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Does federal contracting spur development? Federal contracts, income, output, and jobs in US cities

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Federal contracts, income, output, and jobs in US cities

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Abstract

Government contracts are frequently courted by firms and governments alike as a solution to generate more jobs, income, and economic growth. However, the development impact of government contracts remains controversial. This paper uses georeferenced data on United States (US) federal contracts, distinguishing between the location of the recipient and the location of performance, for the years 2005-2014 in order to assess the extent to which federal government contracting has contributed to job and wealth generation and economic growth in metropolitan areas of the US. The results of the analysis show that individuals living in cities with a higher share of contract spending per capita witnessed improvements in employment. Aggregate GDP per capita also rose in cities hosting the companies receiving the contracts. However, the effects – once reverse causality and spurious trends are controlled for using a fine-scale fixed effect strategy and instrumentation – are very small, raising reasonable questions about the viability of federal contracting as a vehicle for economic development.

Keywords: federal contracting; government spending; jobs; wages; economic growth; urban development

JEL: R11; R38; O23; E62; R58

1. Introduction

In the fiscal year 2016, the US federal government awarded a total of $409,229,751,215 in contracts above a $3,000 threshold. These contracts financed essential public goods and services required for the economy to operate and for society to function. They were granted to firms located all over the US and were expected to contribute to create jobs and generate economic dynamism, both in the places where the firms were located as well as in those where the contracts were executed.

The jobs, income, and economic growth presumably associated with government contracting have meant that federal contracts have been actively courted as a basic instrument for economic development not only by firms, but also by local decision-makers. At the federal level, President Obama's economic advisors also regarded federal contracting as a development tool. A multiplier of 1.6 dollars was expected for every dollar of government spending related to the 2009 fiscal stimulus package (Economist, 2009; Romer, 2009). Yet, the economic development impact of US federal contracts is still shrouded in mystery. The reasons for this are twofold. First, the effect of federal contracts on the development of cities and states in the US has attracted relatively little interest despite the volume of funds disbursed. The impact of contracts on wages and jobs has drawn some academic curiosity, but the aggregate growth effects of federal government contracts have deserved much less attention. Second, the results of research examining the geographical impact of federal intervention – through grants, subsidies and, to a much lesser extent, contracts – are far from homogenous. In terms of job generation, although an investment of $35,000 per new job created is frequently quoted (Ramey, 2011), the figure generally ranges from $25,000 to $125,000 in studies using spatial approaches (Shoag, 2013; Wilson 2012). Similarly, multipliers range between 0.8 and 1.5 in macro and time series studies (Ramey, 2011). The variation is greater – from 0.5 to 2.4 – in analyses that consider spatial differences (Nakamura and Steinsson, 2014; Fishback and Kachanovskaya, 2010; Clemens and Miran, 2012).

This paper delves into the development impact of federal contracts on urban areas in the US between 2005 and 2014. After mapping the geographical location of federal contract spending, the aim of the paper is to unveil geographical patterns with the objective of assessing the extent to which federal government contracting contributes to aggregate job creation and economic growth across 300 to 500 urban areas – depending on the analysis conducted – of the US. Using cities as the level of analysis allows us to control in detail for confounding explanations and to deal with potential reverse causality in the relationship between federal contract reception, on the one hand, and jobs and production, on the other. We also distinguish between where the firm benefitting from the contract is located (recipient location) and where the activity related to the contract takes place (location of the activity).

The results of the analysis prove that federal contracting is a non-negligible driver of urban growth in the US. A dollar in a federal contract generates 1.4 dollars in additional GDP. Cities with a higher share of federal contract spending per capita have experienced greater growth than those with a lower share. Its citizens have been the beneficiaries of improvements in the amount of economic activity and, to a much lesser extent, employment. The effects of federal contracting have, however, been lower than anticipated and there are differences between the impact in the cities where the recipient firms are located and those where the contract is executed. Aggregate GDP changes are fundamentally felt in the cities where the recipient firms are established and there is hardly an impact at the location of execution. By contrast, those places where the contract is executed benefit more in terms of employment generation.

Overall, the development effects of federal contracting – once reverse causality and spurious trends are controlled for – are small, with job generation generally lower than highlighted in previous literature and almost inexistent effects on wages. Moreover, the economic effects are spatially localized, generating close-range territorial spillovers that tend to fade away rather quickly, with no measurable effects two years after the contract is awarded. These results raise reasonable questions about the practicality of federal contracting as a viable and sustainable tool for urban economic development.

In order to reach these conclusions, the paper adopts the following structure. This introduction is followed by a review of the extant literature in the next section. Section 3 looks at the data, while section 4 focuses on the methodology. The results of the analysis are presented in section 5, which is followed by the conclusions.

2. Public intervention and wages, jobs, and output

Government contracting has become increasingly regarded as a tool for economic development, especially since the financial crisis of 2008. Not only firms, but also local politicians and decision-makers lobby central or federal governments in favor of their constituencies, pursuing public contracts as a means to breed economic activity and development. Given the lofty volumes of public expenditure, the spending impacts have drawn attention at the national level, but hardly at the regional or urban level – especially relative to the focus on grants and subsidies. This is surprising for three reasons: federal contracts are often considered vehicles for local development; there is a lack of consensus on their effectiveness; and the urban scale allows novel methods to tackle the question of causality plaguing such studies.

Related literature looks at various outcomes of public expenditure. Most analyses have been concerned with the wage and job impact of public contracting. Studies of the effect of government contracting on output at the local level have been less frequent, while those dealing with the effect on aggregate growth are the least common.

2.1. ***Wages and jobs***

Among the studies dealing with wages and jobs, the norm has been that public intervention generally leads to improvements on both counts. The dimension of this impact differs widely, however, depending on the study, its location, and the method used. Increases in wages are observed by, among others, Greenstone et al. (2010) as well as by Ham et al. (2011), Hanson and Rohlin (2013), and Busso, Gregory and Kline (2013) when considering enterprise and empowerment zones. For Greenstone et al. (2010) areas that receive greater public investments and subsidies perform better in terms of not only wages, but also employment and TFP, than those areas that failed to secure the same investments. Similar increases in wages and employment are reported by Hanson (2009), Busso, Gregory and Kline (2013), and Hanson and Rohlin (2013) as a consequence of the tax incentives and grants to firms located in enterprise or empowerment zones, relative to those firms with similar characteristics outside the zone.

However, not everyone finds a positive link between public support and increases in wages. This is the case of Criscuolo et al. (2012), who estimate that UK Regional Selective Assistance (RSA) grants, while a driver of employment generation, have not raised the wages of workers.

The empirical evidence of the impact of public contracting, subsidies, and grants on employment is more extensive. The results of research focusing on jobs tend to be generally positive, with most studies underscoring that public intervention, regardless of the guise, leads to employment generation. In the case of the UK, RSA grants have been found to be behind the creation of firm-level jobs (Criscuolo et al., 2012). Similarly, in the case of Italy, government subsidies conditional on hiring have been shown to produce much larger increases in employment than output (Bernini and Pellegrini, 2011).

More specific North American studies reach similar conclusions. This is the case of Suárez Serrato and Wingender (2016), who use revisions of the population estimates for US counties as a source of extra public investment and report significant increases in job creation linked to federal spending. Wilson (2012) instruments spending marked as fiscal stimulus from the American Recovery and Reinvestment (ARR) act with the spending predicted according to allocation formulas and discovers that the ARR has had a significant impact on job creation. Feyrer and Sacerdote (2011) also examine the impact of ARR spending (including grants and loans), both at the state and county level over a period of 20 months. Their analysis involves regressing employment to population ratios on spending per capita and instrumenting spending with the years served by a congressional delegation. They argue that longer periods of service facilitate access to leading positions. The results also emphasize a positive impact of ARR government intervention on job generation. Similar results were found by Chodorow-Reich et al. (2012), after instrumenting Medicaid spending.

However, not all studies report a positive link between public intervention and employment generation. That is the case of the work conducted by Kline and Moretti (2014) on federal government investment in public infrastructure by the Tennessee Valley Authority. They inform that public intervention in the treated areas produced a displacement in employment, as the new jobs created in the subsidized areas were at the expense of jobs in neighboring areas that were initially considered for public investment, but did not receive program funding. Even more damning is the analysis by Dupor (2015) of the territorial allocation of military spending. He finds a close to zero net effect of this sort of public investment, despite large employment reallocations.

Even more controversial than the net effect on jobs is the cost of each new job associated to public subsidies and/or investments. In some cases, the returns of public investments and subsidies are considered to be extremely high. For Criscuolo et al (2012) a mere 4,900 GBP of public investment – on top of facilitating new firm entry – sufficed to create one additional job in 2010. Somewhat higher investment and/or subsidy levels are reported by Shoag (2013) and Chodorow-Reich et al. (2012). Shoag (2013) uses US state-wide pension plans returns during the crisis and how they affect state government spending. The estimated public expenditure cost per job is around $23,000, with an additional effect of job reallocation on reducing unemployment. Chodorow-Reich et al. (2012) put the figure $26,000 of Medicaid expenditure to deliver a new job. This is more effective than the $31,000 they calculate for private-sector investment. The overall figure rises to $30,000 in Suárez Serrato and Wingender (2016), while Wilson (2012) puts the cost per job at $125,000. The widest range of costs is provided by Feyrer and Sacerdote (2011), who, when analyzing ARRA spending at the state and county level, put the cost per additional job between $43,000 and $100,000 at the state level, and between $500,000 and $3.3 million at county level.

2.2. ***Output***

In contrast to the amount of research conducted on wages and employment, the focus on output and economic growth is comparatively small. The difficulties in making the transfer from the micro- to the macro-scale imply that only a few papers have ventured into the link between public investments of and economic growth. In addition, much of this work has been conducted at country level, as has been the case of Kraay (2012). Kraay (2012) looks at government spending financed by World Bank loans in 29 developing countries and argues that, as loan schemes are determined in advance, they can be considered exogenous to output shocks. The results of his work indicate that World Bank investment yields a multiplier of around 50 cents per dollar spent.

Subnational analyses involving the US have been conducted by Fishback and Kachanovskaya (2010), Nakamura and Steinsson (2014), Suárez Serrato and Wingender (2016), and Shoag (2013). Nakamura and Steinsson (2014) explain state-level GDP per capita on the basis state-level military spending, using quarterly data between 1966 and 2006 and instrument military spending using a shift-share approach. They report a multiplier of military spending on output of 1.4 for states (reaching 1.8 for census regions), which rises to 2.4 with the shift-share instrument. Shoag (2013) estimates the multiplier also at 1.4, exploiting state-level variation in pension fund returns. The multiplier rises to between 1.7 and 2 in the work of Suárez Serrato and Wingender (2016), who exploit revisions of population estimates as a source of exogenous variation in federal expenditure. Lower figures are generally provided by Fishback and Kachanovskaya (2010), when analyzing the New Deal grants at the state-level during the 1930s and 1940s – with the multiplier dropping to 1.1. Their estimates of the effects of government purchases (excluding direct transfers) go up to 1.8 dollars, although the confidence intervals are wide. The estimate of the multiplier probably is also found to depend on whether the spending is associated with an increase in the local tax burden. Clemens and Miran (2012), for example, use fiscal budgeting to identify multipliers in a balanced-budget situation. They report multipliers well below 1 – around 0.4 – when estimating the impact of debt-financed spending at state level.

2.3. **Going beyond past research**

Overall, past research highlights that public investment generally has a positive impact on wages and employment and creates multiplier effects that enhance GDP. In particular, in the case of the US and despite considerable diversity in the results contingent on the cases and methods adopted, public investment and subsidies are deemed to not only increase wages and generate employment at costs that range between $25,000 and $125,000, but also to produce multiplier effects that benefit society as a whole. With most multipliers between 1.4 and 1.6 the consensus is not far off from the 1.6 multiplier estimated by President Obama's economic team for federal spending included in the 2009 fiscal stimulus package (Economist, 2009).

However, the consensus on the multiplier and cost estimations does not imply that the issue is settled. As indicated by Ramey (2011), there are a number of other issues that still need to be dealt with. First among them is the causality question between output and spending. While public spending seems to indeed generate greater output, the reverse is also true: increases in output may result in higher levels of public spending.[1] One popular solution to address this issue has been to resort to spending on military build-ups, as external wars are argued to be exogenous to output (Barro, 1981; Ramey, 2011). Nevertheless, this sort of instrument is not

[1] Although, especially in cross-sectional variation, it may be argued that the federal government could also be channelling larger expenditure per head to lagging areas.

particularly useful in trying to distinguish the differential impact of public investment and subsidies in different parts of the US.

A second caveat is that the great majority of past research has not been concerned with differences in impact across space. It is not only that the impact on wages and employment as well as the multiplier may vary from one city to another or from one state to another, but also that, when there is considerable cross-location variation, the multiplier itself becomes hard to identify. As Ramey (2011: 681) puts it:

> *"suppose the economy behaves according to a simple traditional Keynesian model. In this case, if the government transfers $1 to Mississippi and finances it by increasing lump-sum taxes across all states, the true aggregate multiplier is 0, since the taxes and transfers cancel in the aggregate. However, if we run a panel regression with time fixed effects (which net out the economy-wide rise in tax liabilities), we will estimate a multiplier of mpc/(1-mpc), where mpc is the marginal propensity to consume. If the marginal propensity to consume were 0.6, then we would estimate a multiplier of 1.5 at the state level, even though the aggregate multiplier for this experiment is 0."*

A contribution of this paper is to consider the impact of federal contracts in geographical detail. The impact of contract expenditure is pinpointed at a fine geographical scale, while financing decisions are made at the national level. Consequently, we should expect few Ricardian effects: citizens of a city receiving a federal contract should not anticipate substantial changes in federal tax rates, as a result of awarding an individual contract to a firm in their city. In that sense, our paper identifies an expenditure multiplier on purely non-balanced budgets. This is in contrast to, for instance, Clemens and Miran (2012), who report an expenditure multiplier that is associated with offsetting tax increase, The advantage of our approach is that it isolates pure expenditure effects irrespective of tax distortions; the disadvantage is that our results need to be benchmarked against a separately estimated cost of public funds to assess the efficiency of federal contract expenditure.

Past research has also failed to distinguish between the place where the contract is allocated to (recipient location) and that where the expenditure is effectively conducted. As the beneficiary firms are often located in places far away from where the contract is implemented, this difference can massively affect our perception of the returns of public expenditure, especially in less developed regions, where the mismatch between the recipient location and the location of the activity is likely to be greatest. Nor is there any evidence of the geographical reach of the impact of federal contracting.

In this paper, we aim to address the shortcomings of previous research by particularly stressing the geographical and time dimension of the impact of federal contracting across urban areas in the US. We assess whether the multiplier of expenditure on federal contracts is not only similar – or higher or lower, if at all – to what has been highlighted in the literature, but whether its impact differs between the place of the location of the recipient firm and that of the location of the execution. We also revisit the question of how public expenditure affects jobs and wages and focus on whether the impact of public expenditure increases over space and time or, by contrast, tends to fade with distance and after a few years after the initial investment.

3. Data

3.1. *The urban geography of federal contracting*

In order to analyze the impact of public expenditure on the economic development of cities in the US, we have gathered a dataset containing information about every single federal contract above $3,000 for the period between 2005 and 2014. The details on all federal contracts are published under the Federal Funding Accountability and Transparency Act (FFATA) of 2006. The act requires that information about all federal contracts worth more than $3,000 is made publicly available. A wealth of information is provided per contract: what contractor was awarded the contract, its value, the location of performance and industrial classification, and procurement conditions, among others. The data stem from the Bureau of Fiscal Services (Dept. of Treasury).

The focus of our research is exclusively on contracted work involving federal spending. Federal contracting represents a fair share of overall federal spending and amounted to around $400 billion annually between 2004 and 2015 – or close to $4.82 trillion during the whole period of analysis. In 2014 alone $446 billion were allocated to federal contracts above the $3,000 threshold. This represents $1,375 for every American and ranks only behind grants (603$ billion) and other financial assistance (1.7$ trillion) – which includes Medicare, unemployment benefits, and the like – as the third most important category of federal spending. The data do not include federal salaries.

Given our interest on the urban impact of federal contracting, each individual contract is allocated to a specific geographical location.[2] Individual contracts normally include information about the location of performance – the place where the contracted work is performed – as well as the address of the contract recipient. Often both locations coincide. In many cases, however, public services and products are produced at a different location from that where the contracting firm is based. Not all contracts are georeferenced. 85% of the total value of federal contracts can be positioned geographically when considering the location of performance, while this percentage rises to 94% when looking at the address of the contract recipient. In order to match contract data to the other variables used in the analysis, the contract information is aggregated at the US metropolitan and micropolitan statistical area level. Our spending data identify contract recipients in 920 core-based statistical areas. However, we use fewer urban areas – or cities, as we will refer to them – across the 48 continental US states, as the match with other sample is imperfect: at most 373 cities in the GDP-based regression and at most 655 cities in wage and employment-based regressions.. These cities concentrate 80% of all identified federal contracting in the US, the rest going to rural areas and/or to the states of Alaska and Hawaii.

The federal contracts identified imply an urban bias in federal contracting – also reproducing the concentration of US firms in cities – as they represent an average expenditure per annum of $1,260 per capita at the location of performance and $1,500 at the location of the recipient (see also Table 1) – or 91.6 and 109.1% respectively or all federal contracting per capita.

The production of goods and services through federal contracting is, however, uneven across US cities. Figure 1 maps the location of federal contract spending per capita in 2014, taking into account the location of performance.

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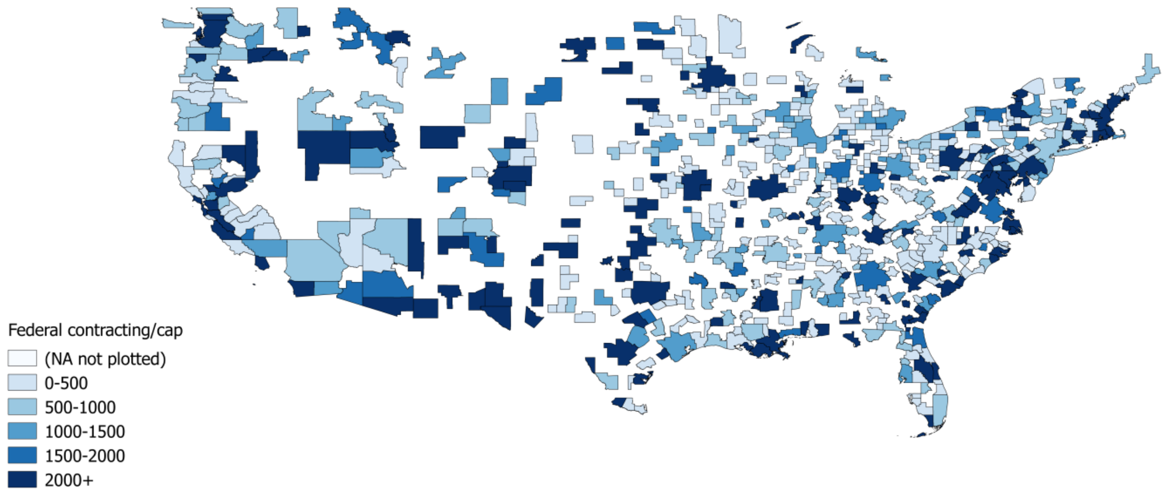
[2] The data are freely available. Individual federal contracts were processed using a high-performance computing cluster, as the source files are large (around 70Gb). The codes and processed datasets are available from the authors' websites.

Figure 1. Contract spending per capita 2014 (location of performance)



Source: Own elaboration with Bureau of Fiscal Services (Dept. of Treasury) data.

Federal contracting was heavily concentrated in a small number of cities and regions. Some large cities, such as Washington, Boston, Atlanta, St Louis, Denver, Salt Lake City, Cincinnati, San Jose, or Sacramento were among the few urban areas that attracted more than $2,000 of expenditure per capita in 2014 (Figure 1). That many of the cities with the greatest concentration of federal contracting where either the federal capital – Washington, D.C. – or state capitals, such as Boston (Ma.), Concord (N.H.), Hartford (Ct.), Providence (R.I.), Richmond (Va.), Atlanta (Ga.), Denver (Co.), Salt Lake City (Ut.), or Sacramento (Ca.) speaks volumes about the connection between federal contracting and political power. By contrast, the largest American agglomerations, such as New York, Los Angeles, and Chicago benefitted relatively little from federal contracts. Expenditure in the greater areas of New York and Chicago, at $480 and $590 per capita respectively in 2014, was well under the national average. Similarly many medium-sized and often declining cities along the Mississippi valley, the Rust- or Snowbelts, the mid-West, or the California Central Valley were also among the least favored by federal contracting (Figure 1). Expenditure through contracting was high in cities with a strong military tradition, such as Norfolk, Va., San Diego, Ca., or Tacoma, Wa. or Tucson, Az. (with a respective expenditure per capita of $1,800; $2,520; $2,050; and $3,200).

The distribution of contract expenditure across cities also differs by type of spending. Figure 2 shows federal contract expenditure in infrastructure (Figure 2a) – which includes among other things, water and sewer structures, power infrastructure, highways, streets and other civil engineering – and manufacturing (Figure 2b).

Figure 2. Infrastructure and manufacturing contract expenditure per capita (location of performance)

Figure 2a

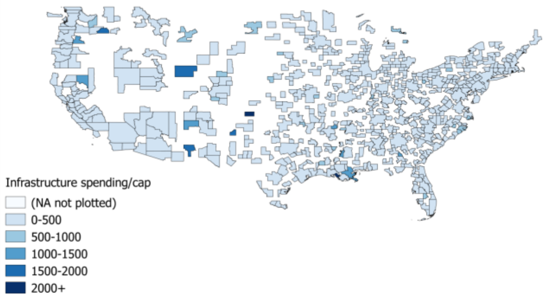
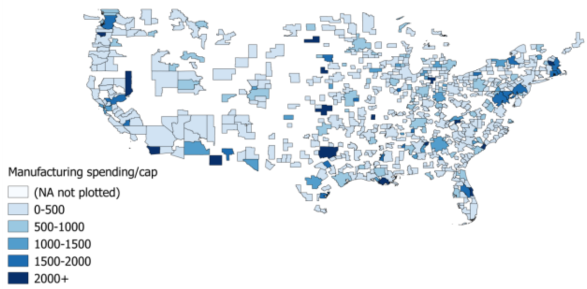


Figure 2b

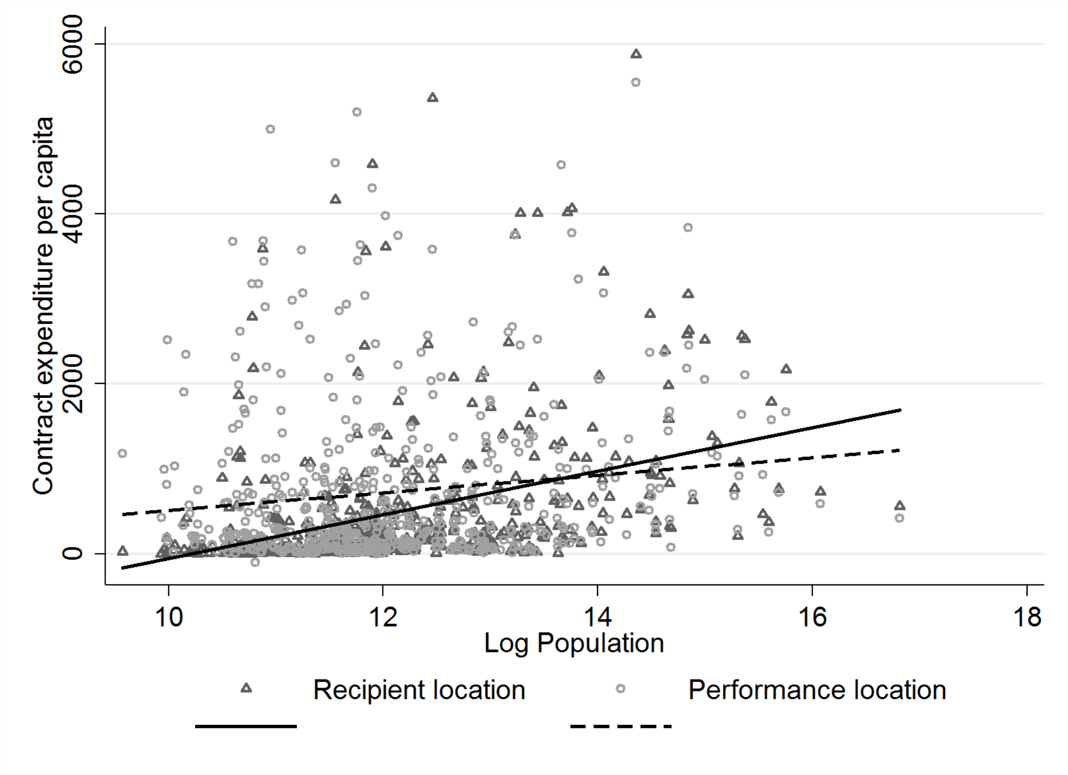


Source: Own elaboration with Bureau of Fiscal Services (Dept. of Treasury) data.

Federal contracting in infrastructure in 2014 was low across urban America (less than $250 per capita in more than 90% of continental US cities), with some concentrations in cities, such as New Orleans, that are still recovering from natural disasters (Figure 2a). Expenditure in manufacturing more closely matched overall federal contracting expenditure, with concentrations in cities with a strong military tradition, such as Norfolk (Va.), San Diego (Ca.), or Tucson (Az.) (Figure 2b).

The correlation between federal contract expenditure and city size is confirmed by Figure 3, which plots expenditure in federal contracting per capita in 2014 against city size, proxied by the log of the population. Both the allocation according to place of performance and place of recipient display that, as a general rule, the larger the city, the greater the federal spending in contracts. The regression line is marginally steeper when the recipient location rather than the location of performance is considered. The slope of the fit is around 80 for place-of-performance contracts (elasticity around 0.2) and 340 for place-of-recipient contracts (elasticity around 0.6), meaning that the firms winning tenders for federal contracts are more concentrated than the actual implementation of public goods and services. This reinforces the idea that some contracted activities in smaller cities are executed by contract recipients from larger cities. The relationship between city size and federal contracting is, nevertheless, highly imperfect, as, especially in the case of place of performance, many of the main beneficiaries are medium-sized cities (Figure 3).

Figure 3. Contract expenditure and city size (2014).



Source: Own elaboration with Bureau of Fiscal Services (Dept. of Treasury) data.

3.2. *Matching data*

In the analysis federal contract data are combined with other urban data for the micropolitan and metropolitan areas of the 48 continental states of the US. The other data include city population, from the US Census Bureau; output and GDP, from the US Bureau of Economic Analysis; and wages and employment, from the American Community Surveys. Table 1 reports both the unweighted city averages as well as weighted by population in 2014, for consistency with aggregate statistics.

Table 1. Descriptive statistics (2014).

Variable	Mean (unweighted)	Standard deviation	Mean (pop. weighted)
Dollars contracted (recipient)	$4.49 bln	$1344 bln	$6.08 bln
Dollars contracted (performance)	$5.34 bln	$824 bln	$4.41 bln
Dollars contracted/cap (recipient)	$703	$2,655	$1,502
Dollars contracted/cap (performance)	$1,011	$1,855	$1,261
GDP per capita	$41,922	$14,595	$52,177
Employment rate	0.92	0.03	0.92
Wage	$24,750	$6,831	$30,724
Hours worked per week (if employed)	39.15	1.11	39.17
Weeks worked per year (if employed)	45.61	0.93	46.06

4. Methodology

To make the analysis comparable with previous research, we use the variation in the intensity of federal contracting across US cities to explain differences in overall production, employment, and wages. In particular, we draw on the literature on fiscal multipliers to measure the impact on economic output generated by any additional dollar allocated to fiscal contracting.

The mere statistical association between output and federal contract expenditure does not necessarily help to understand the economic effects of federal contracting. Plausibly, the allocation of contracts across cities is not random: the economic circumstances of a city are likely to be a fundamental determinant in deciding where to allocate federal contracts, while – as seen in Figures 1 and 2 – political motivations cannot be discarded as factors influencing which cities receive a greater share of federal contracting. Equally, there may be unobserved variables that explain the two: if a large firm goes bankrupt, its demise decreases production in the city where it was located, possibly reducing the overall amount of federal contract money secured by the city. In order to deal with this issue, we pursue two different strategies: a) shift-share instrumentation of local contract expenditure and b) controlling for the initial situation in each city as well as unobserved shocks by means of city and state-year fixed effects. A standard multiplier equation following Nakamura and Steinsson (2014) at the city-year level is implemented in order to disclose the effects of federal contracting on output. It adopts the following form:

$$\left(\frac{GDP}{cap}\right)_{c,t} = \beta\left(\frac{Spending}{cap}\right)_{c,t} + \alpha_c + \alpha_{s,t} + \varepsilon_{c,t},$$

Where subscripts c, s, and t denote metro- or micropolitan area, state, and year, respectively. We run the weighted (instrumental) least squares version of this regression, using the population measures as weights, to recover the nationally applicable estimates.

Our analysis addresses the concern of causal interpretation through the use of an instrumental variable approach. We resort to a shift-share (or Bartik) approach, which captures the exogeneity of the aggregate spending in individual cities by projecting national contracting growth rates in different industries on the initial allocation of contracts across cities in those industries. We exploit the variation across cities, assuming that national spending decisions are not driven by shocks to specific cities. Using a related strategy, Nakamura and Steinsson (2014) argue that differences in the allocation of military spending between states is exogenous, even if the aggregate spending is endogenous. By construction, the instrument is still sensitive to changes in national spending and the initial spending. However, given the scope of the dataset, we can exploit variation that is both unrelated to patterns in the initial contract allocation across cities; and unrelated to national expenditure decisions. These are controlled for in the fixed effect strategy.

Two different sets of fixed effects are included. First, the city-level fixed effects α_c control for any time-invariant selection processes, such as the location of the city or other forms of spatial heterogeneity. By absorbing long-run contracting patterns for each city, the instrument is not sensitive to time-invariant city specialization patterns, such as a large automobile industry or high-tech tradition.

The second set of fixed effects, $\alpha_{s,t}$, controls for shocks at the state-year level. This set of fixed effects absorbs unobserved shocks among nearby groups of cities. State-year fixed effects control for differences in regional business cycles (Domazlicky, 1980; Hess and Shin, 1998; Beraja, Hurst, and Ospina 2016). They should also account for political selection, particularly for pork-barrel practices, whereby elected representatives in Washington secure federal contracts for their home city or state (Larcinese et al., 2006; Cohen, Coval and Malloy, 2011). With 300-500 cities included in the sample (depending on what variables enter the regression), the analysis can more effectively rule out endogeneity related to political representation of any specific city than national (time) fixed effects can.

4.1. ***Instrumentation***

The shift-share instrument is constructed as follows. We first predict spending in a particular industry in a city, by projecting the national contracting changes in that industry on a city's initial allocation of spending. We use 1-digit NAICS-coded industries as categories. *G* denotes the amount of spending; *i* the metro- or micropolitan area; *US* the national aggregate; *c* the industry; and *t* the year. The projected spending is:

$$\hat{G}_{i,c,t} = G_{i,c,2005} \frac{G_{US,c,t}}{G_{US,c,2005}}.$$

This term is the product of the amount of contract dollars a metro-(micro-)politan area was awarded for industry *i* in 2005; and the national growth rate of contracting in that industry.

Aggregating the projected spending by industry yields a prediction of the aggregate spending by metropolitan area:

$$\hat{G}_{c,t} = \sum_{i} \hat{G}_{i,c,t}.$$

This projection still contains the national changes, which are plausibly not exogenous. However, given the identification of subnational variation, national shocks to federal spending can be controlled for using fixed effects. Equally, the long-run industrial pattern of a city may be endogenous, but variation is eliminated using MSA fixed effects. The instrument thus effectively exploits the differential sensitivity of cities to contracting in each industry.

5. Results

5.1. ***Effects on output***

Our baseline regression has the objective of exposing the effects of federal contracting on GDP across cities in the US. In particular and in contrast to previous literature, we aim to distinguish if differences between the location of recipient firms and where the provision of public goods and services effectively took place affect the economic impact of contracts. Federal contract spending in a given city is lagged by one year, as we expect that the federal effort will have a greater impact on output the year after the expenditure occurs. In Appendix Table A1, we provide a set of regressions with different lag structures that suggests that the lagged contract spending yields the best fit.

Table 2 displays the results of the baseline regression, distinguishing between the recipient location (Table 2a) and the location where the activity took place (Table 2b). Column 1 reports the simple OLS correlation, while columns 2 through 5 include instrumental variable (IV) analysis – inserting in the different columns metropolitan area and state-year fixed effects – as a means to interpret the causal relation between federal contracting and GDP per capita. Finally, column 6 estimates the connection between the allocation of funds to the location of the recipient and to the location of activity, in order to discriminate between both.

The results in column 1 indicate that GDP per capita in a given US city became 1.7 dollars higher for every additional dollar awarded to a firm located in that city (recipient location), and 1.8 dollars higher for every additional dollar eventually spent in the city (location of the activity). This positive and significant connection between federal contracting and increases in GDP per head is robust to the introduction of the shift-share instrument for contract expenditure as well as city-level and state-year level fixed effects (Table 2, Columns 2-5). In all cases the coefficient is significantly different from zero, while the Cragg-Donald F-statistic for weak identification suggests no weak instrument problem. Across the whole range of results, instrumenting increases the estimated coefficient substantially. The coefficients remain always positive and significant, with exception of Column 4 in Table 2a. This is to be expected, as the federal government tends to give priority – at least in relative terms – to poorer areas when awarding contracts.

Table 2. Effects of federal contracting on metropolitan GDP per capita by recipient location and location of the activity.

2a. Recipient location

	(1) GDP/cap OLS	(2) GDP/cap IV	(3) GDP/cap IV	(4) GDP/cap IV	(5) GDP/cap IV	(6) GDP/cap OLS
Recipient location						
Contract spending/cap$_{t-1}$	1.733***	2.189***	3.617***	0.707	1.378***	1.639***
	(0.141)	(0.201)	(0.286)	(0.444)	(0.424)	(0.380)
Location of the activity						
Contract spending/cap$_{t-1}$						0.00577
						(0.234)
Observations	3,015	3,015	3,015	3,015	3,015	3,015
MSA FE				yes	yes	
state-year FE			yes		yes	

2b. Location of the activity

	GDP/cap OLS	GDP/cap IV	GDP/cap IV	GDP/cap IV	GDP/cap IV	GDP/cap OLS
Recipient location						
Contract spending/cap$_{t-1}$						1.639***
						(0.380)
Location of the activity						
Contract spending/cap$_{t-1}$	1.812***	1.776***	1.530***	2.870***	5.500**	0.00577
	(0.252)	(0.280)	(0.264)	(0.663)	(2.209)	(0.234)
Observations	3,015	3,015	3,015	3,015	3,015	3,015
MSA FE				yes	yes	
state-year FE			yes		yes	

Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

We also discriminate between the location of the awardee of the contract from that where the actual activity took place. Both types of locations are only introduced together in Column 6 because, although the straightforward approach would be to estimate both measures jointly in all regressions, this is not possible as the two indicators are closely related and their Bartik instruments even more so. This results from the fact that, although the location of the contract recipient may not always necessarily be the location where the contract is implemented and city-level growth rates differ across expenditure measures, the national growth rate used in the instrument is the same for all locations. This invalidates the joint use of both variables in IV regressions, as using the two related instruments causes a weak instrument problem.

In order to overcome this issue and assess whether the influence of federal contracting on GDP is greater on the location of the recipient or that where the activity takes place, we artificially nest the regressions based on the different expenditure measures. This implies, first, estimating two individual IV regressions – one for each expenditure measure – and predicting the GDP per capita. Second, the predicted values obtained from those regressions are re-introduced as independent variables in our preferred direct IV regression on the other measures. The t-test on the predicted values serves as an "encompassing test" or J-test, with the null that the model introduced through the predicted values has no additional explanatory power. Using this artificial nesting test, the p-value for the recipient location based prediction of GDP per capita is lower than 0.01 in the performance location based regressions. Reversed, the p-value for the performance location is 0.609 in the recipient based model. This suggests that the recipient location based measure encompasses the explanatory power of the other model. Overall, the results of the recipient location model (Table 2a) trump those of the location of the activity (Table 2b). This is corroborated in Column 6 of Table 2, where the coefficient for the recipient location is positive and significant, while that of the location of the activity is insignificant. Hence, once this is taken into account, the preferred estimation is that of Table 2a, Column 5 – including city and state-year fixed effects – which indicates a multiplier of 1.38, implying that 1 dollar in contracting expenditure generates a revenue of 1.38 dollars of GDP the year after the investment takes place.

5.2. ***Effects on private GDP per capita, population, and wages.***

The effects of federal contracting across cities in the US are, however, not limited to GDP. Private GDP, population, and wages as well as employment may also be influenced by geographical differences in the channeling of public funds through federal contracting. The results for the first three factors are presented in Table 3a. Column 1 reports the effects of contracting on private GDP, which excludes government services. The effects on private GDP are considerably smaller than those on aggregate GDP. The coefficient of 1.115 is 81% of that reported for the equivalent analysis on aggregate GDP in Table 2a, Column 5. As private GDP in the US represents around 86% of total GDP, the effects of contract expenditure may be marginally lower in that part of production. Table 3b reports a regression that use the contracting measure based on location of performance. The location of performance-measure has no explanatory power conditional on the location of recipient-measure. We find a multiplier of 6.68 for the location of performance, which has a p-value of 0.06. This estimate is extremely high (as it was for the overall GDP estimates),. Given the high collinearity with the alternative measure, this estimate cannot be considered credible.

The impact on population changes is also positive, economically meaningful, and significant. As the estimations are done in per capita terms, it is expected that migration should not significantly bias the results. Yet, the results of Table 3a, Column 2 indicate that $135,000 in federal contracts has led to the attraction of one additional migrant to the city where the money is allocated. As these data stem from the Community Surveys, the coverage in terms of cities is higher than the GDP number published by the Census. The results for Columns 2-5 are similar to those the sample used in Column 1; those results are reported in the Appendix A1. In Table 3b, we report the same regression for the location of performance-based contracting measure. No effect on migration is identified – the estimated coefficient is negative and statistically insignificant.

The effect of contracting on wages is statistically insignificant, regardless of whether wages (Table 3a, Column 3), the log wages (Column 4), or the log wages purged for standard controls[3] (Column 5) are considered. This result is repeated when considering the location of performance (Table 3b, Columns 3-5).

Table 3. Effects of federal contracting on private GDP per capita, population, and wages.

3a. Recipient location

	(1) private GDP/cap	(2) population	(3) wage	(4) log wage	(5) purged log wage[a]
	IV	IV	IV	IV	IV
Recipient location					
Contract spending/cap$_{t-1}$	1.115***	30.37***	0.278	4.43e-06	-1.34e-06
	(0.387)	(9.295)	(0.253)	(8.34e-06)	(5.29e-06)
Observations	3,015	5,194	5,237	5,237	5,237
state-year FE	yes	yes	yes	yes	yes
metropolitan FE	yes	yes	yes	yes	yes

3b. Location of the activity

	private GDP/cap	population	wage	log wage	purged log wage[a]
	IV	IV	IV	IV	IV
Location of the activity					
Contract spending/cap$_{t-1}$	6.687*	-52.60	0.299	6.83e-06	2.24e-05
	(3.551)	(72.08)	(0.872)	(3.52e-05)	(3.82e-05)
Observations	3,015	5,195	5,238	5,238	5,238
state-year FE	yes	yes	yes	yes	yes
metropolitan FE	yes	yes	yes	yes	yes

Regressions weighted according to the location's population. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
[a]the log wage residual after controlling for education; race; gender and age effects.

5.3. *Effects on employment*

Job generation is another important argument for decision-makers seeking federal contracts for their city. In Table 4, we explore, applying the same framework as in Table 2, the impact of federal contracting on urban employment in the US. Once again, we artificially test for the

[3] We ran a regression on census microdata explaining and individual's log wage from fixed effects for educational attainment; race fixed effects; gender; and age and age squared. The purged wage effect is the unexplained wage by metropolitan area by year; expressed in relative terms (as the equation is estimated in logs). The unexplained variation at the city-year level is at most a factor 2 (100%), the standard deviation is around 12%.

relevance of the location of the recipient of the contract and that where the activity takes place. As in the case of the analysis of the impact on GDP per head, the estimations include fixed effects at the city and state-year level and the instrumentation of the spending per capita variable. The results highlight that federal contracting is linked to employment generation both at the location of the recipient firm as well as in the cities where the activity occurs. However, and in contrast to the impact on GDP per head, the effects on employment are greater at the location of the activity than at the recipient location. The effect is statistically significant in all estimations; it operates through getting the unemployed to work (extensive margin) as well as by getting those already employed to work more hours (intensive margin). Most of the impact on jobs happens, however, through the intensive margin, that is, through additional work for those already in employment rather than through the creation of new jobs – 65% of all additional work goes to already employed workers, while the share of hours accounted for by previously unemployed individuals is 35%.

The level of expenditure in federal contracting required to generate additional employment is also considerably higher than that previously highlighted by the literature. The row 'cost per jobs' calculates the overall cost of generating an additional FTE job (40 hours of work per week, times 45 weeks). The results highlight that more than $256,000 of federal contracting are needed to create a new full-time job at the place of the location of the activity, while this figure rises to more than $600,000 at the location of the recipient firm (Table 4, Column 1). By contrast, the cost for the increase in the number of hours is considerably lower, varying from close to $60,000 at the place of the location of the activity to 146,000 at the location of the recipient (Table 4, Column 4). These results confirm that federal contracting has been better at increasing the number of hours worked by those already in employment than at generating new jobs for those unemployed or entering the market for the first time. Adding up the individual margins puts the cost of an extra FTE at $90,300 of federal contracting.

Table 4. Effects of federal contracting on jobs.

4a. Recipient location				
	(1) employment	(2) weeks (conditional[a])	(3) weekly hours (conditional[a])	(4) total hours (unconditional[a])
	IV	IV	IV	IV
Recipient location				
Contract spending/cap$_{t-1}$	2.65e-06**	3.43e-05	0.000200***	0.0123***
	(1.26e-06)	(4.31e-05)	(5.13e-05)	(0.00446)
Cost per job (US$)	608,429	2,221,000	338,785	146,889
Observations	5,195	5,195	5,195	5,195
state-year FE	Yes	Yes	Yes	yes
metropolitan FE	Yes	Yes	Yes	yes

4b. Location of the activity				
	employment	weeks (conditional[a])	weekly hours (conditional[a])	total hours (unconditional[a])
	IV	IV	IV	IV
Location of the activity				
Contract spending/cap$_{t-1}$	6.29e-06***	0.000229**	0.000282***	0.0303***
	(1.67e-06)	(0.000104)	(7.88e-05)	(0.00664)
Cost per job (US$)	256,372	333,018	240,340	59,320
Observations	5,195	5,195	5,195	5,195
state-year FE	yes	yes	yes	yes
metropolitan FE	yes	yes	yes	yes

Regressions weighted according to the location's population. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. [a] (Un)conditional on already in employment.

5.4. *Spatial and time effects*

How big is the spatial reach of the economic impact of federal contracting? A contract in one location may induce additional demand in another location. Failing to take this into account would bias our results for two reasons. First, any estimation would understate the effects of contract expenditure, because increases in GDP outside the city where the firm receiving the contract is located or where the activity related to the contract occurs are not considered. Second, as state-year fixed effects are included in the analysis, any spatial effects may be compared to an incorrect benchmark. Let's suppose, for instance, that contracting in one city draws away resources from other cities in the same state. As the impact on cities is compared to the state-average, this suggests that the contracting effect is measured twice: once in the higher GDP in the city that received the contract, and the second time in other cities that saw a downturn as a result.

To investigate whether there are any spatial effects of federal contract expenditure, we measure the impact of contracting using concentric rings around each city. This imposes no functional form (except that we expect the decay in contracting effects to be related to distance). Given the size of the continental US states, and to include a non-negligible amount of cities in every concentric ring, we choose rings of 200km around the city where the contract is awarded or is implemented. Table 5 reports the results for different sets of concentric rings.

Table 5. Spatial effects of contracting.

	(1) Contract spending /cap OLS	(2) GDP/cap OLS	(3) GDP/cap OLS	(4) GDP/cap IV	(5) GDP/cap IV	(6) GDP/cap IV
Recipient location (per capita, t-1)						
Contract spending		1.639***	1.647***	1.601**	1.429**	1.423**
		(0.270)	(0.271)	(0.735)	(0.590)	(0.583)
Contract spending 0-200km	0.0301*	-0.314***		1.702		
	(0.0166)	(0.113)		(1.318)		
Contract spending 200-400km	-0.0147	0.309*		-0.0307		
	(0.0196)	(0.179)		(1.133)		
Contract spending 400-600km	0.0143	0.604***		0.384		
	(0.0208)	(0.174)		(0.619)		
Recipient location (mln $, t-1)						
Contract spending 0-200km			-0.0256**		0.141*	
			(0.0116)		(0.0769)	
Contract spending 200-400km			0.0148		0.00452	
			(0.0128)		(0.0280)	
Contract spending 400-600km			0.0453***		-0.0475	
			(0.0135)		(0.0403)	
Location of activity (mln $, t-1)						
Contract spending 0-200km						0.284**
						(0.130)
Contract spending 200-400km						0.0435
						(0.0409)
Contract spending 400-600km						-0.165***
						(0.0621)
Observations	3,015	3,015	3,015	3,015	3,015	3,015
state-year FE				yes	yes	yes
MSA FE				yes	yes	yes
F-stat rings		0.00	0.00	0.28	0.07	0.00
Aggregate multiplier			1.94		1.79	1.85

Regressions weighted according to the location's population. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

First, we check whether federal contract expenditure correlates across neighboring cities. Table 5 first presents the regressions of federal contracting per capita in a given city on expenditure per capita in surrounding cities (the average is weighted by city populations) (Table 5, Column 1). This regression suffers a reflection problem which causes an upward bias in the coefficients. Despite this upward bias, the estimated coefficients are small, suggesting that the expected city level contract expenditure rises by around 3 cents for every additional dollar of contract expenditure within 200km. The effect vanishes completely beyond 200km.

For comparison, Columns 2 and 3 in Table 5 report the OLS estimates without fixed effects for expenditure per capita and aggregate expenditure in concentric rings around the city. Column 2 reports the effects of per capita expenditure. They suggest that high expenditure per capita within a 200 kilometer range has negative effects on the local GDP per capita, but a positive effect beyond 200 kilometers. The F-statistic reports the p-value of a joint F-test on all concentric ring variables, confirming statistical significance. A similar pattern emerges when looking at aggregate contract expenditure instead of per capita contract expenditure in nearby cities. Dollars spent at ranges under 200 kilometers are associated with lower GDP per capita, while dollars spent at higher ranges are associated with higher GDP per capita.

The expenditure in nearby cities makes it hard to compare the multipliers reported earlier in the baseline results of Table 2. There is a direct effect captured in the coefficient for local per capita contract expenditure, similar to the coefficient reported in Table 2. However, there are additional effects, because contracting outside the city can increase or decrease GDP, too. To calculate an aggregated multiplier, we assume that there is an extra dollar of contract expenditure per capita in all cities. We calculate the indirect effects by imputing the GDP effect of expenditure in each city's ring. We derive a multiplier by comparing the sum of direct and indirect GDP effects to the cost of raising contract spending by one dollar per capita. For the OLS estimates, adding the indirect effects increases the multiplier to 1.9.

The preferred specification introduces the fixed effects structure and shift-share instruments for the spending measures in the cities as well as in the rings. The results of these specifications (Table 5, Columns 4-6) show beneficial effects of contract spending at close range. Per capita contract spending near the city (column 4) does not significantly affect local GDP – coefficients for the rings are statistically insignificant, both individually and jointly. However, *aggregate* expenditure near the city is statistically significant, whether specified by vendor location or by location where the activity is performed. Both measures report a positive impact of nearby expenditure. The F-statistic shows statistical significance for the rings, particularly for the contract measures based on the location of activity. The direct impact almost mirrors the baseline results (1.38 vs 1.43), but the spatial impacts of contract spending increase the effect to around 1.8 dollars in GDP per dollar in contract expenditure.

The shift-share instruments for contract spending in each city and in the rings around each city do not show a substantial correlation. This helps in identifying the effect of individual rings. In Appendix Tables A2 and A3, we show that i) the correlations between instruments for different rings are rather low and ii) that instruments for individual rings predict contract expenditure in their respective ring, conditional on the other instruments.

What about the time dimension? Table A4 in Appendix reports the baseline regression using different time lags. The individual lags in isolation (Table A2, Columns 1-3) show positive coefficients that fade over time. The individual fit is best for the 1-year lag, that is, one year after the expenditure linked to the contract happens (Table A2, Column 4). Introducing simultaneously the no-lag and the 1-year lagged measure shows that the estimation of the impact of federal contracting on the year the expenditure is disbursed becomes statistically insignificant once the 1-year lag variable is introduced in the regression. Although it could be argued that the instrument between the two expenditure measures may be correlated, the joint Cragg-Donald F-statistic of 11.4 suggests no problem of weak instruments (the 10% max IV size critical F-statistic is 7.03). Adding more lags (Table A2, Columns 5-6) renders all coefficients insignificant. All this implies that the impact of federal contracting on urban economic performance in the US is short-lived: it barely lasts more than two years after the expenditure takes place and is best observed after one year.

4.6 *Local government responses*

Local governments may react by adjusting their own expenditure decisions when a contract is allocated to their region. In some cases, the federal contract may lead to a reduction of local government spending, if that spending substitutes for a task performed by the local government. In others, local governments may increase spending to facilitate or match the contracted activities.

The response of local governments is important for identification. If local governments typically respond to the local allocation of federal contracts, we observe an outcome that is the result of both federal expenditure, and the correlated but unobserved local expenditure. This is not an issue for state-level decisions, as our methodology controls for state-level shocks. However, there may be a bias from the sub-state level government expenditure decisions.

In this section we examine the responses of county governments to federal contracts in detail to rule out that they affect the results. We use the Historical Finance database for county governments from the Census, which runs up to 2012 and covers many of the counties that comprise the micro- and metropolitan governments in our dataset. We aggregate the counties of each micro/metropolitan area and calculate the annual total current expenditure per capita by county in each micro/metropolitan area. There is a slight difference in the coverage of the counties in the Historical Finance database and the areas for which we have Census information on GDP. We report regressions on all identified areas from the Historical Finance database, as well as the subsample of cities that appears in both datasets.

Table 6 presents the results of regressing county government expenditure per head on federal contracts per head in the same city. The OLS regression of Column 1 suggests that each dollar linked to a federal contract increases county-level expenditure by less than 3 cents. Restricting our attention to the sample used in our main results, the effect rises to less than 4 cents. This mainly affects the location of the recipient as the result for the location of performance the coefficient is insignificant (Column 3). Applying fixed effects and the shift-share IV (Column 4) slightly reduces the estimate to around 3 cents of county government expenditure per dollar of federal contracts. This estimate is probably a better approximation of the causal effect and the magnitude remains similar to the OLS regressions. Once again, no effect is reported for the location of the activity.

Overall, the response of county government current expenditure to local federal contracting is limited. Assuming that the output multipliers and employment effects of county government are not excessively higher than the multiplier of federal contracts, the confounding effects of county government responses seem minor.

Table 6. Effects on county government current expenditure per capita (2005-2012).

	(1) County expenditure per cap	(2) County expenditure per cap	(3) County expenditure per cap	(4) County expenditure per cap IV	(5) County expenditure per cap IV
Recipient location					
Contract spending/cap$_{t-1}$	0.0265***	0.0381***		0.0293**	
	(0.00708)	(0.00862)		(0.0129)	

Location of the activity					
Contract spending/cap$_{t-1}$			0.00149		0.0303
			(0.00808)		(0.0231)
Observations	3,880	2,389	2,389	2,389	2,389
state-year FE	No	no	no	yes	yes
metropolitan FE	No	no	no	yes	yes

Regressions weighted according to the location's population. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

5. Discussion

In some instances, the results of our analysis confirm previous research, but, in others, lead to new and, in certain cases, considerably different results.

First, in terms of overall output, the multiplier of 1.4 is generally consistent with past evidence. It coincides with the majority of previously reported multipliers looking at transfers to individuals and/or states – rather than, as in our case, federal contracting – which generally range between 1.1 and 1.7 (Fishback and Kachanovskaya 2010; Nakamura and Steinsson; 2014; Suárez Serrato and Wingender 2016). Our multiplier is also in line with the 1.6 multiplier linked to the 2009 fiscal stimulus package (Romer and Bernstein, 2009). However, the results underline that the positive impact on output tends to happen in places where the recipient firm is located, meaning that the effects on the areas specifically targeted by federal contracting are likely to be smaller than originally intended. They also highlight that the impact of federal contracting is geographically bounded – as it does not extend beyond 200 km from where the contracted firm is based – and short-lived.

The effects on output are best interpreted as the consequences of expenditure, isolated from taxation effects. First, our results show that the contract expenditure effects are clearly felt at the city level. However, as the contract expenditure is financed federally, it is unlikely that individual firms and citizens in a recipient city will change their behavior due to anticipated tax burden changes. This makes it harder to consider against what number we should compare the multiplier of 1.4. In an optimal tax system, the cost of raising a dollar in tax revenue might be 1 (Jacobs, 2016). Empirical estimates suggest that the marginal costs of public funds are higher due to distortions in labor supply or redistribution, but often less than 1.4 (Massiano and Picco, 2013). Second, the impact of federal contracting on jobs, while significant (Greenstone et al., 2010; Ham et al., 2011; Hanson and Rohlin, 2013; Busso, Gregory and Kline, 2013), is also lower and costlier than highlighted in previous literature. As indicated in the analysis, federal contracting has contributed more to raise the amount of work conducted by those already in employment than to create new jobs. Almost two thirds of the additional work generated by federal contracting has resulted in increased time for those already in employment, vis-à-vis the just one third that has contributed to new jobs. The cost in federal contracts of any additional job is also considerably higher than in previous analyses, and especially those based on grants rather than contracts. The $250,000 of federal contracting needed to produce an additional full-time job in the city where the contract is developed (or even the $60,000 for an additional FTE linked to estimating the aggregated hours per person) dwarf much lower estimations by Criscuolo et al (2012), Shoag (2013), Chodorow-Reich et al. (2012), or Suárez Serrato and Wingender (2016). Only the work of

Feyrer and Sacerdote (2011) is close to our results. Moreover, the impact on employment is higher in the place of implementation than in the city where the recipient firm is located. This distinction between the geographical impact on outputs and employment may draw, as indicated by Feyrer and Sacerdote (2011), on the fact that while changes in employment and wages for any sort of public investment usually take place where the activity is being conducted, the sourcing of the activity – for instance, the materials needed for the construction of a road – can be drawn from elsewhere in the country and often from where the constructing firm is located, undermining the local economic impact of contracting.

Finally, the results stress that the effects of federal contracting on wages are, in the line of Criscuolo et al. (2012), more limited than what has been stressed by the US literature looking at the impact of incentives, subsidies, and grants (Greenstone et al., 2010; Ham et al., 2011; Hanson and Rohlin, 2013; and Busso, Gregory, and Kline, 2013). Federal contracting, despite leading to increases in the amount of hours worked, has had virtually no impact on workers' wages. These lower estimates for employment and wage effects are not surprising, as federal contracts are not generally designed to create jobs.

6. Conclusions

At a time when competition for increasing output, employment, and wages is rifer than ever, securing public funding – whether through grants, subsidies, or contracts for local firms – has become a must for any local decision-makers. Government funds are generally regarded as a panacea for enhancing local economic activity and as one of the catalysts to achieve greater wealth and employment. In the US alone an average of $1,500 per individual per annum have been channeled to the generation of economic activity through federal contracting. But is this huge amount of public funds delivering greater output, jobs, and wages in those areas of the US that have most benefitted from it?

It is somewhat surprising that, in spite of the huge sums involved (more than $400 billion per year), this question has remained unanswered for so long. Most of the studies dealing with the impact of public funds at a territorial level have mainly concentrated on grants and subsidies, but have overlooked federal contracts. Hence, our knowledge about the efficiency of federal contracting for output and jobs has been rather limited. This paper has tried to address this gap in the literature.

The results of the analysis point to federal contracting as a rather imperfect and expensive instrument to generate income and jobs across cities in the US. Federal contracting contributes to moderate increases in output – with GDP multipliers equivalent to those reported by previous analyses focusing on grants and subsidies – and to enhance the overall amount of work conducted by workers in US cities, but its impact is far more restricted in terms of generating new employment. Two thirds of the additional work-related activity benefits those in already in employment. Only one third is behind the creation of new employment. Moreover, at a cost for each new job which ranges between more than $250,000, in the place where the activity is performed, and almost $610,000, at the location of the recipient firm, federal contracting represents a pricey public policy employment strategy. Hence, this type of contracts creates less employment and at higher prices than alternatives based on subsidies and grants, as reported by the extant literature.

The impact of federal contracting also varies depending on whether one considers the location of the firm securing the contract or where the contracted work actually takes place.

The output effect is far greater at the recipient location, while the impact on jobs is far more significant in the cities where the activity is conducted. Hence, the impact not always necessarily takes place where it is intended. Finally, the analysis also stresses that the effects of federal contracting are felt onl felt within a 200km radius from the place of contracting, leading to some indirect effects, and that the contracting effects fade after two years.

Overall, the results of the paper raise reasonable questions about the convenience and viability of federal contracting as a vehicle for economic development. Courting the federal government for contracts by firms and local governments may yield lower economic returns than anticipated by local decision-makers and entrepreneurs. Federal contracts do indeed help generate additional economic activity and have an impact on output. They may even deliver votes. But their influence on employment is far more limited. Pursuing federal contracting can thus not be considered the most efficient strategy to spur development: it is costly, relatively ineffective in comparison to alternatives, and too dependent on politics to produce a reliable stream of economic activity and, in particular, new jobs. City governments across the US would thus do better in terms of economic development by pursuing more direct, active strategies aimed more specifically at generating employment and facilitating the development of economic activity based on local capabilities and potential rather than spending more time and efforts on securing the next big government contract.

APPENDIX

Table A1: Alternative indicators in the GDP sample

Effects of contracts on other outcomes

A1a. Recipient location

	(1) private GDP/cap IV	(2) population IV	(3) wage IV	(4) log wage IV	(5) purged log wage[a] IV
Recipient location					
Contract spending/cap$_{t-1}$	1.115***	29.79**	0.287	5.54e-06	-4.80e-08
	(0.387)	(12.83)	(0.378)	(1.27e-05)	(7.84e-06)
Observations	3,015	3,015	3,015	3,015	3,015
state-year FE	yes	yes	yes	yes	yes
metropolitan FE	yes	yes	yes	yes	yes

A1b. Location of the activity

	private GDP/cap IV	population IV	wage IV	log wage IV	purged log wage[a] IV
Location of the activity					
Contract spending/cap$_{t-1}$	6.687*	-81.90	1.016	3.07e-05	3.60e-05
	(3.551)	(132.6)	(1.575)	(4.88e-05)	(6.22e-05)
Observations	3,015	3,015	3,015	3,015	3,015
state-year FE	yes	yes	yes	yes	yes
metropolitan FE	yes	yes	yes	yes	yes

Regressions weighted according to the location's population. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

[a]the log wage residual after controlling for education; race; gender and age effects.

Table A2 Correlation matrix for the Bartik (shift-share)

	Instrument local	Instrument 0-200km	Instrument 200-400km	Instrument 400-600km
Instrument local	1			
Instrument 0-200km	0.0373	1		
	(0.0406)			
Instrument 200-400km	-0.0089	0.0157	1	
	(0.6233)	(0.3879)		
Instrument 400-600km	-0.0159	-0.0658	0.0480	1
	(0.3839)	(0.0003)	(0.0083)	

Note: Based on the baseline sample of 3,015 observations. p-value in parentheses.

Table A3 Seemingly unrelated regression for shift-share (Bartik) instruments with Spending measures as dependent variables

Panel a: Expenditure rings based on location of vendor

	(1)	(2)	(3)	(4)
Dependent variable	Spending/cap	Spending	Spending	Spending
Range		0-200km	200-400km	400-600km
Location measure	vendor	vendor	vendor	vendor
Location of Vendor				
IV per capita spending	0.07***	-0.00	-0.00**	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
	62.72	**-0.41**	**-2.41**	**0.01**
Instrument 0-200km	-0.00	0.06***	0.00***	-0.00***
	(0.00)	(0.00)	(0.00)	(0.00)
	-1.46	**502.58**	**6.29**	**-2.86**
Instrument 200-400km	-0.00	0.00	0.06***	0.00***
	(0.00)	(0.00)	(0.00)	(0.00)
	-0.82	**0.77**	**434.87**	**2.67**
Instrument 400-600km	-0.00	0.00	0.00***	0.06***
	(0.00)	(0.00)	(0.00)	(0.00)
	-0.30	**0.23**	**2.95**	**365.80**
Observations	3,015	3,015	3,015	3,015

Panel b: Expenditure rings based on location of activity

	(1)	(2)	(3)	(4)
Dependent variable	Spending/cap	Spending	Spending	Spending
Range		0-200km	200-400km	400-600km
Location measure	vendor	activity	activity	activity
Location of Vendor				
IV per capita spending	0.07***	0.01***	0.01***	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
	62.74	**9.51**	**2.65**	**1.02**
Location of activity				
Instrument 0-200km	-0.00*	0.04***	0.00***	0.00*
	(0.00)	(0.00)	(0.00)	(0.00)
	-1.69	**310.26**	**12.95**	**1.94**
Instrument 200-400km	-0.00	0.00***	0.04***	0.00***
	(0.00)	(0.00)	(0.00)	(0.00)
	-0.86	**7.49**	**271.43**	**9.65**
Instrument 400-600km	0.00	0.00	0.00***	0.04***
	(0.00)	(0.00)	(0.00)	(0.00)
	0.17	**0.94**	**6.27**	**248.94**
Observations	3,015	3,015	3,015	3,015

Standard errors in parentheses; t-statistics in bold. *** p<0.01, ** p<0.05, * p<0.1

Table A3 shows two seemingly unrelated regressions, one for each location measure. The regression shows what instruments predict which spending measure (the endogenous variables in Table 5), while controlling for potentially correlated error structures. The numbers in bold show the t-statistics, as the standard errors are typically low.

The t-statistics in Table A3 show that the instrument for every ring predicts its spending measure well. That holds, conditional on adding the other instruments. Moreover, based on the t-statistics, the relevant instrument seems to be the dominant explanatory variable – for instance, Panel a, Column 2 shows that the instrument for the 0-200km range has a t-statistic of over 500, while the other instruments have t-statistics smaller than 1.

Table A4. *Lagged contracting effects*
Effects on GDP per capita with different lags

	(1) GDP/cap IV	(2) GDP/cap IV	(3) GDP/cap IV	(4) GDP/cap IV	(5) GDP/cap IV	(6) GDP/cap IV
Recipient location						
Contract spending/cap$_t$	1.870***			-0.0483	0.846	0.861
	(0.683)			(0.885)	(1.011)	(1.014)
Contract spending/cap$_{t-1}$				2.065**	1.078	2.849
				(0.962)	(1.275)	(1.981)
Contract spending/cap$_{t-2}$		1.616*			1.487	-0.132
		(0.952)			(1.185)	(1.747)
Contract spending/cap$_{t-3}$			1.325			2.119
			(0.945)			(1.725)
Observations	3,382	2,648	2,281	3,015	2,648	2,281
state-year FE	Yes	yes	yes	yes	yes	yes
MSA FE	Yes	yes	yes	yes	yes	yes

Regressions weighted according to the location's population. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

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