The Innovation Dividend of Defense Spending in US

Juarros P. Muratori U. Valderrama D.*

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Abstract

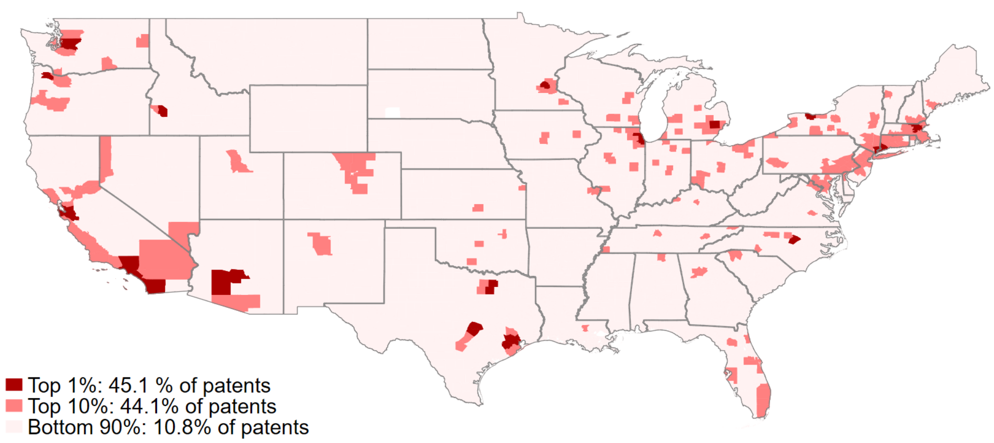
Innovation, a widely acknowledge determinant of economic growth, is disproportionately concentrated in few large-productive cities. Can fiscal policy spur innovation to lagged economic areas? This paper answers this question using contract level data on defense spending that identifies the R&D component of each dollar spent by the department of defense between 1980-2010. We find that defense spending does not increase the number of patents registered but it increase the number of citations of those patents. This results are stronger in middle-sized cities, suggesting that innovation spending alone can not spur innovation, it requires to be targeted to places that have complementary amenities.

JEL Codes: R10, G20, H10

1 Introduction

Economic growth in United States has been uneven across the territory. Since 1980s, few super-star cities have been experiencing larger growth rates in employment, investment, and output than the rest of the country. These trends evoked a "*winner-takes-most*" dynamics, where regions left behind are unable to attract investment and create a virtuous circle of economic growth. Some studies have linked this dynamics to the digital technologies and innovation (Atkinson et al., 2019; Eckert et al., 2019). These cities host the bulk of the high-tech clusters, and as consequence develop a significant fraction of U.S. innovation. Figure 1 shows that the top 1% of the U.S counties account for 45.1% of all the patents registered between 2001 and 2013 (7 millions of patents), while the bottom 90% account for only one-tenth of the patenting activities.[1]

Figure 1: Geographic Distribution of Patents by Percentiles



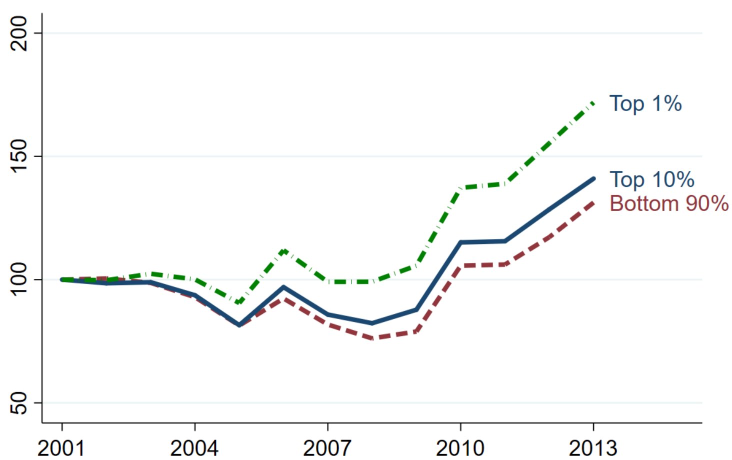
Note: Data are collected from the USPTO. The percentiles are defined based on the the distribution of total number of patent over the period 2001-2013.

The concentration of innovation in super-star cities has been increasing in the

[1]This facts have led to an intensive policy debate by different think tanks such as Brookings (here, here, and here) and the Equitable Growth (here, here, and here).

last two decades. During 2001-13, the production of patents in the top 30 most innovative counties grew by 75%, while for the bottom 90% of the counties, the growth of patenting has been below 50% (see Figure 2). The increasing trend in output and innovation divergence has been related to other pressing problems like the increase in wage inequality (Moretti, 2013; Eckert et al., 2019),[2] the fall in intergenerational mobility (Bell et al., 2019; Chetty and Hendren, 2018)[3] and the current upsurge of political polarization (Autor et al., 2016).[4]

Figure 2: Growth Rates of Patents by Percentiles



Note: Data are collected from the USPTO. The percentiles are defined based on the 2001 distribution of patents. All values are indexed using the 2001 number of patents of the respective percentile.

Place-based policies have been proposed as a policy tool to level the field and boost economic activity and innovation in economic distressed areas. This policies

[2]Eckert et al. (2019) shows that 30% of the increase in wage inequality is due to skilled-tradeable sector in superstar cities.

[3]Chetty and Hendren (2018) finds that the characteristics of the county where you born matters for your adulthood outcomes. In the same vein, Bell et al. (2019) finds that childhood exposure to innovation increase the probability of becoming an inventor.

[4]Autor et al. (2016) finds that local labor markets more affected by import competition from China prefer less moderate candidates, i.e increase the political polarization. Moreover, Rodrik (2018) finds that places that where most affected by globalization shocks have vote more for populist governments in Latin America (left-wing populist) and Europe (right-wing populist).

can be justified in the presence of agglomeration economies and spatial misallocation that responds to frictions in the labor and housing market (Kline and Moretti, 2014; Hsieh and Moretti, 2019; Ganong and Shoag, 2017).[5]

However, the rise of the spatial concentration in innovation can be seen as a threat to placed-based policies. Since innovation is the main determinant of long-run economic growth, place-based policies need to incubate innovation outside the super-star cities, otherwise it will be difficult for them to generate a *big push*. National fiscal policy can affect this geographical concentration through spending and taxes. How does federal fiscal policies affect local innovation and long-run economic growth? Can place-based policies reverse the trend of innovation-concentration by generating new innovation clusters? This paper aims to contribute to answer these questions by exploring the effects of a local fiscal stimulus in the form of defense spending on both innovation and economic growth. Defense spending is a large, discretionary and R&D-intensive policy instrument. This fact has been used by the macroeconomics literature to estimate the size of fiscal multipliers. However, with the exception of Akcigit et al. (2017), there is no evidence about how this demand shock can affect innovation, and subsequently, the persistence of growth after the fiscal resources are gone.

This paper combines several administrative records. First, we collect a rich contract level data on defense spending. It is used to measure the amount and innovation-intensity of defense purchases at county level.[6] Second, we use county-level measures of innovation in terms of outputs (number of patents) and inputs (number of science-related jobs). Based on these measures we will be able to explore

[5]Agglomeration economies make the social returns of these policies higher than the private returns, this make desirable for the government to invest in places with high agglomeration elasticity. Spatial misallocation implies a dead-weight loss because workers in economical distressed areas can not migrate to productive places and therefore are employed (if lucky enough) in low productivity jobs.

[6]The innovation intensity of government purchases is measured as the share of purchases that demand R&D services, other categories in the data are infrastructure, cleaning services and non-durable goods.

trough which channels defense spending affects aggregate innovation.[7] We use GDP, personal income and total employment to evaluate the impacts of defense spending on economic growth. To the best of our knowledge, this project is the first that uses military spending, patenting, and GDP data at a such fine geographic level.

Our identification strategy exploits the quasi-experimental time and cross-sectional variation in defense spending across counties that followed the declaration of the War on Terror in September of 2001. To circumvent some endogeneity concerns in the allocation of military spending across localities we use a shift-share instrument proposed by Nakamura and Steinsson (2014).[8] As any shift-share instrument it creates a predicted defense spending at local level by combining national variation in military spending with a measure of local comparative advantage to receive defense contracts, the latter is parameterized by the share of total military spending that goes to a specific county before 2001. The main identification assumption is that the two components used to computed the predicted defense spending are conditionally independent of contemporary and future shocks in our outcomes of interest - innovation and growth.

The defense spending data is suitable to study the effects of local fiscal policies on innovation and economic growth for three reasons: First, defense spending has been a long standing sponsor of innovation worldwide, it funded the creation of internet, GPS, among other path-breaking discoveries. About 15% of defense spending demands R&D, while the rest is spend in infrastructure, services and non-durable goods. Therefore defense spending is a local fiscal policy that affects innovation both directly, by demanding contracting out the research services of the corporate sector, and indirectly by other general equilibrium mechanisms (Moretti et al., 2019;

[7]Investigating the role of the extensive (number of researchers) and intensive (number of patents by researcher) margin, is particularly important for the current policy concerns about the decline in research productivity in U.S (Bloom et al., 2017).

[8]This same strategy has been used recently by Auerbach et al. (2019) to estimate fiscal multipliers at CBSA level and by Demyanyk et al. (2019) to measure how consumer debt affect the size of the fiscal multiplier.

Draca et al., 2012).[9] Second, defense spending is discretionary at aggregate level and responds to geopolitical events rather than to local economic cycles. The weak linkage between local economic cycles and aggregate defense spending alleviates concerns of lack of exogeneity of our instrument due to simultaneity problems (Ramey, 2011). Third the declaration of the War on terror break the aggregate trend in defense spending (it was declining during the whole decade of 90s). Moreover, the fact that defense spending is discretionary leverage a large and unexpected county level variation that we exploit for identification.

Related literature This project contributes to three strands of the literature. The first is the literature of place-based policies. A large part of the literature has focused on the effect of those policies in employment and growth without considering outcomes like innovation.[10] The only exception are those papers that have exploited changes in tax rates across U.S states to measure several innovation outcomes like number of inventors and patents Akcigit et al. (2018). Other papers have explored the effect of taxes on R&D, though not strictly under the framework of a place-based policy but a firm-based policy. (Bloom et al., 2002; Dechezleprêtre et al., 2016).[11] We contribute to the analysis of innovation placed based policies by using fiscal stimulus instead of taxes as policy instrument. In spite defense spending does not target specifically innovation, it works as a demand-induced innovation shock due to the amount of purchases of R&D services made by the military sector. This type of shocks may have different effects than the ones found by Akcigit et al. (2018) using tax incentives.

Second, as we focus on innovation as primary outcome and use military spending as approach for a fiscal policy we directly contribute to the literature that studies the effects of military spending on innovation. This literature has focused on un-

[9] Among these mechanisms could be knowledge spillovers, relaxing financial frictions or increasing agglomeration economies.

[10] See Neumark and Simpson (2015) for a literature review on general placed-based policies.

[11] See Bloom et al. (2019) for a excellent review of innovation policies.

derstanding the firm level responses to public spending, particularly, they find that it does crowd in corporate investment at firm (Draca et al., 2012), (Slavtchev and Wiederhold, 2016) and international level (Moretti et al., 2019). Our contribution is to focus on aggregate innovation at local level and explore the effects of military spending on creating innovation clusters. Moretti et al. (2019) study this question at national level, our main advantage by using a local level approach is that we are allowed to decompose the aggregate effect into its local and spillover components.

The third contribution is to the literature of relative fiscal multipliers (RFM). This literature tends to use longitudinal data at state or county level to estimate the impact of fiscal policy on local output. A review of Chodorow-Reich (2019) concludes that the bulk of this studies use either military spending or the fiscal policy implemented during the American Recovery Act of 2009 to draw causal inference on the size of the RFM.[12] This paper contributes by asking how the innovation intensity of the military spending matters for the effects of the local fiscal stimulus on the economy. Therefore we contribute by exploring one of the channels trough which fiscal stimulus can affect economic growth. The study more close to ours in this regard is Akcigit et al. (2017) who use the WWII innovation spending as instrument to estimate the effect of patents on economic growth. The policy implications of this project relies at the core of the mission of the Washington Center for Equitable Growth. Our question aim to identify the efficacy of local fiscal stimulus on a key determinant of long-run economic growth, namely, innovation. This will help to design place-based policies that aim to reduce regional disparities that affect U.S today and are deprived many citizens from participating from the dividends of a digital economy.

[12] Nakamura and Steinsson (2014), Suárez Serrato and Wingender (2016) and Auerbach et al. (2019) focus on contemporaneuos RFM, while other studies use the WWII as natural experiment to estimate long-run RFM (Fishback and Cullen, 2013; Fishback and Jaworski, 2016; Li and Koustas, 2019), most of the long-term studies find no significant results on GDP per capita but significant results on population growth and structural transformation.

2 Identification strategy

Our identification strategy exploits a time and spatial variation in military outlays that followed after the declaration of the War on Terror by United States in September of 2001. This event allow us to exploit an unexpected and large increases in defense spending. Before 2001, defense spending was in decline because of several spending cuts that were implemented as a result of the end of the cold war. This makes the rise in defense spending an unexpected event. Moreover the war against Afghanistan and Iraq have been the most expensive military build ups for U.S since WWII. Following Auerbach et al. (2019), we estimate a flexible specification described by equation (1),

$$\frac{Y_{l,t+k} - Y_{l,t-1}}{Y_{l,t-1}} = \beta \frac{G_{l,t+k} - G_{l,t-1}}{GDP_{l,t-1}} + \alpha_l + \delta_{t+k} + \epsilon_{l,t+k}$$

where l, t and k refers to county, time and horizon ($k = 0, 1, 2, 3, 4, 5, 6$). Our outcome variable measures the k period growth rate $\frac{Y_{l,t+k} - Y_{l,t-1}}{Y_{l,t-1}}$ for both innovation and economic growth (e.g output, employment or private earnings). Defense spending is normalized by local output. α_l absorb linear county specific trends and δ_{t+k} for any mechanical correlation due to secular trends in defense spending and local economic growth.

Still, the realized changes in military spending $G_{l,t+k} - G_{l,t-1}$ may respond to unobserved local shocks that also affect innovation. Private firms located in counties with higher military contracts may have exerted more effort trough lobby and investment in order to bring those contracts home. As long as this firm specific shocks also affect innovation and growth our coefficients will be biased. Also, politicians have an active role in trying to allocate contracts to their constituencies during economic downturns. To circumvent this endogeneity problem we use a shift-share instrument that interacts the nation-wide changes in defense spending $(G_{t+k} - G_{t-1})$ with the share of national defense spending that each locality received during the decade of the 90's. The equation below describes our instrument:

$$G_{l,t+k} - G_{l,t-1} = s_l \times (G_{t+k} - G_{t-1})$$

where s_l can be defined as a predetermined comparative advantage of certain counties to receive military spending and it is defined by the share of total national spending that was allocated to locality l during the decade of the 90s $s_l = \frac{\sum_{t=90}^{00} G_{lt}}{\sum_{t=90}^{00} G_t}$. This identification has been used in the literature before at state level by Nakamura and Steinsson (2014) and at metropolitan area by Demyanyk et al. (2019) and Auerbach et al. (2019).

Since our main regression is in changes and we use locality fixed effects, the conditional variation that is provided by our instrument comes from national level changes in military spending, which is plausible exogenous to local shocks that affect innovation and growth. An emerging literature on Bartik designs provides a new set of test to make more transparent the assumptions behind the use of this type of instruments (Goldsmith-Pinkham et al., 2018; Adao et al., 2019). We will develop their suggested robustness test in future stages of this project, another value over previous approaches.

3 Data

3.1 Innovation

Microdata for granted patents are publicly released by the United States Patent and Trademark Office (USPTO).[13] Following the USPTO's practice, we classify the patent origin based on the residence of the first-named inventor. Because readily available inventor residence information generally is limited to the city and state at the time of patent grant, we will use the U.S. Post Office zipcode reference file to

[13] We will scrape the data from the following link https://bulkdata.uspto.gov/.

aggregate the available geographic information by county. We will use this measure as an indicator of innovation activities.

The plain count of number of patents suffers from several measurement issues that are well-established in the literature. The two main issues are: 1) the upward secular trend in patenting; 2) the importance of the different patents.[14] We will complement the empirical analysis by investigating two adjusted measures of innovation. To address the upward trend in patenting, we will adjust the count of patents by implementing the 'fixed-effects' proposed in Hall et al. (2001). To account for the difference in quality and importance among patents, as standard practice in the literature, we will weigh each patent by the number of citations received in a 5-years window after the application year. All information needed for computing the adjustment measures are available in the above mentioned USPTO's files. For the measure of number of researchers see section 3.3.

3.2 Defense spending

We will use data on military contracts for the period 1990-2018. Up to now, we have collected and processed data for the sub-period 2001-2013. These data for the recent years is available at USAspending.gov. The data on military contracts for the years before 2001 will be collected from the National Archives and Records Administration-NARA.[15] To our knowledge, our projects is the first that will make a consistent series of military contracts between 1990-2018.[16]

The data allow us to distinguish many characteristics of each contract: i) name and unique identifier of the firms who receive the contract, ii) type of good and service contracted (e.g. missiles, R&D, office supplies, or cleaning services for military

[14] We are aware of other problems with patent data, such as for example, the truncation problem. Due to military expenditures data limitations, we are forced to drop the last few years of the sample. We believe the truncation issue is significantly attenuated.

[15] We will follow the harmonization standards recommended in Draca et al. (2012)

[16] We plan to share this data once the project is finished.

bases), iii) geographic location of the primary contractor and where the majority of the work has been performed, iv) duration of the contract, v) the industry classification by 6-digits NAICS of the firm who has received the contract.

3.3 Other data

Data on GDP and personal income is taken from the Bureau of Economic Analysis (BEA). GDP data at county level is a new dataset that was just released for first time on December 2019, which allow our project to find and study new stylized facts about growth dispersion in U.S. We use data on employment from County Business Patterns (CBP) from the Census Bureau, this dataset allows us to observe employment for each 4-digits NAICS. We will use this advantage to estimate number of workers in R&D intensive sectors.

4 Preliminary results

This section presents the preliminary results of our empirical specification using the instrumental variables strategy. These results are preliminary for three main reasons. First, as explained in the sub-section 3.2, due to the large computation power needed to process the military spending data, we have been processing the data gradually. We expect to conclude the creation of the full dataset by the end of May. Second, the results in this section use the plain count of patents by county. As described in sub-section 3.1, we plan to adjust this measure of innovation along several dimensions. Third, we will explore alternative identification strategies to investigate the robustness of our findings.[17] For all these reasons, the results should be interpreted with a grain of salt.

The data used in this analysis consists of a balanced panel of counties with data on defense spending, patent, GDP and total employment for the period 2001-2013.

[17] See section 5 for details.

We keep all counties with more than 1,000 jobs, non-missing values for GDP, DoD spending and non-zero granted patents. Table 1 suggest that expansionary local spending shocks have a strong positive impact on local innovation on impact and in the long run. The results suggest that fiscal stimulus have sizable effects on the number of patents for the average county.

Table 1: *Local spending and effects on local innovation*

	Innovation multiplier			
	On Impact	2-year	4-year	6-year
	(1)	(2)	(3)	(4)
Spending	2.795**	2.370**	3.404**	1.667*
	(1.262)	(1.024)	(1.402)	(0.882)
County and Time FE	Yes	Yes	Yes	Yes
Obs	11,184	9,320	7,456	5,592
# of Counties	932	932	932	932
1^{st} Stage F-stat	28.74	22.20	13.14	12.07

***: p<0.01 ;**: p<0.05;*: p<0.1

However, we show that there is substantial concentration of patents across counties. Then, the question is if this policy increase innovation in those counties that are lagging behind, i.e. reducing geographical concentration, or the fiscal stimulus boost the concentration of patents in a few counties. To preliminary provide a test for this key policy question, we split counties according to the level of patents in year 2001 (first available). Table 2 suggest that DoD spending seems to have a higher effect on those counties that were in the bottom of the geographic distribution of patents in year 2001.

Preliminary results seems to lead to belief that national government spending can boost local innovation and then local economic growth. Interestingly, the local fiscal stimulus seems to increase innovation in those places that were lagging behind. Placed based policies may help to reduce the differences between counties in terms of what they can offer to firms and workers. This may end up in social gains in

Table 2: *Heterogeneous effects of spending on local innovation*

	Innovation multiplier					
	Bottom 75%			**Top 25%**		
	On Impact	2-year	4-year	On Impact	2-year	4-year
	(1)	(2)	(3)	(4)	(5)	(6)
Spending	2.987**	2.624**	3.683**	0.796	-0.058	-0.245
	(1.399)	(1.266)	(1.671)	(0.811)	(0.916)	(1.271)
County and Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs	8,388	6,990	5,592	2,796	2,330	1,864
# of Counties	699	699	699	233	233	233
1^{st} Stage F-stat	24.74	16.72	10.36	3.956	8.869	6.201

***: p<0.01 ;**: p<0.05;*: p<0.1

terms of spatial equality.

5 Proposed plan

The next stage of the project can be divided in three parts. The first is the data collection part. We will extend the sample period for DoD spending series to 1990-2018, by downloading and organizing the data from USAspending.gov and NARA. We will scrape USPTO's to add the application year to our county level on patents and also to control for the innovation trends before 2000. We will use the CBP data to estimate science-related jobs and research productivity and include them as outcomes in our estimations. We plan to finish this activity by August of 2020.

The second, is to improve our identification strategy and write down the first draft of the paper. We will add local industry composition to our shift-share instrument, this depends on the quality of the defense contracts to detect DoD national trends at the industry level. Also, we will explore the use of synthetic control methods proposed by Zou (2018). Finally, we will test the robustness of the shift-share instruments following recent econometric guidelines proposed by Goldsmith-Pinkham et al. (2018) and Adao et al. (2019). We plan to finish this activity by December of 2020, additionally by that time we will have the first draft of the paper with all our empirical results.

Third, we will match our empirical results and moments of the data to a endogenous growth model that considers agglomeration economies and spatial heterogeneity. This will allow us to asses the welfare implications of this particular place-based policy on long-run economic growth. Particularly, the model will help us to answer two questions: i) How the spatial dispersion of fiscal policy affects aggregate outcomes of the economy? Is it better to spend equally across the board or concentrate spending in the most productive cities?. ii) How long a fiscal-induced demand shock should stay in order to generate persistent effect over time?. We plan to finish this activity by December of 2021.

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Appendix A Data

Appendix A extensively describes the data we have used in the paper and explains the steps for deriving the final sample used in the empirical analysis.

Appendix A.1 Military Spending

We collect data on Department of Defense (DoD) military procurement contracts for the period 1984-2006 from the National Archives and Records Administration (NARA) database.[18] These data contain detailed information including the contract identification, the dates of action and completion, the transaction value, the location where the contract is performed, and the Federal supply classification code. There are two main advantages of our data compared to previous works, for example Auerbach et al. (2019). First, we have geographically disaggregated data available for a much longer period than previous studies. This feature of the data enables us to better study the historical trends and exploit more variation in military spending in identifying the causal effect. Second, our data include the product classification of contracts and permit us to distinguish the effect for different product classes and to exploit more variation in constructing the instrument.

We define the year in which a contract is approved as the year of the signature date.[19] The signature date corresponds to the date of action that corresponds to the date when a contract is either awarded or modified. The contract completion date corresponds to the delivery date of the goods or services requested from the contract. The contract dollar value is reported in nominal term. For comparability over time, we convert the nominal transaction value into real values by using the

[18]The military procurement data are available fro download on the NARA's website: Link.

[19]The government fiscal year has been defined from October 1^{st} to September 30^{th} since 1976. The mismatch between the fiscal year of the government and the calendar year could cause a time inconsistency between the the military spending and the innovation and GDP variables. Thus, we use the calendar year as reference year.

Consumer Price Index from the US Bureau of Labor Statistics.[20]

In addition to new contracts, the dataset also includes modifications to existing contracts. Some of these modifications consist in downward revisions to contract amounts that are reported as negative entries.[21] We often observe contracts with positive and negative entries of similar amounts. We consider contracts with obligations and de-obligations with magnitudes within 0.5% of each other to be null and void. Thus, we drop all these contracts that correspond to 2.2% of the total number of contracts.

We also drop the D350 military contracts that are contracts with a value below $25,000.[22] Finally, we remove from the sample contracts with missing signature dates,[23] and contracts whose completion date is before the signature date.

We follow two approaches to allocate the military spending for a contract across years. The first approach consists of assigning the entire value of the contract to the year in which the contract has been signed. The second approach follows Auerbach et al. (2019) and it smooths the allocation of the contract value over the duration of the contract, computed as the period between the signature date and the completion date.[24] As the empirical analysis is carried at the annual frequency, we then aggregate the value of a contract by the years covered between the signature and the completion years.

The collected data include the Federal supply classification code that is a proxy for the type of product that the contract is requested to deliver. The classification

[20] Data can be downloaded from the BLS' website: https://data.bls.gov/pdq/SurveyOutputServlet.

[21] The contract value is reported as an alphanumeric code. The last digit of the code identifies whether the contract is an obligation or a de-obligation. We use the contract dictionaries to decode the alphanumeric strings into numeric values.

[22] These contracts fill different forms and several variables of interest are not reported.These contracts account fro % of the total number of contracts.

[23] There are only 24 contracts with missing signature dates.

[24] The completion date is missing for 25% of the total number of contracts. That's the case because the completion date is not reported for contracts signed before 1990. As in this second approach, we need to calculate the period passed between the signature and the completion dates, we restrict our analysis only to contracts that have both dates.

includes 300 different product codes.

The detailed location data permit us to geolocate contracts in narrow geographic areas. We assign a contract to the county where the contract is performed. We then use the spatial crosswalks provided by the National Bureau of Economic Research (NBER) to aggregate the county-level military contracts into CBSAs.[25]

Figure 3: Military Spending Comparisons

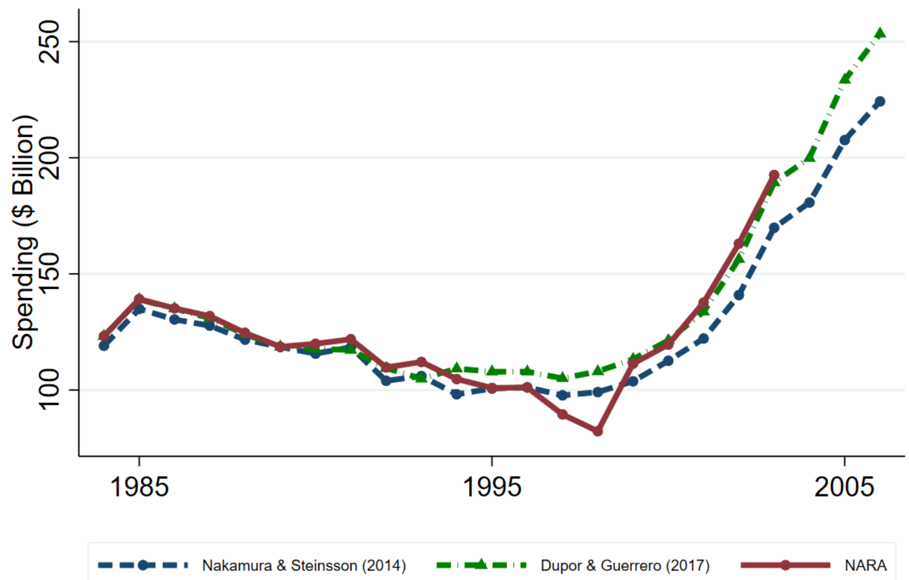


Figure 3 compares the aggregate military spending in the US by fiscal years between our newly-created series and previous works (**??**). Our data match well the trend and the level of aggregate spending from previous works. We notice some differences in the spending in 1997 and 1998. These differences are due to the fact we drop contracts whose signature date is after the completion date. Without removing these contracts, our series perfectly matches the aggregate spending from previous works in 1997 and 1998 as well. Overall, we conclude our data are well-representative of the aggregate US military spending.

[25]The spatial crosswalks are available on the NBER's website: https://www.nber.org/research/data/census-core-based-statistical-area-cbsa-federal-information-processing-series-fips-county-crosswalk.

As our empirical specification uses variation in military spending at local level, we also compare at geographically-disaggregated level our newly-built data with the data used by previous studies (**??**). To this end, we regress the

We restrict our analysis to geographic units and years that are available in both our and the comparison datasets. Table 3 reports the results of our comparison. In column 1, we disaggregate the spending at state-level and compare our calculations with the data reported by **?**.

Table 3: Data comparisons by geographic units

	(1)	(2)	(3)
	?	**?**	**?**
β			
	()	()	()
Observations			
Geographic Unit	State	State	CBSA
Period	1984 – 2003	1984 – 2003	2001 – 2003
R^2			

Overall, these tests validate the high-quality and comparability of our data with respect to data previously used in the literature and coming from different sources.

Appendix A.2 Patents

We collect patent data from PatentsView at the end of 2019.[26] The PatentsView database contains the universe of granted patents from the US Patent and Trademark Office (USPTO) starting from 1976 until 2019. These data contain patent-level information including application and grant dates, assigned technology class,

[26]More recent updates of the patent data can be downloaded from the following link: https://patentsview.org/download/data-download-tables.

the type of patent, the claims, the name of the assignees, the latitude and longitude of their addresses, and the citations' network consisting of the number of citations made to and received from other patents.

We restrict our analysis to utility patents that cover the creation of a new or improved product, process, or machine. Utility patents are also commonly known as "patents for invention," and they account for about 98% of the universe of patents granted by the USPTO. We also restrict to patents with application year starting from 1976.

In the patent data, we do not observe the exact date in which an innovation occurs. As common in the literature, we identify the year when an innovation occurs as the application year of a patent, which is the year when the provisional application is considered complete by the USPTO, and a filing date is set. A patent assignee that is the juridical entity that owns the right to the patent and it is usually a firm employing the inventor or, for independent inventors, the inventor herself, has economic incentives to apply for the review process at the USPTO as soon as possible in order to prevent other firms from exploiting the innovation and extracting the economic benefits. Thus, the choice of the application year as innovation year seems reasonable.

As well-discussed in the patent literature, for example Hall et al. (2001) or **?**, patent data suffer from three major issues that affect the over-time comparison of patent statistics: 1) the changes in the propensity to cite; 2) the lifespan of a patent; and 3) the truncation bias. We address both issues and adjust the microdata by following different strategies developed in the literature.

The propensity to cite bias is generated by the changing patterns of patenting and citing over time. Starting from the late eighties, we have seen a dramatic acceleration in patenting and citing activity in the US. This increase in the use of patents is view as a response to the increase in the patent protection provided by the legislator, rather than an endogenous rise in the amount of innovation. An over-time comparison of patent activities without a proper adjustment to correct these trends could generate misleading conclusions. Similarly, in addition to changes in the propensity to cite, also a shift to the left of the citation-lag distribution, implying that citations are coming sooner than they used to in the previous decades, could lead to the same misreading of the results.

We address the propensity to cite bias by implementing the "quasi-structural" approach developed by Hall et al. (2001). Under the assumptions that the shape of the lag distribution over time is independent of the total number of citations received (proportionality assumption) and the lag distribution does not change over time (stationarity assumption), the "quasi-structural" approach disentangles the effect of changes in the propensity to cite from changes in the innovation fertility. To implement this approach, we use the patent citations' network,[27] and we estimate the following specification:

$$\frac{C_{j\tau t}}{P_{j\tau}} = \alpha'_0\, \alpha'_\tau\, \alpha'_t\, \alpha'_j\, f_j(L),$$

or equivalently,

$$\log\left(\frac{C_{j\tau t}}{P_{j\tau}}\right) = \alpha_0 + \alpha_\tau + \alpha_t + \alpha_j + f_j(L),$$

with L is the citation lag calculates as $t - \tau$,[28] and the function $f_j(L)$ describes the shape of the citation-lag distribution and is defined as

$$f_j(L) = (e^{-\beta_{1j}L})(1 - e^{-\beta_{2j}L}),$$

where the parameter β_{1j} captures the depreciation of knowledge and β_{2j} captures its diffusion. These two sets of estimates differ across technological classes. $C_{j\tau t}$ is

[27] We drop citing and cited patents with missing application year, citing patents whose application year is before the application year of the cited patent, and citing patents with missing technological class.

[28] We constrain $L \in \{1, 2, ..., 35\}$, and that the sum of $\exp(f_j(L))$ over the domain is equal to the unity. We also normalize $\alpha_{\tau=1} = \alpha_{t=1} = \alpha_{j=1} = 0$.

the total number of citations to patents in year τ and technological class j, coming from citing patents in year t.[29] $P_{j\tau}$ is total patents observed in technological class j in year τ. α_τ, α_t, and α_j represent the cited year fixed effects, citing year fixed effects, technological class fixed effects, respectively. As the function $f_j(L)$ is non-linear, the estimation of the two sets of fixed effects, α_τ and α_t, is possible, but difficult. Therefore, as common in the literature, we group the cited year effects and we estimate coefficients for α_τ fro five-year intervals, while we estimate separately the effects for the citing years. This assumption implies that the fertility of invention changes slowly.

We use the estimated coefficients to correct for changes in the propensity to cite over time. In this respect, we deflate the citations received by a patent i in a specific year t by the pure propensity to cite factor. This factor is calculated as the ratio between the citing year fixed effects, α_t, and the patent index, computed as the ratio between the total numbers of granted patents applied in year t divided by the total number of granted patents in 1976. Our citation measures are calculated from these adjusted citations.

The second critical time effect is linked to the lifespan of a patent. Older patents have longer time to accumulate citations than more recent patents. Thus, if we do not account for this issue, we could get misleading results. As common in the literature, we address this problem, once the previous adjustments have been made, by measuring the quality-adjusted innovation rates within a fixed window of five years after the application year. In this way, we make the citing activities comparable between patents that have different lifespans.

Finally, the truncation bias also mechanically affects the the number of citations that a patent receives from other patents. The truncation bias arises from the fact

[29] We define the technological classes for this estimation level of the International Patent Classification (IPC). There are 8 categories we include in the regression, each representing a macro aggregation such as for example, A - Human Necessities or B - Performing Operation and Transporting.

that patent records are only released at the grant dates when the review process by the USPTO is completed. The truncation issue is even more severe for citations. Indeed, it is relatively rare for a patent to be cited by another patent before its approval by the USPTO. As a result, the truncation bias causes that the patents in the last years of our sample are mechanically less cited, independently of their innovativeness. The USPTO reports that "*As of 12/31/2012, utility patent data, as distributed by year of application, are approximately 95% complete for utility patent applications filed in 2004, 89% complete for applications filed in 2005, 80% complete for applications filed in 2006, 67% complete for applications filed in 2007, 49% complete for applications filed in 2008, 36% complete for applications filed in 2009, and 19% complete for applications filed in 2010.*" Thus, the review process takes essentially 8 years to be fully completed. Following the same logic, to have reliable measures of quality-adjusted innovation rates and address the truncation bias, we restrict the analysis to patents that applied by the end of 2006. We undertake this very cautious approach because we also want to ensure that citing patents in the 5-year window have completed their review process.

We associate a patent with a CBSA by using the latitude and longitude of the address of the patent assignees. We geolocate the latitude and longitude into CBSAs by using the 2010 TIGER/Line Shapefile constructed by the Us Census Bureau.[30] When patents have multiple assignees whose addresses are in different CBSAs, less than 1% of patents, we equally split those patents across CBSAs according to the number of assignees. In the geographical aggregation, we restrict our sample to patents whose at least one assignee is located in one CBSA of the US 50 states plus Washington, DC.[31]

We exploit the rich information about the citations' network, technology classes,

[30] The TIGER/Line Shapefiles are available on the Census Bureau's website: https://www.census.gov/geographies/mapping-files/time-series/geo/tiger-line-file.html

[31] Less than 1% of patents is located outside CBSAs, these patents are dropped from our analysis. We match $2,971,614$ patents to 928 CBSAs.

and claims to compute 6 CBSA-level time series of measures of quality-adjusted innovation rates:

1. *Number of patents.* This statistic measures the number of raw patents that have filed the application at the USPTO.

2. *Patents weighted by the number of adjusted forward citations within a 5-year windows after the application year.* This measure accounts for changes in the propensity to cite, the truncation bias, and different lifespans. Forward citations are citations received by a patent.

3. *Share of patents in the top* 10%*,* 5%*, and* 1% *of the forward citation distribution a given year.* The citation distribution is computed using only citations received in the 5-year window after the application year. As pointed out by **?** these statistics capture the non-linearities between the value of a patent and the number of forward citations.

4. *Patents weighted by the number of their claims.* As argued by **?**, this measure captures the breadth of a patent that corresponds to the impact that a patent has by evaluating the number of claims linked to patent during the review process.

5. *Patents weighted by their generality index.* We follow Hall et al. (2001), and we compute the generality index of a patent j as

$$Generality_i = 1 - \sum_{j=1}^{J} \left(\frac{s_{j,t}}{\sum_{j=1}^{J} s_{j,t}} \right)^2$$

where the summation corresponds to the Herfindahl index (HHI) of the technological classes of the forward citations. The HHI is calculate as the sum of the squared ratio between the share of forward citations from a technological class j in the 5-year window after the application year t and the total number of forward citations. The technological classes are defined at the at the four-digit level of the International Patent Classification (IPC).[32]

6. *Patents weighted by their originality index.* We follow Hall et al. (2001) and we compute the originality index of a patent j as

$$Originality_i = 1 - \sum_{j=1}^{J} \left(\frac{t_j}{\sum_{j=1}^{J} t_j} \right)^2$$

where the summation corresponds to the Herfindahl index (HHI) of the technological classes of the backward citations. The HHI is calculate as the sum of the squared ratio between the share of backward citations from a technological class j and the total number of backward citations. The technological classes are defined at the at the four-digit level of the International Patent Classification (IPC).

As alternative measures, we also compute the previous set of innovation indicators as per capita measures using the working age population as deflator.

Appendix A.3 Other Data

- ***Population Counts***. We collect data on population counts from the National Bureau of Economic Research (NBER) as part of Survey of Epidemiology and End Results (SEER) starting from 1969.[33] The data are collected at county-level. We geolocate counties into CBSAs by using the spatial crosswalk provided by the NBER. Since some individual counties are not available for

[32] There are 3654 four-digit level technological classes in the International Patent Classification (IPC).

[33] The data can be downloaded from the NBER's website: https://www.nber.org/research/data/survey-epidemiology-and-end-results-seer-us-state-and-county-population-data-age-race-sex-hispanic.

the entire period, we either drop them or make them CBSA-consistent.[34] We use the adjusted population counts to account for large population gains and losses in counties affected by intense 2005 Hurricanes Katrina and Rita. We use the population counts to create per capita measures of quality-adjusted innovation rates, military spending, and personal income. As the population counts are disaggregated by age groups, we restrict the count to the working-age population between 15 and 64.

- ***Personal Income***. We collect data on personal income at CBSA-level from the US Bureau of Economic Analysis starting from 1969.[35] In our data, Metropolitan and Micropolitan Statistical Areas are geographically delineated by the Office of Management and Budget (OMB) in bulletin no. 20-01 issued March 6, 2020, and the definitions are updated as new information warrants.[36] We use the personal income to normalize the changes in military expenditure at local-level. We convert nominal personal income into real by using the Consumer Price Index from the US Bureau of Labor Statistics.[37] We also use the growth rate of the personal income as a proxy for economic growth.[38]

Appendix A.4 Final Sample

We merge the military spending, the innovation indicators, the personal income and the population counts by CBSAs and years. Our baseline sample consists of a fully balanced panel that includes only CBSAs that have non-zero military spending

[34] Additional details about the changes in the county codes and borders are available on the SEER's website: https://seer.cancer.gov/seerstat/variables/countyattribs/ruralurban.html.

[35] The data can be downloaded from the BEA's website: https://apps.bea.gov/iTable/iTable.cfm?reqid=70step=1acrdn=6.

[36] See for more information the BEA's website: https://apps.bea.gov/regional/docs/msalist.cfm.

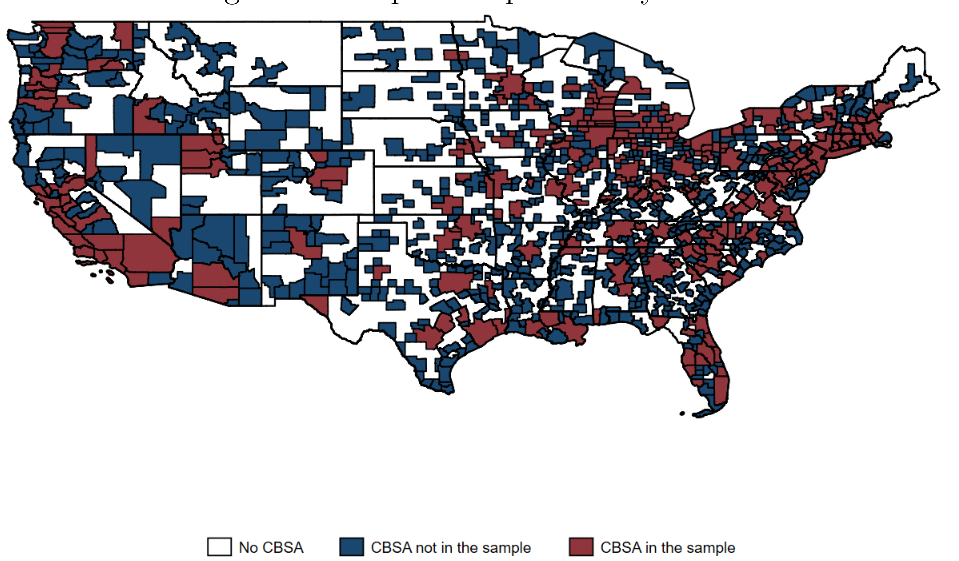
[37] Data can be downloaded from the BLS' website: https://data.bls.gov/pdq/SurveyOutputServlet.

[38] We cannot use the growth rate of GDP at CBSA-level because the data are only available starting from 2001.

and patents fro all year between 1984 and 2003. We also remove CBSAs that have for at least one year zero patents weighted by citations. These sample restrictions prevent from having extremely volatile growth rates in the variables of interest for the analysis. We finally drop CBSAs that have at least in one year less than $50,000$ inhabitants.[39]

The baseline sample consists of a balanced panel of $4,540$ observations coming from 227 CBSAs.[40] Figure 4 shows the geolocation of the CBSAs included in the baseline sample. Most CBSAs are located in the North-East and in the Mid-West regions.

Figure 4: Sample Composition by CBSAs



The CBSAs excluded from the baseline sample are CBSAs with at least one year of missing values in either military spending or patents or both. Figure 5 investigates the reason that led to the exclusion of the CBSAs from the baseline sample. The figure reports the

[39] We also run robustness checks in which we remove some or all these sample restrictions.

[40] The sample includes 208 Metropolitan Statistical Areas (MSAs) and 19 Micropolitan Statistical Areas.

Figure 5: Sample Composition by CBSAs

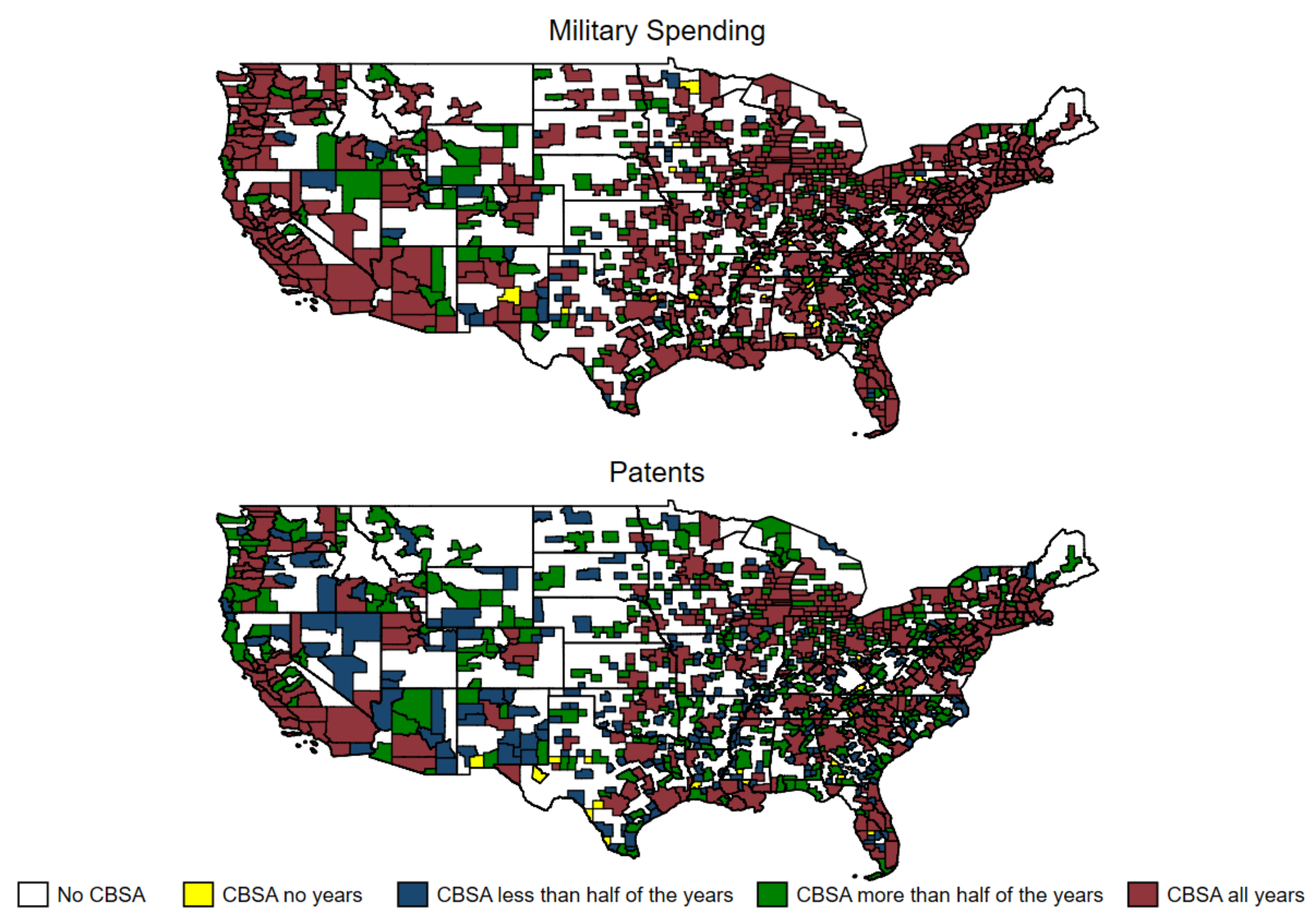


Table 4: Descriptive Statistics - Comparisons

	Baseline Sample	Full Data
	Panel A: Military Contracts	
Number	1,190,738	1,481,465
Total Value (USD)	1.55 E+12	1.81 E+12
Mean Value (USD)	375,813	349,247
Standard Deviation (USD)	6,523,718	6,103,280
Median Value (USD)	46,648	45,588
Share Number	80%	-
Share Value	86%	-
	Panel B: Patents	
Number	2,854,621	2,992,391
Total Citations	1,252,271	1,301,941
Mean Citations	0.5	0.49
Standard Deviation Citations	1.7	1.7
Median Citations	0.17	0.17
Share Number	95%	-
Share Citations	96%	-