Government Contracting, Labor Intensity, and the Local Effects of Fiscal Consolidation: Evidence from the Budget Control Act of 2011*

Timothy M. Komarek†, Kyle Butts‡, and Gary A. Wagner§

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Abstract

The U.S. federal government awards billions of dollars of contracts annually to private-sector firms to produce a wide range of goods and services. However, little is known about how a reduction in federal procurement, also referred to as fiscal consolidation, impacts local labor markets. In this paper, we leverage the institutional details of the Budget Control Act of 2011 (BCA) and highly detailed transaction-level data for procurement by all federal agencies to estimate the effect of fiscal consolidation on local employment and wages. Our identification strategy uses a shift-share instrument and is based on the exogeneity of the BCA-induced spending cuts across industries, i.e. exogenous shocks. Our results show that the local effects of consolidation depend on the factor intensity of the sectors receiving federal dollars. We find that a $1 million reduction in federal contract spending reduces employment by more than 12 jobs in high labor-intensive industries (factor intensity of over 45% of production) and only around seven jobs in low labor-intensive industries (factor intensity of less than 15%). We also find that, relative to wages, employment appears to be the key margin for local labor market adjustment in the wake of consolidation. These findings suggest that nominal wage rigidity is an important mechanism in negative demand shocks.

Keywords: Federal Contracting, Urban Development, Local Labor Market, Job Displacement, Wage Rigidity
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†Associate Professor of Economics, Old Dominion University, Email: tkomarek@odu.edu.
‡PhD Candidate, University of Colorado: Boulder, Email: kyle.butts@colorado.edu.
§Acadiana Business Economist Endowed Chair and Professor of Economics, University of Louisiana at Lafayette, Email: gary.wagner@louisiana.edu.

1 Introduction

The U.S. federal government annually awards approximately $500 billion in procurement contracts to private-sector firms all over the nation. These awards cover a tremendous diversity of goods and services, ranging from basic landscaping to advanced weapon systems. The primary objective of government procurement is to acquire the necessary products and services for the federal government to operate effectively. However, there is often a second objective – spending to enhance economic opportunities for targeted locations and groups of people.[1] The literature exploring the impact of procurement spending, and government spending more generally, on labor market outcomes has focused on how increases in stimulus spending can spur economic development.[2] However, reliance on government contracts can also harm local economies when government spending declines. In this paper, we address this lesser studied alternative to fiscal stimulus: What are the local labor market impacts resulting from fiscal consolidation, i.e. periods of declining government spending?

A typical explanation for why the labor market impact from declining government spending differs from the impact of growing outlays points to the inflexibility of wages during a negative economic shock, a phenomenon known as downward nominal wage rigidity (Holden and Wulfsberg 2008; Elsby 2009; Agell and Bennmarker 2007). Wages, like prices in general, allocate labor and adjust to facilitate the market response to economic shocks. The literature offers several potential mechanisms for wage rigidity during a negative demand shock. Notably, firms may worry that employees would react strongly to wage cuts, resulting in lower morale and productivity (Yellen 1984; Kaur 2019; Blinder and Choi 1990; Bewley 1999), while the presence of institutions that protect jobs, such as labor unions, could yield menu costs for wage-setting and increase the costs of separation (Cacciatore et al. 2021).[3] These factors could lead firms to adjust employment and lay off workers instead of adjusting nominal wages. In comparison, positive demand shocks may induce firms to increase wages or offer workers more hours at overtime pay. However, recent literature

[1] The Small Business Administration, for instance, has programs to help veterans, women, and historically disadvantaged individuals and firms secure federal procurement awards. For more information see: https://www.sba.gov/document/support–small-business-procurement-scorecard-overview.

[2] See Ramey (2019) for an overview of the macro literature and Chodorow-Reich (2019) for the substate regional literature.

[3] Fallick et al. (2016) highlight other mechanisms that could induce wage rigidity, including contracting issues between workers and firms, efficiency wages, and government regulations, among other behavioral factors.

using microdata has challenged this conventional wisdom, arguing that downward wage rigidity may be less binding than has been traditionally thought (Elsby et al. 2016; Bowlus et al. 2002; Shin and Solon 2007).

By combining highly detailed federal procurement contract data with the reduction in federal procurement in the wake of the Budget Control Act of 2011 (BCA), this paper creates a testing ground to measure how local wages and employment adjust to a negative labor demand shock. Our identification strategy is based on a Bartik-style shift-share instrument and uses panel data from metropolitan core-based statistical areas (CBSAs), our measure of labor markets, for fiscal years (FY) 2009 to 2015.[4] The instrument combines BCA-induced national industry-level shocks in federal procurement spending at the 3-digit NAICS level with differential exposure to these shocks at the local level. We argue that the BCA-induced spending cuts created exogenous shocks across industries, which Borusyak et al. (2022) show is theoretically sufficient for a valid shift-share instrument. The BCA led to an across-the-board reduction in discretionary spending (known as the sequester) in FY 2013, and to federal spending caps in subsequent years such that actual spending fell below pre-BCA baseline projections. Since federal agencies have different missions and budget priorities, it is plausible that they independently differentiate which procurement spending is "necessary" and which spending can be cut. If this is indeed the case, the aggregate federal procurement shock consists of multiple independent industry-level shocks.

Acknowledging that instrument validity is not directly testable, we provide evidence for its exogeneity in two ways. First, to assess the possibility that the government targeted procurement cuts to certain CBSAs based on their economic well-being, we conduct a test akin to the "pre-trends" test in a difference-in-differences model. In doing so, we find that 2009-2010 changes in (per-capita) employment and wages are uncorrelated with the average shift-share shocks. Insofar as pre-trends predict trends in 2011-2015, this suggests that exposure to budget cuts was not systematically correlated with pre-BCA economic trajectories. Second, to alleviate concerns that a CBSA may have avoided spending cuts owing to its "political power," we use four different proxy variables for political power and find no correlation between them and spending shocks. Overall, these tests and the institutional details of the BCA give us confidence in the validity of

[4]Shift-share instruments have become common in empirical applications, and also called Bartik Instruments after the seminal work in Bartik (1991).

our instrument.

This paper contributes to the literature in several ways. First, we build upon the burgeoning "local" fiscal multiplier literature by studying how reduction in federal procurement due to the BCA impacts local labor markets. This differs from the previous literature on local fiscal multipliers, which has largely focused on stimulus spending (Chodorow-Reich 2019). Since our results isolate the labor market effects from fiscal consolidation, it is instructive to compare them with estimates from fiscal expansion. We focus this comparison on the primary results from Gerritse and Rodríguez-Pose (2018) and Auerbach et al. (2020) who study the same type of spending, federal procurement, in periods of federal spending growth. Our results suggest that a decline in total procurement spending of around $95,000 results in one job loss in a CBSA. In contrast, Gerritse and Rodríguez-Pose (2018) and Auerbach et al. (2020) find it takes an increase of $250,000 and $120,000 in spending, respectively, to create one job. Turning to wages, we show that a $1 decline in spending reduces aggregate wages by about $0.19. Auerbach et al. (2020) find that wages increase by $0.32 for every $1 increase in procurement spending, almost twice as much as our estimate.

Furthermore, our results shed light on the role of an important potential mechanism impacting the labor market during fiscal consolidation: nominal wage rigidity. As Kaur (2019) notes, previous work on nominal wage rigidity has focused on changes in the distribution of wages over the business cycle. For example, studies show evidence of wage inflexibility by measuring year-over-year changes in wages bunching at zero (i.e. no change in nominal wages) during a negative shock. However, Kaur (2019) goes on to explain that these studies provide little evidence of the subsequent impact on employment.

We also examine aggregate levels of wage flexibility and employment adjustment during fiscal consolidation by comparing spending based on the labor share in production for each industry. In particular, we use estimates of industry-level factor intensity created by Jorgenson et al. (2019) to categorize industries by the share of value added that comes from labor. We re-estimate our primary specification using industries with different degrees of labor intensity. Our estimates show that, as the labor share of the industry increases, firms react more strongly on the employment margin. For industries with a labor share between 0 and 15%, a decline in $1 million in spending destroys around 7.5 jobs. In contrast, a reduction of $1 million in spending in industries on the upper end of the distribution, a labor share over 45%, destroys more than 12 jobs. On the other hand, we show that the effect on wages remains relatively modest and constant across labor shares. These results bolster the view that in the face of a negative labor demand shock, wages remain relatively rigid and employment adjusts.

More generally, our paper contributes to the fiscal multiplier literature by highlighting the heterogeneous impacts of different kinds of federal spending on labor market outcomes. Previous work studying the impacts of federal procurement spending concentrated on total spending levels (e.g. Gerritse and Rodríguez-Pose 2018, Auerbach et al. 2020, Nakamura and Steinsson 2014), implicitly treating all procured goods and services as homogeneous. However, recent work by Cox et al. (2020) highlights the heterogeneity in federal procurement spending and the limitations of models that do not account for these explicit differences. In this light, our results show that employment and wage multipliers depend on differences in the type of spending based on relative factor intensities of the production function.

2 Background

We focus our analysis on the reduction of federal discretionary spending due to the expenditure caps imposed by the Budget Control Act of 2011 (BCA). The BCA was proposed, and later signed into law, because of concerns over growing federal deficits and the debt limit (Saturno et al. 2016).[5] The federal debt ceiling had been raised by a total of $4.5 billion between 2008 and 2010. However, another "crisis" quickly ensued as the debt level was projected to reach the (new) ceiling in early-to-mid 2011.[6] After some negotiation, an amended BCA was passed by both houses of Congress and signed into law by President Obama in August 2011.

The BCA increased the debt ceiling by $900 billion in exchange for $917 billion in cuts over 10 years and a plan for further deficit reduction. The deficit reduction plan placed tight caps on (planned) discretionary federal spending for each fiscal year from FY 2013 to FY 2021. The Congressional Budget Office (CBO) projected that the caps would reduce the federal deficit by roughly $1.5 trillion (including interest savings) over the same time period.[7] Figure 1 illustrates the projected path of discretionary federal spending with and without the BCA. Excluding interest, the

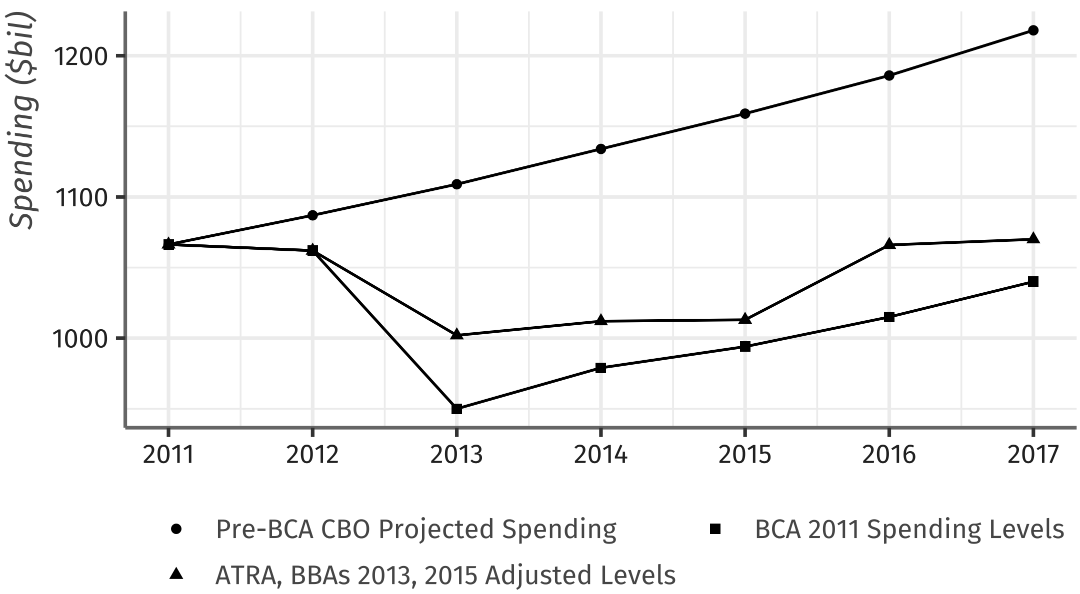
[5] The initial legislation, S. 365 (112th Congress), was introduced by Senator Tom Harkin (D-IA) on February 16, 2011.

[6] Secretary of the U.S. Treasury Timothy Geithner, letter to Majority Leader Harry Reid, dated January 6, 2011.

[7] See CBO report "Estimated Impact of Automatic Budget Enforcement Procedures Specified in the Budget Control Act" from September 12, 2011.

$1.5 trillion in savings estimate was the difference between the pre-BCA CBO projected spending and the BCA 2011 spending levels shown in the figure.

Figure 1: Aggregate Discretionary Federal Spending: FY 2011-2017



Note: BCA, ATRA, and BBA denote the Budget Control Act of 2011, American Taxpayer Relief Act of 2012, and the Bipartisan Budget Acts of 2013/2015. The pre-BCA baseline is from Table 1, Adjusted March 2011 Baseline, Congressional Budget Office (CBO) letter to Hon. John Boehner and Hon. Harry Reid, August 1, 2011. Other estimates are from Congressional Research Service Report 44874, The Budget Control Act: Frequently Asked Questions, 2019, Table 1, page 11.

The BCA, written as an amendment to the Balanced Budget and Emergency Deficit Control Act of 1985 (the Gramm-Rudman-Hollings Act), had several mechanisms to incentivize bipartisan cooperation to achieve deficit reduction. First, half of the $1.5 trillion in spending cuts would come from defense programs, typically favored by Republicans, and the other half from non-defense programs, more typically supported by Democrats. Second, if discretionary spending levels in any fiscal year exceeded the BCA-approved caps, then an automatic across-the-board reduction in spending (otherwise known as sequestration) would be triggered to enforce the caps. If a sequestration occured, the Office of Management and Budget (OMB) would be responsible for calculating the percentage and dollar amount of reductions required in each non-exempt budget account to comply with the legislation.[8] Within OMB's calculations however, individual agencies had discretion over

[8]The basic rules in the Budget Control Act of 2011 pertaining to a sequester's across-the-board reductions were established in Sections 255 and 256 of Balanced Budget and Emergency Deficit Control Act of 1985 (Driessen and

how to achieve the needed reductions within a given program (Saturno et al. 2016). In other words, if OMB determined that a program such as 024-58-5543 International Registered Traveler must be reduced by (say) 4% to comply with the cap, the Customs and Border Protection agency had discretion regarding how to make those reductions.

Since pre-BCA discretionary expenditures were projected to be greater than the BCA-approved caps (see Figure 1), the first possible sequestration was scheduled to occur on January 2, 2013, if superseding legislation had not been passed to reduce spending below the cap. There was widespread agreement among pundits and policymakers that the across-the-board nature of a sequester could harm U.S. interests. For example, it would prohibit Congress and federal agencies from reallocating funds based on spending priorities or protecting certain programs. Steve Ellis of the Taxpayers for Common Sense said of sequestration in a 2013 interview with PolitiFact: "Part of the whole reason (lawmakers) thought that the sequester would work was it was so stupid and awful." The BCA did provide a potential path to avoid a sequester by creating the Joint Select Committee on Deficit Reduction, known as the "Super Committee." This committee was charged with developing an alternative deficit-reduction plan by January 12, 2012.

The Super Committee failed to reach an agreement by its deadline. Because the federal government was operating under continuing resolution budget authority that exceeded the BCA caps, the first sequester in U.S. history was triggered in FY 2013 when the American Taxpayer Relief Act of 2012 (the "fiscal cliff deal") failed to establish an alternative deficit-reduction plan. The fiscal cliff deal delayed the start of sequestration from January 2, 2013, to March 1, 2013, and it reduced the total size of the budgeted cuts in FY 2013 from $109 to $85 billion split equally between defense and non-defense agencies.

On March 1, 2013, the OMB provided Congress with a 70-page report documenting specific agency-by-program reductions needed in FY 2013 to comply with the (BCA and fiscal cliff deal) caps.[9] Within FY 2013, the sequester reduced total federal spending by just over 2%, with 5% coming from reductions in discretionary non-defense spending and almost 8% coming from reductions in defense spending (Spar 2013). The percentage differences in OMB's calculations for defense

Labonte 2015). Jeffrey Zients, deputy director of the Office of Management and Budget, described sequestration as a "blunt and indiscriminate instrument" because program-level reductions were established by the authorizing legislation and individual agencies had no discretion over those cuts.

[9] Office of Management and Budget, letter to the Speaker of the House John Boehner, dated March 1, 2013.

and non-defense agencies arise because of exemptions in the BCA that largely followed guidelines established in the 1985 Gramm-Rudman-Hollings Act (Driessen and Labonte 2015). For instance, Social Security and Medicaid were exempt from the spending caps. The BCA also limited the reductions in Medicare reimbursements to 2% and exempted military personnel pay, ultimately resulting in important differences in terms of how defense- and non-defense agencies were affected.

Although the threat of additional sequesters remained, Congress never authorized budget authority for spending exceeding the caps. The discretionary caps were also raised on multiple occasions with the passage of the Bipartisan Budget Acts of both 2013 and 2015. Figure 1 shows how the American Taxpayer Relief Act of 2012 (ATRA) and the Bipartisan Budget Acts of 2013 and 2015 modified the original BCA spending limits.

The institutional details of the BCA provide several notable features for our identification strategy, outlined in Section 4. First, the across-the-board sequester in FY 2013 resulted in an unexpected, exogenous reduction in discretionary spending from already appropriated funds. While non-exempt programs across defense- and non-defense agencies experienced similar percentage reductions, agencies had discretion on what (goods and services) and where (locations) to cut based on operational goals. At a national level, these independent/unrelated agency-by-industry-by-location independent adjustments add up to as good as a random shock.

Second, the spending caps constrained the normal appropriations process in subsequent fiscal years. Agency-level spending was significantly below what would have been anticipated based on the CBO's pre-BCA baseline projections (see Figure 1). Federal agencies have different missions, priorities, and needs. It is plausible, perhaps even likely, that agencies may prioritize their purchases of private sector goods and services differently because of those goals. In other words, it is unlikely that procurement shocks will systematically target a given industry and location because each federal agency is unique. However, because Congress has discretion to adjust spending priorities within the allowable caps, we rule out political manipulation in Section 4 by explicitly exploring the link between a CBSA's political clout and the distribution of sequester reductions.

3 Data and Descriptive Statistics

The federal procurement process starts with legislative appropriations and moves to agencies in the executive branch that manage procurement through procedures specified by the Federal Acquisition Regulation (FAR). The FAR requires agencies to promote transparency and competition among firms as well as to provide "the best value to the government." Toward this end, agencies must announce unclassified procurement of over $25,000 and clearly define both the performance requirement and the bid evaluation criteria.

To analyze the impact of federal spending on local labor market outcomes, we exploit individual procurement contract data drawn from USAspending.gov.[10] The USAspending.gov program began as part of the Federal Funding Transparency Act of 2006 and provides information on individual transactions for most federal contracts, grants, loans, and other financial assistance. Data are updated monthly and federal prime contract data are pulled directly from the Federal Procurement Data System (FPDS), which is the real-time, single source for U.S. government procurement data.[11]

The data reported on USAspending.gov captures all transactions for prime recipient contracts of more than $3,000, and grant, loan, and other financial assistance of more than $25,000. The transactions include initial contracts along with modifications. Modifications to a contract can take place for a variety of reasons, among them a supplemental agreement for work within the scope of the original contract, the exercise of an option, or the termination of the contract.

The majority of contracts, around 85%, are never modified, and a modification requires the approval of both the vendor and government contracting agent. Contracting agents are encouraged to utilize performance-based contracts to protect the government's interests, meaning that vendors only receive a payment when a deliverable has been met. Federal agencies may authorize advance payments, but they are considered "extraordinary contractual actions" and tend to be concentrated in contracts awarded to defense firms.[12] In general, contract recipients have limited ability to delay or accelerate payments without the explicit approval of their contracting officer.

According to a report from a senior procurement executive, coverage in the Federal Procurement Data System, the underlying source for USAspending.gov, averaged 97.7% of all procurement awards over the period 2009 - 2014. This broad, in-depth coverage provides us with confidence that

[10]See https://www.usaspending.gov/Pages/Default.aspx for more information.

[11]Data on USAspending.gov are available as far back as FY 2000. However, when we compared aggregate federal procurement contracts, loans, and grants from USAspending.gov to their counterparts in the (now discontinued) Consolidated Federal Funds Reports, we found large discrepancies in the annual figures prior to FY 2008. For a more detailed description of the data in USAspending.gov, the Federal Procurement Data System, and the Consolidated Federal Funds Reports see Congressional Research Service (2019).

[12]See parts 18 and 43 of the Federal Acquisition Regulation for more information on advance payments (Section 18) and contract modifications (Section 43).

our dataset accurately reflects the full scope of procurement transactions.[13]

The data encompass every federal agency, covering purchases ranging from services like landscaping and information technology to products such as clothing, eating utensils, and helicopters. The data fields are extensive, including the starting and ending dates of the contract, the dollar value (obligated funds), the zip code for the place of performance and for the address of the firm headquarters, and the federal agency funding the award, among others. Each transaction also has unique identifiers that show whether the transaction is a new contract or a modification to an existing contract.[14] Furthermore, it also includes the industry classification (NAICS code) to describe the type of good or service being purchased by the government. A single contract may include multiple products or services. Nevertheless, like the geographic identifiers, the NAICS codes are based on the predominant good or service purchased.

We group all contract obligations and any modifications together to create a proxy spending path for each contract using the contract's starting date, ending date, and total obligations. Like Auerbach et al. (2020), we construct the contract spending path by allocating the obligation amount equally over the relevant time frame. For example, a $75,000 annual contract is assumed to result in $6,250 worth of spending in each of 12 months.

In our main federal spending measures, we aggregate the spending series over several dimensions. First, we aggregate the data to align with the federal fiscal year so that procurement spending is connected with the budgetary process.[15] Second, to aggregate the spending to labor markets, we use the *place of performance zip code*, which is the principal location where the majority (at least 51%) of the actual work is expected to be performed or where the goods and services are expected to be purchased.[16] We use metropolitan core-based statistical areas (CBSAs) as the labor market geography of interest.

[13]The Office of Management and Budget issues regular reports on the quality of federal government procurement data. See https://www.fsd.gov/gsafsd_sp?id=kb_article_viewsysparm_article=KB0048871 for more information [accessed Jan 8, 2022].

[14]Most contracts can be uniquely identified by the field *prime_award_piid*. Contracts under the Indefinite Delivery Vehicles program can be uniquely identified using the *prime_award_piid* and *prime_award_idvpiid* fields. The field *modnumber* identifies whether the transaction is an initial or new contract (by a value of 0) or a modification to an existing contract. For more information, see the USAspending.gov data dictionary. Our data is based on version 1.5.

[15]The U.S. Federal government fiscal year runs from October 1 to September 30. The fiscal year is denoted by when it ends, thus FY 2017 starts on October 1, 2016, and ends September 30, 2017.

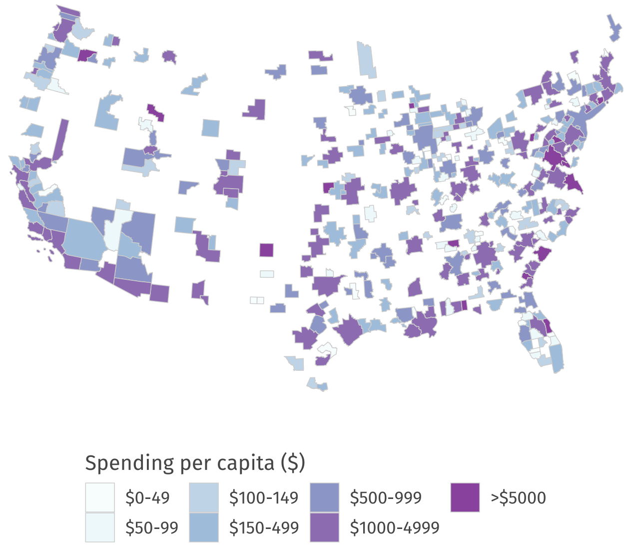
[16]In defining the place of performance, the Federal Procurement Data System states that "the information in this field should reflect where the items will be produced, manufactured, mined, or grown or where the service will be performed. This field refers to the contractor's final manufacturing assembly point, processing plant, construction site, place where a service is performed, location of mines, or where the product is grown."

We combine our procurement spending measures with labor market data from the Bureau of Labor Statistics (BLS) and the Census Bureau. The Bureau of Labor Statistics' Quarterly Census of Employment and Wages (QCEW) data provides county-level quarterly measures of both total employment and wages. We aggregate the labor market outcomes to the corresponding CBSA and fiscal year using 2015 CBSA definitions. The QCEW contains comprehensive employment and payroll data for U.S. establishments. We also use the Census Bureau's measure for local population.

There was a considerable amount of geographic heterogeneity in federal procurement spending per capita across CBSAs over our sample period (Figure 2). Using data for FY 2010, which predates the Budget Control Act, the 20 metropolitan CBSAs with the lowest per capita spending each received less than $50 per person, while the 20 CBSAs receiving the highest per capita procurement spending each received more than $4,400.

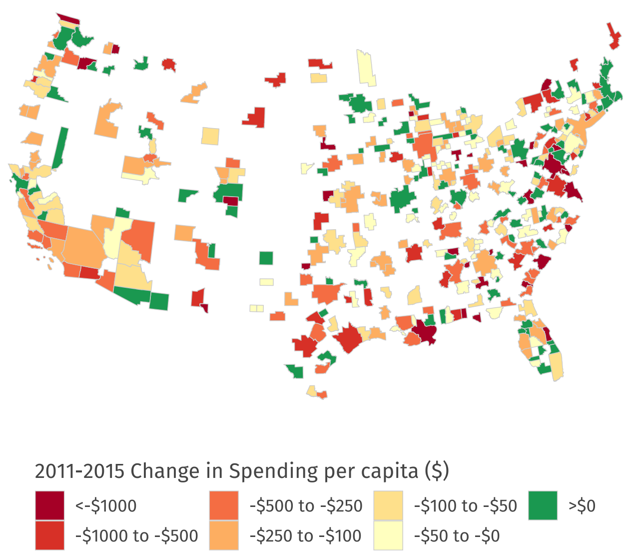
Figure 3 shows that the change in federal spending due to the BCA was also uneven across space. Two hundred and ninety CBSAs experienced a decrease in per capita spending averaging $475. The Oshkosh-Neenah, WI, CBSA experienced the largest decline, with per capita procurement spending falling from $25,481 in FY2010 to $9,810 in FY2015. This was largely due to the loss of contracts to the Oshkosh Corporation, a firm specializing in manufacturing military vehicles. In contrast, the remaining 92 CBSAs experienced increases in procurement spending that averaged $309 per capita. The CBSAs that experienced reductions in real per capita procurement spending account for more than 86% of metropolitan CBSA residents nationwide.

Figure 2: Per Capita Federal Procurement Spending: FY2010



Note: Authors' calculations using data from the USAspending.gov. Metropolitan CBSAs in Alaska and Hawaii are omitted from the figure.

Figure 3: Change in Per Capita Federal Procurement Spending: FY2010 - FY2015



Note: Authors' calculations using data from the USAspending.gov. Metropolitan CBSAs in Alaska and Hawaii are omitted from the figure.

Table 1 shows descriptive statistics for number of transactions, number of modifications, and measures of award/transaction values. The effect of the FY2013 sequester is evident with the sharp drop in the number of transactions in FY2013 and FY2014. The number of overall transactions dropped by 22% between FY2012 and FY2013. The number of Department of Defense contracts declined by almost 8%, and the number of awards that were modified fell by 7%. The large reduction in transactions relative to modifications suggests that agencies more often adjusted to the fiscal shock by awarding fewer contracts than by adjusting existing awards. This is consistent with the stylized fact, noted earlier, that a large majority of contracts (around 85%) are never modified.[17]

Table 1: Descriptive Statistics of Procurement Transactions

Fiscal Year	Transactions	Defense Share	Modifications	Award Value	Mean Transaction Value
2010	3,111,058	42.6%	894,407	$481B	$154,681
2011	2,968,636	44.3%	972,008	$479B	$161,632
2012	2,702,186	46.2%	933,970	$457B	$169,129
2013	2,102,016	54.7%	867,153	$407B	$193,847
2014	2,131,847	55.0%	840,152	$401B	$188,543
2015	3,926,118	75.8%	862,024	$396B	$100,870

Notes: Authors' calculations using data from USASpending.gov. The Award Value column is in billions of real dollars. Mean transaction value is in real dollars.

Although non-defense agencies grant a sizable share of procurement awards by both value and number, the largest recipient firms are dominated by the defense industry. Table 2 shows the top 10 recipient firms, by total procurement awards, from FY2010 through FY 2015.

[17] A small fraction of contracts drive the total number of modifications. Most of these contracts tend to be defense-related awards for weapon systems.

Table 2: Top Recipient Firms of Procurement Contracts

Firm	Transactions	Aggregate Awards	Establishments
Lockheed Martin	188,143	\$221B	172
Boeing	80,733	\$127B	101
General Dynamics	84,047	\$91.3B	138
Raytheon Company	62,720	\$82.1B	102
Northrop Grumman Corporation	53,148	\$49.2B	101
Goodrich/United Technologies	114,708	\$45.1B	145
L-3 Communications	64,556	\$37.9B	156
BAE Systems	63,646	\$33.7B	93
McKesson Corporation	145,382	\$32.7B	33
SAIC Inc.	127,181	\$32.4B	110

Notes: Authors' calculations using data from USASpending.gov. Figures are from Fiscal Year 2010 through Fiscal Year 2015. Aggregate Awards are in billions of real dollars.

An additional notable feature of the USASpending.gov data is that individual establishments can be linked to parent firms through their Dun & Bradstreet's Data Universal Numbering System numbers (DUNS). Firms, or establishments, wishing to pursue government contracts are required to have a DUNS. Take Lockheed Martin as an example. While the firm itself received \$221 billion in awards over this six-year period, the awards were dispersed across 172 distinct establishments (or subsidiaries). We assign the procurement to a CBSA based on the location of the recipient establishment where a majority of the work is expected to occur. Assigning the awards to the location of the parent firm could generate misleading estimates of the effects on local labor markets.

4 Empirical Strategy

Our objective is to quantify how federal spending reductions affect local labor market outcomes. To estimate the impacts, we use the standard local multiplier framework (Gerritse and Rodríguez-Pose 2018; Nakamura and Steinsson 2014):

$$y_{ct} = \beta \text{spending}_{ct} + \alpha_c + \delta_t + \varepsilon_{ct}, \tag{1}$$

where y_{ct} are per-capita labor market outcomes (employment or wages) and spending_{ct} are per-capita federal procurement spending measures in CBSA c and fiscal year t. Variables are scaled to per-capita terms by the contemporaneous year population for each CBSA c and fiscal year t. α_c are a vector of CBSA fixed effects and δ_t are FY fixed effects. Our full sample contains 382 CBSAs for FY 2011 through FY 2015.

There are two challenges to interpreting an estimate of β as causal. First, the allocation of spending is not random across CBSAs. Unobservable CBSA-specific characteristics that draw in federal spending may also affect local economic development. For example, the U.S. Navy has a large presence and significant procurement spending in the Virginia Beach-Norfolk, Va., CBSA due to the region's deep maritime channel. Independent of federal spending, the region's location and natural amenities could also affect long-term economic growth. Our location fixed effects partially address this by removing the time-invariant relationship between local labor markets and federal spending. That is, we control for persistent economic effects induced by the economic history of the CBSA. We also use FY fixed effects to control for shocks common to all labor markets in a given year, which could be confounded with shocks to federal procurement spending in the same year.

A second concern is that federal spending shocks in a given year are not randomly distributed. For example, the government could be concerned with equitably distributing the spending shocks by, for example, avoiding cuts in areas that suffer from stagnant labor markets. In this case, places with better labor market trajectories might receive larger spending cuts and our estimates would be biased towards zero. Additionally, areas with more political clout might manage to insulate themselves from spending cuts. Our coefficient would be biased if these locations have systematically different labor market developments.

To avoid these pitfalls, we instrument for federal spending using a shift-share instrument (Bartik 1991; Borusyak et al. 2022; Goldsmith-Pinkham et al. 2020). The instrument is formed as follows:

$$\text{Predicted Spending}_{c,t} = \text{Spending}_{c,2010} * (1 + \sum_n s_{c,n,2010} * g_{n,t}), \tag{2}$$

where $s_{c,n,2010}$ is the 2010-share of federal procurement spending for a CBSA in a given industry n, defined by 3-digit NAICS code ($s_{c,n,2010}$ add up to one in a CBSA) and $g_{n,t}$ is the percentage point change in procurement spending for a given industry n at the national level. In words, we predict spending for CBSA c in fiscal year t by taking a measure of per-capita spending in 2010 (pre-BCA) and multiplying it by a CBSA's exposure to national spending shocks. The sum $1 + \sum_n s_{c,n,2010} * g_{n,t}$ represents the predicted percentage point change in a CBSA's procurement spending if the BCA-induced spending cuts were distributed *uniformly* across the country. The instrument leverages only the variation in spending shocks due to national industry shocks from the BCA and removes the portion of variation of spending shocks from the government strategically distributing spending cuts differentially across CBSAs (e.g. due to differences in political clout or based on economic well-being).

A recent econometric literature has formalized the different identifying assumptions needed when using shift-share instruments (Borusyak et al. 2022; Goldsmith-Pinkham et al. 2020).[18] To show that our instrument falls into the shift-share form, note that we can rewrite equation (2) as

$$\text{Predicted Spending}_{c,t} = \text{Spending}_{c,2010} + \sum_n \underbrace{\text{Spending}_{c,2010} * s_{c,n,2010}}_{\text{`shares'}} * \underbrace{g_{n,t}}_{\text{`shocks'}}, \tag{3}$$

and that the first term on the right-hand side will be removed by location fixed effects.

We follow Borusyak et al. (2022) and argue our identification comes from the exogeneity of the BCA-induced spending cuts across industries, i.e. "exogenous shocks." Our identifying assumption is that there is no systematic correlation across agencies in terms of which industries will suffer procurement spending cuts. The aggregate national spending cuts for a given industry would therefore be composed of a large set of independent shocks. The federal-level spending shock, the sum of these agency shocks, is therefore plausibly uncorrelated across industries. In Section A.2 of the Appendix we provide diagnostics on the properties of the industry shocks and exposure shares recommended by Borusyak et al. (2022). Intuitively, there could be a problem if some industries receive a very large share of total procurement spending. In this case, we would effectively have so few shocks that spurious correlations between them and local economic factors might appear. In short, the fact that agencies have different priorities and make spending decisions independent of other agencies makes our identifying assumption plausible.

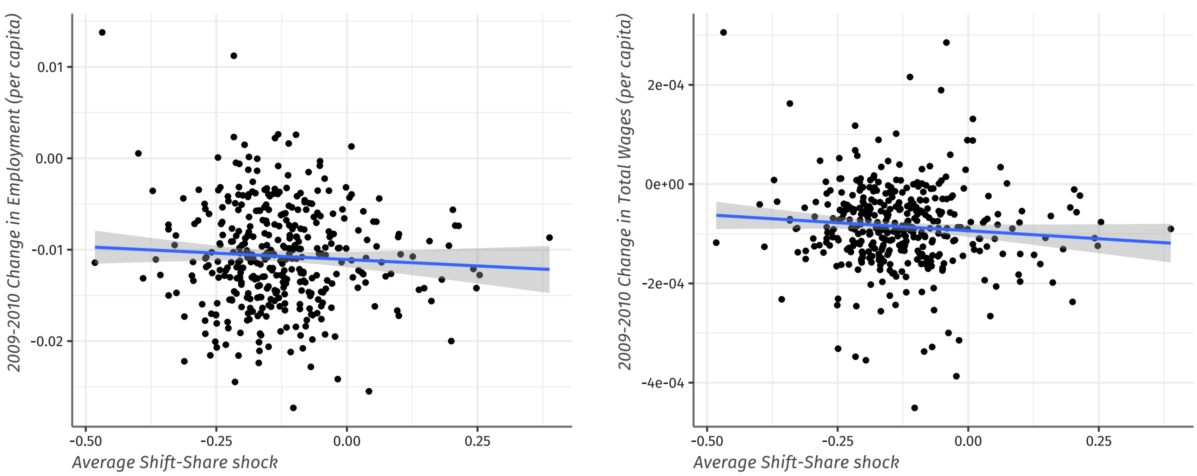
One simple example of this identifying assumption failing would be if (i) most agencies cut procurement spending in manufacturing sectors and (ii) economies with a larger share of procurement spending in manufacturing also had worse labor market trends.[19] In this case, shocks would be

[18] Goldsmith-Pinkham et al. (2020) discuss how to leverage the "shares" as the exogenous source of variation. In our setting, the share of spending in a given industry times the CBSA total per-capita spending. We do not believe these shares are plausibly exogenous to employment changes since procurement spending in CBSAs is likely correlated with factors that also affect local labor market growth patterns.

[19] Note that the correlation is with procurement spending shares and not employment shares.

correlated with economic trajectories, and our instrument would be invalid. We are not able to test this assumption in our treated periods because we cannot observe counterfactual economic trends, in other words those that would have occurred absent the BCA. However, we are able to proxy for this counterfactual trend by testing whether changes in employment and wages from 2009-2010 (pre-trends) are predicted by the average shift-share shock from 2011-2015. This test, akin to a test of the pre-existing trends in difference-in-differences models, is a recommended diagnostic following Borusyak et al. (2022). Figure 4 shows that there is no significant correlation between changes in employment and wages from 2009-2010 (pre-trends) and the average shift-share shock from 2011-2015. The weak correlations in Figure 4 suggest that the shift-share shocks are exogenous to local labor market trends (insofar as pre-trends may reveal counterfactual trends in 2011-2015).

Figure 4: Placebo Test of Identification Strategy



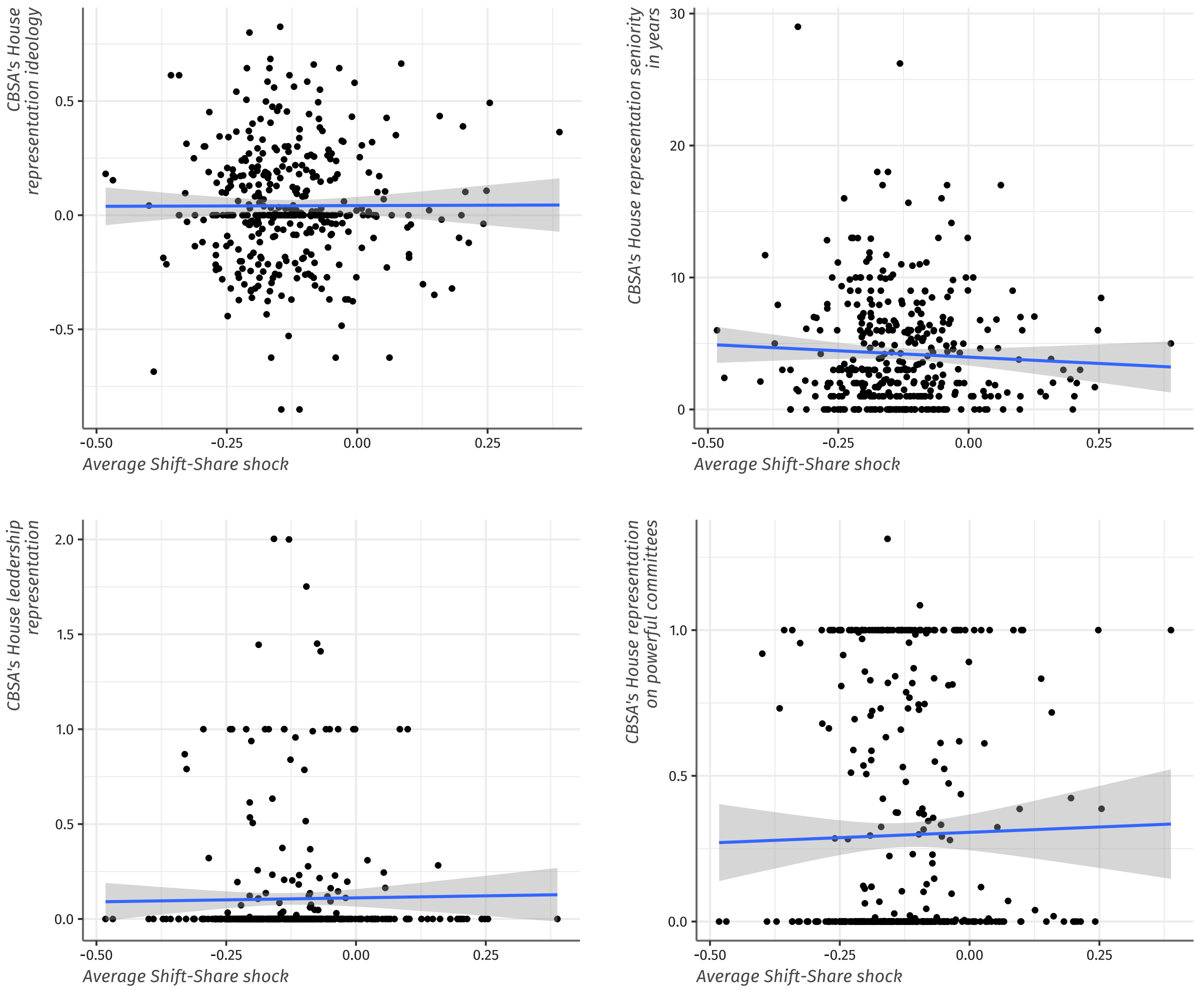
Note: The figures plot first-differences of employment and wages per capita from 2009-2010 (pre-BCA) on the average shift-share shock from 2011-2015. Regressions are a cross-section of 382 metropolitan CBSAs.

An alternative identification concern is that CBSAs with more political power or influence could systematically insulate their constituents from local spending cuts. For example, a politician could apply pressure on agencies to prevent cuts to industries or firms in their district or state. If political power is correlated with local labor market development, this would result in non-randomly assigned industry shocks that would bias our results. We use several dimensions of “political power” from the 112^{th} Congress (2011-2013) to test for correlations between political representation and the distribution of sequester spending shocks. If a CBSA's political power at the time the BCA was drafted and approved is unrelated to subsequent sector shocks, then one would expect to find no correlation in the data.

The political power measures are based on scores/values from the U.S. House of Representatives. For each CBSA, a given value is the population-weighted average of their representatives. We used the Missouri Census Data Center's Geocorr 2014 to create a population-weighted crosswalk between counties and congressional districts. The first political variable we explore is the widely utilized Nokken-Poole measure of ideology (Nokken and Poole 2004). An individual legislator's Nokken-Poole score ranges from -1 to +1, with Republicans generally falling in the 0 to +1 range and Democrats in the -1 to 0 range. The number of years of seniority in the House chamber is used as the second measure of political power. Next, we measure the power of political leadership in a CBSA using the weighted average of an indicator variable that equals unity if a representative is the Speaker of the House, majority leader, minority leader, or a party whip. The final political power variable is the CBAS's weight-average number of representatives (of any party) who are members of three very powerful House committees: Appropriations, Armed Services, and Ways and Means.

Figure 5 shows the results from regressing each of the four measures of political power on the CBSA's observed average shift-share shock. A significant positive or negative correlation could signal that some CBSAs were able to avoid sequester cuts because of their political clout. Insofar as our proxy variables accurately capture the CBSA's "political power," these results provide evidence that CBSAs were not systematically able to avoid spending shocks. Overall, Figures 4 and 5 provide evidence in support of our identifying assumption that the shocks were randomly assigned across industries and metropolitan CBSAs.

Figure 5: Is Pre-BCA Political Power Correlated with the Sequester Shocks?



Note: The figures plot the regression of alternative measures of CBSA "political power" on the average shift-share shock from 2011-2015. Political outcomes are from members of the House of Representatives in the 112th Congress (2011-2013) when the Budget Control Act of 2011 was proposed, amended, and passed into law. CBSA values are the population-weighted averages of House members whose districts overlap with the CBSA boundaries. Regressions are a cross-section of 382 metropolitan CBSAs.

5 Results

5.1 Main Results

We estimate equation (1) using weighted instrumental variables regression. We use population weights to recover nationally applicable multiplier estimates (Gerritse and Rodríguez-Pose 2018). In general, we conduct inference in two ways. First, we allow for shocks to be correlated within a CBSA over time by clustering at CBSA level. However, since our source of exogenous variation is across industries, we follow the methodology of Borusyak et al. (2022) and form standard errors from an auxiliary "industry-level" regression. The "industry-level" regression forms point estimates identical to the shift-share regression but allows our standard errors to be clustered by industry.[20] Our main results below will display both standard errors.

In Table 3 we display our baseline instrumental-variables regression estimates for the effect of total federal procurement spending on aggregate employment and wages. Since our empirical strategy leverages the spending reduction from the BCA, it is useful to interpret the estimated coefficients in this light. The employment estimates in column 1 suggest that a million-dollar reduction in spending results in local employment declining by approximately 10.5 jobs. This implies that a spending reduction of $95,000 results in one local job loss. Similarly, column 3 shows the impacts on the number of jobs using only CBSAs that experience a reduction in federal procurement spending on average over the sample period. The results are quite similar to our main specification and give further credence to the likelihood that our instrumental variables strategy is estimating effects from fiscal consolidation. Our findings show a greater employment adjustment during fiscal consolidation than is shown in the literature on procurement spending during fiscal expansion. In particular, Gerritse and Rodríguez-Pose (2018) find that $250,000 in increased spending creates one job and Auerbach et al. (2020) find that $120,000 in increased defense spending creates one job.[21] In both cases, we find that it takes a smaller reduction in procurement spending to "destroy" a job than an increase in spending to create one.

We examine if wages are rigid to spending reductions in columns 2 and 4 of Table 3. Since both spending and wages are scaled by $1 million, our estimates show that a $1 dollar reduction in procurement spending leads to a $0.19 decline in wages. In contrast, Auerbach et al. (2020) estimates that a $1 dollar increase in spending causes an increase in wages of about $0.32, which is roughly twice as large as our estimate. These results provide evidence consistent with the notion that in periods of fiscal consolidation firms respond along the employment margin, while in periods of fiscal expansion firms are more likely to adjust wages (and potentially hours).[22]

[20]See section A.1 of the Appendix for more details on inference using the shift-share instrumental variables approach.

[21]Since Auerbach et al. (2020) use defense spending, we run an additional specification, shown in the Appendix, using only defense spending and find that a reduction in spending of about $84,000 results in one local job loss.

[22]Estimates using state-by-FY fixed effects show slightly larger costs to destroy one job of about $145,000 but these are far noisier estimates. On the other hand, effects on wages using state-by-FY fixed effects show a decline in wages of about $0.18 for every $1 decline in federal spending. Regressions with state-by-FY fixed effects are not our

One salient explanation for these results is that nominal wage rigidity prevents firms from adjusting to spending declines on the intensive (wages) margin and forces them instead to adjust on the extensive (employment) margin (Howitt 2002). Both Holzer and Montgomery (1993) and Kaur (2019) find similar evidence that wage rigidity distorts local labor market adjustment, albeit in different contexts. The former study uses microdata and looks at how firms adjust employment and wages based on demand shifts either from sales growth or decline. The authors find a small wage adjustment compared to employment. A study by Kaur (2019) uses shocks to the marginal revenue product of labor to show similar labor market adjustments in a developing-country context. Finally, a recent strand of macro research also highlights the role of nominal wage rigidity to explain different magnitudes of multipliers from positive or negative government spending shocks (Barnichon et al. 2022).

To further validate our instrumental variable strategy, we conduct several diagnostic tests. In each model, we show the Kleibergen-Paap Lagrange multiplier (KPLM) test for under-identification and the robust Kleibergen-Paap Wald (KPW) F statistic for weak instruments. Conditional on CBSA and time-fixed effects, the KPLM and its subsequent p-values reject under-identification at conventional levels, while the KPW tests suggest our instrument has strong explanatory power in the first-stage regression.

Table 3: Baseline Regression Results

	(1) Employment	(2) Wages (millions \$)	(3) Employment	(4) Wages (millions \$)	(5) Employment	(6) Wages (millions \$)
Procurement spending per capita (million \$)	10.55	0.1881	6.907	0.1165	11.12	0.1971
	(1.651)	(0.0415)	(1.768)	(0.0394)	(1.007)	(0.0223)
	[4.65]	[0.0966]	[5.315]	[0.1174]	[1.388]	[0.0379]
Implied \$ per job	\$94,815.35		\$144,786		\$89,910.27	
Time FEs	FY	FY	State × FY	State × FY	FY	FY
Sample	Full	Full	Full	Full	Negative Shocks	Negative Shocks
Observations	1,910	1,910	1,910	1,910	1,695	1,695
Adjusted R^2	0.99042	0.99136	0.99390	0.99316	0.99296	0.99279
F-test (1st Stage)	450.11	450.11	263.15	263.15	1,341.3	1,341.3
Kleibergen-Paap LM	9.9308	9.9308	6.6397	6.6397	101.31	101.31
Kleibergen-Paap LM P-value	0.00163	0.00163	0.00997	0.00997	7.85×10^{-24}	7.85×10^{-24}

Note: All models include CBSA fixed effects. The standard errors in parenthesis are clustered at the CBSA-level, and the standard errors in brackets are produced from the auxiliary "industry-level" regression as recommended by Borusyak et al. (2022). Kleibergen-Paap LM is the Lagrange multiplier test, and the Wu-Hausman p-value is the heteroskedasticity-robust test for exogeneity. The null hypothesis is exogeneity. Regressions using only negative shocks include CBSAs that experience negative declines in federal procurement spending on average over the sample period.

preferred specification because they consider only within-state variation. This removes a large amount of variation in procurement, yielding noisy estimates.

5.2 Results by Labor Intensity of Spending

In this section we examine heterogeneity in type of spending and continue to explore the potential of wage rigidity for periods of fiscal consolidation. In particular, we use the labor shares by industry estimates by Jorgenson et al. (2019).[23] Nominal wage rigidity would predict larger employment effects for industries with larger labor shares. To test this empirically, we rerun our analysis using subsets of industries with different labor shares. The shift-share instrument is created in the same way as equation (2) with the summation covering only industries with labor shares in a given range. The ranges are $\{[0\%, 15\%), [15\%, 30\%), [30\%, 45\%), [45\%, 100\%]\}$, and each range contains 17-41 industries.

Table 4: Estimated Coefficients For Procurement Spending on Employment by Labor Intensity

	(1) Employment	(2) Employment	(3) Employment	(4) Employment
Own quartile spending per capita (million $)	1.496	5.347	10.80	15.09
	(6.729)	(13.80)	(4.269)	(3.686)
Other quartiles spending per capita (million $)	13.05	11.32	10.47	7.171
	(2.530)	(3.200)	(1.960)	(3.102)
Implied $ per job	$668,600	$187,015	$92,560.44	$66,258.60
Labor Share	$0\% \leq x < 22.91\%$	$22.91\% \leq x < 37.35\%$	$37.35\% \leq x < 45.01\%$	$45.01\% \leq x < 100\%$
≈ Quartile	Quartile 1	Quartile 2	Quartile 3	Quartile 4
n Industries	27	25	26	29
Observations	1,910	1,910	1,910	1,910
Adjusted R^2	0.99242	0.99005	0.99045	0.99191
F-test (1st Stage)	17.390	85.070	482.42	1,100.5

Note: The regressions estimate equation (1) by instrumental variables. Each column subsets the main dataset to only the industries that fall within the given range for that column as indicated by the "Labor Share" row, and the shift-share instrument is generated by equation (2) with the sum over only the included industries. Labor shares measures are from the KLEMS data. The standard errors in parenthesis are clustered at the CBSA-level, and the standard errors in brackets are produced from the auxiliary "industry-level" regression as recommended by Borusyak et al. (2022).

Table 4 presents point estimates for the effect on per-capita employment. Each column estimates equation (1) with a shift-share instrument given by (2) using only industries with labor shares in the range as labeled in the "Labor Share" row. Note that from left to right the columns use industries with increasing levels of labor intensity. The estimates in Table 4 clearly show that the impact on employment rises as the labor share of production for spending increases.[24] For industries with a

[23] We use the KLEMS estimates from Jorgenson et al. (2019) to examine spending heterogeneity, instead of other industry-level NAICS categories, such as goods and services provided by the Bureau of Labor Statistics (BLS). We believe the KLEMS data are a more useful measure to understand spending heterogeneity and nominal wage rigidity. In general, the relationship with the BLS definition and estimates from the KLEMS data fits expectation. For instance, the average labor intensity for goods-producing industries was about 0.28%; for service-producing industries it was 0.43%. There are some notable exceptions. For example, the BLS considers the construction industry (NAICS 23) as goods-producing, because of the tangible final output. However, the KLEMS data shows that construction is a relatively labor-intensive process (labor share of approximately 43%). For our purposes, it is more informative that construction utilizes a significant share of labor for production than that its final output is a "good".

[24] Following the placebo tests in Section 4, we also explored whether pre-BCA political power was correlated with

labor share of less than 15%, about 7.5 jobs are destroyed for $1 million dollars. For industries with a labor share of more than 45%, about 12.25 jobs are destroyed for $1 million dollars, an increase of 63 percentage points. The results also become more precise for industries with larger labor shares, suggesting a stronger relationship between spending and employment.[25]

The above result could be driven by labor's comprising a larger share of the production function so that a reduction in output mechanically results in larger reductions in employment. However, this logic would also predict that changes in wages would grow with labor share. Table 5 presents analogous results for per-capita wages. The results of this table show that the adjustment of wages does not systematically vary across labor shares. Estimates are centered at our main result in Table 3 with a decline of $0.18 in wages per dollar of decreased procurement spending.

These two tables together provide evidence that in periods of fiscal consolidation, employment is the primary adjustment that firms make. More generally, these results strongly suggest that heterogeneity in spending type and factor intensity of production are key determinants of labor market adjustment during fiscal consolidation.

Table 5: Estimated Coefficients For Procurement Spending on Wages by Labor Intensity

	(1) Wages	(2) Wages	(3) Wages	(4) Wages
Own quartile spending per capita (million $)	0.1722	-0.0694	0.1778	0.2372
	(0.1876)	(0.4518)	(0.0898)	(0.0874)
Other quartiles spending per capita (million $)	0.1925	0.2264	0.1911	0.1517
	(0.0466)	(0.0816)	(0.0520)	(0.0 790)
Labor Share	$0\% \leq x < 22.91\%$	$22.91\% \leq x < 37.35\%$	$37.35\% \leq x < 45.01\%$	$45.01\% \leq x < 100\%$
≈ Quartile	Quartile 1	Quartile 2	Quartile 3	Quartile 4
n Industries	27	25	26	29
Observations	1,910	1,910	1,910	1,910
Adjusted R^2	0.99153	0.99064	0.99133	0.99177
F-test (1st Stage)	17.390	85.070	482.42	1,100.5

Note: The regressions estimate equation (1) by instrumental variables. Each column subsets the main dataset to only the industries that fall within the given range for that column as indicated by the "Labor Share" row; the shift-share instrument is generated by equation (2) with the sum over only the included industries. Labor shares measures are from the KLEMS data. The standard errors in parenthesis are clustered at the CBSA-level and the standard errors in brackets are produced from the auxiliary "industry-level" regression as recommended by Borusyak et al. (2022).

the average shift-share shock in low/high labor-intensive industries. We find no evidence of a consistent correlation between a CBSA's political power and the average shock it experienced in low and high labor-intensive sectors.

[25]Establishments that rely heavily on government contracts could react differently to a spending shock than do establishments with a larger private-sector customer base. Thus, if the government sales intensity of establishments and the labor intensity of procurement spending varied in a systematic manner across CBSAs, then this could bias our estimates. Using establishment-level sales figures from the National Establishment Time Series database and matching by DUNS, we find no correlation between government contracts as a share of sales and the share of procurement spending in low-, mid-, or high-intensive industries across CBSAs.

6 Conclusion

An extensive literature has developed in the past decade exploring how changes in federal spending influences local economic outcomes. These studies have tended to focus on fiscal stimulus as a tool to counter recessions. Federal procurement contracts, which totaled over $400 billion in FY 2011, provide another avenue for the government to impact the labor market and to target economic development efforts. In this vein, work by Gerritse and Rodríguez-Pose (2018) and Auerbach et al. (2020) has looked at the ability of contract spending to spur economic growth and employment. They find that it takes between $120,000 to $247,000 of total procurement spending to *create* a job. Less attention has been paid in the literature to the fact that changing national priorities may decrease spending in some areas and impact the local labor market by reducing demand. Furthermore, the literature has focused on aggregate spending, implicitly assuming that local effects are homogeneous no matter what the federal government procures.

In this paper, we show that the impact of fiscal consolidation depends not only on the amount of spending reduction in a region, but also on the composition or type of spending that declines. We exploit spending caps imposed by the Budget Control Act of 2011 to isolate how fiscal consolidation in federal government contracting affected local employment and wages. Using highly detailed transaction-level data for procurement by all federal agencies, we document large differential effects on local labor market outcomes based on the labor intensity of production for goods and services supplied to the federal government. For instance, we find that a $1 million reduction in federal contract spending reduces employment by more than 12 jobs in high labor-intensive industries (a factor intensity of over 45% of production) and only around seven jobs in low labor-intensive industries (factor intensity of less than 15%). We also find that, relative to wages, employment appears to be the key margin for local labor market adjustments resulting from consolidation.

We argue that together these results suggest that the local labor markets suffer from nominal wage rigidity that becomes apparent in the wake of a negative demand shock. Even though we study federal government purchases, it is important to keep in mind that the purchases are made from private-sector firms under a competitive bid process. Not being government employees, the workers in these firms are subject to the same labor market institutions and job protections (or lack thereof) as other private-sector employees.

The sequester in FY 2013 was unexpected for pundits, policymakers and private-sector firms alike. Since past Congresses had managed to avert the scenario multiple times, it was reasonable to assume a deal would be reached prior to the trigger date.[26] In fact, a Government Accountability Office study notes that the Department of Defense instructed their agencies in September 2012 to maintain spending at normal levels and take no action in anticipation of sequestration (Government Accountability Office 2015). However, after the BCA caps were implemented, the spending reduction was more likely to be viewed as long-term rather than transitory. It will be helpful to view our results on the labor market's adjustment to a negative shock in this context.

More generally, our results reveal that studies aggregating federal spending mask important regional dynamics related to the specific goods and services produced by local firms. This is because aggregate local multipliers are effectively a weighted average of local multipliers based on specific classifications of spending. Our results confirm this sizable local heterogeneity, which has direct implications for the design of effective place-based policies promoting both short-term fiscal stabilization and longer-term economic development.

[26]Prediction markets generally shared this view as they assigned very low probabilities to the sequester at least until mid-to-late December 2012. The prediction market Inkling Markets had a probability of less than 0.50 that sequestration would occur on January 1, 2013 until December 10, 2012.

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A Appendix

A.1 Additional Details on Inference in the Shift-Share IV Approach

In the paper, we form standard errors for our estimates in two ways. First, we allow for clustering within a CBSA over time which is the standard way to conduct inference in our panel regression approach. However, Adao et al. (2019) show that standard errors could be systematically too small if there is correlated shocks to the same industry across CBSAs. To address this concern, we estimate an auxiliary "industry-level" IV regression proposed by Borusyak et al. (2022) that produces the identical point estimates but allows us to cluster the standard errors by industry.

To do this, the data must be aggregated to the industry level. First, the dependent variables (per-capita employment and wages) and per-capita procurement spending are regressed on CBSA and FY fixed effects and residualized. Then, for each industry n in each FY t, we compute a weighted average of those residualized variables $\bar{q}_{n,t} = \sum_l s_{l,n} q_{l,n,t}$ using the shares $s_{l,n}$ described in (3). This results in an industry by FY panel dataset consisting of $\{\bar{y}_{n,t},\ \overline{\text{Spending}}_{n,t}\}_{n,t}$ where y are the outcome variables.

The following equation can then be estimated by a (weighted) IV regression using the national procurement spending shocks $g_{n,t}$ described in (3) as the instrument for $\bar{x}$. Weights are the national shares of procurement spending in that industry $s_n \equiv \sum_c s_{c,n}$:

$$\bar{y}_{n,t} = \alpha + \beta \bar{\text{Spending}}_{n,t} + u_{it}. \tag{4}$$

The estimate for β using the weighted IV regression will be identical to β from the corresponding IV estimates in (2).[27] The advantage of this method is that heteroskedasticity robust standard errors will also be robust to clustered shocks at the industry level (Borusyak et al. 2022).

A.2 Properties of Industry Shocks and Exposure Shares

In addition to the falsification checks using political variables and pre-shock employment and wage trends, we conduct a set of validity checks following those in Borusyak et al. (2022). As an overview, these validity checks ensure that (i) there is a enough variation in shocks after residualizing unit and time fixed effects and that (ii) the effective sample size is large enough for proper inference

[27] Borusyak et al. (2022) provide a `Stata` command `ssaggregate` that transforms the original data-set into this form. This paper uses a corresponding package in `R`.

when clustering standard errors using the industry-level model in equation (4).

First, there is potential concern that after removing CBSA-invariant and period-invariant components of $g_{n,t}$ that there would be little remaining variation left in the shocks. This would result in very noisy estimates that would be hard to do inference on. After residualizing our shocks $g_{n,t}$ on CBSA and fiscal year fixed effects, we have a mean shock of 0, a standard deviation of 0.256, and an interquartile range of 0.479, or about half a percent.[28] This gives us confidence that there is ample residual variation in the shocks to be able to accurately estimate our treatment effect.

Second, the identifying assumption in our shift-share IV approach is that there are plausibly exogenous shocks to many industries. Our identification checks presented in the main body of the paper give us confidence that the shocks are plausibly exogenously assigned to CBSAs. However, there is a potential second, more subtle, concern that there are not 'many' industries. For an extreme example, consider two industries manufacturing and non-manufacturing. Suppose that both shocks are random and manufacturing makes up 90% of government procurement spending, so there is effectively only one industry (manufacturing) being affected by the shock. Even if the shock is randomly assigned, you would still be subject to omitted variable bias because places with more manufacturing receive larger values of the instrument and have potentially other observable factors that can be correlated with contemporaneous employment shocks.

This extreme example should build intuition that we need many industry shocks. Borusyak et al. (2022) recommend using the inverse-Herfindahl index (inverse-HHI) of the share weights s_n to determine the effective sample-size of industries. In our context, the largest industry makes up only 6% of procurement spending and only 4 industries contain a share larger than 1%. Our effective industry sample size is 34.2 industries (out of 107 total). This is close to, though slightly smaller than, the effective sample size in Autor et al. (2013) of 58.4 industries (out of 136 total). Overall, the results of these additional validity checks further support the use of the shift-share instrument.

A.3 Results for Department of Defense (DoD) Spending

In this section, we re-estimate our main regression results using only Department of Defense (DoD) procurement spending to make our estimates more comparable to Auerbach et al. (2020) who estimate the effect of an increase in DoD spending. To do so, we replace our shift-share

[28] All statistics are weighted by industry exposure shares s_n described in (3).

instrument with a modified version with the shares of DoD procurement spending in industry n in CBSA c in 2010. The shocks are national changes in DoD procurement spending in industry n.

Table A.1 presents the results of the main specification. The results are very similar to Table 3, with an estimated decline in DoD procurement spending of $84,000 leading to a loss of 1 job, as compared to the main result of $95,000. The estimated effect on wages is also very similar; a decrease of per-capita wages of $0.21 per $1 decrease in DoD procurement spending. This is very close to our initial estimate of about $0.19 per $1 decrease in overall procurement spending. Since the results match each other so closely, we solely present the baseline estimates in the main paper.

Table A.1: DoD Spending Results

	(1) Employment	(2) Wages (millions $)
DoD Procurement spending per capita (million $)	11.94	0.2104
	(2.159)	(0.0524)
	[6.805]	[0.1389]
Implied $ per job	$83,729.96$	
Time FEs	FY	FY
Sample	Full	Full
Observations	1,910	1,910
F-test (1st Stage)	416.23	416.23
Kleibergen-Paap LM	16.447	16.447
Kleibergen-Paap LM P-value	5×10^{-5}	5×10^{-5}

Note: All models include CBSA fixed effects. The standard errors in parenthesis are clustered at the CBSA-level and the standard errors in brackets are produced from the auxiliary "industry-level" regression as recommended by Borusyak et al. (2022). Kleibergen-Paap LM is the Lagrange multiplier test, and the Wu-Hausman p-value is heteroskedasticity-robust test for exogeneity. The null hypothesis is exogeneity.