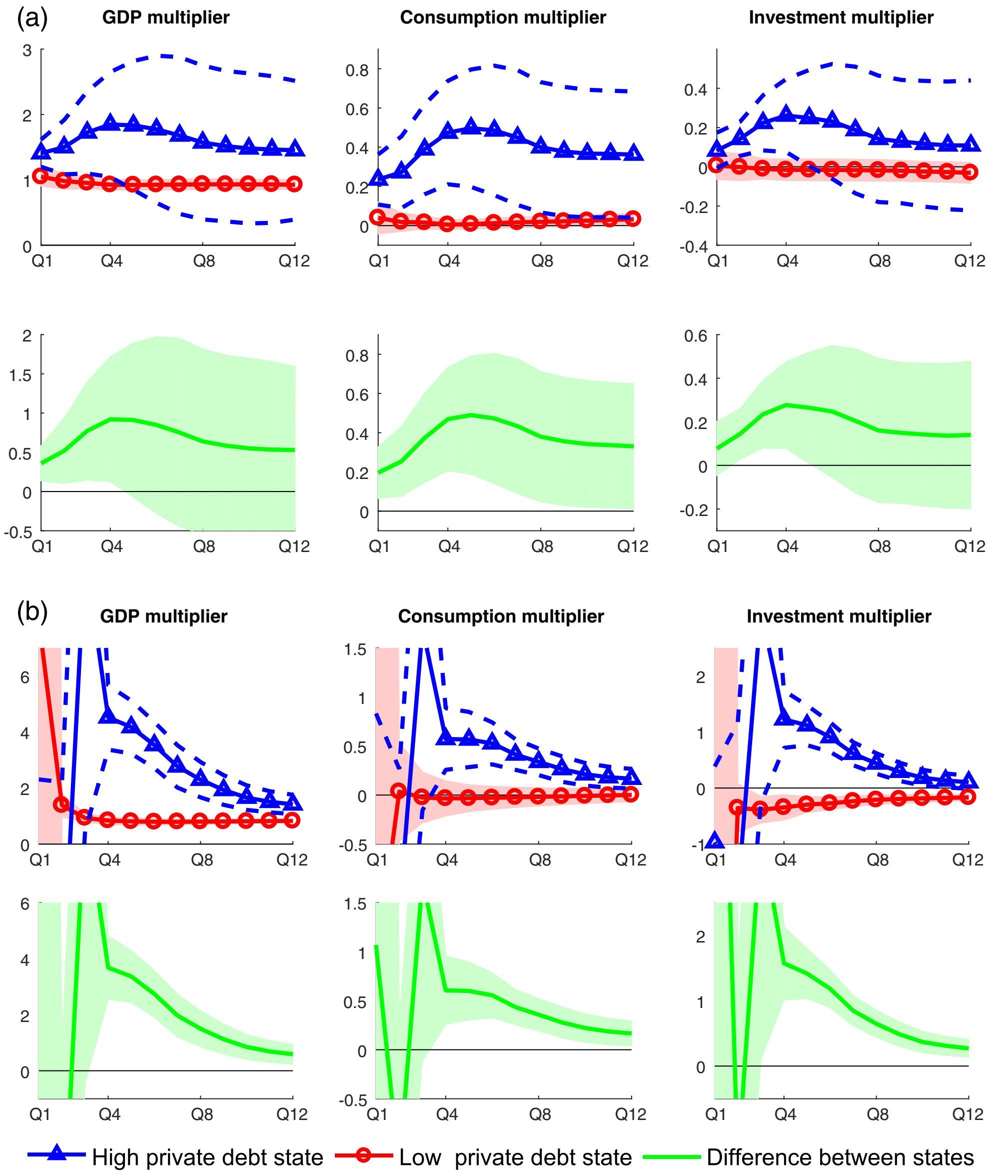

FIGURE 4 Cumulative multipliers in high and low private debt states: (a) BP shocks; (b) DN shocks. Panels show the cumulative multipliers for Blanchard–Perotti shocks (BP) and Ramey's Defense News (DN) in high and low private debt states. The horizontal axes measure the analyzed horizon (expressed in quarters). The bands show the 90% confidence interval, based on heteroskedasticity and autocorrelation (HAC) consistent standard errors [Colour figure can be viewed at wileyonlinelibrary.com]

As can be observed in Figure 3, there is a significant increase in government spending and output after both shocks in both states, but the pattern is different. After a BP shock, government purchases and output increase immediately, while both variables respond much more sluggishly to a DN shock, in particular government purchases. This can be explained by the different nature of the shocks. Specifically, BP shocks capture instantaneous shifts in government spending, whereas DN shocks portray news about future changes in spending. Figure 3 further reveals that the increase in government spending tends to be more pronounced in low-debt states than in high-debt states.

Despite the different nature of the spending innovations, the estimated government spending multipliers turn out to be considerably larger in high private debt periods. For both shocks, we find output multipliers that are positive, but smaller than one in low private debt states. The point estimates of the output multipliers range between 0.8 and 0.9 in the medium to long run for both shocks. In contrast, the estimated multipliers in periods of debt overhang are significantly greater than one, and even reach values of 1.9 for BP shocks and 4.5 for DN shocks. The magnitudes of the long-run multipliers (after 12 quarters) for BP and DN shocks are remarkably similar at longer horizons (i.e., respectively 1.5 and 1.4). The

TABLE 1 GDP cumulative multipliers in high and low private debt states

	(a) BP shocks					(b) DN shocks			
	Impact	Q4	Q8	Q12		Impact	Q4	Q8	Q12
High private debt					*High private debt*				
Point estimate	1.41	1.85	1.57	1.45	Point estimate	−3.20	4.52	2.30	1.42
Standard error	(0.13)	(0.49)	(0.72)	(0.65)	Standard error	(3.35)	(0.70)	(0.38)	(0.21)
1st-stage *F*-stat.	∞	55.95	6.49	3.93	1st-stage *F*-stat.	1.29	35.11	63.62	30.14
Low private debt					*Low private debt*				
Point estimate	1.05	0.93	0.93	0.92	Point estimate	7.96	0.84	0.80	0.83
Standard error	(0.09)	(0.05)	(0.05)	(0.04)	Standard error	(53.18)	(0.08)	(0.06)	(0.04)
1st-stage *F*-stat.	∞	115.32	47.29	19.66	1st-stage *F*-stat.	0.02	6.44	7.51	7.47
Difference					*Difference*				
Point estimate	0.36	0.92	0.64	0.53	Point estimate	−11.16	3.68	1.50	0.59
p-value (HAC)	[0.01]	[0.06]	[0.37]	[0.41]	*p*-value (HAC)	[0.83]	[0.00]	[0.00]	[0.01]
p-value (AR)	[0.01]	[0.08]	[0.31]	[0.32]	*p*-value (AR)	[0.67]	[0.14]	[0.10]	[0.05]

Note. The table reports the GDP cumulative spending multipliers in high and low private debt states at different horizons: impact, 4 quarters, 8 quarters and 12 quarters after the shock. Panel (a) is based on Blanchard–Perotti (BP) shocks, whereas panel (b) is based on Ramey's defense news (DN) shocks. For each debt state and each horizon, we report the point estimate, heteroskedasticity and autocorrelation consistent (HAC) standard error and the 1st-stage *F*-statistic. The relevant threshold for the *F*-statistic is always 23.1 for the 5% critical value and 19.7 for the 10% critical value. We also report the difference in the multiplier between high and low private debt states together with HAC and weak instrument robust Anderson–Rubin (AR) *p*-values.

difference between both states is economically meaningful and mostly statistically significant.[17]

A closer inspection of the responses and cumulative multipliers of personal consumption and investment in Figures 3 and 4 reveals a key reason for the different multipliers in both states. Specifically, in periods when private debt is below its long-term trend, the consumption multiplier is around zero and the investment multiplier is negative for both shocks. In other words, we find evidence of crowding-out of private domestic demand, which is consistent with Neoclassical and several New-Keynesian models. The opposite is, however, the case in periods of private debt overhang. We systematically document a positive contribution of personal consumption and investment to the size of the government spending multiplier—a result that is more in line with traditional Keynesian reasoning.

4.2 | Sensitivity analysis of baseline results

4.2.1 | Use of historical data

Sample periods excluding the years of World War II and the Korean War are characterized by a very small amount of variation in the key variables, which limits the ability to measure multipliers. This is even more the case in a nonlinear setting.[18] However, such extraordinary events were also accompanied by potentially distorting features (e.g., rationing, mandatory increases in production, surges in patriotism) and, more generally, historical data sources might be characterized by lower quality standards. To check whether our results depend on the use of historical data, we reestimate our model with BP shocks over alternative sample periods. We do not consider DN shocks for this exercise because, as discussed in Ramey (2011), the DN variable contains very little information once World War II and the Korean war are removed from the estimation sample.

In a first exercise, we exclude the years of World War II and the Korean War from the sample. In a second exercise, we reestimate the model using only modern (postwar) data.[19] The results are summarized in Figure 5 and Table 2. We observe somewhat lower multipliers in both states in the recent sample period compared to the baseline sample period, a finding which is consistent with Ramey and Zubairy (2018). This seems to be even more the case in periods

[17]Some studies only report the impact multipliers of innovations to government spending (as explained above, the impact multiplier is not informative for the DN shock). For the BP shocks, we find impact multipliers of 1.41 and 1.05 in, respectively, the high and low private debt state. The former is significantly different from one, the latter not. The difference between the impact multipliers (0.36) is statistically significant.

[18]As argued in Hall (2009), "there is little hope of learning much about the multipliers from any data after the mid-1950s." All the existing evidence "is limited in its ability to measure multipliers for the period from 1948 onward by the lack of variation in government purchases, especially in its most exogenous component, military purchases."

[19]Practically, the first exercise excludes from the estimation sample the time periods 1941:Q3–1945:Q4 and 1950:Q2–1953:Q3, while the second exercise excludes the interval 1919:Q1–1951:Q4. In both cases, we also exclude the respective lags and leads.

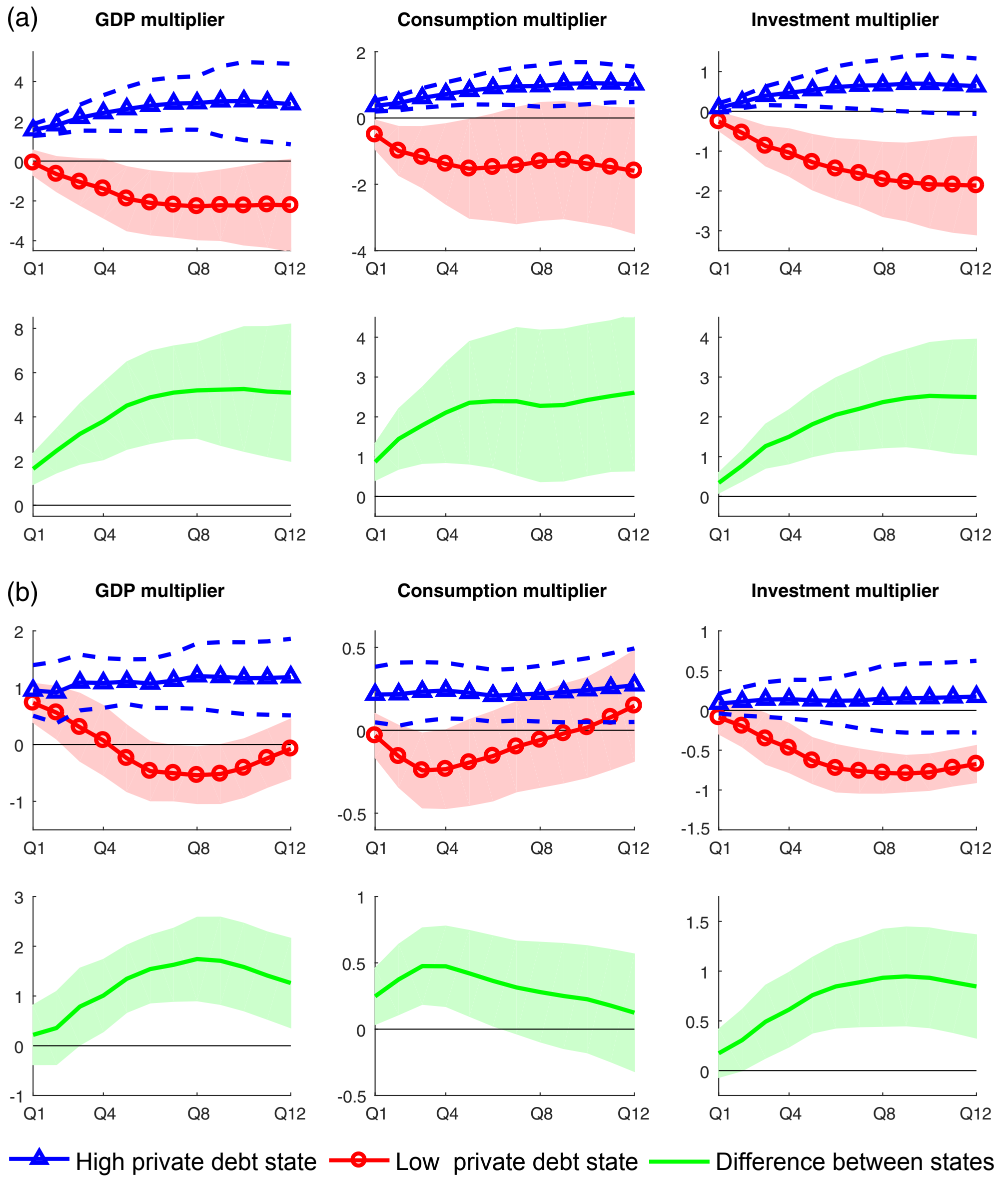

FIGURE 5 Sensitivity analysis—use of historical data: (a) BP shocks—excluding World War II and Korean War; (b) BP shocks—using only modern sources. Panel (a) shows the cumulative multipliers for Blanchard–Perotti shocks (BP) excluding the years of World War II (1941:Q3–1945:Q4) and the Korean War (1950:Q2–1953:Q3). Panel (b) shows the cumulative multipliers for Blanchard–Perotti shocks (BP) starting the sample from 1952:Q1. The horizontal axes measure the analyzed horizon (expressed in quarters) and the vertical axes measure the percentage change. The bands show the 90% confidence interval, based on heteroskedasticity and autocorrelation (HAC) consistent standard errors [Colour figure can be viewed at wileyonlinelibrary.com]

of low private debt when we only exclude the years of the wars. Overall, this indicates that spending multipliers during the wars were not as low as some other studies have suggested.[20] Most importantly in the context of our analysis, the estimated multipliers for both alternative sample periods are significantly higher in periods of ample private debt relative to low private debt states. The differences are economically and statistically significant. In sum, despite the difficulties of identifying state-dependent effects with very limited variation, our key result that government spending multipliers are greater in periods of private debt overhang remains when we remove the most informative part of the sample period.

[20]For example, Hall (2009) and Gordon and Krenn (2010) infer the presence of a negative net effect of World War II on the multiplier. Barro and Redlick (2011), in contrast, reach the opposite conclusion, making the case for a positive net effect.

TABLE 2 Sensitivity analysis-use of historical data

	(a) BP shocks: excluding World War II and Korean War					(b) BP shocks: using only modern sources			
	Impact	Q4	Q8	Q12		Impact	Q4	Q8	Q12
High private debt					*High private debt*				
Point estimate	1.56	2.42	2.92	2.87	Point estimate	0.95	1.08	1.20	1.19
Standard error	(0.18)	(0.54)	(0.82)	(1.23)	Standard error	(0.27)	(0.27)	(0.35)	(0.41)
1st-stage *F*-stat.	∞	67.71	52.92	18.91	1st-stage *F*-stat.	∞	128.03	53.47	40.83
Low private debt					*Low private debt*				
Point estimate	−0.09	−1.38	−2.28	−2.23	Point estimate	0.73	0.07	−0.54	−0.07
Standard error	(0.38)	(0.90)	(1.02)	(1.41)	Standard error	(0.21)	(0.37)	(0.30)	(0.32)
1st-stage *F*-stat.	∞	36.63	12.34	10.04	1st-stage *F*-stat.	∞	102.24	56.65	37.05
Difference					*Difference*				
Point estimate	1.64	3.80	5.20	5.09	Point estimate	0.22	1.01	1.74	1.26
p-value (HAC)	[0.00]	[0.00]	[0.00]	[0.01]	*p*-value (HAC)	[0.55]	[0.02]	[0.00]	[0.02]
p-value (AR)	[0.00]	[0.01]	[0.00]	[0.01]	*p*-value (AR)	[0.55]	[0.07]	[0.04]	[0.06]

Note. The table reports the GDP cumulative spending multipliers in high and low private debt states at different horizons: impact, 4 quarters, 8 quarters and 12 quarters after the shock. Both panels are based on Blanchard–Perotti (BP) shocks. Panel (a) shows results from a specification which excludes the years of World War II (1941:Q3–1945:Q4) and the Korean War (1950:Q2–1953:Q3), whereas panel (b) shows results for a sample starting in 1952:Q1. For each debt state and each horizon, we report the point estimate, the heteroskedasticity and autocorrelation consistent (HAC) standard error and the 1st-stage *F*-statistic. The relevant threshold for the *F*-statistic is always 23.1 for the 5% critical value and 19.7 for the 10% critical value. We also report the difference in the multiplier between high and low private debt states together with HAC and weak instrument robust Anderson–Rubin (AR) *p*-values.

4.2.2 | Definition of private debt states

The results are also robust when we consider the alternative measures of high and low private debt states that have been discussed in Section 3.1. These robustness checks are summarized in Table 3. More precisely, the use of household debt or total private debt (i.e., including the financial sector) delivers very similar results, although the differences between both states are smaller than the baseline results for BP shocks. In addition, the results do not appear to be strongly affected by the detrending method. For both BP and DN shocks, we find differences that are much greater than in the baseline estimations when we use low-frequency moving averages as a way to detrend the data (see Section 3.1), particularly in the medium run. Changes in the smoothness of the HP trends, instead, do not seem to affect the estimated multipliers in the case of DN shocks, and tend to deliver slightly larger differences in the case of BP shocks.

Furthermore, we check the robustness of the results when we consider a continuous instead of a discrete (dummy variable) debt gap indicator. In order to preserve the baseline specification in Equation 3, we use the empirical cumulative distribution of the debt-to-GDP gap to obtain an indicator I_t which varies continuously between 0 and 1. The advantage of a continuous indicator is that it explicitly takes into account the relative severity of the high (and low) debt episodes. A drawback is that it increases the influence of the detrending method. In particular, as we have shown in Section 3.1, alternative detrending methods tend to identify similar periods of high and low debt overhang, but the deviation from trend depends, often considerably, on the detrending method. However, as can be seen in Table 3, the use of a continuous debt indicator turns out to deliver very similar results. We find slightly smaller differences between both states for BP shocks and moderately larger differences for DN shocks.

Existing studies are not univocal on the exact influence that private debt regimes could have on fiscal multipliers. So far, we have made a distinction between regimes of high and low private sector debt. Accordingly, periods of *debt overhang* are situations where households and firms have accumulated a high stock of debt. At the margin, such a situation tends to increase the debt burden or the willingness to borrow when a shock occurs and, at the same time, could make it more difficult to get new loans to refinance existing debt (Johnson & Li, 2010; Peersman & Smets, 2005). For example, Mian, Rao, and Sufi (2013) document a much higher marginal propensity to consume of highly leveraged households. In other words, these are periods where the intensive and/or extensive margin of debt-constrained borrowers may be higher than normal. As a result, consumption and investment may behave differently from what is predicted by intertemporal optimization, leading to larger multipliers. Theoretically, such a distinction is consistent with Kaplan, Violante, and Weidner (2014) and Kaplan and Violante (2014), who show that debtors that own large illiquid assets (e.g., houses) may display larger propensities to consume out of additional transitory income, resulting in greater multipliers due to liquidity constraints. Such liquidity constraints likely increase when the stock of debt and hence debt service of households are higher. Finally,

TABLE 3 Sensitivity analysis-definition of the private debt states

	(a) BP shocks				**(b) DN shocks**			
	Impact	**Q4**	**Q8**	**Q12**	**Impact**	**Q4**	**Q8**	**Q12**
Household debt								
High private debt	1.29	1.54	1.16	1.07	−2.65	4.10	2.06	1.35
Low private debt	1.03	0.93	0.92	0.90	5.00	0.80	0.77	0.80
Difference	0.26	0.61	0.24	0.16	−7.65	3.31	1.29	0.56
p-value (HAC)	[0.04]	[0.12]	[0.58]	[0.61]	[0.88]	[0.00]	[0.00]	[0.00]
p-value (AR)	[0.04]	[0.12]	[0.53]	[0.59]	[0.84]	[0.13]	[0.11]	[0.04]
Total private debt								
High private debt	1.30	1.57	1.15	1.06	−3.08	4.28	2.12	1.35
Low private debt	1.05	0.92	0.93	0.92	5.78	0.85	0.80	0.83
Difference	0.24	0.65	0.23	0.14	−8.86	3.44	1.32	0.52
p-value (HAC)	[0.12]	[0.08]	[0.57]	[0.69]	[0.70]	[0.00]	[0.00]	[0.01]
p-value (AR)	[0.12]	[0.10]	[0.53]	[0.67]	[0.57]	[0.17]	[0.12]	[0.07]
Higher smoothness ($\lambda = 1e7$)								
High private debt	1.42	2.08	3.28	−37.32	−2.75	4.45	2.09	1.35
Low private debt	1.06	0.98	0.97	0.95	2.47	0.65	0.76	0.82
Difference	0.36	1.09	2.31	−38.27	−5.23	3.81	1.34	0.53
p-value (HAC)	[0.06]	[0.08]	[0.26]	[0.94]	[0.15]	[0.00]	[0.00]	[0.00]
p-value (AR)	[0.06]	[0.27]	[0.30]	[0.26]	[0.40]	[0.13]	[0.11]	[0.05]
Lower smoothness ($\lambda = 4e5$)								
High private debt	1.44	1.94	1.68	1.57	−3.17	4.46	2.28	1.40
Low private debt	1.03	0.90	0.91	0.90	3.32	0.81	0.78	0.81
Difference	0.41	1.03	0.78	0.67	−6.49	3.65	1.50	0.58
p-value (HAC)	[0.02]	[0.00]	[0.15]	[0.32]	[0.49]	[0.00]	[0.00]	[0.00]
p-value (AR)	[0.02]	[0.09]	[0.17]	[0.24]	[0.59]	[0.15]	[0.11]	[0.05]
Moving average band (15–20 years)								
High private debt	1.28	1.98	2.08	1.95	3.59	2.85	3.11	3.31
Low private debt	1.01	0.88	0.89	0.89	−0.36	0.75	0.79	0.83
Difference	0.27	1.10	1.19	1.05	3.95	2.11	2.32	2.49
p-value (HAC)	[0.13]	[0.01]	[0.14]	[0.30]	[0.65]	[0.06]	[0.01]	[0.02]
p-value (AR)	[0.13]	[0.04]	[0.18]	[0.35]	[0.72]	[0.06]	[0.04]	[0.06]
Continuous debt gap indicator (empirical cumulative distribution)								
High private debt	1.29	1.68	1.37	1.26	0.79	3.22	2.37	1.73
Low private debt	1.10	0.98	0.95	0.90	−0.30	0.29	0.53	0.67
Difference	0.19	0.70	0.42	0.37	1.09	2.93	1.84	1.07
p-value (HAC)	[0.35]	[0.15]	[0.49]	[0.49]	[0.96]	[0.06]	[0.00]	[0.00]
p-value (AR)	[0.35]	[0.23]	[0.48]	[0.50]	[0.96]	[0.13]	[0.08]	[0.02]
Dynamics								
Deleveraging	0.94	0.91	0.87	0.81	−1.23	0.16	0.67	0.71
Leveraging	1.17	0.95	0.99	1.01	0.18	0.91	−209.97	−0.15
Difference	−0.23	−0.03	−0.11	−0.20	−1.41	−0.74	210.64	0.86
p-value (HAC)	[0.02]	[0.82]	[0.45]	[0.18]	[0.65]	[0.27]	[1.00]	[0.77]
p-value (AR)	[0.02]	[0.82]	[0.52]	[0.31]	[0.48]	[0.52]	[0.42]	[0.55]

Note. The table summarizes the results of several robustness checks on the definition of the private debt states. It reports the GDP cumulative spending multipliers in high and low private debt states at different horizons: impact, 4 quarters, 8 quarters, and 12 quarters after the shock. We also report the difference in the multiplier between high and low private debt states together with heteroskedasticity and autocorrelation consistent (HAC) and weak instrument robust Anderson–Rubin (AR) *p*-values.

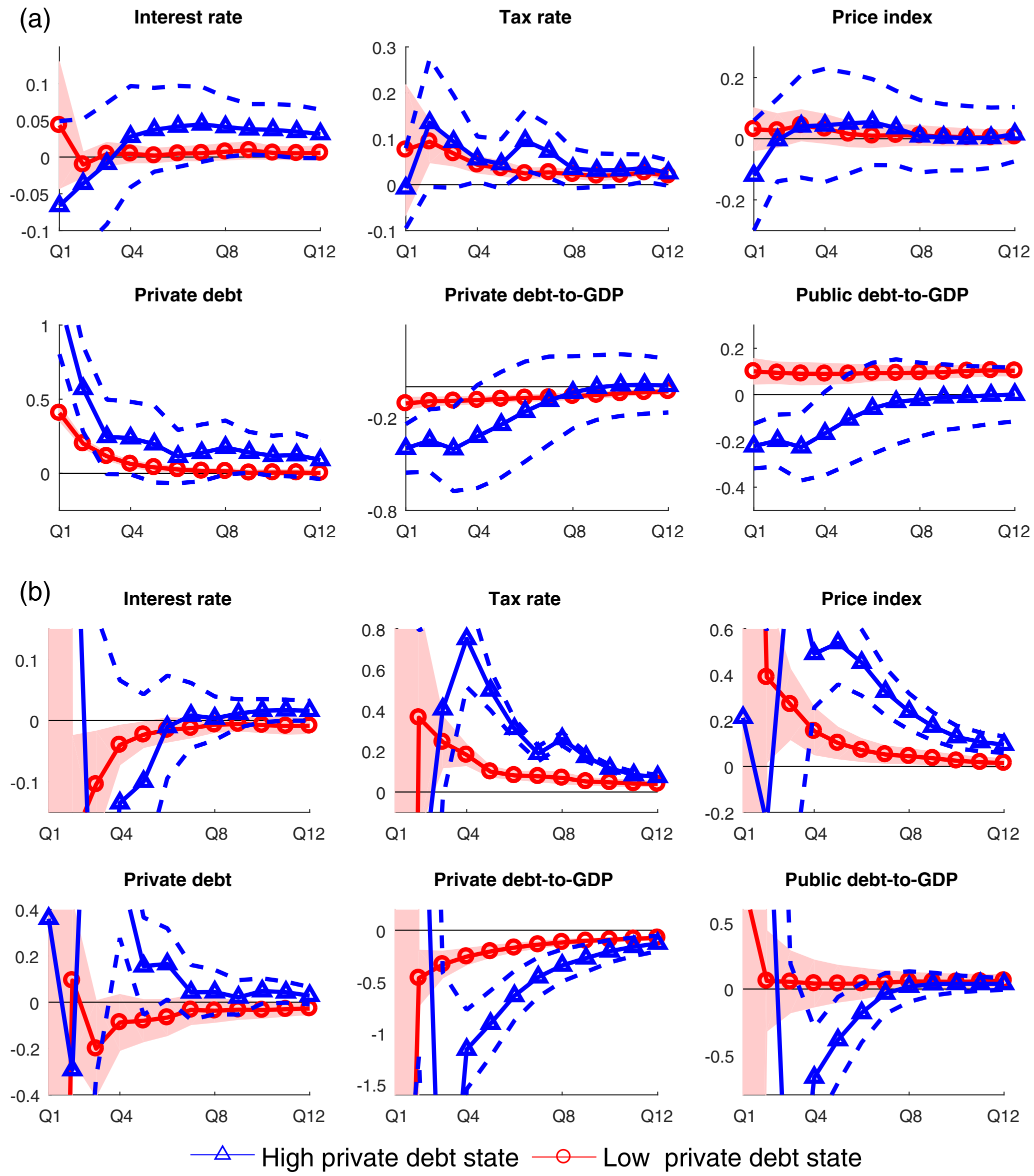

FIGURE 6 State-dependent economic transmission: (a) BP shocks; (b) DN shocks. Panels show the effects of Blanchard–Perotti shocks (BP) and Ramey's Defense News (DN) on the nominal interest rate, the average marginal tax rate, the GDP price deflator, private debt, and private and public debt to-to-GDP ratios in high and low private debt states. All impulse responses are normalized by the corresponding cumulative response of government spending. The horizontal axes measure the analyzed horizon (expressed in quarters). The bands show the 90% confidence interval, based on heteroskedasticity and autocorrelation (HAC) consistent standard errors [Colour figure can be viewed at wileyonlinelibrary.com]

Andres et al. (2015) demonstrate that, other things being equal, fiscal policy reaches its maximum effectiveness at the peak of the debt cycle.

However, other studies consider agents to be less constrained to borrow in periods of credit expansion, while the opposite is the case in episodes of *deleveraging* (e.g., Corsetti, Meier, & Müller, 2012). The latter is a situation where some borrowers, for some reason (e.g., a decline in the borrowing limit or a change in debt preferences), decrease their outstanding debt through a cut in personal expenditures. Since the cut is not motivated by a change in spending preferences, the borrower could find himself liquidity (debt) constrained, augmenting fiscal multipliers. This interpretation suggests that multipliers may be different depending on the flow of debt. Eggertsson and Krugman (2012), for example, show that the effectiveness of fiscal policy is greater during periods characterized by private debt deleveraging, in combination with passive monetary policy. Note that both debt overhang and deleveraging may also affect the multiplier simultaneously since these theories are not mutually exclusive.

Although it is the *attempt* to decrease debt that theoretically matters, several studies identify deleveraging periods as episodes when debt-to-GDP ratios decline.[21] As a final sensitivity check of the baseline specification, we therefore use a peak-to-trough classification to capture different debt regimes. Specifically, we reestimate the baseline specification, but consider periods of leveraging (trough-to-peak of detrended debt-to-GDP ratio) versus deleveraging (peak-to-trough) as the two debt regimes. The identified regimes are shown in Figure 7(c), while the results are shown at the bottom of Table 3. For this type of classification of the debt cycle, it appears that the multipliers are not different between both debt regimes. Only for BP shocks on impact do we find stronger effects in periods of leveraging.[22] In other words, it seems that the stock of debt is an important driver of the multiplier, rather than debt flows. In Section 6 we will confirm that this is also the case when we consider both states simultaneously.

Overall, the large battery of sensitivity checks confirms our conclusion that the amount of private debt in the economy appears to be a crucial indicator for the repercussions of fiscal stimulus and consolidation programs on private economic activity. In Section 6 we will further assess the robustness of our findings when we control for alternative possible determinants of the multiplier.

5 | EFFECTS ON OTHER VARIABLES

Although our approach cannot determine the exact reason for the different behavior in both states—an issue which is out of the scope of this paper—in this section we estimate the state-dependent effects of government spending shocks on a set of other variables to shed more light on possible mechanisms that could trigger or amplify the differences of multipliers between both states. The results of this exercise are shown in Figure 6. More precisely, the panels in the figure present the effects of the shocks on respectively the nominal interest rate, the average marginal tax rate, the price index, real per capita private debt, as well as the private and government debt-to-GDP ratios. In order to be comparable across states, all impulse responses are normalized by the corresponding cumulative response of government spending in the denominator.

A number of interesting observations can be made. First, the nominal interest rate response is negligible and almost never significant. In the low private debt state, in the case of DN shocks, there is even a mild decline following an expansionary spending shock. A passive or accommodative monetary policy reaction is typically also found in other empirical studies (e.g., Perotti, 2014; Ramey, 2011). This suggests that the presence of the zero lower bound on the nominal interest rate—that is, the lack of a response of the nominal interest rate to fiscal policy—is by itself not a unique situation. Also in other periods, the interest rate seems to have remained constant after spending shocks. Overall, the monetary policy response to the fiscal impulse appears not to be a reason for larger multipliers in high debt states.

Second, as pointed out by Baxter and King (1993), the way increases in government spending are financed might matter for the macroeconomic consequences. Multipliers are expected to be lower when spending is financed by distortionary taxes, rather than deficits. The estimated state-dependent responses of the marginal tax rate, however, suggest that the different multipliers in high and low private debt states are not driven by the way spending is financed. In particular, the response of the marginal tax rate does not differ between states for the BP government purchases shocks. For the DN shocks, the tax rate increases even relatively more in the high private debt state. The tax response could hence also not explain why spending multipliers are larger in periods of private debt overhang.

Third, a potential channel through which private debt might affect the macroeconomic transmission of fiscal shocks is the debt deflation (inflation) amplification described in Fisher (1933). While the results are inconclusive for BP shocks, we find that positive government spending shocks are followed by a rise in the aggregate price index for the DN shocks, which is higher in a private debt overhang regime. Furthermore, for both shocks we observe a decline in the private debt-to-GDP ratio. Most importantly, the reduction in private debt ratios is for both shocks much larger in high private debt states. This finding is by itself not a surprise, given the greater multiplier, but the stronger decline in the private debt ratio could also serve as an amplifier of the government purchases shocks. Specifically, expansionary government spending shocks improve the balance sheets of households and firms more in high private debt states, making several of them less debt constrained, which could in turn stimulate consumption and investment through credit, further reducing the debt burden, etc. This possible amplification mechanism is consistent with the strong sustained increase in new borrowing in the high private debt state, as illustrated by the responses of real per capita private debt in Figure 6. The opposite is

[21] In general equilibrium, the attempt to decrease the debt position through a cut in consumption could raise the debt-to-GDP ratio, due to a fall in GDP this might induce. For an empirical use of the term deleveraging, see, for example, Justiniano, Primiceri, and Tambalotti (2015).

[22] For this specification, we obtain very erratic estimates, as well as huge standard errors, for the cumulative multipliers of DN shocks.

obviously the case for a restrictive fiscal policy shock. Such a mechanism, which could amplify the differences across private debt states, is also consistent with Mian et al. (2013), who observe that the marginal propensity to consume out of wealth is much higher for highly leveraged households.

Finally, the results also reveal that government debt consolidations via a reduction in expenditures were not effective in reducing government debt in periods of debt overhang in the private sector. In contrast, fiscal expansions have, on average, reduced government debt in high private debt states. As shown in Figure 6, expansionary (restrictive) shocks to government purchases tend to be followed by a decline (rise) in the government debt-to-GDP ratio in high private debt states, whereas the opposite is true in low private debt states. Put differently, the permanent income of (intertemporal maximizing) Ricardian agents does not decrease after an expansionary fiscal policy shock in high private debt states. Specifically, since government debt does not increase, there is also no rise in future tax liabilities created by the government purchases. Hence Ricardian agents probably do not cut consumption in high private debt states. In contrast, they might even increase consumption in response to the fall in government debt-to-GDP, further reinforcing the government spending shock. In sum, the stronger effect on private debt in high private debt states, as well as the favorable effects of a positive spending shock on government debt, could have acted as an accelerator mechanism for government purchases.

6 | THE ROLE OF OTHER PROMINENT STATE VARIABLES

So far, following the state-dependent local projections and regime-switching VAR literature, we have allowed the economy to switch between two alternative states, defined as periods of respectively high and low private debt-to-GDP ratios. In this section, we relax this assumption by estimating "augmented" state-dependent local projection models. By doing this, we are able to assess whether our results still hold when we control for other potential states of the economy that could have had an influence on the government spending multiplier. For example, there could have been an overlap of high private debt states with recessions or periods when the nominal interest rate reached the zero lower bound. There exists a literature which argues that these features also augment the multiplier. We first describe the augmented state-dependent local projection model that we use, and then discuss the estimation results when we control for a set of financial, business cycle, and policy states.

6.1 | Augmented state-dependent LP-IV model

For each variable, and each horizon, we estimate the following IV-LP model for cumulative multipliers:

$$\begin{aligned}\sum_{l=0}^{h} z_{it+l} = &\left[\alpha_{A,h}^{mz} + \psi_{A,h}^{mz}(L)\, ctr_{t-1} + \beta_{A,h}^{mz}\sum_{l=0}^{h} g_{it+l}\right]\\ &+ I_{B,t-1}\left[\alpha_{B,h}^{mz} + \psi_{B,h}^{mz}(L)\, ctr_{t-1} + \beta_{B,h}^{mz}\sum_{l=0}^{h} g_{it+l}\right]\\ &+ I_{C,t-1}\left[\alpha_{C,h}^{mz} + \psi_{C,h}^{mz}(L)\, ctr_{t-1} + \beta_{C,h}^{mz}\sum_{l=0}^{h} g_{it+l}\right] + \epsilon_{t+h},\end{aligned} \tag{5}$$

where the notations are essentially the same as in the benchmark model. However, $I_{B,t-1}$ is now a dummy variable for being in a high private debt state, whereas $I_{C,t-1}$ is an indicator for an additional state variable (e.g., recession periods).[23] $\beta_{A,h}^{mz}$ represents what we label as the "neutral" multiplier; that is, the cumulative multiplier outside both states (e.g., low private debt nonrecession periods). The coefficients $\beta_{B,h}^{mz}$ and $\beta_{C,h}^{mz}$ measure the additional effects of moving from the "neutral state" to a particular state of the economy.[24] Before discussing the additional states and the related results, a note of caution is needed. Since DN shocks occurred only 25% of the time in the sample period, the lack of nonzero observations could result in imprecise estimates of the state-dependent effects when two state variables are considered simultaneously. In contrast to the BP shocks, the estimates for DN shocks are therefore not always very accurate, and the related results should be interpreted carefully.

[23] For this specification, the three endogenous variables $\sum_{l=0}^{h} g_{it+l}$, $I_{B,t-1}\sum_{l=0}^{h} g_{it+l}$, and $I_{C,t-1}\sum_{l=0}^{h} g_{it+l}$ are now instrumented by $shock_t$, $I_{B,t-1}shock_t$ and $I_{C,t-1}shock_t$.

[24] The state-dependent cumulative multipliers (controlling for the other state variable) are hence the sum of the coefficients $\beta_{A,h}^{mz}+\beta_{B,h}^{mz}$ and $\beta_{A,h}^{mz}+\beta_{C,h}^{mz}$, respectively (e.g., the multiplier in periods of high private debt and the multiplier in recession).

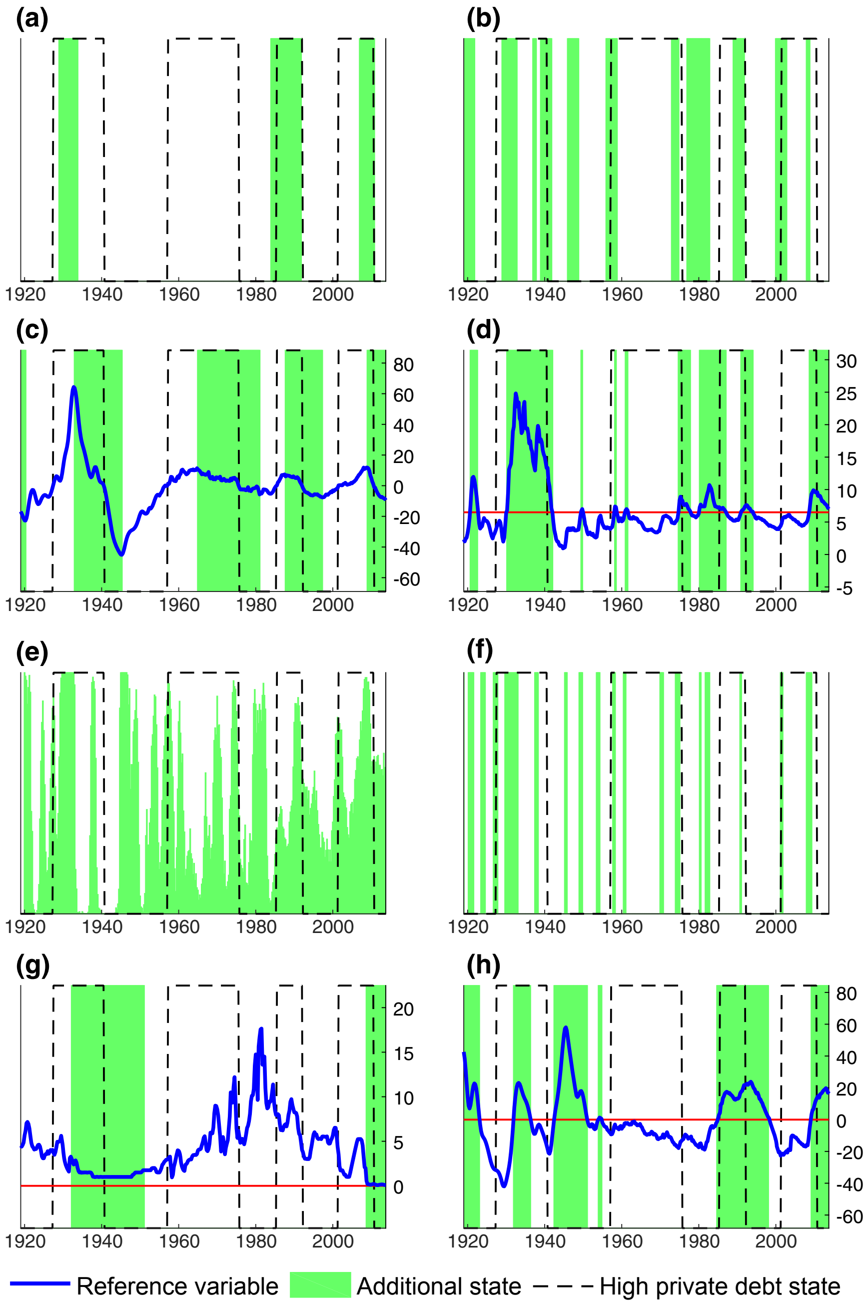

FIGURE 7 Additional state variables analyzed in the augmented state dependent local projection models. (a) banking crises; (b) stock market crashes; (c) deleveraging periods (peak-to-trough); (d) unemployment slack; (e) AG recessions; (f) NBER recessions; (g) zero lower bound periods; (h) high government debt. Each figure compares the additional state (green bars) with the high private debt state (dotted lines). When available, we show the reference variable used to define the additional state (blue lines) [Colour figure can be viewed at wileyonlinelibrary.com]

6.2 | Controlling for alternative financial states

Many theoretical papers refer to financial frictions as a class of potential drivers of the fiscal multiplier. In this subsection, we check whether our baseline state variable is still meaningful when we control for indicators that proxy financial states that are characterized by enhanced financial frictions. For example, in a cross-country panel analysis, Corsetti et al. (2012) find larger multipliers in the banking crisis periods identified by Reinhart and Rogoff (2011). The underlying assumption is that borrowing constraints for the private sector are prevailing during such periods. Not surprisingly, Figure 7(a) shows

TABLE 4 Augmented state-dependent local projections: Additional effects

	(a) BP shocks				(b) DN shocks			
	Impact	**Q4**	**Q8**	**Q12**	**Impact**	**Q4**	**Q8**	**Q12**
Controlling for alternative financial states								
High private debt	0.13	0.45	0.17	0.09	−12.74	3.36	1.06	0.36
	(0.16)	(0.25)	(0.30)	(0.19)	(83.36)	(0.89)	(0.30)	(0.10)
Banking crises	0.04	−0.63	−3.37	−4.84	11.32	20.19	−2.75	−13.20
	(0.35)	(0.63)	(0.89)	(1.64)	(111.27)	(93.64)	(13.44)	(26.31)
High private debt	0.20	0.78	0.54	0.41	−2.44	4.15	1.71	0.85
	(0.16)	(0.28)	(0.49)	(0.47)	(16.75)	(1.27)	(0.36)	(0.36)
Stock market crashes	−0.04	−0.15	−0.15	−0.13	−0.72	0.41	0.02	−0.22
	(0.24)	(0.21)	(0.19)	(0.21)	(14.09)	(1.15)	(0.46)	(0.43)
High private debt	0.20	0.58	0.53	0.62	−1.08	2.26	0.40	0.02
	(0.11)	(0.29)	(0.55)	(0.59)	(2.42)	(1.14)	(0.45)	(0.35)
Deleveraging periods	−0.31	−0.14	−0.26	−0.39	0.52	0.04	−1.24	0.68
	(0.09)	(0.09)	(0.10)	(0.14)	(0.81)	(0.38)	(10.28)	(2.34)
Controlling for the business cycle								
High private debt	0.47	0.79	0.62	0.54	−39.22	4.55	1.62	0.75
	(0.22)	(0.50)	(0.70)	(0.55)	(456.89)	(1.49)	(0.44)	(0.35)
Unemployment slack	−0.28	−0.12	−0.03	−0.05	35.78	−0.66	−0.49	−0.39
	(0.19)	(0.20)	(0.27)	(0.19)	(417.86)	(0.50)	(0.42)	(0.36)
High private debt	0.39	0.72	0.53	0.46	−3.09	1.16	0.31	0.07
	(0.13)	(0.29)	(0.39)	(0.38)	(3.33)	(0.51)	(0.36)	(0.21)
AG recessions	0.29	0.14	0.15	0.18	−3.22	−0.47	−0.02	0.28
	(0.10)	(0.09)	(0.17)	(0.23)	(2.76)	(0.17)	(0.31)	(0.40)
High private debt	0.45	0.75	0.55	0.53	−6.75	2.84	1.03	0.44
	(0.14)	(0.34)	(0.52)	(0.59)	(6.22)	(0.81)	(0.26)	(0.16)
NBER recessions	0.02	0.02	0.30	0.49	0.32	0.44	0.75	1.33
	(0.15)	(0.14)	(0.20)	(0.31)	(4.02)	(0.21)	(0.33)	(0.45)
Controlling for monetary and fiscal policy conditions								
High private debt	0.35	0.80	0.42	−0.29	−0.10	2.20	0.61	0.18
	(0.12)	(0.41)	(0.52)	(1.47)	(0.77)	(0.86)	(0.36)	(0.25)
Zero lower bound	0.06	0.18	−0.37	−3.12	−1.34	1.44	0.18	−0.39
	(0.21)	(0.40)	(0.60)	(4.54)	(0.90)	(1.79)	(1.11)	(1.52)
High private debt	0.63	1.22	1.21	1.15	2.18	1.51	2.91	0.90
	(0.15)	(0.37)	(0.72)	(0.86)	(3.16)	(28.15)	(5.33)	(0.32)
High public debt	0.43	0.28	0.47	0.42	6.89	3.80	−2.95	−1.49
	(0.13)	(0.15)	(0.25)	(0.42)	(4.72)	(15.49)	(17.93)	(2.44)

Note. The table shows the additional effects on the neutral output cumulative multiplier (i.e., the difference) during periods of high private debt and an alternative state at different horizons: impact, 4 quarters, 8 quarters, and 12 quarters after the shock. For each augmented state and each horizon, we report the point estimate and the heteroskedasticity and autocorrelation consistent (HAC) standard error.

that the episodes of banking crises identified for the USA always occurred during periods of high private debt.[25] As a first check, we therefore estimate Equation 5 with the banking crisis periods as an additional state variable. The results for the cumulative output multipliers on impact and in the fourth, eighth, and twelfth quarters are reported in Table 4. The results confirm that multipliers are considerably higher in periods of private debt overhang for both shocks, even when we control for banking crises. For banking crisis periods, the estimations based on BP shocks show a short-run positive additional effect (in line with Corsetti et al., 2012), who use a similar identification strategy), which turns negative at longer horizons. DN shocks generate similar results, although with very large standard errors.

As a second check, we control for stock market crashes to proxy periods of declining net worth of the private sector. Again, we rely on Reinhart and Rogoff (2011) to identify such periods. As shown in Figure 7(b), in contrast to banking crises, stock market crashes also occurred in periods that we have classified as low private debt states. The estimation results in Table 4 reveal that, for both type of shocks, spending multipliers are still much larger in high private debt

[25]Since Reinhart and Rogoff (2011) identify banking crises at an annual frequency, we assume this applies to all individual quarters of the year.

periods. On the other hand, somewhat surprisingly, we find evidence of lower multipliers during stock market crashes, independently of the shock that we use.

As a final check, we control for deleveraging periods. As discussed in Section 4, spending multipliers could increase when many agents are forced into deleveraging. Figure 7(c) manifests that this definition captures the second part of a debt overhang episode and the initial part of the subsequent low private debt episode. Table 4 shows that spending multipliers are still significantly higher in periods of private debt overhang for BP shocks, while multipliers seem to be lower in deleveraging periods. We do not find different multipliers in both states for DN shocks, but these estimates are very erratic and not informative. Overall, the augmented state-dependent local projections confirm the baseline finding that multipliers are greater in periods of private debt overhang, even when we control for alternative financial states.

6.3 | Controlling for the business cycle

Michaillat (2014) shows that the effects of government policies may be stronger in recession periods, because there is less crowding out of a rise in public employment on private employment when labor supply is convex. More generally, despite the lack of sound theoretical foundations, a popular Keynesian idea is that increases in government purchases might be more effective in recessions since more idle resources should be available for production. The empirical evidence is, however, mixed. Although some studies find the existence of a countercyclical multiplier (Auerbach & Gorodnichenko, 2012, 2013a; Baum, Poplawski-Ribeiro, & Weber, 2012), Owyang et al. (2013), and Ramey and Zubairy (2018) do not confirm the presence of high-spending multipliers in recessions. The latter studies use the same local projections method and a similar sample period to ours.[26]

To assess whether our results are not spuriously driven by an overlap of periods of slack with the high-debt state, we control for the business cycle. We use three different indicators. First, we augment the model with *unemployment slack* states. Following Ramey and Zubairy (2018), we define this state as periods when the unemployment rate was above the threshold level of 6.5%. This proxy tries to directly capture periods when a large fraction of resources was not employed. Second, we use the continuous recession indicator (*AG recessions*) constructed in Auerbach and Gorodnichenko (2012), which is based on a seven-quarter moving average of output growth rate. This variable tries to capture smooth changes in the cycle without focusing on discrete events. Finally, we use the *NBER recessions* periods. All three state variables are shown in Figure 7.

As can be seen in Table 4, the results confirm that multipliers are considerably higher in periods of debt overhang, irrespective of the state and measure of the business cycle. We find evidence of a rise in the cumulative multiplier at all horizons for the BP shocks, and at medium to long-run horizons for the DN shocks. On the other hand, the sign and significance of the impact of the business cycle on the multiplier is mixed. In particular, the estimated additional effect during periods of unemployment slack is negative for both shocks. For the AG recession indicator, we find a positive effect of BP shocks and a negative additional effect of DN shocks. Finally, during NBER recessions, we find a positive effect on the multiplier for BP and DN shocks. Overall, in contrast to the role of private debt, it is not possible to draw robust conclusions on the influence of the business cycle on government spending multipliers.

6.4 | Controlling for monetary and fiscal policy conditions

The final set of control variables that we consider are related to monetary and fiscal policy. Specifically, several theoretical studies conclude that the multiplier is larger in periods when there is a *binding zero lower bound on nominal interest rates* (Christiano et al., 2011; Eggertsson, 2011; Woodford, 2011). The mechanism can be described as follows. An increase (decrease) in government spending leads to a rise (fall) in inflation expectations. When the nominal interest rate is held constant, this results in a fall (rise) of the real interest rate, spurring (repressing) the economy. Christiano et al. (2011) show that the multiplier can be much larger than one when the nominal interest rate does not respond to an increase in government purchases. Note also that the estimated responses of the nominal interest rate in both states turn out to be quite modest (see Section 5). However, Carrillo and Poilly (2013) show that the government spending multiplier rises in a liquidity trap, beyond the impact on the interest rate.

[26]We also do not find robust evidence of greater multipliers when we estimate a two-state local projection model where the state variable is a business cycle indicator. Caggiano, Castelnuovo, Colombo, and Nodari (2015) report significant differences only when they consider deep recessions and strong booms.

In order to control for zero lower bound or extremely accommodative monetary policy episodes, we use the dummy variable of Ramey and Zubairy (2018), who identify two such periods; that is, 1932:Q2–1951Q1 and 2008:Q4–2013:Q4. As can be seen in Figure 7, there has indeed been an overlap with periods of private debt overhang, in particular in the 1930s. The results reported in Table 4 demonstrate that multipliers are considerably larger in periods of private debt overhang, even if we control for the presence of the zero lower bound. For the zero lower bound periods, in contrast, we do not find robust evidence of different multipliers.

As a final check, we control for *government debt overhang*. Perotti (1999) shows that the multiplier is a negative function of the initial government debt level. The higher the debt owned by the government, the higher the expected future tax rate when tax distortions are convex, resulting in a stronger negative wealth effect on private consumption. For a panel of 19 OECD countries, Perotti finds evidence that government expenditure shocks have a large positive effect on private consumption when government debt is low, whereas this effect vanishes when debt-to-GDP levels are high. Giavazzi and Pagano (1990) provide international evidence of expansionary fiscal consolidations at exceptionally high public debt-to-GDP levels. We are not aware of empirical studies that have examined the impact of government debt on spending multipliers in the USA.

To define periods of government debt overhang, we use the same method as described in Section 3 for private debt; that is, periods of government debt overhang are identified as the periods when there was a positive deviation of the government debt-to-GDP ratio from a smooth HP trend ($\lambda = 10^6$). The result of this exercise is shown in Figure 7(h). Also in this case, we can confirm our main finding that there is evidence of an additional positive and significant effect on the output spending multiplier during periods of high private debt. For government debt, we do not find an unambiguous influence on the multiplier.

7 | CONCLUSIONS

In this paper, we have used state-dependent local projection methods and historical US data to examine whether government spending multipliers have been different in periods of private debt overhang. The latter are identified as periods when the private nonfinancial debt-to-GDP ratio was above its long-term trend. We have compared the effects of, respectively, innovations to government purchases à la Blanchard-Perotti and Ramey's narrative defense news shocks.

We find that government spending multipliers were considerably larger in periods of private debt overhang. Specifically, in periods when the debt-to-GDP ratio was below its trend, the estimated spending multipliers turn out to be below one, which is the consequence of a mild crowding-out effect of government purchases on personal consumption and investment. These effects are in line with Neoclassical and some New-Keynesian models. The picture, however, totally changes in periods of ample private debt. The estimated multipliers in high private debt states are significantly greater than one. This is the result of a strong crowding-in effect on personal consumption and investment activities—a feature which is more in line with traditional Keynesian models. Moreover, we find that the government debt-to-GDP ratio decreases after an expansionary shock to government purchases in high private debt states, while there is a stronger decline of the private debt-to-GDP ratio. Both features could act as an amplifier of fiscal policy measures in periods of private debt overhang.

These results are new stylized facts and deserve additional research. For example, it is not clear what the exact reason is for the different behavior of the private sector in periods of debt overhang. Can it be explained by an increase in the extensive and/or intensive margin of debt-constrained borrowers, as in the models of Eggertsson and Krugman (2012) and Andrés et al. (2015)? Is it driven by a much higher marginal propensity to consume of highly leveraged households—a feature that has been documented by Mian et al. (2013)? Or are there alternative explanations? These are all issues that could be explored in future research. Another relevant extension of our analysis is the question whether also tax multipliers are different across private debt states.

Our findings also have some relevant policy implications. In particular, the state of private debt seems to be an important indicator for the consequences of fiscal consolidations and stimulus programs. In periods of debt overhang in the private sector, it is probably not advisable to implement austerity policies, because they could have dramatic negative effects on economic activity. In contrast, deficit-financed government purchases policies could significantly stimulate and stabilize the economy in periods when households are constrained by their debt. On the other hand, once private debt levels are again below trend, the timing is perfect to conduct fiscal consolidations, having minor negative consequences for economic activity.

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DATA APPENDIX

Table A1 lists the data used in the empirical analysis. For each variable, we report the sample period, the formula (using official IDs), the aggregation method, and the source.

TABLE A1

Variable	Sample	Formula and official IDs	Aggr.	Source
Real government spending	1919:Q1–1946:Q4	G	GR	GK10
	1947:Q1–2013:Q4	GCE/GDPCTPI		FRED
Real GDP	1919:Q1–1946:Q4	Y	GR	GK10
	1947:Q1–2013:Q4	GDP/GDPCTPI		FRED
Real consumption	1919:Q1–1946:Q4	CND+CS	GR	GK10
	1947:Q1–2013:Q4	(PCND+PCESV)/GDPCTPI		FRED
Real investment	1919:Q1–1946:Q4	IPDE+IRES+INRES	GR	GK10
	1947:Q1–2013:Q4	(PRFI+PNFI)/GDPCTPI		FRED
Population	1919:Q1–1951:Q4	pop	GR	ORZ13
	1952:Q1–2013:Q4	POP		FRED
Nominal interest rate	1919:Q1–1946:Q4	R	ST	GK10
	1947:Q1–1955:Q4	M13009USM156NNBR	ST	FRED
	1956:Q1–2013:Q4	FF		FRED
Price index	1919:Q1–1946:Q4	YDEF	GR	GK10
	1947:Q1–2013:Q4	GDPCTPI		FRED
Average marginal tax rate	1919:Q1–1949:Q4	*Federal individual income tax*	ST	BR11
	1950:Q1–2013:Q4	*All tax units (series 1)*		M13
Private debt-to-GDP	1919:Q1–1951:Q4	*(Cj875-Cj887)/Ca10*	GR	HSUS
	1952:Q1–2013:Q4	(TODNS-SLGSDODNS-FGSDODNS)/GDP		FRED
Public debt-to-GDP	1919:Q1–1951:Q4	*Cj871/Ca10*	GR	HSUS
	1952:Q1–2013:Q4	(SLGSDODNS+FGSDODNS)/GDP		FRED
Total private debt-to-GDP	1919:Q1–1951:Q4	*Cj875/Ca10*	GR	HSUS
	1952:Q1–2013:Q4	(TODNS-SLGSDODNS-FGSDODNS+DODFS)/GDP		FRED
Household debt-to-GDP	1919:Q1–1951:Q4	*Cj879 /Ca10*	GR	HSUS
	1952:Q1–2013:Q4	CMDEBT/GDP		FRED
Unemployment rate	1919:Q1–1947:Q4	unemp	ST	ORZ13
	1948:Q1–2013:Q4	UNRATE		FRED
NBER recessions	1919:Q1–2013:Q4	USRECQ	—	FRED
Banking crises	1919:Q1–2013:Q4	*Banking crises (7)*	—	RR11
Stock market crashes	1919:Q1–2013:Q4	*Stock market crashes (4)*	—	RR11

Note. Data related to different sources are merged either using the growth rate (GR) or simply by stacking (ST) them one on top of the other. The sources are Federal Reserve Economic Data (FRED), Carter et al. (2006) (HSUS), Gordon and Krenn (2010) "Cont Qrtly" data (GK10), Barro and Redlick (2011) (BR11), Mertens (2013) (M13), Owyang et al. (2013) (ORZ13), and Reinhart and Rogoff (2011) (RR11). The formulas in italics indicate annual series. Concerning the debt-to-GDP ratios, we transform the series in quarterly frequency using the cubic spline interpolation. Concerning the average marginal tax rate, the banking crises and stock market crashes episodes, annual figures are repeated for each quarter in the year.