Declining Federal Spending and the Social Safety Net: Evidence from the Budget Control Act of 2011*

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Abstract

In 2011 political debate on federal spending and growing federal deficits climaxed with a showdown over raising the debt ceiling. This led to the Budget Control Act of 2011, which resulted in a steep decline in federal discretionary spending. We leverage the institutional details from the Budget Control Act to examine how the contraction in federal procurement contracts with private-sector firms impacted individuals and the social safety net. Our results show procurement reductions in industries with high labor intensity resulted in more pronounced local social safety net expansions. We also examine heterogeneity by demographic groups.

Keywords: Fiscal Consolidation, Federal Contracting, Social Safety Net
JEL Classification Numbers: H3, H5, I3, R1

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1 Introduction

The federal government in the United States is an important player in the private-sector economy. This ranges from means-tested and social insurance programs that assist individuals and families (Moffitt and Scholz 2010) to place-based policies that work through the tax code like enterprise zones (Neumark and Simpson 2015). However, the federal government also impacts the economy through procurement (purchases) from firms in the private sector. In this respect, the U.S. federal government allocates approximately $500 billion in procurement contracts to private-sector firms across the country each year, covering a wide array of goods and services, from basic landscaping to advanced weaponry.

The previous academic literature has focused on how much increases in government spending, typically procurement, stimulate economic activity. In this respect, there is a large macroeconomics literature on fiscal stimulus using a variety of empirical strategies, such as spending shocks due to military conflicts (Ramey 2011). There is an emerging body of research on fiscal stimulus that leverages cross-sectional variation (Chodorow-Reich 2019).[1] Several recent studies have leveraged variation across states or metro areas to examine rising total or defense procurement spending (Gerritse and Rodríguez-Pose 2018; Auerbach et al. 2020; Nakamura and Steinsson 2014). These papers focus on labor market and output effects from government procurement spending. In general, they find fiscal multipliers above 1,[2] while Gerritse and Rodríguez-Pose (2018) and Auerbach et al. (2020) estimate it takes an increase of $250,000 and $120,000 in spending, respectively, to create one job.

However, several recent episodes have shown the perils of austerity or fiscal consolidation, which has received less attention in the literature. The terms austerity and fiscal consolidation often have similar connotations and involve some combination of reducing spending and/or increasing revenue.[3]

[1] The recent cross-sectional studies do not explicitly focus on federal procurement spending and come in two distinct groups. Papers that examine the effects of plausibly exogenous components of the American Recovery and Reinvestment Act of 2009 (E.g. Chodorow-Reich et al. (2012), Conley and Dupor (2013), Dupor and Mehkari (2016), Wilson (2012), Feyrer and Sacerdote (2011)) and those that use a variety of other sources of regional variation (E.g. Acconcia et al. (2014), Bruckner and Tuladhar (2014), Clemens and Stephen (2012), Shoag (2013), Nakamura and Steinsson (2014), Suarez Serrato and Wingender (2016), Hoffman and Mast (2019), Atems (2019)).

[2] For instance, Nakamura and Steinsson (2014) find a multiplier from defense procurement of 1.5 and Gerritse and Rodríguez-Pose (2018) estimate a multiplier using all federal contracts of 1.4, while Auerbach et al. (2020) find an imprecisely measured GDP multiplier of around 1.

[3] For instance, Kitson et al. (2011) describes austerity as "forms of cutting back on spending, notably (but not only) that of governments. From an economic policy perspective, austerity measures are usually implemented to reduce a country's current fiscal deficit. Austerity programs therefore include some combination of measures to

In the aftermath of the Great Recession, several European countries, including the United Kingdom, Italy, Ireland, Greece, Portugal, and Spain, experimented with austerity policies that included both expenditure cuts and increases in tax rates to reduce their government budget deficits. The United States, on the other hand, reduced expenditures when political debate peaked in mid-2011 due to growing federal deficits with a showdown over raising the debt ceiling limit.[4] This led to the Budget Control Act of 2011 (BCA), which created caps and subsequent reductions in federal spending on discretionary programs.

In this paper, we focus on the economic impacts from fiscal consolidation by using the negative procurement spending shock induced by the BCA. The BCA is a helpful testing ground, because it targeted expenditure reductions. This distinguishes it from broader austerity policies. Specifically, we seek to quantify how the negative procurement spending shock affects the labor market and self-reported participation in social safety net programs[5] in different demographic groups.

To accomplish this we use highly detailed transaction-level data for procurement from private-sector firms by all federal agencies with institutional features of the BCA. The BCA created federal spending caps starting in FY2013. Since federal spending breached the caps in the first year, there was an across-the-board reduction in discretionary spending (known as a sequester or sequestration of appropriated funds). Federal agencies were responsible for determining how to implement budget reductions from sequestration. The spending caps also constrained the normal appropriations process and agency-level spending was significantly below what would have been anticipated before the BCA. Due to the diverse missions of individual agencies, it is plausible that they independently assessed which procurement expenses were essential. Thus, our design-based identification strategy using a shift-share instrument, following Komarek et al. (2022), leverages plausibly exogenous shocks across different industries.

reduce public expenditure and to increase tax revenues and other government receipts (such as the selling off of non-financial assets).” Similarly, Alesina et al. (2015) notes “fiscal consolidations are typically multi-year processes in which a government announces and then implements a sequence of deficit-reduction policies.” While the OECD notes that fiscal consolidation invokes policy instruments on both the revenue and expenditure sides, however, the focus of consolidation tends to be more on reducing expenditures (Sutherland et al. 2012).

[4]There has been an increased discussion of reducing government spending as well as government efficiency in recent years. For example, the idea of the “Department of Government Efficiency” proposed in 2024 has a stated goal that would reduce procurement spending. Further, the U.S. government was projected to again hit the debt ceiling in mid-2023. The political rhetoric on raising the debt ceiling had a strong parallel to the showdown in 2011. Most notably, much of the discussion on deficit reduction is on cutting (or capping) federal discretionary spending.

[5]We include unemployment as one of our outcomes. However, in the American Community Survey data we are unable to explicitly distinguish individuals reporting receiving unemployment insurance compensation from all forms of public assistance income.

In particular, we construct a Bartik-style shift-share instrument that leverages the national industry-level (3-digit NAICS) shock from the BCA with local (CBSA) exposure to these shocks based on past federal procurement spending. Borusyak et al. (2022) show that this design-based set-up using credibly exogenous industry shocks is sufficient for identification in a shift-share instrument design. Intuitively, since the total procurement shock comes from many independent agency-level shocks it is plausible that the variation in spending is exogenous to local economic trajectories. We probe this identifying assumption through diagnostic tests recommended by Borusyak et al. (2022). This includes testing whether changes in our key outcomes from 2009-2010 are predicted by the average shift-share shock from 2011-2015. This test is akin to a test of the pre-existing trends in difference-in-differences models.

This paper contributes to the literature in several ways. First, we extend previous work on the impacts of procurement spending and the labor market (Komarek and Wagner 2020; Komarek et al. 2022; Gerritse and Rodríguez-Pose 2018; Auerbach et al. 2020). Gerritse and Rodríguez-Pose (2018) and Auerbach et al. (2020) study federal procurement spending in times of fiscal expansion, while Komarek et al. (2022) examine a period of fiscal consolidation. Together the results from these studies suggest it is more expensive ($250,000 and $120,000 in spending, respectively) to create a job from increasing spending than to lose a job from decreasing spending ($90,000). We expand this by showing that with *declining* spending the reduction in employment is split evenly between exiting the labor force and becoming unemployed (4 per $1,000,000 spending reduction each). We also show how this is sensitive to the type of spending by integrating a notion of spending in labor-intensive industries and non-labor-intensive industries using KLEMS estimates from Jorgenson et al. (2019).

The labor market impacts of declining spending lead to our second contribution to the literature. We provide evidence on how individuals in different demographic groups participate in social safety net programs during fiscal consolidation, which presumably stems from adjustments in the labor market. This contributes to a broad literature on economic activity and social safety net programs (e.g. Black et al. (2002), Charles et al. (2018) and Maestas et al. (2021)).

In this respect, our work most closely aligns with Auerbach et al. (2022), who look at how demand *stimulus* from Department of Defense procurement affects a variety of social outcomes. Together we shed light on the asymmetry between the impact from procurement spending stimulus and consolidation on social safety net outcomes. Auerbach et al. (2022) show evidence that increases in local defense spending provide some benefits to groups targeted by government transfer programs. Our results consider similar demographic groups (age, education, race, and marital status). However, we only focus on unemployment, along with self-reported participation in disability and Supplemental Nutrition Assistance Program (SNAP), and differentiate spending by labor-intensive industries and non-labor-intensive industries. While Auerbach et al. (2022) doesn't look explicitly at unemployment, but instead considers the employment rate. In comparison, both our consolidation results for unemployment and stimulus spending for employment show an adjustment by a range of demographic groups. That is, many demographic groups benefit in the labor market from increased spending, while they are also harmed by increased unemployment from decreased spending. However, our fiscal consolidation estimates differ from increases in procurement with respect to SNAP and disability. For disability Auerbach et al. (2022) show four demographic groups (no bachelor's degree, age 41-61, white and female) decrease their participation in disability when spending increases. Our results do not show any demographic group increasing their participation in disability following a negative spending shock. While on SNAP the estimates in Auerbach et al. (2022) do not show wide-ranging benefits (i.e. reducing SNAP), however, we show that SNAP increases among many of our demographic groups.

2 Background

In January 2011 a group of fiscal conservatives, members of the newly formed Tea Party Caucus, were sworn into office in the 112th United States Congress. This escalated the political debate concerning growing federal deficits and the appropriate level of federal spending and climaxed with a showdown over raising the federal debt limit in early-to-mid 2011. The federal debt ceiling had typically been raised without much fanfare. For instance, the federal debt ceiling was raised from by a total of \$4.5 trillion between 2008 and 2010.[6] To avoid a default on U.S. government debt, Congress and the Obama Administration agreed to and signed into law the Budget Control Act of 2011 (BCA)(Saturno et al. 2016).[7] Our analysis and empirical strategy exploit institutional features of the BCA, notably the expenditure caps and subsequent reduction in discretionary federal

[6]See Austin (2015) for more information on changes in the U.S. debt limit.

[7]The initial legislation, S. 365 (112th Congress), was introduced by Senator Tom Harkin (D-IA) on February 16, 2011. The BCA was enacted on August 2, 2011, and was written as an amendment to the Balanced Budget and Emergency Deficit Control Act of 1985 (the Gramm-Rudman-Hollings Act).

spending.

Depending on one's perspective the BCA was either a meaningful strategy to reign in government spending or simply a means to end the 2011 debt-ceiling crisis. The BCA and subsequent amendments had several noteworthy features. To start, the debt ceiling was increased by $900 billion coupled with $917 billion in cuts over 10 years. To accomplish the deficit reduction the BCA placed caps on discretionary federal spending for fiscal years FY 2013 through FY 2021. According to the Office of Management and Budget (OMB), this would result in a total savings of approximately $1.5 trillion (Congressional Budget Office 2011). Next, the BCA included several mechanisms to encourage bipartisan cooperation on deficit reduction. The reduction in government spending was split between defense and non-defense programs, which tend to be favored by Republicans and Democrats, respectively. Furthermore, discretionary spending levels breaching the BCA caps in any fiscal year would trigger an automatic across-the-board reduction, or sequester, of appropriated funds.[8] The sequestration of appropriated funds was seen as particularly imprudent.[9] Individual agencies lacked discretion over program-level reductions due to the across-the-board nature of sequestration, preventing them from reallocating funds. Instead, agencies retained discretion over how to achieve necessary reductions within specific program categories (Saturno et al. 2016). Finally, the BCA provided a path to avoid sequestration with the creation of the Joint Select Committee on Deficit Reduction (also called the "Super Committee"). This committee was charged with developing an alternative deficit-reduction plan by January 12, 2012.

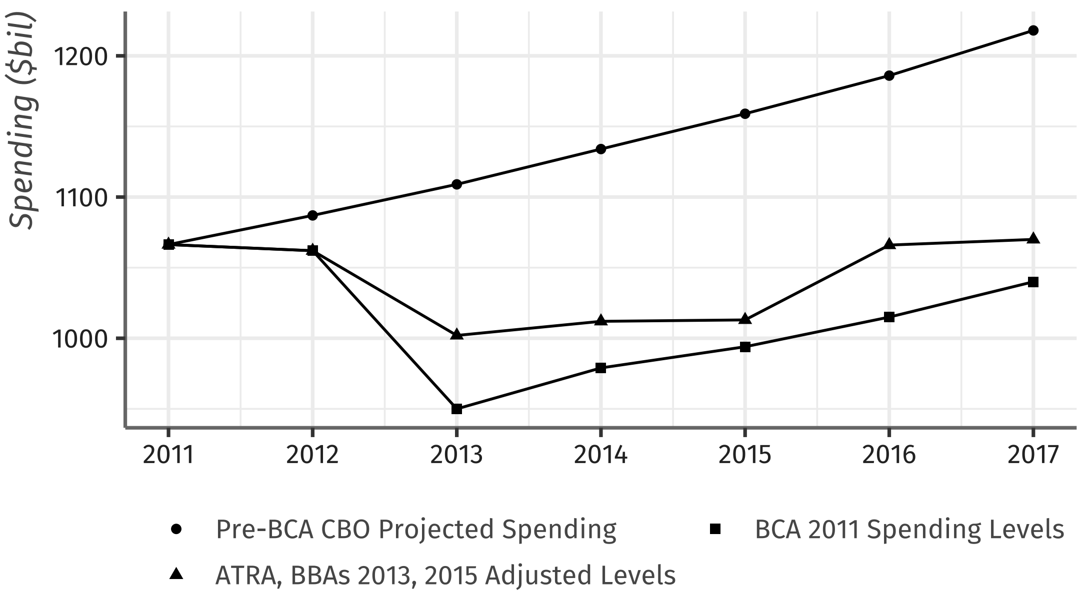
In spite of their endeavors, the Super Committee's pursuit of a comprehensive agreement on deficit reduction proved fruitless. This resulted in the first sequester in U.S. history (in FY 2013) because the federal government had been operating under a continuing budget resolution that exceeded the BCA caps. To mitigate the impact, the American Taxpayer Relief Act of 2012, colloquially known as the "fiscal cliff deal," postponed sequestration from January 2, 2013, to March 1, 2013. Furthermore, it attenuated the magnitude of spending reductions to $85 billion, equally divided between defense and non-defense agencies. To provide a visual representation of the situation,

[8] If a sequester is triggered, the OMB is the agency responsible for determining the funding reductions for non-exempt budget accounts to comply with the BCA caps. 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 the Balanced Budget and Emergency Deficit Control Act of 1985 (Driessen and Labonte 2015).

[9] Steve Ellis of the Taxpayers for Common Sense in an interview with PolitiFact remarked: "Part of the whole reason (lawmakers) thought that the sequester would work was it was so stupid and awful."

Figure 1 illustrates the trajectory of discretionary federal spending, encompassing both projected expenditures preceding the BCA and the subsequent imposition of caps and amendments.[10] The spending caps underwent multiple alterations through the Bipartisan Budget Acts of 2013 and 2015.

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 Budget Control Act (BCA) and its subsequent amendments had far-reaching implications for discretionary federal spending. The sequester in FY 2013 reduced within-fiscal year discretionary federal expenditures by 8% and 5% for defense and non-defense spending, respectively (Spar 2013). The difference between these two categories is due to exemptions granted within the BCA (Driessen and Labonte 2015). Specifically, the spending caps imposed by the BCA did not apply to Social Security and Medicaid, while Medicare reimbursements faced a 2% reduction. Importantly, military personnel and their compensation were exempted from the provisions of the BCA, thus engendering discrepancies in how defense and non-defense agencies were affected.

[10]The $1.5 trillion in savings projected by the Congressional Budget Office comes from the difference between the pre-BCA projected spending and the original BCA 2011 spending caps.

Several features of the BCA are important for our empirical identification strategy. First, the FY 2013 sequester resulted in an exogenous reduction of appropriated discretionary spending. Since the spending cut was across-the-board, agencies only had discretion on what and where to cut spending, but not in the magnitude of the loss. Therefore, it is plausible that the agency-by-industry-by-location spending changes were as good as random shocks. Second, Figure 1 shows how the BCA caps constrained discretionary spending below CBO projections. This led to agency-level spending being considerably lower than previously anticipated before the Budget Control Act became law. This disruption in the typical appropriations process likely forced agencies to prioritize spending based on operational needs. Since agencies have different missions they may also prioritize their procurement in the private sector differently. Therefore, it is unlikely that procurement shocks would systematically target a particular industry or location, considering the unique characteristics of each federal agency.[11]

3 Data and Descriptive Statistics

We examine the impact of federal spending on local labor market outcomes and reported social safety net participation using individual procurement contract data from *USAspending.gov*. This program, which originated in 2006 via the Federal Funding Transparency Act, furnishes a wealth of information concerning federal contracts, grants, loans, and financial assistance. Regular updates are provided on a monthly basis, with the data deriving from the Federal Procurement Data System (FPDS), the primary real-time database for US government procurement data.

The reported data on *USAspending.gov* encompasses all transactions pertaining to prime recipient contracts exceeding $3,000, as well as grants, loans, and financial assistance surpassing $25,000. These transactions comprise of both initial contracts and subsequent modifications, which may transpire due to a variety of factors, including supplemental agreements, option exercises, or contract terminations. It is worth noting that roughly 85% of contracts remain unmodified, with modifications necessitating approval from both the vendor and the government contracting agent. In order to safeguard the government's interests, the implementation of performance-based con-

[11] Nonetheless, it is important to acknowledge that Congress possesses the authority to exercise discretion in reallocating spending priorities within the permissible caps. To dismiss any concerns regarding potential political manipulation, Komarek et al. (2022) examine the correlation between a CBSA's political influence and the distribution of sequester reductions. Their findings eliminate any suspicion of political manipulation. We present similar results in Appendix A.1.

tracts is actively encouraged. Under this arrangement, vendors receive payment only upon fulfilling specified deliverables. Advance payments authorized by federal agencies are considered exceptional and are typically concentrated in defense contracts. Vendors generally possess limited authority to alter payment timelines without explicit approval from their contracting officer, thereby indicating minimal leeway for circumventing sequester-related cuts.

The FDPS provides comprehensive coverage, averaging 97.7% of all procurement awards from 2009 to 2014, according to a senior procurement executive's report (Komarek et al. 2022). This extensive coverage ensures that our dataset accurately reflects the entire scope of procurement transactions across federal agencies, including purchases of services, products, and equipment. The dataset includes various information fields, such as contract start and end dates, obligated funds, zip codes for performance and headquarters, funding agencies, and unique identifiers distinguishing new contracts from modifications. Additionally, industry classifications (NAICS codes) describe the type of goods or services purchased, although they may be based on the predominant item. To create a spending proxy for each contract, we follow the literature and aggregate all obligations and modifications, distributing the obligation amount equally over the contract's relevant timeframe (Auerbach et al. 2020; Komarek and Wagner 2020; Komarek et al. 2022). For instance, a $150,000 annual contract is assumed to result in $12,500 spending per month (12 months).

We aggregate the data across different dimensions to construct our federal spending measures. Firstly, we align the spending series with the calendar year to link procurement spending with the timing of dependent variables. Secondly, we aggregate the spending based on the place of performance zip code, which represents the primary location where at least 51% of the work or goods and services are expected to be utilized. We utilize metropolitan core-based statistical areas (CBSAs) as the relevant labor market geography.

Furthermore, we examine how heterogeneous spending affects local labor markets and the social safety net. Particularly, we use the labor shares of production by industry estimates from Jorgenson et al. (2019) to categorize industries by labor intensity.[12] The data are provided at the four-digit NAICS code. We average these estimates at the 3-digit level and, following Komarek et al. (2022), bin industries in quartiles for their labor shares. The labor intensity ranges for each quartile

[12]We utilize the KLEMS estimates from Jorgenson et al. (2019) rather than relying on industry-level NAICS categories such as goods and services provided by the BLS. Overall, the relationship between the BLS definition and the estimates from the KLEMS data aligns with our expectations.

are approximately $\{[0\%, 23\%), [23\%, 37\%), [37\%, 45\%), [45\%, 100\%]\}$, and each range contains 25-29 industries.[13] We then classify the industries in the highest quartile as labor-intensive and the other industries as non-labor-intensive and compute the respective procurement spending in goods and services from each "type" of industry.

In addition to our procurement spending measures, we incorporate labor market data from the Bureau of Labor Statistics (BLS) and the Census Bureau. We utilize county-level data from the BLS Local Area Unemployment Statistics, which includes information on the number of employed individuals, unemployed individuals, and those participating in the labor force. We also integrate self-reported data from individuals using data from the Census Bureau's American Community Survey (ACS). To construct measures related to unemployment, and self-reported participation in SNAP and disabled individuals, we utilize individual-level data from the ACS and aggregate it to the CBSA level. We construct the disaggregate measures of individuals by several demographic categories such as age, education, race, and marital status. We aggregate these labor market measures to the calendar year and use the 2015 CBSA definitions. We also incorporate the Census Bureau's local population measure to enhance our analysis.

Table 1 shows the descriptive statistics of procurement spending per capita, labor market outcomes, and the number of individuals enrolled in social safety net programs at the core-based statistical area level between 2011 and 2015. Notably, the average total procurement spending in CBSAs was over $1,000 per person with a large standard deviation of over $2,000. The procurement spending is split with approximately 35% in labor-intensive industries and 65% in non-labor-intense industries. Two hundred and ninety CBSAs saw a decrease in spending over our study period, accounting for more than 86% of CBSA residents nationwide. Finally, our measures of unemployment come from two different sources, BLS Local Area Unemployment Statistics and ACS, respectively. Due to differing survey methods, they provide different estimates of average local unemployment.

[13] The industries with the lowest labor intensities include Petroleum Manufacturing (2.4%), Chemical and Primary Metal Manufacturing (11%), Crop and Animal Production (14%), and Transportation Equipment Manufacturing (17%). In the middle range, we find Fabricated Metal Manufacturing (27%), Electronic Manufacturing (35%), Governmental Administration Programs (37%), and Heavy and Civil Engineering Construction (43%). On the higher end, we have Social Assistance (54%), Professional, Scientific, and Technical Services (50%), and Repair and Maintenance (47%).

Table 1: Descriptive Statistics: Spending, Labor Market Outcomes and Safety Net (2011 - 2015)

	Mean	St. Dev.	Min	Max
Procurement Spending: $'s Per Capita				
Total Spending	1,065.607	2,064.250	0.830	30,260.300
Labor Intense Spending	364.737	1,036.360	0.100	13,929.490
Non Labor Intense Spending	700.870	1,487.006	0.272	30,196.850
Labor Market (BLS): Outcome Per Capita				
Employed	0.449	0.048	0.225	0.585
Labor Force	0.484	0.045	0.244	0.602
Unemployed	0.034	0.012	0.014	0.130
Quantities of Individuals (ACS): Outcome Per Capita				
Unemployed	0.041	0.014	0.008	0.109
SNAP Recipient	0.176	0.062	0.038	0.652
Disabled	0.146	0.043	0.066	0.623

Note: All variables are aggregated to the CBSA level. The procurement spending is from individual contracts in the Federal Procurement Data System, the labor market variables are aggregated from the county-level Bureau of Labor Statistics Local Area Unemployment Statistics and the data on the quantities of people unemployed, receiving SNAP benefits and disabled are aggregated to the CBSA-level from individual-level American Community Survey. SNAP stands for the Supplemental Nutrition Assistance Program.

4 Empirical Strategy

Our empirical strategy follows Komarek et al. (2022) and is in the vein of other standard frameworks in the literature (Gerritse and Rodríguez-Pose 2018; Nakamura and Steinsson 2014). In particular, we use the following model to estimate the impact of federal procurement reductions on local economic outcomes:

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

where y_{ct} are economic outcomes (i.e. labor market and self-reported enrollment in social safety net programs) and spending_{ct} are federal procurement spending in CBSA c and year t. Both the economic outcomes and the procurement spending variables are scaled by contemporaneous year population for each CBSA c and year t. δ_t are year fixed effects and α_c are a vector of CBSA fixed effects. ε_{ct} is the random disturbance term. Our full sample contains 382 CBSAs for 2011 through 2015.

Including CBSA and year fixed effects aid in addressing bias and allowing a potential causal interpretation of β in Equation (1). This is due to the fact that spending is not distributed randomly across CBSAs. Unobservable CBSA-specific characteristics could affect both federal procurement spending and local economic development. By using CBSA fixed effects we control for the long-run (time-invariant) economic history of a region, while year fixed effects account for time-varying factors that affect all CBSAs that could be misattributed to shocks to federal procurement spending in the same year.

Notwithstanding, there is an additional concern that federal spending shocks in a given year are not randomly distributed. For instance, this could come from political clout insulating some areas from cuts (or increasing spending) or the desire to limit spending reductions in economically depressed regions. To account for potential bias from the non-random distribution of spending shocks we use a shift-share instrumental variables strategy (Bartik 1991; Goldsmith-Pinkham et al. 2020; Borusyak et al. 2022). Our shift-share instrument exploits variation in procurement spending from the shock to national industries due to the BCA. Thus, it purges the variation in spending from strategic decisions on how to distribute funds, such as political sway.

Formally, the predicted annual change in spending for CBSA c - our Bartik instrument - is formed by the following expression:

$$\Delta \text{Predicted Spending}_c = \sum_n \text{Spending}_{c,2010} * s_{c,n,2010} * g_{n,t} \tag{2}$$

where $\text{Spending}_{c,2010}$ represents the per capita spending in CBSA c in 2010 (prior to the BCA's passage), $s_{c,n,2010}$ is the 2010-share of federal procurement spending for a CBSA in a given industry n, defined by its 3-digit NAICS code, and, $g_{n,t}$ is the percentage point change in procurement spending for a given industry n at the national level. Together the instrument combines a measure of per-capita spending in 2010 (pre-BCA) with a CBSA's exposure to national spending shocks. Thus, the instrument is tantamount to the predicted change in local spending if the spending cuts from the BCA were uniform across the country. We recover the predicted *level* of spending in each fiscal year by adding the level of spending in 2010 for each CBSA to the predicted change in spending in Equation (2).[14]

Our instrument weights the variation in spending shocks resulting from national industry shocks caused by the BCA. It eliminates the portion of spending shocks attributed to the government stra-

[14] Specifically, the following equation produces the predicted level of spending for fiscal year t and CBSA c: $\text{Predicted Spending}_{c,t} = \text{Spending}_{c,2010} + \sum_n \text{Spending}_{c,2010} * s_{c,n,2010} * g_{n,t}$. See Komarek et al. (2022) for more details on the instrument creation in the context of the decline in federal procurement spending induced by the BCA.

tegically allocating spending cuts unevenly across CBSAs. By controlling for CBSA and year fixed effects, we are comparing between CBSAs that experience larger negative shocks to those with smaller negative shocks, along with a few observations that encounter positive spending shocks, in our empirical strategy. To ensure the robustness of our findings, we address the potential impact of small positive shocks that may be averaging with the effects of negative shocks in our estimates. Specifically, we demonstrate in Section A.2 of the Appendix that removing observations with positive shocks does not significantly alter our point estimates. This analysis provides additional support that we are accurately estimating the impact of negative procurement shocks.

Recent literature has formalized the identifying assumptions and diagnostic tests for shift-share instruments. Goldsmith-Pinkham et al. (2020) discuss shift-share instruments that leverage exogenous "shares", while Borusyak et al. (2022) examine the case for exogenous "shifts" or "shocks." Since our setting leverages plausibly exogenous spending cuts across industries and locations due to the BCA - i.e. "exogenous shocks" - our approach is aligned with Borusyak et al. (2022). The key assumptions in this setting are i) quasi-random shock assignment; and, ii) many uncorrelated industry shocks. Intuitively, it would be problematic if the share of federal procurement spending were concentrated in a few industries, and the regions with a larger share of procurement in the concentrated industries had deteriorating economic conditions. This could lead to a spurious correlation between industry shocks and local economic trajectories.

Borusyak et al. (2022) provide several diagnostics to examine the validity of the instrumental variables strategy. First, they provide diagnostics for the industry shocks and exposure shares. They suggest examining the variation in $g_{n,t}$, the industry-level shocks, after residualizing for CBSA and year fixed effect. This ensures there is enough variation to precisely estimate the coefficients of interest. After residualizing our shocks $g_{n,t}$ on CBSA and year fixed effects and weighting by the industry exposure shares, s_n from Equation (2), the mean shock is 0, with a standard deviation of 0.256, and an interquartile range of 0.479. This provides sufficient residual shock variation for our estimated impact. We also calculate the effective industry sample size by using the inverse Herfindahl index (inverse HHI) of the share weights (s_n). Our effective industry sample size is 34.2. Only four out of the 107 total industries have a share larger than 1% of procurement spending.[15]

[15] The largest industry share makes up 6% of procurement spending. In comparison, Autor et al. (2013) have an effective sample size of 58.4 out of 136 industries.

Next, we examine whether the BCA-induced procurement spending shocks are plausibly exogenously assigned to CBSAs. First, Komarek et al. (2022) explore whether local political power or influence systematically impacts changes in spending to CBSAs due to the BCA. They use several measurements of "political power" from the 112^{th} Congress (2011-2013), such as the Nokken-Poole measure of ideology and years of seniority in the House of Representatives, among others, and find that CBSAs were not able to systematically evade sequester cuts due to their political influence.[16] Second, we conduct a diagnostic test analogous to testing for pre-existing trends in a difference-in-differences model (Dix-Carneiro and Kovak 2017; Dix-Carneiro et al. 2018; Borusyak et al. 2022). We test whether changes in labor market outcomes or social safety net spending from 2009-2010 (pre-trend) are correlated with the average shift-share shock from 2011-2015. Table 2 presents the placebo test for our identification and shows that changes in the primary variables of interest before the BCA do not predict the average CBSA shift-share shock. These results give credence to our argument that the shift-share shocks are exogenous to local labor market trajectories. The findings, coupled with the inability of CBSAs to avoid spending shock because of their political clout, support our identifying assumption that the shocks were randomly assigned across industries and metropolitan CBSAs.

Additionally, our empirical strategy is also aimed at examining the impacts of heterogeneous types of spending on local labor market outcomes and social safety net enrollment and payments. Particularly, we follow Komarek et al. (2022) and use estimates for industry labor shares from Jorgenson et al. (2019) to characterize labor-intensive and non-labor-intensive industries. We then estimate the following model of diverse spending:

$$y_{ct} = \beta \text{ Labor Intensive Spending}_{ct} + \eta \text{ Non-Labor Intensive Spending}_{ct} + \alpha_c + \delta_t + \varepsilon_{ct} \tag{3}$$

where Labor Intensive Spending$_{ct}$ represents the procurement spending in industries with labor shares in the highest quartile of the distribution, Non-Labor Intensive Spending$_{ct}$ is the procurement spending level in non-labor intensive industries. The remaining parameters are similar to Equation (1). In this case, our identification strategy relies on two shift-share instruments following Equation (2). The first only considers industries within the fourth quartile of the distribution

[16] In Section A.1 we perform the same robustness check for our identification strategy and discuss the results in detail.

of labor shares in the summation and the second the non-labor-intensive industries (i.e. quarters 1-3 of the labor share distribution).

Table 2: Pre-Trend Results: Placebo Test

	Employed (1)	Labor Force (2)	Unemployed (BLS) (3)	SNAP (4)	Unemployed (ACS) (5)	Disabled (6)
Constant	-0.0090***	-0.0072***	0.0018***	0.0195***	0.0024**	0.0004
	(0.0015)	(0.0014)	(0.0003)	(0.0022)	(0.0008)	(0.0010)
Average Shift-Share Shock	-0.0053	-0.0035	0.0018	-0.0233*	-0.0084*	-8.83e-5
	(0.0076)	(0.0075)	(0.0015)	(0.0108)	(0.0038)	(0.0050)
Observations	382	382	382	381	381	381
R^2	0.00129	0.00059	0.00357	0.01224	0.01272	8.17e-7
Adjusted R^2	-0.00134	-0.00204	0.00094	0.00963	0.00175	-0.00264

Note: Each column represents a cross-section regression with the average shift-share shock from 2011-2015 and the change in each outcome variable per capita from 2009-2010. The models using American Community Survey data do not include the CBA for Enid, Oklahoma because of missing data. The employed, labor force, and unemployed labor market variables are aggregated from the county-level Bureau of Labor Statistics Local Area Unemployment Statistics. We use data from the U.S. Census American Community Survey on self-reported SNAP, unemployment, and disabled . SNAP stands for the Supplemental Nutrition Assistance Program. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

5 Results

5.1 Labor market outcomes

Following Gerritse and Rodríguez-Pose (2018) and Komarek et al. (2022), we estimate Equation (1) using weighted instrumental variables regression with population weights to recover nationally applicable estimates. We provide inference in two ways. First, by allowing shocks to be correlated within a CBSA over time by clustering at the CBSA level and second by constructing an auxiliary "industry-level" regression suggested by Borusyak et al. (2022) to allow our standard errors to be clustered by industry.[17] Table 3 presents the results for our baseline instrumental-variables regressions for labor market outcomes. In Panel A of Table 3, we focus on the effect of total federal procurement spending on various labor market indicators at the CBSA-level. It is crucial to interpret the estimated coefficients in the context of our empirical strategy, which leverages the spending reduction resulting from the BCA.[18] The impact of the BCA on the labor market helps set the stage for the social safety net implications that follow. The estimates in column 1 indicate

[17] We can not estimate the industry-level standard errors proposed by Borusyak et al. (2022), because their methodology assumes a single endogenous variable.

[18] As discussed in Section 4, it is possible that our results are averaging some positive spending shocks with the vast majority of negative procurement spending shocks at the local level. We check whether our results are dependent on such an averaging effect and show in Section A.2 that the impacts on the number of employed individuals, labor force participation, and, unemployed individuals using only CBSAs that experience a reduction in federal procurement spending on average over the sample period (about 86% of total CBSA population) are quite similar to our main specification.

that a \$1 million reduction in spending results in the number of employed individuals declining by approximately 8.4. This implies that a local spending reduction of less than \$120,000 results in one fewer employed individual. Columns 2 and 3 of Table 3 show the labor responses in terms of labor force participation and unemployment. The estimates in column 2 of Panel A show that a \$1 million reduction in spending results in local labor force participation declining by roughly 4, while the estimates in column 3 suggest unemployed individuals increasing by approximately 4.4 at the local level. Taken together, these results suggest that the reduced employment resulting from a decline in federal procurement spending is roughly equally divided between individuals exiting the labor force and those becoming unemployed and actively seeking employment.

The results in Table 3 use employment data from the BLS Local Area Unemployment Statistics program and we find substantial employment adjustments during fiscal consolidation on local labor markets. Our results are more moderate than those presented in Komarek et al. (2022) using data from the Quarterly Census of Employment and Wages (QCEW) for the numbers of jobs, which may lead to overestimation of individual employment level impacts. For example, people with multiple jobs are employed, but those jobs are counted multiple times in the QCEW data. Nevertheless, our findings show a greater employment adjustment during fiscal consolidation than the literature on procurement spending during fiscal expansion such as Gerritse and Rodríguez-Pose (2018), but similar to Auerbach et al. (2020) that find that \$120,000 in increased defense spending creates one job.

However, it is helpful to consider that various forms of expenditure can have differential effects on the local labor market's adjustment process. Procurement spending exhibits notable heterogeneity. In Panel B of Table 3 we investigate the influence of diverse types of procurement spending on local labor markets during periods of fiscal consolidation. Each column estimates Equation (3) with shift-share instruments dictated by Equation (2) for labor-intensive industries and non-labor-intensive industries.

The results indicate that the impact of procurement spending on labor market outcomes becomes more pronounced as the labor share of production increases compared to the aggregate spending effects examined in Panel A. The results for total procurement spending essentially show a weighted average of the labor intensity impacts. For instance, column 1 shows that, for labor-intensive industries, about 11 fewer individuals were employed for every \$1 million reduction in procurement spending at the local level. For non-labor-intensive industries, the same $1 million reduction in procurement results in roughly 6 fewer employed individuals in a CBSA. Nevertheless, the estimated coefficient for non-labor-intensive industries is not statistically significant at the 5% level. A similar pattern is observed in column 2, for the impact of procurement spending on labor force participation. For every $ 1 million reduction in procurement spending for labor-intensive industries, we estimate a reduction of the number of individuals in the local labor force by 6.4. Although the magnitude is smaller, the impact on non-labor-intensive industries is not statistically significant at conventional levels.

In column 3 of Panel B, we examine the impacts of different types of spending on the number of unemployed individuals in local labor markets. Unlike the outcomes discussed above, the increase in the number of unemployed individuals caused by reductions in procurement spending is similar in labor-intensive and non-labor-intensive industries. Akin to the estimated coefficient for aggregate spending (shown in Panel A), a $1 million reduction in procurement spending increases the number of unemployed individuals by approximately 4.1 to 4.8 (non-labor intensive and labor-intensive industries, respectively). These findings suggest a more pronounced adjustment to economic shocks in labor-intensive industries, where a significant portion of job losses leads to individuals exiting the local labor markets.

Generally, the results presented in Panel B of Table 3 strongly suggest that heterogeneity in spending type and factor intensity of production are key determinants of labor market adjustment during fiscal consolidation. Similar to Komarek et al. (2022), we document that the impacts of declines in procurement spending on employment and labor force participation can vary substantially depending on the types of goods and services produced. Our findings suggest that procurement spending in labor-intensive goods and services is driving the impacts of spending reductions on the number of individuals employed and labor force participation at the local level. This evidence enriches our understanding and complements the results presented in Komarek et al. (2022), further indicating the amplified employment impacts of fiscal consolidation on labor-intensive sectors.

To enhance the credibility of our instrumental variable approach, we perform a series of diagnostic tests. Within each model, we present the Kleibergen-Paap Lagrange multiplier (KPLM) test to assess under-identification and the robust Kleibergen-Paap Wald (KPW) F statistic to eval-

Table 3: Baseline Regression Results: Labor Market Outcomes

Panel A: Total Procurement Spending			
	Employed	Labor Force	Unemployed
	(1)	(2)	(3)
Spending (million $s)	8.370***	3.952***	-4.418***
	(1.531)	(1.234)	(0.8056)
	[5.305]	[2.950]	[2.576]
Fixed-effects			
CBSA	Yes	Yes	Yes
Year	Yes	Yes	Yes
Observations	1,910	1,910	1,910
F-test (1st stage)	434.40	434.40	434.40
Kleibergen-Paap	8.3249	8.3249	8.3249
Kleibergen-Paap, p-value	0.00391	0.00391	0.00391

Panel B: Spending by Labor Intensity			
	Employed	Labor Force	Unemployed
	(1)	(2)	(3)
Labor Intensive Spending (million $s)	11.14***	6.379***	-4.762***
	(2.516)	(1.609)	(1.373)
Non-Labor Intensive Spending (million $s)	6.187*	2.040	-4.147**
	(3.401)	(2.658)	(1.772)
Fixed-effects			
CBSA	Yes	Yes	Yes
Year	Yes	Yes	Yes
Observations	1,910	1,910	1,910
F-test (1st stage), Labor Intensive	1,057.6	1,057.6	1,057.6
F-test (1st stage), Non-Labor Intensive	98.477	98.477	98.477

Note: 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 suggested by Borusyak et al. (2022). Kleibergen-Paap LM and the corresponding p-value the heteroskedasticity-robust test for exogeneity of the instrument. For the CBSA-level clustered standard errors, significance levels are shown as: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

uate the strength of our instruments. Controlling for CBSA and time-fixed effects, the KPLM test, along with its corresponding p-values, rejects the possibility of under-identification at standard significance levels. Additionally, the KPW tests indicate that our instrument exhibits substantial explanatory power in the first stage regression.

5.2 Safety net participation by demographic groups

Next, we focus on the labor intensity estimates from Equation (3) with the number of individuals self-reporting as unemployed, disabled, and participating in the SNAP program by demographic group as the dependent variable. We examine the local-level responses to industry-specific procurement spending reductions in Table 4. In column 1, we show that a $1 million reduction in procurement spending on labor-intensive goods and services leads to an increase of more than 6 individuals who self-report to be unemployed for all demographic groups. Disaggregating this result, we show the increase occurs mainly for the population between 18 and 40 years old (an increase of almost 4 individuals) and from 41 to 61 (an increase of roughly 2 people), the two groups that compose the vast majority of the working-age population. The estimated coefficients are not statistically significant individually for the other age groups.

In addition, the increase in the reported unemployed after fiscal consolidation affecting labor-intensive industries seems to be concentrated in individuals without a bachelor's degree or higher education. The estimated coefficient is more than 5 times larger compared to individuals with a bachelor's degree or higher. These results are qualitatively in line with Auerbach et al. (2022), who find substantial impacts of increases in defense spending on local employment rates of demographic groups without a bachelor's degree. In terms of race, reduced spending appears to more directly increase the number of white individuals unemployed as the impact is smaller and imprecise for both black and Hispanic groups (not statistically significant at the 5% level). Finally, the disaggregation by marital status indicates that married individuals are proportionally more affected by the reduction in government spending than unmarried individuals. In general, although the coefficients present the expected sign, the impacts of the reduction in procurement spending in non-labor-intensive industries on the number of individuals unemployed are imprecisely measured in either aggregate or by demographic group.

With regard to the impact of fiscal consolidation on the number of individuals who self-report as disabled in local economies, the results are quite stable. Column 2 of Table 4 shows that reductions in procurement spending across both labor-intensive and non-labor-intensive industries do not appear to affect the number of potential disability transfer recipients. Our findings point to the detachment of the number of individuals who self-report disabilities at the local level from fiscal consolidation. Interestingly, our results diverge partially from previous literature. Notably, Autor and Duggan (2003) observed significant responsiveness of disability insurance application rates to negative labor demand shocks for more than a decade following the mid-1980s reforms. Similarly, Maestas et al. (2021) find a notable impact of the Great Recession on applications for disability insurance. However, the question of whether self-reported disability reacts to short-run labor market conditions remains uncertain. In a recent work, Auerbach et al. (2022) document that self-reported disability rates decreased locally with increased defense spending. Nonetheless, we provide evidence that BCA-induced spending reductions do not seem to have impacted self-reported disability in CBSAs.

In column 3, we present the results of the impacts of procurement spending reductions on the number of individuals self-reporting as SNAP benefit recipients. Similar to unemployment, the reductions in spending in labor-intensive industries are larger in magnitude and statistically significant at conventional levels compared to non-labor-intensive spending. Thus, we will focus on the results for these industries. Initially looking at all demographics, a reduction of $1 million in procurement spending for labor-intensive industries leads to an increase in the number of reported individuals receiving SNAP benefits by approximately 13. That is, for every $75,000 reduction in government spending, an additional individual reports as receiving transfers through SNAP. Unlike the case of unemployment, the increase in the number of people receiving SNAP transfers with fiscal consolidation is mostly among individuals under 20 years old and between 18 and 40 years old (approximately 5 and 7 more individuals, respectively, for every $1 million reduced). Nevertheless, our findings indicate that the population without higher education is responsible for the majority of this increase. The estimated coefficient is more than 10 times higher than that of the group with a bachelor's degree or higher. Although the coefficients show greater magnitude for whites and Hispanics, the overall result of the increased participation in the program does not seem to be driven by any particular race, while the breakdown by marital status appears to be largely due to married people. This could in part be due to institutional features of the transfer programs, such as the different requirements based on family structure (e.g. number of children) to receive aid (Schanzenbach 2019; Schanzenbach 2023).

Table 4: Outcomes for Quantities of Individuals by Demographic Groups

Social Outcomes	Unemployed (1)		Disabled (2)		SNAP (3)	
Demographic Groups	Labor Intensive	Non-Labor Intensive	Labor Intensive	Non-Labor Intensive	Labor Intensive	Non-Labor Intensive
All	-6.42*** (1.76)	-4.02 (2.50)	0.605 (1.81)	-2.36 (2.92)	-13.3** (4.98)	2.51 (5.74)
Age						
Age Under 20	0.045 (0.144)	-0.088 (0.209)	-0.307 (0.284)	-0.569 (0.574)	-5.27** (1.86)	0.820 (2.42)
Age 18 - 40	-3.78*** (1.04)	-1.50 (1.46)	-0.114 (0.561)	-0.761 (0.954)	-6.80*** (1.94)	1.23 (2.21)
Age 41 - 61	-2.30*** (0.665)	-1.80 (0.987)	0.279 (0.875)	-1.66 (0.950)	-1.32 (1.08)	0.216 (1.31)
Age 62	-0.390 (0.218)	-0.626 (0.322)	0.747 (0.883)	0.636 (1.52)	0.050 (0.505)	0.251 (0.571)
Education						
Bachelors or Higher	-0.961* (0.373)	-0.597 (0.473)	-0.404 (0.399)	0.133 (0.684)	-1.18** (0.416)	0.532 (0.470)
No Bachelors	-5.46*** (1.57)	-3.42 (2.15)	1.01 (1.65)	-2.49 (2.54)	-12.2** (4.65)	1.98 (5.51)
Race						
White	-6.11*** (1.45)	-1.66 (1.60)	1.11 (1.72)	0.359 (2.77)	-5.78* (2.44)	1.76 (3.38)
Black	1.31* (0.592)	-0.926 (1.07)	-0.502 (0.618)	-2.02 (1.09)	-2.13 (1.98)	-2.69 (1.62)
Hispanic	-1.68* (0.660)	-1.05 (0.772)	0.400 (0.515)	-1.11 (0.869)	-5.23* (2.03)	2.27 (1.98)
Other	0.063 (0.235)	-0.384 (0.368)	-0.403 (0.397)	0.415 (0.545)	-0.211 (0.915)	1.17 (1.68)
Marital Status						
Married	-3.54*** (0.908)	-2.60. (1.36)	1.76 (1.57)	-1.34 (1.34)	-7.85*** (2.02)	1.08 (3.05)
Not Married	-2.88* (1.23)	-1.42 (1.31)	-1.15 (1.08)	-1.02 (1.87)	-5.49 (3.42)	1.43 (3.45)
Observations	1,910		1,910		1,910	
F-test (1st Stage)	1,057.6	98.477	1,057.6	98.477	1,057.6	98.477

Note: This table shows separate regression results of quantities (rates) of individuals by demographic groups using data from the American Community Survey. Each model includes both variables for labor-intensive and non-labor-intensive spending. Each model includes year and CBSA fixed effects and the standard errors in parenthesis are clustered at the CBSA level. Kleibergen-Paap LM and the corresponding p-value the heteroskedasticity-robust test for exogeneity of the instrument. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

6 Conclusion

This study examines the impact of fiscal consolidation on local labor markets and the social safety net in the United States. Leveraging a shift-share instrumental variables approach and employing population-weighted regression, we estimate the impact of reductions in federal procurement spending on various labor market and social indicators at the CBSA level. Our results shed light on labor market adjustments due to declines in spending. They reveal a nuanced pattern of employment changes. In particular, we show that a reduction of $1 million in procurement spending leads to a decrease of approximately 8.4 employed individuals, which is split evenly between reductions in labor force participation and increases in unemployment.

The analysis also uncovers heterogeneity in the effects of procurement spending on labor market outcomes. Specifically, we show that labor-intensive industries bear a larger adjustment to economic shocks, leading to significant job losses and individuals exiting local labor markets. In short, we find that a $1 million reduction in procurement spending is associated with 11 job losses in these industries, with roughly a 6.4 reduction in the local labor force. On the other hand, non-labor-intensive industries exhibit relatively smaller impacts on employment and labor force participation. This evidence enriches our understanding of the differential effects of spending reductions on different sectors, further building upon Komarek et al. (2022).

We also explore the distributional effects of spending reductions by demographic group. Our results reveal that unemployment increases predominantly affect individuals between 18 and 40 years old, and particularly individuals without a bachelor's degree. In contrast, disability remains largely unaffected by fiscal consolidation. Moreover, SNAP participation rises for individuals in several demographic groups such as age below 40, individuals without higher education, and married individuals.

Our results help provide context for the magnitude of the BCA-induced economic shock on the U.S. economy. Between 2011 and 2015 total procurement spending declined approximately $47 billion. Our estimates suggest that aggregating the CBSA impacts resulted in almost 400,000 fewer individuals employed and reduced the labor force by approximately 180,000. Additionally, over 200,000 individuals experienced unemployment and self-reportedly participated in the SNAP program.

Overall, this research contributes to the literature by exploring the interplay between fiscal policies and the social welfare system. We provide empirical evidence on the labor market and social welfare implications of government procurement spending during periods of fiscal consolidation. Our results underscore the importance of considering sector-specific and demographic characteristics when formulating and implementing fiscal policies. As government spending decisions reverberate across the economy, understanding these complex interactions can inform policymakers in devising measures that foster both economic stability and social well-being.

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A Appendices

A.1 Political power and local spending cuts

As discussed in Komarek et al. (2022), a potential concern regarding our identification strategy is that CBSAs with greater political power or influence may systematically protect their constituents from local spending cuts. This could occur when politicians exert pressure on agencies to prevent cuts to industries or firms in their districts or states. If the level of political power is correlated with the development of the local labor market, it would introduce non-random assignment of industry shocks, thereby biasing our results.

To address this concern, we analyze multiple dimensions of "political power" during the 112th Congress (2011-2013) and examine their correlation with the distribution of sequester spending shocks. By investigating whether a CBSA's political power at the time the BCA was drafted and approved is unrelated to subsequent sector shocks, we can determine if there is any observable correlation in the data. If no significant correlation is found, it would suggest that the political power exerted during the BCA drafting and approval process does not influence the occurrence of sector shocks in the CBSAs.

Table A.1 display the regression results of each of the four political power measures against the observed average shift-share shock of the CBSAs. The presence of a significant positive or negative correlation could indicate that certain CBSAs managed to evade sequester cuts due to their political influence. If our proxy variables effectively capture the concept of "political power" in CBSAs, these results offer evidence that CBSAs did not systematically escape spending shocks.

Table A.1: CBSA House of Representatives Political Power Pre-BCA and Sequester Shocks

	Political Ideology (1)	Seniority (years) (2)	Leadership Representation (3)	Powerful Committees (4)
Constant	0.0394**	3.934***	0.1117***	0.3088***
	(0.0199)	(0.3293)	(0.0240)	(0.0319)
Average Shift-Share Shock	-0.0122	-1.943	0.0399	0.0841
	(0.1041)	(1.720)	(0.1253)	(0.1668)
Observations	382	382	382	382
R^2	0.00004	0.00334	0.00027	0.00067
Adjusted R^2	-0.00260	0.00072	-0.00236	-0.00196

Note: Each column represents a cross-section regression with 382 CBSAs with the average shift-share shock from 2011-2015 and measures of CBSA "political power" 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

A.2 Negative shocks results

Table A.2: Negative Shock Regression Results: Labor Market from BLS LAUS Data

Panel A: Total Procurement Spending			
	Employed	Labor Force	Unemployed
	(1)	(2)	(3)
Spending (million \$s)	9.186***	4.666***	-4.520***
	(1.190)	(1.033)	(0.7228)
Fixed-effects			
cbsa	Yes	Yes	Yes
year	Yes	Yes	Yes
Observations	1,715	1,715	1,715
F-test (1st stage)	1,480.6	1,480.6	1,480.6
Kleibergen-Paap	33.279	33.279	33.279
Kleibergen-Paap, p-value	7.98×10^{-9}	7.98×10^{-9}	7.98×10^{-9}

Panel B: Spending by Labor Intensity			
	Employed	Labor Force	Unemployed
	(1)	(2)	(3)
Labor Intensive Spending (million \$s)	8.285***	4.538***	-3.747***
	(2.123)	(1.583)	(1.188)
Non-Labor Intensive Spending (million \$s)	10.04***	4.788*	-5.256***
	(3.011)	(2.829)	(1.631)
Fixed-effects			
cbsa	Yes	Yes	Yes
year	Yes	Yes	Yes
Observations	1,715	1,715	1,715
F-test (1st stage), Labor Intensive Spending (million \$s)	1,124.2	1,124.2	1,124.2
F-test (1st stage), Non-Labor Intensive Spending (million \$s)	342.04	342.04	342.04

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 and the corresponding p-value the heteroskedasticity-robust test for exogeneity of the instrument. Regressions using only negative shocks include CBSAs that experience negative declines in federal procurement spending on average over the 2011-2015 sample period.