# The Effects of "Buy American" Restrictions on Government Purchases

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**Abstract**

In this paper, we ask whether Buy American restrictions on government procurement lead to stronger macroeconomic effects of government spending. We propose a methodology based on a comprehensive understanding of Buy American regulations to identify government spending that is more- and less-constrained by domestic content restrictions. We validate our methodology in a novel firm-level dataset, and then apply it to federal spending at the state level. Augmenting the relative fiscal multiplier approach of Nakamura and Steinsson (2014), we show that government dollars that are not constrained by domestic content restrictions have a higher relative multiplier than constrained contract dollars. We provide suggestive evidence that constrained spending generates more crowding out of investment than unconstrained spending, as firms adjust production to satisfy the Buy American restrictions.

**Keywords:** Buy America, domestic content restrictions, government contracts, fiscal multiplier

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# 1 Introduction

The U.S. federal government is the largest single buyer of goods and services in the world, spending an average of $600 billion per year. For almost a century, the government has wielded this power by placing domestic content restrictions on its own spending through a policy called the "Buy American Act." Policymakers claim that by ensuring that "when the federal government spends taxpayer dollars, they are spent on American made goods by American workers and with American-made component parts," federal spending will bolster U.S. manufacturing, increase the resilience of U.S. supply chains, strengthen the industrial base, and ultimately create a "better," stronger economy. By providing a source of stable demand for domestic products, policymakers envision that firms may shift production to occur within U.S. borders. Despite having been in place for almost a century, there is little to no empirical evidence of the macroeconomic effects of such policies.

In this paper, we ask whether Buy American restrictions lead to stronger macroeconomic effects of government spending. Making progress on this question was complicated by the fact that Buy American restrictions are not directly observable in even the most detailed data on U.S. government spending. We propose a methodology based on a comprehensive understanding of Buy American regulations to identify government spending that is more- and less-constrained by these regulations. We validate our methodology in a novel firm-level dataset. We then apply our methodology to federal spending at the state level. Augmenting the relative fiscal multiplier approach of Nakamura and Steinsson (2014), we show that unconstrained contract dollars have a higher relative multiplier than constrained contract dollars. We provide suggestive evidence of a potential mechanism for these findings: constrained spending generates more crowding out of investment than unconstrained spending as firms adjust their production to satisfy the Buy American restrictions.

Our first contribution is one of measurement: We leverage the complexity of the Buy American legislation—loopholes, amendments, and exemptions—to identify government spending that is more and less subject to Buy American restrictions. We apply our methodology to the universe of federal defense procurement contracts between 1979 and 2000, and the entire universe procurement contracts between 2001 and 2019. These data provide detailed information on the contracting relationships between U.S. firms and the government, but provide almost no window into how much firms rely on imported inputs for their production and, therefore, how much they will be affected by Buy American restrictions. However, the general Buy American restrictions are not applied uniformly to all government contracts. For example, smaller contracts are exempt from Buy American restrictions altogether, while certain military purchases are regulated more stringently than non-military purchases. We

propose three ways for grouping contracts into "constrained" and "unconstrained" contracts, where each of the three approaches is based on a different aspect of the Buy American legislation. We take advantage of details of the legislation in order to ensure that the contracts in these groups are similar to each other, up to the extent to which they are subject to Buy American restrictions. Therefore, we argue that we have identified variation in the strength of domestic content restrictions that is plausibly exogenous to firm-level and macroeconomic variables, allowing us to identify the causal effects of the Buy American restrictions.

We validate our measures of constrained and unconstrained spending by pairing the procurement data with data on firm-level import shipments from S&P Panjiva. These shipments are recorded on bills of lading that firms file with U.S. Customs on their maritime imports. Thanks to the high-frequency nature of the bill-of-lading data, we can track the real-time import behavior of firms when they receive constrained or unconstrained government contracts. We show that a firm's import response after receiving a constrained contract is significantly smaller than its import response to an unconstrained contract *for the same product*. These results provide evidence that the "constrained vs. unconstrained" breakdowns of contracts do indeed cause different import behavior by firms. We take this as evidence that, despite being imperfect measures of Buy American restrictions, our breakdowns still identify meaningful differences in the strength with which these restrictions are applied.

Having shown that we can effectively measure variation in the Buy American policy in the government contracts data, we turn to our primary question: How do Buy American restrictions influence the macroeconomic effects of government purchases? To address this question, we follow a longstanding literature that measures the "fiscal multiplier"—the percentage increase in output that results when government spending is increased by one percent of GDP. In particular, we rely on the relative fiscal multiplier approach put forth by Nakamura and Steinsson (2014), and exploit variation in regional military procurement associated with changes in aggregate defense spending. We augment their baseline specification to distinguish between the effects of government dollars that are more constrained by Buy American restrictions, and the effect of government dollars that are less constrained. We find a relative multiplier on output of 1.0 for constrained spending, and 2.1 for unconstrained spending. This implies that an increase in unconstrained spending equal to one percent of GDP in one state relative to another has roughly double the effect on relative state output than a commensurate increase in unconstrained spending. Similarly, we find that constrained spending has roughly half the effect on state-level employment rates as unconstrained spending. We confirm that these characteristics are not driven by inherent differences in the industrial composition of constrained versus unconstrained contracts, nor by differences in inter-state spillovers that might challenge our identifying assumptions.

From a simple accounting perspective, our findings on the relative multiplier may appear surprising. Constrained dollars are forced to remain in the U.S., but have smaller effects on relative output than unconstrained dollars, which can "leak" out through imports. We provide evidence that constrained contracts generate more crowding out of private investment than unconstrained contracts, which is consistent with our multiplier results. First, the fact that Buy American restrictions cause firms to import less suggests that the Buy American restrictions appear to be binding. The fact that firm import behavior differs when it receives a constrained versus unconstrained contract for the same product suggests that firms may have separate production processes for regulated and unregulated demand. We provide anecdotal evidence to support this view. Devoting resources to satisfy domestic content constraints might dampen a firm's ability to invest. We test this hypothesis by merging the procurement contracts data with Compustat, and estimating the effect of constrained and unconstrained contracts on firm-level investment. We show that long-term investment responds positively when a firm receives an unconstrained contract, but is flat when a firm receives a constrained contract for the same product.

This paper contributes to several strands of literature. First, there has been theoretical analysis on domestic content restrictions in trade agreements, from the seminal paper of Grossman (1981) to the more recent work of Grossman et al. (2023), who study whether it is optimal for governments to subsidize particular sources of inputs to mitigate supply chain risk. Additionally, rules of origin requirements in trade agreements have been studied both theoretically and empirically. Conconi et al. (2018), for example, show that the rules of origin requirements in NAFTA led to a reduction in imports of intermediate goods from third countries. Head et al. (2023) point, as we do, to the potentially counterproductive nature of rules of origin requirements. Other recent work by Allcott et al. (2024) studies the effects Buy American restrictions in the electric vehicle industry. Though related in spirit, we ask a fundamentally different question in this paper: whether domestic content restrictions on government purchases affect the fiscal transmission mechanism.

The body of work studying domestic content restrictions on government procurement, specifically, is smaller, particularly for the United States. In a policy brief, Hufbauer and Schott (2009) argues that Buy American provisions in the American Recovery and Reinvestment Act would do more damage to American employment than it would help. Dixon et al. (2018) use a computable general equilibrium model to evaluate the employment effects of the Buy American Act, and find that BAA is ineffective in generating employment. In a recent working paper by Bombardini et al. (2024), the authors use a quantitative trade model to assess the costs of Buy American provisions, and also conclude that Buy American rules, and in particular the recent tightening of the Buy American constraints, will create few jobs at

a relatively high cost. Our approach differs from these papers: by identifying Buy American restrictions directly in the data, we are able to produce causal estimates of the effects of the Buy American Act without relying on the assumptions of a particular structural model. There is also work by, for example, Herz and Varela-Irimia (2020) and García-Santana and Santamaría (2021, 2024) who study the effects of home bias in government contracting in Europe.

On the fiscal policy side, several papers have used different subsets of the government contract data to study fiscal transmission—e.g., Fisher and Peters (2010), Dupor and Guerrero (2017), Demyanyk et al. (2019), Auerbach et al. (2020), Cox et al. (2024)—as well as different aspects of the federal procurement process and its effects on firms—e.g., Hebous and Zimmermann (2021), Bajari et al. (2014), Ryan (2020), Warren (2014), Kang and Miller (2022), Bandiera et al. (2009), Gagnepain et al. (2013), and di Giovanni et al. (2022). Relative to these papers, we study a different fiscal transmission mechanism—how domestic content restrictions affect fiscal transmission in the United States.

Methodologically, our paper builds on the relative fiscal multiplier approach of Nakamura and Steinsson (2014). Our findings suggest that the regulations applied to government spending play a large role in determining the effect of that spending on output. In terms of data, we have benefited greatly from the work of Flaaen et al. (2021) in understanding the strengths and weaknesses of the Panjiva data that we rely on in our analysis. To our knowledge, ours is the first paper attempting to isolate variation in the import content of procurement contracts, and then measure how regulations on that content affects firms and the macroeconomy.

In the rest of the paper, we proceed as follows: In Section 2, we provide background and institutional details on the Buy American Act, in Section 3, we describe our primary data sources, and how we use them to measure variation in Buy American Act stringency across contracts; in Section 4 we present exercises that validate our measures, which we then use in our estimates of the relative fiscal multiplier in Section 5. In Section 6 we discuss the results and provide suggestive evidence of a potential mechanism, before concluding in Section 7.

# 2 Background on the Buy American Act

The United States has had domestic content restrictions on federal procurement in place for almost a century. The flagship policy—The Buy American Act—was signed on President Herbert Hoover's last day in office in 1933. The policy, which has changed very little since its inception, has two basic requirements: when procuring materials, supplies, or manufactured end-use products, the government must purchase products that are manufactured:

1. "substantially all" from materials that are produced in the United States, and
2. are manufactured in the United States

Determining whether a product qualifies as being domestically produced in accordance with Buy American standards involves some nuanced interpretation. Unmanufactured products must be mined or produced in the United States to qualify.[1] Manufactured end-use products qualify as domestic if they are manufactured in the United States and the cost of all intermediate components that are mined, produced, or manufactured in the United States exceeds 65 percent. (This threshold was just recently increased by the Biden Administration from 55 percent, and is scheduled to tighten further, to 75 percent by 2029.)

On top of these general requirements, there exist both exceptions and additional regulations for some procurement spending. For example, Buy American Act requirements only apply to procurement contracts above a certain monetary threshold—the "micro-purchase threshold," which is currently set at $10,000. There are also exceptions if awarding the contract to a domestic manufacturer would come at an "unreasonable cost." To determine whether the cost is "unreasonable," when a domestic bid for a contract is not the lowest bid, the procuring agency must add a cost penalty to the foreign bid (inclusive of duties) before determining which offer is the lowest. The size of the penalty is currently between 20 and 50 percent, depending on the firm and contracting agency.[2] After the penalty is applied, if the foreign offer is still lower than the domestic offer, the agency may award the contract to the foreign bidder at the initially proposed price. On the flip-side, there are certain cases in which the BAA requirements are even more stringent. The Berry Amendment, which has existed since the 1940s, provides that certain goods procured by the Department of Defense (DOD) must be entirely produced in the United States (i.e., 100 percent of the cost of intermediate components must be produced or manufactured domestically). These are just a few examples of the many complicated details of the Buy American Act legislation.

Until recently, there had been very few changes to the Buy American Act since its inception. During the Great Recession, some Buy American requirements were built into the American Recovery and Reinvestment Act. The Trump Administration also used Executive Orders to reaffirm aspects of the Buy American Act that apply to iron and steel production. The largest change to the legislation in 70 years, however, occurred during the Biden Administration. In May 2022, the Biden Administration announced that the domestic content threshold would be gradually increased from its then rate of 55 percent to 75 percent by

[1]For products consisting wholly or predominantly of iron or steel, the cost of domestic iron or steel must be greater than 95 percent of the cost of all components to qualify. See 48 C.F.R. §25.003.

[2]Currently, there is a 20 percent price penalty when the lowest domestic offer is from a large business, a 30 percent penalty when the lowest domestic offer is from a small business, and a 50 percent penalty when the contracting agency is part of the Department of Defense (DOD).

2029. By ensuring that "when the federal government spends taxpayer dollars, they are spent on American made goods by American workers and with American component parts," the stated goals of the policy are to bolster U.S. manufacturing, increase the resilience of supply chains and create a stronger industrial base, and simply create a "better," stronger economy. As the Biden Administration explains on the newly-launched "Made in America" website, they expect that"strengthening Made in America policies will send clear market signals to give suppliers confidence that manufacturing in the U.S., with America's workers, will provide greater opportunities. Ensuring Made in America laws are implemented clearly and consistently across government will support domestic suppliers." The goal of this paper is to provide empirical evidence as to whether Buy American restriction, indeed, lead to the stronger macroeconomic effects of government spending that policymakers promote.

# 3 Measurement of Buy American Restrictions

Against the background of the Buy American Act laid out in the previous section, we now describe how we are able to identify government purchases that are more and less subject to Buy American restrictions since, as far as we know, there is no direct measurement of this aspect of federal spending.

## 3.1 Government Procurement Contracts

Our measures of government spending are based on federal government procurement contracts. When the federal government purchases goods and services, it goes through a multi-stage solicitation and bidding process, that ends when the contract is awarded to one of the bidding firms. In the contracts data, we see detailed information about every contract action that is taken—from government purchases of military equipment to office supplies to administrative services. From 1979–2000, we have access to contracts issued by the military with values over $25,000 ($10,000 before 1984), collected on forms DD-350 and available from the National Archives.[3] Starting in 2000, we have access to the universe of government spending contracts, which are available from USAspending.gov and are described in detail by Cox et al. (2024). When analyzing these data over a sample that covers both the pre- and post-2000 data, we subset the latter data to only military spending, to make it consistent with the former.[4]

[3]Available at `https://www.archives.gov/research/electronic-records/reference-report/federal-contracts`.

[4]Specifically, we follow Nakamura and Steinsson (2014) and restrict our sample to contracts awarded by the Department of the Army, Department of the Navy, Department of the Air Force, and the Defense

For each contract transaction, we observe a fairly comprehensive set of characteristics. Most relevant for our study, we see the date the transaction occurred (the `action_date`), the awarding agency (e.g., the Department of Defense or the Department of Homeland Security), the recipient firm, the dollar amount obligated from the government to the recipient firm, the six-digit NAICS code of the product or service being purchased (in later data), an additional product code known as the Federal Supply Classification (FSC), and the primary location in which the contract is to be fulfilled (at the zip code level). The granularity of the contracts data allows us to flexibly aggregate spending into government purchases of different types of goods or spending in different firms, industries, and locations. For example, Nakamura and Steinsson (2014) aggregate these data to the state-year level, and Boehm (2016) aggregates them to the industry-year level. In fact, for data before 1989 we use the procedure laid out by Boehm (2016) to infer SIC industries from the "product service" or "federal supply" codes that accompany each contract. We rely on data aggregated to either the firm $\times$ sector $\times$ month or quarter level (in Sections 4 and 6) or state $\times$ year level (in Section 5).

## 3.2 Direct Measurement of Buy American Restrictions

Assessing the extent to which the government consumes imported content and, in turn, is impacted by Buy American regulations, is challenging, even with the most disaggregated government procurement data. The government can purchase imported goods either directly, by contracting with a foreign supplier, or indirectly, by contracting with a U.S. manufacturer that uses imports as intermediates in production. The federal procurement data provide only limited information on these two channels. In the data, for each contract, we observe the country in which the procured good or service originated. If the government imports goods directly, it should be indicated by this variable. Table 1 shows that direct imports by the government from foreign countries are virtually non-existent: over the period from 2001-2021, 90 percent of contracted dollars are spent on goods or services produced in the United States, roughly 5 percent of contracted dollars are spent on goods or services produced in Iraq, Afghanistan, and Kuwait—spending on items used by deployed forces in those countries—and the remaining 5 percent is spread out over a number of countries. Ultimately, directly contracting with foreign companies does not appear to be a pervasive feature of government procurement.

The second way that the government can consume imported good is if it purchases products that are manufactured in the U.S. using imported intermediate inputs. In the contracts

Logistics Agency. These defense contracts represent roughly half of all contracts by count and around 60 percent by value.

Table 1: Federal Purchases: Country of Origin

| Country Code | Share of Contracts (%) |
|---|---|
| USA | 90.13 |
| IRQ | 1.42 |
| AFG | 1.36 |
| KWT | 0.92 |
| DEU | 0.74 |
| KOR | 0.58 |
| JPN | 0.58 |
| ARE | 0.41 |
| GBR | 0.41 |
| CAN | 0.34 |

NOTE. This table shows how all contracted dollars, from 2001-2021, are distributed across countries. We exclude contracts that are missing a country code.

data, there is a variable called `place_of_manufacture`, which provides some information to this effect. There are eleven possible designations for this variable, describing whether the good was manufactured inside or outside of the United States, and if outside, what exception to the BAA rules permitted that purchase to happen. Consistent with Table 1, Table 2 shows that around 90 percent of contracted dollars are for items manufactured in the United States. Roughly 5 percent are manufactured outside of the U.S. for use outside of the U.S. (e.g., spending on products for deployed forces in the Middle Eastern countries shown in Table 1), and the remaining 5 percent are manufactured outside the U.S. for various reasons. Of particular interest is the place of manufacture code "A," which represents items that are manufactured inside the United States, but are not counted as "domestic" under BAA because they contain more than 50 percent foreign content. According to the data, less than one percent of contracted dollars fall into this category.

## 3.3 Indirect Measurement of Buy American Constraints

Taken at face value, the data suggest that the direct import content of federal procurement is very low. On indirect imports, the binary nature of what is indicated in the data masks an understanding of how much government purchases actually rely on intermediate inputs. In this subsection, we describe an alternative approach that allows us to capture variation in "Buy American Act" restrictions in order to measure the effects of those restrictions on firms and the macroeconomy. Specifically, we exploit complexities in the law that generate quasi-exogenous variation in the stringency of BAA restrictions. Leveraging these details, we can

Table 2: Federal Purchases: Place of Manufacture

| Code | Place of Manufacture | Share of Contracts (%) |
|---|---|---|
| D | Mfg. in U.S. | 89.74 |
| E | Mfg Outside U.S. for use Outside U.S. | 4.53 |
| B | Mfg Outside U.S. | 3.39 |
| L | Mfg Outside U.S.—Qualifying Country (DOD) | 0.94 |
| A | Mfg in U.S., More than 50 perc. Foreign Content | 0.84 |
| J | Mfg Outside U.S.—Domestic Nonavailability | 0.26 |
| G | Mfg Outside U.S.—Trade Agreements | 0.15 |
| H | Mfg Outside U.S.—Commercial IT | 0.1 |
| F | Mfg Outside U.S.—Retail | 0.03 |
| I | Mfg Outside U.S.—Public Interest | 0.01 |
| K | Mfg Outside U.S.—Unreasonable Cost | 0.01 |

NOTE. This table shows the percent of contracts that are manufactured in the U.S., or manufactured outside of the U.S. for one of many designated reasons. We exclude contracts in which the place of manufacture is missing, and we exclude contracts for goods that are "not manufactured end products" (where BAA restrictions do not apply).

distinguish between contract dollars that are more and less constrained by domestic content requirements, allowing us to test the impact of these constraints on firms and macroeconomic variables. In particular, there are three exceptions and amendments to the Buy American Act that we leverage: the micro-purchase threshold, the Berry Amendment, and the WTO Government Procurement Agreement. We describe each in detail below.

**The Micro-Purchase Threshold** The Federal Acquisition Regulations describe a micro-purchase as "an acquisition of supplies or services, the aggregate amount of which does not exceed the micro-purchase threshold."[5] The micro-purchase thresholds for supplies and professional services are currently $10,000. Importantly, the Buy American Act only applies to purchases valued at more than the micro-purchase threshold. This means that purchases of, say, $10,100 must be produced with domestic content. Purchases of only $9,999, however, are free to be imported or to be produced in the United States using an unrestricted level of imported inputs.

**The Berry Amendment** The Berry Amendment is a statutory requirement that requires purchases of certain goods by the Department of Defense be made of 100 percent domestic content. The Amendment was first passed as part of the 1941 Fifth Supplemental Department of Defense Appropriations Act in order to protect the domestic industrial base in the

[5]See FAR 2.101.

time of war. The Amendment was included in subsequent defense appropriations acts until it was made permanent in 1994 and eventually codified into law in 2002.[6] The Berry Amendment applies to a certain set of products (classified by what are called Federal Supply Classification (FSC) codes), including textiles, clothing, footwear, food, hand or measuring tools, stainless steel flatware, and dinnerware. When these covered products are sold to the Department of Defense (or a Defense sub-agency), they must be made with 100 percent domestic content. When the same products are sold to a non-Defense agency, however, they must only satisfy the standard Buy American regulations.

**The WTO Government Procurement Agreement** The WTO's Agreement on Government Procurement (GPA) is a plurilateral agreement within the framework of the WTO. Currently 22 parties are official members of the Agreement, and several other countries participate either as observers or have initiated accession negotiations. The goal of the GPA is to mutually open government procurement markets among its members. Participating countries agree on certain procurement activities that are covered by the agreement, and for these activities, domestic content restrictions are not permitted. The United States' coverage schedule has both a product-based component and a threshold-based component. Only certain products are covered by the GPA (again, designated by FSC codes), and among covered products, only those above $250,000 are covered. Products that are not covered by the agreement (even if above the $250,000 threshold) include the products covered by the Berry Amendment (described above) and many types of transportation equipment, weapons, and specialty metal products. We collect the specific products and threshold values from coverage schedules that are published by the WTO.[7] For our purposes, when a contract is both a relevant product and is above the value threshold, contracting firms are permitted to use imports from any of the 22 member countries freely. When a contract is not covered by the Agreement, however, the usual Buy American restrictions apply.

**Constrained and Unconstrained Contracts** Using the three policy amendments and exceptions described above, we create three pairs of spending series, each with a "constrained" component and an "unconstrained" component. (We will refer to the "unconstrained" series, though in most cases these contracts are not completely unconstrained, just less constrained than the constrained series.) The definitions, described in Table 3, are based either on the product or service being purchased (designated by FSC code) or the contract value. For the Micro-Purchase Threshold, we define "constrained" contracts to be those just

[6] See 10 U.S.C. 2533a.

[7] These schedules can be found at `https://www.wto.org/english/tratop_e/gproc_e/gp_app_agree_e.htm`.

Table 3: Definitions of Constrained and Unconstrained Contracts

| Amendment or Exception | Constrained Definition | Unconstrained Definition |
|---|---|---|
| Micro-Purchase Threshold | Procurement contract value $\in [\text{MPT}_t, \text{MPT}_t + \$2500)$, where $\text{MPT}_t$ is the micro-purchase threshold in year $t$ | Procurement contract value $\in [\text{MPT}_t - 2500, \text{MPT}_t)$, where $\text{MPT}_t$ is the micro-purchase threshold in year $t$ |
| Berry Amendment | Products (FSC codes) covered by the Berry Amendment, purchased by the Department of Defense | Products (FSC codes) covered by the Berry Amendment, purchased by a non-Defense agency |
| WTO GPA | Products (FSC codes) *not* covered by the GPA OR contracts valued below $\text{GPA}_t$, where $\text{GPA}_t$ is the relevant value threshold in year $t$ | Products (FSC coes) covered by the GPA and contract valued above $\text{GPA}_t$, where $\text{GPA}_t$ is the relevant value threshold in year $t$ |

above the micro-purchase threshold (between the threshold and the threshold plus \$2500).[8] "Unconstrained" micro-purchase contracts are those valued just below the threshold, (between the threshold and the threshold minus \$2500). For the Berry Amendment, we define constrained contracts as contracts for products (FSC codes) covered by the Berry Amendment and purchased by the Department of Defense. Unconstrained contracts are defined as products that are covered by the Berry Amendment, but purchased by a Non-Defense agency, so the Berry Amendment does not apply. Lastly, for the WTO Government Procurement Agreement, we define unconstrained contracts as those covered by the GPA—this includes products (FSC codes) that are covered by the agreement when the contract value is above the relevant GPA threshold. Constrained GPA contracts include both products that are not covered by the GPA and products that are covered, but when contract values are less than the GPA threshold.

Our claim is that using either of these breakdowns, we are able to construct series of government spending in the data that vary primarily with respect to how stringent their Buy American restrictions are. In the next section, we provide evidence validating these measures, which we then use to test the macroeconomic effects of the Buy American Act.

# 4 Measure Validation Using Bill of Lading Data

In this section, we provide empirical evidence that the "constrained" and "unconstrained" spending series that we construct appear to measure spending that is more and less con-

[8]The micro-purchase threshold is time-varying, but is around \$10,000 for most of the sample.

strained by the Buy American Act. To do this, we merge data on firm-level import shipments with the procurement contract data. We show that firms appear to import less when they receive what we define as a "constrained" contract relative to when they receive an "unconstrained" contract *for the same product*.

## 4.1 Bills of Lading Data from S&P Panjiva

We combine the government contracts data with publicly-available data on firm-level imports, collected by the U.S. Customs and Border Patrol (CBP). While CBP does not host the data, other third parties process and make the bill of lading data available for a cost. We use data supplied by S&P Panjiva, and are sensitive to the effort of Flaaen et al. (2021) who describe the strengths and weaknesses of the data. On the strengths side, we take advantage of the fact that the data show shipments at the firm level, which we are able to match to the procurement data. Another strength of the data is that they are also available in high frequency—by way of analogy with financial markets, the data are available at a "tick" frequency. Here, a tick is a shipment, though we aggregate these data at the firm $\times$ month level.

One weakness of the Panjiva data is that they are an unreliable source of import *values*, though volumes are measured well. While Panjiva makes an effort to impute the products (with Harmonized System (HS) codes) contained in a shipment, we have concluded anecdotally (as Flaaen et al. suggest) that this imputation contains a fair amount of measurement error. In many cases, the HS codes are imputed using the text description of imported products. In one case, S&P classified a shipment by a company that produces *vacuum measuring instruments* as a *vacuum cleaner*. Table 5 in Appendix A shows imports by HS code associated with government contracts in different NAICS sectors. The first column, for example, shows which HS codes are imported when a firm receives a contract for "aircraft manufacturing." Clearly, while some of the products make sense, others (like "toys, including model planes") are examples of where the text-based imputation goes wrong. Therefore, we are unable to reliably impute a *dollar amount* of imports that are caused by a government contract, and largely avoid doing so in our analysis, relying on import volumes instead.

## 4.2 Measure Validation

We start by merging the universe of federal procurement contracts with the firm-level data on import shipments from Panjiva using S&P's "Business Entity Cross Reference Service Dataset." For each government contract transaction, we see information on the date the contract was agreed upon (or, the `action_date`), the recipient firm, the value of the contract,

the six-digit NAICS sector that the contract is for, and the Federal Supply Classification.[9] Using these characteristics for each firm × NAICS 6 industry × month, we aggregate the set of contracts that the firm receives that are constrained (according to one of our three definitions) and the set that are unconstrained. Then, because the bill-of-lading data is provided at a daily frequency, we can track the real-time import behavior of firms when they receive these constrained and unconstrained government contracts. We aggregate these to the firm-month level.

Using our merged dataset, we then estimate equation 1:

$$m_{f,t+h} - m_{f,t-1} = \delta_{f,i,h} + \delta_{t,h} + \beta_h^c G_{f,i,t}^c + \beta_h^u G_{f,i,t}^u + \varepsilon_{f,i,t,h}, \tag{1}$$

where $m_{f,t}$ is the log of import weight (in kg) (from Panjiva) by firm $f$ in month $t$. Our primary independent variables of interest are $G_{f,i,t}^C$—the log of deflated constrained contracts by firm $f$ in industry $i$, month $t$, and $G_{f,t,i}^u$—the log of deflated unconstrained contracts by firm $f$ in industry $i$, month $t$.[10] We include firm × industry and time fixed effects, and interact both contract series ($G_{f,i,t}^c$ and $G_{f,i,t}^u$) with an indicator that equals 1 if a firm's lifetime contracts are majority manufacturing and $i$ is a manufacturing industry.

Of particular importance is the fact that we are comparing constrained and unconstrained contracts to the *same firm* for a product in the *same industry*. Holding these characteristics fixed and making use of the careful definitions of constrained and unconstrained contracts presented above, our claim is that the differential response of imports to these contracts is plausibly exogenous with respect to other fundamentals of the contract. In other words, while the level of the estimated $\beta$ coefficients may potentially be biased if, for example, more productive firms import more and receive more contracts, our claim is that whether or not they receive a *constrained* contract in a month is independent of their productivity. This allows us to interpret the difference between $\beta^c$ and $\beta^u$ as the causal effect of the policy. We estimate this regression for all three definitions of "constrained" and "unconstrained" spending. Due to the limitations of the Panjiva data described earlier, we refrain from trying to interpret the magnitudes of the estimated coefficients of interest, $\beta_c$ and $\beta_u$. Instead, we view the estimated coefficients as reflecting an elasticity of firm import volume to a constrained or unconstrained contract dollar.

The results are plotted in Figure 1. In the left panel of the figure, we plot the individual elasticities of imports to constrained and unconstrained contracts, $\beta^c$ and $\beta^u$, respectively. In the right panel, we plot the difference between the two elasticities. In both figures, the shaded regions indicate 95 percent confidence intervals. The top panel of the figure shows the

[9] There are many other observables for each contract, but these are the relevant ones.

[10] We deflate these series by the U.S. producer price index.

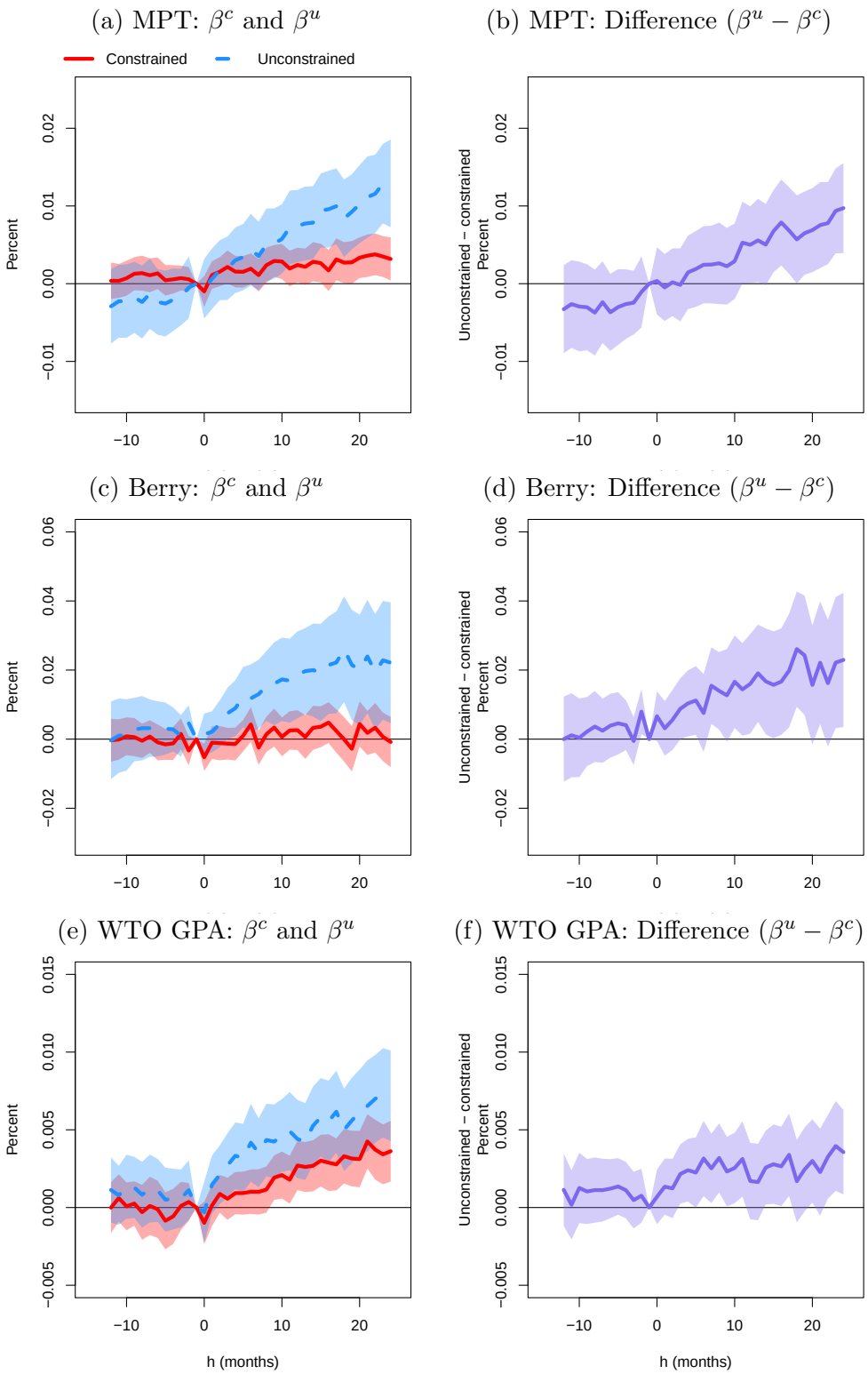
results when we use the micro-purchase threshold (MPT) to define constrained and unconstrained contracts. Upon receipt of a constrained contract for a particular product (NAICS 6 industry), we see a very limited response of imports in the Panjiva data—indicated by the relatively flat, solid red line. Receipt of an unconstrained contract by the same firm and for the same product, however, generates a positive and statistically significant import response in the Panjiva data over the subsequent two years. And most importantly, the panel on the right shows that the response of imports to constrained contracts is statistically significantly lower than the import response to unconstrained contracts. The middle and bottom panels show results from estimating the same specification, but using the Berry Amendment definition of constrained and unconstrained (middle panel) and WTO government procurement agreement definition (bottom panel).

The results are similar for all three formulations. Of particular note are the Berry Amendment results in the middle panel. Recall, the Berry amendment requires that when certain products are purchased by the Department of Defense, they are required to be made of 100 percent domestic content. When those same products are purchased by a non-Defense agency, only the regular Buy American requirements apply. In the regression, we are comparing the import response of a firm, $f$, when it receives a contract for a Berry product, $i$, from the Defense Department (constrained) to the response of the same firm when it receives a contract for the same Berry product from a non-Defense agency (unconstrained). As predicted, the constrained Berry contract elicits zero import response while we see a slight positive response for the unconstrained contract. In what follows, we explore whether these constraints have implications for the macroeconomic effects of government spending.

# 5 BAA and the Fiscal Multiplier

Using our measures of constrained and unconstrained government spending, we turn to our primary question: How do Buy American restrictions influence the macroeconomic effects of government purchases? To address this question, we follow a longstanding literature that measures the "fiscal multiplier," defined as the percentage increase in output that results when government spending is increased by one percent of GDP. In particular, we rely on the approach put forth by Nakamura and Steinsson (2014), who empirically estimate the "regional fiscal multiplier"—the effect of an increase in government spending on output in one region of the United States relative to another. Similar to Nakamura and Steinsson, we exploit variation in state military procurement associated with aggregate changes in aggregate defense spending. However, we augment their baseline specification to distinguish between the effects of government dollars that are constrained by Buy American restrictions

Figure 1: Elasticity of Imports to Constrained vs Unconstrained Contracts




NOTE. The left panels show estimates of $\beta_h^c$ and $\beta_h^u$ from equation 1. The right panels show estimates of the differences. The shaded regions represent 95% confidence intervals constructed using standard errors that are clustered by firm-industry and month.

and the effect of government dollars that are unconstrained, or less constrained.

## 5.1 Baseline Estimates

Our baseline empirical specification is a relative fiscal multiplier regression:

$$\frac{Y_{s,t} - Y_{s,t-2}}{Y_{s,t-2}} = \beta^c \left( \frac{G^c_{s,t} - G^c_{s,t-2}}{Y_{s,t-2}} \right) + \beta^u \left( \frac{G^u_{s,t} - G^u_{s,t-2}}{Y_{s,t-2}} \right) + \alpha_s + \gamma_t + \varepsilon_{s,t}. \tag{2}$$

The left-hand side is the two-year percent change in either real per-capita GDP or the employment-to-population ration in state $s$ in year $t$.[11] The right-hand side includes the two-year percent change in defense spending in state $s$ in year $t$, expressed as a fraction of lagged GDP. We split defense spending into two components: "constrained" spending, captured by $G^c_{s,t}$ and "unconstrained" spending, captured by $G^u_{s,t}$. We describe this breakdown below. State and time fixed effects absorb any state-specific trends in output and government spending, and aggregate shocks. We deflate all variables by the U.S. consumer price index, and put them in per-capita terms using state-level population data from the Census Bureau. Our sample period starts in 1981—two years after we have government contract data—and ends in 2019.

One concern with equation 2 is that, given the political nature of government spending, the federal government may award contracts to certain states in times of economic need. To address this concern, we pursue an instrumental variables approach similar to that of Nakamura and Steinsson, but extend the approach to account for the two types of government spending that we study. Specifically, we instrument for the changes in $G^c_{s,t}$ and $G^u_{s,t}$ with changes in aggregate constrained or unconstrained spending, each interacted with an indicator for state $s$. The identification of $\beta^c$ and $\beta^u$ thus comes from the fact that states have different sensitivities to changes in aggregate (constrained or unconstrained) defense spending. To interpret these estimates as causal, we assume that changes in aggregate defense spending are exogenous to state economic conditions. In other words, we assume that the government does not engage in military build ups in order to help particular economies, and that the allocation of spending between more- and less-constrained contracts during military build-ups reflects the needs of the military, and not the needs of particular states.

We split each dollar of defense spending based on how constrained that dollar is by domestic content restrictions. What we call "unconstrained" purchases, here, are those that were covered by the WTO's Government Procurement Agreement. "Constrained" purchases are any remaining defense procurement purchases. This breakdown allows us to effectively

[11] Our data on state-level GDP come from the Bureau of Economic Analysis. Employment data are from the Bureau of Labor Statistics' establishment survey.

Table 4: Fiscal Multipliers by Constrained and Unconstrained Spending

| | GDP | EPOP |
|---|---|---|
| $G_{s,t}$, constrained | 1.0 | 0.7 |
| | (0.5) | (0.4) |
| $G_{s,t}$, unconstrained | 2.1 | 1.5 |
| | (0.6) | (0.4) |
| Observations | 1984 | 1984 |
| p(uncon<con) | 0.03 | 0.01 |
| Sample | 1981–2019 | 1981–2019 |

NOTE. This table presents IV estimates of equation (2) for state real per-capita GDP and the state employment-to-population ratio. We test the null hypothesis that the coefficient on constrained is larger than the coefficient on unconstrained spending, and report the associated $p$ value in the row row labeled "p(uncon<con)." Standard errors clustered by state and year are in parentheses.

split aggregate contract spending in half. The other breakdowns that we described in Section 3—those governed by the Berry Amendment and Micro-purchase threshold—provide us series of contracts that represent only a small portion of government procurement—too small to have measurable macroeconomic effects.

Table 4 contains our baseline estimates of equation (2). The first row shows the effect of increasing government spending by one percent of GDP when that spending is relatively constrained by domestic content restrictions. The second row shows the effect when the spending is relatively unconstrained. We find a relative multiplier of 1.0 for constrained spending, and 2.1 for unconstrained spending. To put these estimates into perspective, the corresponding estimates of Nakamura and Steinsson for spending that is not disaggregated by Buy American Act constraints is 1.43. In Appendix B.1, we confirm that our data and sample period yields a similar multiplier of 1.9 when not disaggregated. The results for the employment-to-population ratio are similar: Constrained spending has roughly half the effect of unconstrained spending.

## 5.2 Threats to Identification

**Industry Composition.** Our interpretation of the results presented in Section 5.1 is that domestic content restrictions dampen the macroeconomic effects of government spending. That interpretation depends on whether domestic content restrictions are the main distinguishing feature between what we call "constrained" and "unconstrained" contracts. The construction of our baseline breakdown, based on the WTO Government Procurement Agreement (GPA), opens the possibility that the set of constrained contracts has a slightly different

industry composition than the set of unconstrained contracts. This is because contracts covered by the GPA—what we call unconstrained contracts—are contracts that (1) are exceed a certain threshold and (2) are for the purchase of a specific set of products, $\Omega$. This means that the set of unconstrained contracts excludes contracts for any goods that are not in $\Omega$, which may skew the industry composition of unconstrained contracts relative to constrained contracts. We present two exercises to help mitigate this concern.

First, we consider an alternative breakdown of contracts that holds the set of products fixed. The alternative series of constrained spending includes contracts for products in $\Omega$ with contract values that are between zero dollars and the threshold. The alternative series of unconstrained spending includes contracts for products in $\Omega$ with contract values that are between the threshold and double the threshold. Despite being a much smaller set of contracts, we detect macroeconomic effects that are comparable and consistent with our baseline estimates. Appendix B.2 describes this exercise in more detail.

Second, we make use of the fact that the WTO Government Procurement Agreement has changed over time. The agreement was originally established in 1979 as the "Tokyo Round Government Procurement Code," but was renegotiated and expanded as part of the Uruguay Round of the General Agreement on Tariffs and Trade, the predecessor of the WTO. The more modern version was signed in 1994. We perform a placebo test of sorts in which we classify contracts by the *modern* GPA definition of thresholds and covered products, in the pre-1994 data. The idea is that before 1994, those classifications did not directly demarcate more- and less-constrained contracts. We then estimate our baseline regression, but separately estimate coefficients for constrained and unconstrained spending pre- and post-1994. The results are shown in Appendix B.3. In the pre-1994 data, we cannot reject the null hypothesis that $\beta^c > \beta^u$. Indeed, we find the we can reject the opposite null. Post-1994, we strongly reject the null that $\beta_c > \beta_u$. This suggests that it is the policy change, not the industry composition of the spending series, driving our baseline results.

**Inter-state spillovers.** Inter-state spillovers presents a challenge to the identification of relative fiscal multipliers in general, and is potentially more of a concern in our setting. Consider the experiment that identifies relative fiscal multipliers: A national military build-up begins and, because of existing characteristics (like Connecticut's industry composition), more contract dollars (as a percent of GDP) flow to Connecticut than New York. We then compare how GDP changes in Connecticut relative to New York. If, however, Connecticut contractors use inputs from New York to fulfill their contracts, the relative increase in Connecticut's GDP will be understated. In our setting, we consider the effect of spending a *constrained* dollar in Connecticut. Because the contracting firm in Connecticut is required to

fulfill that contract using domestic inputs, it may be even more likely to source inputs from New York. This would lead to an even larger understatement of the regional fiscal multiplier for constrained contracts, which could explain the smaller multiplier that we estimate for constrained spending relative to unconstrained spending.

We have several reasons to believe that inter-state spillovers are not driving our primary results. First, there is evidence from previous work which suggests that inter-state spillovers from procurement contracts are limited. Nakamura and Steinsson (2014), for example, show that the value of shipments from defense oriented industries to the government within states moves one for one with military procurement in that state. In other words, a dollar sent from the government to Connecticut tends to be fulfilled in value by shipments from Connecticut back to the Government. Auerbach et al. (2020) show that fiscal multipliers dissipate quickly with distance: a contract given to a firm in one city has a high local multiplier, but a multiplier of 0 in locations that are more than 50 miles away.

Second, we provide further supporting evidence using our data that inter-state spillovers are unlikely to be driving our results. Specifically, we aggregate our data from the state-level to the Census-region level. A Census region is a group of neighboring states, which we construct following the definition of Nakamura and Steinsson (2014). Again, the concern is that inter-state spillovers may be leading us to overstate the difference between $\beta^c$ and $\beta^u$. Under the assumption that inter-region spillovers are less likely than inter-state spillovers, if inter-state spillovers were driving the difference, we should expect a smaller difference using regional data. In terms of the example, the contract dollar given to Connecticut but spent in New York would be part of the same Census region, so the role of interstate transfers should be diminished in the regional data. Instead, as we show in Appendix B.4, the difference between $\beta^c$ and $\beta^U$ actually grows when we aggregate to the regional level. Taken together, the evidence suggests inter-state spillovers are likely not driving our results.

**Buy-American at the national level.** The identification in our baseline IV estimation of Equation (2) depends on the assumption that the allocation of national spending between more- and less-constrained contracts during military build-ups reflects the needs of the military, and not the needs of particular states. An alternative approach with a weaker assumption is to instrument for changes in $G^c_{s,t}$ and $G^u_{s,t}$ with state-indicators interacted with national military procurement that is not split by the extent of domestic content restrictions. We present those estimates in Appendix B.5. Over our entire sample period, the results using this alternative instrument are nearly identical for output, though the standard errors are wider and we cannot reject the null that $\beta_u > \beta_c$. After the implementation of the GPA in 1994, we find statistically significant differences, consistent with our baseline.

**Many instruments.** Our IV estimation of Equation (2) includes two instruments for every state: we interact a state indicator with constrained spending at the aggregate level, and unconstrained spending at the aggregate level. The presence of many instruments can complicate inferences. We therefore present an alternative approach based on a Bartik instrument. For each state, the instrument for constrained spending is constructed by multiplying the change in aggregate spending by the average fraction of constrained spending to GDP in that state between 1979 (the start of our data) and 1993 (the year before the implementation of the GPA). While the standard errors on each of the point estimates are much larger, we continue to find strong statistical evidence that favors the hypothesis that constrained spending leads to a smaller relative multiplier than unconstrained spending.

# 6 Potential Mechansims

We show in the previous section that government dollars that are constrained by Buy American restrictions have a statistically significantly lower relative multiplier than unconstrained dollars. From a simple accounting perspective, this finding may seem surprising: constrained dollars are forced to remain in the United States, while unconstrained dollars can "leak" out through imports, potentially putting downward pressure on GDP. In this section we explore potential explanations for this finding.

## 6.1 Import responses to Buy American constraints

The findings from Section 4 help to shed light on what happens within a firm when it receives a constrained government contract. First and foremost, it appears that the Buy American policy has some bite: firms getting similar contracts for similar products change their behavior to comply with the regulations. In addition, the finding that firms show a different import response upon receipt of a constrained contract relative to an unconstrained contract for the same product suggests that firms may have separate production processes for regulated and unregulated demand. Indeed, it is easy to find anecdotal examples of this. Consider, for example, a U.S. footwear company called Capps Footwear (now a subsidiary of Original Footwear Company). Capps is a large recipient of government contracts because it produces dress shoes that are certified for use by all U.S. Armed Services. They also supply dress shoes for uniforms for agencies that are not part of the Department of Defense, like the Coast Guard (part of the Department of Homeland Security), and for non-federal entities like police and fire. Under the Berry Amendment, footwear that is purchased by the Department of Defense must be produced with 100 percent U.S. content. Footwear sold to

the Coast Guard must be Buy American compliant, but not necessarily Berry Amendment compliant. And, of course, shoes sold to non-federal entities need not adhere to either policy.

A glance at the Capps Shoes website reveals that the company produces three lines of women's shoes: the "Angel Series," the "Capital Series," and the "Colonel Series." All three lines are certified approval for Armed Services and approved by the Uniform Board for use by Police and Sheriff's Departments Nationwide. However, the website states that the Angel Series shoes (which cost $109 per pair) are "Made in the USA with imported parts." The Capital Series ($155.99 per pair) are "Made in the USA," and the Colonel Series ($195.99 per pair) are "Made in the USA and Berry Amendment Compliant." In other words, this company appears to have three distinct production processes to produce three pairs of similar leather uniform shoes, depending on the regulations it faces. To the extent that the creation of these separate processes detracts from other productive investments, Buy American restrictions may not be output maximizing on the part of the firm.

## 6.2 Crowding Out from Constrained Contracts

The long-standing debate about the effectiveness of fiscal stimulus centers around whether government spending has a positive multiplier, or whether the spending "crowds out" private spending enough that it can actually have dampening effects on economic growth. In a simple New Keynesian framework, government spending can potentially have this crowding out effect if increased demand from the government creates inflationary pressures that induce an increase in interest rates, which will in turn, make it harder for private entities to borrow money and invest.

We show that the data is consistent with a stronger "crowding out" effect of constrained dollars relative to unconstrained dollars, suggesting a plausible explanation for our baseline findings. To do this, we make use of a subset of the government procurement contracts that can be merged with Compustat data. We then use a similar specification to the one estimated in equation 1, to estimate the effects of recieving a constrained or unconstrained contract on firm financial variables of interest. Specifically, we estimate:

$$y_{f,t+h} - y_{f,t-1} = \delta_{f,i,h} + \delta_{t,h} + \beta_h^c G_{f,i,t}^c + \beta_h^u G_{f,i,t}^u + \Gamma_h x_{i,s,t} + \varepsilon_{f,i,t,h}, \tag{3}$$

where $G_{f,i,t}^c$ and $G_{f,i,t}^u$ are constrained and unconstrained contracts recieved by firm $f$ for a product in industry $i$ at time $t$. We use the three breakdowns of constrianed and unconstrained from before: the micro-purchase threshold, the Berry Amendment, and the WTO Government Procurement Agreement. In $x_{i,s,t}$, we include four lags of the $G$ variables to

absorb serial correlation. On the left-hand side we include measures from Compustat of firms' long-term investment.[12]

The results are shown in Figure 2. The left panel shows the estimates of $\beta^c$ (solid red line) and $\beta^u$ (blue dashed line). The right panel shows the difference between $\beta^c$ and $\beta^u$. The shaded regions in both the left- and right panels reflect 95 percent confidence intervals. From top to bottom, the panels show results estimated for the micro-purchase threshold definition of constrained vs unconstrained, the Berry Amendment breakdown, and the WTO Government Procurement Agreement breakdown, respectively. Regardless of the specification, a similar picture emerges: the response of long-term investment by a firm $f$ in month $t$ positively responds to receipt of an unconstrained contract, but exhibits zero response upon reciept of a constrained contract for the same product.

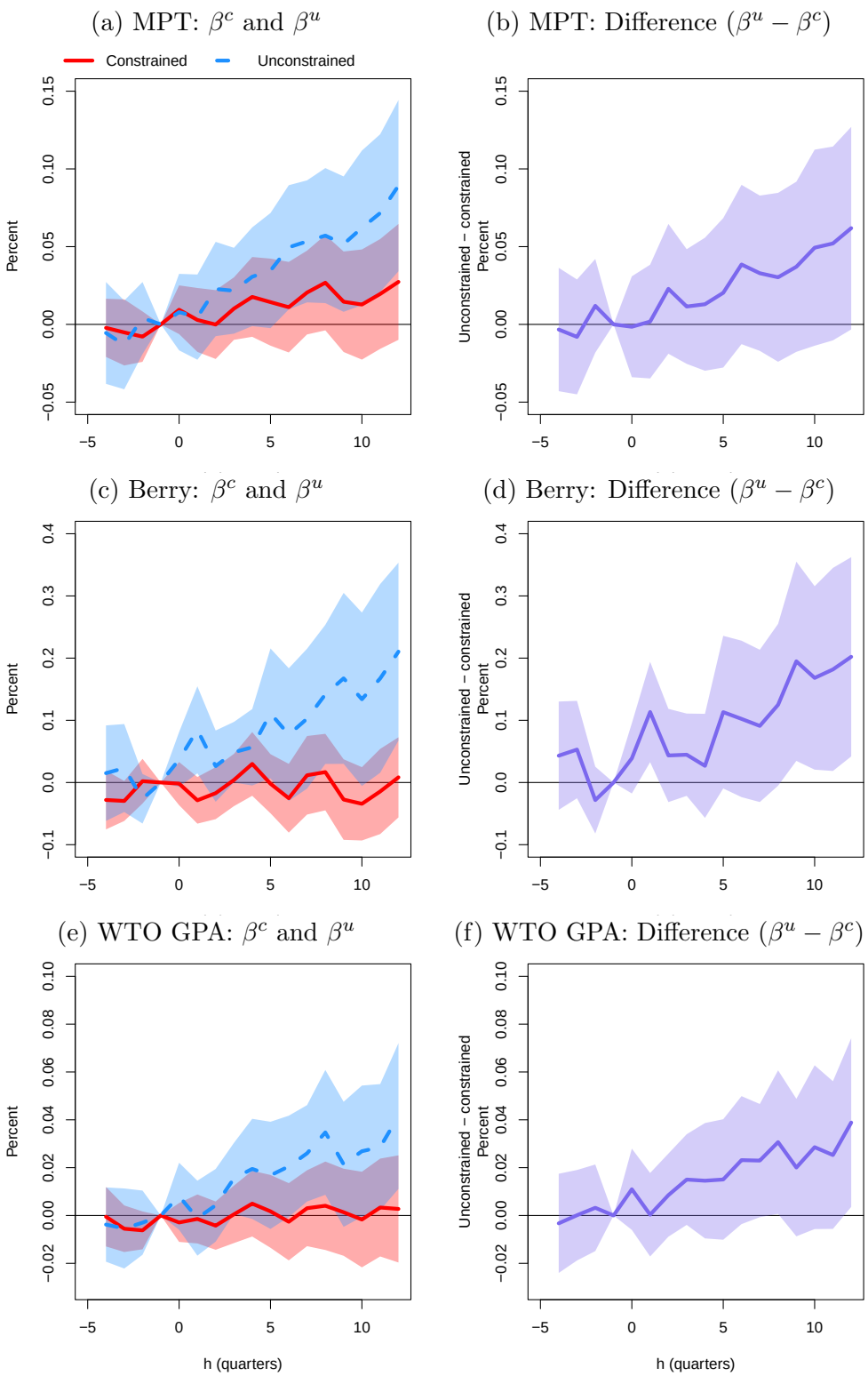
# 7 Conclusion

In this paper we study the macroeconomic effects of a longstanding policy that regulates the foreign content in government procurement: The Buy American Act. Our primary interest is in understanding whether Buy American Act restrictions lead to stronger macroeconomic effects of government spending, as policymakers hope. From an empirical standpoint, this is a challenge: even in the most detailed data on U.S. government spending, Buy American regulations are hard to observe directly. We propose a new way of measuring Buy American Act restrictions that leverages the complexity of the law itself. The various loopholes, amendments, and exemptions allow us to identify variation in the strength of domestic content restrictions across contracts that is plausibly exogenous to firm-level and macroeconomic variables, allowing us to identify causal effects.

To address our primary question, we follow the longstanding "fiscal multiplier" literature, specifically following the work of Nakamura and Steinsson (2014). Exploiting variation in regional military procurement associated with changes in aggregate defense spending, we are able to show that government dollars that are less constrained by Buy American restrictions have a relative multiplier that is double that of more constrained spending. From a simple accounting perspective, our findings may appear surprising. However, we provide suggestive evidence that constrained contracts have a stronger "crowding out" effect on private investment than unconstrained contracts, which is consistent with our findings. Empirical and anecdotal evidence suggests that Buy American restrictions induce U.S. firms to produce using a different production process than they use to fulfill unregulated demand. Devoting resources to satisfy these domestic content restrictions may dampen a firm's ability

[12]The Compustat variable name is `invltq`.

Figure 2: Long-Term Investment Response to Constrained vs Unconstrained Contracts




NOTE. The left panels show estimates of $\beta_h^c$ and $\beta_h^u$ from equation 6.2. The right panels show estimates of the differences. The shaded regions represent 95% confidence intervals constructed using standard errors that are clustered by firm and quarter.

to invest in other productive activities. Indeed, our data show that when firms receive a constrained government contract, they engage in significantly less long-term investment than when they receive an unconstrained contract. Further research is needed to better understand the underlying mechanisms and to study other impacts of Buy American restrictions, like whether they promote resilience to foreign shocks.

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# A Data and Measurement

Table 5: Top Imported Goods by Industry of Government Contract

| Aircraft manufacturing | Ship building/repair | Missiles and spacecraft | Navigation instruments | Aircraft parts |
|---|---|---|---|---|
| Toys, including model planes | Heat exchange units | Aircraft launching gear | X-ray tubes | Tapered roller bearings |
| Parts for electrical circuits | Parts of cooling equipment | Exercise equipment | Lamps | Cases/boxes/packages |
| Rubbers/plastic extruders | Cast articles of iron or steel | Bottles/flasks | Cases/boxes/packages | Parts for regulating instruments |
| Parts for gas filtering | Drive Axles | Other wooden furnutire | Drive Axles | Parts for gas filtering |
| Chemical wood pulp | Motor starting equipment | Specialty motor vehicles | Toys, including model planes | Ovens/stoves/etc. |

NOTE. This table shows the 6-digit Harmonized System products (rows) that respond most strongly to imports in the 6-digit NAICS manufacturing or trade industries that received the most government contracts since 2000. Within each industry, we find the most-responsive products by estimating $\log(g_{it}) = \beta_i \log(m_{it}) + \delta_t + \delta_i + \varepsilon_{it}$, where $g_{it}$ is the sum of contracts received by firms importing HS product $i$ in quarter $t$, and $\log(m_{it})$ is the sum of firm level inputs of good $i$ in quarter $t$ for the same firms. The HS products shown above have the highest $\beta_i$, conditional on having a $t$-statistic above 2.

# B Fiscal Multiplier: Robustness

This appendix contains supporting tables for Section 5.

## B.1 No disaggregation by type of spending

The sample period for our baseline estimates, 1979–2019, is shifted about 13 years later than the sample period studied by Nakamura and Steinsson. In Table 6, we estimate a specification analogous to that of Nakamura and Steinsson:

$$\frac{Y_{s,t} - Y_{s,t-2}}{Y_{s,t-2}} = \beta \left( \frac{G_{s,t} - G_{s,t-2}}{Y_{s,t-2}} \right) + \alpha_s + \gamma_t + \varepsilon_{s,t}, \tag{4}$$

where $G_{s,t}$ is total defense procurement in state $s$ in year $t$. The difference, relative to our baseline, is that we do not disaggregate government spending by how subject it is to Buy American restrictions. This serves as a benchmark for our disaggregated baseline estimates, and as a check that the two sample periods yield similar results. The point estimates for output and employment are similar to the corresponding estimates of Nakamura and Steinsson, who estimate 1.4 and 1.3, respectively. The estimates lie in between the estimates for constrained and unconstrained contracts presented in Table 4.

To address the concern that the allocation of national military spending into constrained and unconstrained contracts may be a response to state-specific factors, we consider alternative instruments in Table 7. Specifically, we instrument for $G^c_{i,t}$ and $G^u_{i,t}$ using the interaction of the change in national spending and state indicators. Relative to our baseline, we do not split national spending by whether it is more- or less-constrained. Panel (a) presents results for the full 1981–2019 sample. For output, the point estimates are very similar to our baseline estimates. The estimates for output and employment, however, are much less precisely estimated than in the baseline, and we cannot reject the null hypothesis that constrained contracts have a larger multiplier. Because the constraints imposed by the GPA began in 1994, Panel (b) presents estimates split by pre- and post-1994 time periods. Before 1994, constrained and unconstrained spending continues to have statistically indistinguishable effects from one another. In contrast, after 1994 we can strongly reject the null hypothesis that constrained spending has a larger multiplier than unconstrained spending.

Table 6: Regional multiplier for total government spending

| | GDP | EPOP |
|---|---|---|
| $G_{s,t}$, total | 1.9 | 1.4 |
| | (0.6) | (0.4) |
| Observations | 1984 | 1984 |
| Sample | 1981–2019 | 1981–2019 |

NOTE. This table presents IV estimates of equation (4). We instrument for the change in state-level spending, relative to GDP, using the interaction of the analogous change national spending and state indicators. Standard errors clustered by state and year are reported in parentheses.

Table 7: Alternative instrument: national total government spending

(a) Full sample period

| | GDP | EPOP |
|---|---|---|
| $G_{s,t}$, constrained | 1.2 | 1.5 |
| | (1.2) | (1.2) |
| $G_{s,t}$, unconstrained | 2.3 | 1.3 |
| | (1.1) | (0.7) |
| Observations | 1984 | 1984 |
| p(uncon<con) | 0.30 | 0.45 |
| Sample | 1981–2019 | 1981–2019 |

(b) Pre- and post-1994

| | GDP | EPOP |
|---|---|---|
| $G^c_{s,t} \times \mathbf{1}\{y < 1994\}$ | 4.0 | 2.5 |
| | (1.4) | (1.2) |
| $G^u_{s,t} \times \mathbf{1}\{y < 1994\}$ | 1.7 | 1.5 |
| | (0.8) | (0.7) |
| $G^c_{s,t} \times \mathbf{1}\{y \geq 1994\}$ | -1.7 | -0.4 |
| | (1.2) | (0.4) |
| $G^u_{s,t} \times \mathbf{1}\{y \geq 1994\}$ | 3.7 | 1.6 |
| | (1.6) | (0.6) |
| Observations | 1984 | 1984 |
| p(uncon<con), pre-1994 | 0.10 | 0.26 |
| p(uncon>con), post-1994 | 0.01 | 0.00 |
| Sample | 1981–2019 | 1981–2019 |

NOTE. This table presents IV estimates of equation (2). We instrument for the change in state-level spending, relative to GDP, using the interaction of the analogous change national spending, state indicators, and, in panel (b), a post-1994 indicator. We test the null hypothesis that the coefficient on constrained spending is larger than the coefficient on unconstrained spending in the full sample in panel (a) and in the post-1994 period in panel (b), and report the associated $p$ value in the row row labeled "p(uncon<con)." We test the null hypothesis that the coefficient on constrained spending is smaller than the coefficient on unconstrained spending in the pre-1994 period, and report the associated $p$ value in the row row labeled "p(uncon<con)." Standard errors clustered by state and year are reported in parentheses.

## B.2 Industry Composition

Our baseline breakdown of procurement contracts is based on the GPA. There, we say that unconstrained contracts are those covered by the GPA: These are contracts for a certain set of products, $\Omega$, that are above a certain value threshold, $\Theta$. Constrained contracts is the remainder of defense contracts. There is therefore a set of products—those not in $\Omega$—that are only in the constrained contracts. This raises the concern that we may only be identifying the effects of spending on different industries.

We have created another breakdown of contracts that does not depend on which product is being purchased. We instead rely only on the threshold, $\Theta$, to determine whether a contract is constrained or unconstrained. Constrained contracts are those with dollar amounts between zero and $\Theta$. Unconstrained contracts are those with dollar amounts between $\Theta$ and $2\Theta$. The second condition ensures that unconstrained contracts are roughly similar in size to constrained contracts, i.e., it does not include the relatively fat right tail of procurement contracts highlighted by Cox et al. (2024).

One complication that arises in evaluating the effects of this relatively small set of contracts is that the observed magnitudes of year-to-year changes in their dollar amounts, relative to GDP, are very small. The inner ninetieth percentile of two-year changes as a percent of GDP—the right-hand-side of equation (2)—is $\pm 0.06$ percent for constrained contracts (those below $\Theta$) and $[-0.03, 0.04]$ percent for unconstrained contracts (those from $\Theta$ to $2\Theta$). There are about ten times as small as our baseline breakdown, where the corresponding ranges are $[-0.8, 0.9]$ and $[-0.7, 0.8]$ and percent for constrained and unconstrained contracts, respectively. Interpreting the effect of a one-percent increase in spending in this smaller set of contracts is therefore extrapolating far outside of what is observed in the data. To address this, we scale up each component by its share of total state-level spending. The right-hand side of our regression therefore replaces $\frac{G^i_{s,t}-G^i_{s,t-2}}{Y_{s,t-2}}$ with $\frac{G^i_{s,t}-G^i_{s,t-2}}{Y_{s,t-2}} \times \frac{G_{s,t-2}}{G^i_{s,t-2}}$ for $i \in \{c, u\}$, where $G_{s,t-2}$ is total military procurement spending in state $s$ in year $t-2$. Note that for our baseline breakdown, these two objects are identical, since there constrained and unconstrained spending sum to total spending (so the scaling term is unity). In this alternative breakdown, on average, the two components sum to 15% of total spending.

We present the results in Table 8. We continue to find statistically and economically larger multipliers for unconstrained spending. Thus, it does not appear that the potential difference industry composition in our baseline breakdown of contracts is driving our results.

Table 8: Threshold-only breakdown

| | GDP | EPOP |
|---|---|---|
| $G_{s,t}$, constrained | -1.6 | -0.1 |
| | (0.7) | (0.3) |
| $G_{s,t}$, unconstrained | 2.1 | 0.7 |
| | (0.6) | (0.3) |
| Observations | 1984 | 1984 |
| p(uncon<con) | 0.00 | 0.05 |
| Sample | 1981–2019 | 1981–2019 |

NOTE. This table presents IV estimates of equation (2) using our alternative definition of constrained and unconstrained contracts based solely on the GPA threshold, and not on the GPA product list. Standard errors clustered by state and year are reported in parentheses.

Table 9: Pre- and post-1994 estimates

| | GDP | EPOP |
|---|---|---|
| $G^c_{s,t} \times \mathbf{1}\{y < 1994\}$ | 2.4 | 1.4 |
| | (1.0) | (0.8) |
| $G^u_{s,t} \times \mathbf{1}\{y < 1994\}$ | 1.5 | 1.1 |
| | (0.6) | (0.5) |
| $G^c_{s,t} \times \mathbf{1}\{y \geq 1994\}$ | -0.2 | 0.1 |
| | (0.6) | (0.2) |
| $G^u_{s,t} \times \mathbf{1}\{y \geq 1994\}$ | 2.0 | 1.2 |
| | (0.9) | (0.4) |
| Observations | 1984 | 1984 |
| p(uncon<con), pre-1994 | 0.23 | 0.36 |
| p(uncon>con), post-1994 | 0.00 | 0.00 |
| Sample | 1981–2019 | 1981–2019 |

NOTE. This table presents IV estimates of equation (2) for different time periods. We interact the constrained and unconstrained state-level spending, and the corresponding national aggregates, by a post-1994 indicator. We test the null hypothesis that the coefficient on constrained spending is larger than the coefficient on unconstrained spending in the post-1994 period, and report the associated $p$ value in the row row labeled "p(uncon<con)." We test the null hypothesis that the coefficient on constrained spending is smaller than the coefficient on unconstrained spending in the pre-1994 period, and report the associated $p$ value in the row row labeled "p(uncon<con)." Standard errors clustered by state and year are reported in parentheses.

## B.3 WTO GPA Placebo

The WTO Government Procurement Agreement definitions that we use to demarcate constrained and unconstrained contracts came into effect in 1994. In Table 9, we present our baseline specification split down by time period. We interact the constrained and unconstrained state-level spending, and the corresponding national aggregates, by a post-1994 indicator. Because the GPA breakdown was not a legislated determinant of the extent of Buy American restrictions before 1994, we can use that period as a placebo test for our treatment variables.

The top two rows of Table 9 show that the effects on both output and employment are similar between constrained and unconstrained contracts before the GPA took effect. Indeed, we cannot reject the null hypothesis that two effects are equal before 1994. After 1994, we see that the effects of constrained spending are economically and statistically indistinguishable from zero. Instead, we see large effects for unconstrained contracts. Therefore, it appears that it is the constraints imposed by the policy, and not some other inherent feature of the contract breakdown, that is driving the result that unconstrained contracts have larger economic effects than constrained contracts.

## B.4 Regional Estimates

In Table 10, we present estimates of a version of our baseline specification in which we replace state-level data with region-level data. Under the assumption that region-to-region spillovers are less likely than state-to-state spillovers, this serves as a test of whether the difference in constrained vs. unconstrained multipliers that we identify arises simply from the fact that constrained spending might simply require more inter-U.S. transfers of goods. We find, instead, that the difference in regional multipliers is larger than the difference in state-level multipliers. This suggests that it is within-state differences in how procurement contracts are fulfilled that drives the difference in their differential economic effects.

## B.5 Bartik Instrument

In Table 11, we present the results of estimating our baseline specification using Bartik instruments. Given that the GPA did not come into effect until 1994, in panel (a) we construct the instrument by multiplying national constrained spending with the average ratio of state-level constrained spending to GDP between 1979 and 1993. We construct the instrument for unconstrained spending analogously. In panel (b), we construct the instrument similarly, but using and end-date for the construction of the instrument in 1983 (instead of 1993),

which we then interact with a post-1994 indicator. We study both definitions of the Bartik instruments to ensure that the sample period used for its construction is not driving the results. In both cases, we find results that are consistent with our baseline: larger, and positive, effects of unconstrained spending. The effects are driven by the period during which the GPA was in effect.

Table 10: Regional estimates

| | GDP | EPOP |
|---|---|---|
| $G_{s,t}$, constrained | 1.5 | 1.6 |
| | (0.8) | (0.8) |
| $G_{s,t}$, unconstrained | 3.9 | 2.6 |
| | (1.4) | (1.5) |
| Observations | 390 | 390 |
| p(uncon<con) | 0.07 | 0.19 |
| Sample | 1981–2019 | 1981–2019 |

NOTE. This table presents IV estimates of equation (2) using data aggregated to the regional level, rather than at the state-level. Our definition of regions follows that of Nakamura and Steinsson (2014): It is nearly identical to the regions defined by the Census, except that we split the South Atlantic region into two. One includes Delaware, Maryland, Washington DC, Virginia, and West Virginia. The other includes North Carolina, South Carolina, Georgia, and Florida. Standard errors clustered by region and year are reported in parentheses.

Table 11: Bartik Instrument

(a) Full sample period

| | GDP | EPOP |
|---|---|---|
| $G_{s,t}$, constrained | -3.0 | -1.4 |
| | (2.1) | (0.7) |
| $G_{s,t}$, unconstrained | 3.3 | 2.3 |
| | (1.5) | (1.0) |
| Observations | 1326 | 1326 |
| p(uncon<con) | 0.01 | 0.01 |
| Sample | 1994–2019 | 1994–2019 |

(b) Pre- and post-1994

| | GDP | EPOP |
|---|---|---|
| $G^c_{s,t} \times \mathbf{1}\{y < 1994\}$ | 5.0 | 3.0 |
| | (1.9) | (1.3) |
| $G^u_{s,t} \times \mathbf{1}\{y < 1994\}$ | 2.4 | 3.3 |
| | (1.5) | (1.3) |
| $G^c_{s,t} \times \mathbf{1}\{y \geq 1994\}$ | -3.0 | -1.3 |
| | (2.1) | (0.7) |
| $G^u_{s,t} \times \mathbf{1}\{y \geq 1994\}$ | 3.0 | 2.4 |
| | (1.5) | (0.9) |
| Observations | 1984 | 1984 |
| p(uncon<con), pre-1994 | 0.19 | 0.44 |
| p(uncon>con), post-1994 | 0.01 | 0.00 |
| Sample | 1981–2019 | 1981–2019 |

NOTE. This table presents IV estimates of equation (2) using an alternative set of instruments than in our baseline. In panel (a), we instrument for state-level constrained spending growth using the product of national-level spending growth and the average fraction of constrained state-level spending to GDP from 1979–1993. The unconstrained instrument is analogous. In panel (b), the sample used to construct the instruments is 1979–1983. There, we interact the instrument, and the instrumented variables, with a post-1994 indicator. See the note to Table 7 for a description of the $p$-values reported towards the bottom of each panel. Standard errors clustered by state and year are reported in parentheses.