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Article (Accepted version) (Refereed)

Original citation:
Rodríguez-Pose, Andrés and Gerritse, Michiel (2018) *Does federal contracting spur development? Federal contracts, income, output, and jobs in US cities.* Journal of Urban Economics. ISSN 0094-1190

DOI: 10.1016/j.jue.2018.07.002

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Abstract

Firms and governments alike frequently court federal government contracts to generate more jobs and trigger economic growth. However, the employment and output impact of government contracts remains controversial. We use georeferenced data on United States (US) federal contracts, distinguishing between the location of the recipient and the location of the activity, for the years 2005-2014 in order to assess the employment and output impacts of federal contracting in metropolitan areas of the US. We resort to a shift-share instrument and precise location-specific fixed effects to estimate the causal impact of spending. Cities that receive more contract expenditure witness an expansion in output – with contracts generating $1.4 per dollar spent – but experience only modest increases in employment. The impact is also constrained geographically and short-lived. The results suggest that, on average, the effects of federal contracting on local economies are modest, meaning that attracting federal contracts may not be an effective urban development strategy.

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 across the US and were expected to create jobs and stimulate production in recipient firms and locations. Presidential economic advisors have viewed federal contracting as a development tool: a multiplier of $1.6 in output was expected for every dollar of government spending [see the motivation for the 2009 fiscal stimulus package proposed by President Obama (Economist, 2009)].

However, it was not only firms that pursued government contracts. As federal contracts are expected to generate jobs and trigger economic growth, local decision-makers have also actively courted them. As a norm, US Senators regularly use their websites to advertise successes in securing federal funding for their home states. Yet, the economic development impact of these contracts remains shrouded in mystery. The reasons for this are twofold. First, the effect of federal contracts on the development of cities and states has attracted relatively little interest despite the volume of funds disbursed. 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, $35,000 has been often quoted as the public expenditure needed to create a new job (Ramey, 2011). However, the range is vast: from $25,000 to $125,000 (Shoag, 2013; Wilson 2012). Similarly, various spatial quantitative analyses have suggested that the multiplier effect associated with government contracts may be as low as 0.5 or as high as 2.4 (Nakamura and Steinsson, 2014; Fishback and Kachanovskaya, 2010; Clemens and Miran, 2012).

This paper delves into the economic impact of federal contracts across urban areas in the US between 2005 and 2014. It exploits contract-level data to estimate the impact of federal contract spending on urban employment and GDP. The analysis also draws a distinction between where, on the one hand, the firm benefitting from the contract is located (recipient location) and where, on the other, the activity related to the contract takes place (location of the activity). The high granularity of the data collected allows us to go beyond previous literature and analyze the impact of national expenditure decisions as an exogenous source of variation in local contract expenditure at a city level. Moreover, we are able to control, in a more precise way than hitherto, for localized economic circumstances as well as for state-level political representation. These are factors that may otherwise bias the estimates of the GDP and employment impacts of public spending.

The results of the analysis show that federal contracting is a non-negligible driver of urban growth in the US. One dollar of federal contract spending generates close to \$1.4 in additional GDP. Output and employment increase in cities that benefit from more federal contract spending per capita. The employment effects are, however, lower than related studies suggest. Output changes are more often realized not in the cities where the contract is executed, but rather in those where the recipient firms are established. By contrast, cities where the contracts are executed witness moderate increases in employment.

Overall, our analysis suggests that the economic outcomes associated with federal contracts – once reverse causality and spurious trends are controlled for – are small and short-lived. No measurable output effects remain in evidence two years after a contract is awarded. Likewise, the output effects of federal contracts outside the location of expenditure are virtually negligible.

2. Public intervention and wages, jobs, and output

Government contracting is frequently regarded as a tool for economic development. Firms and local decision-makers lobby central or federal governments for contracts. However, the economic impact of public contract expenditure at the regional or urban scale has so far attracted limited attention and consequently remains poorly understood, especially in relationship to the greater scholarly interest in grants and subsidies.[1]

In this paper we explore this question by linking our research to two strands of literature. First is the large body of literature estimating the output multipliers and employment impacts of fiscal spending. Our contribution to this strand is derived from our measurement of federal contract expenditure and our focus on urban outcomes. Second, the paper relates to research on the local employment, wage, and production impacts of public policies and to geographically targeted policies, such as spatial subsidies or zoning, aimed at attracting firms and creating jobs. While the territorial unit of analysis employed here is comparable to such studies, we examine a different kind of public spending: federal contracts. These contracts cater to public demand and are not motivated by the development of specific areas. They are awarded through competitive tenders and generally not targeted to individual firms or areas. Importantly, federal contract expenditure in the US is far larger than the budget for location-specific incentives.

2.1 Spatial variation in public expenditure, jobs, and output

Several recent papers have employed spatial variation in US public spending to identify job and output effects. The estimated job impacts vary with the identification strategy used. Suárez Serrato and Wingender (2016) used exogenous shocks to spending resulting from changes in county population and reported that every $30,000 in spending creates one additional job. Allocation rules in fiscal spending – e.g. those related to the American Recovery and Reinvestment Act (ARRA) – have also been used as a source of exogenous

[1] Focusing on federal contracting in cities has, nevertheless, advantages relative to research at a larger geographical scale, as the urban dimension permits methodologies for the identification of causal impacts of contract expenditure that are unavailable at larger territorial level.

variation in spending across states. Instrumenting state-level spending with ARRA allocation rules, Wilson (2012) estimated that a job is created for every $125,000 spent. Feyrer and Sacerdote (2011) considered the impact of actual ARRA spending (including grants and loans) on employment-to-population ratios at the state and county level over 20 months of the program. Exploiting the years served by the local congressional delegation as an exogenous predictor of the amount of ARRA spending channeled to a specific location, they calculated that the creation of an additional job requires between $43,000 and $100,000 of public spending at the state level, and between $500,000 and $3.3 million at the county level. Chodorow-Reich et al. (2012) similarly examined exogenous spending due to allocation rules and put the job cost at $26,000. Shoag (2013) investigated state variation in public expenditure arising from (exogenous) financial shocks to state pension funds, calculating the cost per job at $23,000. These job impact estimates have generally focused on employment changes within a particular state, ignoring the spatial impact of expenditure.

Subnational analyses have considered the output increase attributable to one dollar of public expenditure – the multiplier – to range between $1.1 and $2, although there are significant deviations. Nakamura and Steinsson (2014) explained 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 reported 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. Similarly, Shoag (2013) found that the multiplier on state spending identified from pension fund return shocks is also 1.4. Higher multipliers (between 1.7 and 2) have been revealed by Suárez Serrato and Wingender (2016). Conversely, Fishback and Kachanovskaya's (2010) analysis of the New Deal grants at the state-level during the 1930s and 1940s yielded a significantly lower multiplier (1.1). Their estimates of the effects of government purchases (excluding direct transfers) rise to $1.8, although the confidence intervals are wide. When accounting for the taxation financing expenditure, the multiplier may be lower – Clemens and Miran (2012), for example, show that balanced-budget multipliers can fall well below 1 – around 0.4.

Different multipliers on output and costs per employment have also been reported by the literature on local incentives, subsidies and zones. Firm subsidies and tax incentives are deemed to lead to increases in wages and employment, although the estimated impacts vary by methodology, program and location (cf. Greenstone et al., 2010; Ham et al., 2011, Neumark and Simpson, 2015). These multipliers are, however, less intimately related to our analysis, as such research concentrates on types of public spending that pursue different aims.

2.2. ***Going beyond past research***

The paper takes the literature on spatial variation in public expenditure, jobs, and output as a starting point, but goes beyond existing knowledge on two counts. First, we focus on a significant source of public spending which has been neglected at the urban level. Much of the relevant urban-oriented work that precedes our research focuses on grants, subsidies, and general investment and development programs that purposely target job creation and production in specific geographical areas. The influence of public contracting on economic development has been largely ignored. The distinction between government subsidies or grants, on the one hand, and federal contracting, on the other is, however, important. Government contracting leads to the acquisition or production of public goods and services and investment. Local development is thus a side-effect or a byproduct. Subsidies and economic zone interventions are explicitly designed to promote development in specific areas. Because of this difference, the local effects of public contracts per dollar spent are conceivably smaller than those associated with public subsidies.[2] However, funds earmarked for public contracting tend to exceed those deployed as grants and subsidies, meaning that, despite a potentially smaller per-dollar impact, their aggregate effect could be far larger.

Second, the granularity of our data facilitates a more precise analysis than what has been undertaken in the past. We are thus able to offer new evidence on the output multiplier and job impact of public expenditure. The exact contributions derived from the analysis at this finer spatial scale are three-fold. First, impact estimates are often plagued by concerns of causality: while public spending can generate greater output, the reverse is also true: increases in output may result in higher levels of public spending.[3] The vast majority of research in the field applies a shift-share instrument under the assumption that the allocation of spending over states can be considered exogenous, even if aggregate national spending may be endogenous. However, it is highly possible that some federal expenditure decisions are taken with particular US states in mind. Additionally, states are well represented at the federal level, potentially leading to targeting due to pork-barrel politics (Larcinese et al., 2006). By exposing city-level variation, we are able to i) relax the assumption that individual states (in a group of 50 states) do not affect national expenditure in exchange for the less stringent assumption than individual cities (in a group of over 300) do not determine aggregate national expenditure in broad (NAICS-1) categories; and ii) estimate this conditional on fixed effects accounting for state-specific shocks. This involves including state-level representation, by comparing the outcomes of national expenditure decisions across cities in the same state. As a second contribution, we are able to identify the localized

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[2] Public contracting is not mainly directed towards areas where the potential resources are under-utilized and therefore can crowd out private demand. Government contracts in some cases may also play a distinctive development role: competitions for contracts can take into account the city of the bidding firm. As that adds a different (political) motivation to the contract award, the impact may differ. Unfortunately, our dataset does not record that motivation. We design the empirical strategy to avoid that political targeting conflates with our causal interpretation of the impact of expenditure.

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

impacts of federal contract spending, implying that the measured effect is isolated from the taxation that finances expenditure. Citizens of a city receiving a federal contract can generally not anticipate substantial changes in federal tax rates. This sets our study apart from others that report the policy impact net of taxes (e.g., Clemens and Miran, 2012), as we segregate pure expenditure effects irrespective of tax distortions. This does, however, introduce the disadvantage that our results need to be benchmarked against a set of separately estimated costs of public funds to assess the efficiency of federal contract expenditure. Third, the geographical detail enables us to measure spatial effects. Past research has been unable to distinguish between the place where the contract is allocated (recipient location) and the place where the expenditure is effectively conducted (location of the activity). This difference may be especially relevant, particularly in lagging areas where the mismatch between the recipient location and the location of the activity is likely to be greatest. Moreover, we can measure the impact of federal contracting in areas close to where the expenditure took place. This is relevant for understanding the overall impact of expenditure at the urban level, but also for identification. As Ramey (2011) suggests, in cross-sectional estimates with time-fixed effects, spatial movements and transfers may easily be mistaken for positive impacts on economic growth and employment. The analysis we conduct also captures the extent of spatial impacts and relocation.

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 assembled 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 over $3,000 are published under the Federal Funding Accountability and

Transparency Act (FFATA) of 2006 by the Bureau of Fiscal Services (Dept. of Treasury). The contracts vary in amounts, locations and types of goods. For instance, in January 2014, the smallest contract in the state of Oregon was for a delivery of apples to the Defense Logistics Agency of the Department of Defense by the Portland-based Pacific Coast fruit company. The largest contracts in Oregon during that period related to the temporary procurement of firefighting helicopters from Oregon and California. Other contracts were awarded for window and sewer repairs, the procurement of IT and mechanical equipment, and the provision of occupational training courses and consultancy services.

Federal contracting accounts for a fair share of overall federal spending. Close to $4.82 trillion – or, on an annualized basis, around $400 billion – were spent over the period of analysis. In 2014 alone $446 billion were allocated to federal contracts above the $3,000 threshold. This is equal to $1,375 spent per 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.

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 by NAICS 1-digit industry.[4] We combine contracts by the location codes or by the contract 5-digit zip codes that belong to each core-based statistical area (CBSA). 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. These locations often coincide. In some cases, however, public services and products are produced at a different location from that where the contracting firm is based. Not all contracts are

[4] 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.

georeferenced. 85% of the total value of federal contracts can be positioned geographically, if location of performance is considered. This percentage rises to 94% when the address of the contract recipient is used. Our spending data identify contract recipients in 920 core-based statistical areas. These cities concentrate 80% of all identified federal contracting in the US, with the rest going to rural areas and/or to Alaska and Hawaii. The GDP data for CBSAs, however, do not cover all urban areas in the contract dataset. Matching both datasets results in a coverage of a maximum of 373 cities.[5] Employment and wage data are available at a finer geographical scale, meaning that a maximum of 655 cities are included in the wage and employment-based regressions. For 2014, the full contracting city sample covers around 285 million people (or 89% of the US total population). When looking exclusively at GDP data at urban level, the coverage is reduced to 256 million people (around 80% of total population). There is a distinct urban bias in federal contracting that reflects the concentration of firms in cities. Contracts awarded to firms in urban areas 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 of federal contracting per capita.

[Figure 1 about here]

The production and provision of goods and services by federally contracted entities 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. 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, and Sacramento were among the few urban areas that attracted more than $2,000 of expenditure per capita in 2014 (Figure 1). Federal and state capitals drew relatively greater amounts per capita in federal contracts. This was true of Washington, D.C., Boston (Ma.), Concord

[5] The unmatched areas have less population, are generally small, and have no reported GDP.

(N.H.), Hartford (Ct.), Providence (R.I.), Richmond (Va.), Atlanta (Ga.), Denver (Co.), Salt Lake City (Ut.), and Sacramento (Ca.). By contrast, the country's three largest agglomerations – New York, Los Angeles, and Chicago – benefitted relatively little from federal contracts. Expenditure in the metropolitan 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, by contrast, high in cities with a strong military tradition, such as Norfolk, Va., San Diego, Ca., Tacoma, Wa., or Tucson, Az. (with a respective expenditure per capita of $1,800; $2,520; $2,050; and $3,200). The Department of Defense was the biggest single contributor to public contracts. In some of the years included in the analysis, it disbursed over half of all the funds linked to federal contracting. The object of defense contracts ranged from aircraft and vehicles to IT and consulting services.

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 sewerage services, power infrastructure, highways, streets, and other civil engineering – and manufacturing (Figure 2b). Federal contracting in infrastructure in 2014 was under $250 per capita in more than 90% of continental US cities. There was, however, evidence of concentration of expenditure in some cities, New Orleans included, that were recovering from natural disasters (Figure 2a). Patterns of expenditure in manufacturing more closely matched those in overall federal contracting expenditure (Figure 2b).

[Figure 2 about here]

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. It implies that federal contract expenditure is positively related to city size; as a rule, the larger the city, the greater the federal spending in contracts per capita. The relationship holds for both place of performance and of recipient. The regression line that summarizes the relationship between federal contract expenditure at the recipient location and city size is, however, marginally steeper than that which captures the association between expenditure at the place of performance and population. This suggests that the spatial concentration of firms that succeed in winning tenders exceeds that of where the work takes place. Hence, a portion of contracted activities in smaller cities are likely executed by contract recipients from larger cities. All of that said, however, because many of the main beneficiaries of contracts are medium-sized cities, the relationship between city size and federal contracting is imperfect, especially in the case of place of performance (Figure 3).

[Figure 3 about here]

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 (US Census Bureau); output and GDP (US Bureau of Economic Analysis); and wages and employment (American Community Surveys). Table 1 reports both the unweighted and weighted (by population in 2014) city averages for all of the above.

[Table 1 about here]

4. Methodology

In order to make the analysis comparable with previous research – especially with that on fiscal multipliers – we explain GDP and employment using the variation in federal contract expenditure across US cities. A mere statistical association between output and federal contract expenditure does not necessarily uncover the economic impact of federal contracting. The allocation of contracts across cities is, in all likelihood, not random. The economic circumstances of a city determine the geographical allocation of expenditure. Political motivations cannot be discarded as factors influencing the distribution of federal contracting. Equally, unobserved variables may also play a role: if a large firm goes bankrupt, its demise decreases production in the city where it was located, reducing, at the same time, the chances of a city being awarded a contract. To deal with this issue, we pursue two different strategies: a) the shift-share instrumentation of local contract expenditure and b) controlling for the initial situation and unobserved shocks in each city by means of city and state-year fixed effects. We employ a standard multiplier equation (Nakamura and Steinsson, 2014) at the city-year level to estimate the impact of spending on a number of economic outcomes. The equation 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 estimate this equation by means of the weighted (instrumental) least squares version of this regression – using the population measures as weights – to be able to recover nationally applicable estimates.

In order to provide a causal interpretation, we use a shift-share (or Bartik) instrumental variable approach. This approach 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 uncover the variation across cities, assuming that national spending decisions are not driven by shocks to specific cities. The identification of the IV regressions relies on the idea that the effects of national contracting decisions vary across cities in an exogenous way – the instrument is focused on the interaction between cities' initial shares of contract expenditure in different industries and the national growth rates of expenditure in those industries. Thus, the shift-share instrument exploits the local impact of national expenditure decisions in order to rule out that city-specific shocks that may affect both federal contract expenditure and GDP or employment (like the loss of individual firms) explain our result. Using a related strategy, Nakamura and Steinsson (2014) posit that differences in the allocation of military spending between states is exogenous, even if the aggregate spending is endogenous.

We include two different sets of fixed effects. First, the city-level fixed effects, α_c, control for 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 the presence of a large automobile industry or a 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 co-located cities. State-year fixed effects also control for differences in business cycles (Domazlicky, 1980; Hess and Shin, 1998; Beraja, Hurst, and Ospina 2016). They as well account for political selection, particularly for pork-barrel practices, whereby elected representatives in Washington secure federal contracts for their home state (Larcinese et al., 2006; Cohen, Coval and Malloy, 2011), because the fixed effects absorb any state-level advantages in representation. With 300-500 cities in the sample (depending on which variables are included in the regression), the employment of state-year fixed effects facilitates a more effective ruling out of endogeneity related to political representation from any specific city than that which is possible via the use of national (time) fixed effects.

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. 1-digit NAICS-coded industries are used 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 $\hat{G}_{i,c,t} = G_{i,c,2005} * G_{US,c,t}/G_{US,c,2005}$. 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 for each metropolitan area yields a prediction of the aggregate spending by metropolitan area: $\hat{G}_{c,t} = \sum_i \hat{G}_{i,c,t}$. This projected spending reflects city-level spending as if in the city, given the initial pattern of contracts received, the growth in contract expenditure followed the national average. The projection still contains national changes, which are plausibly not exogenous. However, given the identification of subnational variation, national shocks to federal spending can be controlled for using (state-) annual fixed effects. Equally, the long-run industrial pattern of a city may be endogenous, but variation is eliminated using location fixed effects. The instrument thus effectively exploits the differential sensitivity of cities to contracting in each industry.

For the shift-share instrument to be relevant, some of the local changes in federal contracting need to be driven by national trends. Similarly, city-level variation in the initial conditions for the predicted total contracting expenditure changes is required. When decomposing the expenditure shares,[6] roughly 12% of the variation in city-sector-year-specific contracting expenditure shares is accounted for by sector-year fluctuations; 41% by location-specific factors (invariant to time and sector); and 48% by idiosyncratic elements. The instrument passes the relevance tests: it predicts city-level expenditure well, conditional on city and state-year fixed effects. One potential risk is that national trends perfectly predict the

[6] This is done as follows. s_{ict} is the share of contracting of sector *i* in the total contracting expenditure in city *c* at year *t*; s_{ic} is the over-time average contract expenditure share of sector *i* in city *c*; s_{it} is the across-city (national) average contract expenditure share of industry *i*; and $\tilde{s}_{ict} = s_{ict} - s_{ic} - s_{it}$ is the residual share. The variance decomposition is carried out by calculating $\frac{covar(s_{ict}, s_{ic})}{var(s_{ict})}$, $\frac{covar(s_{ict}, s_{it})}{var(s_{ict})}$ and $\frac{covar(s_{ict}, \tilde{s}_{ict})}{var(s_{ict})}$ respectively. The decomposition adds up to one (rounded percentages are reported).

developments of individual cities, if individual firms are very large or some industries are highly spatially concentrated. This, however, seems highly unlikely, as a fairly broad 1-digit classification is used. In 2014, the firm that secured the largest contract represented 2.4% of the contract value in its sector and the contract value share of the largest contract averaged 3.2% across industries. As a general rule, the share of funding of the largest contract in an industry was larger in smaller industries. The share was 0.6% in the largest industry and 6% in the smallest one. To the extent that specialized location patterns persist over time, the city fixed effects should absorb any small share of variation.

5. Results

5.1. ***Effects on output***

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). We report the results using federal contract spending measures lagged by one year.[7] Column 1 shows the results of the OLS regression. They indicate that GDP per capita in a given US city was $1.7 higher for every additional dollar awarded to a firm located in that city (recipient location) and $1.8 higher for every additional dollar spent (location of the activity). In order to rule out reverse causality, columns 2 to 5 summarize the instrumental variable regressions using the shift-share instrument as well as variations of the fixed effects structure. Across the whole range of results, instrumenting increases the estimated coefficient substantially. As the instrumentation eliminates the role of contract targeting by the federal government, the higher coefficients are consistent with a priority to award contracts to firms in poorer cities. The coefficients remain positive and significant, with exception of Column 4 in Table 2a. A

[7] In Appendix Table A1, we provide a set of regressions with different lag structures, which suggests that the lagged contract spending yields the best fit in terms of t-statistic on the coefficient, overall F-statistics, and residual sum of squares.

heteroskedasticity-robust Wu-Hausman test for endogeneity suggests that in the preferred specifications, our spending measures cannot be considered exogenous (though that can be the case in some specifications with a less stringent fixed effects structure). The fixed effects also have a significant impact on the point estimate. State-year fixed effects increase the coefficient estimate, which could be explained by the implementation of anti-cyclical federal expenditure policies. City (MSA)-level fixed effects, by contrast, reduce the coefficient. The substantial impact on the coefficient is consistent with the substantial share of time-invariant variation between cities discussed in section 4.1.

We run several diagnostic tests to ascertain the quality of the shift-share instrument. The Kleibergen-Paap (KP) Lagrange multiplier test – conditional on the fixed effects structure -- rejects underidentification for all models. The Kleibergen-Paap F-statistic suggests less than 10% IV coefficient bias in the recipient location model (Table 2a), judging by the Stock and Yogo values. The bias may, however, be over 25% for the preferred model for the location of the activity (Table 2b, column 6). Conditional on the fixed effects, the instrument based on the location of performance is weaker than the instrument based on the location of the recipient. In addition, we conduct two indirect tests for instrument exogeneity following Goldsmith-Pinkham et al. (2017). First, we test if pre-trends are parallel, to rule out that unobserved variables predict both a city's GDP developments and its (future) reception of contracts. We allow for serial correlation in the instrument and find that future values of the instrument do not predict second-stage residuals in our preferred specification (Table A1, Column 7), showing no sign of diverging pre-trends. Second, we isolate the principal components of inner-product variation in our instrument (the interaction of initial city-level contract expenditure shares in different industries and the national expenditure growth rates of those industries, conditional on location and time fixed effects) to test for overidentification. The results, reported and discussed in Appendix B, show no signs of overidentification.

The location of the awardee of the contract and the location of the activity itself are only introduced together in Table 2, Column 6. The straightforward approach to evaluate the two models (one based on the location of the recipient and the other on the place where the work was carried out) would be to estimate both measures jointly in all regressions. However, the close correlation between the two shift-share instruments (based on the same national growth rates) causes a weak instrument problem. To overcome this issue, we apply an artificial nest of the two instrumental variable regressions (Davidson and MacKinnon, 1981).[8] The p-value for the recipient location based prediction of GDP per capita is lower than 0.01 in the performance location-based regressions. That for the performance location is 0.609 in the recipient-based model. This implies that the recipient location-based measure encompasses the explanatory power of the other model, as corroborated by the OLS nest reported in Column 6, Table 2, where the coefficient for the recipient location is positive and significant, while that of the location of the activity is insignificant. Consequently, in the preferred estimate (Table 2a, Column 5), which includes city and state-year fixed effects, one dollar in contracting expenditure generates a revenue of $1.35 of GDP the year after the spending took place.

[Table 2 about here]

[8] First, the model is estimated with one of the location measures using the preferred specification (IV with fixed effects) to generate the predicted values (e.g., $\widehat{\left(\frac{GDP}{cap}\right)_{c,t}^{recipient}} = \hat{\beta}\left(\frac{Spending}{cap}\right)_{c,t}^{recipient} + \hat{\alpha}_c + \hat{\alpha}_{s,t}$). Second, we introduce the predicted values of one model into the preferred specification of the other. In the first case, we estimate $\left(\frac{GDP}{cap}\right)_{c,t}^{location} = \beta\left(\frac{Spending}{cap}\right)_{c,t}^{location} + \gamma\widehat{\left(\frac{GDP}{cap}\right)_{c,t}^{recipient}} + \alpha_c + \alpha_{s,t} + \varepsilon_{c,t}$ by IV (and vice versa for the other model). Encompassing is tested as $\gamma = 0$, with the null that the model introduced through the predicted values has no additional explanatory power over the model in which it was inserted.

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

To understand better the overall economic effect of federal contracts, we consider several other outcomes. Table 3 presents the results, using shift-share instrumentation and fixed effects. Column 1 of Table 3 reports the impact of federal contracts on private GDP, excluding government production. The coefficient of 1.16 points to a smaller impact than on general GDP (14% lower than the 1.35 coefficient in Table 2a, Column 5). However, as private GDP represents 86% of overall GDP, there is no evidence that federal contracts stimulate private production to a greater extent than public production, or vice versa. When using the location-of-performance measure, the impact is high, but this measure has no explanatory power when controlling for the spending measure based on the location of the recipient.

Column 2 of Table 3a reports a positive and significant impact of contract spending on population. The estimate implies that an expenditure of $175,000 in federal contracts attracts one additional migrant to the city where the money is allocated (Table 3b).

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[9] (Column 5) are considered. This result is reproduced when considering the location of performance (Table 3b, Columns 3-5).[10]

[Table 3 about here]

[9] We ran a regression using census microdata explaining individuals' log wages 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%.

[10] The population and wage data from the Community Surveys cover more cities than the GDP data from the Census. Re-estimating the wage and population model on the original, smaller Census sample gives similar results – see Appendix C.

The estimated impact of federal contract public expenditure may be biased if other governments in the same area adjust their spending in response. If local governments cut spending, for instance, an unobserved variable bias arises, leading to underestimate the output impacts of contracts. We examine the response of local governments in Appendix D. Local governments barely increase their spending if their city receives a contract, suggesting little role for bias.

5.3. ***Effects on employment***

Job generation is cited as another reason for local decision-makers to pursue federal contracts for their city. In Table 4, we apply the same framework as in Table 2 to explore the impact of federal contracting on urban employment in the US.[11] Once again, we artificially nest the relevance of the location of the recipient of the contract and that where the activity actually 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 that of the recipient. The nesting test also suggests that the recipient-location model has no explanatory power in addition to the location of activity model. The employment effects are statistically significant in all estimations. Employment generation operates through both getting the unemployed to work (extensive margin) and making those employed work more hours (intensive margin). Most of the impact on jobs happens, however, through the intensive margin, that is, through

[11] The dataset used in the employment analysis is larger than that used in the GDP analysis, as data for jobs are available for more metropolitan areas. In any case, the results hardly vary if the employment regressions are conducted for the exact same sample used in the GDP analysis. The regressions for this smaller sample are reported in Appendix C.

additional work for those already in employment rather than through the creation of new jobs – 60% of all additional work goes to already employed workers, while the share of hours accounted for by previously unemployed individuals is limited to 40%.

The row 'cost per jobs' calculates the overall cost of an additional FTE job (40 hours of work per week, times 45 weeks). The results stress that more than $247,000 in federal contracting are needed to create a new full-time job at the location of the activity along the extensive margin – moving unemployed workers into employment. That is not far off the migration response of one citizen per roughly $175,000. The extensive margin job cost rises to more than $518,000 at the location of the recipient firm (Table 4, Column 1). By contrast, incorporating both the external and internal margins (i.e. measuring as well the increase in hours for those already in employment), an additional FTE job is created for every $68,000 spent at the location of the activity, or every $164,000 spent 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 in employment than at generating new jobs for those unemployed or entering the market for the first time.

[Table 4 about here]

5.4. ***Spatial and time effects***

How big is the spatial reach of the economic impact of federal contracting? A contract in one place may induce additional demand in other locations. Failing to consider this would bias the 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 us suppose, for instance, that contracting in one city draws resources away 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: first, in the higher GDP in the city benefitting from the contract and, second, in other cities experiencing a downturn as a result.

[Table 5 about here]

The potential spatial effects of federal contract expenditure can be identified by considering the GDP impacts of federal expenditure in nearby locations. We assume the impacts decay with distance, although we have no theoretical prior about the exact extent of the spillover effect. We consider three dimensions of geographical distance. First, we draw concentric rings around each city included in the analysis, with a 200 km radius. We aggregate expenditure in federal contracts within these rings (the measures are mutually exclusive, meaning that the outer rings can be considered as "doughnuts"). This is our preferred measure, as, given US geography and the distance between the cities included in the analysis, all cities considered have at least one neighboring city in the first concentric ring . Secondly, we follow the same procedure for three smaller rings: using 50 km incremental radiuses in each additional ring. Third, we estimate the local economic impacts of federal contracting taking place in a) the five nearest cities; b) the next five nearest cities; and c) the cities ranked between ten and fifteen in terms of distance. This approach takes into account that relative, rather than absolute, distance may determine the impacts of federal contracts.

Columns 1, 2 and 3 in Table 5 report the IV regressions that explain GDP per capita in a city by contract expenditure in surrounding cities. Column 1 shows the model with contract expenditure in concentric rings of 200 km radius each. The coefficient for local expenditure is slightly higher than in the baseline case, but federal contracting at a distance of between 400 and 600 km from the city is associated with slightly lower local GDP per capita. In order to compare the aggregate multiplier, we simulate an added dollar of expenditure per capita for every city, and calculate the aggregate GDP increase (including spatial effects). For the preferred spatial model in column 1, the aggregate multiplier is only marginally higher than the baseline estimate of 1.35. A joint F-test of the ring variables suggests that they are significant, although the introduction of expenditure in rings surrounding the city does not affect the overall results. [12]

Columns 2 and 3 reproduce the exercise for rings with 50 km radiuses and groups of nearest cities respectively. They also show significance of the spatial variables from a joint F-test. The aggregate multipliers are comparable to column 1 (if somewhat higher when using the 50km rings). The Table also reports an overall model F-statistics as a goodness-of-fit measure conditional on the fixed effects included in the model. The F-statistics are comparable across specifications.

For reference, column 4 of Table 5 repeats the baseline model using OLS and no fixed effects. Again, the coefficient estimates are higher in this case and the ring variables are significantly associated with local GDP, suggesting the presence of spillovers. Avoiding dealing with causality leads, however, to different outcomes. In particular, the coefficient for expenditure in surrounding rings become positive, while in the model addressing endogeneity, they are zero or negative. A potential explanation for this difference is the targeting of contracts to areas with higher GDP, which surfaces in column 4, but not column 1. Columns 5 and 6 test alternative specifications. Column 5 reports a model with per capita expenditure in the nearest cities.[13] Column 6 reports expenditure measures in the rings based

[12] The shift-share instruments seem relevant for the ring they were created for. Tables F1 and G1 in Appendix, show that i) the correlations between instruments for rings of different ranges are not high and ii) that instruments for individual rings predict contract expenditure in their respective ring, conditional on the other instruments.

[13] This model is estimated exclusively for the nearest-city sample, because, by construction, all cities in this sample have observations for per capita federal contract spending in neighboring areas. This is not the case, for example, when considering observed federal contracting in concentric 50 km radius rings. A considerable

on the location of activity rather than on the location of the recipient. Both point to somewhat higher impacts from federal contracting expenditure at close range or inside the city. The impact is, in contrast, negative the greater the distance. The aggregate multiplier is comparable to previous results.

The presence of spatial autocorrelation (conditional on fixed effects) can bias the estimates. Regressing federal contract expenditure per capita in a metropolitan area on contract expenditure per capita taking place within a 200 km radius only hints at small impacts from the contracting in surrounding areas (coefficient 0.03). A Moran's I plot based on inverse distance shows that for a given city, the inverse-distance weighted average expenditure of other cities cannot explain contract expenditure (the statistic is 0.005) (Figure E1 in Appendix).

To examine whether spatial dependence affects the baseline estimates, we introduce spatial lags of GDP per capita, using normalized inverse distance as weights. The model is estimated by spatial two stage least squares, which can accommodate an additional endogenous variable as well as the fixed effects. The spatial lag model in column 7 of Table 5 can be compared to the OLS model in Column 1 of Table 2 (coefficient: 1.73), which was run without fixed effects or shift-share instrumentation. The significant coefficient on the spatial lag insinuates the existence of potential indirect effects of expenditure. However, when introducing the shift-share instrument and the fixed effects structure in Column 8, the spatial lag is statistically insignificant and the estimated effect of contract expenditure hardly changes after allowing for a spatial autoregressive term.[14]

number of US metropolitan areas are not located within a 50 km distance from another city. This would lead to considerable difficulties in interpreting the coefficients.

[14] This is not entirely surprising. The spatial lag model in matrix form is $\frac{\boldsymbol{GDP}}{\boldsymbol{cap}} = \rho \boldsymbol{W} \frac{\boldsymbol{GDP}}{\boldsymbol{cap}} + \beta \frac{\boldsymbol{Spending}}{\boldsymbol{cap}} + \boldsymbol{\alpha_c} + \boldsymbol{\alpha_{st}} + \boldsymbol{\varepsilon}$, where the bold face indicates column vector for variables; $\boldsymbol{W}$ denotes the weights matrix and ρ is the spatial autoregressive coefficient. In reduced form, this is $\frac{\boldsymbol{GDP}}{\boldsymbol{cap}} = [I - \rho \boldsymbol{W}]^{-1} \left[\beta \frac{\boldsymbol{Spending}}{\boldsymbol{cap}} + \boldsymbol{\alpha_c} + \boldsymbol{\alpha_{st}} + \boldsymbol{\varepsilon}\right]$, which effectively adds distance-weighted observations of spending per capita to the non-spatial specification, instead of aggregate expenditure. The

What about the time dimension? Table A1 in Appendix reports the baseline regression using different time lags. The individual lags in isolation (Table A1, Columns 1-3) display positive coefficients that fade over time. Judging by the overall F-statistic, the individual fit is best for the 1-year lag, that is, one year after the expenditure linked to the contract happens (Table A1, Column 2). Introducing simultaneously the no-lag and the 1-year lagged measure in column 4 leads to a relatively high coefficient for the 1-year lag variable. The coefficient for the no-lag variable is, by contrast, reduced in comparison to those when the different time lags are regressed in isolation from one another. Although it could be argued that the instrument between the two expenditure measures may be correlated, the joint Cragg-Donald F-statistic of 9.80 suggests no problem of weak instruments (the 10% max IV size critical F-statistic is 7.03). Adding more lags (Table A1, Column 5-6) renders all coefficients insignificant. Similar regressions examining 3-year lags (unreported) show the same pattern. 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.

6. Discussion

The analysis is broadly consistent with earlier findings on the magnitude of multipliers, but suggests that the wage and employment effects of federal contracts are much lower than reported by previous literature.

Our multiplier estimates of 1.4 to 1.6 are within the bounds set by earlier studies that, utilizing cross-state variation, arrive at multipliers of between 1.1 and 1.7 (Fishback and Kachanovskaya 2010; Nakamura and Steinsson; 2014; Suárez Serrato and Wingender 2016). They are also relatively close to the multiplier of 1.6 assumed for certain public policies, like

specification is thus reminiscent of the results in Table 5, Column 4 (spending per capita in concentric rings), which showed no significant spatial effects.

the 2009 fiscal stimulus package (Romer and Bernstein, 2009). We also find evidence to suggest that output effects often occur at the location of the recipient firm and not where the expenditure was projected, casting doubt on whether federal contracts can be accurately targeted. The results, moreover, imply that the impact is spatially limited and short-lived.

The results show that federal contracts are far less effective at creating jobs in cities than other forms of spending (e.g. Greenstone et al., 2010; Ham et al., 2011; Hanson and Rohlin, 2013; Busso, Gregory and Kline, 2013). With one full-time job created for every $68,000 (and one person moving from unemployment to employment for every $250,000), the job cost estimates tower not only over those of dedicated job-creation programs (e.g. Criscuolo et al., 2012), but also over estimates for overall government spending (e.g., Shoag, 2013; Chodorow-Reich et al., 2012; Suárez Serrato and Wingender, 2016). Most employment effects occur at the intensive margin: e.g., the expansion of the hours worked by those already in employment. This is not be surprising, as it is likely that firms will grow to consider federal contracts as incidental demand shocks, for which they do not want or deem reasonable to expand their workforce.

Our estimates also provide little in the way of evidence for a substantial wage impact. This is a notable point of divergence between our research and previous investigations into the returns of incentives, subsidies, and grants (e.g. Greenstone et al., 2010; Ham et al., 2011; Hanson and Rohlin, 2013; and Busso, Gregory, and Kline, 2013).

The employment impacts are concentrated at the location of contract implementation, rather than at the location of the recipient firm. This mirrors the impacts on output, which occur at the location of the recipient firm. There are two potential explanations for this. First, as indicated by Feyrer and Sacerdote (2011), while employment – i.e. the work itself – usually takes place where the public contract is conducted, inputs – the materials needed for the construction of a road, for example – can be procured from anywhere and may, in particular, be sourced from suppliers near the recipient firm. That can undermine the output effects at the location of performance. Second, the result may be due to accounting differences between the employment and GDP data. The recipient firm may hire workers at the location where the work is executed, but the value added ends up in the accounts of the firm, placing the output effect at the recipient firm's location. As our employment estimates stem from US census microdata and hence do not rely on firm-level data, the employment impacts may be registered where the contracted work is performed. Unfortunately, the data do not allow testing these two contrasting explanations.

7. Conclusions

In the US, an average of $1,500 per individual is spent in federal contracts every year. The contracts fund the federal acquisition of goods and services, but they are also regarded by some as tools for economic development. Consequently, the competition among urban and local decision-makers for these contracts is intense. But do federal contracts really catalyze urban economic growth and employment? This question has attracted far less attention than it deserves given the huge sums of money that are channeled in the direction of federal contracts (more than $400 billion per year, which easily exceed the more commonly assessed territorial policies, such as grants, subsidies, and zoning).

This paper has addressed this gap in our knowledge and analyzed the job and output impacts of federal contact expenditure for a panel of more than 300 cities. We address confounding explanations, such as the targeting of the funds, via a spatial fixed effect strategy with shift-share instrumentation. The results suggest that federal contract expenditure has a modest impact on output. The multiplier ranges between $1.4 and $1.6 in GDP for every contract dollar spent. The impact is also short-lived and spatially limited. A new full-time equivalent job is created for every $68,000 in expenditure, but two thirds of this impact comes from increasing the working hours for those already in employment, rather than from the creation of entirely new jobs. For every $250,000, one unemployed worker is employed. The output effects of contracts occur mainly at the location of the firm that receives the contract, while the employment effects tend to manifest themselves at the location of contract execution, which may be a different location.

Overall, the results of the paper raise reasonable questions about the potential of federal contracts as tools for the promotion of economic change. While our estimates point to modest output effects, the employment impacts are limited in absolute terms, and relative to other literature. This is consistent with the aim of federal contracts. Federal contracts, in contrast to incentive and development programs, are simply not granted with a view to impel development. They are not designed as instruments for the promotion of economic progress and dynamism. City governments may pursue federal contracts to cultivate local economic activity or deliver votes, but in light of our results, particularly on employment effects, a reliance on federal contracts seems an ill-advised economic development policy when contrasting their impacts to the costs.

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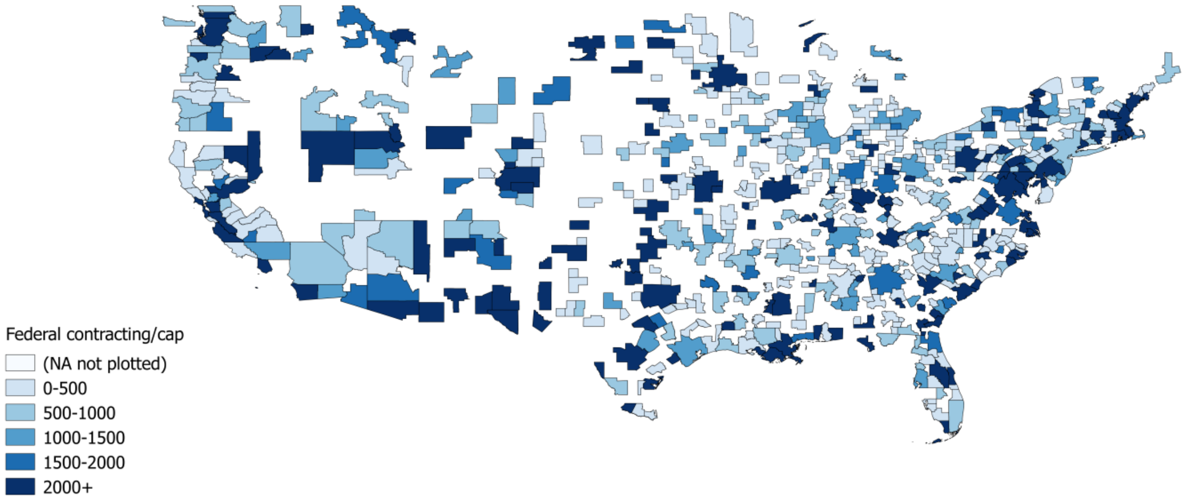
FIGURES AND TABLES IN THE MAIN TEXT

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



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

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

Figure 2a

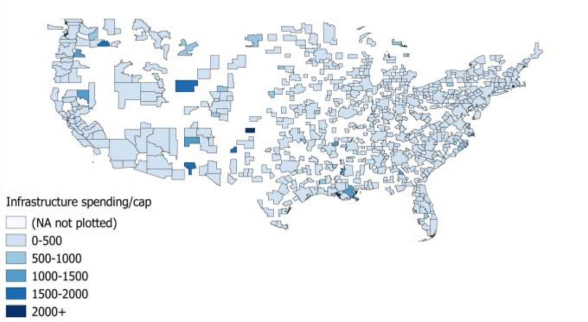
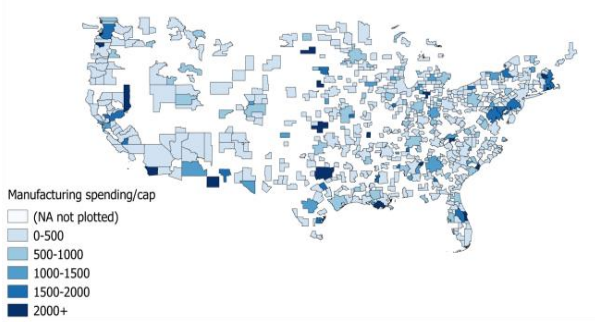
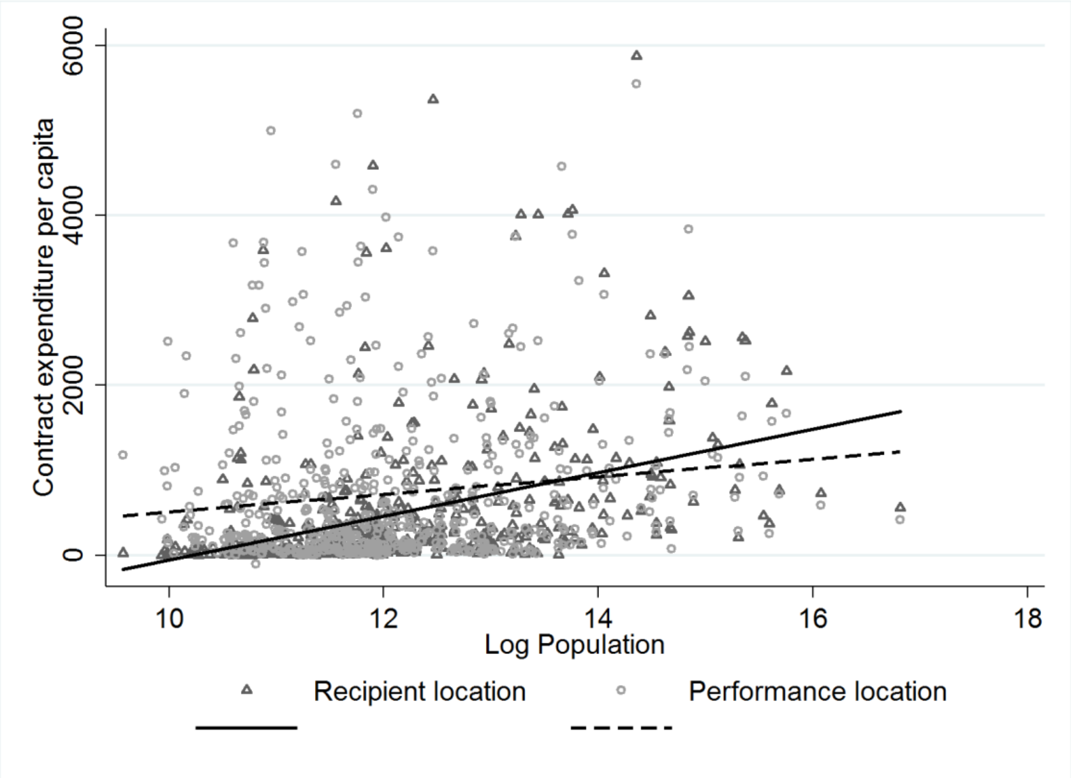


Figure 2b



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

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



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

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

Note: sources: expenditure measures stem from the Bureau of Fiscal Services (Dept. of Treasury), combined with population data from the US Census Bureau. GDP in real terms is extracted from the US Bureau of Economic Analysis. Employment is the shared of employed in the labor force. Wage (nominal) and employment data are from author elaborations using data from the American Community Surveys.

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***	3.146***	0.707	1.352***	1.639***
	(0.141)	(0.201)	(0.274)	(0.444)	(0.491)	(0.380)
Location of the activity						
Contract spending/cap$_{t-1}$						0.00577
						(0.234)
Kleibergen-Paap LM		13.27	48.18	13.72	27.29	
p-value		0.00	0.00	0.00	0.00	
Wu-Hausman p-value		0.00	0.00	0.12	0.00	
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***
						(0.579)
Location of the activity						
Contract spending/cap$_{t-1}$	1.812***	1.776***	1.553***	2.870***	6.001**	0.00577
	(0.252)	(0.280)	(0.220)	(0.663)	(2.447)	(0.356)
Kleibergen-Paap LM		23.80	107.5	18.46	6.203	
p-value		0.00	0.00	0.00	0.01	
Wu-Hausman p-value		0.74	0.55	0.00	0.01	
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. Kleibergen-Paap LM is the Lagrange multiplier test. Wu-Hausman p-value is the p-value of the heteroscedasticity-robust test for exogeneity of the spending measure (the null is exogeneity).*** p<0.01, ** p<0.05, * p<0.1

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

3a. 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$_{t-1}$	1.157***	23.18**	0.214	2.85e-06	-3.04e-06
	(0.439)	(10.82)	(0.241)	(7.96e-06)	(4.98e-06)
Kleibergen-Paap LM	27.29	27.99	24.90	24.90	24.90
p-value	0.00	0.00	0.00	0.00	0.00
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 IV	population IV	wage IV	log wage IV	purged log wage[a] IV
Location of the activity					
Contract spending/cap$_{t-1}$	5.041**	-4.844	0.277	5.54e-06	1.66e-05
	(2.117)	(16.86)	(0.690)	(2.81e-05)	(2.56e-05)
Kleibergen-Paap LM	6.203	0.769	0.618	0.618	0.618
p-value	0.01	0.38	0.42	0.42	0.42
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. Kleibergen-Paap LM is the Lagrange multiplier test. Wu-Hausman p-value is the p-value of the heteroscedasticity-robust test for exogeneity of the spending measure (the null is exogeneity).*** p<0.01, ** p<0.05, * p<0.1.

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

Table 4. Effects of federal contracting on jobs.

4a. Recipient location	(1) employment IV	(2) weeks (conditional[a]) IV	(3) weekly hours (conditional[a]) IV	(4) total hours (unconditional[a]) IV
Recipient location				
Contract spending/cap$_{t-1}$ ($1,000)	0.00311**	0.0343	0.133***	10.97**
	(0.00125)	(0.0409)	(0.0429)	(4.456)
Kleibergen-Paap LM	30.22	30.22	30.22	30.22
p-value	0.00	0.00	0.00	0.00
p-value Wu Hausman	0.01	0.27	0.00	0.01
Cost per job ($1,000 US)	518.3	2,226	508.0	164.1
s.e. ($1,000 US)	208.0	2,656	163.1	66.64
Observations	5,194	5,194	5,194	5,194
state-year FE	Yes	Yes	Yes	yes
metropolitan FE	Yes	Yes	Yes	yes

4b. Location of the activity	employment IV	weeks (conditional[a]) IV	weekly hours (conditional[a]) IV	total hours (unconditional[a]) IV
Location of the activity				
Contract spending/cap$_{t-1}$ ($1,000 US)	0.00652**	0.217**	0.189*	26.47**
	(0.00298)	(0.107)	(0.109)	(12.36)
Kleibergen-Paap LM	6.701	6.701	6.701	6.701
p-value	0.01	0.01	0.01	0.01
p-value Wu Hausman	0.00	0.00	0.01	0.00
Cost per job ($1,000 US)	247.2	352	359.5	68
s.e. ($1,000 US)	112.9	174.1	207.4	31.76
Observations	5,194	5,194	5,194	5,194
state-year FE	yes	yes	yes	yes
metropolitan FE	yes	yes	yes	yes

Regressions weighted according to the location's population. Kleibergen-Paap LM is the Lagrange multiplier test. Wu-Hausman p-value is the p-value of the heteroscedasticity-robust test for exogeneity of the spending measure (the null is exogeneity). Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. [a] (Un)conditional on already in employment.

Table 5. Spatial effects of contracting.

	(1) GDP/cap IV	(2) GDP/cap IV	(3) GDP/cap IV	(4) GDP/cap OLS	(5) GDP/cap IV	(6) GDP/cap IV	(7) GDP/cap S2SLS
Recipient location							
($ mln, t-1)							
Contract spending	1.487***	1.629***	1.640***	1.736***	1.879***	1.953***	1.683***
	(0.560)	(0.593)	(0.581)	(0.140)	(0.679)	(0.675)	(0.153)
Contract spending 0-200km	0.0400			0.0230			
	(0.0269)			(0.0282)			
Contract spending 200-400km	0.0179			0.0638***			
	(0.0326)			(0.0186)			
Contract spending 400-600km	-0.0369**			0.0703**			
	(0.0186)			(0.0346)			
Contract spending 0-50km		0.125					
		(0.908)					
Contract spending 50-100km		-0.0832					
		(0.0661)					
Contract spending 100-150km		0.111**					
		(0.0434)					
Contract spending 1-5 nearest			0.0774**				
			(0.0360)				
Contract spending 6-10 nearest			-0.0628				
			(0.0538)				
Contract spending 11-15 nearest			-0.0606				
			(0.0409)				
Recipient location							
(per capita, t-1)							
Contract spending 0-200km					0.610*		
					(0.319)		
Contract spending 200-400km					-0.309		
					(0.248)		
Contract spending 400-600km					-0.654*		
					(0.361)		
					(0.0425)		
Location of activity							
($ mln, t-1)							
Contract spending 0-200km						0.178**	
						(0.0827)	
Contract spending 200-400km						0.0380	
						(0.0826)	
Contract spending 400-600km						-0.135**	
						(0.0542)	
W GDP/cap							0.244**
							(0.098)
Observations	3,015	3,015	3,015	3,015	3,015	3,015	3,015
state-year FE	yes	yes	yes	no	yes	yes	
MSA FE	yes	yes	yes	no	yes	yes	
F stat overall	2.481	2.649	1.757	49.59	1.776	3.148	
F-stat rings	0.0283	0.0147	0.0902	0.000634	0.0916	0.00185	
Aggr. multiplier	1.411	1.733	1.483	3.601	1.527	1.524	

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

APPENDICES

Appendix A

Table A1. *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	(7) $\widehat{\text{GDP/cap}}$ OLS
Recipient location						
Contract spending/cap_t	1.052*			0.438	0.826	
	(0.623)			(0.755)	(0.871)	
Contract spending/cap_{t-1}		1.418**		1.456	0.948	
		(0.633)		(0.988)	(1.130)	
Contract spending/cap_{t-2}			1.004*		1.232	
			(0.565)		(1.045)	
Contract spending/cap_{t-3}						
Contract spending/cap_{t+1}						-0.048
						(0.079)
Observations	2,648	2,648	2,648	2,648	2,648	
state-year FE	yes	yes	yes	yes	yes	
MSA FE	yes	yes	yes	yes	yes	
F-stat	2.08	3.67	2.31	0.87	0.81	
Kleibergen-Paap LM	26.06	26.96	25.35	23.25	15.85	
MSS (10^{-9})	33.8	31.7	122.4	33.7	34.2	

Regressions weighted according to the location's population. F-stat is the F-statistics of the regression conditional on partialling fixed effects. Kleibergen-Paap LM is the Lagrange multiplier test. RSS is the residual sum of squares. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Appendix B

Table B1. *Overidentification tests based on principal component analysis*

Effects on GDP per capita for different sets of principal components as instruments

	(1)	(2)	(3)	(4)	(5)	(6)
	GDPcap	GDPcap	GDPcap	GDPcap	GDPcap	GDPcap
	IV	IV	IV	IV	IV	IV
	PCA1	PCA1-2	PCA1-3	PCA1-4	PCA1-5	PCA1-6
Contract spending/cap$_{t-1}$	1.325	1.411***	1.403***	1.373***	1.293***	1.247***
	(1.213)	(0.487)	(0.447)	(0.434)	(0.393)	(0.392)
Observations	3,015	3,015	3,015	3,015	3,015	3,015
state-year FE	yes	yes	yes	yes	yes	yes
MSA FE	yes	yes	yes	yes	yes	yes
Cumulative factor prop	18%	28%	39%	49%	58%	68%
Kleibergen-Paap LM	1.512	17.15	17.76	17.77	19.61	20.96
p-value	0.219	0.000189	0.000492	0.00137	0.00148	0.00186
Hansen- J		0.00451	0.00712	0.502	0.545	0.917
p-value		0.946	0.996	0.919	0.969	0.969

Standard errors clustered at MSA level in parentheses
*** p<0.01, ** p<0.05, * p<0.1

The estimates summarized by Table A5 reflect the procedure outlined by Goldsmith-Pinkham et al. (2017), by using the components of the inner product (the interaction between locational contract expenditure shares and national contract expenditure growth rates) to test for overidentification of a Bartik-type instrument. The procedure benefits from the fact that all individual shift-share predictors (each MSA's initial contracting in a sector multiplied by the national contracting growth in that sector) are individual instruments. Employing all these instruments leads to a weak instrument problem, but when the number of instruments is reduced through principal component analysis, the components can be used to test for overidentification of the shift-share instrument. We, therefore, run a principal component analysis on the projected contracting per capita, using the MSA-year as observation and the sectoral expenditure as variables. This allows us to extract components that predict fluctuations in groups of categories of federal contract expenditure per capita. The extracted components are then used as instruments, instead of the direct Bartik instrument, and in a J-test of overidentification.

The first column shows the baseline regression with the first principal component as an instrument. The first component explains most of the variation in the original shift-share instrument: its factor proportion is roughly 18%. Using only this variation, the null of underidentification (of the Kleibergen-Paap Lagrange multiplier test) cannot be rejected. Since there is a single instrument, overidentification cannot be tested. Adding the second principal component substantially increases the relevance of the instrument set, rejecting underidentification. The null of the Hansen J test (validity of the overidentifying restriction) cannot then be rejected. When subsequently extending the instrument set by iteratively including the next component with most explanatory power, this pattern persists.

Appendix C

Table C1. *Alternative indicators in the GDP sample*

Effects of contracts on other outcomes

C1a. *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$_{t-1}$	1.157***	29.11*	0.259	4.91e-06	8.99e-07
	(0.439)	(15.23)	(0.293)	(9.71e-06)	(5.94e-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
Kleibergen-Paap LM	27.29	19.80	19.80	19.80	19.80
p-value	0.00	0.00	0.00	0.00	0.00

C1b. *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$_{t-1}$	5.041**	-7.972	0.781	2.23e-05	2.35e-05
	(2.117)	(22.86)	(0.860)	(2.64e-05)	(3.10e-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
Kleibergen-Paap LM	6.203	0.732	0.732	0.732	0.732
p-value	0.0128	0.392	0.392	0.392	0.392

Regressions weighted according to the location’s population. Kleibergen-Paap LM is the Lagrange multiplier test. 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 C2. Effects of federal contracting on jobs in the GDP sample.

C2a. 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$_{t-1}$ (1,000$)	0.00323**	0.0501	0.139***	12.09**
	(0.00130)	(0.0429)	(0.0450)	(4.695)
Kleibergen-Paap LM	27.29	27.29	27.29	27.29
p-value	0.00	0.00	0.00	0.00
p-value Wu Hausman	0.02	0.16	0.00	0.01
Cost per job ($1,000 US)	499.6	1521	487.1	148.8
s.e. ($1,000 US)	200.7	1301	157.5	57.79
Observations	3,015	3,015	3,015	3,015
state-year FE	Yes	Yes	Yes	yes
metropolitan FE	Yes	Yes	Yes	yes

C2b. 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$_{t-1}$ ($1,000)	0.00653**	0.225**	0.209*	27.60**
	(0.00310)	(0.113)	(0.119)	(13.22)
Kleibergen-Paap LM	6.203	6.203	6.203	6.203
p-value	0.01	0.01	0.01	0.01
p-value Wu Hausman	0.00	0.00	0.01	0.00
Cost per job ($1,000 US)	246.9	338.8	324.9	65.23
s.e. ($1,000 US)	117.1	170.4	185.3	31.25
Observations	3,015	3,015	3,015	3,015
state-year FE	yes	yes	yes	yes
metropolitan FE	yes	yes	yes	yes

Regressions weighted according to the location's population. Kleibergen-Paap LM is the Lagrange multiplier test. Wu-Hausman p-value is the p-value of the heteroscedasticity-robust test for exogeneity of the spending measure (the null is exogeneity). Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. [a] (Un)conditional on already in employment.

Appendix D

Local government responses

Local governments may adjust their own expenditure decisions if a firm in their city receives a federal contract. Federal expenditure may be used as a substitute for certain types of local government expenditure, or local governments may facilitate the contracted activities. The response of local governments is important for identification: local government spending might form an unobserved variable that correlates to contract expenditure as well as GDP.

To address this concern, we examine local government spending responses to the awarding of a federal contract. We use the Historical Finance database for county governments from the Census, which runs up to 2012. We aggregate the local government expenditure of counties 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 D1. Effects on county government current expenditure per capita (2005-2012).

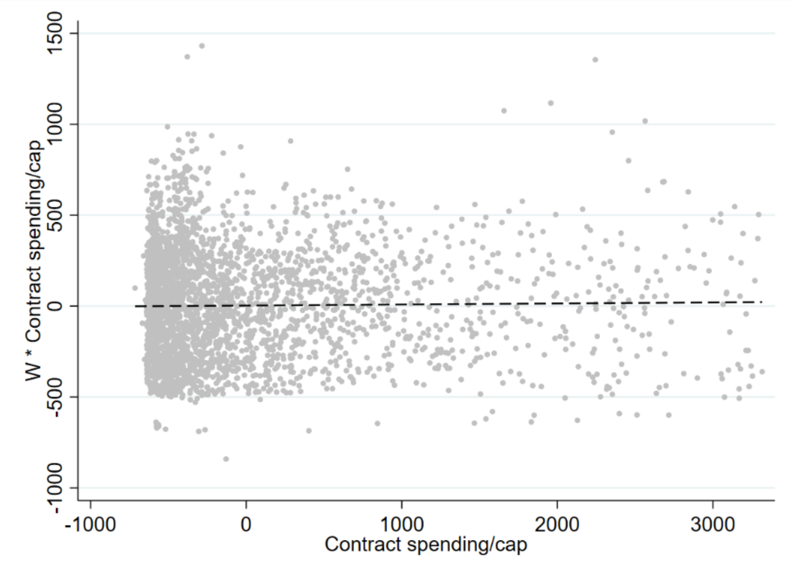
	(1) County expenditure per cap	(2) County expenditure per cap	(3) County expenditure per cap	(4) County expenditure per cap IV	(5) County expenditure per cap IV
Recipient location					
Contract spending/cap$_{t-1}$	0.0265***	0.0381***		0.0303**	
	(0.00708)	(0.00862)		(0.0119)	
Location of the activity					
Contract spending/cap$_{t-1}$			0.00149		0.0292
			(0.00808)		(0.06161)
Kleibergen-Paap LM				17.91	4.02
p-value				0.00	0.04
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. Kleibergen-Paap LM is the Lagrange multiplier test. Wu-Hausman p-value is the p-value of the heteroscedasticity-robust test for exogeneity of the spending measure (the null is exogeneity). Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table D1 reports the results of resorting to the methodology with local government expenditure as an outcome. The OLS regression of Column 1 suggests that each dollar linked to a federal contract at recipient location increases county-level expenditure by less than 3 cents. In the sample of our baseline analysis, it is under 4 cents (Column 2), and insignificant when examining expenditure at the location of performance. Applying instrumentation and fixed effects, we find an accommodation of 3 cents for every dollar (recipient location, Column 4) and insignificant results for dollars at the location of activity (Column 5). Overall, the response of county government current expenditure to local federal contracting is limited, and the confounding effects of local government expenditure seem minor.

Appendix E

Figure E1. *Moran's I plot for Contract Expenditure per capita based on location of recipient (main sample)*



Appendix F

Table F1. *Correlation matrix for the Bartik (shift-share)*

	Instrument local	Instrument 0-200km	Instrument 200-400km	Instrument 400-600km
Instrument local	1			
Instrument 0-200km	0.0417	1		
	(0.0406)			
Instrument 200-400km	-0.0089	0.1907	1	
	(0.0272)	(0.0000)		
Instrument 400-600km	0.0045	0.0844	0.2468	1
	(0.6793)	(0.0000)	(0.0000)	

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

Appendix G

Table G1. *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***	-3,564.61	5,470.09	2,866.97
	(0.00)	(4,310.61)	(7,128.30)	(7,768.91)
	62.64	**-0.83**	**0.77**	**0.37**
Instrument 0-200km	-0.00	0.04***	0.01***	-0.00*
	(0.00)	(0.00)	(0.00)	(0.00)
	-1.40	**107.01**	**7.98**	**-1.78**
Instrument 200-400km	-0.00	0.00***	0.04***	0.00***
	(0.00)	(0.00)	(0.00)	(0.00)
	-0.37	**7.15**	**103.12**	**4.95**
Instrument 400-600km	0.00	0.00	0.00***	0.05***
	(0.00)	(0.00)	(0.00)	(0.00)
	0.34	**1.09**	**5.97**	**103.70**
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 G1 shows a seemingly unrelated regression. 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 G1 show that the instrument for every ring predicts it spending measure well. That holds, conditional on adding the other instruments. Furthermore, based on the t-statistics, the relevant instrument seems to be the dominant explanatory variable – for instance, column 2 shows that the instrument for the 0-200km range has a t-statistics of over 100, while the other instruments have a t-statistics smaller than 10.