# Big *G*

Lydia Cox

*University of Wisconsin–Madison and National Bureau of Economic Research*

Gernot J. Müller

*University of Tübingen and Center for Economic Policy Research*

Ernesto Pastén

*Central Bank of Chile*

Raphael Schoenle

*Brandeis University and Center for Economic Policy Research*

Michael Weber

*University of Chicago and National Bureau of Economic Research*

"Big *G*" typically refers to aggregate government spending on a homogeneous good. We confront this notion with five facts for the universe of federal purchases. First, they are volatile and account for the largest part of the short-run variation in total spending. Second, the origin of their variation is granular. Third, purchases are subject to procurement and bidding. Fourth, they are concentrated in long-term contracts. Fifth, their composition is biased toward sectors in which private sector prices are sticky. We develop a two-sector New Keynesian model consistent with these facts and find where the government spends is key for aggregate effects.

We are very grateful to the editor, Harald Uhlig, and four anonymous referees for helpful comments. We also thank Klaus Adam, Regis Barnichon, Christian Bayer, Benjamin

Electronically published August 16, 2024

*Journal of Political Economy*, volume 132, number 10, October 2024.

https://doi.org/10.1086/730426

## I. Introduction

What is "big *G*"? In the national accounts, *G* represents government spending—the part of gross domestic product (GDP) that comprises government consumption of goods and services plus investment. This convention possibly helps explain why research on fiscal policy typically entertains a somewhat abstract notion of government spending as spending on a homogeneous good, isomorphic to GDP. In empirical and theoretical work, we frequently refer to it as *G*, and the literature assumes that policy makers can adjust it freely and quickly over time. The recent "renaissance in fiscal research" surveyed by Ramey (2019) has changed little in this regard. And while recent research emphasizes the role of heterogeneity for the fiscal transmission mechanism, it largely maintains—notwithstanding some exceptions that we discuss below—the perspective on government spending as a homogeneous good and focuses on heterogeneity in the private sector.

By contrast, the starting point of our paper is the observation that government spending itself is fundamentally heterogeneous. Rather than being one large transaction, it is composed of a large number of smaller transactions whose composition differs from the other components of aggregate demand. We formalize this insight by establishing five facts about government spending based on the universe of procurement contracts by the federal government. These federal purchases are very volatile and account for the largest part of the short-run variation of government spending, including the variation due to identified fiscal shocks. In addition, we establish the granular nature of these purchases, the extent to which they are subject to solicitation and competition, and their long duration. These facts matter because they defy the notion that *G* is simply another policy instrument that can be fine-tuned to manage the business cycle in a timely fashion.

Federal purchases are also special in another dimension, as our fifth fact establishes, because of a sectoral bias: their allocation across sectors differs systematically from that of private expenditure and is concentrated in sectors in which private sector prices are particularly sticky. To understand the implications of these five facts, we revisit the fiscal

Born, Gabriel Chodorow-Reich, Zeno Enders, Emmanuel Farhi, Xavier Gabaix, Yuriy Gorodnichenko, Edward Knotek, Valerie Ramey, Ludwig Straub, and conference and seminar participants at the Banco Central de Chile Heterogeneity in Macroeconomics Conference, Federal Reserve Bank of Boston, Federal Reserve Bank of Cleveland, National Bureau of Economic Research Monetary Economics Meeting, Pontificia Universidad Católica de Chile, University of Tübingen, and Verein für Socialpolitik Monetary Meeting Frankfurt for valuable comments. Schoenle and Weber also gratefully acknowledge support from the National Science Foundation (grant 1756997). Finally, we acknowledge outstanding research assistance by Jiacheng Feng. The views expressed herein are solely those of the authors and do not necessarily reflect the views of the Central Bank of Chile, the Federal Reserve Bank of Cleveland, or the Federal Reserve System.

transmission mechanism in a stylized two-sector model that is consistent with the empirical facts (but omits modeling the microfoundations of the procurement process). In the model, how government spending impacts the economy fundamentally depends on the sector in which it originates, underscoring our main point: there is no big *G*, only many little *g*'s. And while this aspect makes fiscal stabilization challenging, it also creates opportunities for targeted interventions.

Our analysis of federal purchases relies on a database that only recently has become accessible: USAspending.gov. The database provides detailed information on the universe of procurement contracts by the federal government since 2001. For each year, the database records several million government procurement transactions. And while it does not cover all expenditure items of the general government—that is, *G* or total government spending—it is unique in detail and scope: it covers 40% of federal government spending and 16% of total government spending, which in turn accounts on average for 18.7% of GDP in our sample. The government wage bill and government spending at the state and local level are the largest components of *G* that are not part of federal purchases.

Our analysis takes a business cycle perspective, focusing on the short-run variation in *G*. Accordingly, the first of our five facts establishes that federal purchases are very volatile and account for about half of the variation of *G* at quarterly frequency, even though their average share in *G* is only one-sixth. Moreover, variation of federal purchases is largely exogenous to the business cycle: while federal purchases respond significantly to identified government spending shocks—in contrast to the other components of government spending—they account for only a small fraction of the large-scale fiscal stimulus packages during our sample period.

Our second fact pierces the origins of the variation of federal purchases. Contrary to what conventional models of the business cycle assume, variation emerges from only a few influential sectors and firms, making them granular in the sense of Gabaix (2011). Consistent with this granular origin, time-fixed effects add little explanatory power in panel regressions of federal purchases. Hence, the variation of government spending originates at the micro rather than at the macro level. As a result of such a granular nature, federal purchases may have limited scalability, constraining fiscal stabilization policy as it is traditionally conceived.

The third fact that we establish is that the government makes very few off-the-shelf purchases. Instead, purchases are made through a two-stage procurement process: a solicitation period, in which the government solicits bids and proposals for work, followed by a selection process, in which the government awards the contract to a bidder. Overall, this process can be lengthy—substantiating the notion of substantial implementation lags in government spending—and tends to be competitive, patterns we characterize in detail using the micro data.

The fourth fact is about the duration of federal purchases. A large part of the value of federal purchases is tied up in long-term contracts. Moreover, the median tenure of firms that interact with the government as contractors is also long. This fact provides a microfoundation for the assumption that shocks to government spending are persistent, which in turn has been identified as a major determinant of the aggregate effects of fiscal spending shocks (Baxter and King 1993).

While the first four facts concern the dynamics of federal purchases, the fifth fact highlights a cross-sectional property: federal purchases are biased toward specific firms and sectors. We thus confirm and extend earlier findings by Ramey and Shapiro (1998) that document sectoral bias for episodes of military buildups along several dimensions: at business cycle frequency (for both defense and nondefense contracts), across a more recent time period, and at the firm level. In addition, we make a new related observation. Federal purchases tend to be concentrated in sectors in which prices for private transactions are relatively sticky: the average frequency of price changes in these sectors is about half of the frequency in the remaining sectors.

These descriptive facts are important for understanding the fiscal transmission mechanism and the implementation of fiscal policy. Facts 1–4 illustrate key challenges when it comes to fiscal stabilization via big *G*. Fact 5 instead presents policy makers with an opportunity. This insight emerges as we revisit the fiscal transmission mechanism in a two-sector version of the New Keynesian model that is consistent with our five facts. In particular, we allow government spending to vary exogenously across firms. Still, aggregate dynamics depend on its sectoral distribution because sectors differ as a result of sectoral bias and private sector pricing frictions. Procurement prices instead are irrelevant for the allocation because their effect on households' tax bills is offset by their effect on firm profits, which are rebated to households.

We show that—unlike in a one-sector model—crowding out of private expenditure can be infinite and the government spending multiplier can be negative if fiscal shocks originate in the sector in which prices for private transactions are relatively flexible. Conversely, a fiscal shock in the sector in which prices are sticky impacts output more strongly. Intuitively, if the government spends in relatively sticky sectors, monetary policy needs to tighten less in order to keep inflation stable, and hence less crowding out of private expenditure occurs. We also trace out the transmission mechanism in detail on the basis of model simulations and, following Uhlig (2010), report cumulative discounted fiscal multipliers: they depend fundamentally on the sector in which fiscal shocks originate and robustly so across time horizons.

Last, we confront the predictions of the model with new evidence, taking a time series perspective. Specifically, we aggregate federal purchases into two sets of sectors. In the first set of sectors, prices are relatively sticky and these sectors receive a large share of federal purchases relative to their weight in private spending. We include this time series in a vector autoregressive (VAR) model together with a time series of the remaining federal purchases. The VAR also features times series data of inflation, interest rates, and an index of GDP, and we estimate the model on monthly data from 2001 to 2019. We identify shocks to federal purchases in both sectors recursively and compare their aggregate effects. As predicted by the model, we find that shocks in the relatively flexible sectors do not lead to an increase in economic activity. Instead, inflation and interest rates increase. The opposite adjustment patterns obtain in response to shocks in the sticky sectors.

Taken together, while federal purchases account only for only about 3% of GDP (16% of 18.7%), investigating them in detail yields important insights: The richness of the data provides us with an empirical laboratory to study the granular nature of the business-cycle fluctuations in $G$ and how sectoral characteristics such as price stickiness influence the spending multiplier. The results also likely carry over to the other components of government spending unless they are directed to sectors which cater exclusively to the government. At the same time, given the extent of idiosyncratic variation in federal purchases that we document, the contracts data can be used as instruments in future studies in the spirit of Nakamura and Steinsson (2014), Gabaix and Koijen (2020) or Chodorow-Reich (2019).

*Related literature.*—Our analysis relates to four distinct strands of the literature. First, the literature that emphasizes the importance of sectoral heterogeneity in fiscal policy transmission, notably the influential study of Ramey and Shapiro (1998): we generalize some of their empirical results, as highlighted above. At a conceptual level, our analysis also offers a generalization. Whereas Ramey and Shapiro (1998) highlight the importance of specific sectoral shifts in government spending—notably those associated with military buildups—our analysis suggests that the variation in $G$ at business cycle frequency is generally best understood as emerging bottom-up from the firm and sectoral level. Perotti (2008) and Nekarda and Ramey (2011) in turn exploit heterogeneity of sectoral government spending to create industry-specific government demand variables, which they then use to identify the effects of government spending shocks. Yet this industry-specific demand measure presupposes aggregate variation in $G$ as the source of shocks, whereas we highlight its granular origin. More recent work develops richer multisector models and models with input-output structure to shed light on the fiscal transmission mechanism (Flynn, Patterson, and Sturm 2022; Bouakez, Rachedi, and Santoro 2023) and stresses that it matters what exactly the government buys (Boehm 2020). Bouakez, Rachedi, and Emiliano (2022) share our perspective on the sectoral origin of fiscal shocks, offer a detailed analysis in a flexible price model, but limit their empirical analysis to defense spending contracts. Fisher and Peters (2010) and Hebous and Zimmermann (2021) exploit firm-level heterogeneity to identify fiscal policy shocks.

Second, a recent literature investigates how heterogeneity at the household level and geographic heterogeneity alter the effects of fiscal policy. Regarding the former, a central aspect is the importance of credit constraints and, in particular, how fiscal policy alters disposable income (e.g., Galí, López-Salido, and Vallés 2007; Demyanyk, Loutskina, and Murphy 2019; Auclert et al. 2023). We introduce credit constraints in a robustness analysis and show that it does not interfere with our central results. Regarding the latter, some recent work identifies fiscal shocks and determines local multipliers based on geographic variation (e.g., Acconcia, Corsetti, and Simonelli 2014; Chodorow-Reich 2019; Auerbach, Gorodnichenko, and Murphy 2020). Theoretical work, notably in the context of monetary unions, exists that studies the role of geographic heterogeneity for fiscal transmission (e.g., Galí and Monacelli 2008; Nakamura and Steinsson 2014; Hettig and Müller 2018).

Third, our analysis uses highly granular contract-level data, as does recent work on public procurement (e.g., Warren 2014; Decarolis et al. 2020; Kang and Miller 2022). In contrast to this literature, which we discuss in more detail below, we do not attempt to analyze the procurement process at the micro level and offer a macro perspective instead.

Finally, in terms of theory, our model shares features with recent work that accounts for heterogeneity on the production side across sectors and firms, tracing out the implications for business cycle fluctuations (e.g., Acemoglu et al. 2012; Ozdagli and Weber 2017; Baqaee and Farhi 2020; Bigio and La'o 2020; Pasten, Schoenle, and Weber 2020, 2024; La'O and Tahbaz-Salehi 2022). A key aspect in our model is that sectors differ in terms of private sector pricing frictions, and hence we can draw on insights developed in earlier work on the interaction of monetary and fiscal policy (Christiano, Eichenbaum, and Rebelo 2011; Woodford 2011; Farhi and Werning 2016).

## II. Data

We first provide a brief description of USAspending.gov, our data source for federal procurement contracts. We describe its background, details, and scope while also noting its limitations. We also describe the pricing data we use for fact 5.

### *A. Data Sources*

USAspending.gov covers the universe of federal procurement contracts. It was created in response to the Federal Funding Accountability and

Transparency Act (FFATA), which was signed into law on September 26, 2006. The FFATA requires federal contract, grant, loan, and other financial assistance awards of more than $25,000 to be publicly accessible on a searchable website. In accordance with FFATA, federal agencies are required to collect and report data on federal procurement. The USAspending.gov database, which the US Department of the Treasury hosts, compiles the data from these various reporting systems and collects information from the recipients of the awards themselves. Though FFATA was not signed into law until 2006, data are available going back to 2001 through an external organization. Limited contract data are available before 2001 through the National Archives but are not comprehensive enough for our purpose.[1]

Complementary to the federal procurement data, we also utilize data on the frequency of producer price changes at the two- and six-digit North American Industry Classification System (NAICS) sector levels, derived from the micro data that underlie the construction of the Producer Price Index (PPI) by the Bureau of Labor Statistics. We obtain the frequencies at the sector level from Pasten, Schoenle, and Weber (2020). The PPI data record transaction prices in the private sector but not the government sector. We revert to this point when discussing the facts about pricing.

### B. *Details and Scope of the Data Set*

Our primary data set includes all federal government contracts from fiscal years 2001–21. The Federal Acquisition Regulation defines these contract actions as "any oral or written action that results in the purchase, rent, or lease of supplies or equipment, services, or construction using appropriated dollars over the micro purchase threshold, or modifications to these actions regardless of dollar value." The goods and services that the government consumes span a wide range, from janitorial services for federal buildings to information technology support services to airplanes and rockets. Contracts can be short-term relationships (e.g., a 1-month contract awarded by the US Department of Agriculture Rural Housing Service to Sikes Property and Appraisal Service for single-family housing appraisals in September 2008) or longer-term relationships (e.g., the 43-year and 10-month contract awarded by the US Department of Energy to Stanford University for the operation and management of the Stanford Linear Accelerator Center National Accelerator Laboratory).

[1] See https://www.archives.gov/research/electronic-records/reference-report/federal-contracts. To facilitate future research using the USAspending.gov data, we provide cleaned subsets of the data and sample code at https://www.coxlydia.com/contracts_data.html.

On average, 3.2 million individual contract records exist per year, with almost 5 million annual contracts toward the end of the sample period. Recipients comprise an average of over 160,000 parent companies per year, spanning over 1,000 six-digit NAICS sectors. The median contract value is $3,640, whereas the mean contract value is $206,023, suggesting that the distribution is heavily right skewed. The majority of contracts (82% by count) represent positive obligations from the government to firms, but deobligations with a negative value also exist, occurring when certain types of modifications to an initial contract are performed. Each observation in the data traces a contract action from its origin (the parent agency) to the recipient firm and the sector and zip code within which the award is executed (see online fig. 1 for a schematic representation of the data). In addition to the value of the contract, the data contain information about the contract's duration, modifications of existing contracts, the mode of competition, and the pricing structure. We rely on the contract micro data to compute statistics at different levels of aggregation, from the contract level (for statistics on contract duration) to the firm and sector levels.

In terms of scope, federal procurement contracts include both purchases of intermediate goods and services as well as investment in structures, equipment, and software. The data do not include compensation of federal government employees (though they may include compensation for contractors) or consumption of fixed capital, which make up 26% and 21%, respectively, of federal government spending. Since most federal government investment in research and development (R&D)—about 11% of federal government spending—comes through grants, it is also not included in the contracts data. Overall, our contract data account for 40% of federal government spending and 16% of total government spending, commonly denoted by $G$ in macro models. Relative to GDP, the data therefore account for 3% of activity. We illustrate schematically the scope and limitations of the coverage for our data in the appendix (see online fig. 2). Going forward, we refer to the federal procurement contracts covered by our data as federal purchases.

## III. Five Facts on Government Spending

In this section, we establish five facts about the nature of federal purchases. We first document that they account for the largest part of the variation in $G$ and that this variation reflects shocks rather than systematic stabilization policy. The next three facts go some way toward rationalizing the first fact and are related to the granularity of federal purchases, the nature of public procurement, and the duration of the contracts that underlie these purchases. Our last fact establishes that federal purchases are biased toward sectors in which prices charged to private sector buyers are relatively sticky.

Common to these facts is the observation that the government does not purchase a homogeneous good, contrary to what the traditional notion of *G* suggests. The five facts also apply to a breakdown of the data into purchases by the US Department of Defense (roughly one-half) and the remaining purchases (see app. C).

### A. *Sources of Variation*

Our first fact concerns the sources of the business cycle fluctuations in government spending. We establish that fluctuations in federal purchases account for the majority of variation in total government spending—*G*—even though they represent only 16% of *G* in levels, or just under 3% of GDP.

FACT 1. Federal purchases account for the majority of the variation in *G*.

1. Federal purchases are 3.8 times as volatile as GDP (while *G* is only 1.2 times as volatile) and explain 48% of the variation in the growth rate of *G*.
2. Identified government spending shocks materialize almost exclusively as federal purchases.
3. Federal purchases account for less than 5% of the discretionary fiscal stimulus under the American Recovery and Reinvestment Act (ARRA) of 2009 and the COVID relief packages.

First, to establish the finding about volatility, we focus on the standard deviation of the cyclical component of federal purchases. We obtain it from Hodrick-Prescott (HP) filtering a proxy for the USAspending.gov contracts data that we construct from the National Income and Product Accounts (NIPA). The proxy variable comprises federal purchases of intermediate goods and services as well as gross investment in structures, equipment, and software. The proxy is highly correlated with our contract data—the correlation coefficient is 0.96—but has the advantage that it allows us to perform decompositions of *G* consistently within the NIPA (e.g., with the same treatment for seasonal adjustment).[2] At quarterly frequency, the standard deviation of its cyclical component is 3.8 times the standard deviation of the cyclical component of GDP.[3] In absolute

[2] See app. sec. A.2 (apps. A–F are available online) for details on this proxy variable and its high correlation with the contract data.

[3] For the HP filter, we use a smoothing parameter of 1,600. At monthly frequency, the relative volatility of federal purchases is 12.1. In the latter case, we apply the HP filter to the seasonally adjusted series of federal purchases, using a smoothing parameter of 129,600, as suggested by Ravn and Uhlig (2002). As a monthly measure of real GDP, we use an index compiled by S&P Global (https://www.spglobal.com/marketintelligence/en/mi/products/us-monthly-gdp-index.html).

TABLE 1
Contribution to Variation of *G*

| Component | Shapley (Partial $R^2$; %) (1) | Contribution to $\sigma^2_{\Delta G}$ (%) (2) | Weight (% of *G*) (3) |
|---|---|---|---|
| Federal purchases | 48 | 39 | 16 |
| Government wages | 23 | 13 | 49 |
| Residual | 29 | 18 | 34 |

Note.—The table shows decompositions of the quarterly growth rate of *G*. Column 1 shows the Shapley values, or partial $R^2$'s, which indicate the percent of the overall $R^2$ accounted for by the given component. Column 2 shows the contribution of each component to the overall variance of $\Delta G$ (one-quarter log changes), with the remainder captured by the covariance terms. Column 3 reports the share in GDP. The residual category comprises nonwage, noncontract purchases.

terms, the standard deviation is 3.9%; as a share of GDP, it is 0.4%. In contrast, total government spending is only 1.2 times as volatile as output, consistent with earlier findings (Christiano and Eichenbaum 1992).

Federal purchases also explain the majority of the variation in *G*, a fact we establish through two decompositions that set in relation to each other fluctuations in total government spending and fluctuations of its underlying components: federal purchases, wages, and the remaining residual items. As before, in both exercises, we use the proxy for federal purchases. In the first exercise, we estimate a regression of one-quarter log changes in *G* on the one-quarter log changes of its underlying components. Column 1 of table 1 shows the Shapley value, or partial $R^2$, for each component.[4]

Federal purchases explain 48% of the variation in the growth rate of *G*, despite accounting for only 16% of its level, as shown in column 3 of table 1. Wages account for 23% of the variation, and the residual component accounts for around 29%, both less than their respective shares in levels of 49% and 34%, respectively.[5]

In an additional exercise, we calculate the contribution of each subcomponent to the variance of one-quarter growth rates of *G*. Reported in column 2 of table 1, this decomposition yields similar results, with federal purchases explaining a large share of the variance relative to both its weight and to the other components. These results are consistent with the fact that federal purchases are much more volatile than the other

[4] The Shapley value computes the marginal contribution to the explanatory power from adding in each component to the model, taking into account all possible permutations of regression models. It is defined by $R^2_{x_j} = \Sigma_{M\subset V\setminus\{x_j\}}((n!(p-n-1)!)/p!)(R^2(M\cup\{x_j\}) - R^2(M))$, where *M* denotes the model with *k* regressors but without the component of government spending $x_j$ we consider. Set *V* contains the regression specifications with all combinations of regressors.

[5] When we just consider the nonwage portion of *G*, federal purchases explain over 80% of the variation.

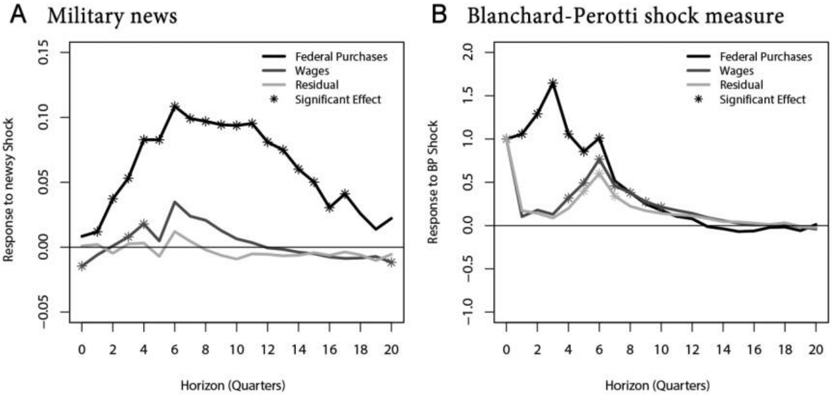


FIG. 1.—Response of $G$ components to established fiscal shocks. The figure shows impulse responses of the components of $G$ to fiscal shocks based on the shock measure and the local projection (and controls) of Ramey and Zubairy (2018). Asterisks indicate point estimates that are statistically significantly different from zero at the 5% level.

components of $G$. A likely explanation is that fluctuations in the federal government's demand for goods and services naturally arise in a much lumpier fashion than demand for government workers. For example, the need for military spending may change drastically, whereas the need for government analysts is quite stable. Moreover, protective labor laws make it difficult for the federal government to adjust spending through the labor margin, especially downward.

Second, identified government spending shocks materialize almost exclusively as federal purchases, again measured by the NIPA proxy. In contrast, the other components of $G$ respond very little to government spending shocks. To establish this point, we estimate impulse responses of the different components of $G$ to the defense news measure and the Blanchard-Perotti measure of fiscal shocks, compiled by Ramey and Zubairy (2018). Figure 1 shows the response of each government spending component at time $t + h$ to the fiscal shock at time $t$ over a horizon of 20 quarters. Asterisks indicate point estimates that are statistically significantly different from zero at the 5% level. Federal purchases exhibit the strongest response to both shocks, whereas wages and the residual components barely react.[6] Hence, federal purchases not only account for the bulk of variation in $G$, they also account almost exclusively for the variation that is caused by established government spending shocks.

Third, federal purchases do not feature prominently in the two largest fiscal stimulus packages during our sample period: the ARRA of 2009

[6] As we show in the appendix, both shock series move mostly the defense rather than the nondefense component of federal purchases (see online fig. 13).

and the COVID relief packages. These pieces of legislation represented sizeable discretionary fiscal stimulus—that is, systematic policy responses to economic crises—totaling about $800 billion and $4.2 trillion, respectively. Yet these stimulus packages were largely comprised of transfers—direct aid to individuals, loans to businesses, tax relief, and so on, which are not part of *G*, that is, exhaustive spending on goods and services by the government. For the ARRA, roughly half of funds were spent on tax relief/incentives and direct aid to individuals.[7] For the COVID relief package, these items account for over two-thirds. Instead, federal purchases represented only a very small fraction of these stimulus packages: 5% in the case of ARRA (between 2009 and 2013) and just over 1% for the COVID relief packages.[8]

In sum, our first fact establishes that federal purchases account for the largest part of the variation in *G*, even though they account for only 16% of its level. Federal purchases are also very volatile, and their variation appears largely exogenous to the business cycle rather than a systematic response to the cycle. The following facts partially rationalize this finding.

### *B. Granularity*

We establish a second fact studying in more detail the origins of variation in federal purchases: not only are federal purchases disproportionately important for the variation in *G* (fact 1), this variation itself also emerges from only a few influential sectors and firms.

FACT 2. The variation of federal purchases at business cycle frequency is granular.

1. The top 10 firms (NAICS six sectors) explain 15%–20% (29%–42%) of the variation in federal purchases.
2. Time fixed effects increase the variation explained in the growth rate of federal purchases by 2.2 (0.3) percentage points at the firm (sector) level.

To establish the granular nature of federal purchases, we regress its quarterly growth on the granular residual, following Gabaix (2011). These regressions measure how much variation in the growth of aggregate federal

[7] Oh and Reis (2012) document for a sample of member countries of the Organization for Economic Cooperation and Development that the increase of government expenditures between 2007 and 2009 was mostly in transfers. The effect of transfers is studied by Oh and Reis (2012), Woodford (2022), and Bayer et al. (2023).

[8] ARRA contracts are identified using the Federal Procurement Data System–Next Generation, which publishes a report listing government procurement contracts that were associated with the Recovery Act from 2009 through September 2019. Details on COVID-19 spending come from USAspending.gov.

purchases comes from a few influential firms or sectors rather than from common aggregate movements.[9] We start by calculating the granular residual, $\Gamma_t$, for federal purchases. We denote total purchases from sector or firm $i$ in month $t$ by $g_{i,t}$ and the four-quarter growth rate of these purchases by $z_{i,t} = \ln(g_{i,t}) - \ln(g_{i,t-4})$.[10] The granular residual is defined as follows:

$$\Gamma_t = \sum_{i=1}^{K} \frac{g_{i,t-4}}{G_{t-4}} (z_{i,t} - \bar{z}_t), \tag{1}$$

where $G_t$ are aggregate federal purchases in quarter $t$ and $\bar{z}_t = Q^{-1}\Sigma_{i=1}^{v} z_{i,t}$ is the average growth rate of purchases from the top $Q$ sectors or firms. In other words, the granular residual is the weighted difference in growth rates for the top $K$ sectors or firms relative to the average growth rate for the top $Q$ sectors or firms, where $Q \geq K$. We calculate the granular residual over the top $K = 10$ six-digit NAICS sectors or firms (defined in terms of overall purchases over the full sample period) and take averages over the top $Q = 1{,}000$ sectors or firms. The granular residual provides a measure of the importance of idiosyncratic variation in the growth rate of purchases. To see this point, consider the case in which variation in government spending grows at a uniform rate in all sectors or firms: $z_{it} = z_{jt} = \bar{z}_t$. In this case, the granular residual would be zero. Instead, absent perfect correlation, idiosyncratic deviations from any common increase in government spending will be reflected in a nonzero residual.

To quantify the significance of granularity for the variation of federal purchases, we regress the growth rate, $Z_t = \ln(G_t) - \ln(G_{t-4})$, on the granular residual and its lags:

$$Z_t = \beta_0 + \beta_1\Gamma_t + \beta_2\Gamma_{t-1} + \beta_3\Gamma_{t-2}. \tag{2}$$

Results are highly statistically significant and reported in panel A of table 2. Columns 1 and 2 for sectors and columns 3 and 4 for firms show that the granular residual explains 28%–42% and 14%–20%, respectively, of the variation of the growth rate of federal purchases, measured in terms of $R^2$. These results are in the range of Gabaix's (2011) estimates for the explanatory power of the granular residual for GDP growth.

As a complementary perspective on the granular nature of federal purchases, we show that granular rather than aggregate variation characterizes the government spending process, with the associated innovations often strongly positively or negatively correlated. We proceed under the

[9] Using the approach of Foerster, Sarte, and Watson (2011), we obtain similar results (see app. sec. B.1). For evidence on the variation of contracts within firms, see app. sec. B.5. The distribution of contracts, firms, and sectors can be well approximated by a lognormal distribution (see app. sec. B.6).

[10] We use four-quarter growth rates to deal with the highly seasonal nature of the data.

TABLE 2
GRANULARITY IN VARIATION OF FEDERAL PURCHASES

| | Sectors | | Firms | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | A. Explanatory Power of Granular Residual for Aggregate Purchases | | | |
| $\Gamma_t$ | 1.010*** | .884*** | .883*** | .791** |
| | (.190) | (.177) | (.257) | (.256) |
| $\Gamma_{t-1}$ | | −.655*** | | −.534 |
| | | (.177) | | (.269) |
| $\Gamma_{t-2}$ | | −.364* | | −.374 |
| | | (.176) | | (.265) |
| Observations | 72 | 72 | 72 | 72 |
| $R^2$ | .287 | .422 | .144 | .203 |
| | B. Importance of Aggregate Factors for Purchases at Sector/Firm Level | | | |
| Lagged spending | .472*** | .440*** | .590*** | .579 |
| | (.021) | (.022) | (.012) | (.013) |
| Sector fixed effects | Yes | Yes | No | No |
| Firm fixed effects | No | No | Yes | Yes |
| Time fixed effects | No | Yes | No | Yes |
| Observations | 1,750 | 1,750 | 5,003 | 5,003 |
| $R^2$ | .943 | .947 | .602 | .624 |

NOTE.—In panel A, we show the results of estimating the following regression: $Z_t = \beta_0 + \beta_1\Gamma_t + \beta_2\Gamma_{t-1} + \beta_3\Gamma_{t-2}$, where $Z_t = \ln(G_t) - \ln(G_{t-4})$ is the quarterly year-on-year growth rate of $G_t$ and the $\Gamma_t$'s are the granular residual and its lags, given by $\Gamma_t = \sum_{i=1}^{K}(g_{i,t-4}/G_{t-4})(z_{i,t} - \bar{z}_t)$, where $K = 10$. $G_t$ is total government spending in period $t$, and $\bar{z}_t$ is the average growth rate over the top $Q = 1{,}000$ six-digit NAICS sectors or firms. In panel B, we show the results of estimating the following regression at a quarterly frequency: $g_{i,t} = \alpha_0 + \rho g_{i,t-4} + \alpha_i + \varepsilon_{i,t}$, where $g_{i,t}$ denotes sectoral- or firm-level purchases. Columns 2 and 4 also include time fixed effects, $\alpha_t$, in addition to sector or firm fixed effects. We use data from 2001–19 to avoid the COVID period. Standard errors are in parentheses.
* $p < .05$.
** $p < .01$.
*** $p < .001$.

assumption that the processes for purchases can be approximated by AR(1) processes:

$$g_{i,t+1} = \alpha_0 + \alpha_i + \alpha_t + \rho_i g_{i,t} + \varepsilon_{i,t+1}, \tag{3}$$

where $g_{i,t}$ denotes the log of purchases from the two-digit sector or firm $i$ at time $t$. Parameters $\alpha_i$ and $\alpha_t$ are sectoral/firm and time fixed effects, respectively. Our main interest lies in the statistical importance of common aggregate factors, captured by $\alpha_t$.

Our findings, reported in panel B of table 2, are twofold. First, estimating the specification at a quarterly frequency and at the sector level, we find that the inclusion of time fixed effects, $\alpha_t$, only marginally raises the $R^2$ from 94.3% to 94.7% (compare cols. 1 and 2). We get similar results when we estimate model (3) using purchases from the top 100 firms: the inclusion of time fixed effects only raises the $R^2$ from 60.2% to 62.4% (cols. 3 and 4). Hence, aggregate variation does not explain much of the time variation of purchases at the firm and sector level; instead, firm- and sector-specific variation is far more important. Second, we find that large positive and negative correlations characterize innovations for many sector or large-firm pairs. While centered around zero by construction, many of the correlations are between −0.5 and 0.5 (see online fig. 5), consistent with substantial granular variation.

Hence, aggregate shocks to government spending seem to play a limited role at business cycle frequency, contrary to what conventional models assume. Yet we cannot formally reject the notion that $G$ is determined by a political process and then allocated top-down across various firms or industries: the allocation could follow nonuniform random processes, consistent with the importance of the granular residual. For example, any extra spending could randomly go to a single firm or sector. However, as we show in the context of fact 4 below, a few sectors and firms with long tenure in the data receive an approximately constant share of federal purchases over time. In light of this additional fact, fact 2 is more consistent with a constituent model of government spending growth: variation in spending arises bottom-up rather than top-down.

### C. *Procurement and Bidding*

Our next fact concerns the process through which the government purchases goods. Conventional New Keynesian business cycle models frequently make a simple assumption: the government goes out and buys goods at sticky prices, with firms perfectly elastically supplying all quantities demanded. This assumption is at odds with the facts about the public procurement process, which we characterize in our third fact.

FACT 3. Federal purchases are not off-the-shelf.

1. The solicitation period lasts from 0 days to over 5 years, with an average of 55 days.
2. Sixty-seven percent of federal purchases are awarded through full and open competition and have a mean (median) number of eight (three) bids.

Federal purchases are made through a two-stage process: a solicitation period, in which the government solicits proposals for work, followed by a selection period. Earlier work has highlighted various aspects of this process, focusing mostly on determinants of the quality of the procurement outcome, such as public sector capacity, management structures, or political connections (Bandiera, Prat, and Valletti 2009; Chakravarty and MacLeod 2009; Gagnepain, Ivaldi, and Martimort 2013; Coviello, Guglielmo, and Spagnolo 2018; Campos et al. 2021).[11] On the basis of our comprehensive data set, we instead seek to characterize key parameters of the public procurement process more broadly.[12]

When the federal government wishes to purchase a good or service, it begins by issuing a solicitation. The Federal Acquisition Regulation requires that contract actions are publicized in order to increase competition, broaden participation, and assist small and minority-owned businesses in obtaining (sub-)contracts. Contracting officers must publicize solicitations on the System for Award Management (SAM) website (https://sam.gov), where organizations can then submit bids or proposals and compete for the contract. The duration of the solicitation period varies from acquisition to acquisition. Contracting officers are required to allow a minimum of 30–45 days of response time, but exceptions exist depending on the circumstances (e.g., urgency). We do not have information about the length of the solicitation process in our main data set. However, a sample of about 10,000 active solicitations posted on SAM provides a sense of the distribution of the length of the solicitation period: the average (median) number of days between the solicitation publication date and the response deadline is 55 days (19 days) and ranges from 0 days to over 5 years.[13] Importantly, this solicitation process occurs for all types of government purchases, from coffee cake mix purchased for a federal prison to ballistic laser training systems to be utilized by the US Coast Guard.

The solicitation procedure is designed to increase competition in the selection process—the second stage of the procurement process. We can characterize this stage in detail using our data, because contracts are categorized into one of nine different "extent competed" categories. Table 3 shows that roughly two-thirds of contracts by count and just over half of contract dollar value are awarded competitively. We define fully competed contracts as those classified under full and open competition, competitive delivery orders, and simplified acquisition procedures—a designation given to contracts under a certain dollar threshold.

[11] Procurement outcomes also depend on how contracts are renegotiated and enforced (Bajari, Houghton, and Tadelis 2014; Ryan 2020). Evidence for wasteful year-end spending hints toward political economy frictions in procurement (Liebman and Mahoney 2017). Bosio et al. (2022) offer a comprehensive international perspective on public procurement, highlighting the interplay of legal frameworks and actual practice in 187 countries.

[12] Subsets of this data have been used to study specific aspects of the procurement process. Warren (2014) analyze approximately 150,000 contracts from civilian agencies to assess the effect of workload on various characteristics of the procurement process, including the reliance on competitive procedures. Decarolis et al. (2020) focus on the procurement of services and works, relying on data for 122,533 projects to analyze how bureaucratic competence impacts procurement outcomes.

[13] We consider solicitations, presolicitations, and combined synopsis/solicitations that were posted on or before July 1, 2022.

TABLE 3
Competition and Bidding Summary Statistics (2001–21)

| | Bidding (Offers Received) | | |
|---|---|---|---|
| Extent Competed | Share of Contracts (Weighted) | Mean (Weighted) | Median (Weighted) |
| Fully competed | .67 (.56) | 8.0 (5.7) | 3 (3) |
| Partially competed | .08 (.11) | 6.6 (7.5) | 3 (3) |
| Not competed | .24 (.33) | 1.1 (1.3) | 1 (1) |

Note.—The table shows summary statistics of contracts characterized by their degree of competition and bidding. Here, we exclude contracts for which the number of offers received is coded as 999 (<2% of the value of the contracts with no usable numerical record). Values in parentheses are weighted by contract value.

About 10% of contracts by count and value are partially competed, a category that contains contracts that are classified as full and open competition after exclusion of sources, whereas two-thirds of contracts by value are fully competed. At the same time, full and open competition does not necessarily imply a large number of bidders. The weighted and unweighted median number of bidders in contracts awarded through full and open competition is 3. The mean (weighted mean) number of bidders is higher at 8.0 (5.7), an observation that Kang and Miller (2022) rationalize in a principal-agent model in which the procurement agency can extract informational rents from sellers.

In some circumstances, procurement contracts can be designated as not available for competition, meaning that the contracting agency does not require a full and openly competitive bidding process. This is permissible, for instance, when supplies are available from only one or a limited number of responsible sources. As a whole, about 25% of contracts by count (33% by value) are noncompetitive. This share doubles when it comes to contracts going to the top 10 firms. Considerable heterogeneity across sectors in the share of competed contracts also exists. The share of dollars allocated through bidding ranges from 13% at the low end in NAICS 336411 (aircraft manufacturing) to 76% at the high end in NAICS 517110 (wired telecommunications carriers). At the NAICS two-digit level, the share of dollars allocated through bidding ranges from 88% in NAICS 32 (manufacturing) to a low of 30% in the utilities sector (NAICS 22).

These facts have two important implications. First, while not ruling out perfectly elastic supply at sticky prices, as the New Keynesian model assumes, they suggest that sticky prices are unlikely to dominate public procurement.[14] Second, they also point to the challenge that policy makers

[14] Our data do not allow us to directly follow prices for the same goods in a consistent fashion as the Bureau of Labor Statistics (2011) does to construct the PPI. The main reason lies in the absence of quantity data, so (changes in) nominal contract values cannot be attributed to (changes in) prices or quantities.

face if they seek to adjust the level of government spending to cyclical fluctuations in a timely manner, providing a fresh perspective on the implementation lag, which features prominently in the traditional debate about fiscal policy.

### *D. Durations*

The modern literature on fiscal policy emphasizes the persistence of shifts in government spending as a key determinant of their effects (Baxter and King 1993). And conventional models of fiscal policy take a certain persistence of fiscal shocks for granted. As we zoom in on the duration of the contracts underlying federal purchases and the tenure of firms in the data as recipients of those contracts, our next fact provides a rationale for why shifts in government spending are persistent in the first place.

FACT 4. Long durations of contracts and firm tenure.

1. For contracts, the value-weighted median of the duration is 1,279 days; the 10th and 90th percentiles are 92 and 4,411 days, respectively.
2. For firms, the value-weighted median tenure in the data is 18 years; the 10th and 90th percentiles are 6 and 19 years, respectively.

The USAspending.gov data are recorded at the transaction level: a contract comprises one or more transactions. We calculate the duration of a contract as the time between the start date of the first associated transaction and the end date of the final transaction.[15] The majority of the contracts in our sample—87%—are made up of a single transaction; however, these single-transaction contracts represent only 17% of total contracted dollars. The remaining 83% of contracted dollars are contained in multi-transaction contracts.

When we calculate statistics about the duration of contracts weighted by value, we find that contracted dollars are largely concentrated in contracts that are long in duration. The median of the value-weighted duration of contracts is 1,279 days (about 3.5 years), and the 10th and 90th percentiles are 92 and 4,411 days, respectively. Similarly, the government's relationships with individual firms—or firms' tenure in the data—are also relatively long-lived. To be considered in the data in a given year, we require that a firm must be associated with a new contract transaction in that year. We then refer to firm tenure as the number of years that a firm is in the data. The value-weighted median firm tenure in the data is 18 years, with 10th and 90th percentiles of 6 and 19 years, respectively. We also observe

[15] We consider only contracts with a duration between 0 and 7,300 days (20 years), a subsample that contains 99% of the contract value in our data.

that the top firms and sectors consistently capture a large share of federal purchases over time (see online figs. 9, 10). As discussed in section III.B, long firm tenure and stable spending shares support a bottom-up view of *G*.

Because the majority of contracts by count are smaller single-transaction contracts, a different picture emerges if we compute the corresponding statistics without weighting by contract value. The unweighted median contract has a duration of only 25 days, and the vast majority of contracts have durations that are shorter than 1 year: the 10th percentile of the duration of contracts is 1 day, and the 90th percentile is 333 days. Figure 2*A* displays the weighted and unweighted distributions. Similarly, firm tenure in the data also appears much shorter when we consider the unweighted statistics: the unweighted median tenure of a firm in the data is 2 years, and the 10th and 90th percentiles are 1 and 9 years, respectively. Figure 2*B* shows the weighted and unweighted distributions of firm tenure.

The small subset of multitransaction contracts almost exclusively drives the long median duration of weighted contract length. Even when weighted by value, single-transaction contracts tend to have much shorter durations, with a median of only 115 days. Yet whereas single-transaction contracts represent 87% of contracts by count, they represent only 17% of value. Hence, the bulk of purchases that come in the form of large multitransaction contracts is characterized by long durations. The correlation between contract size and duration is 0.5 when using total contract dollar values as weights. However, if we use total contract values

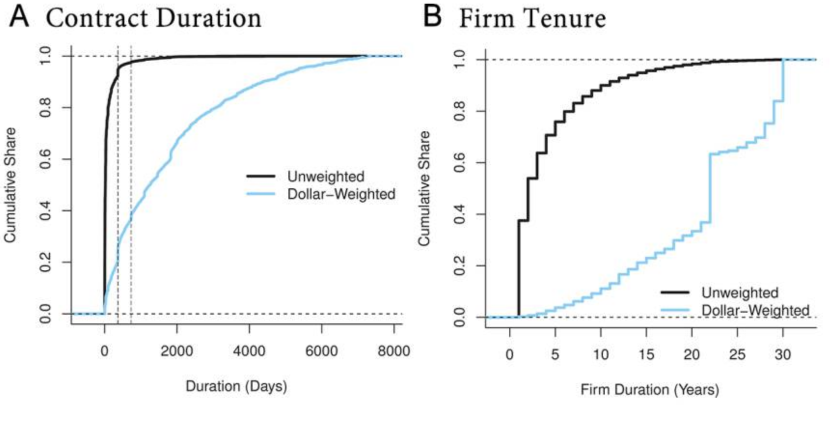


FIG. 2.—Distribution of contract and firm duration. The figure shows the unweighted and weighted empirical cumulative distribution functions of contract duration (*A*) and firm duration (*B*). In the weighted figures, weights are given by the total value of the contract (*A*) and the total value of obligations the firm receives (*B*). Vertical dashed lines in *A* represent the 1- and 2-year marks.

normalized by the number of contract days as weights, the correlation between size and duration vanishes and is −0.01 (see also online fig. 11).[16]

The granular influence of these large contracts and firms—while dominating the average relationship—camouflages substantial underlying heterogeneity in the relationship between contract/firm size and duration/tenure. Only 40% of large contracts (above the 90th percentile in value) are long in duration (above the 90th percentile in duration); 5% of large contracts are actually short in duration (below the 10th percentile). A similar pattern holds for firms: only 44% of large firms (above the 90th percentile in lifetime contract obligations) are also characterized by long duration in the data (above the 90th percentile), and 3% are characterized by a short duration (only 1 year).

The fact that the majority of federal purchases occurs through long-term contracts limits the scope for discretionary spending plans. However, one dimension along which the government may command some flexibility in making its purchases is through contract modifications. In the data, if a contract is modified after the initial award, subsequent transactions are recorded with a modification number.[17] In particular, exercising an option—a proxy for the notion of a shovel-ready spending plan—amounts to 10% of federal purchases. In general, purchases associated with modifications are unsurprisingly substantial. Their share is in fact higher than that of purchases from initial nonmodification contracts: 55% of contract dollars are obligated through contract modifications, illustrating that the ability to make adjustments is heavily used.

### *E. Sectoral Bias*

While the first four facts concern the dynamics governing federal contract spending, our final fact highlights a cross-sectional property: a systematic bias in the distribution of federal purchases across firms and sectors relative to private spending. Moreover, federal purchases tend to be concentrated in sectors in which private sector prices are relatively sticky.

[16] Overall, large persistent contracts in specific sectors partially (but not fully) explain why some sectors are large suppliers to the government via contracts: average contract size explains about 70% of the variation in sectoral shares. We provide further distributional details in app. sec. B.8.

[17] More than 20 types of modifications exist, with some that reflect no change to the value of the contract (e.g., a change of address) but others that reflect additional obligations or deobligations (e.g., an order for additional work or exercising an option that was established in the initial award). The three most common modifications by count cover "other administrative actions," "funding-only actions," and "supplemental agreements for work within scope." By value, the three most common modifications are "funding-only actions," "exercise an option," and "supplemental agreements for work within scope."

FACT 5. Sectoral bias.

1. The top three firms (six-digit, two-digit sectors) account for 10% (23%, 67%) of federal purchases but only 0.6% (1.2%, 18%) of private spending.
2. The average frequency of price changes in sectors in which government purchases are concentrated is 11%—half the frequency of the remaining sectors (22%).

The first point generalizes the sectoral bias during specific military buildups established by Ramey and Shapiro (1998). The bias turns out to be a pertinent feature of the data for the past two decades: at business cycle frequency (rather than for specific long swings in the data), for the universe of federal contracts including nondefense contracts (which make up half of federal contracts by count), and by looking at both the firm and the sector level.

The distribution of federal purchases across sectors is quite distinct from that of private spending. The top three two-digit NAICS sectors—manufacturing (33); professional, scientific, and technical services (54); and administrative and waste management (56)—receive 67% of federal purchases but account for only 18% of private spending. We illustrate this point graphically at the six-digit sector and firm level in figure 3.

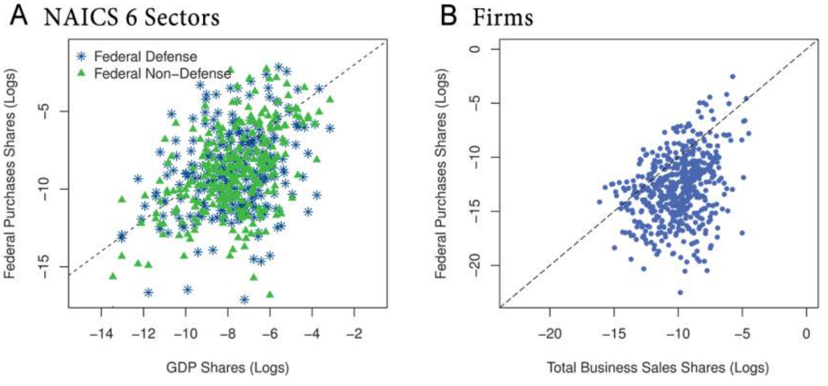


FIG. 3.—Federal versus private consumption shares. *A* shows the distribution of federal purchase shares across 350 sectors (*y*-axis) plotted against the distribution of sectoral GDP in the economy (*x*-axis). Values represent averages over the 2001–21 sample period. Sectoral GDP is calculated as total industry output net of output sold as intermediates to other sectors and net exports. Data sources are the BEA Input-Output Accounts (make and use tables). *B* shows average federal purchase share over 2001–18 versus private spending shares for 538 firms that we can match with Compustat. Private sales shares are calculated as firm sales (from Compustat) over total business sales in the United States (from the US Census Bureau). We match firms in the contracts data to firms in Compustat using the concordance from Hebous and Zimmermann (2021).

TABLE 4
SECTORAL BIAS FOR TOP FIRMS AND SECTORS

| | Government Share (%) | Private Share (%) |
|---|---|---|
| | A. Firms | |
| Lockheed Martin | 8.0 | .3 |
| Northrop Grumman | 1.5 | .2 |
| Leidos Holdings | 1.2 | .1 |
| | B. Six-Digit Sectors | |
| 541700: Scientific research and development services | 8.3 | .2 |
| 336411: Aircraft manufacturing | 7.8 | .4 |
| 541300: Architectural, engineering, related services | 7.2 | .6 |
| | C. Two-Digit Sectors | |
| 33: Manufacturing | 30.6 | 12.7 |
| 54: Professional, scientific, technical services | 28.1 | 4.0 |
| 56: Administrative and waste management | 8.8 | 1.1 |

NOTE.—The table shows spending shares for top federal spending firms and sectors. Six-digit sectors are from the BEA Input-Output Accounts, roughly corresponding to six-digit NAICS sectors. Private shares are measured by total business sales for firms and output sold as final goods for sectors (net of output sold as intermediates to other sectors and net exports).

In figure 3*A*, the vertical axis measures the share of a six-digit sector $k$ in federal purchases (shown in logs), $G_k/G$, and the horizontal axis measures the (log) share of the same sector in GDP, $\mathrm{GDP}_k/\mathrm{GDP}$. $\mathrm{GDP}_k$ is computed using the make and use tables of the US Bureau of Economic Analysis (BEA) as total industry output net of output sold as intermediates to other sectors (including itself) and net exports. This measure represents the portion of sector $k$'s output that is sold as final goods.

If government spending and private spending had the same composition, the shares would align perfectly along a 45° line. However, government and private spending shares differ substantially, that is, $(G_k/G) \neq (\mathrm{GDP}_k/\mathrm{GDP})$. Some sectors that are big suppliers to the federal government are almost negligible for GDP. Figure 3*B* shows that a similar pattern holds at the firm level, where we compare the shares of individual firms in government purchases to their shares in total US business sales. As an illustrative example, table 4 reports the spending shares of government and private spending for the top firms and sectors. The top firms and sectors represent a much larger portion of government purchases than purchases in the private sector.[18] Generally, these top government suppliers

[18] In line with these findings, federal purchases are also much more concentrated in a few firms compared with concentration in private purchases, with less of a difference at the industry level. For example, the top 10 sellers to the government capture 29.3% of federal purchases, whereas the top 10 sellers to the private sector capture only 10.4% of private purchases. Online table 8 presents some summary statistics.

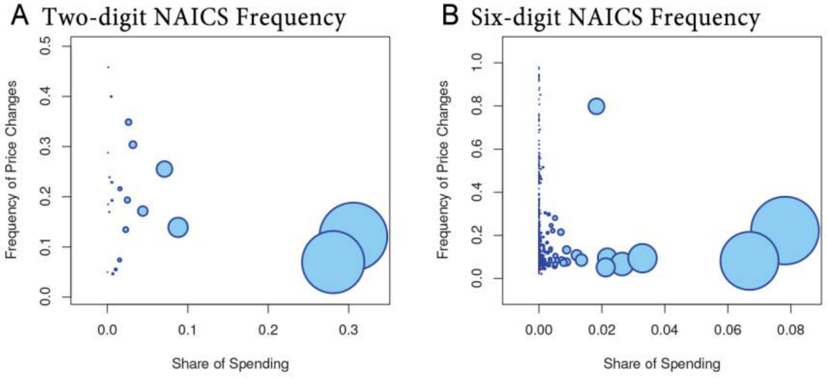


Fig. 4.—Frequency of price adjustment. The figure shows the average annual share of federal purchases in each two- and six-digit sector, plotted against the frequency of price changes for private purchases in these sectors, based on Bureau of Labor Statistics and USAspending.gov data. The size of the circles emphasizes the average sectoral share of annual spending (also shown on the *x*-axis).

also have a higher number of employees and more R&D expenditures, assets, gross profits, and invested capital relative to the median firms in Compustat, as online table 7 summarizes.

Our second cross-sectional observation is that federal purchases are concentrated, on average, in sectors with relatively sticky prices for private purchases. The average frequency of price changes in the sectors in which the government purchases are concentrated is about half the frequency of the remaining sectors. Our analysis builds on data for the frequency of monthly price changes computed by Pasten, Schoenle, and Weber (2020). This frequency data is based on prices for sales to the private sector. While it might also be of general interest to analyze price setting of firms when they sell to the government, our data do not allow for such an analysis, because we observe only nominal contract values and thus cannot discern price or quantity movements.[19] Still, when we relate the incidence of federal purchases to the prices charged for private purchases in the same sectors, a robust finding emerges: government spending is heavily concentrated in sectors with relatively sticky prices for private purchases.

Figure 4 illustrates this fact at the two-digit and six-digit levels. The size of the circles corresponds to the average sector share in annual federal purchases (the circles merely highlight visually the numerical information on the *x*-axis). The government spends the vast majority of dollars in sectors with low frequencies of price adjustment by the private sector: the frequency of price changes for private sector purchases is 11% in the largest three two-digit NAICS sectors in which the government purchases

[19] We provide additional details and examples in app. sec. B.4.

(the same sectors shown in panel C of table 4). The average frequency of price changes is 22% for all remaining sectors. Figure 4*B* illustrates a similar pattern at a more disaggregated level.

In sum, federal purchases are biased toward specific sectors, and these sectors stand out in terms of the stickiness of private sector prices. In order to evaluate the implications of this fact for the fiscal transmission mechanism, we resort to a stylized model of the business cycle.

## IV. Revisiting the Fiscal Transmission Mechanism

The five facts matter for how fiscal policy operates. To illustrate this point in a transparent way, we rely on a two-sector version of the New Keynesian model, which is frequently used to study the effects of government spending (e.g., Woodford 2011). Most model parts are standard, and we relegate details to appendix D. Here, we highlight the modeling choices that are relevant in light of the empirical facts.

To account for fact 1, we follow much of the earlier work and study the effect of exogenous variation in government spending. Consistent with fact 2 but in contrast to much of the earlier work, we model government spending bottom-up, starting at the firm level. We use $j \in [0, 1]$ to index firms: $n$ firms operate in sector 1 and the remaining firms in sector 2. Letting $G_t(j)$ denote the government's purchases from firm $j$, we define sector-level purchases as $G_{1,t} \equiv \int_0^n G_t(j)\,dj$ and $G_{2,t} \equiv \int_n^1 G_t(j)\,dj$. Eventually, only the variation of spending at the sector level matters for aggregate dynamics because labor (the only factor of production) is mobile within and immobile across sectors: changing the level of government purchases for one firm affects the wage and hence marginal costs of all firms in the sector equally.

Importantly, in line with fact 3 and again different from much of the earlier work, we do not assume that the government necessarily makes its purchases at posted prices. Instead, as we explain below, our setup allows us to remain agnostic about how procurement prices are determined. If we let $P_t^G(j)$ denote the price that firm $j$ charges to the government, the tax bill of government purchases is given by $T_t = \int_0^1 P_t^G(j)G_t(j)\,dj$. These taxes are levied on households in a lump-sum fashion. Their timing is irrelevant because Ricardian equivalence holds. In line with common practice, for which fact 4 provides support, we specify a persistent process for government spending for both sectors. We do so in terms of deviations from steady state, denoted with small-scale letters, $g_{1,t}$ and $g_{2,t}$. Parameter $\rho$ governs the persistence of the AR(1) process at the sector level. We assume identical persistence in both sectors but relax this assumption in our numerical analysis below.

Last, in accordance with fact 5, sectors differ in terms of their (1) importance for private and public spending (sectoral bias) and (2) pricing frictions. We abstract from investment and model private spending as consumption. Parameters $s_1^{\mathrm{p}}$ and $s_1^{\mathrm{g}}$ capture the steady-state shares of private and government spending in sector 1. The remaining share goes to sector 2. If we let $s^p$ denote the share of private spending in GDP, the size of sector 1 and 2 is given by $n = s^{\mathrm{p}} s_1^{\mathrm{p}} + (1 - s^{\mathrm{p}}) s_1^{\mathrm{g}}$ and $1 - n$, respectively.

The Calvo parameters $\alpha_1$ and $\alpha_2$ capture the degree of rigidity of private sector prices in the two sectors. Importantly, we do not model procurement prices (and the details of the procurement process) at all because they are, in fact, irrelevant for macroeconomic outcomes. It is also irrelevant whether they are adjusted infrequently or not. First, they are not allocative because spending is determined exogenously. Second, given that the central bank targets consumer price inflation and taxes are lump sum, the prices charged to the government also do not matter for the equilibrium allocation. To see this point, consider aggregate firm profits, given by

$$\Gamma_t = \int_0^1 \left\{ P_t(j) C_t(j) + P_t^G(j) G_t(j) - W_t(j) [G_t(j) + C_t(j)] \right\} dj,$$

where $P_t(j)$, $C_t(j)$, and $W_t(j)$ denote prices charged to consumers, private consumption goods, and wages. Profits $\Gamma_t$ are rebated to households lump sum: adding the tax bill $T_t$ and profits $\Gamma_t$ shows that procurement prices do not enter the household's budget. Intuitively, a firm may overcharge the government and earn an extra profit that is then paid out to the household via dividends. Yet these dividends are eventually funded by the same household's tax bill.[20]

We solve the model on the basis of a first-order approximation of the equilibrium conditions around a zero inflation steady state. We first assume that monetary policy pursues a strict inflation target to derive a number of closed-form results in section IV.A. Afterward, we run numerical simulations assuming a Taylor rule. To study the role of monetary policy in more detail, we consider a simplified version of the model in appendix E; moreover, we explore model extensions featuring rule-of-thumb households and behavioral agents as in Galí, López-Salido, and Vallés (2007) and Gabaix (2020), respectively, in appendix F. Qualitatively, all results we establish below hold in these model extensions.

### A. *The Aggregate Effect of Sectoral Shocks*

We now establish how shocks to government spending impact aggregate output. We focus on sectoral shocks, which in turn may represent granular

[20] Once we consider alternative model specifications and depart from Ricardian equivalence, the gap between procurement and private sector prices has real effects through the government budget (see app. F).

shocks at the firm level. Moreover, to obtain closed-form results, we assume for now flexible prices in sector 1, $\alpha_1 = 0$, whereas we allow for nominal price rigidity in sector 2, $\alpha_2 \in (0, 1]$. In our two-sector model, aggregate private demand and output respond sluggishly to even purely transitory shocks. The adjustment dynamics are governed by the evolution of prices in sector 1, $p_{1,t}$, relative to those in sector 2, $p_{2,t}$, which we refer to as the terms of trade, $\tau_t \equiv p_{1,t} - p_{2,t}$, and for which we solve first.

PROPOSITION 1 (Solution for terms of trade). If we assume that prices in sector 1 are fully flexible ($\alpha_1 = 0$) and monetary policy targets consumer price inflation ($\pi_t = 0$), the solution for the terms of trade is given by

$$\tau_t = \Lambda_0 \tau_{t-1} + \Lambda_1 (1 - s^{\mathrm{p}}) s_1^{\mathrm{g}} g_{1,t} - \Lambda_2 (1 - s^{\mathrm{p}}) s_2^{\mathrm{g}} g_{2,t}, \tag{4}$$

where $s_2^{\mathrm{g}} = 1 - s_1^{\mathrm{g}}$, $\Lambda_0 \in (0, 1)$ and $\Lambda_1, \Lambda_2 \geq 0$.

We provide proofs for all propositions and expressions in terms of the underlying model parameters in appendix section D.4. Proposition 1 simply states that, all else equal, government spending in sector 1 increases the terms of trade and conversely for spending in sector 2. The next proposition establishes our main result.

PROPOSITION 2 (Crowding out of consumption). If we assume that prices in sector 1 are fully flexible and monetary policy targets consumer price inflation, the solution for consumption is

$$c_t = \Theta_0 \tau_{t-1} - \Theta_1 (1 - s^{\mathrm{p}}) s_1^{\mathrm{g}} g_{1,t} - \Theta_2 (1 - s^{\mathrm{p}}) s_2^{\mathrm{g}} g_{2,t}, \tag{5}$$

where $\Theta_0 \in (0, 1)$, $\Theta_1 \in [0, \infty)$, and $\Theta_2 \in [0, (s^{\mathrm{p}})^{-1}]$, with $\partial\Theta_1/\partial\rho < 0$, $\partial\Theta_1/\partial\alpha_2 > 0$, $\partial\Theta_2/\partial\rho > 0$, and $\partial\Theta_2/\partial\alpha_2 < 0$, and the ratio $\Theta_1/\Theta_2$ is increasing in $s_1^{\mathrm{p}} - s_1^{\mathrm{g}}$.

Higher terms of trade imply higher consumption ($\Theta_0 > 0$) because they put downward pressure on marginal costs in sector 1. For markups to remain constant in this flex price sector, consumption needs to go up in order to put upward pressure on the real wage. Our main result, however, is that the consumption response to a government spending shock differs depending on the sector in which the fiscal impulse originates. It is captured by coefficients $\Theta_1$ and $\Theta_2$, whereas the terms $(1 - s^{\mathrm{p}}) s_1^{\mathrm{g}}$ and $(1 - s^{\mathrm{p}}) s_2^{\mathrm{g}}$ in expression (5) normalize the size of the shock to one unit of steady-state output. $\Theta_1$ and $\Theta_2$ are both nonnegative: sectoral government spending crowds out consumption.[21] The strength of the crowding out depends on (1) sectors' relative pricing frictions, (2) the composition of private and government demand in steady state, and (3) the persistence of the shock. We discuss these features in turn.

[21] For the model extensions, we find that the extent of crowding out is reduced or even crowding-in occurs, but the sources of heterogeneity in the response to sectoral shocks that operate in our baseline model continue to matter (see app. F).

Consider the differential pricing friction first. An increase in government spending in either of the two sectors raises production and employment as well as marginal costs in the sector. As a result, upward pressure on prices arises, which induces monetary policy to raise interest rates and incentivizes households to lower consumption. Hence, a sectoral shock spills over to the other sector, and its macro impact is potentially large because monetary policy can steer only aggregate rather than sectoral demand. In fact, the extent of crowding out is potentially unlimited if the shock originates in the flex-price sector 1, $\Theta_1 \in [0, \infty)$ (see also app. E). Further, as price stickiness in sector 2 increases, monetary policy needs to respond more aggressively in order to offset the inflationary pressure resulting from a sector 1 shock. As a result, more crowding out occurs as price stickiness (in sector 2) increases: $\partial\Theta_1/\partial\alpha_2 > 0$. However, $\partial\Theta_2/\partial\alpha_2 < 0$: if sector 2 prices are stickier and hence less responsive to sector 2 shocks, monetary policy needs to react less to engineer a contraction of consumption to stabilize inflation. Note that whereas monetary policy shapes the overall response of the economy to the fiscal impulse, the monetary stance is of little consequence for how strongly the economy adjusts to sector 1 shocks relative to sector 2 shocks. In fact, when we study interest rate rules (instead of inflation targeting), we are able to establish conditions under which the ratio $\Theta_1/\Theta_2$ is completely independent of the parameters of the policy rule (see app. E).

Second, we consider the role of the sectoral composition of private and government demand in steady state, captured by $s_1^{\mathrm{p}}$ and $s_1^{\mathrm{g}}$. To develop intuition, we assume that all prices are flexible ($\alpha_2 = 0$) so that monetary policy plays no role in the consumption response. Instead, the consumption response now depends exclusively on the response of hours worked (the only factor in production). Consider, for the sake of the argument, a positive government spending shock in sector 1. The shock pushes labor demand up in that sector, with $s_1^{\mathrm{p}}$ and $s_1^{\mathrm{g}}$ determining by how much. All else equal, the higher $s_1^{\mathrm{g}}$ is, the larger sector 1 is, because the share of government spending in sector 1 is larger ($n = s_1^{\mathrm{p}} s^{\mathrm{p}} + s_1^{\mathrm{g}}(1 - s^{\mathrm{p}})$). A government shock of a given magnitude is then smaller relative to the size of the sector and hence the sector's labor market: therefore, less pressure on wages arises the larger $s_1^{\mathrm{g}}$ is, and hours respond less to the shock, resulting in more crowding out. Raising $s_1^{\mathrm{p}}$ has the same effect, but at the same time, consumption is more exposed to a sector 1 shock. As a result, we find that relatively more crowding out occurs in response to a sector 1 shock, as sectoral bias increases: $\Theta_1/\Theta_2$ increases in $s_1^{\mathrm{p}} - s_1^{\mathrm{g}}$. We also establish conditions under which $\Theta_1/\Theta_2 < 1$ in appendix E.

Third, the persistence of the shock matters for the consumption response—but differently for sector 1 and sector 2 shocks. If the shock originates in the flex price sector 1, persistence reduces the crowding out of consumption. If, instead, the shock originates in the sticky price sector 2, persistence strengthens the crowding out of consumption. Intuitively, in the first case, output is supply determined and households' labor supply increases more if the shock is more persistent. In the second case, output is demand determined and consumption decreases more if the shock is more persistent.

Finally, we can now establish the effect of government spending on output, that is, the multiplier.

PROPOSITION 3 (Output multipliers). If we assume that prices in sector 1 are fully flexible and monetary policy targets consumer price inflation, the solution for output is given by

$$y_t = \Gamma_0 \tau_{t-1} + \Gamma_1 (1 - s^{\mathrm{p}}) s_1^{\mathrm{g}} g_{1,t} + \Gamma_2 (1 - s^{\mathrm{p}}) s_2^{\mathrm{g}} g_{2,t}, \tag{6}$$

where $\Gamma_0 \in (0, 1)$, and

$$\Gamma_1 = 1 - s^{\mathrm{p}} \Theta_1 \text{ and } \Gamma_2 = 1 - s^{\mathrm{p}} \Theta_2. \tag{7}$$

Moreover, $\Gamma_0 \in (0, 1)$, $\Gamma_1 \in [1 - 1/s_1^{\mathrm{p}}, 1]$, and $\Gamma_2 \in [0, 1]$.

The coefficients $\Gamma_1$ and $\Gamma_2$ in proposition 3 directly capture the impact multiplier of government spending on output, that is, the change in output caused by a change in government spending equal to one unit of steady-state output. We focus on impact multipliers in proposition 3 in order to highlight differences due to the sectoral origin of shocks and report present value multipliers below. Equation (7) shows that the multipliers are equal to the sum of the direct effect of higher spending on output and the indirect effect on private consumption. Given the results in proposition 2, it follows that $\Gamma_1$ may actually be negative: the fiscal multiplier can be negative, in contrast to the one-sector New Keynesian model.[22] $\Gamma_2$ instead is bounded by zero from below. Moreover, multipliers may not exceed unity, just as in the baseline one-sector New Keynesian model, unless the zero lower bound on interest rates binds (Woodford 2011). Last, we note that the persistence of the shock changes the impact multiplier differently, depending on the sector of origin. Proposition 2 implies that $\Gamma_1$ increases in $\rho$ and $\Gamma_2$ declines in $\rho$.

## *B. Adjustment Dynamics*

We simulate the model to study adjustment dynamics. For this purpose, we assign parameter values in line with the evidence introduced above. In particular, we assume that total government spending amounts to 18.7% of output in steady state and explore the effects of shocks to federal purchases, which account for 16% of total government spending but

[22] Baxter and King (1993) also obtain a negative multiplier in case taxes are distortionary. Instead, we assume throughout that taxes are lump sum.

the bulk of its fluctuations. We set the time discount factor $\beta$ to 0.997, assuming that a period represents 1 month. We set the inverse of the Frisch elasticity $\varphi$ to 4 (e.g., Chetty et al. 2011).

Importantly, we calibrate sector 2 to represent the top four sectors in which federal purchases are concentrated and consumer prices are relatively sticky: manufacturing (32 and 33); professional, scientific, and technical services (54); and administrative support and waste management and remediation services (56). Together they account for 76% of federal purchases (see online table 1). Their size in the economy, measured in terms of value added, instead amounts to only 35%. Accordingly, we set the size of sector 1 to $n = 0.65$. We assume that the sectoral allocation of total government spending in steady state is the same as for federal purchases and set $s_1^g = 1 - 0.76$. Given our sector classification, we compute the average price duration across sectors and obtain values $\alpha_1 = 0.78$ and $\alpha_2 = 0.89$, meaning that private sector prices are considerably more sticky in sector 2 than in sector 1, with implied average price durations of 4.5 and 9 months, respectively. We set the persistence parameters to $\rho_1 = 0.65$ and $\rho_2 = 0.73$. We obtain these values from estimating equation (3) on monthly time series for federal purchases aggregated across the sectors, which account for sectors 1 and 2, respectively. Finally, for monetary policy, we assume a simple interest rate feedback rule, $i_t = 1.5\pi_t$, instead of strict inflation targeting (as sec. IV.A). In this way, we illustrate that our main insights also obtain for alternative specifications of monetary policy.

Our simulation results contrast the effect of a 1 standard deviation shock to federal purchases originating in sectors 1 and 2 (keeping constant the other components of government spending). The estimates for the standard deviations are based on the VAR model that we estimate in section V: they are 13.5% and 13.1% in sector 1 and sector 2, respectively (see also fact 1 above and the accompanying discussion). Figure 5 shows the impulse responses for the two sectors. From top to bottom, we show the responses of federal purchases, consumer price index inflation, the interest rate, and output.

In each panel, the solid line shows the deviation from steady state (vertical axis) in percent for our baseline calibration. The dashed line shows the responses for a counterfactual calibration that assumes a perfectly symmetric economy with $s_1^p = s_1^g = n = 0.5$ and equal pricing frictions for both sectors, corresponding to the weighted average of our baseline ($\alpha_1 = \alpha_2 = 0.81$). In the symmetric model, the effects of the shocks differ only by the sector of origin to the extent that the shocks differ in terms of size and persistence. But these differences are small and immaterial. Note that while the shocks to federal purchases are large, the overall size of the fiscal impulse remains moderate (approximately 0.2 percentage points of steady-state output) because of the small weight of

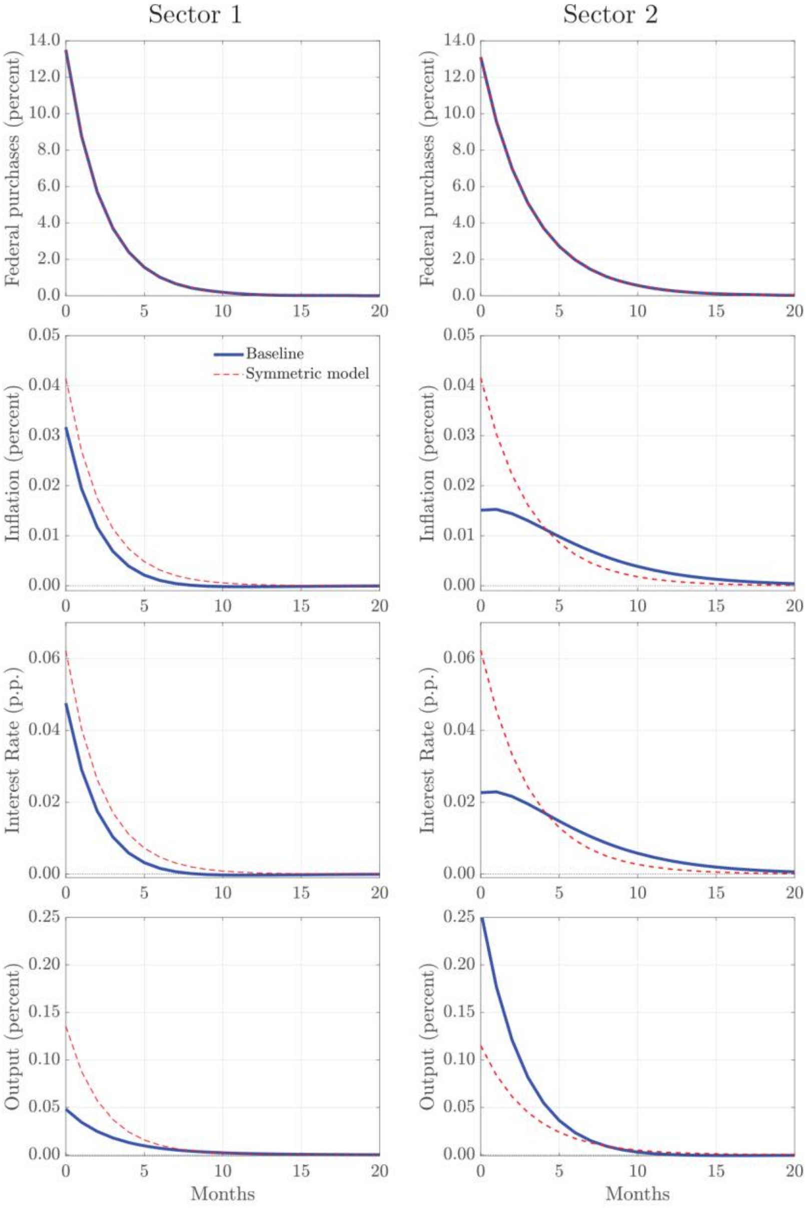


Fig. 5.—Impulse responses to federal purchases shock originating in sectors 1 and 2. The figure shows impulse responses to 1 standard deviation shocks to federal purchases in sector 1 (*left*) and sector 2 (*right*) based on model simulation. Solid line indicates baseline; private sector prices are more sticky in sector 2. Dashed lines show responses for a symmetric calibration. Shock size is 13.5% and 13.1%, in line with VAR estimates of standard deviations based on monthly time series; see section V, which provides the empirically estimated impulse response functions in figure 7. Vertical axis = deviation from steady state; horizontal axis = time in months; p.p. = percentage points.

federal purchases in the economy. Inflation increases because federal purchases push up marginal costs—that is, wages—which in turn triggers a monetary policy tightening. The interest rate goes up and induces sizeable crowding out of consumption (not shown): output increases on average by 0.12 percentage points on impact.

A different picture emerges for our baseline calibration, which accounts for sectoral heterogeneity (solid lines). The effects of the shocks now depend on the sector in which they originate. As discussed above, the difference in responses depends on the price stickiness of both sectors and the sectoral composition of private and government demand in steady state. In response to a sector 1 shock, inflation increases because sector 1 has relatively flexible prices and it has a large weight in aggregate inflation. Instead, in response to a sector 2 shock, inflation is substantially more muted (second row, right panel). Hence, the policy response differs substantially (third row). The inflationary effect of a sector 1 shock causes a sharp monetary tightening, whereas a sector 2 shock induces a more moderate policy reaction. As the Taylor principle holds, the real interest rate moves strongly for a sector 1 shock, crowding out private consumption, but increases less in response to a sector 2 shock, which leads to little crowding out. Such differential crowding out of consumption explains the differential output responses in the bottom panels: output increases only weakly in response to a sector 1 shock but more strongly in response to a sector 2 shock.[23] A similar picture emerges when we add hand-to-mouth households, following Galí, López-Salido, and Vallés (2007), although in this case consumption is crowded-in in response to a sector 2 shock (see app. sec. F.1).

The output effect in figure 5 is generally small because federal purchases account for a small fraction of GDP. Yet our results show how the data on federal purchases may serve as a laboratory to study how sectoral characteristics, such as price stickiness, influence the spending multiplier. Against this background, we now turn to a unit-free measure of the effects of fiscal shocks. Figure 6*A* reports cumulative discounted fiscal multipliers, computed as in Uhlig (2010): the total net present value of the change in output from period 0 to $k$ (horizontal axis), divided by the net present value of the change in government spending (measured in units of steady-state output) over the same period. The solid line represents the multipliers for a sector 1 shock, and the dotted line represents the multipliers for shocks originating in sector 2. The multipliers for sector 2 shocks are considerably larger for all horizons, although the difference

[23] The relative strength of the output effects may flip if monetary policy is constrained by the effective lower bound. In this case, the stronger response of inflation in response to a sector 1 shock does not trigger a monetary tightening. Hence, real interest rates decline, crowding in private consumption (unreported).

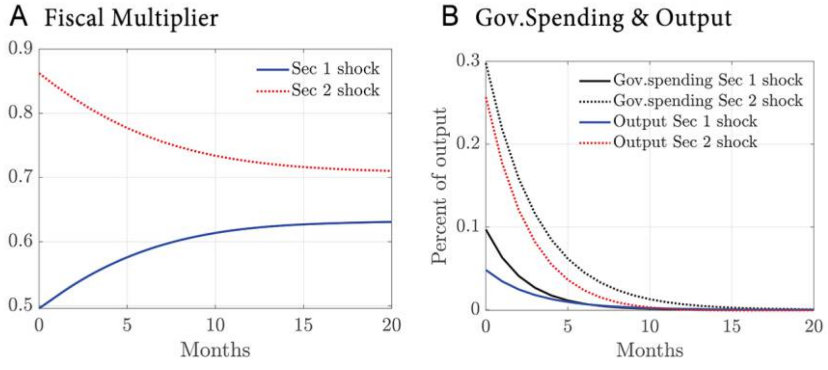


FIG. 6.—Net present value fiscal multipliers. *A* reports the net present value fiscal multiplier, computed following Uhlig (2010) as total net present value of output (deviations measured in percent) from month 0 to month *k*, divided by the net present value of government spending (deviations measured in percent of steady-state output) from month 0 to month *k*. *B* shows the underlying impulse responses. Shock is to government spending in either sector 1 or sector 2, scaled to 1% of steady-state output in each case.

is largest for the impact multiplier. We show the underlying responses of output (reproduced from fig. 5) and overall government spending, measured in percent of steady-state output, in figure 6*B*. And while these results pertain to federal purchases—which account for only 16% of total government spending—they generalize to total government spending to the extent that it varies across sectors in the same proportion.

## V. Some Time Series Evidence

We now turn to time series data in order to validate the model's prediction that sectoral heterogeneity is key for the aggregate effects of sectoral shocks to federal purchases. For this purpose, we rely on a VAR model, which, while simple, mimics our model-based analysis closely. In particular, we identify shocks to federal purchases at the sector level and contrast their effects on the aggregate economy. Specifically, we aggregate purchases in the same four sectors to which we calibrate sector 2 in section IV.B (sticky sector purchases) as well as the purchases in the remaining sectors, in which consumer prices are relatively more flexible (flexible sector purchases). We then include the log of both time series in real terms in a monthly VAR. It features 12 lags, a constant, month of year indicators, and a linear time trend and includes observations for the period 2001:1 to 2019:12.[24] Because federal purchases are highly exposed to oil

[24] We stop in 2019 to exclude the COVID period. We deflate the series with the PCE index because a deflator for government spending is not available at monthly frequency.

and other commodity prices, we include the log of the West Texas Intermediate oil price in the model to avoid a price puzzle. We also include personal consumption expenditures (PCE) inflation, the 2-year treasury yield as a measure for the monetary policy stance, and the S&P proxy for real GDP, in logs.[25]

In the spirit of Blanchard and Perotti (2002), we identify shocks to federal purchases recursively, with federal purchases in the two sectors ordered first and second. Hence, we rule out a response of federal purchases to the other variables in the VAR within the month. This restriction appears mild, given fact 1.[26]

Figure 7 shows the estimated impulse responses to purchase shocks in the flexible price (left) and sticky price (right) sectors. In each panel, the solid line represents the point estimate, and the shaded region indicates 90% confidence bands. The horizontal axis measures time in months, and the vertical axis measures the percent (or percentage point) deviation from the preshock level. We show in each row the responses of variables corresponding to those shown in figure 5. The response of purchases to each shock exhibits moderate persistence, and the time series of purchases in one sector responds only mildly to the shock in the other sector (see app. sec. B.9).

Turning to the responses of inflation, the interest rate, and output, we detect a pattern that qualitatively mimics the model simulations above. A shock to federal purchases in the flexible price sector raises inflation (the response peaking after around 12 months) and triggers a monetary contraction, that is, a rise in the interest rate. At the same time, output does not increase. A different pattern emerges in response to a shock in the sticky sector. Here, inflation and the interest rate do not rise, whereas output does increase, again peaking after around 12 months. Hence, the time series evidence confirms the predictions of the model shown in figure 5.

It also puts a fresh perspective on earlier work that has documented puzzling responses of prices and the interest rate to aggregate government spending shocks (Mountford and Uhlig 2009; Corsetti, Meier, and Müller 2012; Ramey 2016). The VAR estimates underscore the importance of accounting for the sectoral origin of government spending

[25] Unless noted otherwise, our data source is the Federal Reserve Economic Data database maintained at the Federal Reserve Bank of St. Louis. We measure inflation based on the PCE index, year on year.

[26] Because we use monthly data, the assumption that spending does not respond contemporaneously to the other variables included in the VAR is less restrictive than in the original work of Blanchard and Perotti (2002). The ordering of the two series for federal purchases relative to each other also matters in principle, but we find that it makes little difference in practice. In what follows, we show results for the case of ordering the sticky sector purchases first.

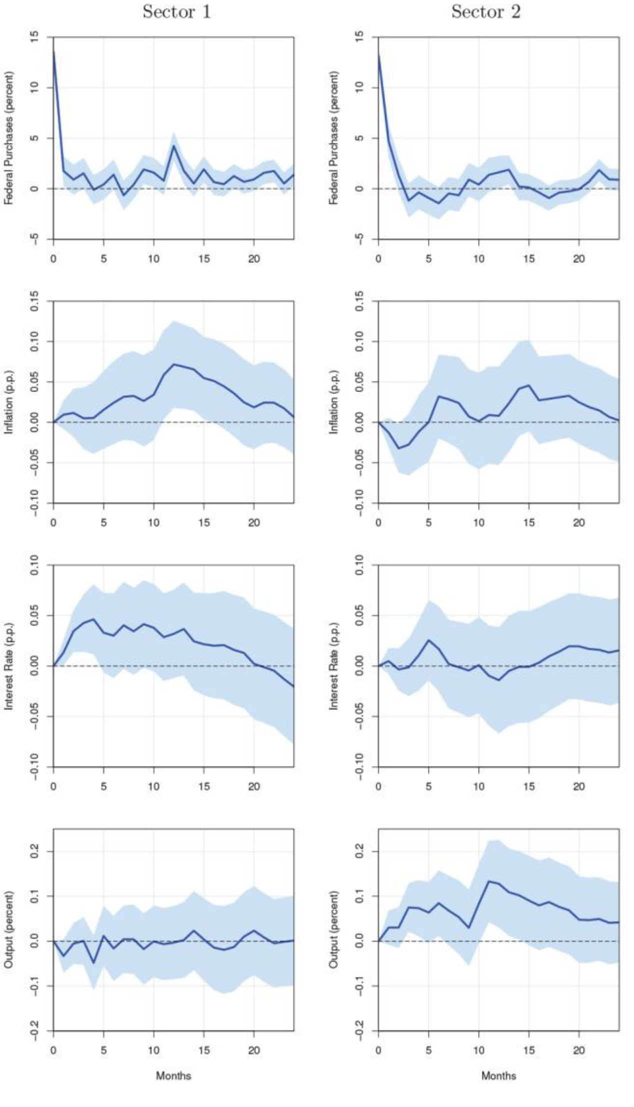


Fig. 7.—Impulse responses to federal purchases shocks originating in sectors 1 and 2. The figure shows impulse responses to 1 standard deviation shock to federal purchases in sector 1 (*left*) and sector 2 (*right*) based on a monthly VAR. Solid line represents the point estimate, and shaded areas indicate 90% confidence bands. p.p. = percentage points.

shocks: whether purchases increase in the sticky or relatively flexible sector can make all the difference in terms of their aggregate effects. While our focus is on the role of price stickiness, it is unlikely the only or—for that matter—the most import differentiator when it comes to the effects of federal purchases.[27]

## VI. Conclusion

In this paper, we take a business cycle perspective and provide an anatomy of the universe of US federal procurement spending since 2001. These federal purchases account for about one-sixth of overall government spending, which in turn accounts on average for 18.7% of GDP in our sample. Yet despite their small share in overall spending, federal purchases account for about one-half of its variation over time. We establish this and four more facts on the nature of government spending: $G$ is granular and heterogeneous—there is no big $G$, only many little $g$'s.

This granularity matters for policy: government spending is not a single policy instrument that can be easily adjusted to fine tune the business cycle, contrary to what conventional models and policy discussions assume. The existence of many little $g$'s, however, presents policy makers with an opportunity. It suggests that one may devise sector-specific fiscal stabilization policies, paving the way for future research in this area. And while these results pertain to federal purchases—which account for only a small share of total government spending—they generalize to total government spending to the extent that it varies across sectors in similar proportion.

## Data Availability

Code replicating the tables and figures in this article can be found in Cox et al. (2024) in the Harvard Dataverse, https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/8RCMZP.

## References

Acconcia, A., G. Corsetti, and S. Simonelli. 2014. "Mafia and Public Spending: Evidence on the Fiscal Multiplier from a Quasi-Experiment." *A.E.R.* 104 (7): 2185–209.

Acemoglu, D., V. M. Carvalho, A. Ozdaglar, and A. Tahbaz-Salehi. 2012. "The Network Origins of Aggregate Fluctuations." *Econometrica* 80 (5): 1977–2016.

Auclert, A., M. Rognlie, and L. Straub. 2023. "The Intertemporal Keynesian Cross." Working paper.

[27] We provide time series data for federal purchases aggregated to the sectoral level to facilitate further research (https://www.coxlydia.com/contracts_data.html).

Auerbach, A., Y. Gorodnichenko, and D. Murphy. 2020. “Local Fiscal Multipliers and Fiscal Spillovers in the USA.” *IMF Econ. Rev.* 68 (1): 195–229.

Bajari, P., S. Houghton, and S. Tadelis. 2014. “Bidding for Incomplete Contracts: An Empirical Analysis of Adaptation Costs.” *A.E.R.* 104 (4): 1288–319.

Bandiera, O., A. Prat, and T. Valletti. 2009. “Active and Passive Waste in Government Spending: Evidence from a Policy Experiment.” *A.E.R.* 99 (4): 1278–308.

Baqaee, D. R., and E. Farhi. 2020. “Productivity and Misallocation in General Equilibrium.” *Q.J.E.* 135 (1): 105–63.

Baxter, M., and R. G. King. 1993. “Fiscal Policy in General Equilibrium.” *A.E.R.* 83 (3): 315–34.

Bayer, C., B. Born, R. Luetticke, and G. J. Müller. 2023. “The Coronavirus Stimulus Package: How Large Is the Transfer Multiplier.” *Econ. J.* 133 (652): 1318–47.

Bigio, S., and J. La’o. 2020. “Distortions in Production Networks.” *Q.J.E.* 135 (4): 2187–253.

Blanchard, O., and R. Perotti. 2002. “An Empirical Characterization of the Dynamic Effects of Changes in Government Spending and Taxes on Output.” *Q.J.E.* 117 (4): 1329–68.

Boehm, C. E. 2020. “Government Consumption and Investment: Does the Composition of Purchases Affect the Multiplier?” *J. Monetary Econ.* 115:80–93.

Bosio, E., S. Djankov, E. Glaeser, and A. Shleifer. 2022. “Public Procurement in Law and Practice.” *A.E.R.* 112 (4): 1091–117.

Bouakez, H., O. Rachedi, and S. Emiliano. 2022. “The Sectoral Origins of the Spending Multiplier.” Working paper.

Bouakez, H., O. Rachedi, and E. Santoro. 2023. “The Government Spending Multiplier in a Multisector Economy.” *American Econ. J. Macroeconomics* 15 (1): 209–39.

Bureau of Labor Statistics. 2011. “PPI Methods Overview Page.” Chap. 14. https://www.bls.gov/ppi/methods-overview.htm.

Campos, N., E. Engel, R. D. Fischer, and A. Galetovic. 2021. “The Ways of Corruption in Infrastructure: Lessons from the Odebrecht Case.” *J. Econ. Perspectives* 35 (2): 171–90.

Chakravarty, S., and W. B. MacLeod. 2009. “Contracting in the Shadow of the Law.” *RAND J. Econ.* 40 (3): 533–57.

Chetty, R., A. Guren, D. Manoli, and A. Weber. 2011. “Are Micro and Macro Labor Supply Elasticities Consistent?” *A.E.R.* 101 (3): 471–75.

Chodorow-Reich, G. 2019. “Geographic Cross-Sectional Fiscal Spending Multipliers: What Have We Learned?” *American Econ. J. Econ. Policy* 11 (2): 1–34.

Christiano, L. J., and M. Eichenbaum. 1992. “Current Real-Business-Cycle Theories and Aggregate Labor-Market Fluctuations.” *A.E.R.* 82 (3): 430–50.

Christiano, L. J., M. Eichenbaum, and S. Rebelo. 2011. “When Is the Government Spending Multiplier Large?” *J.P.E.* 119 (1): 78–121.

Corsetti, G., A. Meier, and G. J. Müller. 2012. “Fiscal Stimulus with Spending Reversals.” *Rev. Econ. and Statis.* 94 (4): 878–95.

Coviello, D., A. Guglielmo, and G. Spagnolo. 2018. “The Effect of Discretion on Procurement Performance.” *Management Sci.* 64 (2): 715–38.

Cox, L., G. Mueller, E. Pasten, R. Schoenle, and M. Weber. 2024. “Replication Data for: ‘Big *G*.’” Harvard Dataverse, https://dataverse.harvard.edu/dataset.xhtml?persistentId=doi:10.7910/DVN/8RCMZP.

Decarolis, F., L. M. Giuffrida, E. Iossa, V. Mollisi, and G. Spagnolo. 2020. “Bureaucratic Competence and Procurement Outcomes.” *J. Law, Econ., and Org.* 36 (3): 537–97.

Demyanyk, Y., E. Loutskina, and D. Murphy. 2019. "Fiscal Stimulus and Consumer Debt." *Rev. Econ. and Statis.* 101 (4): 728–41.

Farhi, E., and I. Werning. 2016. "Fiscal Multipliers." In *Handbook of Macroeconomics,* vol. 2, edited by J. B. Taylor and H. Uhlig, 2417–92. Amsterdam: Elsevier.

Fisher, J. D., and R. Peters. 2010. "Using Stock Returns to Identify Government Spending Shocks." *Econ. J.* 120 (544): 414–36.

Flynn, J., C. Patterson, and J. Sturm. 2022. "Fiscal Policy in a Networked Economy." Working Paper no. 20339, NBER, Cambridge, MA.

Foerster, A. T., P.-D. G. Sarte, and M. W. Watson. 2011. "Sectoral versus Aggregate Shocks: A Structural Factor Analysis." *J.P.E.* 119 (1): 1–38.

Gabaix, X. 2011. "The Granular Origins of Aggregate Fluctuations." *Econometrica* 79 (3): 733–72.

———. 2020. "A Behavioral New Keynesian Model." *A.E.R.* 110 (8): 2271–327.

Gabaix, X., and R. S. J. Koijen. 2020. "Granular Instrumental Variables." Working Paper no. 28204, NBER, Cambridge, MA.

Gagnepain, P., M. Ivaldi, and D. Martimort. 2013. "The Cost of Contract Renegotiation: Evidence from the Local Public Sector." *A.E.R.* 103 (6): 2352–83.

Galí, J., J. D. López-Salido, and J. Vallés. 2007. "Understanding the Effects of Government Spending on Consumption." *J. European Econ. Assoc.* 5 (1): 227–70.

Galí, J., and T. Monacelli. 2008. "Optimal Monetary and Fiscal Policy in a Currency Union." *J. Internat. Econ.* 76 (1): 116–32.

Hebous, S., and T. Zimmermann. 2021. "Can Government Demand Stimulate Private Investment? Evidence from U.S. Federal Procurement." *J. Monetary Econ.* 118:178–94.

Hettig, T., and G. J. Müller. 2018. "Fiscal Policy Coordination in Currency Unions at the Effective Lower Bound." *J. Internat. Econ.* 115:80–98.

Kang, K., and R. A. Miller. 2022. "Winning by Default: Why Is There So Little Competition in Government Procurement?" *Rev. Econ. Studies* 89 (3): 1495–556.

La'O, J., and A. Tahbaz-Salehi. 2022. "Optimal Monetary Policy in Production Networks." *Econometrica* 90 (3): 1295–336.

Liebman, J. B., and N. Mahoney. 2017. "Do Expiring Budgets Lead to Wasteful Year-End Spending? Evidence from Federal Procurement." *A.E.R.* 107 (11): 3510–49.

Mountford, A., and H. Uhlig. 2009. "What Are the Effects of Fiscal Policy Shocks?" *J. Appl. Econometrics* 24 (6): 960–92.

Nakamura, E., and J. Steinsson. 2014. "Fiscal Stimulus in a Monetary Union: Evidence from US Regions." *A.E.R.* 104 (3): 753–92.

Nekarda, C. J., and V. A. Ramey. 2011. "Industry Evidence on the Effects of Government Spending." *American Econ. J. Macroeconomics* 3 (1): 36–59.

Oh, H., and R. Reis. 2012. "Targeted Transfers and the Fiscal Response to the Great Recession." *J. Monetary Econ.* 59 (suppl.): S50–S64.

Ozdagli, A., and M. Weber. 2017. "Monetary Policy through Production Networks: Evidence from the Stock Market." Working Paper no. 23424, NBER, Cambridge, MA.

Pasten, E., R. Schoenle, and M. Weber. 2020. "The Propagation of Monetary Policy Shocks in a Heterogeneous Production Economy." *J. Monetary Econ.* 116:1–22.

———. 2024. "Sectoral Heterogeneity in Nominal Price Rigidity and the Origin of Aggregate Fluctuations." *American Econ. J. Macroeconomics* 16 (2): 318–52.

Perotti, R. 2008. "In Search of the Transmission Mechanism of Fiscal Policy." In *NBER Macroeconomics Annual 2007,* vol. 22, edited by D. Acemoglu, K. Rogoff, and M. Woodford, 169–226. Cambridge, MA: NBER.

Ramey, V. A. 2016. "Macroeconomic Shocks and Their Propagation." Working Paper no. 21978, NBER, Cambridge, MA.

———. 2019. "Ten Years after the Financial Crisis: What Have We Learned from the Renaissance in Fiscal Research?" *J. Econ. Perspectives* 33 (2): 89–114.

Ramey, V. A., and M. D. Shapiro. 1998. "Costly Capital Reallocation and the Effects of Government Spending." *Carnegie-Rochester Conference Series Public Policy* 48:145–94.

Ramey, V. A., and S. Zubairy. 2018. "Government Spending Multipliers in Good Times and in Bad: Evidence from US Historical Data." *J.P.E.* 126 (2): 850–901.

Ravn, M. O., and H. Uhlig. 2002. "On Adjusting the Hodrick-Prescott Filter for the Frequency of Observations." *Rev. Econ. and Statis.* 84 (2): 371–75.

Ryan, N. 2020. "Contract Enforcement and Productive Efficiency: Evidence from the Bidding and Renegotiation of Power Contracts in India." *Econometrica* 88 (2): 383–424.

Uhlig, H. 2010. "Some Fiscal Calculus." *A.E.R.* 100 (2): 30–34.

Warren, P. L. 2014. "Contracting Officer Workload, Incomplete Contracting, and Contractual Terms." *RAND J. Econ.* 45 (2): 395–421.

Woodford, M. 2011. "Simple Analytics of the Government Expenditure Multiplier." *American Econ. J. Macroeconomics* 3 (1): 1–35.

———. 2022. "Effective Demand Failures and the Limits of Monetary Stabilization Policy." *A.E.R.* 112 (5): 1475–521.