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How Do Income and the Debt Position of Households Propagate Public into Private Spending?

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Abstract

We study the household sector's post-tax income and debt position as propagation mechanisms of public into private spending, in postwar U.S. data. In structural VARs, we obtain the consumption "crowding-in puzzle" for surges in public spending and show this consumption response to be accompanied by a persistent increase in disposable income. Endogenously reacting income, however, is insufficient to rationalize conditional comovement of private and public spending: once we hypothetically force (dis)aggregate measures of income to their pre-shock paths, consumption still rises. Corroborating these findings within an external-instruments-identified VAR, which constitutes an adequate laboratory for the simultaneous interplay of financial and macroeconomic time-series, we provide causal evidence of fiscal stimulus prompting households to take on *more* credit. This favorable debt cycle is paralleled by dropping interest rates, narrowing credit spreads, and inflating collateral prices, e.g., real estate prices, suggesting that softening borrowing constraints support the accumulation of debt and help rationalizing the absence of crowding-out.

Keywords: Government spending shock, household income, household indebtedness, credit spread, external instrument, fiscal foresight.
JEL codes: E30, E62, G51, H31.

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1 Introduction

Is impacting after-tax income an important transmission mechanism by which increases in public spending stimulate private consumption expenditures and, thereby, the broader economy? A vast amount of macroeconomic time-series contributions have established the finding of systematic crowding-in of household spending—induced by unexpected variation in public spending—to be a salient feature in postwar U.S. data.[1] So far, however, the literature has not converged to a consensus when it comes to the underlying drivers of the conditional comovement of private and public spending. While the traditional Keynesian paradigm predicts an *income-induced rise* of consumption, the empirical regularity of consumption crowding-in poses a challenge for plain-vanilla New Keynesian and RBC models, in which "throw-in-the-ocean" public spending induces negative wealth effects and causes optimizing households to substitute from consumption to labor supply.

Several routes have been taken to align these models with the conflicting evidence by introducing mechanisms that are designed to weaken the negative wealth effect, i.e., the reduction of present-value after-tax income, that public stimulus brings about for households.[2] Galí et al. (2007) propose a direct approach to make dynamics in sticky price (wage) models dependent on *current income* by adding Non-Ricardians, that is, households that do not hold physical capital and who consume their earnings in each period. Versions of this "limited asset market participation" or "Two-Agent New Keynesian" model have been commonly adopted in the literature.[3] Notably, "Heterogeneous-Agent New Keynesian" models (Kaplan et al., 2018) rely on richer representations of household finance, but generally feature consumers that, in equilibrium, behave in an income-constrained fashion (Bilbiie, 2019). Overall, the common ingredient to solve the consumption "crowding-in puzzle" across large parts of the fiscal policy literature can be summarized as: impacting income.

In this paper, we take one step back and revisit the role of disposable income as a

[1] See, e.g., Rotemberg and Woodford (1992), Fatás and Mihov (2001), Blanchard and Perotti (2002), Mountford and Uhlig (2009), Ravn et al. (2012), and Perotti (2014). Among contributions studying news shocks on military spending, i.e., *anticipated* variation in government spending, Fisher and Peters (2010), Forni and Gambetti (2016), and Ben Zeev and Pappa (2017) document crowding-in of consumption, while Ramey (2011) reports contractionary effects. Caldara and Kamps (2008) show that postwar U.S. data favor a conditional public-private-spending comovement, across identification schemes, once model specifications are harmonized.

[2] An incomplete selection includes, inter alia, deep-habits in consumption (Ravn et al., 2012), non-separable preferences between consumption and labor (Monacelli and Perotti, 2008), or useful and productive spending (Leeper et al., 2017; Sims and Wolff, 2018).

[3] See Corsetti et al. (2012), Drautzburg and Uhlig (2015), and McKay and Reis (2016) for academic approaches or the FRB/US model for a central bank adoption.

propagation mechanism for public spending, directly in the data. Our approach consists of explicitly modeling post-tax income in structural VARs, thus imposing only few assumptions and structure to recover public spending shocks. As a point of departure, we center around an updated version of the recursively-identified (Blanchard and Perotti, 2002) VAR proposed by Galí et al. (2007), covering data from 1954Q1 to 2015Q4.[4] We identify plausibly unexpected variation in government spending by contemporaneously conditioning on a proxy for fiscal foresight and report that debt-financed fiscal stimulus jointly raises consumption along with post-tax income.

Nevertheless, the public spending induced consumption-income-comovement is at best suggestive to an income-channel that may causally rationalize why consumption is not crowded out, contrary to Neoclassical models. Essentially, it is the *extra effect* that income adds to the response of consumption, which matters for this narrative. We approximate this extra effect of the income-channel via a statistical decomposition. Following the procedure in Bachmann and Sims (2012), we neutralize the endogenous income response with counteracting, exogenous surprises in after-tax income. In the absence of income dynamics, the macroeconomic repercussions of fiscal stimulus are muted, but notably, household absorption still reveals an inverted hump-shaped adjustment pattern. In our preferred model specification, the income-channel explains around one-third of the reaction of consumption; yet, *crowding-out* does not appear to be an empirical regularity, even when post-tax income remains hypothetically fixed. This finding extends to the entire income distribution; that is, when studying the propagation via, and the marginal effects of, disaggregate income-measures, as provided by Piketty et al. (2018), we arrive at similar inference relative to the aggregate counterpart.

The evidence of households being capable of expanding consumption volumes—without supporting income—suggests that households' debt position may adjust to finance the additional spending; that is, *consumption crowding-in may be reinforced by debt accumulation*. In this vein, Fernández-Villaverde (2010) shows how financial frictions may amplify the consequences of public stimulus. His model predicts that crowding-out of investment is counteracted once government spending propagates via imperfect financial markets (see Eggertsson and Krugman, 2012). While he emphasizes a (reverse) mechanism à la Fisher's debt-deflation, Carrillo and Poilly (2013) stress that the propagation of stimulus through the value of firms' collateral may directly narrow credit spreads and support equilibrium debt. To the extent

[4]We focus on postwar data and thus mainly on *civilian spending shocks*. This focus is consistent with Perotti (2014) and Nakamura and Steinsson (2014) who stress that samples covering the Korean war or WWII may cause identification problems due to, e.g., price controls or rationing.

that households face similar credit market imperfections, and borrowing constraints are conditionally mitigated, an expansion of household debt is equally conceivable.[5]

We provide empirical aid to this proposition by carefully modeling the joint dynamics of households' credit conditions and the fiscal policy stance. First, we explore the conditional interplay of prices and quantities in credit markets, i.e., private interest rates/spreads and debt volumes, in fiscal policy VARs. Second, we note that the recursiveness assumption to recover the VAR's structural form is no longer warranted, once fast-moving financial variables are present (see Gertler and Karadi, 2015).[6] To separate fiscal policy surprises from systematic reactions to, e.g., the state of the financial system, we augment the VAR by an external instrument (SVAR-IV) for identification (Stock and Watson, 2012; Mertens and Ravn, 2013), allowing for simultaneous feedback to public spending from all variables in the system. Following Barro and Redlick (2011), we use changes in military spending as an instrument. Military spending exhibits substantial swings over time and plausibly exhibits comovement with the unobserved policy innovations of interest (instrument relevance). In addition, military expenditures regularly reflect developments abroad, e.g., geopolitical instability, such that variation induced by domestic economic or financial conditions appears to be meager, in relative terms (exclusion restriction).[7]

Our main observations for debt-augmented fiscal SVAR-IVs are twofold. First, unexpected variation of public spending causally induces proxies for household credit such as consumer credit, mortgage debt, or total household indebtedness to expand significantly and persistently—in excess to a continuing rise in post-tax income and consumption. This striking interaction of household debt and the fiscal policy stance aligns with Cloyne and Surico (2017), who track variation in U.K. tax rates, and with Bernardini and Peersman (2018), who stress private debt as a *state variable* for fiscal output-multipliers.[8] Yet, both papers are silent on the role of private debt as a *transmission mechanism for public spending*. Second, the conditional debt cycle we document is paralleled by declining interest rates in credit markets. This finding corroborates Ramey (2011), who reports falling bond rates conditional on

[5] Miranda-Pinto et al. (2019) propose that fiscal stimulus may relax credit market conditions via redistribution of income toward saving agents.

[6] Caldara and Kamps (2017) make the case that even with respect to aggregate activity there may exist some degree of within-quarter endogeneity of recursively-recovered fiscal policy innovations. For forward-looking financial time-series, which are strongly correlated with and typically lead the business cycle, such concerns are likely to apply even more so.

[7] In fact, our instrument reveals close-to-zero and insignificant contemporaneous, unconditional correlation coefficients regarding changes in U.S. GDP or interest rates/spreads. To formally test instrument-relevance, we rely on methodological progress of Montiel Olea et al. (2018).

[8] Demyanyk et al. (2019) study consumer debt as a state variable for open-economy relative fiscal multipliers using geographical variation in U.S. defense spending during the Great Recession.

positive *news shocks* about military spending. However, neither does she relate her finding to the debt position of households, as we do, nor does her VAR address the simultaneity problem between financial and macroeconomic variables.[9] Moreover, our findings reinforce Auerbach et al. (2020), who exploit geographical variation in U.S. federal contracts across U.S. cities and find positive effects on local credit markets in data starting at the millennium. Our time-series approach puts their evidence into perspective and provides external validity by exploring the entire U.S. postwar history and by explicitly capturing general equilibrium effects.

Overall, the *conditional divergence of credit market volumes and prices* we observe supports the perception of reinforcing financial conditions strengthening the expansion of debt and, ultimately, the crowding-in of consumption. We provide tentative evidence on the transmission mechanisms underlying our results. First, as we do not reveal inflationary pressure to be unleashed by the surge in public spending, we conjecture that the conditional debt cycle does not appear to be induced by Fisher effects (Fernández-Villaverde, 2010). Second and aligning with the documented price-dynamics, we find no tightening of (real) policy or long-term risk-free rates; this absence of counteracting risk-free rates may thus contribute to the debt accumulation of households. Third, our SVAR-IV model reveals a significant compression of interest spreads in credit markets suggesting a softening of borrowing constraints, that is, easier access to credit for households (Auerbach et al., 2020). Fourth, this loosening of borrowing conditions is likely related to inflating asset prices: we document that public stimulus boosts collateral values such as real estate prices, which should positively impact on households' balance sheets and may reduce their (perceived) default probabilities (see Bernanke et al., 1999, for the related financial accelerator mechanism).

Taken together, our findings imply that income dynamics of households are likely an insufficient empirical moment to help expand our knowledge about the underlying drivers of the propagation of public into private spending. Our causal evidence on credit-augmented fiscal policy VARs prompts the view that the pass-through of stimulus into households' debt position constitutes a vital mechanism to make progress toward that direction.

The remainder of the paper is as follows: Section 2 presents the empirical setting and evidence on income. Section 3 explores debt-augmented VARs, Section 4 provides tentative insight into the transmission mechanisms, and Section 5 concludes.

[9]This concern regarding narrative identification in general is also expressed by Ramey (2016). D'Alessandro et al. (2019) and Miranda-Pinto et al. (2019) also report declining interest rates conditional on surges in public spending, relying on exclusion restrictions that identify their VARs.

2 Empirical framework

In this Section, we provide a structural VAR framework that we use to study the propagation of government spending to the broader economy, in particular, to consumption and disposable income of households. First, we describe the data and the identification strategy; second, we propose a method to isolate the contribution of post-tax income in the shock pass-through; and third, we present empirical results.

2.1 Structural VAR representation

We postulate that the variables of interest can be cast in a finite-order linear VAR representation of the form:

$$\boldsymbol{A_0 x}_t = \sum_{l=1}^{p} \boldsymbol{A}_l \boldsymbol{x}_{t-l} + \boldsymbol{\varepsilon}_t, \text{ with } \mathbb{E}\{\boldsymbol{\varepsilon_t}\} = 0 \text{ and } \mathbb{E}\{\boldsymbol{\varepsilon_t \varepsilon_t}'\} = \boldsymbol{\Sigma_\varepsilon}, \tag{1}$$

abstracting from the intercept for notational convenience. At lag, $l = 1, ..., p$, the $n \times n$ matrix $\boldsymbol{A}_l$ comprises the model's dynamics, and $\boldsymbol{A}_0$ captures contemporaneous relations. $\boldsymbol{\varepsilon}_t$ represents mutually uncorrelated innovations, i.e., $\boldsymbol{\Sigma_\varepsilon}$ is diagonal. We identify a government spending shock by a Cholesky-factorization of the VAR's reduced-form variance-covariance matrix, $\boldsymbol{\Omega}$, assuming government spending to be pre-determined with respect to within-quarter macroeconomic conditions (Blanchard and Perotti, 2002). For direct comparability with the VAR evidence in Galí et al. (2007), we stick to this strategy as the basic structure for identification, but relax the identification assumption in Section 3. However, such a purely recursive approach to recover the VAR's structural form may be subject to fiscal foresight concerns, i.e., the structural innovations may be forecastable, to some extent. Therefore, we recover plausibly unanticipated innovations in the fiscal policy stance by simultaneously conditioning public spending in the structural VAR on measures of fiscal news. As a baseline, we follow Bachmann and Sims (2012) and augment the VAR by military news, g_t^m, as proposed in Ramey (2011), ordered first in $\boldsymbol{x}_t$, followed by government spending (consumption plus investment), g_t, ordered second. Results are similar when conditioning on proxies as provided in Fisher and Peters (2010) or Ben Zeev and Pappa (2017), or when omitting news alltogether.

The remaining $j = 3, ..., n$ entries in vector $\boldsymbol{x}_t$ comprise the variables of the Galí et al. (2007) VAR, in this order: personal disposable income, y_t^{dis}, GDP, y_t, hours worked, h_t, consumption (non-durables and services), c_t, non-residential investment,

i_t, wages, w_t, and the budget deficit, d_t.[10] The after-tax income series we explore is extensive, comprising the following sources of income: U.S. residents' labor income, employer-provided supplements such as insurance, income from owning a business or from rental property, benefits from social security, interest income, and dividends; the series explicitly excludes valuation effects stemming from asset price movements.

We estimate the VAR over a sample beginning in 1954Q1 and extending to 2015Q4, including four lags of the vector of quarterly observables, i.e., $p = 4$. The start of the sample is motivated by, inter alia, Perotti (2014) and Nakamura and Steinsson (2014), who argue that including the WWII era may cause identification problems as the influence of interfering factors like price and production controls, rationing, or the military draft are hard to assess. Similarly, during the Korean war, results may be contaminated by new Fed regulations at the time. The impact of these events is particularly hard to gauge in military spending news identification approaches as, e.g., in Ramey (2011), in which results are mainly driven by the defense spending shocks during wars, whereas in the sample starting in 1954Q1 civilian spending shocks prevail. For the benchmark model, we end the sample in 2015Q4 due to the availability of proxies for fiscal foresight. We find throughout consistent results in estimations that omit the Great Recession episode; and we explore additional sub-sample sensitivity in Section 2.3.

2.2 A systematic analysis of the disposable-income-channel in a VAR

In this Section, we propose a procedure to statistically single out and *approximate the marginal effect* that impacting post-tax income adds to the response of consumption, within the structural VAR framework above. In this vein, the original argument put forth in, e.g., Galí et al. (2007) to call for an immediate income mechanism in New Keynesian models was based, among others, on the empirical comovement of private consumption and income after fiscal stimulus. This justification, however, builds upon a conditional consumption-income-*correlation* and not on a *causal* relation, in a statistical sense. We attempt to give perspective to the latter.

Our strategy consists of statistically decomposing the repercussions of public spending into those effects arising from the endogenous response of disposable income and those observed after fixing this transmission variable to its pre-shock

[10]The budget deficit enters as a ratio to trend GDP, proxied by lagged potential output; all measures enter at the quarterly frequency in real terms. Quantity series are population-normalized. Except for the budget deficit, which we measure in percent, time-series enter in log levels.

path.[11] Specifically, we quantify the so-defined marginal effect of income by positing hypothetical sequences of exogenous, Cholesky-factor-orthogonalized innovations in disposable income. We calculate the latter innovations such as to neutralize the endogenous response of post-tax income, conditional on the initial government spending surprise. Contrasting the benchmark response of consumption with the corresponding response absent movements in income, allows to infer the quantitative importance of the income-channel for the projected path of private consumption. By exactly canceling out the endogenous response of post-tax income, the exercise is capable of capturing rich effects, since the entire sprectrum of directly and indirectly operating effects stemming from the income-variable are shut down. Consequently, we view the corresponding results as an upper bound approximation of an independently operating income-channel.

Importantly, this decomposition is purely statistical in nature. For this reason, we do not assign any economic interpretation to the disposable income surprises we generate. By ordering personal disposable income after public spending in the VAR model, income surprises are allowed to contemporaneously pass-through to all variables in the system, except for government spending (and fiscal news).[12]

In a recursive setting, we compute the innovations to variable $y_t^{dis} \equiv \eta$, that are necessary to force the respective endogenous response to zero, as follows:

$$\boldsymbol{\varepsilon}_{\eta,h} = -\sum_{j=1}^{n} \boldsymbol{\Theta}_{\eta,j} \boldsymbol{y}_{j,h} - \sum_{m=1}^{\min(p,h)} \sum_{j=1}^{n} \boldsymbol{\Theta}_{\eta,mn+j} \boldsymbol{z}_{j,h-m}. \tag{2}$$

$\boldsymbol{y}_{j,0}$ denotes the $t = 0$ effect of a spending shock on variable j, whereas the same effect sans endogenous response of income reads: $\boldsymbol{z}_{j,0} = \boldsymbol{y}_{j,0} + \boldsymbol{\Phi}_{j,\eta,0}\boldsymbol{\varepsilon}_{\eta,0}/\sigma_\eta$, where $\boldsymbol{\Phi}_{j,\eta,0}$ is the $\{j, \eta\}$ element of the impulse response matrix for horizon $h = 0$. The standard deviation of income disturbances is σ_η, and for horizons $h > 0$ we calculate:

$$\boldsymbol{y}_{j,h} = \sum_{m=1}^{\min(p,h)} \sum_{i=1}^{n} \boldsymbol{\Theta}_{j,mn+i} \boldsymbol{z}_{j,h-m} + \sum_{i<j}^{n} \boldsymbol{\Theta}_{j,i} \boldsymbol{y}_{i,h}, \tag{3}$$

and ultimately:

$$\boldsymbol{z}_{j,h} = \boldsymbol{y}_{j,h} + \frac{\boldsymbol{\Phi}_{j,\eta,0}\boldsymbol{\varepsilon}_{\eta,h}}{\sigma_\eta}. \tag{4}$$

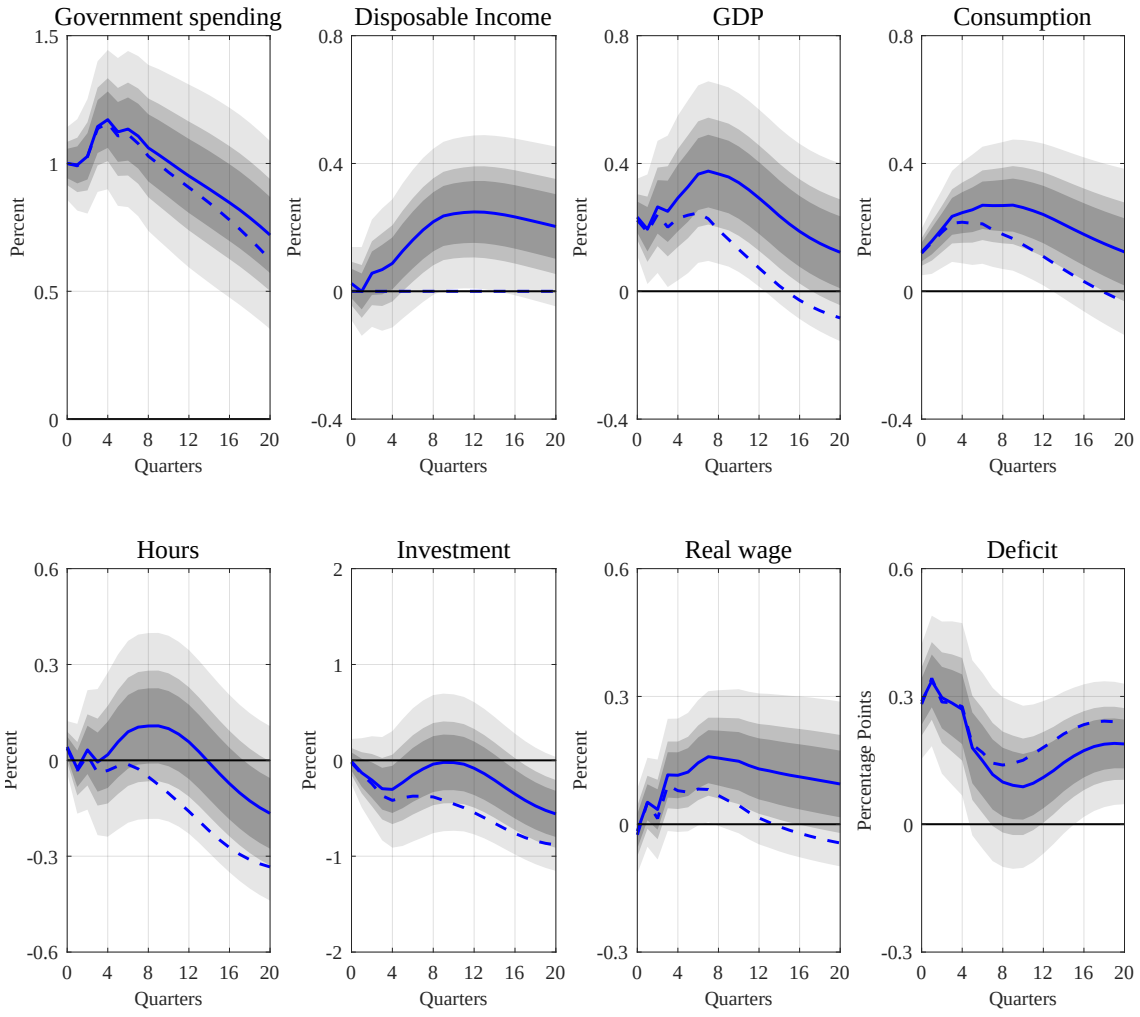
[11] Bachmann and Sims (2012) revitalize this method, which was pioneered by Bernanke et al. (1997), and apply it to the reaction of consumer confidence in their Cholesky-identified fiscal VAR. Recent applications include Bachmann and Rüth (2020).

[12] In the model of Galí et al. (2007), disposable income was ordered last in the VAR. Note however, that the position of income does only matter for the reduced-form disposable income surprises, but does not have any statistical impact on the dynamics triggered by the fiscal policy shock. As a matter of fact, the ordering within the sub-set of variables $j = 3, ..., n$ is orthogonal to the results for the public stimulus shock, with public spending ordered second, i.e., $j = 2$.

2.3 An empirical perspective on the income-channel of public stimulus

Ignoring the dashed lines for the moment, Figure 1 traces VAR dynamics (solid blue lines) conditional on an expansionary government spending surprise, for quarterly U.S. data ranging from 1954Q1 to 2015Q4. Dark to light shaded areas depict 50, 68, and 90 percent confidence intervals, obtained from a bootstrapping procedure (Goncalves and Kilian, 2004). We normalize the shock size such as to move public spending by one percent away from its pre-shock path, following convention and for comparability across different specifications.

Figure 1: Government spending shock: conditional dynamics



Notes: We plot dynamics at the quarterly frequency. Solid blue lines represent point estimates of impulse response functions. Dark to light shaded areas display 50, 68, and 90 percent confidence intervals, which we obtain from 1,000 replications of a recursive-design wild bootstrap procedure. Dashed blue lines denote results for a fixed disposable income experiment, along the lines of Bachmann and Sims (2012).

Despite the fact that we account for fiscal foresight and use a longer sample relative to Galí et al. (2007), whose data end in 2003Q4, we report fully consistent dynamics. Consumption rises sluggishly for two years, before slowly abating and the response is significantly positive for more than three years; that is, we observe the consumption crowding-in puzzle. GDP mimics the consumption response qualitatively and peaks at almost 0.4 percent. The budget deficit increases on impact and reaches the maximum response of 0.3 percentage points shortly after the impact period, i.e., we study a deficit-financed public spending innovation. In addition, we reveal Keynesian dynamics by documenting pro-cyclically responding wages and hours, over the medium run.[13] Investment responds negatively to stimulus, but insignificantly so.[14] In line with the labor market dynamics, post-tax income is sticky in the first year after the shock, but subsequently reveals a protracted, inverted hump-shaped impulse response, which deviates by more than 0.2 percent from its conditional mean, and which remains different from zero in a statistically significant sense for roughly two years.

The dashed blue lines in Figure 1 contrast the benchmark VAR's impulse responses (solid blue lines) with corresponding dynamics observed for a fixed-income scenario. As the solid and dashed lines in the government spending panel are very similar, zeroing-out the disposable income response does barely affect the systematic reaction of public spending to the exogenous shock, i.e. we essentially study the same fiscal stimulus hitting the economy. Post-tax income does, per definition, not react in the fixed-income experiment. The remaining variables' impulse responses in the fixed-income scenario closely track the responses of the benchmark model at short horizons, which is consistent with the protracted reaction of post-tax income in the unrestricted case. Over medium horizons, however, the income-channel appears to become operative. The zeroing-out of income makes the GDP reaction more short-lived and mutes its maximum response by roughly one-third. Wages become less pro-cyclical in the shock propagation and investment as well as hours worked process the shock via declining impulse responses. The public deficit is somewhat amplified, which—given an almost unchanged path of public spending—aligns with the documented lower aggregate activity and thus lower tax revenues. Most importantly, household absorption behaves qualitatively similar to the GDP response; that is, the maximum increase is mitigated and the impulse response returns faster toward

[13] Note that the positively reacting real wage that we document empirically is typically also the key ingredient within several New Keynesian approaches that aim to strengthen the income-channel and, ultimately, attempt to rationalize consumption crowding-in.

[14] Blanchard and Perotti (2002) and Mountford and Uhlig (2009) report an investment decline, while Fatás and Mihov (2001) document an increase.

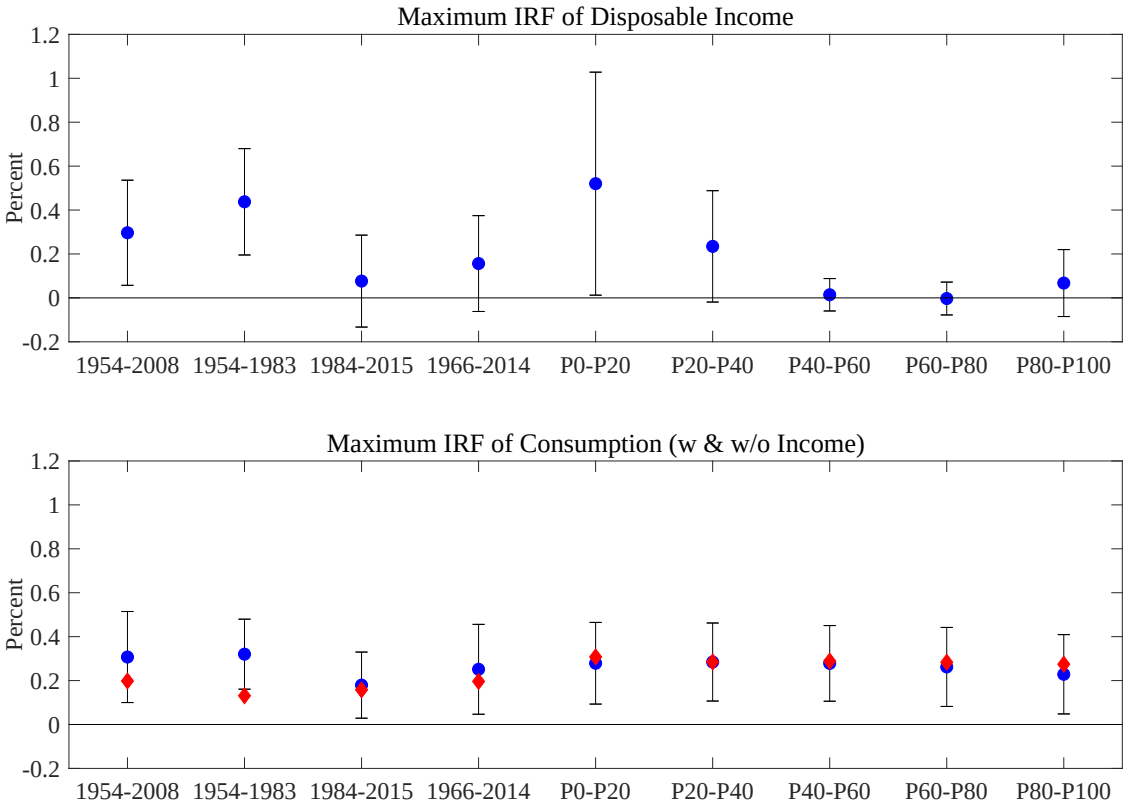
its conditional mean. In accumulated terms, the fixed-income scenario predicts a surge in consumption that is roughly one-third smaller relative to the unrestricted VAR case. Yet, even when we shut down the income-channel, there is no evidence of consumption crowding-out, in contrast to what Neoclassical theory would predict. Put differently, we infer that the conditional dynamics of disposable income are not sufficient to rationalize why consumption is crowded-in by public spending surprises.

In what follows, we scrutinize this finding for different samples and for disaggregate measures of post-tax income of households in order to learn more about the structural properties of our result.

The income-channel during different postwar episodes Given the substantial time-series dimension we have exploited so far, it is instructive to inspect the stability of our results across different postwar episodes. For the different subsamples we consider in what follows, Figure 2 summarizes the *maximum* impulse response coefficients (blue circles), along with 90 percent confidence intervals (black lines), for income (upper panel) and consumption (lower panel) to a one percent government spending expansion. In the Figure's lower panel, the red diamonds further present the corresponding reaction of consumption in a fixed-income scenario.

A natural sample modification involves the omission of the Great Recession during which, inter alia, the effective lower bound on short-term nominal interest rates became binding. We thus follow the dating convention in Ramey and Zubairy (2018), who however do not document significant non-linearities stemming from the zero lower bound, and end the sample in 2008Q3. Our result of a positive and significant shock procession of disposable income and consumption appears to be insensitive to the exclusion of the Great Recession period (utmost left element of Figure 2); in addition, the consumption response in the fixed-income scenario is muted by roughly one-third, but still positive as in the benchmark sample. Another potential source of parameter instability may be the transition from macroeconomic turbulence witnessed in the U.S. during, e.g., the '70s, to a more tranquil episode starting in the '80s that was subsequently coined the Great Moderation episode. Following, among others, Gambetti and Galí (2009) we split the sample into pre- and post-1984 data. The second and third element in Figure 2 depict the corresponding results. For pre-Great-Moderation data, the reaction of household consumption is similar to the baseline model, yet estimated with somewhat higher precision, while the income response is magnified. Interestingly, the marginal effect that after-tax income adds to the consumption response is substantial such that the consumption reactions with and without hypothetically fixed-income are statistically different from each other.

Figure 2: Consumption and income: sub-samples and disaggregate income data



Notes: Blue circles denote the maximum reaction of, respectively, disposable income (upper panel) and private consumption (lower panel) to a one percent increase in government spending; the black lines depict 90 percent confidence intervals. The red diamonds in the lower panel measure the consumption reaction for a scenario, in which the income response remains hypothetically fixed. The first four estimates in each panel report results for sample splits as denoted on the abscissa. The remaining five estimates correspond to VARs using disaggregate income measures, where we interpolated the annual data to the quarterly frequency.

For the episode starting in 1984 the consumption reaction is somewhat muted, but still significantly positive, whereas this is no longer true for disposable income; its maximum impulse response is statistically not different from zero, and the extra effect that income adds to the consumption response is practically zero. This latter finding could loosely be interpreted as constituting an *unrestricted counterfactual* answering the question on how consumption responds to fiscal stimulus without supporting income dynamics since the Great Moderation. The answer is: still positively. That is, consumption crowding-in appears to remain an empirical regularity. Consequently, it is hard to justify the income-channel as constituting *the* structural solution to this empirical puzzle. Our sub-sample finding as well as the hypotheti-

cal fixed-income exercise are thus suggestive of other transmission channels of fiscal policy being operative in excess to impacting income.

Public stimulus along the income distribution Models of the limited asset market participation type emphasize the role of household heterogeneity with regard to consumption behavior. In these models, a subset of constrained agents increases consumption in proportion to a temporary, stimulus-induced windfall in income. Unconstrained agents, by contrast, reduce consumption independent from their current income, in line with the permanent income hypothesis. Could our aggregate time-series results mask important underlying composition effects along these lines?

Under highly stylized conditions, a transmission mechanism along these lines is conceivable to be operative within our time-series evidence; despite *aggregate* income remaining conditionally fixed or insignificant (as for the post-84 sample evidence). Hypothetically, if (i) the share of constrained agents was sufficiently large, (ii) the surge of these agents' income was particularly pronounced, and (iii) post-tax income for unconstrained agents was *declining*, the income reaction may wash out on aggregate; paralleled by (iv) consumption dynamics that would have to be dominated by constrained consumers. For instance, if we assumed that spending programs throughout the postwar U.S. history were financed by hikes of taxes exclusively for Ricardian households—keeping the tax burden for rule-of-thumb households unchanged—resulting aggregate dynamics may be consistent with consumption crowding-in despite fixed aggregate income dynamics. However, such a stylized scenario is hard to align with historical experience and U.S. policy conduct; in line with this reasoning, Mertens and Montiel Olea (2018) argue that, for instance, income tax increases exclusively for top incomes constitute a *counterfactual for the U.S. economy.*

To rule out composition effects along these lines more formally, we need to resort to data on income dynamics of constrained versus unconstrained agents. Model-consistent, disaggregate data to conduct such an analysis are, unfortunately, not available for the U.S. economy at business cycle frequencies over our sample period. However, Piketty et al. (2018) provide a valuable source of information on the full distributional characteristics of household income. For the U.S. economy, their database provides consecutive time-series of real, post-tax income developments for the entire income distribution, from 1966 until 2014. We use this disaggregate data (in population-normalized terms) to proxy for borrowing constraints that are at the heart of limited asset market participation models as follows. Specifically, we replace the aggregate time-series of personal disposable income in the benchmark VAR

against each quintile of the income distribution and study potential heterogeneity in the effects of public spending on these different groups of income.[15]

In a first step, we re-estimate the VAR using aggregate income data over the sample period from 1966-2014, for which disaggregate data are available, for comparison (Figure 2, fourth element). Again, we observe consumption crowding-in; and post-tax income responds positively, but insignificantly so. The extra effect of impacting income is even a bit smaller than in the benchmark model, but overall our inference appears to be in tact during this sample. In a second step, we now explore evidence using disaggregate income series (Figure 2, entries five to nine). Aggregate consumption—for which no disaggregate counterpart data do exist—appears to be fairly insensitive to the inclusion of disaggregate income measures by still responding significantly positive to the shock. For the lowest income quintile, income dynamics are larger relative to other quintiles, yet, the impulse response is estimated with high uncertainty. Richer households tend to benefit less from the stimulus in terms of income dynamics, by exhibiting smaller and typically insignificant coefficients. If these results were to mask important composition effects, we should observe sizable marginal effects stemming from the rise of post-tax income for low-earning households; that is, their income position should explain large parts of aggregate consumption crowding-in. The red diamond in the lower panel of the Figure rejects such a narrative: the extra effect that impacting income of these households adds to the consumption response is minuscule. If anything, this marginal effect even reveals the wrong sign, which is due to the fact that the impulse response of low-earning households' income declines over medium horizons, which is not reported in Figure 2. Taken together, we find no evidence of important composition effects in disaggregate income data that our aggregate results may have masked.

3 Fiscal stimulus, consumption, and household debt

How is it possible that consumption responds positively to stimulus programs if income, across its distribution, can not (fully) explain this reaction? Although our VAR framework, by including the budget deficit, accounts for the prominently discussed role of *public* sector debt (e.g., Reinhart and Rogoff, 2010; Reinhart et al., 2012) the model is, up to this point, silent on the role of *private* sector leverage. In the following, we test whether conditional variation in households' indebtedness constitutes a transmission mechanism of fiscal policy in a structural VAR, which

[15]The working hypothesis in this setting is that the fraction of the income distribution representing the lowest-earning households, i.e., the fifth quintile, is likely to capture Non-Ricardians. Disaggregate income data are provided annually, which we interpolate to the quarterly frequency.

may offer a path to structurally corroborate our findings. The notion is that in excess to supporting post-tax income, consumers raise their debt position to finance the observed expansion in household absorption.

By explicitly testing the hypothesis of stimulus propagating via household debt, we link our evidence on consumption crowding-in to the literature documenting large macroeconomic repercussions emerging from changes in household balance sheets or bank credit growth (e.g., Schularick and Taylor, 2012; Mian et al., 2017). Quite striking movements of private debt can be identified during the U.S. mortgage cycle, which marked its peak prior to the Great Recession. We explicitly capture this boom-and-bust episode, which was paralleled by substantial swings in the fiscal policy stance, in our VAR. In doing so, we put the literature on autonomous credit-variation into perspective by analyzing systematic reactions of private debt, conditional on shifts in public spending. Our approach is therefore also closely related to Bernardini and Peersman (2018), who analyze how deviations of domestic non-financial private sector debt-to-GDP ratios from their trend path affect the fiscal output-multiplier in historical U.S. data as a state—yet not as a transmission—variable (see Demyanyk et al., 2019). Cloyne and Surico (2017) use U.K. survey data and document the consumption response to variation in income taxes to be more pronounced for U.K. mortgagors relative to outright home owners; thus stressing the role of private debt as a propagation mechanism for fiscal policy, as we do.

We proceed by (i) proposing extensions to the reduced-form fiscal VAR that allow us to study credit conditions, (ii) discussing adjustments to recover the model's structural form in an attempt to make identification more credible in a macro-financial setting, and (iii) presenting the corresponding findings, before (iv) providing sensitivity analysis within the SVAR-IV framework.

3.1 Modeling the interplay of credit conditions and the fiscal policy stance

To model the joint dynamics of the fiscal policy stance and of fluctuations in private debt markets, we add to the fiscal VAR representation measures of prices and quantities of credit that are relevant for households. In terms of credit volumes, we rely on the subsequent debt stocks from the U.S. Flow of Funds database, which enter the VAR in logged, GDP deflator-normalized, seasonally-adjusted, per capita terms: overall consumer credit granted by banks, the volume of outstanding home mortgage contracts, and overall household indebtedness. In terms of prices for credit, we are not aware of a consistent and consecutive series on lending rates for U.S. households over our sample period that is available at the quarterly frequency. Thus we proxy overall household borrowing conditions by Moody's Baa corporate bond yields, as

in Bachmann and Rüth (2020), and further study, more household-specific, Federal-Housing-Agency-provided mortgage interest rates, as observed in secondary markets. Mortgage interest rates are, however, only available from 1964.

Against the backdrop of our VAR's rich specification, comprising four lags and nine variables (including fiscal news for which we had not reported IRFs), we rotate one pair of credit market quantities and prices jointly into the VAR, once at a time. Given the typically insignificant nature of the response of hours worked in our VAR, we only proxy labor market conditions in these credit-augmented fiscal VARs by the real wage and abstract from dynamics in hours worked in the estimations for parsimony; that is, the credit-augmented VARs consist of ten variables, measured at the quarterly frequency.[16]

3.2 Identifying fiscal policy shocks in the presence of financial variables

To obtain the unobservable government spending shocks from Equation (1), i.e., to make a structural analysis feasible in our baseline model, we have recovered the parameters in $\mathbf{A_0}$ by a Cholesky-factorization of the reduced-form variance-covariance matrix. While this approach of imposing timing-restrictions appears to be plausible as long as our aim is to orthogonalize shifts in the fiscal policy stance from systematic reactions to the macroeconomic environment, such contemporaneous zero-restrictions are hard to defend in the presence of fast-moving and forward-looking financial time-series. This concern of simultaneity has been acknowledged and addressed by, e.g., Gertler and Karadi (2015) and Caldara and Herbst (2019), for the case of *monetary* policy shocks.[17] We borrow from this strand of literature and tackle the identification challenge, for the case of *fiscal* policy shocks, by employing information from outside of the VAR, i.e., an external instrument. This SVAR-IV methodology allows to recover the unexpected innovations in public spending without necessitating exclusion-restrictions on the contemporaneous relations in the model. By contrast, the identifying information can be obtained from an external proxy-series that correlates with the government spending shock, but is *contemporaneously* uncorrelated with the remaining shocks in the system (see Caldara and Kamps, 2017, for a related strategy involving non-fiscal proxies). Conditional on the discretionary selection by the researcher of such a proxy, the identification is data-determined and the parameters in $\mathbf{A_0}$ can be recovered, even if the VAR comprises

[16]Note that results are insensitive to maintaining hours in the VAR as an eleventh variable.

[17]Moreover, Caldara and Kamps (2017) argue that recursively-recovered fiscal policy shocks may suffer from similar simultaneity problems regarding contemporaneous fluctuations in economic activity. Such reservation may be particularly valid for financial variables, which typically reveal strong leading properties for business cycle movements.

macro-financial linkages. In what follows, we characterize the instrument we will consider, and refer to Stock and Watson (2012) and Mertens and Ravn (2013) for details on the implementation of the SVAR-IV.

Following Barro and Redlick (2011), we employ changes in *actual military spending* relative to lagged real GDP as an external instrument.[18] In our application, by contrast, we use this external information to recover the structural parameters of a fully-specified VAR model and to trace dynamic responses over time (see Miyamoto et al., 2019, for a local projections approach using military spending as an instrument). Why should such variation in military expenditures serve as an external instrument? The answer is: because it plausibly meets the so-called relevance and exclusion restrictions of a proper instrument. With regard to instrument relevance, military spending displays substantial swings in the sample and contributes substantially to the variability in overall fiscal spending; more importantly, it plausibly exhibits comovement with the unobserved policy innovations of interest in $\boldsymbol{\varepsilon_t}$. In fact, when we calculate the correlation of the military spending instrument with the Cholesky-identified shocks from Section 2, we obtain significantly positive coefficients. To test the relevance of the instrument more formally and to construct consistent confidence intervals in the SVAR-IV setting, we follow Montiel Olea et al. (2018). In terms of the exclusion restriction, military expenditures are known to be regularly driven by conditions abroad, especially geopolitical instability such as events in Middle East. Consequently, shifts in military expenditures that reflect domestic economic or financial conditions are plausibly not an important driver. This proposition can be corroborated by calculating correlations of the instrument with changes in GDP or interest rates/spreads. Throughout, these coefficients are estimated close-to-zero and statistically insignificantly different from zero. In addition, we propose alternatives to the baseline instrument for robustness in Section 3.4.

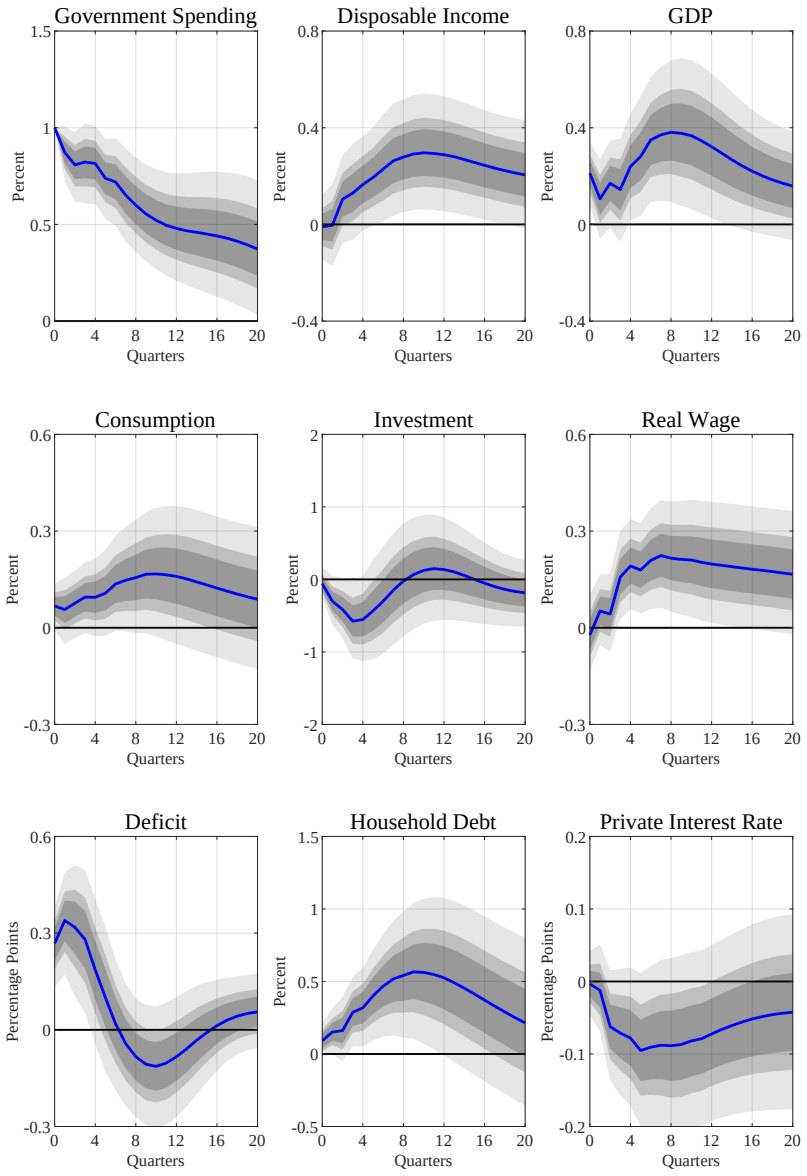
3.3 The interplay of the fiscal policy stance and household credit

In a first step, we formally test the strength of changes in military spending as an instrument. To do so, we calculate a Wald statistic under the null-hypothesis that the instrument is irrelevant, i.e., that it does not correlate with the unobserved government spending innovations. Following the methodology of Montiel Olea et al.

[18] Using *news* about military spending, which foreshadow public spending materializing in the future, is not a viable strategy since we are interested in unexpected shifts in the fiscal policy stance. Moreover, news series are known to be a weak instrument for unexpected short-run variation in public spending (Ramey and Zubairy, 2018). Note, however, that while this variable will not serve as the primary source of information for identification, our VAR will still account for news on military spending in an attempt to tackle fiscal foresight concerns.

(2018), we estimate a Wald statistic of 54.3, which remarkably crosses critical values to reject the Null of a weak instrument, at conventional significance levels. Thus, changes in military spending represent a "strong" instrument for identification. Figure 3 traces adjustment patterns for the credit-augmented fiscal VAR, conditional on an unexpected (one percent) surge of public spending that we identify by changes in military spending as an external instrument; the selected measures of credit market volumes and prices are overall household debt and non-mortgage bond yields.

Figure 3: SVAR-IV public spending shock and credit markets



Notes: We plot dynamics at the quarterly frequency. Solid blue lines represent point estimates of impulse response functions obtained from the SVAR-IV model. Dark to light shaded areas display 50, 68, and 90 percent confidence intervals, which we obtain from the weak-instrument robust procedure in Montiel Olea et al. (2018).

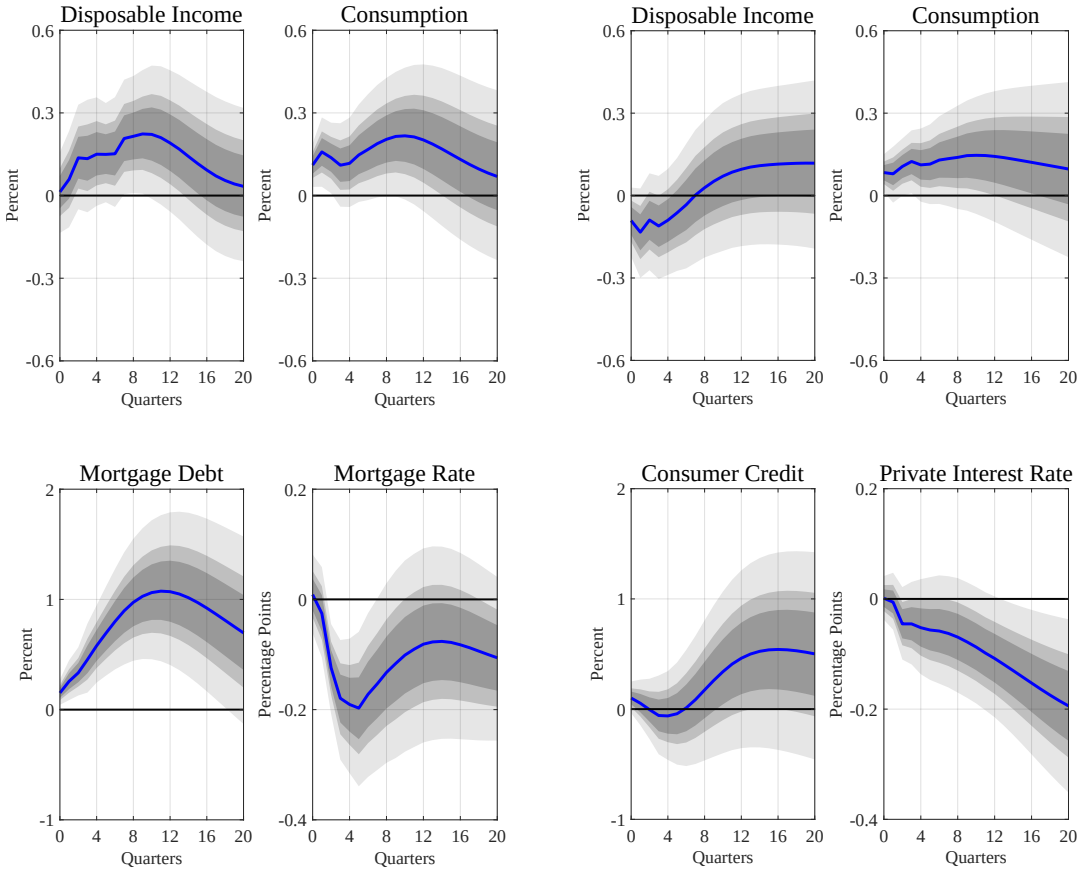
Despite the fact that government spending processes the exogenous shock in a more immediate way, i.e., the impulse response moves faster toward the conditional mean relative to Figure 1 in which the response was hump-shaped, the overall adjustment patterns of the core set of variables are consistent with our main results up to this point. In particular, public stimulus induces a surge in private consumption expenditures, which is paralleled by persistent improvements in post-tax income. Reinforcing the empirical literature on consumption crowding-in, we thus provide evidence that the conditional rise in private spending is a robust empirical regularity, even in a setting that does not require contemporaneous zero-restrictions. In addition, this observation validates our former results from a Cholesky-scheme that had disregarded the critique of Caldara and Kamps (2017) of existing within quarter feedback from economic activity to public spending.

As an additional novel finding, we add to the literature by reporting that fiscal spending significantly propagates through the debt position of households: overall indebtedness increases on impact, slowly builds up until the third post-shock year—reaching its peak response around 0.6 percent above the pre-shock trend-path—before slowly reverting back toward zero. At the same time, borrowing conditions for households, roughly proxied by long-term corporate bond yields, appear to soften. Private interest rates, namely, mirror the reaction of household debt qualitatively, i.e., we observe a hump-shaped decline in credit rates which trough around roughly minus ten basis points, one year after the shock has hit. Related, Ramey (2011) reveals consistent findings in a recursive VAR that she uses to recover military news shocks. Our aggregate evidence on the conditional response of credit markets further aligns with Auerbach et al. (2020), who study local credit markets employing geographical variation in U.S. federal government contracts across U.S. cities.

In Figure 4, we zoom into the components driving these results in more detail, first, by re-estimating the SVAR-IV model using the sub-component of mortgage debt along with mortgage interest rates and, second, by including the consumer credit component along with the benchmark interest rate series. We restrict the presentation to the core set of variables of interest for the sake of a more parsimonious illustration. Panel (a) reveals that the importance of household leverage as an endogenous propagation mechanism of fiscal stimulus is even more sizable when focusing on *mortgage* indebtedness, which is the major component of overall household debt. Mortgage debt wins the horse race in the sense that the magnitude of the mortgage debt reaction exceeds the counterpart reaction of overall debt by increasing around 1.1 percent. In addition, the impulse response is statistically different from zero almost throughout the entire forecast horizon. Correspondingly, mortgage

interest rates process the shock more strongly, as well, and decline by roughly 20 basis points. In Panel (b), the according adjustment patterns for consumer credit are consistent, albeit less pronounced. The impulse response is sticky at short horizons, smaller in absolute magnitude, and estimated with less precision. The interest rate response appears more protracted in this scenario, too. Notably, once we model consumer credit explicitly in the SVAR-IV model, disposable income declines at short horizons and remains statistically insignificant after passing-through its conditional mean into positive territory. The impulse response function of private consumption, however, is fairly insensitive to the inclusion of consumer credit, revealing throughout positive coefficients.

Figure 4: Mortgage debt and mortgage interest & consumer credit and interest rate



(a) Mortgage credit volumes/prices

(b) Consumer credit volumes/prices

Notes: We plot dynamics at the quarterly frequency. Solid blue lines represent point estimates of impulse response functions obtained from the SVAR-IV model. Dark to light shaded areas display 50, 68, and 90 percent confidence intervals, which we obtain from the weak-instrument robust procedure in Montiel Olea et al. (2018). Panel (a) on the left displays IRFs for a fiscal VAR that is augmented by mortgage debt and mortgage interest rates; Panel (b) on the right presents IRFs for a fiscal VAR that is augmented by consumer credit and corporate bond yields.

These observations corroborate our inference that increases in disposable income of households do not suffice to rationalize, why consumption is crowded-in via public stimulus. In particular, we show that there exists a striking leverage that public spending exerts on the debt position of households, raising the latter between 0.5 and one percent, depending on the empirical specification. This finding is remarkable as it implies that, contrary to state-of-the art limited asset participation models, consumers without access to capital markets can not be the only explanation to rationalize crowding-in of private spending. By contrast, the result of surges in household debt implies that also intertemporally optimizing consumers *with* access to credit markets are prompted to take on *more* debt, presumably reinforcing consumption crowding-in. In particular, the dynamics revealed by our VAR stress that this mechanism is likely to be operative particularly via the mortgage debt component of private indebtedness.

Specifically for the U.S. economy, the link of mortgage debt and consumption expenditures—non-durable goods included—is known to be strong, among others, due to the common practice of home equity extraction (see, e.g., Mian and Sufi, 2011, for evidence on household borrowing via the so-called home equity lines of credit).[19] We add to this line of research the finding that the debt channel also kicks in endogenously, when conditioning on exogenous variation in public spending. The prominent role of *mortgage*—and thus long-term—debt in the propagation of the shock further aligns with results from an additional exercise: once we include the durable expenditures component in the baseline consumption variable, crowding-in of private spending is reinforced. The maximum deviation of private spending from its trend increases by approximately 30 percent, relative to the counterpart that excludes durable consumption items.

3.4 Sensitivity analysis for the SVAR-IV setting

Ultimately, we scrutinize our SVAR-IV results along the following dimensions. First, we perform several modifications to the baseline instrument. Instead of expressing changes in military spending relative to lagged real GDP (Barro and Redlick, 2011), we (i) use the civilian population as the normalizing series instead; (ii) we study raw changes in military spending; and (iii) we employ the residual from an AR(1)-regression applied to the benchmark instrument. These adjustments barely affect the impulse response dynamics; the corresponding results are available upon request

[19]There is ample empirical evidence documenting that mortgage financing for households has become a driving factor in commercial banks' lending to the household sector, with the share of mortgage loans on banks' balance sheets having doubled in advanced economies over the course of the twentieth century (Jordà et al., 2016).

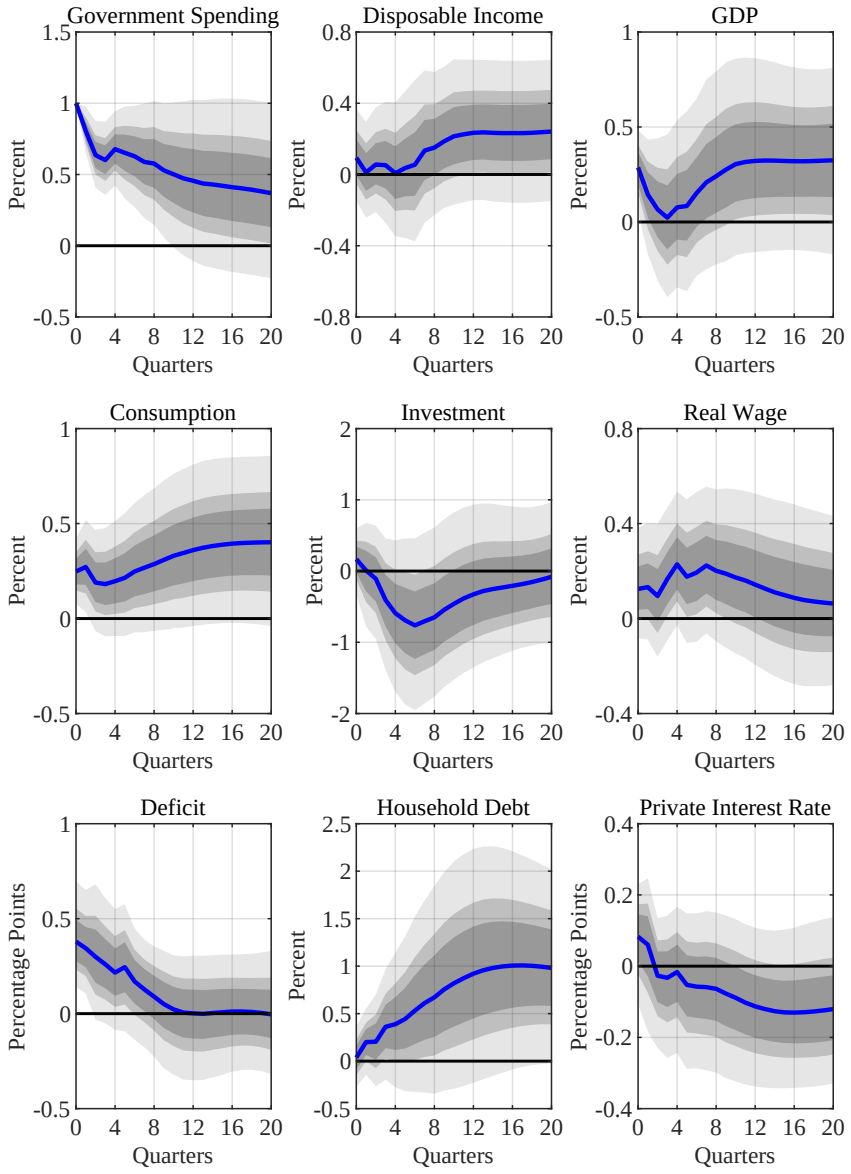
from the authors. Second, as an alternative to using military spending, we introduce professional forecast errors, g_t^{fe}, on *overall* public spending as an instrument into the VAR model, as proposed in Auerbach and Gorodnichenko (2012). While these authors identify unanticipated variation in public spending by modeling forecast errors in a recursive VAR with g_t^{fe} ordered first, we, by contrast, deploy this information as an instrument. As explained above, the SVAR-IV strategy appears to be more appropriate in our setting, as it is insensitive to the ordering of variables and relaxes contemporaneous exclusion-restrictions, thus capturing the simultaneous interplay of macroeconomic and financial variables that enter our VAR.

In a first step, we calculate the Wald statistic to test the strength of this alternative instrument, which amounts to 29.2, i.e., forecast errors are likely not subject to weak instrument concerns.[20] In a second step, we track in Figure 5 the dynamics of a one percent increase in public spending that is identified by forecast errors of professional forecaster. Overall, our inference does not change in this alternative specification, although the impulse response coefficients are estimated with somewhat less precision. Interestingly, the conditional cycle of household debt is quantitatively more pronounced relative to Figure 3; the maximum deviation from the conditional mean exceeds 1 percent. After an initial spike, credit rates ease by more than 10 basis points over the medium run.

As a final modification to our baseline SVAR-IV setting, we analyze to what extent the military-proxy can be used to recover innovations in more disaggregate government spending data, i.e., we separately identify government consumption and government investment shocks. The respective Wald statistics amount to 36.3 and 33.7, respectively; that is both disaggregate surprises in public spending can be recovered via a strong military spending instrument. The triggered dynamics of these shocks with respect to household debt and credit rates are depicted in Figure 6. The expansion in credit appears to be similar in size for both shocks, albeit the shock procession is more persistent in the case of the innovation in government investment. At the same time, interest rates ease significantly for the government consumption shock, whereas the corresponding impulse response is rather flat in the case of a surprise in government investment.

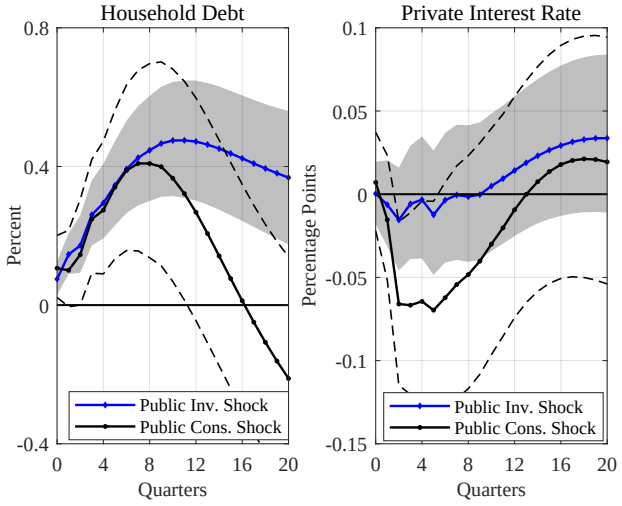
[20]Note that the forecast error data is only available from 1966Q3. The corresponding Wald statistic for our benchmark military spending instrument over the same starting in 1966Q3 is 44.5.

Figure 5: SVAR-IV public spending shock using professional forecast errors



Notes: We plot dynamics at the quarterly frequency. Solid blue lines represent point estimates of impulse response functions obtained from the SVAR-IV model. Dark to light shaded areas display 50, 68, and 90 percent confidence intervals, which we obtain from the weak-instrument robust procedure in Montiel Olea et al. (2018). Due to data availability, the sample covers data ranging from 1966Q3 to 2008Q4, as in Auerbach and Gorodnichenko (2012).

Figure 6: Disaggregate public spending shocks: investment versus consumption



Notes: We plot dynamics at the quarterly frequency for impulse response functions from the SVAR-IV that is identified by forecast errors on government spending (Auerbach and Gorodnichenko, 2012). Solid blue lines (with diamonds) represent point estimates for a government investment shock and shaded areas display the corresponding 68 percent confidence intervals (Montiel Olea et al., 2018). Solid black lines (with circles) represent point estimates for a government consumption shock, and confidence intervals are given as dashed black lines.

4 The transmission mechanism of stimulus to private debt

In this Section, we ultimately provide some tentative insight into the transmission mechanisms that drive our novel aggregate results, i.e., we offer some first-path guidance on what actually underlies the conditional *surge* of households' debt position that helps to sustain consumption. To study such propagation channels, we add to the SVAR-IV model from Figure 3 one additional time-series at a time and report the dynamics for this variable in isolation.

Fisher effects For instance, Fernández-Villaverde (2010) argues that in the presence of financial frictions and multi-period nominal debt contracts, "Fisher effects" may kick in; that is, boosts in inflation may reduce finance premia for borrowing money and thus stimulate debt accumulation and amplify the macroeconomic repercussions of fiscal stimulus. We scrutinize this proposition by incorporating the log of the GDP-deflator and, alternatively, the PCE-deflator into the SVAR-IV model. Panel (a) of Figure 7 illustrates the corresponding impulse response functions, which both reveal a negative, hump-shaped procession of the shock. A surge in public spending hence unleashes dis-inflationary dynamics in our setting, which aligns with empirical findings of, e.g., D'Alessandro et al. (2019). However, while these authors

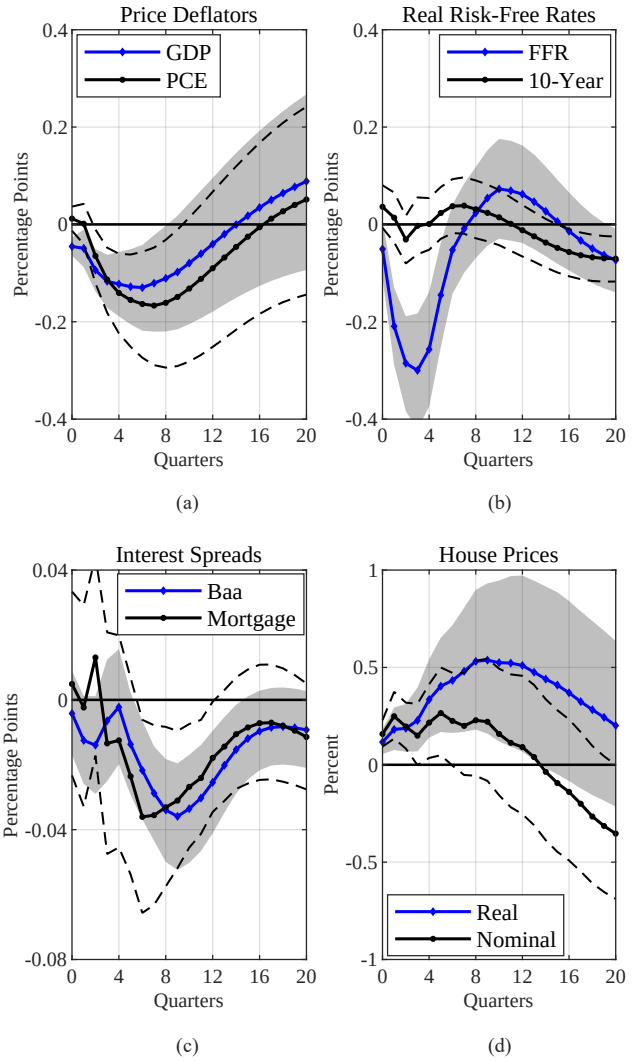
rely on a Cholesky-identification, our external instruments approach—allowing for simultaneous feedback from prices to government spending—still establishes the disinflation result (see Zubairy, 2014, for an estimated DSGE model). Put together, it is unlikely that the conditional debt cycle we observe is driven by Fisher effects.

Passive monetary policy Another related mechanism that is typically emphasized in theoretical strands of the literature is that active monetary policy dampens the effects of fiscal stimulus (e.g., Christiano et al., 2011), which may particularly extend to its influence on private credit markets. Considering the declining price levels we have established above in conjunction with conventional central bank reaction functions, such counteracting factors appear to be unlikely to apply in our setting, ex ante. To test such a narrative, we study the conditional dynamics of risk-free rates, i.e., the Federal funds rate and 10-year Treasury yields, in real terms.[21] Panel (b) of Figure 7 shows that in line with the first panel of the Figure, monetary policy softens and interest rates at the longer end of the yield curve do not reveal significant cost pressure for credit markets as well. These results, in addition, hold for *nominal* interest rates (not reported). Overall, we do not observe tightening financial conditions, as proxied by risk-free rates, that may depress equilibrium debt. The evidence is instead consistent with the expansion in credit markets that we document.

Softening of credit market constraints Closely related to the mechanism stressed by Fernández-Villaverde (2010), Carrillo and Poilly (2013) argue that fiscal policy, if propagated via imperfect financial markets, may more directly support credit conditions. By stimulating economic activity and by supporting asset prices, public spending may inflate collateral values of borrowers (firms in their case). As a consequence and due to improved balance sheets of borrowers, their access to credit eases, which precipitates in a compression of credit spreads. We inspect such a mechanism by, first, studying to what extent fiscal stimulus widens or narrows interest rate spreads in credit markets. We do so, by analyzing long-term interest rates in relative terms to, e.g., the Treasury yield following convention (in the case of the mortgage rate) or Moody's Aaa bond yield (in the case of the Baa yield as in Gilchrist and Zakrajšek, 2012, to alleviate cash-flow or duration mismatch). Second, we track the dynamics of house prices, in real and nominal terms, as provided by Shiller (2005), to evaluate how collateral values of households absorb the surge in public spending.

[21] We follow Gilchrist and Zakrajšek (2012) and calculate nominal interest rates relative to the lagged growth rate of the PCE-deflator. Our results are robust to using the shadow rate of Wu and Xia (2016) instead of the nominal Federal funds rate; alternatively, restricting the analysis to pre-Great Recession data does not affect the result.

Figure 7: Inspecting the transmission channel of fiscal spending to credit markets



Notes: We plot dynamics at the quarterly frequency. Solid blue lines (with diamonds) represent point estimates of impulse response functions for the variable denoted in the respective panel legend, which we obtain from the SVAR-IV model. Shaded areas display 68 percent confidence intervals (Montiel Olea et al., 2018). In each panel, we study an alternative measure (see legend in each panel) for which the point estimates are represented by solid black lines (with circles), and confidence intervals are given as dashed black lines.

Panels (c) and (d) of Figure 7 present the corresponding results. Panel (c) is suggestive to easier lending conditions being part of the story, as we observe a hump-shaped drop in credit spreads. Beyond potential shifts in the sovereign yield curve—as depicted in Panel (b) of Figure 7—a financial accelerator mechanism thus appears to be at work (Bernanke et al., 1999). Baa corporate bond yields measured relative to their Aaa corporate bond yield counterpart, deviate negatively from their conditional mean, reaching their trough in the third year after the shock, before

leveling off. The result of narrowing spreads also extends to spreads for mortgage credit.[22] The last panel of the Figure further reveals significant asset price inflation; the level of real estate prices rises on impact, both in nominal and real terms. Real house prices subsequently rise by more than 0.5 percent, two years after the shock. These dynamics make a case for a collateral channel through which fiscal stimulus compresses credit spreads and impacts on households' debt position, in the presence of financial frictions (see, e.g., Carrillo and Poilly, 2013).

5 Conclusion

Can public spending stimulate the economy and if so, how? These questions are some of the oldest and certainly most important ones, around which large parts of the history of macroeconomics have centered and which received renewed attention during the rapidly unfolding economic disruptions at the onset of the Great Recession. A crucial mechanism that policymakers often seek to activate in order to boost economic activity, is triggering private via public spending. For instance, fiscal stimulus *payments*, such as the tax rebates ranging between $ 500 and $ 1,000 that U.S. Congress authorized during the economic slowdowns of 2001 and the Great Recession, can be viewed as a type of public intervention that was directly intended to raise household absorption. Of course, the success of stimulating private spending—the largest component of aggregate demand—through public stimulus critically hinges on the specific calibration and composition of the public spending program under consideration. Unfortunately, such interventions may not always cause the behavioral adaptions policymakers intend to induce; in this vein, Hoekstra et al. (2017) provide evidence that the 2009 $ 3 billion Cash for Clunkers scrappage program, which was—apart from the idea to put safer and more fuel-efficient vehicles on U.S. roadways—explicitly tailored to promote private spending, might actually have reduced net total vehicle spending by $ 5 billion.

The good news is, however, that empirical evidence by a vast number of time-series contributions supports the notion of *unexpected* shifts in fiscal spending significantly raising private consumption expenditures for aggregate data and on average across programs, in *postwar* U.S. data. The bad news is, however, we structurally still do not satisfactorily understand why consumption reacts in this way. While crowding-in is at odds with the predictions of plain-vanilla New-Keynesian models,

[22] These findings put the results of Born et al. (2020) into perspective, who document a widening of the *sovereign* default premium in response to a *cut* in public spending in a sample of 38 countries, on average over the business cycle. They establish their findings using exclusion-restrictions that identify their VAR.

there is a fast growing literature that tries to rationalize this empirical regularity. Yet, with all contributions offering "*one* solution to a fiscal policy puzzle" (Bilbiie, 2011), the transmission mechanism of public to private spending is still not well-understood. Consequently, rigorous empirical testing of alternative theoretical approaches is key to a better understanding of the propagation of fiscal stimulus.

In this paper, we provide comprehensive empirical evidence that the most widely-adopted modeling device of rationalizing consumption crowding-in by giving disposable income of households a meaningful role, may be insufficient once judged from an empirical perspective. In fact, we observe consumption crowding-in effects, even in the absence of movements in disaggregate and aggregate measures of post-tax income, in postwar U.S. data. Complementing this finding, we test the hypothesis, whether variation in household indebtedness may reinforce the pass-through of public to private spending; thereby we further corroborate the finding that post-tax income may be an insufficient empirical moment to study. We do so, by carefully modeling the simultaneous interplay of the fiscal policy stance, household consumption, after-tax income, and private credit markets in a structural VAR model that is identified by an external instrument.[23] Indeed, we observe a striking role for public spending to prompt surges in the debt position of households; this leverage that fiscal policy exerts on credit markets appears to be particularly strong for the mortgage component of household indebtedness. In addition, the significant household debt cycle and the crowding-in of household absorption are paralleled by declining interest rates in credit markets. This conditional divergence of prices and quantities in credit markets is suggestive to accommodating financial conditions underlying our results. To better understand this mechanism, we provide some first-path guidance: First, we do not find counteracting effects stemming from risk-free rates, such as the Fed's policy instrument or 10-year Treasury yields. Second, since we observe declining price levels, our results are unlikely to be driven by Fisher effects. Third, we reveal a narrowing of interest spreads in credit markets and, fourth, public stimulus significantly improves real estate prices. The latter two results prompt the view of looser collateral constraints—brought about by rising collateral prices—and thus easier access to credit markets for households, reinforcing the conditional comovement of private spending and the debt position of the household sector.

To put our paper into perspective, we emphasize that for an analysis of the macroeconomic repercussions of public stimulus, more generally, it is vital to care-

[23]We thus also add to the literature the observation that consumption crowding-in prevails in a time-series setting that abstracts from zero- or sign-restrictions imposed to recover the VAR's structural form.

fully address the question of how public spending propagates into private credit markets. Future research should explicitly take into account the dynamic interactions we have identified and should attempt to improve our knowledge on how credit supply and demand conditions react to public stimulus. Making progress toward that direction is crucial to better inform the calibration of and modeling strategies for theoretical approaches that aim to inform policymakers. In addition, further work on the transmission channel of public spending into private credit markets may provide a clearer picture on how discretionary fiscal policy can be used as a tool for macroeconomic stabilization. Specifically, in the presence of a private debt channel, countercyclical fiscal policy may be desirable during economic downturns not only due to conventional mechanisms, but because of the stabilizing effect it may exert on private credit markets. In the case of recessions that are triggered or accompanied by pronounced private sector deleveraging, such considerations may be of first-order importance.

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