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The Long-Run Effects of Government Spending†

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Military spending has large and persistent effects on output because it shifts the composition of public spending toward R&D. This boosts innovation and private investment in the medium term and increases productivity and GDP at longer horizons. Public R&D expenditure stimulates economic activities beyond the business cycle even when it is not associated with war spending. In contrast, the effects of public investment are shorter-lived, while public consumption has a modest impact at most horizons. We reach these conclusions using BVAR with long lags and 125 years of US data, including newly reconstructed series of government spending by main categories since 1890. (*JEL* E21, E22, E23, E62, H50, H56, O30)

Can government spending stimulate long-run growth? Large increases in public expenditure—typically associated with defense buildups around wars—have often been credited with the development of new technologies. For instance, the Manhattan Project during WWII led to the development of nuclear energy, the establishment of the Defense Advanced Research Projects Agency (DARPA) in the late 1950s is linked to the creation of the Internet, and NASA's moon landing program of the 1960s spurred several advances in aeronautics and satellite technology, such as GPS. Despite this anecdotal evidence, the macroeconomics literature has not yet established a causal link between large government programs and long-term productivity, innovation, and growth at the aggregate level.

Using the series of military spending news constructed by Ramey and Zubairy (2018) (which builds on Ramey and Shapiro 1998; Ramey 2011b), we find that the effects of an unanticipated increase in defense spending are large and extend well beyond the frequencies typically studied in business cycle analyses. The output multiplier (i.e., the dollar increase in GDP that results from a dollar increase in government spending) is around one in the short run but rises significantly above

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one at long horizons. Total factor productivity (TFP), innovation, consumption, and investment fall in the first years after the shock but then recover and increase persistently over the medium term.[1]

As for the transmission mechanism, we present evidence that military spending stimulates the economy over the medium term because it shifts the composition of public spending toward R&D. While in the short run government consumption, investment in equipment and structures, and R&D all increase following military spending, public R&D is the only category that still responds significantly 10 years after the shock. To tease out the different effects across components, we use an alternative strategy that identifies the shock that maximizes the variance of each spending category within the first year after the shock. We find that persistent increases in output and TFP are associated with shocks that expand the share of government spending going to R&D.

Finally, we scrutinize our newly identified "public R&D shock" and show not only that it is weakly correlated with war spending but also that its historical evolution aligns well with narrative evidence on large R&D federal spending programs, including the Manhattan Project, DARPA, the moon landing program, and Reagan's "Star Wars" initiative. Furthermore, we document that an exogenous increase in public R&D leads to very sustained responses in output, TFP, innovation, and investment that are even larger and more persistent than the effects of military spending on these variables. Our results highlight a new channel through which fiscal policy can support economic activities in peacetime.

Identifying very persistent dynamics requires long, high-quality historical data and empirical methods suited to capture low-frequency correlations. As for the historical data, we have digitized archival statistics and drawn upon narrative evidence to construct new quarterly series of US government spending since 1890, by main categories: consumption expenditure, equipment and structures investment, and R&D. We have also constructed quarterly series for aggregate hours worked, total factor productivity, private investment and consumption, export and imports, building on existing and unpublished annual and quarterly data. This allows us to examine the effects of government spending at any relevant frequency over a period of 125 years that spans major military conflicts and public spending programs, financial crises and recessions, and monetary policy and fiscal policy regimes.

As for the empirical method, we rely on Bayesian Vector Autoregressions (BVAR) with a very long lag structure to compute dynamic causal effects. This approach allows us to capture the gradual patterns of technological diffusion after increases in R&D. It also connects us with the debate in empirical macro about the relative merits between VARs and direct single-equation regressions, known as "local projections" (LPs) (Jordà 2005; Kilian and Lütkepohl 2017; Nakamura and Steinsson 2018). Recent work has highlighted the intimate connection between the two approaches, and in particular, their coincidence up to the lag-order of the VAR (Plagborg-Møller and Wolf 2021). Moreover, Li, Plagborg-Møller, and Wolf (2021b) highlight the nontrivial bias-variance trade-off inherent to the choice between methods, and the attractiveness of shrinkage estimators in this context. We

[1] Throughout the paper, we will use interchangeably "low frequency," "long lasting," "long run," "medium term," "intermediate," and "long horizons" to refer to persistent dynamics that extend beyond business cycle frequencies.

set the lag order of the BVAR equal to 60 quarters, our maximum horizon of interest in the impulse responses, and employ shrinkage to maximize the marginal likelihood of the model (as in Giannone, Lenza, and Primiceri 2015), balancing these statistical considerations. We show that our main findings of significant effects of fiscal policy on output and productivity beyond business cycle frequencies are a robust feature of the US data that emerge also when we (i) exclude WWII or any other cluster of military events from the instrument, (ii) employ alternative model and prior specifications, or (iii) use alternative econometric methods, such as LPs. Finally, a Monte Carlo analysis confirms that our empirical framework has no tendency to spuriously detect long-run effects when those are not present in the data-generating process.

Related Literature.—A large empirical literature has studied the macroeconomic effects of government spending on output over the business cycle. A key challenge is to isolate movements in public expenditure that are exogenous to economic conditions. Leading approaches have used narrative evidence (Ramey and Shapiro 1998), timing restrictions (Blanchard and Perotti 2002), sign restrictions (Mountford and Uhlig 2009), and geographical variation (Nakamura and Steinsson 2014; Chodorow-Reich 2019). In two comprehensive reviews, Ramey (2011a, 2019) summarizes the literature and concludes that the government spending multiplier lies between 0.6 and 1.5, across the reviewed papers. The focus on frequencies beyond the business cycle is a distinctive feature of our analysis.

An important strand of research has focused on the impact of public spending on productivity. Moretti, Steinwender, and Van Reenen (2025) and Deleidi and Mazzucato (2021) find that military expenditure fosters private innovation, while Gruber and Johnson (2019); Gross and Sampat (2023); Diebolt and Pellier (2020); and Ilzetzki (2024) document the long-lasting effects of the two world wars on US patenting and productivity. Kantor and Whalley (2023) show that the Space Race with the Soviet Union of the 1960s had persistent effects on manufacturing growth across US counties. Our historical analysis extends these event studies to a much longer sample and forecast horizon, using a different identification; furthermore, it shows that public R&D can stimulate productivity and output even in peacetime. This latter finding has been recently echoed by De Lipsis et al. (2022) and Fieldhouse and Mertens (2023), who report significant effects of post-WWII public R&D on US output and TFP.[2]

Our results also speak to the public infrastructure research surveyed by Ramey (2020). Fernald (1999) and Leff Yaffe (2020) find that the US interstate highway program boosted industry-level productivity, while Donaldson and Hornbeck (2016) and Hornbeck and Rotemberg (2021) estimate that the US national railroad network improved market access. We complement these studies by showing that public investment in equipment and structures tends to have smaller effects than public R&D at long horizons.

A growing literature, surveyed by Cerra, Fatás, and Saxena (2022), studies the long-lasting effects of demand shocks. Comin and Gertler (2006) and Beaudry,

[2] See Janeway (2012) and Mazzucato (2013) for earlier popular writings on the role of public spending in innovation.

Galizia, and Portier (2020) lay out models with strong internal propagation mechanisms in which nontechnology shocks have effects beyond the business cycle. Benigno and Fornaro (2018) focus on stagnation traps triggered by weak aggregate demand. Jordà, Singh, and Taylor (2020) exploit the international finance trilemma to identify the long-run effects of monetary policy. Akcigit et al. (2022) study the impact of income taxes on innovation and researchers' mobility across US states. Cloyne et al. (2025) estimate persistent responses of R&D, productivity, and GDP to corporate and personal tax changes. Our analysis offers a novel evaluation of the effects of government spending on the aggregate economy at long horizons.[3]

Structure of the Paper.—In Section I, we present our empirical framework, the historical data and the identification strategy. The main findings on productivity, output and the fiscal multiplier are reported in Section II, while, in Section III, we assess the robustness of our low-frequency inference using different samples, model specifications, and econometric methods. Exploring the transmission mechanism of fiscal policy working through the different categories of private and public spending is the focus of Section IV, whereas, in Section V, we contrast the large and long-lasting effects of public R&D shocks with the small and shorter-lived impact of public consumption and public investment innovations. Conclusions are discussed in Section VI. In the Supplemental Appendices, we provide details on the estimation and present further analyses.

I. Empirical Framework

In this section, we motivate the empirical model and the estimation strategy that we propose, including prior and lag length selection. We then present the historical data for the United States and review the identification of government spending shocks based on the military spending news constructed by Ramey (2011b) (which in turn builds upon Ramey and Shapiro 1998) and extended back in time by Ramey and Zubairy (2018). We complement their dataset with extended series for business investment, productivity, patents, consumption, exports, imports, and government spending broken down into its three main categories, including public R&D.

A. *Model Specification and Estimation*

We use a Vector Autoregressive (VAR) model to conduct inference on the effects of government spending on economic activities. The model can be written as

$$(1) \qquad \mathbf{y}_t'\mathbf{A}_0 = \sum_{\ell=1}^{p} \mathbf{y}_{t-\ell}'\mathbf{A}_\ell + \mathbf{c} + \varepsilon_t' \quad \text{for } 1 \leq t \leq T,$$

[3] Following the Great Recessions of 2007–2009, an independent literature has shown that financial shocks can have long-lasting effects on the economy in business cycle models with financial frictions and endogenous total factor productivity. Prominent examples include Anzoategui et al. (2019); Bianchi, Kung, and Morales (2019); Guerron-Quintana and Jinnay (2019); Ikeda and Kurozumi (2019); and Queralto (2020), among many others.

where $\mathbf{y}_t$ is an $n \times 1$ vector of variables, $\boldsymbol{\varepsilon}_t$ is an $n \times 1$ vector of structural shocks, and $\mathbf{A}_\ell$ is an $n \times n$ matrix of parameters for $0 \leq \ell \leq p$, with $\mathbf{A}_0$ invertible. The vector of parameters $\mathbf{c}$ has dimension $1 \times n$ and the letter p refers to the lag length, whereas T denotes the sample size. The vector ε_t, conditional on past information and the initial conditions $\mathbf{y}_0, \dots, \mathbf{y}_{1-p}$, is Gaussian with zero mean and covariance matrix $\mathbf{I}_n$, the $n \times n$ identity matrix.

Denoting $\mathbf{A}'_+ \equiv \left[\mathbf{A}'_1 \cdots \mathbf{A}'_p \mathbf{c}'\right]$, the reduced-form representation implied by equation (1) is $\mathbf{y}'_t = \sum_{\ell=1}^{p} \mathbf{y}'_{t-\ell}\mathbf{B}_\ell + \mathbf{d} + \mathbf{u}'_t$ for $1 \leq t \leq T$, or more compactly, $\mathbf{y}'_t = \mathbf{x}'_t\mathbf{B} + \mathbf{u}'_t$, where $\mathbf{x}'_t = \left[\mathbf{y}'_{t-1}, \dots, \mathbf{y}'_{t-p}, 1\right]$, $\mathbf{B} = \mathbf{A}_+\mathbf{A}_0^{-1}$, $\mathbf{d} = \mathbf{c}\mathbf{A}_0^{-1}$, $\mathbf{u}'_t = \boldsymbol{\varepsilon}'_t\mathbf{A}_0^{-1}$, and $E\left[\mathbf{u}_t\mathbf{u}'_t\right] = \boldsymbol{\Sigma} = \left(\mathbf{A}_0\mathbf{A}'_0\right)^{-1}$. The matrices $\mathbf{B}$ and $\boldsymbol{\Sigma}$ are the reduced-form parameters, while $\mathbf{A}_0$ and $\mathbf{A}_+$ are the structural parameters. Similarly, $\mathbf{u}'_t$ are the reduced-form innovations, while $\boldsymbol{\varepsilon}'_t$ are the structural shocks. The shocks are orthogonal and have an economic interpretation, while the innovations are typically correlated and have no interpretation.

For computational simplicity and in order to preserve degrees of freedom, we assume time-invariant coefficients and Gaussian, homoskedastic innovations. Our macroeconomic data span 125 years and therefore are very likely to exhibit some form of time variation in parameters and volatilities (see, for instance, Sargent and Surico 2011). Given our interest on effects over horizons of up to 15 years, however, this leads to just 8 nonoverlapping samples, and hence, we refrain from any attempt to model time variation. Our impulse response estimates can thus be interpreted as averaging across different macroeconomic regimes over the sample. Furthermore, our impulse-response functions will be consistent even in the presence of heteroskedastic and non-Gaussian errors (Montiel Olea, Plagborg-Møller, and Qian 2022).

In the VAR setting, impulse-response functions (IRFs)—and related objects of interest, such as government spending multipliers, forecast error variance decompositions, etc.—are computed by recursively iterating on the VAR coefficients, $\boldsymbol{\Theta} = \left(\mathbf{A}_0, \mathbf{A}_+\right)$.[4] However, in recent years it has become increasingly popular to compute IRFs using direct regressions of the variable of interest in period $t + h$ on a measure of an identified shock at time t, as well as on control variables. As shown by Jordà (2005), these "local projections" can be written as

$$y_{i,t+h} = \alpha_h + \beta_h \hat{\varepsilon}_t^1 + \psi_h(L)\,\mathbf{z}'_t + \nu_{t+h} \quad \text{for } h = 0, 1, \dots, H, \tag{2}$$

where $\hat{\varepsilon}_t^1$ is a proxy for the identified shock. For comparability and without loss of generality, we assume that the shock in the local projection (2) corresponds to the first shock in the VAR (1).

There has been considerable debate in the literature about the relative advantages of VAR versus local projection (LP) estimates of impulse responses.

[4] For instance, given a value $\boldsymbol{\Theta}$ of the structural parameters, the IRF of the i-th variable to the j-th structural shock at horizon k corresponds to the element in row i and column j of the matrix $\mathbf{L}_k(\boldsymbol{\Theta})$, defined recursively by

$$\mathbf{L}_0(\boldsymbol{\Theta}) = \left(\mathbf{A}_0^{-1}\right)', \quad \mathbf{L}_k(\boldsymbol{\Theta}) = \sum_{\ell=1}^{k}\left(\mathbf{A}_\ell\mathbf{A}_0^{-1}\right)'\mathbf{L}_{k-\ell}(\boldsymbol{\Theta}), \text{ for } 1 \leq k \leq p,$$

$$\mathbf{L}_k(\boldsymbol{\Theta}) = \sum_{\ell=1}^{p}\left(\mathbf{A}_\ell\mathbf{A}_0^{-1}\right)'\mathbf{L}_{k-\ell}(\boldsymbol{\Theta}), \text{ for } p < k < \infty.$$

Plagborg-Møller and Wolf (2021); Montiel Olea and Plagborg-Møller (2021); and Li et al. (2021b) clarify important conceptual and practical aspects and conclude that the two approaches estimate the same impulse responses in population. In particular, their estimands approximately coincide up to horizon p (the maximum lag length of the VAR). Furthermore, standard confidence intervals based on lag-augmented LP have correct asymptotic coverage, uniformly, over the persistence in the data-generating process and over a wide range of horizons. Finally, in small-sample applications, a trade-off emerges between the higher bias of low-order VARs and the higher variance of LPs, such that shrinkage estimators—e.g., Bayesian VARs or penalized LPs (Barnichon and Brownlees 2019)—become attractive.[5] In our context, with nonstationary variables and cointegrating relationships, Bayesian VARs are an effective tool to address the finite sample bias that characterizes autoregressions containing unit roots via priors elicited on the system as a whole (Doan, Litterman, and Sims 1984; Sims, Stock, and Watson 1990; Sims 1993; Sims and Zha 1998; Giannone, Lenza, and Primiceri 2015, 2019). This compares favorably with single-equation methods like LPs.

Our focus on low frequencies requires a careful consideration of the small sample bias-variance trade-off highlighted by Li, Plagborg-Møller, and Wolf (2021a). To balance these two considerations, we set the lag length of our baseline VAR to $p = 60$. The rationale for this choice is twofold. First, we want to look at horizons well beyond the eight years traditionally associated with business cycle frequencies. Second, we are interested in capturing potentially long lags in the diffusion of technological advances after a surge in R&D spending.

As for inference, we take a Bayesian approach and apply priors that shrink coefficients toward zero at a rate that exponentially increases with the more distant lags, in the spirit of the "Minnesota" priors of Doan, Litterman, and Sims (1984) and Sims (1993). The generous choice of lag length brings the impulse responses of the VAR close to what would have been obtained with lag-augmented LPs, whereas the use of shrinkage allows us to mitigate the increase in variance stemming from the very large number of parameters involved. It is worth noting that the Minnesota priors place a heavier shrinkage on more distant lags (centered around the value of zero), and therefore, the data need to speak strongly about the presence of low-frequency dynamics to counteract the a priori view that these are likely absent. Further details on the specification of the prior are given below.

B. *Prior Specification and Posterior Sampling*

We will use a Normal-Inverse Wishart prior over the reduced-form parameters, $(\mathbf{B}, \boldsymbol{\Sigma})$. This family of distributions is conjugate for this class of models and is the standard choice in empirical work due to its computational tractability (see, for instance, Uhlig 2005; Giannone, Lenza, and Primiceri 2015). Denoting $\mathbf{b} = vec(\mathbf{B})$, the prior distribution is $NIW(\underline{\nu}, \underline{\boldsymbol{\Psi}}, \underline{\mathbf{b}}, \underline{\mathbf{V}})$ As discussed above, we employ the "Minnesota" priors proposed by Doan, Litterman, and Sims (1984), which shrink the VAR coefficients toward simple univariate specifications. In particular, the degrees

[5] Penalized LPs minimize the sum of squared forecast errors plus a penalty term that encourages IRF smoothness.

of freedom of the prior covariance matrix are set to $\underline{\nu} = n + 2$, with $\underline{\boldsymbol{\Psi}}$ a diagonal matrix whose j-th diagonal element is ψ_j.[6] As for the autoregressive coefficients, the prior has the following mean and variance:

$$(3) \qquad E\left[(\mathbf{B}_\ell)_{i,j} | \boldsymbol{\Sigma}\right] = \begin{cases} \delta, & \text{if } j = 1 \text{ and } \ell = 1 \\ 0, & \text{otherwise} \end{cases}$$

$$(4) \qquad \operatorname{cov}\left[(\mathbf{B}_\ell)_{i,j}, (\mathbf{B}_m)_{r,k} | \boldsymbol{\Sigma}\right] = \begin{cases} \lambda^2 \dfrac{1}{\ell^\alpha} \dfrac{\Sigma_{i,r}}{\psi_j / (\underline{\nu} - n - 1)}, & \text{if } j = k \text{ and } \ell = m \\ 0, & \text{otherwise} \end{cases}.$$

The parameter δ, which is the mean of the autoregressive coefficient corresponding to the first lag, is set to 1 for trending variables, to 0.9 for stationary but persistent variables, and to 0 for other variables. As discussed by Del Negro and Schorfheide (2011), among others, the hyperparameter λ controls the overall tightness of the Minnesota prior, whereas the term $1/\ell^\alpha$ implies that more distant lags are shrunk at an exponentially increasing rate toward zero, with the hyperparameter α determining how aggressively longer lags are penalized. Therefore, the Minnesota prior penalizes rich large structures and favors models with shorter lags and "smooth" impulse responses.

Because our dataset contains a mix of stationary and nonstationary variables, we combine the Minnesota prior with the "Single Unit Root" prior proposed by Sims (1993) and Sims and Zha (1998). This prior addresses the problem of the excessive explanatory power of initial conditions and deterministic components, which translates into downward bias in the persistence of autoregressive coefficients (see Sims and Uhlig 1991; Sims 2000; Jarociński and Marcet 2014; Giannone, Lenza, and Primiceri 2019). It is usually implemented by appending an artificial ("dummy") observation for $\mathbf{y}$ and $\mathbf{x}$, denoted y_* and x_*, respectively, at the beginning of the sample:

$$(5) \qquad \mathbf{y}^*_{1\times n} \equiv \frac{\bar{y}}{\theta}; \quad \mathbf{x}^*_{1\times(n\cdot p+1)} \equiv \left[\frac{1}{\theta}, y_*, \ldots, y_*\right],$$

where $\bar{y}$ is the average of the first p observations and the hyperparameter θ controls the tightness of the prior. A smaller θ implies a tighter prior in favor of unit roots and cointegration in the system as a whole, inducing a priori correlation between the constant and the different lags of the VAR.[7] This combination of priors is widely used in empirical macroeconomics. The conjugate nature of the prior allows us to sample from the posterior distribution in a straightforward way, using the standard algorithm described in the Supplemental Appendix.

In our context where the number of parameters is large relative to the sample size, the choice of prior hyperparameters might become important for the posterior impulse responses. In particular, if λ or θ are large (or α is small), the priors are too

[6] As common, we set $\underline{\boldsymbol{\Psi}}_{j,j} \equiv \psi_j$ to the residual variance of a univariate AR (1) estimated on the full sample.

[7] The likely presence of cointegration in our dataset leads us to not use in our baseline results the other well-known prior used in the empirical macroeconomics literature, known as the "sum of coefficients" prior. See the discussion in Giannone, Lenza, and Primiceri (2019). We explore the role of this prior in Section IIIB.

loose, and the large number of parameters means that IRFs will be estimated imprecisely. On the other hand, as $\lambda, \theta \rightarrow 0$ and/or $\alpha \rightarrow \infty$, medium-term dynamics may be smoothed away by the priors, similar to using a smaller amount of lags. Giannone, Lenza, and Primiceri (2015) propose a theoretically grounded method to optimally choose the prior hyperparameters, based on maximization of the marginal likelihood. Based on this procedure, we select $\lambda = 0.36$, $\alpha = 2$, and $\theta = 0.01$ for our baseline estimates. In Section IIIB and the Supplemental Appendix, we explore in detail the impact of the priors specification on the empirical results, whereas in the Supplemental Appendix we assess the sensitivity of the marginal likelihood to different hyperparameter choices.

C. *Data and Identification*

Our starting point is the dataset in Ramey and Zubairy (2018), which spans the sample 1890:I to 2015:IV and contains the present discounted value of military news (Ramey 2011b), government spending, real GDP, the log GDP deflator, the short-term interest rate, the surplus-to-GDP ratio, and the Debt-to-GDP ratio. In drawing inference about low frequencies, our baseline approach is to express nonstationary variables in log-levels. Sims, Stock, and Watson (1990) show that, even in the presence of cointegration, this specification leads to consistent estimates. When computing government spending multipliers, however, the log-level specification requires scaling the impulse responses by the steady-state value of Y/G. As discussed by Owyang, Ramey, and Zubairy (2013) and Ramey and Zubairy (2018), multiplier estimates can be quite sensitive to this conversion factor measured from historical averages. Accordingly, we also compute output multipliers from alternative models in which GDP and government spending are scaled either by GDP in the previous quarter (as in Barro and Redlick 2011) or by the measure of GDP trend proposed by Ramey and Zubairy (2018): a sixth-degree polynomial for log GDP, 1889:I–2015:IV, excluding 1930:I–1946:IV. The baseline transformation includes an intercept and thus implicitly controls for a linear trend; the second transformation is akin to estimating the VAR in differences, hence removing a stochastic trend; the third transformation has the disadvantage of purging low-frequency movements in potential output that may be particularly important to account for highly persistent effects of government spending. We include it mostly for comparability with the estimates in Ramey and Zubairy (2018).

We extend the baseline data along several dimensions. First, we construct new quarterly series of private consumption and investment, as well as exports and imports. We obtain unpublished annual estimates of investment since 1901 by the Bureau of Economic Analysis. Before that, we rely on the Macrohistory Database of Jordà, Schularick, and Taylor (2017), which also offers a measure of annual private consumption since 1890. We interpolate these series to quarterly frequency using the consumption and investment series from NIPA (after 1947), Gordon (2007) (between 1919 and 1940), and real GDP (before 1919 and from 1941 to 1946), and use import and export data to ensure consistency of our estimates with the national accounts identity. Second, we construct quarterly measures of hours worked and TFP. The annual hours and productivity series comes from Bergeaud, Cette, and Lecat (2016). We adjust TFP for capital and labor utilization following Imbs (1999).

We interpolate this using the quarterly series of adjusted TFP in Fernald (2012) (after 1947) and real GDP (before 1947). The data on patents are by IFI CLAIMS Patent Services via Google Public Data.

In addition, we construct new historical series of public consumption and investment, distinguishing between expenditure in Equipment and Structures (E&S) and in Research and Development (R&D). Official NIPA estimates start in 1929. We reconstruct the series of public investment and its components for the period 1890–1929 by digitizing detailed government outlays data from both the *Historical Statistics of the United States* (Bureau of the Census 1949) and the annual *Statistical Abstracts* published by the census. We rely on the narrative evidence in Bush (1954) and Dupree (1986) to classify investment into E&S and R&D. Finally, we interpolate the resulting annual series using quarterly government spending and back out public consumption as residual. Further details on the construction of all series are provided in the Supplemental Appendix.

When moving from annual to quarterly frequency, we use the method by Chow and Lin (1971). It is worth emphasizing that the impulse responses at long horizons, which are the primary focus of our analysis, depend mainly on the low-frequency properties of the data, which in turn are pinned down by the properties of the annual series. With the exception of the reconstructed government spending series, these annual series are mostly available from existing sources, which we take at face value. The interpolation affects mostly the high-frequency properties of the data (i.e., within the year), and, as such, it seems unlikely to have an effect on the estimated IRFs at longer horizons. We verify this hypothesis in Section IIIA.

To identify the structural parameters of the VAR, we follow the approach labeled as "internal instruments" by Plagborg-Møller and Wolf (2021) and also used by Ramey (2011b). This approach includes the instrumental variable (in our case the military spending news series) in the VAR and identifies the shock of interest by ordering the instrument first in a Cholesky decomposition. This approach is attractive because it will automatically control for any residual predictability contained in the instrument and still yield valid impulse responses when the instrument is contaminated with measurement error that is unrelated to the shock of interest.[8]

II. The Effects of Military Spending

In this section, we report our main results, which are based on a quarterly VAR with 60 lags and the following variables (described in the previous section): military news, government spending, real GDP, adjusted total factor productivity, the short-term interest rate, the surplus-to-GDP ratio, and the debt-to-GDP ratio.[9] We begin by analyzing the impulse responses to a military spending shock and then move to the estimates of the output multipliers across forecast horizons, up to 60

[8] Plagborg-Møller and Wolf (2021) point out that this approach yields valid impulse response estimates even if the shock of interest is noninvertible. However, in presenting estimators such as the Forecast Error Variance Decomposition (see Supplemental Appendix), and in the alternative identification based on the maximum share of the variance (Section VB), we will require invertibility and no measurement error.

[9] Relative to the seven-variable VAR in Ramey and Zubairy (2018), we have replaced GDP deflator with TFP, as the latter is central to the transmission mechanism highlighted in this paper. But, in Antolin-Diaz and Surico (2022), we have verified that our VAR (60) estimates very similar effects at long horizons using their original set of 7 variables.

quarters. In the next section, we assess the reliability of our low-frequency inference by presenting an extensive set of robustness checks, evaluating the role of the priors, conducting Monte Carlo analyses, and reporting frequentist estimates of local projections. In Section IV, we present the results of an extended VAR, where we add newly constructed time series of consumption, investment, trade, patents, and the three main components of government spending since 1890:I to shed light on the transmission of fiscal shocks.

A. *Impulse Response Analysis*

A simple way to summarize the estimates of a VAR is to report impulse responses of the endogenous variables to the identified shock of interest. We select a forecast horizon of 60 quarters to match the number of lags chosen in the estimated VAR (60) and report pointwise 68 percent and 90 percent posterior credible sets (as shaded areas). For ease of interpretation, the military spending news shock is normalized so as to increase government spending by 1 percent of GDP over the first year after the shock. The top row of Figure 1 presents the responses of government spending and real GDP. The middle row refers to the short-term nominal interest rate and TFP, whereas the bottom row focuses on the government balance sheet: fiscal deficit and public debt, both expressed as a share of GDP.

The main findings from our VAR (60) can be summarized as follows. During the first four years after the shock, government spending increases sharply and then reverts, triggering an equally persistent increase in GDP, a notable fiscal deterioration with government debt peaking around 1.5 percent of GDP, and a delayed but significant increase in productivity. At frequencies between five and eight years, government spending goes back to its initial level, causing a short-lived slowdown in both output and TFP. This is associated with a switch toward fiscal surplus that contributes to revert the path of the debt-to-GDP ratio.[10]

In the long term, conventionally defined as frequencies beyond eight years, the response of government spending becomes significant again, but its peak is now a fraction of what was at shorter horizons. The fiscal surplus is no longer statistically different from zero, and public debt slowly returns to pre-shock levels. In contrast, GDP and total factor productivity witness a second boom that is not only as large in magnitude as the first peak but also appears more persistent. Interestingly, the timing of the productivity response is consistent with the empirical literature on the rate of technological diffusion, which typically estimates adoption lags between 6 and 17 years (Comin and Mestieri 2014; Pezzoni, Veugelers, and Visentin 2022).

There is some tentative evidence that the effects on output and TFP might weaken somehow after 15 years. It should be noted, however, that given the large number of lags and the long forecast horizon (relative to the sample size), caution should be exercised in claiming that empirical analyses such as ours could possibly distinguish

[10]The sequence of fiscal surpluses from year 4 to 10 in Figure 1 are notably smaller than the fiscal deficits triggered by the initial government spending expansion. This suggests that the (second wave of) GDP response plays a major role in reducing the debt-to-GDP ratio to pre-shock levels, consistent with the evidence in Hall and Sargent (2011).

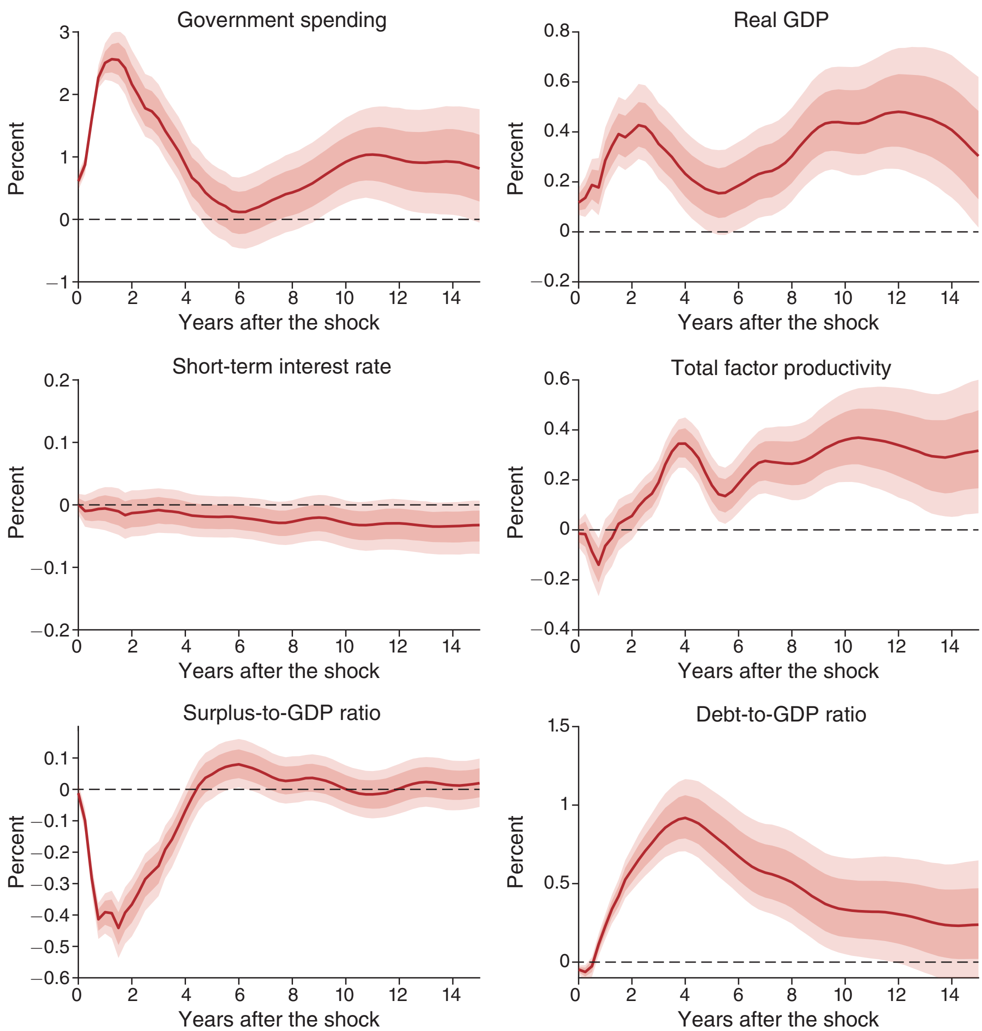

Figure 1. Impulse Responses to Military News Shock

Notes: The impulse responses are based on an estimated VAR with 60 lags of military spending news, government spending, real per capita GDP, utilization-adjusted TFP, short-term interest rate, fiscal deficit to GDP ratio, and government debt to GDP ratio. Government spending, GDP, and TFP enter the VAR in log-levels. Military spending news is ordered first in the Cholesky factorization. The size of the shock is normalized such as to increase government spending by 1 percent of GDP over the first year after the shock, based on a median G/Y ratio of 19. The darker (lighter) shaded areas represent the central 68 percent (90 percent) high–posterior density (HPD) intervals. The darker solid lines are the median estimates. Results are based on 5,000 posterior draws.

between truly permanent and very persistent dynamics.[11] Our preferred interpretation of the evidence in Figure 1 (and the rest of our analysis) is that the effects of government spending on output and TFP are very likely to extend beyond business

[11] Forecasting 15+ years ahead using 125 years of data relies on fewer than 8 nonoverlapping samples of 15 years.

cycle frequencies. Finally, the effects on the short-term nominal interest rate are negligible throughout.[12]

For completeness, we report the Forecast Error Variance Decomposition (FEVD) in the Supplemental Appendix. Military spending shocks explain between 30 percent and 40 percent of the variation in government spending at business cycle frequencies, and about 20 percent of fluctuations at longer horizons. These shocks account for a nontrivial fraction of the variance of GDP and productivity, around 10 percent. This is consistent with the evidence in Rossi and Zubairy (2011) on the role of fiscal policy in explaining US medium-term fluctuations.

In summary, we estimate significant long-lasting effects of government spending on both output and productivity. Unlike the short-run dynamics where the movements in government spending tend to be of a similar magnitude (if not larger) than the response of GDP, the lower frequency estimates suggest a large multiplier at long horizons, as the effects on output are associated with far smaller changes in government spending at longer horizons. In the next part of this section, we corroborate this conjecture by formally computing the multiplier across forecast horizons.

B. *The Government Spending Multiplier in the Short Run and the Long Run*

In the previous section, we have estimated a larger output response at longer horizons relative to the smaller lower-frequency movements in government spending, and the opposite at higher frequencies. In this section, we formally quantify these relative effects by computing the fiscal multiplier of government spending on output across forecast horizons. This is interesting for at least two reasons. First, government spending may have different effects at different horizons, and comparing the multipliers at high, business cycle, and low frequencies within the same estimated model can help shed light on this issue. Second, as noted by Ramey (2019), different studies often compute the multiplier at different horizons, and reporting how the estimates of this statistics vary with the forecast horizon may help reconcile seemingly conflicting findings in the literature.

In line with earlier work, we define the output multiplier for each horizon h as the ratio between the cumulative impulse response of real GDP to military spending news up to horizon h and the cumulative impulse response of government spending to the same shock over the same horizon. Following Mountford and Uhlig (2009), we use the sample average nominal interest rate to discount the estimates between one and h quarters ahead. In Figure 2, we display the present value multiplier for each horizon between $h = 0$ (i.e., the impact multiplier) and $h = 60$ (i.e., the long-run multiplier). Panel A refers to the specification in log-levels and uses the historical median of $G/Y = 19\%$ to transform the estimated elasticities into multipliers. Panel B refers to the specification in which output and government spending are both scaled by Y_{t-1}. Panel C is based on a model where both government

[12] Using the yield on 10-year government bonds instead of the short-term rate in the VAR produces very similar findings. As noted by Meltzer (2004), until the Treasury-Fed accord of 1951, the Fed pegged interest rates at a low level to facilitate the financing of government debt during wartime. Friedman and Schwartz (1963) argue that the Fed choice of not controlling the growth of the monetary base over this period contributed to fueling inflation. This is consistent with the responses of prices in Antolin-Diaz and Surico (2022) and interest rates in Figure 1, respectively.

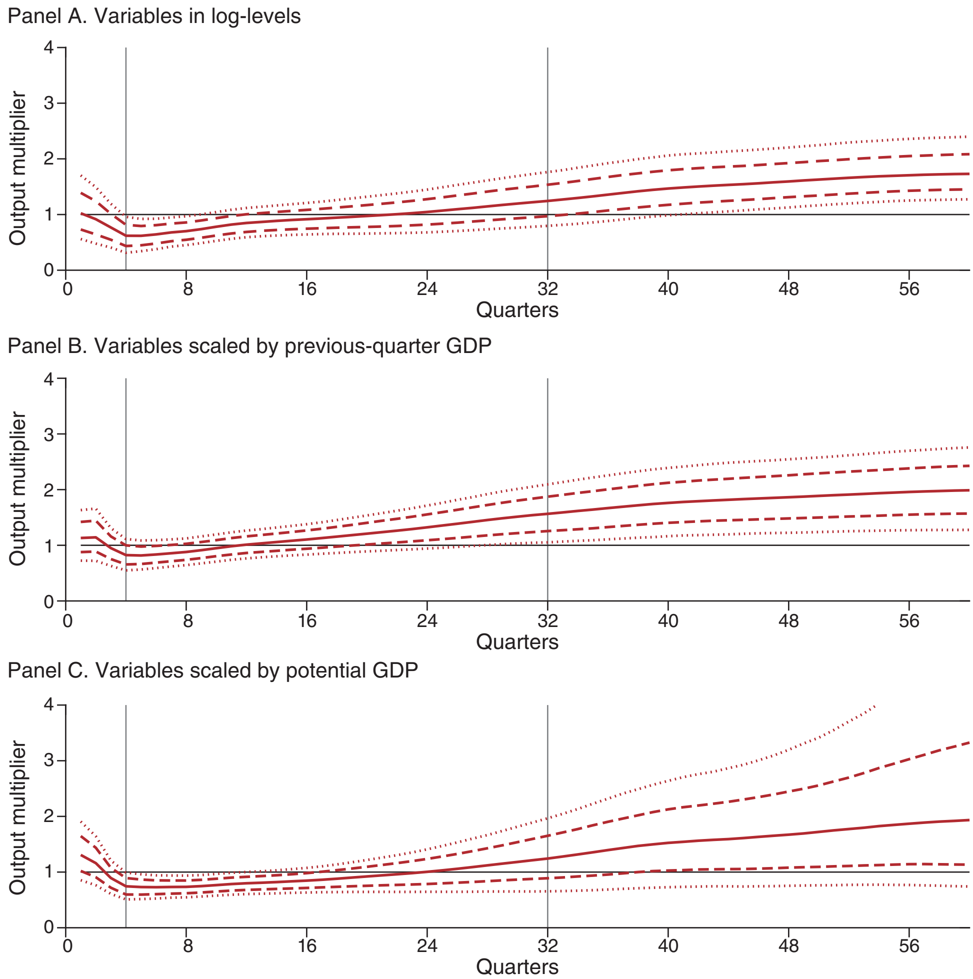

Figure 2. The Government Spending Multiplier across Horizons

Notes: The present value multiplier at each horizon h is computed as the ratio of the integral up to horizon h of the output response and the integral up to horizon h of government spending response to a military spending news shock, discounted using the steady-state interest rate. The estimates are based on VARs with 60 lags. In the top panel, government spending and output enter the VAR in log-levels, and the multipliers are obtained using the elasticity formula and the historical median G/Y ratio of 19 percent. In the middle panel, output and government spending are both divided by Y_{t-1}. In the bottom panel, they are expressed in percent of potential output as defined in Ramey and Zubairy (2018). The broken (dotted) lines represent the central 68 percent (90 percent) HPD interval. The solid line stands for the median estimate. Results are based on 5,000 posterior draws.

spending and real GDP are scaled by potential output, as defined by Ramey and Zubairy (2018). The latter two strategies provide direct estimates of the multipliers and do not rely on the government spending-output ratio.

The estimates in Figure 2 reveal that the posterior distributions of the government spending multiplier display, on impact, median values between 1 and 1.35, with most mass above 1. After the first year, however, the multiplier decreases below 1, consistent with the evidence in Hall (2009); Barro and Redlick (2011); and Ramey and Zubairy (2018). These estimates are relatively stable over the following three to five years before growing with the forecast horizon. The posterior median takes values above 1 at frequencies beyond 8 years, and it peaks at values between 1.7 and 2 (across specifications) in the forecasts 15 years ahead. Interestingly, despite relatively close median estimates, both the log-level and the previous-quarter-GDP specifications lead to more accurate inference about the long-run multiplier than the model that removes potential output.

In summary, the findings of this section suggest two main conclusions about the effects of government spending on output. First, on impact and at business cycle frequencies (i.e., from 6 to 32 quarters) the multipliers span the range of point estimates available in the fiscal policy literature, between 0.6 and 1.5, thereby offering a possible reconciliation of apparently conflicting results in earlier empirical macro studies. Second, while the multipliers at business cycle frequencies tend to exhibit values below or around 1, the multipliers at low frequencies (i.e., beyond 32 quarters) display much larger values and eventually exceed 1 significantly at long horizons.

III. Assessing Inference at Low Frequencies

In the previous section, we have reported strong evidence of significant effects of government spending on output and productivity at horizons beyond business cycle frequencies (i.e., after eight years). In this section, we want to assess the robustness of our findings to several modifications of our sample, model specification, and econometric method. We start by reporting results for samples that exclude from the instrument either WWII or one cluster of military spending events at the time to make sure no single episode drives the identification. Then, we look at IRFs based on VARs estimated using annual data (the frequency of some of our primary sources) or a considerably shorter lag length. Next, we move to assess the role of the priors by (i) modifying either their mean or variance, (ii) estimating jointly the prior hyperparameters using hierarchical priors, and (iii) applying the methods developed by Müller (2012) to measure both the sensitivity and informativeness of the priors. Then, we conduct a Monte Carlo analysis under two very different data-generating processes featuring, respectively, fully i.i.d. and nonstationary time series; the goal is to evaluate whether our empirical model has any tendency to spuriously detect long-lasting effects when these are actually *not* present in the data-generating process. Finally, we present frequentist estimates based on local projections, which have lower bias but higher variance at long horizons than the estimates based on VARs with short lag length (Li, Plagborg-Møller, and Wolf 2021b). All the analyses in this section corroborate, by and large, the notion that the effects of government spending extend significantly beyond business cycle frequencies.

A. *Sensitivity Analysis*

In this section, we check the robustness of our results to different samples, data frequency, and lag length selection. We record these results in Figure 3, whose columns refers to output and TFP, respectively. The top two rows refer to samples that exclude from the instrument either WWII (first row) or 12 major clusters of military spending news events one at a time (second row). As argued by Friedman (1952),

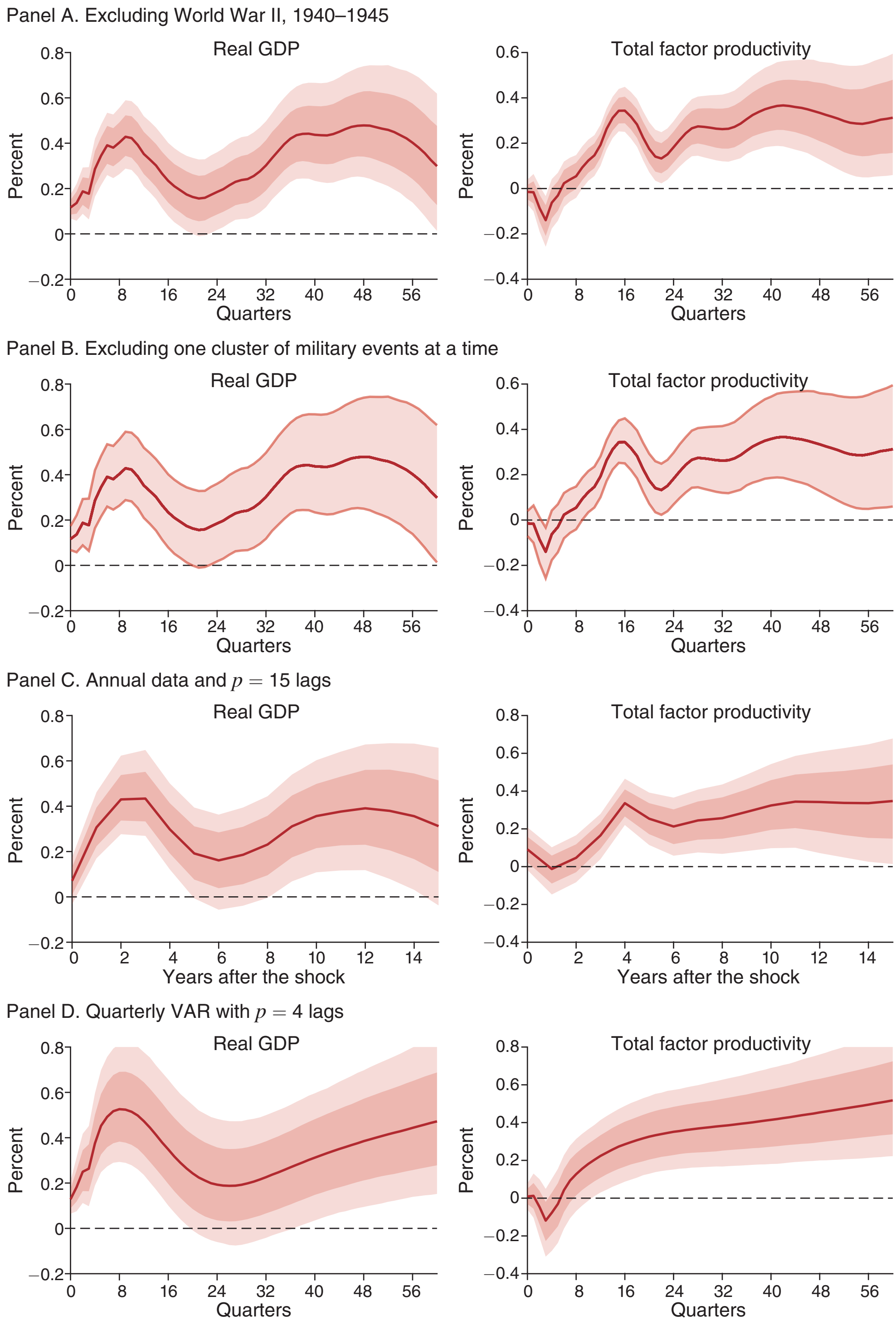

FIGURE 3. SENSITIVITY ANALYSIS

Notes: The solid lines represent the median posterior response. The darker shadow area represents the sixty-eighth posterior credible intervals, while the lighter shadow area represents the ninetieth posterior credible intervals. Results are based on 5,000 posterior draws.

exploiting large wars and military spending for identifying the effects of government expenditure is attractive for at least two reasons. First, the variation in military spending associated with wars (abroad) is typically independent from the state of the (domestic) business cycle and thus should prevent reverse causality running from GDP to government spending. Second, public spending swings tend to be large in historical perspective, thereby offering sufficient variation in the leading variable. On the other hand, using wars as source of exogenous variation poses the external validity challenge that a specific episode may be driving the results. This concern is particularly acute for WWII, which represents—by far—the largest increase of real government spending in US economic history, both relative to GDP and in absolute value.

Excluding the years from 1940 to 1945, in the first row of Figure 3, produces IRFs for output and productivity that are very similar to the estimates in Figure 1. The response of GDP to a military spending shock occurring outside the WWII period is significant over the first 4 years, slows down between 16 and 32 quarters, and then increases again, for a longer period, at frequencies beyond year 8. The first peak of TFP is also very significant but appears delayed relative to the peak in GDP. The increase in productivity decelerates in years 5 and 6 before taking off again, persistently, at intermediate and long horizons. In the second row of Figure 3, we report the envelope of 90 percent credible intervals for 12 different exercises in which we have removed—one at a time—each major cluster of military spending.[13] The swath of median posterior estimates for GDP and TFP are very close to their Figure 1 counterparts. The envelope of 90 percent credible sets is somewhat larger than in the baseline results, but it is worth emphasizing that our main finding of large and significant effects of government spending at frequencies beyond the business cycle is not overturned by the removal of any of these events.[14] We also discuss subsample stability in the Supplemental Appendix.

In the third row of Figure 3, we come to terms with the fact that many of the variables we have reconstructed for the quarterly analysis have been interpolated, as historical data are typically reported at annual frequency by most primary sources. In Section IIA, we have discussed the reasons why this is unlikely to pose a threat to our low-frequency estimates. Here, we wish to verify that argument by running our model on annual data. For consistency with the quarterly analysis, we reduce the number of lags to 15 and adapt the priors to embed the same degree of persistence

[13] Historically, military news shocks tend to cluster around major wars and significant historical events. Accordingly, in the second row of Figure 3, we report estimated impulse responses based on subsamples in which we have removed one cluster of military spending at a time from the instrument. This is a more stringent test than simply removing one observation at a time. The clusters are June 1890 (Navy Bill), June 1898 to September 1898 (Cuban War), December 1915 to December 1918 (World War I), June 1940 to December 1945 (World War II), September 1950 to December 1953 (Korean War), December 1957 (Sputnik), March 1961 to December 1961 (Kennedy era), March 1965 to December 1967 (Vietnam War), March 1980 to March 1981 (Cold War buildup), December 1986 to March 1992 (end of Cold War), September 2001 to September 2008 (War on Terror), and December 2008 to December 2015 (Afghanistan War surge).

[14] We have also verified that the exclusion of any pair of events among the three largest war-induced military spending episodes (namely, WWI, WWII, and the Korean War) does not overturn our main conclusions: The long-run effects on output and productivity are still large and significant. On the other hand, excluding all these three large war episodes at once produces small and insignificant output and productivity responses at longer horizons. In other words, each and every one of these war-induced, large increases in government expenditure seems sufficient to elicit significantly persistent effects on the US economy in the long run, though none of them is actually necessary.

relative to the priors of the quarterly model. The estimates for output and TFP are very similar to the IRFs in Figure 1. The output response is characterized by two peaks, with a more persistent effect after year 8, and the productivity response is delayed, with a persistent increase at intermediate and long horizons.

In the fourth row of Figure 3, we ask whether a VAR with only four lags, a standard choice in most empirical macro analyses on quarterly data, is capable of fully capturing the dynamic responses of GDP and TFP. On the one hand, the evidence from the VAR (4) points to large and significant effects of military spending on output and productivity at frequencies beyond the business cycle. On the other hand, the estimated dynamics are—by construction—much smoother, and the effects look now even more persistent. As discussed in Section IIA, however, we stress that given the large number of lags, long forecast horizon and relatively short sample, the reader should resist the temptation to draw inference based upon whether the effects reported in this paper are best viewed as permanent or very persistent. Notwithstanding this interpretation caveat, all analyses in this paper point toward large and significant effects of government spending on GDP and TFP beyond business cycle frequencies. This is our favorite interpretation of our main findings.

B. *The Role of the Priors*

In this section, we analyze the impact of our Bayesian priors on our main empirical results. We are particularly interested in confirming that it is the information contained in the likelihood, rather than some feature of the priors, that drives our finding of significant effects of military spending on GDP and TFP beyond business cycle frequencies. An additional concern is that because the Minnesota priors shrink some series toward being random walks and others toward being persistent, stationary processes, and because the single unit root priors favor cointegration in the system, we might be building in, a priori, a bias toward finding very long-lasting effects when, in fact, these are transitory.

In Figure 4, we report a number of alternative specifications of the prior distributions, which are aimed at understanding the impact of their structure on the results. In panel A, we remove the single unit root prior, while in panel B, we additionally set the prior mean of the first autoregressive coefficient, δ, equal to zero for all variables. The latter choice can be regarded as a misspecification of the priors, as variables in level such as real GDP are known to be characterized by trending behavior. A further exercise is presented in panel C, where we introduce the "Sum of Coefficients" prior originally proposed by Doan, Litterman, and Sims (1984).[15] Again, the use of this type of prior might be regarded as a source of misspecification, as it introduces a bias *against* the presence of cointegration (see the discussion in Giannone, Lenza, and Primiceri 2019). The robust message delivered by the first three rows of Figure 4 is that the posterior results are qualitatively very similar to the estimates in Figure 1. We interpret this finding as evidence that no specific feature of

[15] Similar to the single unit root specification, this prior can be implemented by the addition of a series of dummy observations stacked on top of the data matrix, in particular $\mathbf{y}^{**}_{n\times n} \equiv \operatorname{diag}(\bar{y}/\mu); \mathbf{x}^{**}_{n\times(n\cdot p+1)} \equiv [0, y_*, \ldots, y_*]$, with μ being a hyperparameter controlling the tightness of the prior and $\bar{y}$ the average of the first p observations.

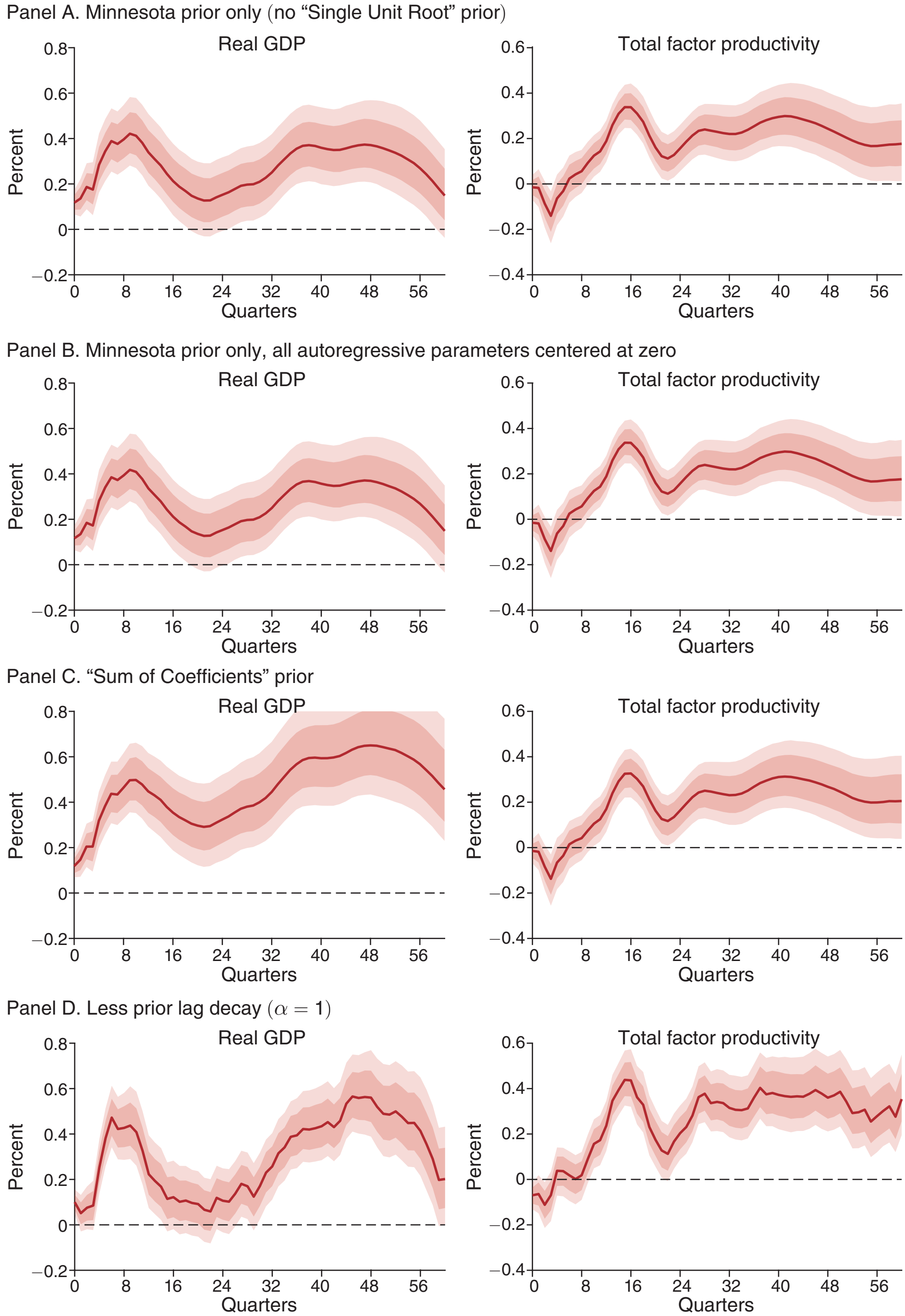

FIGURE 4. IMPULSE RESPONSE FUNCTIONS UNDER ALTERNATIVE PRIORS

Notes: The solid lines represent the median posterior response. The darker shadow area represents the sixty-eighth posterior credible intervals, while the lighter shadow area represents the ninetieth posterior credible intervals. Results are based on 5,000 posterior draws.

our baseline priors is responsible for the finding of significant effects of government spending on GDP and TFP at horizons beyond business cycle frequencies.

Another important question concerns whether the aggressive prior lag decay implied by the Minnesota prior could induce the empirical autocovariances at long lags ($\ell \approx 60$) to have only a minor influence on the posterior inference about impulse responses at long horizons. To address this issue, we also examine the alternative strategy of setting a linear, rather than exponential, decay pattern, that is, setting the hyperparameter $\alpha = 1$. As seen in panel D of Figure 4, the IRFs of GDP and TFP become substantially less smooth, but if anything, the results become stronger. Therefore, one can interpret our baseline specification of the priors as conservative, in the specific sense that our baseline choices tend to push the impulse responses at long horizons toward zero.

While the above exercises are reassuring, they still have the drawback that we are examining the isolated impact of changing features of the priors one at a time. We present below two exercises aimed at understanding the impact of the prior and its different hyperparameters altogether. First, following Giannone, Lenza, and Primiceri (2015), rather than searching for the value of the prior hyperparameters that maximizes the marginal likelihood, we simulate them from their full posterior distribution, allowing us to account for their estimation uncertainty.[16] This approach, which has a natural interpretation of a Bayesian hierarchical model, is implemented using a Metropolis step to draw the low-dimensional vector of hyperparameters. Using this alternative specification, in Figure 5, we present IRFs that are the counterparts of the estimates in Figure 1. Two main findings stem out from this exercise. First, the posterior credible sets of all impulse responses are wider than in the baseline case. This is not surprising because the hierarchical prior structure integrates across possible values of all hyperparameters, and this introduces another layer of uncertainty in the IRF estimation. Second, and more importantly, we confirm that the responses of both GDP and TFP exhibit a very persistent second hump that is still strongly significant at long horizons.

An additional strategy to assess the *joint* impact of all priors on the posterior distributions is developed by Müller (2012). This method is specifically designed to assess the relative importance of both the priors and the likelihood on the posterior estimates and can be computed for nonlinear transformations of the underlying parameters, such as impulse responses and output multipliers. More specifically, we calculate the measures of Prior Sensitivity (PS_γ) and Prior Informativeness (PI_γ) proposed by Müller (2012). The first metric approximates the largest change of the posterior mean that can be induced by changing the prior mean by the multivariate analog of one prior standard deviation. The second metric, which is contained in the interval $[0,1]$, summarizes the relative amount of prior information in the posterior distributions. Following the analysis in Müller (2012), we also report the statistic R^2_γ, which is a measure of goodness of fit in a linear regression of the impulse response values on the underlying parameters in the posteriors and priors. This statistic is a useful complement to (PS_γ) and (PI_γ) because it measures the

[16] In this step, we follow Giannone, Lenza, and Primiceri (2015) and allow for both the "Single Unit Root" and "Sum of Coefficients" priors to be present simultaneously. Therefore, we are integrating over four hyperparameters: $\lambda, \alpha, \theta, \mu$. In the Supplemental Appendix, we report prior and posterior distributions of these four hyperparameters.

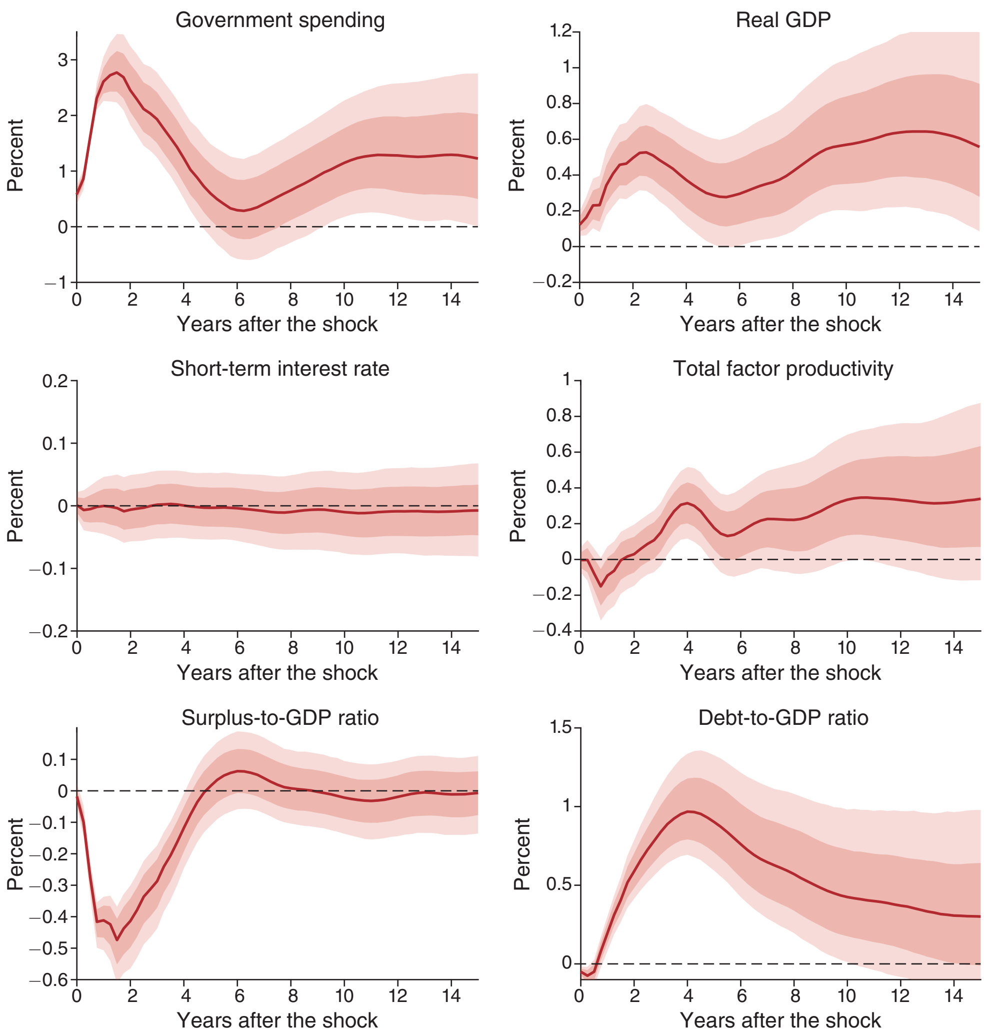

Figure 5. Impulse Responses Using Hierarchical Priors

Notes: The impulse responses are based on an estimated VAR with the same specification of the baseline figure, but where the prior hyperparameters are estimated using the hierarchical MCMC method described in Giannone, Lenza, and Primiceri (2015). The size of the shock is normalized such as to increase government spending by 1 percent of GDP over the first year after the shock, based on a median G/Y ratio of 19 percent. The darker (lighter) shaded areas represent the central 68 percent (90percent) HPD intervals. The darker solid lines are the median estimates. Results are based on 5,000 posterior draws.

validity of approximating the nonlinear transformation of the parameters with a linear function in the Prior Informativeness calculations.

In panel A of Table 1, we report the findings for the GDP impulse responses, whereas in panel B we record those for the output multiplier. Each row represents a different forecast horizon, from 4 up to 60 quarters, whereas the columns refer to the posterior mean, the posterior standard deviation, and the three statistics discussed above, PS_γ, PI_γ, and R^2_γ. In both panels, the measure of prior sensitivity increases with the horizon but always takes very low values. For instance, looking at the GDP

Table 1—Prior Sensitivity and Prior Informativeness Analysis

Horizon (h)	μ_π	σ_π	PS_γ	PI_γ	R^2_γ
Panel A. GDP impulse response					
4	0.29	0.08	0.04	0.00	1.06
8	0.40	0.09	0.04	0.00	1.08
12	0.36	0.09	0.05	0.00	1.04
20	0.16	0.10	0.05	0.00	0.98
40	0.44	0.13	0.06	0.00	1.01
60	0.31	0.18	0.08	0.00	0.96
Panel B. Output multiplier					
4	0.62	0.18	0.08	0.00	1.05
8	0.74	0.16	0.07	0.00	1.09
12	0.87	0.16	0.08	0.00	1.12
20	0.99	0.21	0.10	0.00	1.08
40	1.51	0.34	0.15	0.00	1.04
60	1.77	0.66	0.27	0.00	0.74

Notes: μ_π is the posterior mean of the object of interest; σ_π its standard deviation; PS_γ is the Prior Sensitivity, which approximates the largest change of the posterior mean that can be induced by changing the prior mean by the multivariate analog of 1 prior standard deviation; and $PI_\gamma \in [0,1]$ is the prior informativeness, which summarizes the relative amount of prior information in the posterior. R^2_γ is the R^2 in a linear regression of the impulse response value on the underlying parameters in the posterior and prior.

impulse response at the 60-quarter horizon, a change in the prior mean would induce a change in the posterior that is less than half of the posterior standard deviation. Consistent with this, we find values for the prior informativeness statistics that are essentially zero at all horizons. The results for the output multiplier paint a similar picture. Taken together with the results reported previously, we confirm our assessment that our key results are driven by features of the data rather than features of the prior distributions.

C. *Monte Carlo Analysis*

In this section, we evaluate the ability of our BVAR (60) to draw accurate inference about the long run. Our goal is to establish whether our richly parameterized model has any tendency to spuriously detect long-lasting effects when these are actually *not* present in the data-generating process (DGP). We present two exercises. In our first DGP, the artificial data are fully independently and identically distributed across time:

$$\mathbf{y}_t \sim \mathcal{N}\left(\mathbf{0}_{n\times 1}, \bar{\Sigma}\right) \quad \text{for} \quad t = 1, \ldots, T. \tag{6}$$

We set the covariance matrix, $\bar{\Sigma}$, to the posterior mean of our baseline estimates, so as to preserve the contemporaneous correlation structure present in US data. Given this, the theoretical impulse responses in our first DGP have—on impact—the same magnitude as in our baseline results but decay immediately to zero (and remain there) after the first quarter.

In a second, more empirically relevant and more challenging exercise, we consider a DGP in which (i) some variables display persistence at business cycle frequencies; (ii) GDP, government spending, and TFP share a unit root driven by a single TFP shock, ε_t^{TFP}; and (iii) GDP and government spending are also subject to persistent but ultimately transitory shocks. More specifically, we postulate the following processes:

$$m_t = \varepsilon_t^{news}$$

$$TFP_t = TFP_{t-1} + \varepsilon_t^{TFP}$$

$$\tilde{G}_t = 0.1\, m_{t-1} + 1.7\,\tilde{G}_{t-1} - 0.73\,\tilde{G}_{t-2} + \varepsilon_t^G$$

$$\tilde{Y}_t = 1.3\,\tilde{Y}_{t-1} - 0.4\,\tilde{Y}_{t-2} + \varepsilon_t^Y$$

$$G_t = TFP_t + \tilde{G}_t$$

$$Y_t = TFP_t + \tilde{G}_t + \tilde{Y}_t,$$

where the process for military news, m, is driven by the news shock ε_t^{news}; $\tilde{G}$ is the deviation of government spending from the TFP trend; $\tilde{Y}_t$ refers to the output deviations from the TFP trend; G_t is total government spending; and Y_t is output. The shocks $\left\{\varepsilon_t^{news}, \varepsilon_t^{TFP}, \varepsilon_t^G, \varepsilon_t^Y\right\}$ are i.i.d. and uncorrelated to each other. The parameters of the process above are calibrated so that the impact of the military shock matches the first peak, as well as the share of the variance, observed for government spending in our baseline estimates. This DGP displays the following properties. First, a shock to TFP induces a single unit root that is common to TFP, output, and government spending; second, a military news shock produces a hump-shaped response in government spending and, insofar as spending forms part of output, a similar response in the latter, with no amplification. In other words, the fiscal multiplier is equal to 1, and government spending has zero long-run effect on output. Third, there are confounding shocks that affect the evolution of both government spending and output. The key question is whether in this setting the confounding unit-root shocks contaminate the estimation of the impulse responses and lead to the detection of spuriously very persistent effects.

In both Monte Carlo analyses, we use a panel of $n = 7$ variables and $T = 504$ observations, which is identical to our baseline sample. Moreover, we always use the same baseline priors: GDP, TFP, and government spending are centered around a unit root; the other variables are centered around an AR(1) coefficient with persistence parameter equal to 0.9; the tightness of the hyperparameters is set to the same values used in Section IIA. In Table 2, we record the results of both exercises. Across the rows, we report estimates about the GDP response to the military news shock at various horizons. The first column reports the median difference between the point estimate and the true IRF across MCMC experiments; the second column reports the median length of the 90 percent confidence interval; and the third column represents the coverage probability, that is, the fraction of MCMC draws for which the true value lies inside of the estimated 90 percent confidence intervals.

For the i.i.d. data-generating process in panel A, the point estimates are centered around the true value at all horizons, with narrow confidence intervals. The

Table 2—Monte Carlo Analysis

Horizon (h)	Median bias	Median length	Coverage prob.
Panel A. Fully i.i.d. data-generating process			
4	0.00	1.33	0.91
8	0.01	1.02	0.97
12	0.00	0.77	0.97
20	0.00	0.53	1.00
30	0.00	0.37	1.00
40	0.00	0.28	1.00
60	0.00	0.19	1.00
Panel B. Single unit root data-generating process			
4	−0.03	0.06	0.52
8	−0.06	0.13	0.49
12	−0.06	0.16	0.61
20	−0.05	0.17	0.75
30	−0.03	0.17	0.71
40	−0.02	0.17	0.78
60	−0.01	0.18	0.87

Notes: Median bias of point estimate, median length, and coverage probability of nominal 90 percent confidence intervals at different horizons. Data-generating processes as described in the main text. $T = 504$, i.i.d. standard normal innovations. 100 Monte Carlo repetitions; 1,000 Gibbs sampler iterations.

coverage probability is close to the nominal level at the short horizons but rises above 90 percent at medium to long horizons, indicating confidence intervals that—if anything—are too conservative in this setting. Based on this, it seems unlikely that our estimation procedure would find long-lasting effects if the data were actually characterized by no persistence at all.

As for the more challenging data-generating process of panel B, we find that while there is some evidence of a possible undercoverage at short horizons, there is simultaneously evidence of a small *downward* bias, and at intermediate and long horizons—which are the main focus of our analysis—the impulse responses are centered around the true value, and the coverage probabilities are only slightly below the nominal level of 90 percent. We conclude that it is highly unlikely that our finding of positive and very persistent effects at frequencies beyond the business cycle is driven by either our prior specification or by the presence of confounding unit roots in the data that are common to both GDP and government spending.

D. *Frequentist Local Projections*

A further way to assess whether our results are driven by features of the Bayesian VAR is to use frequentist local projections, without any shrinkage. Making the two methods comparable is challenging because the large number of lags used as controls in the VAR will quickly erode the degrees of freedom in the LPs and thus lead to very imprecise estimates when no shrinkage is used. The trade-off between bias and variance in LPs and the attractiveness of shrinkage methods are discussed in detail by Li, Plagborg-Møller, and Wolf (2021a). Accordingly, we reduce the number of lags to 20, except for the military spending news and the outcome variable, where we keep 60 lags. The LP estimates are reported in Figure 6. Standard errors are adjusted for heteroskedasticity and autocorrelation (HAC). The IRFs produced by the LPs

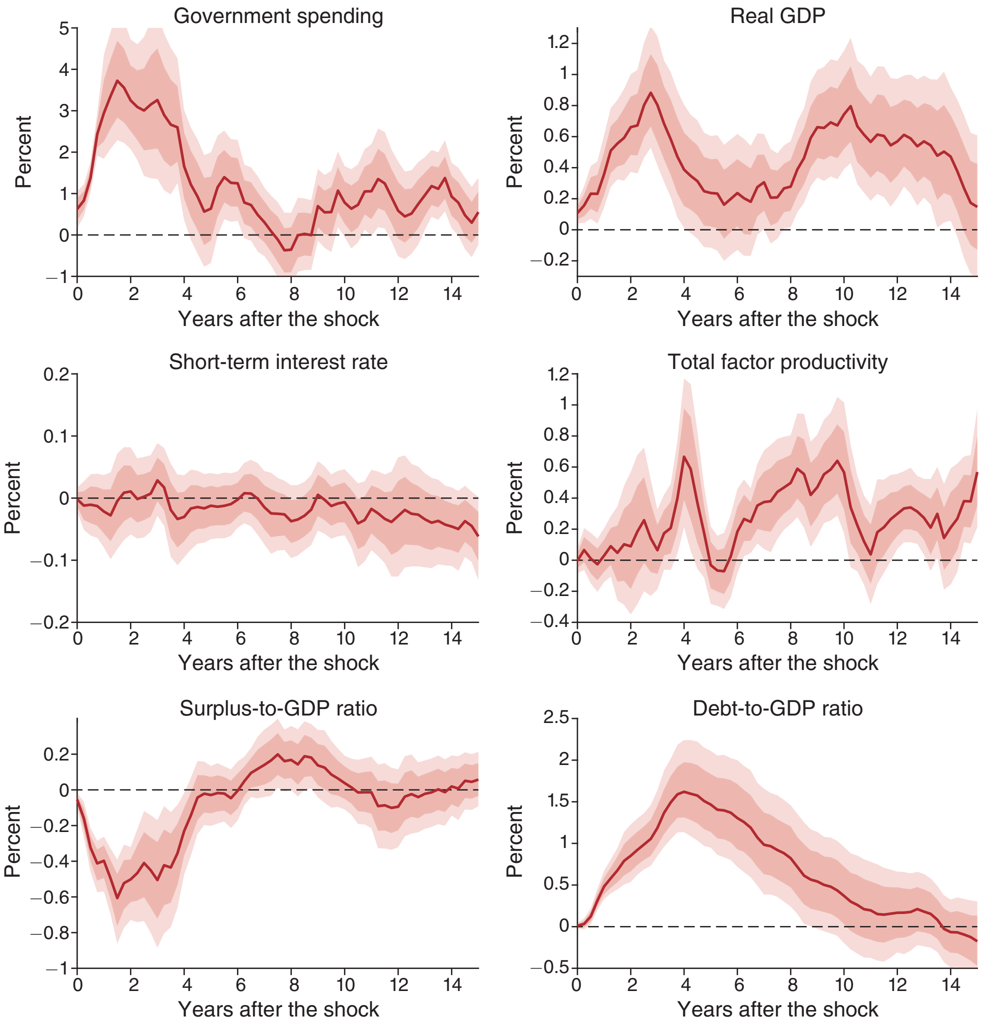

FIGURE 6. IRFs TO MILITARY NEWS SHOCK FROM FREQUENTIST LOCAL PROJECTIONS

Notes: In each panel, the impulse responses are frequentist estimates of local projections with 60 lags of the military spending news and the outcome variable as well as 20 lags of all remaining variables in our baseline 7-variable dataset. Government spending, GDP, and TFP enter the VAR in log-levels. The size of the shock is normalized such as to increase government spending by 1 percent of GDP over the first year after the shock, based on a median G/Y ratio of 19 percent. The darker (lighter) shaded areas represent the central 68 percent (90 percent) frequentist confidence intervals. The darker solid lines are the mean estimates.

appear more jagged than their BVAR counterparts but confirm the very significant and persistent responses of output and TFP at horizons beyond the business cycle. In the Supplemental Appendix, we present the results of an alternative strategy to solve the curse of dimensionality.[17] This involves collapsing the $60 \times 7 = 420$ original controls into $k = 43$ principal components that explain the bulk of their variance.[18]

[17] We thank an anonymous referee for suggesting the two approaches used in this subsection.
[18] We select the optimal number of principal components according to the criteria proposed by Bai (2004).

This exercise points to qualitatively similar medium-run effects on output and productivity following a military spending news shock.

IV. Inspecting the Mechanism

In the previous sections, we have reported extensive evidence of robustly significant and highly persistent responses of output and productivity to a change in military spending. To shed light on the transmission mechanism, in this section we look at the effects of government spending shocks on private sector outcomes and public spending categories.

To mitigate the curse of dimensionality, in each specification, we augment our baseline VAR (60) with at most two variables at a time, which also enter the model in log-levels. For the private sector models, we consider the following four pairs of additions: (i) labor productivity and hours worked (which substitute for GDP), (ii) unadjusted total factor productivity and patents, (iii) private consumption and private investment, (iv) exports and imports. For the public sector specifications, we add in turn (v) public consumption expenditure, (vi) public investment in E&S, and (vii) public expenditure in R&D.

A. *Private Sector*

In Figure 7, we report the posterior credible sets around the responses to a government spending shock, based on the four extended VARs (60) for the private sector described above. The first row focuses on the specification with labor productivity and hours worked, the second row refers to the model with adjusted TFP and innovation (as measured by patents), the third row reports the estimates for consumption and investment changes, and the fourth row summarizes the responses of exports and imports. Several results emerge from Figure 7. First, after a short-lived decline, labor productivity experiences a sustained increase, which peaks significantly at the end of the forecast horizon. In contrast, hours worked rise on impact and peak in their first year (consistent with the temporary productivity drop) before recording small and insignificant changes.

The second row reveals that the effects of government spending shocks on labor productivity are mirrored by the dynamics of unadjusted TFP. Given the low statistical power of empirical time series model to distinguish between permanent and very persistent effects, we stress once more that our preferred interpretation of our evidence is that the effects of government spending on output and productivity are large and significant well beyond the five years forecast horizon typically considered in empirical business cycle analyses. In contrast, the right-hand panel of the second row makes clear that patents are crowded out in the first few years after the shock; their response, however, turns positive and significant in the medium to long term, consistent with the findings in Diebolt and Pellier (2020) that infrequent, large shocks, such as wars, account for the largest pushes in innovation and the very process of economic growth in the United States over the last century. Both IRFs in the second row of Figure 7 are also consistent with the micro evidence on patents in Comin and Mestieri (2014) and Pezzoni, Veugelers, and Visentin (2022), who estimate an average adoption lag between 6 and 17 years in the rate of technological diffusion over the post-WWII period.

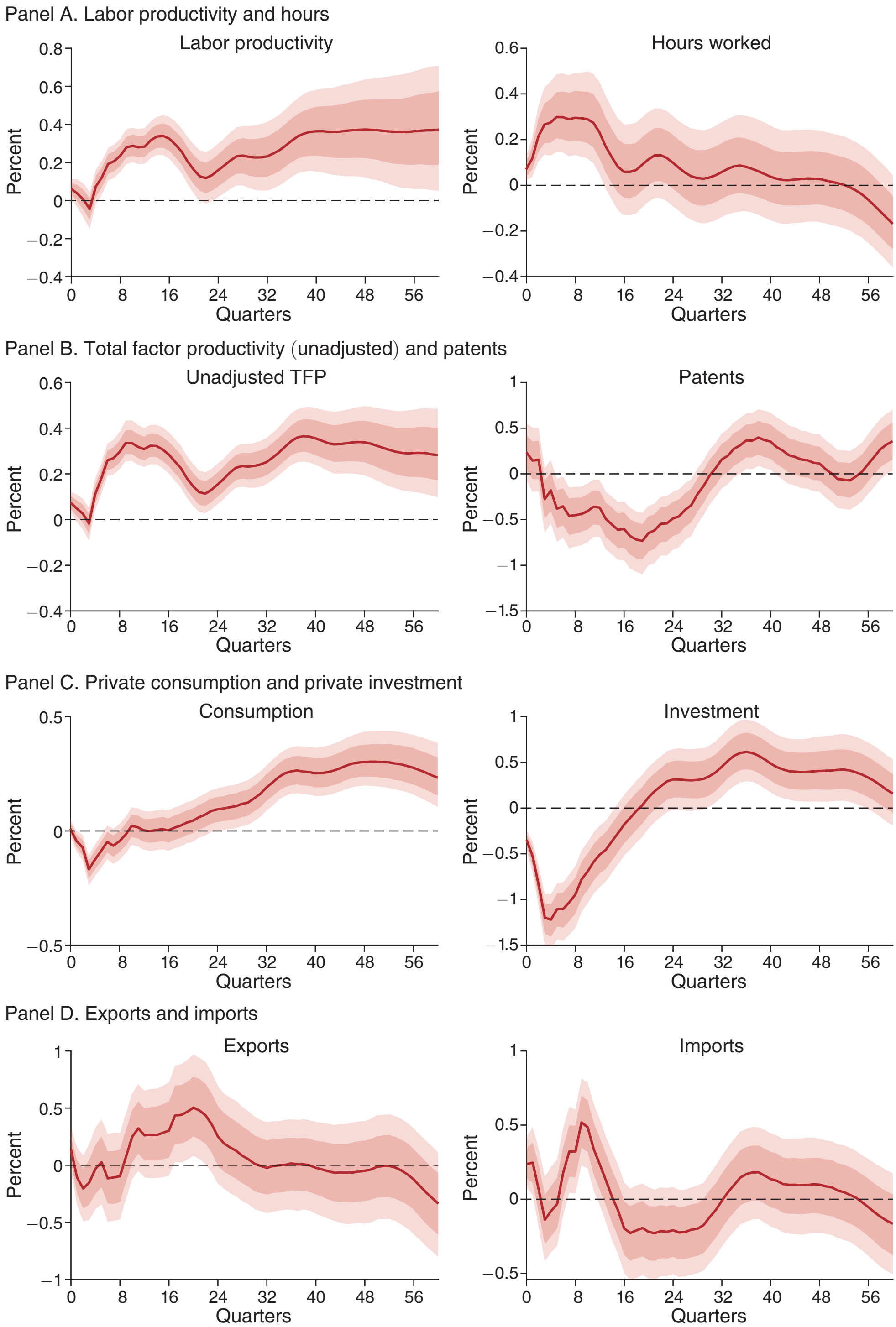

FIGURE 7. EFFECTS OF MILITARY SHOCKS ON PRIVATE SECTOR OUTCOMES

Notes: The impulse responses are based on an estimated VAR with 60 lags adding to the baseline specification the series in each panel. Military spending news is ordered first in the Cholesky factorization. The size of the shock is normalized such as to increase total government spending by 1 percent of GDP over the first year after the shock. The darker (lighter) shaded area represents the central 68 percent (90 percent) HPD band. The darker solid line stands for the median estimates. Results are based on 5,000 posterior draws.

In the third row of Figure 7, a public spending hike crowds out private consumption and private investment in the short run, as reported also by Ramey (2011b). Five years after the shock, however, both expenditures go up significantly, peaking at horizons of about 9 to 12 years. The magnitude of the investment response is larger than the size of the consumption effect, possibly reflecting its more volatile nature and smaller GDP share. Finally, military spending has a significant short-term impact on imports and a delayed effect on exports, which imply a significant trade surplus between two and six years after the shock.

In summary, government spending causes a short-lived rise in hours worked; a temporary crowding out of innovation, private investment, and consumption; and a delayed hump in net exports. In the medium to long run, however, investment and innovation experience significant and sustained increases, which feed into large and very persistent effects on labor and total factor productivity as well as consumption.

B. *Public Sector*

The findings in the previous section are consistent with an important role played by productivity, innovation, and private investment in shaping the responses of output to a government spending shock at long horizons. In this section, we ask whether the particular composition of public spending triggered by a defense budget increase may also have a significant contribution. To this end, we run three separate model specifications in which we augment the baseline VAR (60) of Section II with our newly reconstructed historical time series of public consumption, government investment, and public R&D, respectively.

The results of these public sector–augmented VARs (60) are reported in Figure 8. The first row depicts the response of the *log-level* of each category to the military spending news, while the second row reports the response of each category as a *share* of total government spending. The top panels reveal three main findings. First, the responses of these three public spending categories are highly correlated: Military spending triggers a joint increase in public consumption, investment, and R&D.[19] Second, public investment is the category that responds most in the short run. Third, government R&D expenditure is the only component that displays a large and persistent response.

To appreciate the relative contribution of each category, in the second row of Figure 8, we look at the responses of public consumption, investment, and R&D as *shares* of total government spending. Given the data are in logarithms, these are computed as the difference between the impulse response of each spending category and the impulse response of total government spending at each horizon.[20] Three results stand out from this exercise. First, following a military spending shock, there

[19] We interpret this finding as a cautionary note *against* counterfactual exercises that try to isolate the effects of a specific public spending category by setting to zero at all times the responses of all other components of the government budget. In the context of military spending (and possibly also of other large public programs), this "counterfactual" mix is actually well outside the distribution of historical combinations of public spending components.

[20] Over our long sample, consumption, investment in E&S, and R&D expenditure account, on average, for about 77 percent, 20 percent, and 3 percent of total government spending. During the post-WWII period, the average share of public R&D has increased to around 5 percent, offset almost entirely by a decline in the share of public investment.

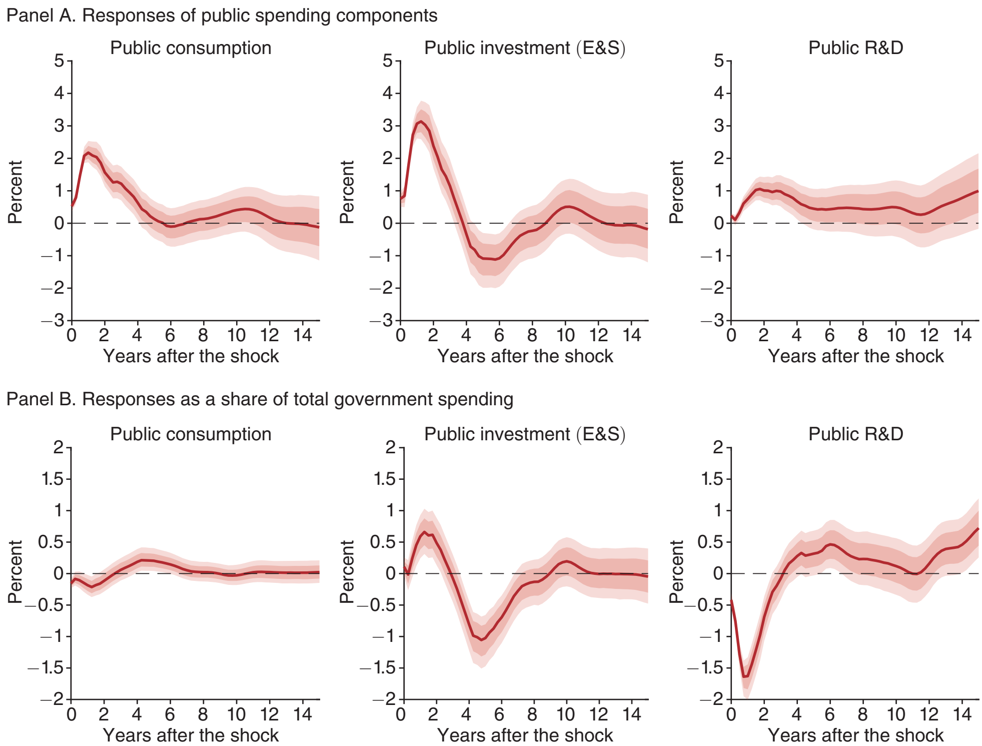

Figure 8. Effects of Military Shocks on Public Spending Components

Notes: The impulse responses are based on an estimated VAR with 60 lags of military spending news, real government spending per capita, real GDP per capita, utilization-adjusted TFP, short-term interest rate, fiscal deficit to GDP ratio, and government debt to GDP ratio. In each of the columns, real public consumption per capita, real public non-R&D investment per capita, and real public R&D expenditure per capita, respectively, are added in turn to the VAR. Each government spending category, total government spending and output enter the VAR in log-levels. Military spending news is ordered first in the Cholesky factorization. The size of the shock is normalized such as to increase total government spending by 1 percent of GDP over the first year after the shock. The top (bottom) panel refers to the response of each public spending category in log-level (as share of total government spending). The darker (lighter) shaded area represents the central 68 percent (90 percent) HPD band. The darker solid line stands for the median estimates. Results are based on 5,000 posterior draws.

are only small movements in the consumption share, except for two small drops at the start and at the end of the forecast horizon. Second, in the short run, the composition of public spending shifts significantly toward investment and away from R&D. Third, in sharp contrast, at intermediate and long horizons, the share of public R&D records a significant increase, which is offset by a decline in the public investment share and, to a lesser extent, in the consumption share.

One interpretation of the responses of the different public spending categories is that public investment in E&S plays a far more important role in explaining the effects of government spending at *short* horizons, whereas public R&D expenditure plays a far more important role in accounting for the effects of government spending at *long* horizons. In the next section, we explore this result further by digging deeper into the role that R&D plays in driving the effects on long-run output.

V. What Drives the Long-Run Effects of Government Spending?

In the previous section, we have shown that military spending has very significant and persistent effects: (i) on public R&D (but not on public consumption or investment) and (ii) on productivity and output. In this section, we ask whether changes in government R&D spending are a main driver of the significant output response at long horizons. Our analysis proceeds in three steps. We start by documenting a significant reduced-form correlation between public R&D and output at low frequencies, based on either our BVAR (60) or the factor model proposed by Müller and Watson (2020). Then, we put forward a simple but intuitive strategy to identify public R&D spending shocks and show that these cause large and very persistent effects on GDP and TFP. Finally, we report the IRFs to public consumption and public investment shocks and find evidence of virtually no response of output and TFP at intermediate and long horizons.

A. *The Low-Frequency Covariability of Public R&D and GDP*

In this section, we look at the reduced-form, low-frequency comovement between government R&D and GDP. We do so by relying on methods recently developed by Müller and Watson (2020), which are specifically designed for conducting inference on covariability among economic time series at very long horizons. Following Müller and Watson (2020), we specify a low-frequency factor model for the growth rates of government R&D, real GDP, and TFP. The inference is based on only a small number of periodic functions that capture the low-frequency properties of the data. More specifically, we focus on frequencies lower than 15 years, consistent with the lag length of our baseline BVAR as well as the forecast horizon that we have used in the IRF analyses throughout the paper. The results of the factor model in Müller and Watson (2020), and how it compares to the BVAR (60), are summarized in the Supplemental Appendix, which also reports a description of the low-frequency factor model. Despite the important methodological differences, both models estimate significant low-frequency correlations between the three variables and produce very similar unconditional forecasts at 25- and 50-year horizons.

B. *Identifying Public R&D Shocks*

The ideal experiment to isolate exogenous movements in government R&D would consist of "shocking" public R&D while keeping fixed both public consumption and public investment. But this very specific policy mix has virtually never happened in our long historical sample, as government spending typically involves a simultaneous expansion in all three categories.[21] Insofar as the correlation is not perfect, however, we can use a statistical approach to tease out the effects of each public spending category on the US economy.

[21] Interestingly, the evidence in Figure 8 reveals that military spending comes close to an ideal (long-run) experiment, as it is associated with a significantly long-lasting response of public R&D but very small and short-lived responses of public consumption and investment. However, the short-run dynamics are very different.

Our starting point is to note that, historically, the major shifts in public R&D spending have been unrelated to business cycle conditions. In the Supplemental Appendix, we discuss the narrative evidence around large public R&D programs and argue that, over our long sample, these have been, in fact, motivated by military rivalries (with Germany until WWII and the Soviet Union afterward), scientific progress, and ideological priorities of the different administrations, rather than by an endogenous response to the state of the US economy.

In addition, the timing and implementation lags associated with large public R&D programs extend well beyond business cycle frequencies or the terms of office of the different administrations. These considerations suggest that, after controlling for the lags of other macro variables, innovations to public R&D expenditure may be regarded as good as exogenous to current or prospective economic conditions, in the spirit of the short-run restrictions on government spending proposed by Blanchard and Perotti (2002) or the narrative identification for income tax changes pioneered by Romer and Romer (2010).

In practice, we drop military spending news from the model and add public R&D, patents, and private investment. We then identify exogenous changes in public R&D by searching for the shock that explains the maximum share of the public R&D innovation variance over the first year, following Uhlig (2004).[22] We focus on the first year, rather than the first quarter, because much of our historical data have been interpolated from annual series and the interpolation method might spuriously affect some of the high-frequencies correlations.

Before presenting the impulse response analysis, we find it useful to verify whether our newly identified shock can match the historical evolution of large federal R&D programs, as discussed in the Supplemental Appendix. To this end, in panel A of Figure 9, we present the historical decomposition of public R&D around three key historical events: (i) the Manhattan Project, from its establishment in 1941 to its dissolution in 1946 with the foundation of the Atomic Energy Commission; (ii) the creation of DARPA in 1958 and the moon landing project from 1961 to 1969; and (iii) Reagan's Strategic Defense Initiative from 1983 to 1987. In each subpanel, the solid black line represents the historical increase in public R&D, while the dotted blue line, and associated 68 percent posterior bands, refers to the part explained by our public R&D shock. In all cases, the movements in government R&D attributed to the shock align very closely with the actual increases around the three events. We interpret this as suggestive evidence that our shock captures the exogenous nature of military or ideologically driven surges in public R&D.[23]

In panel B of Figure 9, we report the time series of the identified public R&D shock, together with 68 percent posterior bands. The shock is plotted as an eight quarter moving average. Two findings are worth noting. First, there are clusters of positive shocks around the three major public R&D programs (vertical dashed

[22] The "max-share" method generalizes to any desired frequency the well-known Cholesky decomposition. The latter imposes the far more restrictive restriction that the identified shock explains the entirety of the variance of the variable of interest on impact. The "max-share" method has been shown to be more robust than the Cholesky factorization in a variety of empirical settings (see, e.g., Kurmann and Otrok 2013; Francis et al. 2014).

[23] In the Supplemental Appendix, we show that—in sharp contrast to the results in this section—the military spending shocks cannot explain the lion's share of movements in public R&D expenditure around these three key historical events.

lines). Similarly, a cluster of negative shocks is visible around the wind-down of the Apollo project. Second, the timing of these programs does not always coincide with major wars (shaded areas). For instance, while WWI and WWII led to large increases in defense-related R&D, the Korean war did not. In other words, the R&D shock seems distinct from the military news shock. Indeed, the sample correlation of the posterior mean of the two shocks is only 0.17.

In Figure 10, we report the IRFs to the public R&D shock. In keeping with previous charts, the shock is scaled so as to increase total government spending by 1 percent of GDP over the first year. At short horizons, the increase in output is much more muted than for the military spending shock and does not display any hump shape. At long horizons, however, the size and persistence of the effects on output and TFP become much larger, with a peak toward the end of the forecast horizon. Interestingly, using a very different identification strategy based on a narrative classification of R&D appropriations over a post-WWII sample, Fieldhouse and Mertens (2023) report long-lasting effects of a public R&D shock on US productivity and GDP trend that are similar, both in size and duration, to the estimates in Figure 10. Finally, the responses of private investment and patents display dynamics that are qualitatively in line with those produced by the military spending shock in Figure 1. For both variables, however, the public R&D shock causes a smaller short-run crowding-out effect, which is no longer statistically significant for patents.

In summary, our identified public R&D shock aligns very well with the narrative account around large public R&D programs in the economic history of the United States. Furthermore, we find that the effects on output, productivity, private investment, and innovation generated by an exogenous increase in government R&D are qualitatively similar to, if not more persistent than, those triggered by military spending, despite a relatively modest correlation between the two identified shocks.[24]

C. *The Role of Public Consumption and Public Investment*

In the previous section, we have identified the effects of public R&D on the economy by searching for the shock that explains most of the public R&D variance during the first year after the shock. For sake of comparability, in this section we adopt an identical strategy for the other two components of government spending and isolate the innovations to public consumption and public investment, respectively, that maximize the share of the forecast error variance of each spending component at the one-year horizon.

It is worth noting, however, that both shocks are in fact associated with significant contemporaneous movements in public R&D, which makes it hard to interpret them as "pure" innovations (i.e., everything else equal) to public consumption and investment. On the other hand, each innovation brings about movements in public R&D of different sizes, and therefore, we can exploit this variation to assess whether the strength of the output responses is correlated with the relative strength or "intensity" of the changes in public R&D.

[24] In the Supplemental Appendix, we further show that the estimated impulse responses for GDP and TFP in Figure 10 are very similar to those obtained by identifying and estimating the effects of a public R&D shock over a post-WWII sample that is characterized by no major war involving the United States.

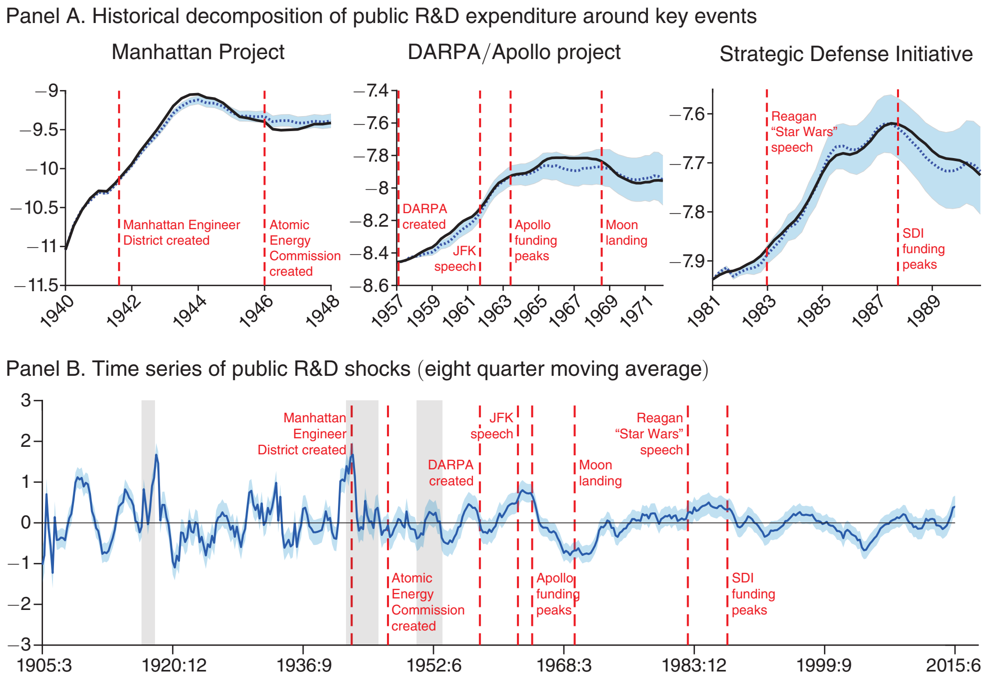

FIGURE 9. HISTORICAL ANALYSIS OF PUBLIC R&D AND PUBLIC R&D SHOCKS

Notes: Panel A plots the historical decomposition of the public R&D series around three key historical events: (i) the Manhattan Project, (ii) DARPA and the Apollo program, and (iii) the Strategic Defense Initiative. In each of the subpanels, the solid black line is the historical increase in real per capita R&D spending by the government. The dotted blue line, and associated 68 percent posterior bands, shows the part of the increase in R&D that can be explained by the effects of the exogenous public R&D shock identified using the max-share method at the one-year forecast horizon. Panel B plots the history of identified public R&D together with 68 percent posterior bands. To facilitate visualization, the shock is plotted as an eight-quarter moving average. Shaded areas represent major wars.

In panel A of Figure 11, we report the output responses to shocks that maximize the one-year-ahead error variance of public consumption, public investment, and public R&D, respectively.[25] Across all specifications, the shocks are normalized such that total government spending moves by 1 percent of GDP over the first year; hence, the three columns can also be thought of as varying the intensity of each spending category. The main finding is that the "consumption-intensive" shock leads to a smaller output response than the "investment-intensive" shock, which in turn triggers a smaller response than the "R&D-intensive" shock.

To explore these results further, in panel B, we look at the effects of each shock on public R&D as a share of total government spending. The shock to public consumption leads to a drop in the R&D share in the short run and a muted response thereafter. This is associated with modest effects on output at long horizons in panel A. The shock to public investment in the middle column also leads to a short-run decline in the R&D share; this is, however, quickly reversed and then replaced by a persistent

[25] The chart in the top-right panel of Figure 11 is therefore a repetition of the top-left panel in Figure 10.

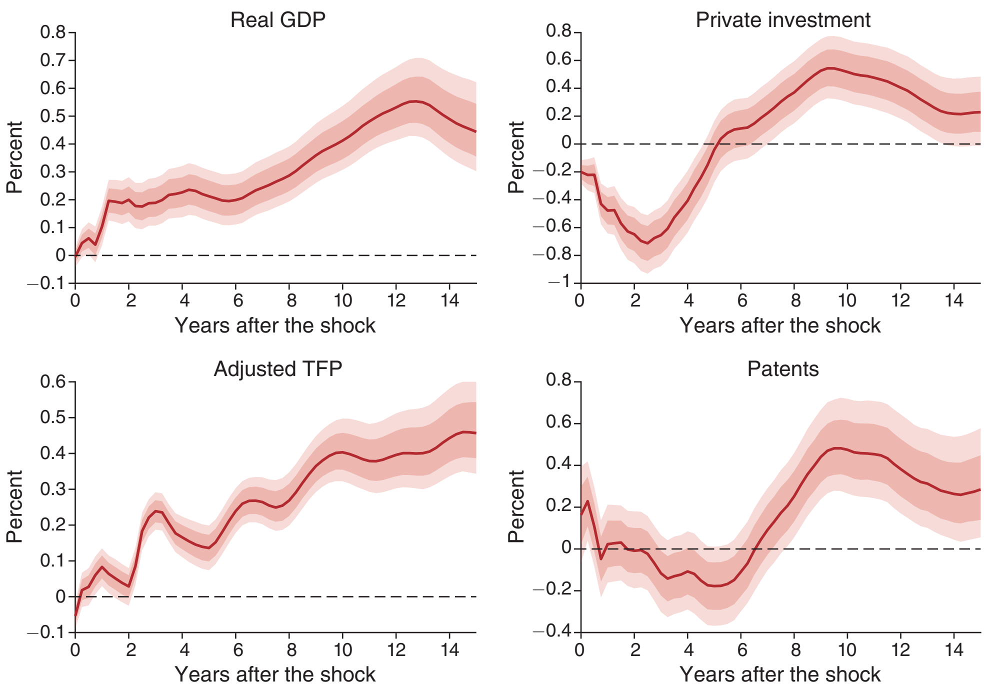

Figure 10. Impulse Responses to Public R&D Shock

Notes: The impulse responses are based on an estimated VAR with 60 lags of real public R&D per capita, real total government spending per capita, real GDP per capita, utilization-adjusted TFP, short-term interest rate, fiscal deficit to GDP ratio, government debt to GDP ratio, real private investment per capita, total factor productivity, and patents. Public R&D, total government spending, GDP, and TFP enter the VAR in log-levels. The public R&D shock is identified using the max-share method at the one-year forecast horizon. The size of the shock is normalized such as to increase government spending by 1 percent of GDP over the first year after the shock, based on a median G/Y ratio of 19 percent. The darker (lighter) shaded areas represent the central 68 percent (90 percent) HPD intervals. The darker solid lines are the median estimates. Results are based on 5,000 posterior draws.

increase, which is mirrored by the output response in the top row of Figure 11.[26] On the other hand, the R&D shock is characterized by both the largest R&D share response *and* the largest and most persistent output response.

In summary, military spending shifts the composition of public spending toward R&D. A shock that raises the relative intensity of government R&D leads to large and persistent responses of investment, productivity, innovation, and output. The latter is far larger and more persistent than the output responses to either more "public consumption–intensive" or more "public investment–intensive" shocks. We interpret this as suggestive evidence that public R&D is a key driver of the effects of government spending on output beyond business cycle frequencies documented in this paper.

[26] This is likely to reflect the patterns of military spending ramp-up, which, as discussed around Figure 8, lead to large short-run responses of investment and a longer-run increase in the share of R&D. Unsurprisingly, the output response to the public investment shock is more similar to the output response to the military spending shock.

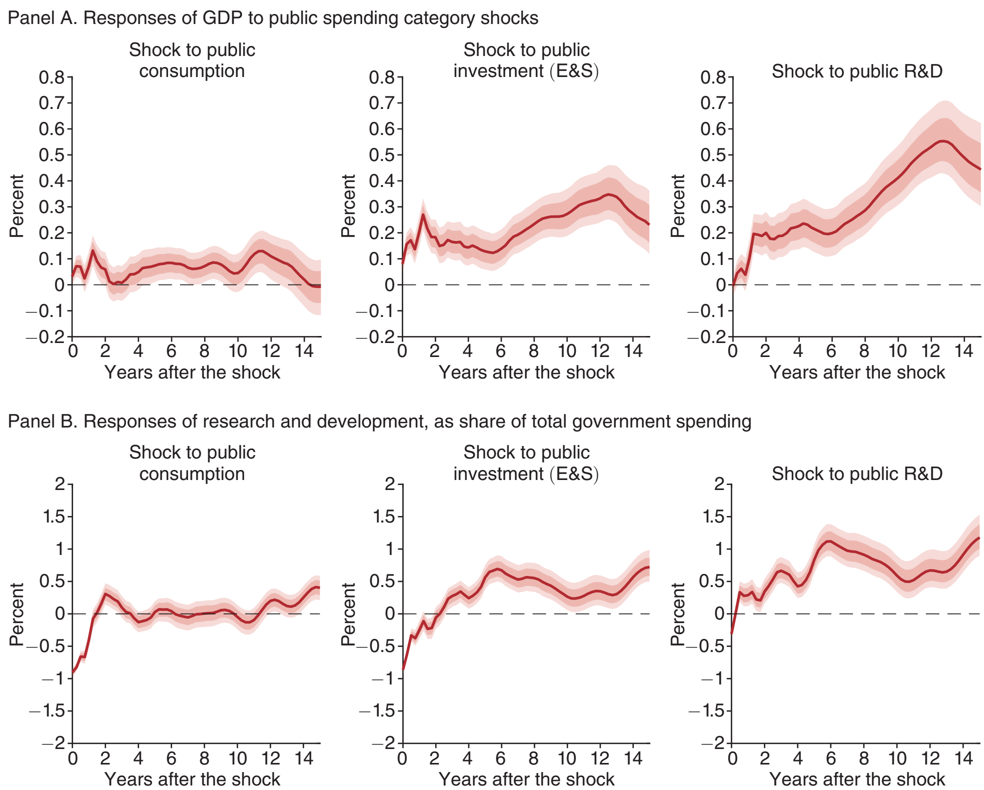

FIGURE 11. IMPULSE RESPONSE TO PUBLIC SPENDING CATEGORY SHOCKS

Notes: The impulse responses are based on an estimated VAR with 60 lags of real government spending per capita, real GDP per capita, utilization-adjusted TFP, short-term interest rate, fiscal deficit to GDP ratio, and government debt to GDP ratio. In each of the columns, real public consumption per capita, real public non-R&D investment per capita, and public R&D expenditure per capita, respectively, are added in turn to the VAR. Each public spending category, total government spending, GDP, and adjusted TFP enter the VAR in log-levels. The shock to each public spending category is identified using the max-share method at the one-year forecast horizon for that category. The size of the shock is normalized such as to increase total government spending by 1 percent of GDP over the first year after the shock. The top (bottom) panel refers to the response of GDP (public R&D as share of total government spending) to shocks to each public spending category. The darker (lighter) shaded area represents the central 68 percent (90 percent) HPD band. The darker solid line stands for the median estimates. Results are based on 5,000 posterior draws.

VI. Conclusions

What are the long-term effects of government spending? Despite the resurgence in fiscal research spurred by the financial crisis of 2007–2009 and the policy debate triggered by the global pandemic of 2020–2022, this question seems to have so far eluded empirical research. We use 125 years of US quarterly data—including newly constructed series of public spending by main categories—and time series models with long lags to shed light on this question. We argue that the combination of historical data, a generous lag length selection, and Bayesian shrinkage makes our framework well suited to draw inference about very persistent dynamics and diffusion patterns, while retaining the ability to look also at the short run.

We uncover four main regularities. First, fiscal policy can stimulate economic activities persistently when it tilts the share of public spending toward R&D, as it does, for instance, during military conflicts. However, we also find that an exogenous increase in public R&D expenditure can have very persistent effects on output and productivity even when it is not systematically associated with war spending. Second, in contrast, government investment has shorter-lived effects, whereas the impact of public consumption on output is modest at most horizons. Third, while government spending crowds out innovation, private investment, and private consumption in the short run, it crowds them in over the medium term, feeding into a very sustained increase in productivity. As a result, the government spending multiplier on output raises above one at longer horizons. Our evidence uncovers a novel mechanism through which fiscal policy can stimulate the economy and productivity, even beyond the business cycle.

Our analysis exploits low-frequency variations and comovements among total government spending, military purchases, public R&D, productivity, and GDP over a historical sample of 125 years, with the goal of identifying the effects of government spending on the US economy at horizons of up to 15 years. The significant advantage of using a long historical sample to identify low-frequency covariability comes, however, at the cost of facing possible subsample instability induced by the fact that economic policy priorities, the structure of the economy, and policy responses to business cycle conditions have varied greatly across the many administrations that spanned the last century. Balancing the trade-off between maximizing low-frequency variation that could help identify long-run effects and minimizing subsample instability that might affect long-run inference is a challenge that we leave for future research.

Finally, the government spending shocks proposed by Ramey (2011b) and Ramey and Zubairy (2018) are based on large and infrequent military outbursts, whereas our R&D spending shock identification essentially exploits the virtually acyclical nature of government R&D payments. Less is known, however, on other forms of public spending. A main challenge is the lack of strong and reliable instruments that may shed light on the effects of the different components as well as of total government spending. Further progress in identifying the effects of public expenditure could be made by taking a more granular approach across different government agencies or policies, as in Cox et al. (2024), and combining it with a careful narrative evaluation of long historical record of congressional documents along the lines of Romer and Romer (2010) or Fieldhouse and Mertens (2023), but spanning a much longer sample. We view this Herculean task as the next milestone in the fiscal policy research agenda.

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