EFFECTS OF SMALL BUSINESS REGULATION ON FIRM-LEVEL OUTCOMES

by

Grant H. Lewis
A Dissertation
Submitted to the
Graduate Faculty
of
George Mason University
in Partial Fulfillment of
The Requirements for the Degree
of
Doctor of Philosophy
Economics

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ACKNOWLEDGEMENTS

Thanks to Alex Tabarrok, Thomas Stratmann, and John Earle for their assistance in refining hypotheses and econometric specifications. John Earle contributed findings from his research into historical small business regulation. Don Walls provided sampling recommendations based on his expertise in small business dynamics and the National Establishment Time Series. The Center for Study of Public Choice at George Mason University and the Cosmos Club Foundation generously funded this research.

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LIST OF ABBREVIATIONS

Average Treatment Effect on the Treated ..... ATT
Core Based Statistical Area ..... CBSA
Data Universal Numbering System ..... DUNS
Department of Agriculture ..... DoA
Department of Commerce ..... DoC
Department of Defense ..... DoD
Department of Education ..... ED
Department of Energy ..... DoE
Department of Homeland Security ..... DHS
Department of Transportation ..... DoT
Environmental Protection Agency ..... EPA
Federal Acquisition Regulation ..... FAR
Federal Procurement Data System ..... FPDS
Federally Funded Research and Development center ..... FFRDC
Government Accountability Office ..... GAO
Department of Health and Human Services ..... HHS
Limited Liability Corporation LLC
Mergers and Acquisitions ..... M&A
National Air and Space Administration ..... NASA
National Bureau of Economic Research ..... NBER
National Establishment Time Series ..... NETS
National Science Foundation ..... NSF
North American Industrial Classification System ..... NAICS
Ordinary Least Squares ..... OLS
Organization for Economic Cooperation and Development ..... OECD
Propensity Score ..... PS
Research and Development ..... R&D
Securities and Exchange Commission ..... SEC
Service-disabled Veteran-owned Small Business ..... SDVOSB
Small Business Administration ..... SBA
Small Business Innovation Research ..... SBIR
Small Business Technology Transfer ..... STTR
Small Disadvantaged Business ..... SDB
Standard Industrial Classification ..... SIC
Weighted Least Squares ..... WLS
Woman-owned Small Business ..... WOSB

ABSTRACT

EFFECTS OF SMALL BUSINESS REGULATION ON FIRM-LEVEL OUTCOMES

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The United States government manages a wide range of programs directing federal contracts to firms on the basis of criteria other than commercial suitability. Examples include goals for the percentage of contracts awarded to small businesses, preferential treatment to minority business owners and grants to small businesses for research and development. The objective of this study is to examine the effect of such programs on economic outcomes. Economic theory implies two competing hypotheses. Directing contracts based on firm characteristics orthogonal to commercial suitability may encourage rent seeking and other counterproductive behavior. Alternatively, entrenched incumbents or historical patterns of discrimination may have left "money on the table" in the form of smaller, more productive firms that are excluded from competition. This analysis examines which of these theories predominates by examining firm-level outcomes of preferential contracting programs. It incorporates contracting data from the Federal Procurement Data System with performance measures in the National

Establishment Time Series to generate a comprehensive data set which I then analyze through a variety of quasi-experimental methods. The results are broadly consistent across programs and model specifications, suggesting the rent-seeking hypothesis, rather than the "money-on-the-table" hypothesis, predominates. With few exceptions, preferential contracting programs tend to inhibit growth in the overall population of participating firms and to encourage rent seeking and strategic behavior.

CHAPTER 1: MARKET OUTCOMES OF PUBLIC VENTURE CAPITAL

Through the Small Business Innovation Research (SBIR) program, the U.S. government provides grants and sole-source contracts to small businesses demonstrating potential for productive innovation. The program is designed to correct market failures associated with research and development inappropriability and information asymmetries in startup financing. I use linked datasets combining SBIR award data with the National Establishment Time Series to reexamine previous case-study- and survey-based investigations that found positive effects of the program on firm performance. Empirical matching models compare program participants to control groups of similar firms and reveal significant underperformance of program participants in both employment and sales growth over a six-year period.

(*JEL* H21, H32, O38)

Keywords: Small Business, Research and Development, Entrepreneurship, Subsidy, Innovation, Inappropriability

Section 1: Introduction

The idea of market failure is common in the economic theory of research and development: firms do not invest in innovation at a socially optimal level due to

technological spillover and the inappropriability of investment returns. With social returns to innovation exceeding private returns, there may be a role for public support of private research and development (Arrow 1962, Griliches 1992, Jones and Williams 1998). Other inefficiencies arise from the nature of new ideas and the people working to commercialize them. Moral hazard and information asymmetry between entrepreneurs and venture capitalists may prevent viable projects from finding the necessary financing. Structure of the venture capital industry itself and the regulations governing it may favor large, established firms at the expense of more innovative but less capitalized competitors (Hall 2002). Such concerns led to the Small Business Innovation Development Act of 1982 (P.L. 97-219), which established the Small Business Innovation Research (SBIR) program to be overseen by the Small Business Administration.

Founded on the idea that innovative small businesses are the engine of U.S. economic growth, the act directs federal agencies engaging in high levels of research and development to set aside a portion of their funding for small businesses. The SBIR program office states on its website, "SBIR targets the entrepreneurial sector because that is where most innovation and innovators thrive. However, the risk and expense of conducting serious R&D efforts are often beyond the means of many small businesses." Through a competitive, multi-phased sequence of investments, agencies select projects with high potential and attempt to bring them from concept development to commercialization through a series of grants and sole-source contracts. Phase I of the program provides limited funding to demonstrate commercial potential. Successful Phase I firms then progress to Phase II in which products are further developed for introduction

to the marketplace. The program currently disburses about $2 billion per year to small businesses (see Figure 1).

Figure 1: Total SBIR/STTR Awards (2012 Dollars)

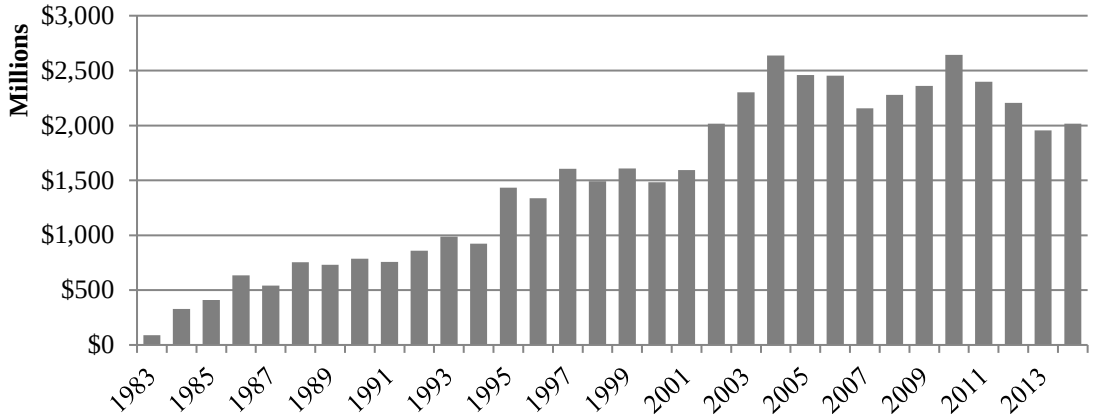

Figure 1: Total annual SBIR/STTR funding since program inception, as measured by Phase I and Phase II awards reported at www.SBIR.gov for fiscal years 1983 through 2014.

The literature on the potential for R&D market failure presents a strong theoretical case for the need to support R&D efforts in small businesses (Romer 1986, 1990; Jones and Williams 1998). However, public choice theory suggests that, whatever the market failures, public solutions must first provide evidence that government organizations are equipped to produce desired outcomes. In the case of the SBIR program, can federal agencies identify and successfully invest in undercapitalized firms with potential for high impact on the economic performance of the United States? Anecdotal evidence would seem to answer this question in the affirmative. Proponents cite success stories such as Apple, Compaq and Intel, all of whom were recipients of government startup funding in their early days (Audretsch 2003). However, broader

evidence supporting the conclusion that the SBIR program has been a success arises primarily from interviews, surveys and case studies of participating firms, and is therefore susceptible to the challenge that it comes from those most likely to overstate the program's impact (Lerner and Kegler 2000). Objectively derived datasets are largely absent in the literature, and the paucity of rich data on the subject allows notable (and by definition, outlying) cases to drive the debate.

This study provides a counterpoint to common case-study analyses of government R&D intervention with a dataset larger than any previously used, and comes to sharply different conclusions. Since interest in evaluating the SBIR program peaked a number of years ago, large quantities of publicly available data on SBIR awards have been collected by the program office. The challenge has been pairing these awards with data on firm performance since most SBIR program recipients are not publicly traded firms. The only significant study to undertake such a pairing was Lerner (1999), who manually combined survey data with information from a variety of public sources to conduct the only econometric analysis of the program to date with a sample size large enough to be convincing. The study found support for the program's beneficial effects.

I use a similar approach, linking SBIR awardee firms with employment and sales data through DUNS numbers also reported in the National Establishment Time Series (NETS). Although consistent DUNS number matches are only possible beginning in approximately 2007 (government procurement data quality greatly improved following the Federal Funding Accountability and Transparency Act of 2006), the following six years are adequate to discern program effects. I first replicate previous studies such as

Lerner (1999), generating treatment and control groups by matching participating firms to non-participants by limited sets of firm characteristics. I obtain similar results, suggesting that SBIR firms grow faster than similar firms not benefiting from financing.

I then modify the matching procedure to account for additional data not available at the time of the early study. A critical problem in identifying the SBIR program's effect on firm performance is disentangling the economic effects of the targeted innovation from those of the funding itself. A firm whose business model focuses on capturing non-competitive government contracts may indeed grow larger, but the goal of stimulating broader economic development and innovation will not be met. Examination of a larger panel of award data suggests that this is likely a problem: the same firms consistently win repeated SBIR awards over the span of many years (see Table 1). Case studies examining follow-on effects from single awards do not account for this serial correlation. A common finding that individual award recipients consistently outperform non-recipients in subsequent years may reflect the simple fact that initially successful firms continue to win SBIR awards. However, the skill set required to win federal contracts is likely different from that required to create value through innovation. By matching awardee firms with control firms based on a more robust set of variables, the models presented here control for both the serial correlation problem and other issues identified in previous studies such as localized availability of private venture capital and other critical inputs (Audretsch and Feldman 1996).

Table 1: Number of SBIR/STTR Awards by Year, FY2000-2014

Firm Name	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013	2014	Total
Julia Group, The													1	1		2
Fulcrum Corp.		2		2	1											5
Laser Sensing Co.													2			2
Paragon Space Development Corp.			1		3	4	1	2	3	2	4	5	2	4	5	36
Zircon Computing LLC													1			1
Ekos Corporation	2	2														4
Nia Solutions Corporation					1											1
Advanced Cell Diagnostics, Inc.							1		1		1		1		1	5
Cornerstone Research Group, Inc.	3	3	5	13	8	12	13	10	13	12	15	13	8	3	10	141
Sims Brothers Incorporated											1	1				2
Hawthorne Mushrooms Inc.				1					1							2
Martek Bioscience Corporation	1	1														2
Bl Healthcare, Inc.				1		2										3
Energy Quest Technologies Inc.											1		2		1	4
Scentczar Corp.	1	4														5
Ventana Research												1				1
Isotron Corporation			1	3	1	2		1								8

Table 1: Total number of separate Phase I and Phase II awards received by a random sample of SBIR/STTR program participants in fiscal years 2000 through 2014.

I begin with a limited sample created by matching Phase II award recipients with firms who won Phase I funding but failed to move to Phase II and examine employment and sales of the resulting treatment and control groups. The positive effect found here, as reported in Lerner (1999) and many other case-study-type analyses, disappears when firms are more carefully matched in subsequent model specifications. Matching treatment and control firms on location or total federal funding in addition to industry and size eliminates or even reverses the effects found in previous studies. Rather than matching Phase II firms with Phase I firms, I next match them with non-SBIR firms which held other R&D contracts with the federal government. The larger sample size allows for more

careful matching procedures, and the effect of SBIR participation is strongly negative, with participants underperforming in employment and sales growth relative to their peers. Alternative specifications confirm the robustness of this finding.

Analysis of the effects of SBIR funding on individual firm performance does not address larger questions of beneficial knowledge spillovers and stimulation of private venture capital funding for research and development (Gans and Stern 2000). Most existing studies, however, are based on the premise that public venture capital programs indeed improve performance of the target firms (Audretsch 2003; Wessner 2000). They then explore how these salubrious effects spill over to the broader economy and whether the benefits are greater than the costs. Audretsch, Link and Scott (2002) go so far as to state, "We do not debate the appropriateness of the government's support of [the SBIR program] but take that as a historical given and turn directly to evaluating the program's results." The following analysis suggests that a fruitful avenue of research is the assumed private beneficial effect itself. If government contracting officers are unable to "pick the winners" and succeed only in sustaining underperforming firms, then the proper question is not how much of the technology spills over, but rather whether a more effective mechanism should be used to allocate national R&D resources.

Underperformance of SBIR awardees is not the end of the story. As Wallsten (2000) points out, optimal government support for entrepreneurs should be targeted to marginal firms, not the high performers who could obtain financing through private-sector means. While private venture capitalists attempt to fund firms generating positive private benefits, public venture capital targets firms with positive social benefits. In the

presence of knowledge spillovers, the social benefit is greater than the private benefit, leading private venture capital to require a higher net expected return. Venture capitalists of all types, however, cannot determine in advance which projects will be profitable. Empirical research suggests that they err significantly on the side of funding many negative-net-return projects, relying on a few star performers to make up the difference.

The margin of private funding thus falls below the margin of private benefit on a continuum of possible net project returns (see Figure 2).

Figure 2: Target for Optimal R&D Subsidies

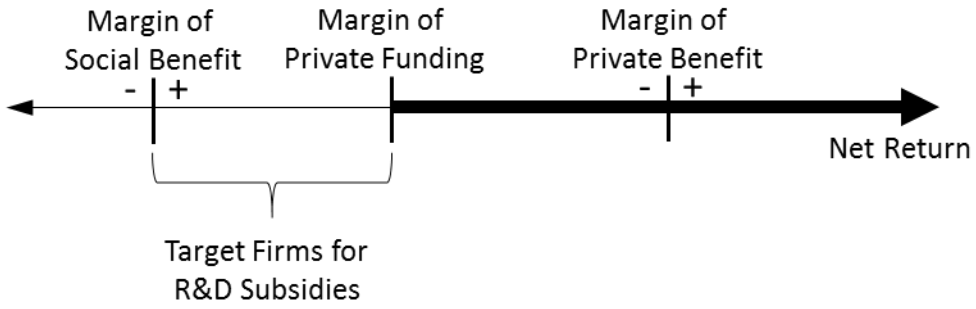

Figure 2: This figure illustrates the target range for projects funded by government R&D subsidies on a continuum of total project returns by firm. The figure assumes that private agents require a larger net return to be willing to invest in a project than is socially optimal. It also assumes that many funded projects do not produce a positive private rate of return. If the margin of private funding falls to the left of the margin of social benefit, then no optimal policy exists for publicly subsidizing R&D projects.

If the private-funding margin is below the social-benefit margin, then the marginal socially beneficial project would be funded by the private sector in the absence of public support. In the presence of deadweight costs of taxation, it will be not only difficult for public venture capital to produce a net social benefit; it will be impossible. The study to follow does not attempt to identify the locations of these critical margins. It is important

to note, however, that any innate inability of the public sector to identify productive investments will make it even more difficult to target firms in the possibly narrow gap between negative social returns and availability of private funding. And of course, program administrators give no indication that selecting subpar performers is deliberate or that their low level of performance is monitored to correspond with the margin between privately funded and unfunded projects. Publicly stated selection criteria include technical merit of proposed research subjects, firm qualifications, and potential for commercial success and benefit to society (www.SBIR.gov).

The SBIR program has objectives other than creating successful new firms, such as helping disadvantaged business owners and equitably sharing revenue among different constituencies. It is therefore possible that we can expect some level of underperformance. The results here are important nonetheless; they are at odds with common perceptions of the program's results and align more closely with traditional theories of regulatory capture (Stigler 1971, Becker 1983). The amount of funding provided to protected groups is also very small: of the nearly $40B disbursed through the program since its inception, only about 0.5% has gone to historically underutilized business zones, 1.3% to minority-owned businesses, and 3.1% to women-owned businesses.

The remainder of the paper is organized as follows. Section II examines the history of the SBIR program and provides a brief survey of the literature on subsidized R&D, as well as alternative research bearing on the subject such as confounding factors and the pitfalls of government intervention in the small business R&D environment. I

describe the dataset and empirical approach in Section III and present results in Section IV. Section V concludes.

Section 2: Background and Theory

The 1982 act of Congress establishing the SBIR program identified four broad goals: "(1) stimulate technological innovation; (2) use small business to meet Federal research and development needs; (3) foster and encourage participation by minority and disadvantaged persons in technological innovation; and (4) increase private sector commercialization of innovations derived from federal research and development." Participating businesses must be for-profit organizations with less than 500 employees and more than 50% ownership by United States citizens. Following precursor programs in the 1950's and 1960's, an early pilot program in the 1970's, and congressional action in 1982, SBIR was subsequently strengthened and expanded over a number of years. The Small Business Technology Transfer (STTR) program was added ten years later, which is similar to SBIR but requires small businesses to partner with universities, federally funded research and development centers (FFRDC) and other non-profit institutions to encourage the transfer of technologies developed there into the private sector. The programs were reauthorized through 2008 by the Small Business Reauthorization Act of 2000 (P.L. 106-554) and have since been further extended and expanded, the most recent of which directs continuation through 2017.

The current law mandates that federal agencies with research and development budgets exceeding $100 million set aside a percentage of this budget for small businesses. Under the SBIR/STTR Reauthorization Act of 2011, this percentage is set to

rise each year, from 2.5% in Fiscal Year 2012 to 3.2% in 2017. The STTR program is smaller than SBIR, with funding of 0.35% in 2012 and similar incremental increases through 2016. Figure 3 shows funding totals by agency, with a large majority provided through the Departments of Defense (DoD) and Health and Human Services (HHS). Federal departments participating in the program also include Agriculture, Commerce, Education, Energy, Homeland Security, Transportation, the Environmental Protection Agency, National Aeronautics and Space Administration and National Science Foundation.

Figure 3: Total SBIR/STTR Funding by Agency

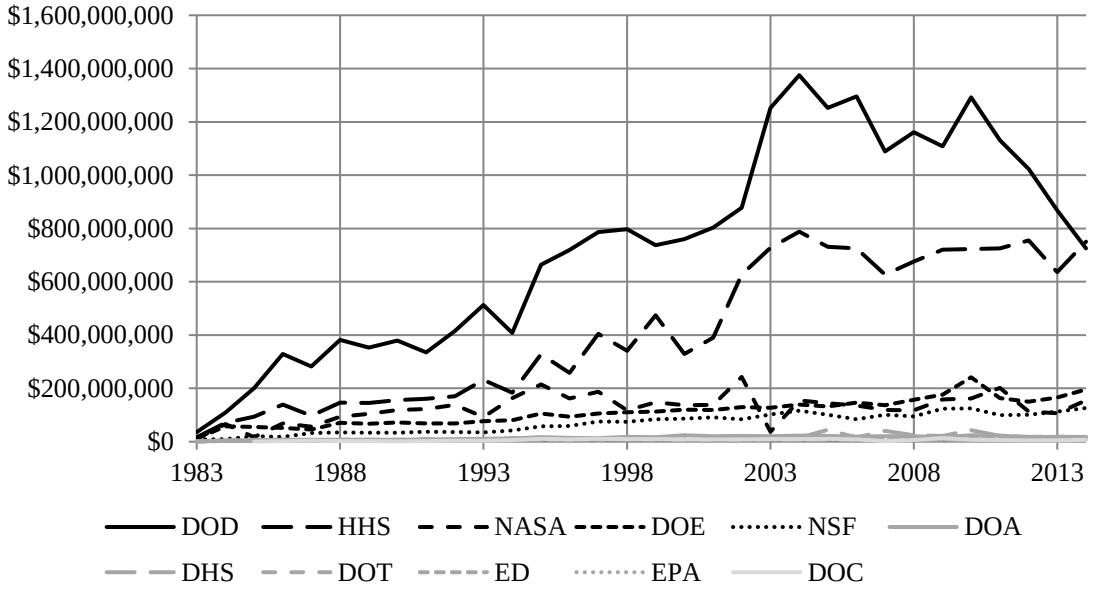

Figure 3: Total SBIR/STTR Phase I and Phase II funding by administering agency in fiscal years 1983 through 2014.

Each participating agency periodically publishes a list of designated R&D topics and selects awardees based on a competitive proposal process. The program has three

phases (Small Business Administration 2014). Phase I is the proof of concept stage in which participating agencies award up to $150,000 over 6-12 months to establish the feasibility and commercial merit of the winning project proposals. Small businesses awarded Phase I contracts continue to Phase II at the discretion of the awarding agency and may be non-competitively awarded up to $1 million in additional funding to continue the R&D effort for up to two years. Agencies may exceed these caps by 50% at their own discretion, so effective limits in Phase I and Phase II are $225,000 and $1.5 million, respectively. The Small Business Administration can waive the limits, and awards frequently exceed these amounts, particularly for HHS medical R&D awards (see Figure 4 and Figure 5). Phase III is the commercialization phase; prior awardees do not receive any further SBIR/STTR funding, but work to obtain private funding and may sell the products, processes or services developed in Phases I and II to the government through non-competitive, sole-source contracts.

Figure 4: Average SBIR/STTR Phase I Awards for Select Agencies (2012 Dollars)

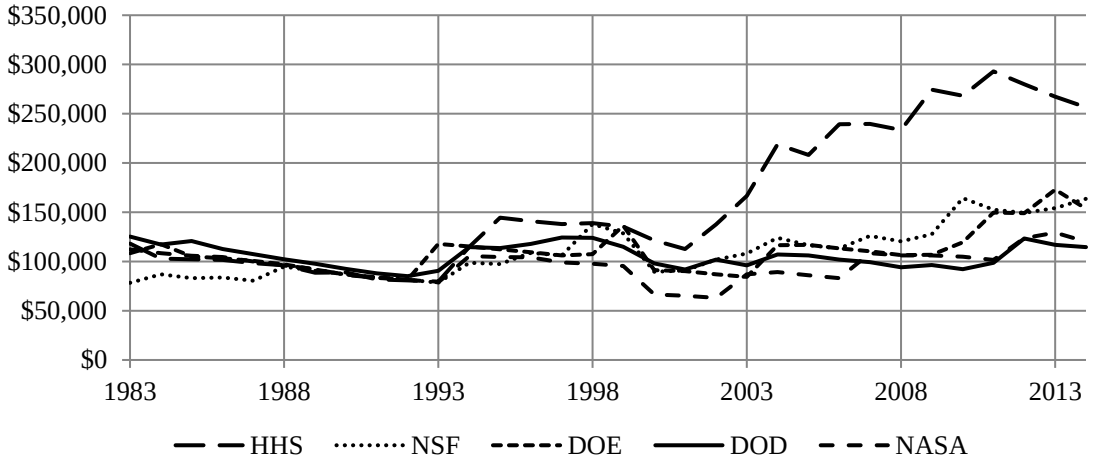

Figure 4: Average size of SBIR/STTR Phase I awards by agency in fiscal years 1983 to 2014.

Figure 5: Average SBIR/STTR Phase II Awards for Select Agencies (2012 Dollars)

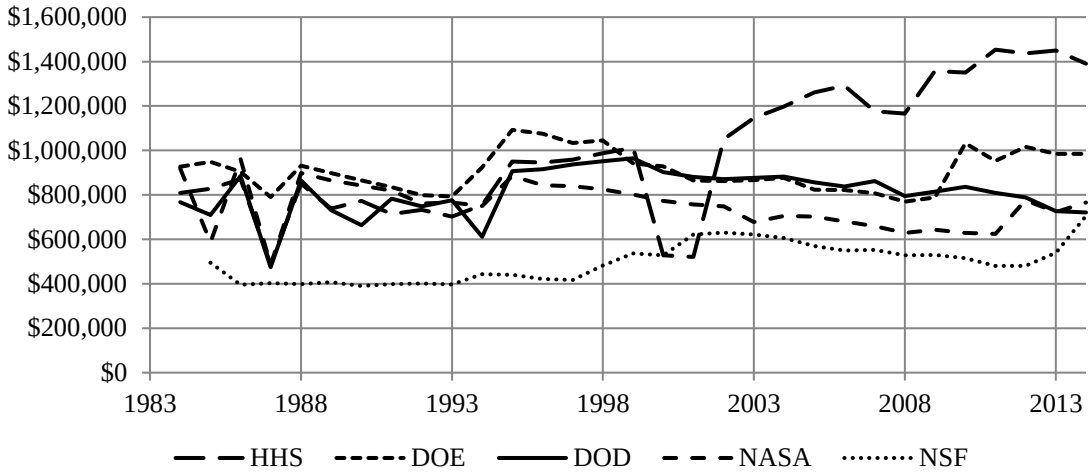

Figure 5: Average size of SBIR/STTR Phase II awards by agency in fiscal years 1983 to 2014.

Section 2.1: Theory on the Inappropriability of R&D Returns

Not long after inception of the SBIR/STTR program, new economic models built support for the general consensus that government plays an important role in private sector R&D that had grown since formalization by Arrow (1962). Romer (1986) incorporated technological change into macroeconomic growth models and showed how innovation is critical to explaining the ability of high-capital regions to consistently outperform poorer areas. Later works by Romer (1990) and Grossman and Helpman (1991) highlighted the non-rival and only partially excludable nature of technology and intellectual property. Given the importance of human capital to long-run growth and the inability of innovators to capture the full returns, too few resources are devoted to research in equilibrium.

Griliches (1958) built empirical support for the large externalities involved in research and development. As early as 1979, however, Griliches pointed out problems

with case studies in assessing the contribution of research and development to economic growth. He noted that econometric estimates of the production function may perform better than case studies by ignoring the detail of outlying events to focus on total output or total factor productivity as a function of past investments in innovation. But even these broader studies are questionable given the challenge of differentiating price changes driven by product improvements from those caused by changes in monetary factors or ordinary shifts in supply and demand. Even if such price measurement difficulties can be sorted out, new ideas influence other research in addition to having direct product effects. While some allocation of property rights is possible through the patent system, resulting prices are non-linear and cannot be used to measure social returns (Griliches 1992).

The more practical problem of data availability arises when studying small SBIR/STTR firms that are not publicly listed; the absence of firm-level data on inputs, outputs, capital stocks and other key variables makes productivity calculations difficult. Whatever the measurement challenges, consensus has built that R&D generates a large social rate of return beyond any private benefits. Jones and Williams (1998) find that common empirical estimates represent a lower bound, and that optimal R&D as a percentage of GDP is two to four times actual investment.

Section 2.2: Support for the SBIR/STTR Program

A growing body of work specifically investigates the SBIR/STTR program and its performance as a vehicle for public venture capital provision to small businesses. Audretsch, Link and Scott (2002) conclude from their analysis of SBIR/STTR participant survey data and case studies that the program indeed stimulates innovation and the

commercialization of technologies developed using federal resources. Case studies also suggest that award recipients would not have carried out the funded research without government support and that their efforts resulted in substantial spillover effects with broad benefit to society. A later similar study uses survey data from the US National Academies' division on Science, Technology, and Economic Policy, which gathered information from founders, owners and employees of SBIR/STTR firms (Audretsch 2003). Evidence from the survey supports the position that SBIR/STTR helps create high-technology companies and strengthens the competitiveness of the US economy. Gans and Stern (2000) take a different approach, using survey data from 100 SBIR/STTR projects to examine the relationship between the appropriability of technology across various industries and the activity of private venture capital. Their findings suggest that policy makers can play a role in addressing challenging appropriability regimes facing small firms, such as intellectual property or antitrust rules which restrict access to assets required for successful commercialization. Lerner (1999) uses GAO survey data to construct treatment and control groups of SBIR participants and non-participants, using a variety of sources to determine sales and employment of awardees and matched firms. Phase II awardees performed significantly better over the approximately seven-year period than non-awardees in each control group.

Lerner and Kegler (2000) and Lerner (2002) examine the role of administrators of R&D subsidy programs like SBIR/STTR and methods of assessing their performance. The authors point out that "survey methods interview those with interest in continuing the project" and that owners of SBIR/STTR firms have even formed a lobbying organization

to advocate the program to lawmakers. The program may nevertheless provide net benefits. Due to financing challenges and information asymmetries, venture capital firms may be too big for small projects to be worth their time, and agency conflict may lead to Akerlof's (1970) lemons problem in the venture capital market. Technological spillovers may be particularly acute for small businesses ill-equipped to defend intellectual property or leverage market power to extract rents from the products they develop. Public venture capital may then play a valuable role in certifying the viability of small businesses to investors and incentivizing innovators to produce social gains that cannot be privately captured (Meuleman and De Maeseneire 2012; Soderblom, et al., 2015).

Private sector investors maintain rigorous methods to select and protect their investments. They thoroughly scrutinize business plans, less than 1% of which are accepted. They require partners to certify the attractiveness of the investment as well as conferral of preferred stock with restrictive covenants and representation on the board of directors. Funds are provided in stages, each of which includes close monitoring. Lerner and Kegler (2000) describe these functions and ask why SBIR/STTR administrators should perform them better than private agents. The paper notes a number of reasons why they may not. Political connections may become more important than technical promise. Past recipients may develop experience navigating red tape or relationships with administrators that facilitate future selection processes. Project technical proposals may not address many factors important to a firm's success that would be evaluated by a private venture capitalist, such as an entrepreneur's experience, legal troubles, or side projects that may distract from core areas of expertise.

Section 2.3: International Evidence

Early empirical support for the importance of R&D to growth, and its under-provision by the market, led to widespread government subsidies for innovative activities around the world. In examinations of international programs the results are mixed, and point to several problems with practical application of R&D support. One study of 2,000 Spanish manufacturing firms receiving subsidies found that while some of these firms would cease their R&D activities in the absence of government support, most of the funding goes to firms that would continue without it (Gonzalez, Jaumandreau and Pazo 2005). Other evidence from Finland suggests that, while social returns to subsidized research projects are as high as 30-50%, technological spillovers from these projects are smaller than their effects on the profits of the targeted firms (Takalo, Tanayama and Toivannen 2013). Bronzini and Iachini (2014) look at Italian investment subsidies and find differential effects by firm size, with small businesses increasing their own investment by approximately the amount of the subsidy while spending at larger businesses is not affected. Montmartin and Herrera (2015) account for the strong spatial dependence of R&D support programs across 25 OECD nations between 1990 and 2009 and find that, while tax incentives increase business-funded R&D, subsidies are more likely to substitute for it.

Section 2.4: Geographical Effects of R&D Subsidies

Krugman (1991), following Marshall (1920), models patterns of development based upon transportation costs, economies of scale and manufacturing as a share of national income, and emphasizes spillovers and increasing returns that drive localized growth. Building on this theory, Jaffe, Trajtenberg and Henderson (1993) use patent

citations to identify patterns of related research and development activity. Patent citations are more likely to come from the same state and metropolitan statistical area as the cited patents, suggesting that local synergies in innovation are important. Regional effects are also found in industry-specific knowledge spillovers: the degree to which an industry is geographically concentrated is directly related to the importance to the industry of new economic knowledge (Audretsch and Feldman 1996). Most SBIR/STTR firms reside in either California or Massachusetts (see Figure 6), with highly concentrated funding in such places as Los Angeles, Boston and Washington, DC (Figure 7). Localized synergies can be a blessing and a curse for subsidy programs. They may magnify beneficial effects by providing a network of support for new business owners within their local community (Huggins and Thompson 2015), but they also create political challenges for programs funded by tax dollars at the federal level, which generate pressure to fund less innovative businesses in other districts.

Figure 6: Total SBIR/STTR Funding by State (Top 5)

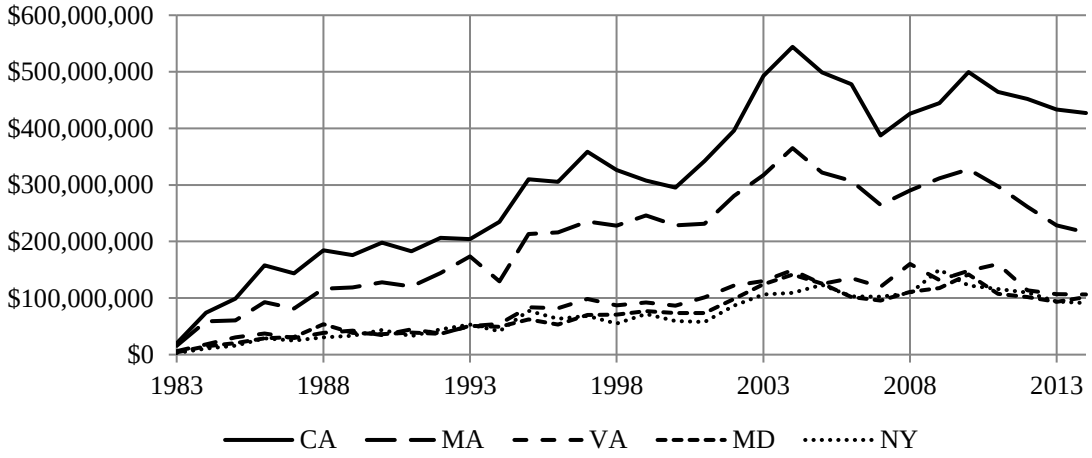

Figure 6: Total SBIR/STTR Phase I and Phase II funding by state of recipient firm in fiscal years 1983 through 2014.

Figure 7: Total SBIR/STTR Funding by CBSA

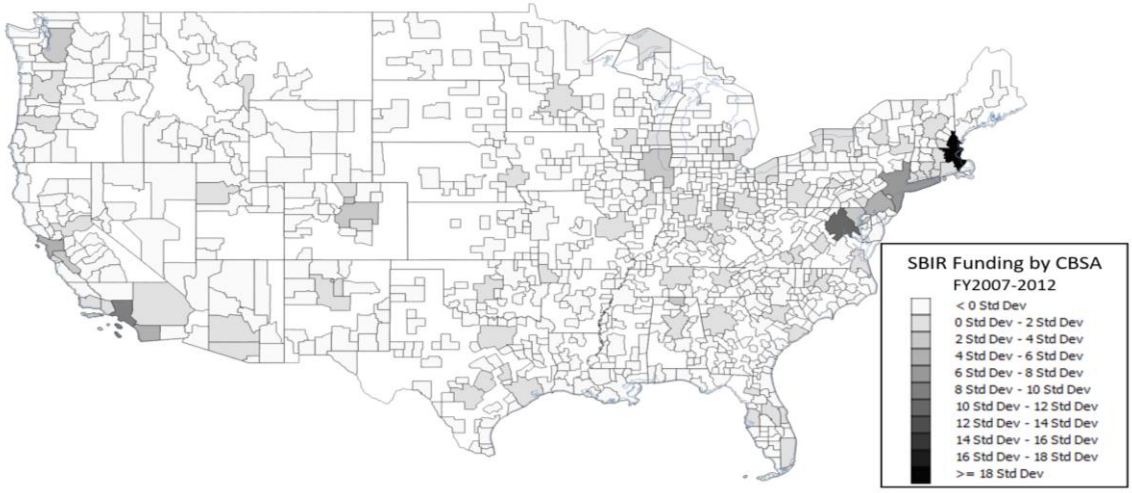

Figure 7: Total SBIR/STTR Phase I and Phase II funding by CBSA in fiscal years 2007 through 2012. Includes only SBIR recipients matched to NETS database firms. Top 5 CBSAs by funding are (respectively) Boston, MA; Washington, DC; Los Angeles, CA; New York, NY; and San Diego, CA.

Section 2.5: Firm Age, Size, and the Contribution of Small Businesses to Growth

A key assumption of lawmakers developing the SBIR/STTR program was that externalities in the market for innovative products are particularly acute for small businesses, and that these externalities hurt a portion of the economy that is especially important to economic development and growth. A growing body of research, however, calls into question early assumptions about the economic role of small businesses. Several confounding factors are often cited. Davis, Haltiwanger and Schuh (1996) point out that while gross rates of job creation are significantly higher for small businesses, survival rates are poor, resulting in a net level of job creation that is far lower. Studies find a vital role in overall job growth played by a few very small firms (Anvadike-Danes, et al. 2015), but these studies are challenged by others which note that age, not size, is the operative characteristic.

Covariance between age and size makes the issue a difficult one to resolve. Decker, et al. (2014), finds that startups currently generate 20% of new jobs, but most exit within 10 years. A small fraction of young firms grow strongly and make up for most of the job losses created by those that don't survive. This highly skewed distribution is important to understanding the challenge of subsidy programs illustrated in Figure 2. While the diagram depicts a continuous range of potential project returns, empirical evidence suggests that the underlying distribution displays strong positive skew. In addition to the locations of the social-benefit and private-funding margins, the location of the modal return may increase or decrease the number of firms falling in the target range for optimal public subsidies.

If age is indeed the key factor, and is correlated with size, then the focus of the SBIR/STTR program on small firms may be justifiable: targeting small firms will incidentally have disproportionate effects on young firms, a few of which are likely to become star performers. Of 6,967 businesses receiving SBIR/STTR awards between 2007 and 2012 and matched to the NETS database, approximately 25% were established prior to 1997. These older firms received 42% of the total funding. More than 40% of 2007-2012 awardees started prior to 2002; these firms received 63% of the funding. Small businesses that have remained small (and hence eligible for the SBIR/STTR program) for more than five to ten years are likely not the star performers ostensibly targeted by the program, but they receive a majority of the subsidies.

A criticism that underlies many of those discussed so far is that public solutions to inefficiencies in private markets face challenges of their own. Program administrators are

likely no more skilled than private venture capitalists at selecting productive firms and viable projects, and have weaker incentives to do so. Stigler (1971), Peltzman (1976) and Becker (1983) examine the nature of competition for government subsidies, asking whether market failures in the provision of R&D financing to small businesses can be corrected by government action. The presence of externalities does not by itself justify intervention. I now turn to this question.

Section 3: Dataset and Empirical Approach

The SBIR/STTR program office publishes all past awards. I use firm names and DUNS numbers to match awardees with detailed firm information in the NETS. The procedure generates a list of 6,967 matched firms that received awards between 2007 and 2012. Matching SBIR/STTR awards with NETS firm performance data by DUNS number is a challenge due to reporting practices of federal agencies. Awards are reported directly by the SBIR/STTR program office, but DUNS number/firm name pairs are reported separately from funding data, and inconsistent firm name structures make it difficult to automatically pair DUNS numbers with funding lines. I paired firm DUNS numbers with SBIR/STTR-reported award data by manually matching similar firm names across the 34,553 distinct names reported by the program office. (For example, a firm name reported in one funding line may differ from another by an omitted comma or period.) DUNS numbers then provided the means to match SBIR/STTR funding to firm performance data in the NETS.

Section 3.1: Obstacles to Unbiased Analysis

Analysis of the program presents a number of other challenges. First, we would expect the effects of regulatory capture to vary over time as firms learn to manipulate administrative processes. Second, only a small number of the many relevant differences between firms are observable, potentially leading to omitted variable bias. Third, the effects of funding on firms in different categories, such as industry or geographic location, are likely to operate with different lags. Fourth, awards are correlated over time, with recipient firms far more likely to continue to win awards than their unsuccessful peers. Fifth, firms must be measured consistently: more accurate data are likely available for older and larger firms, leading to bias in datasets derived from heterogeneous sources. I address each of these concerns in turn.

Early examinations of SBIR were limited by the recent establishment of the program. Firms were unlikely in the first few years to have been able to capture regulators to a degree sufficient to generate rents. If regulatory capture is indeed a problem, we would expect early positive results to reverse in later years as firms paid less attention to market success and more attention to government subsidy. Indeed, the statutory size limit of 500 employees required to keep receiving subsidies incentivizes firms to remain small as their competitors grow, although the precise effects of this threshold are difficult to quantify given the heavily skewed distribution of firm sizes. The dataset here begins 25 years after the program started, which accounts for potentially delayed effects of regulatory capture. Figure 1 illustrates that the program achieved a relatively steady state in overall funding levels over the past 10 to 15 years.

Another problem is missing data and the infeasibility of using fixed effects to account for it. Simple regression analysis ignores significant unspecified differences between regions, industries, and firms, while heterogeneous lag structures associated with industry-unique varieties of innovation limit the usefulness of panel data (as do substantial data entry errors, which create amplified attenuation bias in fixed-effects analysis). Problems associated with data unavailability are described in more detail in Griliches (1992) and Zuniga-Vicente, et al. (2014).

This analysis confronts these challenges by aggregating awards across a six-year period to address heterogeneous lag structures, and applying various firm matching techniques to generate treatment and control groups addressing non-temporal heterogeneities across observations. Aggregation also overcomes lags in data reporting procedures used in compilation of the NETS database. The drawback to examining the six-year period in aggregate is that it underestimates the effect: awards received in the last month of the period are treated the same as those received in the first month, though program participation has clearly had insufficient time to take effect. Advantages are the model's parsimony and tendency to underestimate rather than overestimate the effect of SBIR/STTR participation on firm performance.

Aggregation of awards over the six-year period also addresses what is perhaps the most significant challenge to program assessment: the tendency of the same firms consistently to win SBIR/STTR awards over many years. Serial correlation of awards makes it difficult to distinguish growth due to innovation from growth due to regulatory capture. Accounting for this correlation is a challenge across potential specifications.

Limiting the analysis to only those firms that won no further awards after the period in question cuts the sample size to an unworkably small number of firms. A snapshot in time with a follow-up many years later ignores the intervening period over which growth may have been achieved through product innovation or through further subsidies: the two cases are not distinguished, and which is driving firm growth is left undetermined. The analysis here lengthens the interval over which independent variables are captured to correspond with a period of time long enough to observe their effects. As mentioned before, this leads to underestimation by treating early awards the same as later awards, but it solves the problem of serial correlation that leads to confounding of innovation and regulatory capture effects. Funding levels are matched across treatment and control groups, eliminating unspecified government intervention as a possible source of positive bias in estimates of firm growth due to innovation and market success.

Consistent measurement of firm performance across the full range of data is also critical to eliminating bias in the estimation. Distinct data sources provide more or less accurate data on different categories of firms. For example, larger, listed firms are well represented in SEC filings while data on smaller, independent firms are often gathered from surveys or newspaper accounts of noteworthy events like bankruptcies. Discrete data sources contain unique biases that will be applied non-uniformly across the dataset, potentially resulting in biased estimates. As discussed above, surveys present particular challenges given the incentive of participants to bias answers in ways that encourage continuation of program funding. Structural or cultural variation between industries may also lead to inconsistent reporting practices between subgroups of firms with correlated

performance measures. Data collection for this investigation was uniform across firms, relying upon NETS employment and sales data gathered outside the context of the SBIR/STTR program. While the NETS dataset is more uniform and objective, it suffers from problems of lagged and incremental reporting. It may take up to two years for changes in firm size to be reported in the data, and firms tend to report employment and sales levels in round numbers that jump sharply at arbitrary thresholds (such as 100 employees). Again, aggregation across a lengthy period of time helps to ensure important medium-term effects are captured and short-run variability adequately smoothed.

Section 3.2: Dataset and Analytical Approach

The sample generated in this manner allows for more robust analysis than previously possible. The large dataset makes it possible to construct closely matched control groups based on detailed firm data. I examine three general model specifications. The first compares firms that only received Phase I awards (the control) to those that progressed to Phase II (the treatment). The second allows for an improved matching procedure by expanding the set of potential controls to the 37,465 non-SBIR/STTR firms that had active R&D contracts with the federal government between 2007 and 2012 (as reported in the Federal Procurement Data System); of these, complete data are available for 35,172 firms. I examine Phase I firms in more detail by comparing SBIR/STTR firms that received only Phase I awards during the 2007 to 2012 period (the treatment) with non-participating firms (the control). The third specification again uses the expanded control candidate set and improved matching procedure, but compares control firms to successful SBIR/STTR participants who won Phase II awards. Two additional variations

examine the robustness of the findings by duplicating the second and third specifications while removing the requirement for a geographic match. This modification expands the sample size and develops the picture of program effects and sources of bias.

Specification 1 replicates previous studies that compare firms progressing normally through public venture capital programs with those that begin but do not continue on to subsequent program phases (Lerner 1999; Soderblom, et al. 2015). Eliminating firms with missing data reduces the original 6,967 SBIR/STTR firms to 6,794. Of these, 3,138 received only Phase I funding and failed to progress to Phase II; 3,656 of the firms received Phase II funding. I do not apply propensity score matching in this specification due to the limited size of the population of potential control firms; rather, I trim the dataset to Phase II treatment firms that have precise matches with Phase-I-only firms. I match Phase II awardees to firms of the same 8-digit Standard Industrial Classification (SIC) code with the closest level of employment (required to be within 10 employees) and annual sales (required to be within $500,000). The annual sales requirement combined with similar employment levels proxies for firm labor productivity in addition to serving as another measure of overall size. The large size of the full sample allows firms with insufficiently precise matches to be dropped, resulting in a final sample size of 3,098 firms (1,555 in the treatment group and 1,543 in the control group).

Table 2 displays summary statistics for each group. Treatment firms received positive SBIR/STTR Phase II awards, while control firms received Phase I awards only. By construction, mean employment levels match at approximately 3.5 employees and annual sales at approximately $350,000. Using only this limited number of matching

variables, firms in the treatment group received significantly more Phase I award funding over the seven-year period. This makes sense: firms progressing to Phase II performed better than their counterparts who did not advance and went on to win more future Phase I awards as well. The propensity of initially successful firms to win more Phase I contracts in future years makes them systematically different from matched control firms leading to the biased results described below.

Table 2: Treatment and Control Group Summary Statistics (Specification 1)

	Mean	Std. Dev.	Min	Max	Obs
	Treatment Group				
Employees	3.60	5.63	0	100	1,555
Annual Sales	$370,188	$644,302	$0	$7,269,780	
Phase I Awards	$479,751	$678,345	$0	$6,856,135	
Phase II Awards	$1,739,419	$1,951,572	$57,963	$26,200,000	
	Control Group				
Employees	3.21	5.65	0	90	1,543
Annual Sales	$329,511	$645,319	$0	$7,375,086	
Phase I Awards	$250,825	$319,073	$32,327	$4,806,344	
Phase II Awards	$0	$0	$0	$0	

Table 2: Treatment and control group summary statistics for Specification 1. The treatment group contains firms awarded positive Phase I and Phase II SBIR/STTR awards between fiscal years 2007 and 2012. The control group contains firms that were awarded Phase I contracts but did not progress to Phase II. Treatment firms are matched to control firms by 8-digit SIC, employment level and annual sales.

I build on this procedure in Specifications 2 and 3 making use of the larger data set that includes non-participating federal R&D contractors. To improve the quality of the matches without reducing the sample to an unacceptable size, I match treatment firms to similar firms that did not participate in the SBIR/STTR program, but did have government R&D contract vehicles active in the Federal Procurement Data System (FPDS). Research and development contracts are defined by an FPDS Product/Service

Code prefix of A or particular M-prefix codes associated with operation of government-owned, contractor-operated R&D facilities (see General Services Administration 2011). The universe of firms included as candidates for the control group contains 35,172 private-sector firms with active federal R&D contract vehicles in FPDS between 2007 and 2012 that could be matched to NETS entries by DUNS number and do not have significant missing data. The research discussed in Section II suggests that SBIR/STTR participation may serve a certification function for firms seeking private sector venture capital financing; receipt of non-SBIR/STTR R&D funding would serve a similar function, making other federal R&D contractors plausible matches for SBIR/STTR treatment firms along this dimension.

In Specifications 2 and 3, I include additional criteria in matching treated SBIR/STTR recipient firms to control firms. In the models requiring precise characteristic matches, control firms must be in the same industry as the corresponding treatment firm (defined by 8-digit SIC code), within 10 employees in size and within $500,000 in annual sales, as in Specification 1. In addition, they must be located in the same core-based statistical area (CBSA) as the treatment firm. Geographic matching is important for the reasons discussed in Section II such as network externalities and supply chain synergies in addition to the hypothesis in Lerner (1999) that venture capital tends to disproportionately affect local firms. The treatment and control groups thus contain similar numbers of firms by SIC and CBSA, with similar levels of employment and annual sales. All firms come from the general pool of federal R&D contractors.

I also apply propensity score matching to the samples in Specifications 2 and 3, which allows inclusion of more matching variables without imposing unacceptable limits on sample size. The expanded set of explanatory variables is included in the probit model estimating probability of treatment. The model uses the number of firms in the same 4-digit SIC and CBSA rather than requiring firms to be in the same city and industry. Employment and the logarithm of annual sales are included along with firm age and total federal funding (SBIR/STTR funding or otherwise for non-participating firms). Non-SBIR/STTR federal funding by firm was extracted from FPDS. These additional variables are not included in the precise-match models since they would unacceptably reduce the sample size. Summary statistics for the full population of Phase I and Phase II firms and the population of potential control firms are provided in Table 3.

Table 3: Aggregate Summary Statistics

	Mean	Std. Dev.	Min	Max	Obs
	Recipients of Phase I Funding Only				
Employees, 2007	8.07	25.5	0	475	3,138
Annual Sales, 2007	$1,159,361	$5,281,840	0	$192,000,000	
Firm Age in 2012 (years)	9.84	10.2	1	117	
Phase I Awards	$245,038	$303,941	$22,702	$4,806,344	
	Recipients of Phase II Funding				
Employees, 2007	12.4	25.8	0	330	3,656
Annual Sales, 2007	$2,028,673	$13,100,000	0	$679,000,000	
Firm Age in 2012 (years)	13.4	10.0	1	124	
Phase I and II Awards	$3,064,484	$6,123,499	$106,637	$150,000,000	
	Non-SBIR/STTR Firms with Active Federal R&D Contract Vehicles				
Employees, 2007	115	683	0	34,000	35,172
Annual Sales, 2007	$17,100,000	$161,000,000	0	$23.5B	
Firm Age in 2012 (years)	23.3	25.9	1	376	
Federal R&D Funding	$10,600,000	$230,000,000	0	$19.9B	

Table 3: Summary statistics for the overall populations of SBIR/STTR recipient and non-recipient firms. The top portion of the table contains firms that were awarded Phase I contracts between fiscal years 2007 and 2012 but did not progress to Phase II. The center portion contains firms awarded positive Phase II SBIR/STTR awards. The bottom contains non-SBIR/STTR firms who had active R&D contracts with the federal government between 2007 and 2012.

SBIR/STTR firms employ an average of 8 to 12 people and have annual sales of $1.2M to $2.0M. They are 10-13 years old on average. Phase II firms tend to be larger and older. Non-participating federal R&D contractors are also included in Table 3; as expected, they tend to be significantly older and larger than SBIR/STTR firms, with averages of 115 employees, $17.1M in annual sales, and 23 years of age. I match treatment firms in each specification with their three nearest neighbors by propensity scores estimated in the probit model to form the control group (in contrast to the precise-match model, no caliper restrictions are included).

I then compare treatment and control groups by growth in employment and growth in annual sales over the six-year period using two distinct methods. A difference-in-difference model uses raw reported values of employment and sales at the beginning and end of the period to estimate the treatment group change relative to that experienced by the control group. In the second method, estimation of the average treatment effect on the treated (ATT) combines the beginning and ending values into an overall growth variable, calculated as (value2012 – value2007) / value2007. I adjust sample variances of propensity score matching estimators of the ATT for the first-stage estimation procedure using distributions derived by Abadie and Imbens (2012). Firms that are not in business in either 2007 or 2012 have values of employment and sales coded to zero in that year, with firms coming into existence showing growth of 100% and firms going out of business showing growth of -100%. A model specification that drops these entering and exiting firms from the dataset (not presented here) produces similar results.

Table 4: Treatment and Control Group Summary Statistics (Specification 2)

	Mean	Std. Dev.	Min	Max	Obs
	Treatment Group				
Employees	2.32	6.71	0	110	885
Annual Sales	$221,497	$627,497	$0	$11,000,000	
Phase I Awards	$244,037	$279,581	$32,327	$3,977,982	
Phase II Awards	$0	$0	$0	$0	
	Control Group				
Employees	2.32	6.37	0	106	1,017
Annual Sales	$214,170	$600,650	$0	$11,500,000	
Phase I Awards	$0	$0	$0	$0	
Phase II Awards	$0	$0	$0	$0	

Table 4: Treatment and control group summary statistics for Specification 2. The treatment group contains firms awarded positive Phase I SBIR/STTR awards, but no Phase II awards, between fiscal years 2007 and 2012. The control group contains non-SBIR/STTR firms who were awarded other government R&D contracts. Treatment firms are matched to control firms by 8-digit SIC, CBSA, annual sales (within $500,000) and employment level (within 10 employees).

Table 4 displays treatment and control group summary statistics for the second specification. Treated firms in Specification 2 received an average of $244,037 in Phase I awards between 2007 and 2012. Their average 2007 employment level of 2.32 employees matches that of the control group as does their mean 2007 sales of $221,497. A total of 1,902 firms met the precise matching criteria and are included in the model (885 treatment firms and 1,017 control firms). The probit regression estimating treatment propensities is presented in column 1 of Table 5.

The theory presented above does not provide a clear prediction for how these firms will perform. Winning a Phase I award may indicate that the firm was judged highly innovative by competent panels of experts in their field and can therefore be expected to perform well. It may also serve as a certification function to outside investors, allowing the firm greater access to private-sector financing. On the other hand, failing to progress to Phase II may suggest that these firms have trouble translating

innovative ideas into practical applications and are therefore likely to perform poorly relative to non-participant firms.

Table 5: Probit Regression for Estimation of Propensity Scores

	Specification 2	Specification 4	Specification 3	Specification 5
Treatment Group:	Phase I only		Phase I and Phase II	
Control Group:	Non-SBIR R&D funding		Non-SBIR R&D funding	
Dependent Variable:	Treatment	Treatment	Treatment	Treatment
	(1)	(2)	(3)	(4)
Number of firms of matching SIC & CBSA	-0.00155***		-0.000434***	
	(0.000131)		(0.0000841)	
Number of firms of matching SIC		0.0000564**		0.000019
		(0.0000235)		(0.0000248)
Employment, 2007	-0.00515***	-0.00513***	-0.00329***	-0.0105***
	(0.000666)	(0.000673)	(0.000349)	(0.000755)
Log Annual Sales, 2007	-0.0336***	-0.0343***	-1.12e-09	0.00489*
	(0.00254)	(0.00254)	(1.49e-09)	(0.00283)
Firm Age in 2012	-0.0148***	-0.0139***	-0.0131***	-0.0105***
	(0.00151)	(0.00152)	(0.00071)	(0.00124)
Log Federal Funding	0.161***	0.154***	-8.8e-11***	0.352***
	(0.00277)	(0.00263)	(2.97e-11)	(0.00623)
Constant	-2.32***	-2.32***	-0.983***	-5.27***
	(0.0296)	(0.0287)	(0.0146)	(0.076)
Observations	38,310	38,310	38,828	38,828
Pseudo R-squared	0.2450	0.2374	0.0512	0.4118

Notes: *** Significant at the 1 percent level
** Significant at the 5 percent level
* Significant at the 10 percent level
(Robust standard errors)

Table 5: Results for probit regressions used to estimate propensity scores for treatment/control group matching. The probit model estimates the probability that a given firm receives SBIR/STTR funding given the levels of the included control variables.

Specification 3 matches full participants in the SBIR/STTR program – recipients of both Phase I and Phase II funding – to firms that did not participate. I use the same matching criteria as Specification 2 along all other dimensions. As Table 6 shows, these

firms tend to be slightly larger than Phase-I-only firms in Specification 2. The treatment group averages 4.36 employees and $454,356 in annual sales, versus 4.01 employees and $418,065 in sales for the control group. Treated firms received an average of $462,057 in total Phase I awards and $1,826,980 in Phase II awards between 2007 and 2012. The sample includes 828 firms in the control group and 963 firms in the treatment group. The probit regression estimating treatment probabilities is included in column 3 of Table 5.

Table 6: Treatment and Control Group Summary Statistics (Specification 3)

	Mean	Std. Dev.	Min	Max	Obs
	Treatment Group				
Employees	4.36	7.35	0	80	828
Annual Sales	$454,356	$898,914	$0	$9,462,606	
Phase I Awards	$462,057	$683,967	$0	$5,780,221	
Phase II Awards	$1,826,980	$2,298,800	$126,550	$23,200,000	
	Control Group				
Employees	4.01	7.40	0	80	963
Annual Sales	$418,065	$875,518	$0	$9,126,717	
Phase I Awards	$0	$0	$0	$0	
Phase II Awards	$0	$0	$0	$0	

Table 6: Treatment and control group summary statistics for Specification 3. The treatment group contains firms awarded positive Phase I and Phase II SBIR/STTR awards between fiscal years 2007 and 2012. The control group contains non-SBIR/STTR firms who were awarded other government R&D contracts. Treatment firms are matched to control firms by 8-digit SIC, CBSA, annual sales (within $500,000) and employment level (within 10 employees).

For Specification 3 firms that participated successfully in the SBIR/STTR program from beginning to end, predictions of the conventional analysis and those of the regulatory capture hypothesis are sharply different. The conventional analysis suggests that public venture capital provides the startup funding required to turn innovative ideas into salable goods, and that government subsidies will stimulate growth as participating businesses commercialize the products developed during the program. How much of this

benefit is captured by the awardee and how much spills over to other firms, thus producing larger social benefits, is an open question, but we should nevertheless see some degree of above-average growth at the firm level. Regulatory capture, on the other hand, implies that participating firms will specialize not in innovation, but in obtaining subsidies. The program may even attract losers in the competition for private venture capital, further decreasing the quality of the talent pool and lowering growth levels below the overall average.

Section 3.3: Robustness of Estimated Effects

Two additional specifications test the robustness of the analysis by changing the matching criteria and expanding the sample size. In Specifications 4 and 5, I remove the requirement that firms must match to a similar firm in the same CBSA. Table 7 and Table 8, respectively, display the summary statistics.

Table 7: Treatment and Control Group Summary Statistics (Specification 4)

	Mean	Std. Dev.	Min	Max	Obs
	Treatment Group				
Employees	3.50	9.73	0	220	1,955
Annual Sales	$374,589	$1,134,010	$0	$23,200,000	
Phase I Awards	$234,489	$275,802	$22,702	$4,131,702	
Phase II Awards	$0	$0	$0	$0	
	Control Group				
Employees	3.43	9.20	0	223	2,448
Annual Sales	$365,169	$1,069,146	$0	$23,500,000	
Phase I Awards	$0	$0	$0	$0	
Phase II Awards	$0	$0	$0	$0	

Table 7: Treatment and control group summary statistics for Specification 4. The treatment group contains firms awarded positive Phase I SBIR/STTR awards, but no Phase II awards, between fiscal years 2007 and 2012. The control group contains non-SBIR/STTR firms who were awarded other government R&D contracts. Treatment firms are matched to control firms by 8-digit SIC, annual sales (within $500,000) and employment (within 10 employees). Firms are not matched by CBSA as in Table 4.

Table 8: Treatment and Control Group Summary Statistics (Specification 5)

	Mean	Std. Dev.	Min	Max	Obs
	Treatment Group				
Employees	6.48	13.4	0	300	1,933
Annual Sales	$734,779	$1,829,492	$0	$40,800,000	
Phase I Awards	$536,938	$1,145,657	$0	$24,900,000	
Phase II Awards	$2,059,290	$3,644,023	$57,963	$81,100,000	
	Control Group				
Employees	6.16	14.0	0	300	2,390
Annual Sales	$697,427	$1,811,722	$0	$41,000,000	
Phase I Awards	$0	$0	$0	$0	
Phase II Awards	$0	$0	$0	$0	

Table 8: Treatment and control group summary statistics for Specification 5. The treatment group contains firms awarded positive Phase I and Phase II SBIR/STTR awards between fiscal years 2007 and 2012. The control group contains non-SBIR/STTR firms who were awarded other government R&D contracts. Treatment firms are matched to control firms by 8-digit SIC, annual sales (within $500,000) and employment level (within 10 employees). Firms are not matched by CBSA as in Table 6.

Specification 4 uses a control group that is not matched by CBSA to test the effect on treatment firms of receiving positive Phase-I-only funding. Firms are of similar size, with about 3.5 employees and $370,000 in annual sales at the beginning of the period in 2007. The sample size of precise-match firms is 1,955 in the treatment group and 2,448 in the control group. Specification 5 also uses non-CBSA matching, but compares control firms to full program participants receiving Phase II funds. The firms are slightly larger on average than Specification 4 firms, with about 6 employees and $700,000 in annual sales. The sample includes 1,933 treatment firms and 2,390 control firms. The propensity score matching models are similar to those in Specifications 2 and 3, but remove the geography variable from the probit estimation of propensity scores, which instead includes the total number of firms in the same 4-digit SIC code across the United States rather that in the same CBSA. This probit estimation is included in columns 2 and 4 of Table 5.

Theory reviewed in Section II suggests that any bias introduced to the model by removing geography from the matching procedure is likely to be positive. SBIR/STTR funding is highly concentrated geographically (see Figure 7), and firms residing within industry clusters are likely to affect one another through knowledge spillovers, shared supply chains, concentrated customer bases, and other such agglomeration effects. Matched treatment and control firms from the same CBSA will experience similar geographic synergies, making the difference in their performance levels smaller. Control group firms from other cities, however, will not share industry cluster characteristics. We would expect the difference in performance between these control firms and SBIR/STTR participants to be larger.

Section 3.4: Model Specification

I first use a difference-in-difference model to compare the employment and sales growth of treatment firms with those of control firms between 2007 and 2012. This model is applied to both the limited precise-match samples as well as the broader treatment and control groups matched using propensity scores. With the matching procedure designed to account for non-orthogonal differences between groups, I use ordinary least squares to estimate

Equation 1: SBIR Difference-in-Difference Model

$$y_{i\tau} = \alpha + \beta t_i + \gamma a_\tau + \delta t_i a_\tau + \varepsilon_{i\tau}$$

where $y_{i\tau}$ is the level of sales or employment for firm i in period τ, t_i is a dummy variable indicating treatment status of firm i, a_τ is a dummy variable indicating either 2007 or 2012 values of the dependent variable, and $\varepsilon_{i\tau}$ is an error term that is assumed

orthogonal. The coefficient of interest is δ, which is the difference-in-difference estimator:

Equation 2: SBIR Difference-in-Difference Estimator

$$\hat{\delta} = \left(\bar{y}_{(treat)(after)} - \bar{y}_{(treat)(before)}\right) - \left(\bar{y}_{(control)(after)} - \bar{y}_{(control)(before)}\right)$$

Robust standard errors are clustered by 6-digit North American Industrial Classification System (NAICS) code. Other specifications, not reported here, used alternatives such as CBSA to cluster the standard errors; this had no significant effect on the results.

The probit model used to estimate propensity scores is specified as

Equation 3: SBIR Probit Estimation of Propensity Scores

$$t_i = \alpha + \beta X + \varepsilon_i$$

where t_i is a treatment status dummy and X is a matrix of independent variables including the number of firms in the same CBSA and/or industry, employment in 2007, log of sales in 2007, firm age in 2012 and the log of total federal funding to the firm. I estimate ATT using propensity-score-matched treatment and control groups according to

Equation 4: SBIR Average Treatment Effect on the Treated

$$E[Y_{1i} - Y_{0i} \mid D_i = 1] = E[Y_{1i} \mid D_i = 1] - E[Y_{0i} \mid D_i = 1]$$

where $E[Y_{1i} \mid D_i = 1]$ is the estimated value of the treatment group outcome (change in employment or annual sales) given treatment and $E[Y_{0i} \mid D_i = 1]$ is the counterfactual control group outcome assuming treatment. Matched controls supply counterfactual outcome estimates.

In addition to robustness checks using the non-geographically matched sample, I also include ordinary and weighted least squares estimates of the form

Equation 5: SBIR Ordinary Least Squares Model

$$y_i = \alpha + \beta t_i + \gamma X + \varepsilon_i$$

where y_i is either the percent growth in employment or the percent growth in annual sales for firm *i* between 2007 and 2012. The treatment dummy, t_i, indicates SBIR/STTR participation with β the coefficient of interest. The independent variables included in the propensity scoring probit regression model are also included here as controls in the matrix X. Error is again captured in ε_i and is assumed orthogonal. In addition to OLS, the model is estimated using frequency weights generated by the propensity score matching procedure. I weight each treatment and control observation by the number of corresponding matches from the other group, giving greater weight to firms possessing broader similarities to their peers.

Section 4: Results

The objective of the three primary model specifications is to answer several questions about the performance of the U.S. government's public venture capital program. Is the government, at a large scale and over a long period of time, capable of stimulating productive investment in R&D by small, innovative firms? Has it been successful at replicating the private venture capital industry at a national scale using bureaucratic structures? Does the U.S. economy perform better when agencies deliberately stimulate R&D investment at the most basic level of the small business? There are two primary competing hypotheses. The consensus in the literature examining the SBIR/STTR program is that it has indeed been a success, and that subsidies create innovation and growth that spills over into the broader economy, providing social benefits beyond the impact to individual firms. A second body of literature focuses on public choice theory and the economics of rent seeking; it leads to the conclusion that

such programs are unlikely to drive sustained high levels of performance given the weak and even perverse incentives offered to program participants and administrators.

Section 4.1: Phase II Recipients vs Phase-I-Only Control Group (Spec. 1)

Specification 1 replicates what is arguably the most convincing technique employed to date that suggests the program has been a success. Lerner (1999) supplements the large number of case-study-based analyses of the SBIR/STTR program with a broad look at the data. A comprehensive dataset was unavailable at the time the study was conducted, but the results can be replicated using modern data and similar methods. Table 9 displays t-tests for equality of means across the full treatment- and control-firm candidate populations in each model specification.

Table 9: t-Tests for Equality of Means in Treatment and Candidate Control Groups

			Obs.	Mean	Std. Err.	H_A: Diff. < 0	H_A: Diff. ≠ 0	H_A: Diff. > 0
Specification 1	Emp. Chg.	Control: Phase I	3,138	**0.714**	0.0606	0.775	0.450	0.225
		Treatment: Phase II	3,656	**0.660**	0.0389			
		Combined	6,794	0.685	0.0350			
	Sales Chg.	Control: Phase I	3,138	**1.25**	0.421	0.535	0.930	0.465
		Treatment: Phase II	3,656	**1.20**	0.206			
		Combined	6,794	1.22	0.224			
Specification 2	Emp. Chg.	Control: Non-SBIR	35,172	**1.34**	0.155	0.999	0.0002	0.0001
		Treatment: Phase I	3,138	**0.714**	0.0606			
		Combined	38,310	1.29	0.142			
	Sales Chg.	Control: Non-SBIR	35,172	**2.52**	0.506	0.973	0.0536	0.0268
		Treatment: Phase I	3,138	**1.25**	0.421			
		Combined	38,310	2.41	0.466			
Specification 3	Emp. Chg.	Control: Non-SBIR	35,172	**1.34**	0.155	1.00	0.000	0.000
		Treatment: Phase II	3,656	**0.660**	0.0389			
		Combined	38,828	1.28	0.140			
	Sales Chg.	Control: Non-SBIR	35,172	**2.52**	0.506	0.992	0.0163	0.00820
		Treatment: Phase II	3,656	**1.20**	0.206			
		Combined	38,828	2.39	0.459			

Table 9: t-tests for equality of means across treatment and control candidate populations. Variables of interest are the change in employment and sales between 2007 and 2012, computed as (value2012 – value2007) / value2007. Treatment and control group variances are not assumed equal.

The mean employment level for the 3,138 Phase-I-only firms increases by 71% between 2007 and 2012, compared to 66% for the 3,656 Phase II firms; this difference is not statistically significant. A broad look at sales growth produces a similar result: Phase-I-only firms experienced mean growth in annual sales of 125% (in constant 2012 dollars) compared to 120% for the treatment group. This difference in means is again insignificant.

Table 10: Difference-in-Difference Results, Precise-match Samples (Spec. 1)

	Specification 1					
Treatment Group:	Phase I and Phase II					
Control Group:	Phase I only					
Matching Variables:	SIC, employment, sales		SIC, CBSA, employment, sales		SIC, Phase I awards, employment, sales	
Dependent Variable:	Sales	Empl.	Sales	Empl.	Sales	Empl.
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment * Period	**83600*** (28000)**	**0.377 (0.27)**	**-13000 (77300)**	**-0.209 (0.643)**	**-2710 (88200)**	**-0.495 (0.74)**
Treatment (Treat = 1, Control = 0)	40700*** (4900)	0.39*** (0.0456)	35900*** (10300)	0.472*** (0.0664)	142000*** (25700)	1.27*** (0.267)
Period (After = 1, Before = 0)	155000*** (36500)	2.19*** (0.218)	226000*** (81200)	2.73*** (0.575)	220000** (86700)	2.83*** (0.669)
Constant	330000*** (31100)	3.21*** (0.282)	197000*** (20700)	2.12*** (0.215)	168000*** (22600)	1.77*** (0.216)
Observations	6,196	6,196	2,584	2,584	3,478	3,478
R-squared	0.0130	0.0264	0.0155	0.0352	0.0191	0.0354

Notes: *** Significant at the 1 percent level
** Significant at the 5 percent level
* Significant at the 10 percent level
(Robust standard errors clustered by NAICS)

Table 10: Results for Specification 1. The models are of the functional form $y_{i\tau} = \alpha + \beta t_i + \gamma a_\tau + \delta t_i a_\tau + \varepsilon_{i\tau}$, where $y_{i\tau}$ is the level of sales or employment for firm *i* in period τ, t_i is a dummy variable indicating treatment status of firm *i*, a_τ is a dummy variable indicating either 2007 or 2012 values of the dependent variable, and $\varepsilon_{i\tau}$ is an error term that is assumed orthogonal. The variable of interest is δ, which is the coefficient on the interaction term between t_i and a_τ. Values in the bold row above represent estimated values of the difference-in-difference estimator: $\hat{\delta} = \left(\bar{y}_{(treat)(after)} - \bar{y}_{(treat)(before)}\right) - \left(\bar{y}_{(control)(after)} - \bar{y}_{(control)(before)}\right)$.

A treatment sample matched to control group candidate firms by industry, sales and employment, however, produces more interesting results. Table 10 (columns 1 and 2) presents the results of the precise-match difference-in-difference model comparing employment and sales growth of treated firms receiving Phase II awards to control firms that did not progress beyond Phase I. As in Lerner (1999), the treatment group outperforms the control group. Mean annual sales rises by $83,600 between 2007 and 2012 for Phase II firms compared to Phase-I-only firms, and the result is highly significant, with a p-value of 0.004. These treated firms also experience average relative growth of 0.377 employees over the period, but the result is not statistically significant (p-value 0.165). (Sort order is important to the matching procedure, and some re-sorted specifications achieve a positive result significant at the 10% level.) Figure 8 graphically depicts the difference-in-difference relationship.

Figure 8: Specification 1 Treatment and Control Groups

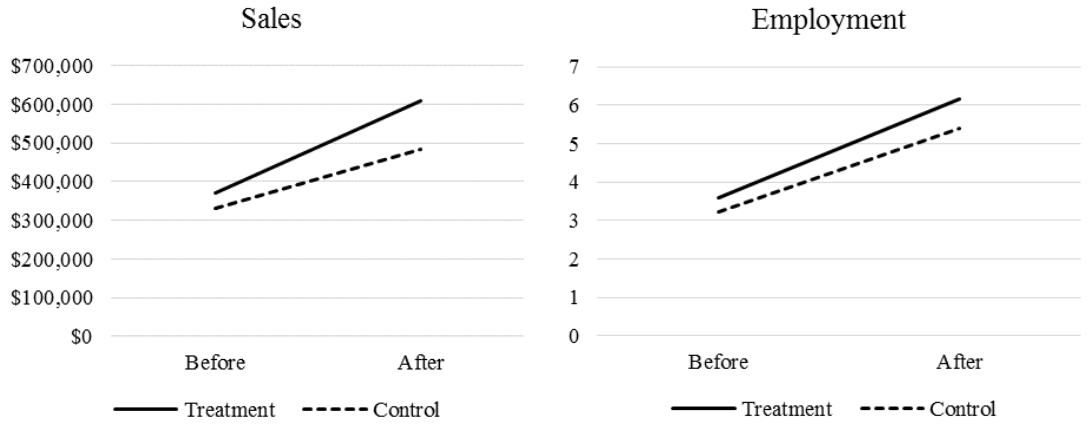

Figure 8: This chart provides visual representations of the difference-in-difference model specified in columns (1) and (2) of Table 10. The treatment group is composed of firms awarded Phase I and Phase II contracts. Control group firms participated in the program but did not progress beyond Phase I. Groups are matched on industry, 2007 employment and 2007 sales. The solid line represents the change in sales or employment of the treatment group between 2007 and 2012. The dashed line represents the change in sales or employment of the control group.

There are a number of reasons, however, why these results are suspect. Industry-, sales- and employment-level matching is insufficient to capture all of the relevant differences between the groups. More awards, rather than any resulting innovation and commercialization, may have produced the observed growth.

Geography, and the accompanying network effect, is a significant factor not captured in the analysis that likely leads to bias. Columns 3 and 4 of Table 10 show a model that also requires firm matches to be in the same CBSA; including this matching variable reverses the sign of the results and eliminates statistical significance, though at a reduced sample size of 635 treatment firms and 657 control firms. Columns 5 and 6 report results of a similar model with firms also matched on total Phase I awards rather than CBSA (using both reduces the sample size to an unworkably small number of firms). Again, the signs reverse and the results lose statistical significance. Matching is more difficult in this case, with 1,276 treatment firms matched to only 463 control firms.

Section 4.2: Phase I Recipients vs Non-SBIR/STTR Control Group (Spec. 2)

Specification 2 examines in isolation the control group from Specification 1: firms that were awarded Phase I contracts but failed to progress to Phase II during the 2007 to 2012 period. In the overall sample, the 3,138 Phase-I-only firms experienced average employment growth of 71% compared to 134% in a full sample of 35,172 non-SBIR/STTR firms (see Table 9). A t-test that the latter exceeds the former rejects the null hypothesis at a level of significance of 0.0001. Annual sales growth also reveals underperformance of Phase-I-only firms, with a 125% increase in the treatment group and 252% for non-participants.

The Specification 2 precise-match difference-in-difference model compares treatment firms to other government R&D contractors that did not participate in the SBIR/STTR program by matching on CBSA in addition to the Specification 1 parameters of industry, sales and employment. This model finds significant underperformance of Phase-I-only firms. Annual sales and employment decline by $1.88M and 11.7 employees relative to the control group (Table 11, columns 1 and 2; and Figure 9). These declines are statistically significant, with p-values of 0.006 and 0.005, respectively.

Figure 9: Specification 2 Treatment and Control Groups

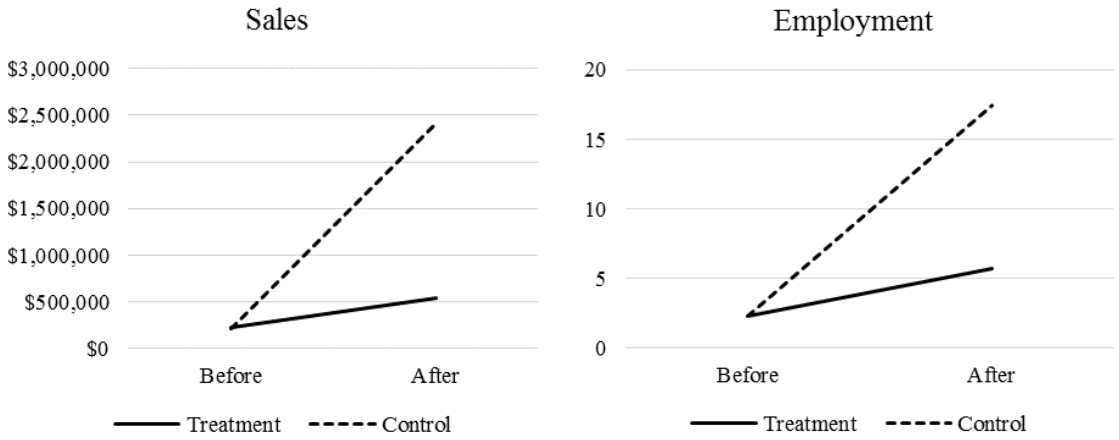

Figure 9: This chart provides visual representations of the difference-in-difference model specified in columns (1) and (2) of Table 11. The treatment group is composed of firms awarded Phase I contracts only. Control group firms did not participate in the program, but did receive other government R&D contracts during the period. Groups are matched on industry, CBSA, sales and employment. The solid line represents the change in sales or employment of the treatment group between 2007 and 2012. The dashed line represents the change in sales or employment of the control group.

Rather than limiting the sample to only those firms with precise control group matches, the propensity scoring method uses the probit model in Table 5 to estimate probabilities of inclusion in the treatment group based on a vector of firm characteristics.

Table 11: Difference-in-Difference Results, Precise-match Samples (Spec. 2-5)

	Specification 2		Specification 3		Specification 4		Specification 5	
Treatment Group:	Phase I only		Phase I and Phase II		Phase I only		Phase I and Phase II	
Control Group:	Non-SBIR R&D funding		Non-SBIR R&D funding		Non-SBIR R&D funding		Non-SBIR R&D funding	
Matching Variables:	SIC, CBSA, employment, sales				SIC, employment, sales			
Dependent Variable:	Sales	Empl.	Sales	Empl.	Sales	Empl.	Sales	Empl.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment * Period	**-1880000*** **	**-11.7*** **	**-1090000** **	**-7.07* **	**-1950000*** **	**-10.6** **	**-1830000*** **	**-14.4*** **
	(655000)	**(4.06)**	**(541000)**	**(3.79)**	**(669000)**	**(4.2)**	**(433000)**	**(3.98)**
Treatment	7330	-0.00318	36300***	0.356***	9420	0.065	37400**	0.318**
(Treat = 1, Control = 0)	(6740)	(0.0468)	(9470)	(0.0778)	(8140)	(0.0673)	(16700)	(0.124)
Period	2210000***	15.1***	1370000**	10.2***	2170000***	13.3***	2070000***	17.2***
(After = 1, Before = 0)	(663000)	(3.9)	(527000)	(3.76)	(674000)	(4.21)	(447000)	(4.07)
Constant	214000***	2.32***	418000***	4.01***	365000***	3.43***	697000***	6.16***
	(39200)	(0.39)	(65900)	(0.486)	(49000)	(0.418)	(82200)	(0.702)
Observations	3,804	3,804	3,582	3,582	8,806	8,806	8,646	8,646
R-squared	0.0048	0.0072	0.0047	0.0049	0.0050	0.0066	0.0055	0.0043

Notes: *** Significant at the 1 percent level
** Significant at the 5 percent level
* Significant at the 10 percent level
(Robust standard errors clustered by NAICS)

Table 11: Results for Specifications 2-5. The models are of the functional form $y_{i\tau} = \alpha + \beta t_i + \gamma a_\tau + \delta t_i a_\tau + \varepsilon_{i\tau}$, where $y_{i\tau}$ is the level of sales or employment for firm *i* in period τ, t_i is a dummy variable indicating treatment status of firm *i*, a_τ is a dummy variable indicating either 2007 or 2012 values of the dependent variable, and $\varepsilon_{i\tau}$ is an error term that is assumed orthogonal. The variable of interest is δ, which is the coefficient on the interaction term between t_i and a_τ. The values in the shaded row of the table above represent estimated values of the difference-in-difference estimator: $\hat{\delta} = \left(\bar{y}_{(treat)(after)} - \bar{y}_{(treat)(before)}\right) - \left(\bar{y}_{(control)(after)} - \bar{y}_{(control)(before)}\right)$.

This technique finds the best matches available from the 35,172 potential control group firms that had active government R&D contracts over the period. Treatment and control groups are then used to estimate the ATT. Matching treatment to control firms based on propensity scores allows for matching on the additional characteristics of firm age and total federal funding without excessive reductions in sample size. It produces negative coefficients, as reported in Table 12.

Table 12: PS Matching Estimates of Average Treatment Effect on the Treated

		Coef.	AI Robust Std. Error	z	P > | z |	Obs.
Spec. 2	Employment	**-3.83**	2.74	-1.40	0.162	38,310
	Annual Sales	**-3.12**	2.41	-1.29	0.196	38,310
Spec. 3	Employment	**-2.17**	0.713	-3.04	0.002	38,828
	Annual Sales	**-2.50**	0.763	-3.27	0.001	38,828
Spec. 4	Employment	**-3.38**	1.54	-2.20	0.028	38,310
	Annual Sales	**-2.98**	1.42	-2.11	0.035	38,310
Spec. 5	Employment	**-2.69**	0.705	-3.81	0.000	38,828
	Annual Sales	**-3.20**	0.821	-3.89	0.000	38,828

Table 12: Estimates of the average treatment effect on the treated (ATT) in the propensity score matching models in Table 5. Reported values are differential growth in employment and annual sales computed as (value2012 – value2007) / value2007. Estimates use Abadie and Imbens (2012) robust standard errors.

Employment growth in the Phase I treatment group is 383% lower than the control group over six years, but with an insignificant p-value of only 0.162. Sales growth is also lower (-312%) and lacking statistical significance, with a p-value of 0.196. I apply a difference-in-difference model similar to the precise-match model employed above, but using the treatment and control groups generated by propensity score matching. The results presented in Table 13 are even more dramatic than those from the smaller samples derived from precise matching on firm characteristics. Phase-I-only firms added 13.7

Table 13: Difference-in-Difference Estimates with PS Matching

	Specification 2		Specification 3		Specification 4		Specification 5	
Treatment Group:	Phase I only		Phase I and Phase II		Phase I only		Phase I and Phase II	
Control Group:	Non-SBIR R&D funding		Non-SBIR R&D funding		Non-SBIR R&D funding		Non-SBIR R&D funding	
PS Match Variables:	CBSA/SIC matches, employment, log sales, age, funding		CBSA/SIC matches, employment, log sales, age, funding		SIC matches, employment, log sales, age, funding		SIC matches, employment, log sales, age, funding	
Dependent Variable:	Sales	Empl.	Sales	Empl.	Sales	Empl.	Sales	Empl.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment * Period	**-2370000*** **	**-13.7*** **	**-6070000** **	**-27.5*** **	**-4090000** **	**-21*** **	**-6340000*** **	**-33.4*** **
	(753000)	**(3.49)**	**(2420000)**	**(8.12)**	**(1910000)**	**(6.34)**	**(2440000)**	**(8.48)**
Treatment	-594000***	-2.02**	-273000	-1.94**	-487000***	-1.48*	-867000***	-3.46***
(Treat = 1, Control = 0)	(206000)	(0.802)	(419000)	(0.96)	(186000)	(0.811)	(278000)	(0.997)
Period	2470000***	16.1***	5880000**	29.9***	4180000**	23.5***	6520000***	36***
(After = 1, Before = 0)	(732000)	(3.37)	(2380000)	(8.06)	(1900000)	(6.28)	(2420000)	(8.42)
Constant	2040000***	12***	3100000***	18***	1920000***	11.3***	3330000***	19.5***
	(167000)	(0.557)	-273000	(0.641)	(143000)	(0.577)	(203000)	(0.708)
Observations	9,592	9,592	8,228	8,228	9,686	9,686	8,308	8,308
R-squared	0.0047	0.0073	0.0024	0.0051	0.0018	0.0044	0.0031	0.0071

Notes: *** Significant at the 1 percent level
** Significant at the 5 percent level
* Significant at the 10 percent level
(Robust standard errors)

Table 13: Difference-in-difference estimates using all treated observations and control groups derived using propensity score matching from the probit model in Table 5.

fewer employees and $2,370,000 less in annual sales than the control group over six years, with p-values of 0.000 and 0.002, respectively. Unlike the ATT reported above, however, standard errors in this model do not account for propensity score estimation in derivation of the treatment and control groups.

Specification 4 is similar to Specification 2, without requiring that matched control firms be in the same CBSA as their treated paired firm. Here I examine the bias effect suggested by the inclusion of CBSA in the Specification 1 model. Based on the theory of agglomeration synergies, dropping the CBSA matching requirement should bias the results upward in absolute value. In the precise-match difference-in-difference model of Specification 4, Phase-I-only firms experience mean declines in annual sales and employment of $1.95M and 10.6 employees relative to their peers, with highly significant p-values of 0.004 and 0.012, respectively (Table 11, columns 5 and 6, Figure 10).

Figure 10: Specification 4 Treatment and Control Groups

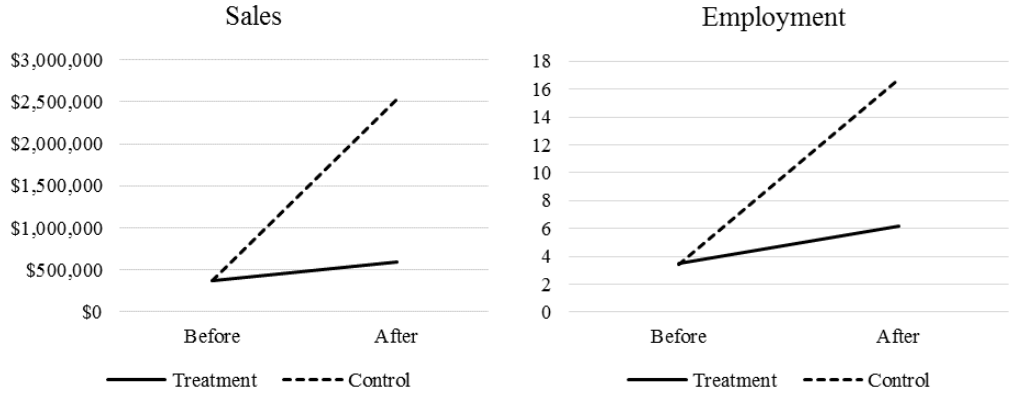

Figure 10: This chart provides visual representations of the difference-in-difference model specified in columns (5) and (6) of Table 11. The treatment group is composed of firms awarded Phase I contracts only. Control group firms did not participate in the program, but did receive other government R&D contracts during the period. Groups are matched on industry, sales and employment, but not CBSA. The solid line represents the change in sales or employment of the treatment group between 2007 and 2012. The dashed line represents the change in sales or employment of the control group.

The propensity score matching model estimates the ATT for employment is a relative drop of 338%, with a p-value of 0.028. Sales growth is also poor, dropping by 298% relative to the control, with a p-value of 0.035 (Table 12). Difference-in-difference estimates calculated using the propensity-score-matched treatment and control groups provide even more sharply negative results, as reported in Table 13, columns 5 and 6.

I contrast these results to those in Specification 2, in which firms required a geographically matching control firm to be included in the sample. Phase-I-only firms in Specification 4 performed at levels similar to the non-geographically-matched control group ($1.95M and 10.6 employee mean decline) as compared to Specification 2, which did require a geographic match ($1.88M and 11.7 employee mean decline). Thus, for Phase-I-only firms we do not see strong bias associated with the geography variable.

The finding that Phase-I-only firms underperform challenges the certification hypothesis that SBIR/STTR awards serve as a signal to outside investors, but it is not conclusive. Private venture capitalists may observe the failed attempt to progress to Phase II and choose not to invest. The early success and later failure may result from clever innovation combined with a management team that is unable to capitalize on the opportunity. Such management difficulties would likely be observed by private investors. Ultimately the results are inconclusive.

Section 4.3: Phase II Recipients vs Non-SBIR/STTR Control Group (Spec. 3)

Specification 3 is more revealing. It examines successful SBIR/STTR participants who continued to progress through the program and finds they also performed significantly worse than similar firms over the six-year period. Compared to the full non-

participant group of 35,172 firms growing at average rates of 134% in employment and 252% in sales over six years, the 3,656 firms receiving Phase II funding grew at only 66% and 120%, respectively (see Table 9). In the precise-match difference-in-difference model, annual sales declined by $1.09M and employment by seven employees relative to the control group, with p-values of 0.049 and 0.067, respectively (Table 11, columns 3 and 4; and Figure 11). Propensity score matching provides additional evidence of underperformance for Phase II award recipients (Table 12), with an ATT of -217%

Figure 11: Specification 3 Treatment and Control Groups

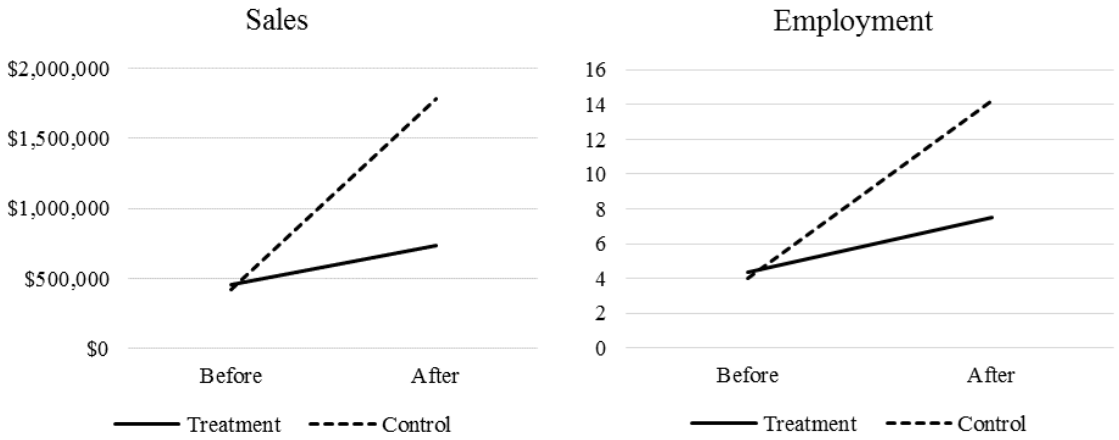

Figure 11: This chart provides visual representations of the difference-in-difference model specified in columns (3) and (4) of Table 11. The treatment group is composed of firms awarded Phase I and Phase II contracts. Control group firms did not participate in the program, but did receive other government R&D contracts during the period. Groups are matched on industry, CBSA, sales and employment. The solid line represents the change in sales or employment of the treatment group between 2007 and 2012. The dashed line represents the change in sales or employment of the control group.

growth in employment and -250% growth in sales (with p-values of 0.002 and 0.001, respectively). Difference-in-difference estimates in Table 13 show Phase II firms growing by 27 fewer employees (p-value = 0.001) and $6.07M less in annual sales (p-

value 0.012) than matched control firms (again with standard errors not accounting for first-stage propensity score estimation).

Similar to Specification 4, Specification 5 examines the findings in Specification 3 by removing the geography variable. Phase II participants in Specification 5 experience similarly poor performance, with declines of $1.83M and 14.4 employees (p-values < 0.0005) in the precise-match difference-in-difference model (Table 11, columns 7 and 8; Figure 12).

Figure 12: Specification 5 Treatment and Control Groups

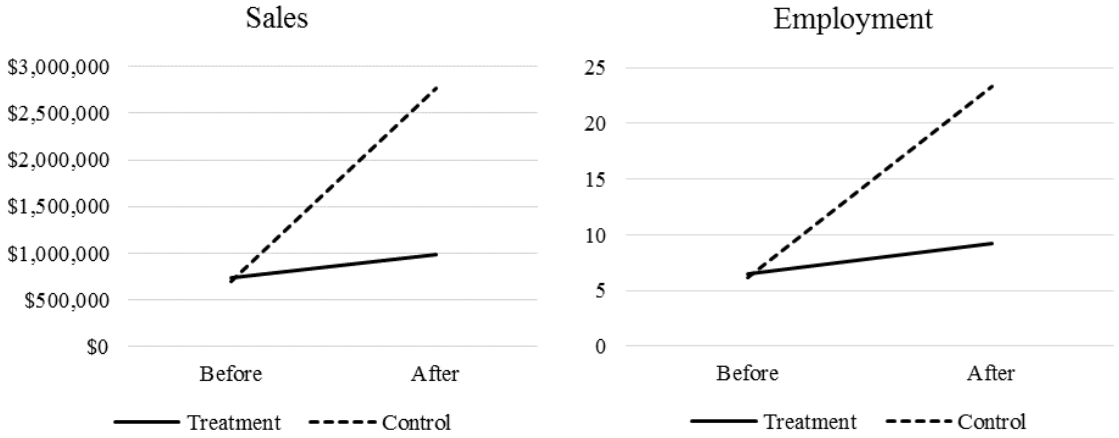

Figure 12: This chart provides visual representations of the difference-in-difference model specified in columns (7) and (8) of Table 11. The treatment group is composed of firms awarded Phase I and Phase II contracts. Control group firms did not participate in the program, but did receive other government R&D contracts during the period. Groups are matched on industry, sales and employment, but not CBSA. The solid line represents the change in sales or employment of the treatment group between 2007 and 2012. The dashed line represents the change in sales or employment of the control group.

Propensity score matching produces an ATT of -269% in employment growth and -320% in sales growth with strong statistical significance (p-value < 0.0005) and the propensity-

score-matched difference-in-difference model produces even stronger evidence of underperformance (Table 13, columns 7 and 8).

Unlike Phase-I-only firms, performance of this treatment group relative to the control ($1.83M and 14.4 employee mean decline; see Table 11) was greater than that of the geographically-matched sample in Specification 3 ($1.09M and 7 employee mean decline), with stronger levels of statistical significance. As we saw in Specification 1, regional agglomeration effects may account for this bias upward in absolute value. Firm performance relative to neighboring firms is more similar, while performance differences relative to a national baseline are magnified. I speculate that the absence of this effect for less experienced Phase-I-only firms may reflect a failure to integrate with the local community of similar firms.

The finding in Specification 3 provides support for the hypothesis that SBIR/STTR participants excel not at innovation, but at remaining small and collecting subsidies while their peers grow. The Specification 3 result is also interesting in comparison to Specification 2. Both groups participated in the program, but firms with longer periods of participation and higher levels of funding experienced levels of poor performance similar to those that failed early on. It is apparent from this result that failure in the SBIR/STTR program (i.e., non-progression to Phase II) is not the operative indicator of broader market failure.

Section 4.4: Alternative Models (Ordinary and Weighted Least Squares)

Ordinary and weighted least squares estimates of treatment effects presented in Table 14 similarly suggest that SBIR/STTR participants grow more slowly than their

Table 14: Ordinary and Weighted Least Squares Estimates

	Specification 1: OLS		Specification 2: OLS		Specification 2: WLS		Specification 3: OLS		Specification 3: WLS	
Treatment Group:	Phase I and Phase II		Phase I only				Phase I and Phase II			
Control Group:	Phase I only		Non-SBIR R&D funding				Non-SBIR R&D funding			
Dependent Variable:	Sales Chg.	Empl. Chg.	Sales Chg.	Empl. Chg.	Sales Chg.	Empl. Chg.	Sales Chg.	Empl. Chg.	Sales Chg.	Empl. Chg.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment Indicator	**-0.385**	**-0.173***	**-2.32*****	**-1.44*****	**-1.47*****	**-1.4****	**-1.95*****	**-1.65*****	**-3.12*****	**-2.37*****
	(0.406)	**(0.09)**	**(0.86)**	**(0.297)**	**(0.417)**	**(0.656)**	**(0.728)**	**(0.338)**	**(0.767)**	**(0.658)**
Number of firms of	0.00205	0.000676***	0.00409***	0.00214***	0.000984***	0.000661***	0.00424***	0.00216	-0.000555***	-0.00018
matching SIC & CBSA	(0.00184)	(0.000233)	(0.000877)	(0.000599)	(0.000363)	(0.000244)	(0.000808)	(0.00174)	(0.000209)	(0.000415)
Employment, 2007	-0.00566***	-0.00341***	0.00326	-0.000328***	-0.000714	-0.000355	0.00328	-0.000322***	-0.00424***	-0.00421***
	(0.00203)	(0.000818)	(0.00233)	(0.000116)	(0.000634)	(0.000489)	(0.00233)	(0.000124)	(0.00061)	(0.000371)
Log Annual Sales, 2007	-0.0535**	-0.0178**	-0.319	-0.0323*	-0.0449**	0.00191	-0.336	-0.0321	-0.0432	-0.00741
	(0.0211)	(0.00812)	(0.211)	(0.0186)	(0.0205)	(0.0369)	(0.218)	(0.0203)	(0.034)	(0.0144)
Firm Age in 2012	-0.0000473	-0.0209***	0.065	-0.00461	-0.01***	-0.00632**	0.0651	-0.0048	-0.00357	-0.000972
	(0.0235)	(0.00362)	(0.077)	(0.00827)	(0.00272)	(0.00269)	(0.0771)	(0.00826)	(0.00394)	(0.00381)
Log Federal Funding	0.233**	0.11***	0.179	0.124***	0.0628	0.283	0.182	0.127***	0.317	0.344**
	(0.0995)	(0.0389)	(0.143)	(0.0423)	(0.0838)	(0.2)	(0.143)	(0.0386)	(0.227)	(0.16)
Constant	-1.16	-0.261	3.08**	0.897***	2.11**	-1.51	3.26**	0.843**	0.115	-1.73
	(1.38)	(0.48)	(1.46)	(0.296)	(1.04)	(2.19)	(1.49)	(0.352)	(3.14)	(1.88)
Observations	6,736	6,736	36,191	36,191	38,056	38,056	36,703	38,828	38,674	38,828
R-squared	0.0006	0.0149	0.0012	0.0008	0.0048	0.0070	0.0012	0.0009	0.0121	0.0103

Notes: *** Significant at the 1 percent level
** Significant at the 5 percent level
* Significant at the 10 percent level
(Robust standard errors clustered by CBSA)

Table 14: Least squares models estimate $y_i = \alpha + \beta t_i + \gamma X + \varepsilon_i$ where y_i represents either the percent growth in employment or sales for firm i calculated as (value2012 – value2007) / value2007. t_i, indicates SBIR/STTR participation with β the coefficient of interest. Control variables are included in the matrix X. Error is captured in ε_i. WLS frequency weights are the number of matching firms based on propensity scores calculated in Table 5.

peers. Using the propensity scores estimated in Table 5, I again compare the matched treatment and control groups used above in an ordinary least squares model that includes the treatment dummy as an independent variable. I also use the number of firms calculated to be matches to weight each observation in the model. Positive Specification 1 results reported above are absent in this model, while in Specifications 2 and 3 participating firms are again shown to grow significantly slower than their peers.

Section 5: Conclusion

Competing schools of thought on the effects of government subsidies on private R&D efforts come to different conclusions. One school emphasizes the externality problems associated with private innovation. Non-appropriable, non-rival ideas spill over to the broader marketplace, producing large social benefits that can only be partially captured by individual investors. Agency conflict in venture capital markets excludes small, dynamic innovators from competition for private-sector financing. This line of reasoning arrives at the conclusion that government subsidies are necessary to correct market failures and maximize innovation and growth of the national economy. The alternative literature asks not whether market failures exist, but whether government agents are able to correct them. Proponents of this school suggest that bureaucrats administering subsidy programs do not have the strong incentives faced by private investors to select high-quality projects and carry them through to commercialization. Applicants for the subsidies are incentivized not to innovate and grow, but to negotiate the process to win government contracts and to remain small and thus eligible for funding.

The conclusion of this investigation is that support for the former position seems to rely on outlying case studies and on evidence provided by individuals motivated to paint the program in a positive light. Using more objective methods and sources of data, recipients of SBIR/STTR funding are found to dramatically underperform relative to their peers. In the most telling model presented here (Specification 3), program participants that progressed successfully through the program experienced growth in annual sales more than $1.09M, or more than 200%, lower than that experienced by similar firms which did not receive subsidies. Their employment growth over the six-year period examined was lower by approximately 7 employees, or again more than 200% (see Table 11 and Figure 11). These results are stronger under alternative specifications of the model (Table 13).

Careful consideration of the evidence presented leads to the conclusion that the results may be worse than the numbers seem to suggest. The model aggregates funding over the full period of the study, meaning that awards received in 2012 are treated the same as those received in 2007. This is unlikely to be the case. Effects of participation on employment and sales growth take time to manifest. While we likely see the full effect for 2007 participants, 2012 participants would continue to feel an impact beyond the end of the observation period.

A subsidy program that distributes funds collected through taxation creates deadweight losses that can be avoided through private funding mechanisms. Optimal public subsidy programs must therefore target marginal firms not funded by the private sector. We can combine this intuition with empirical evidence on the dynamics of small

business in the U.S. economy. Small businesses generate substantial employment growth as they emerge, but few survive beyond five or ten years. The net contribution to growth of the small business sector is far smaller than its gross contribution. The sector relies on a few star performers to make up the difference.

Where on the continuum of net project returns is the marginal firm that just misses out on obtaining private venture capital funding (Figure 2)? This is the firm that should be targeted by subsidy programs such as SBIR/STTR. Intel and Apple may have obtained government startup subsidies in their early years but it is unlikely that Gordon Moore or Steve Jobs would have failed in the absence of public support. That they received subsidies may represent a failure rather than a success of program administrators. Which firms should SBIR/STTR administrators target? Where is the marginal firm? Given the long tail of the distribution of small business performance, and the fact that private venture capital investments usually fail, the marginal unfunded firm may be one that would be ultimately unsuccessful in making a positive social contribution.

I leave determination of the placement of this critical margin for future research, but its importance reveals a deeper theoretical problem for the SBIR/STTR program than the empirical evidence presented here. The empirical evidence suggests that subsidizing R&D for small businesses is a challenge given the misalignment of incentives and the administrative challenge of picking winners. The theoretical evidence suggests that effective subsidy programs may be, in fact, impossible. If the marginal firm in the competition for private investment does not produce positive social benefits, then even an

optimal subsidy program will target firms with a negative net impact on the economic performance of the U.S. economy. If a gap does exist between the margin of social benefit and the margin of private funding, it will be difficult for program administrators to target this gap if outcomes are consistently at odds with stated objectives, as suggested by these findings.

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CHAPTER 2: EFFECTS OF FEDERAL SOCIOECONOMIC CONTRACTING PREFERENCES

The 8(a) Business Development Program supports small disadvantaged U.S. federal contractors through benefits such as set-aside and sole source contracts, management and technical assistance, and mentor-protégé relationships with established firms. This study examines the effectiveness of the 8(a) program at stimulating firm-level growth by comparing 8(a) firms to those participating in other preferential contracting programs with different benefits. The average 8(a) program participant performs well relative to baseline firms that do not receive contracting preferences; however, these effects are driven largely by outliers and biased estimation techniques. They decrease substantially and even reverse upon application of matching techniques to generate quasi-experimental treatment and control groups. The program does not appear to achieve two of its key objectives: stimulating broad-based growth of disadvantaged businesses and greater competition among the population of government contractors.

(*JEL* H32, H57, L25, L53)

Keywords: Entrepreneurship, Firm Sales, Firm Size, National Subsidies, Policy, Public Economics, Public Expenditure

Section 1: Introduction

As the largest single buyer of goods and services in the world, the federal government of the United States has long sought to affect marketplace outcomes beyond the immediate objectives of procurement. The Small Business Administration (SBA) is empowered by Congress to oversee a wide array of preferential contracting programs. Legislation directs percentages of federal contracts be awarded to businesses owned by various social and economic categories of citizen. In addition to broad agency percentage goals, contracting officers are empowered to set aside contracts for competition only among firms owned by members of preferred socioeconomic groups and in some cases to award such contracts without competitive bidding. Subsidized loans and subcontracting incentives also benefit select categories of business owners. Preferred groups include small disadvantaged businesses (which benefit from a legislated goal of five percent of total federal spending), woman-owned small businesses (five percent) and service-disabled veteran-owned small businesses (three percent) [Small Business Act Sec. 15(g)(1)]. The 8(a) Business Development Program, named for Section 8(a) of the Small Business Act and administered by the SBA, goes beyond contract set-asides to provide broad support to disadvantaged businesses through financial and management assistance, mentoring programs, and non-competitive contract awards. This analysis examines the effectiveness of the 8(a) program in achieving its stated goals of fostering growth among disadvantaged businesses and competition among the broader pool of government contractors.

Prior evidence examining the effectiveness of similar government contracting preference programs has produced mixed results. Some studies find that well-executed

programs may indeed be successful at affecting market outcomes (Strupler and Wolter 2016, Boston and Boston 2007); others suggest that they are likely to be ineffectual (Black 1983), encourage gaming of the system (Bates and Williams 1995) or encourage minorities to invest their energies in non-viable business plans (Bates 2004). The underlying theme of all of these studies is that effectiveness depends largely on program characteristics and local circumstances. The federal government operates the largest such socioeconomic contracting program in the world by a wide margin, and yet its program has received remarkably little direct scrutiny. Most studies of it have been anecdotal, examining individual cases of fraud and mismanagement. This paper provides the first large-scale empirical evaluation of the 8(a) program, building on previous examinations of diverse state and local preferential contracting programs.

Theory does not offer clear answers on whether contracting preference programs based on socioeconomic characteristics of business owners will be effective. It is possible that historical patterns of discrimination have left "money on the table" in the form of more productive firms that fail in the competitive market for reasons orthogonal to ability (e.g., skin color). Alternatively, selecting federal contractors based on characteristics unrelated to contract performance may represent a pure subsidy to lower-performing firms and an impetus to costly rent-seeking and political competition. Higher costs to the government may or may not produce the intended results (Stigler 1971, Becker 1983). I examine these contrasting theories to determine which best fits the data.

The empirical design employed uses matching techniques to develop quasi-experimental treatment and control groups, taking advantage of differences between U.S.

federal programs targeted at different socioeconomic categories to distinguish the unique contribution to firm-level performance of various characteristics of the 8(a) Business Development Program. For example, minority- and woman-owned small businesses are often eligible for the 8(a) program, but many do not take part; these non-participants are not eligible for the associated contract preferences (see Table 15). Firms may apply for status as small disadvantaged businesses without applying directly to the 8(a) program; these non-8(a) small disadvantaged businesses, though benefiting from the same five-percent government contracting goal as 8(a) firms, are ineligible for sole source contracts and a range of other 8(a) program benefits. Service-disabled, veteran-owned businesses enjoy an overall funding goal as well as opportunities for set-asides and sole-source awards; they are ineligible for other 8(a) program benefits like technical assistance and facilitated mentor relationships with established firms.

Table 15: Small Business Socioeconomic Certifications

Type	Benefits
Minority Owned Small Businesses *(No explicit certification)*	No preferences aside from overall 23% small business spending target
Small Disadvantaged Businesses *(Mostly minority owned businesses)*	5% spending target
8(a) Business Development Program Participants *(Available to some small disadvantaged businesses)*	5% spending target *(Includes all small disadvantaged businesses)* Set-aside authority Sole source authority Management and technical assistance Business mentor-protégé relationships
Service-Disabled Veteran-Owned Small Businesses	3% spending target Set-aside authority Sole source authority
Woman-Owned Small Businesses	No preferences aside from overall 23% small business spending target (at time of sample)

Table 15: Spending targets refer to federal percentage-of-spend goals. Set-aside authority refers to contracts open to competition only among businesses of the specified type. Sole source authority refers to the ability of contracting officers to make awards without competition. Source: FAR Parts 19 and 26.

By comparing 8(a) firms to members of these other socioeconomic groups with different sets of benefits, this study explores the relative contribution of various program characteristics to firm-level growth. I link procurement data from the Federal Procurement Data System (FPDS) to employment and revenue figures from the National Establishment Time Series (NETS) using firm DUNS numbers reported in both systems. The result is a detailed dataset of 20,000 small businesses who received contract obligations between 2007 and 2012. Obligations in the data set amounted to $33 billion in 2012 (see Figure 13), representing approximately 35 percent of total federal spending to small businesses. Of these firms, 7,942 are minority-owned, 7,187 are woman-owned, 2,134 are service-disabled veteran-owned, and 4,013 are participants in the 8(a) Business Development Program (see Figure 14).

Figure 13: Total Obligations to Sampled Firms

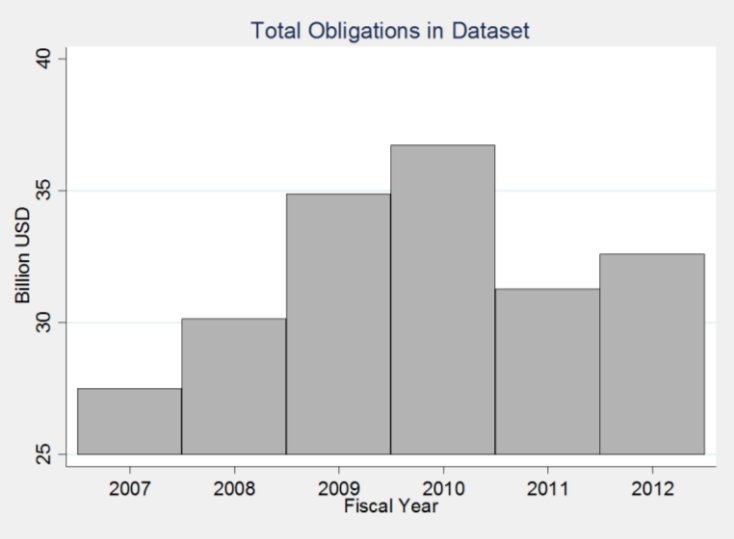

Figure 13: Total contract funding recorded in the Federal Procurement Data System (FPDS) for sampled firms between 2007 and 2012. Funding recorded for each firm is the maximum under a single socioeconomic category in a given year; funding recorded under distinct, non-overlapping categories in the same year is not captured.

Figure 14: Number of Firms in Sample by Socioeconomic Category

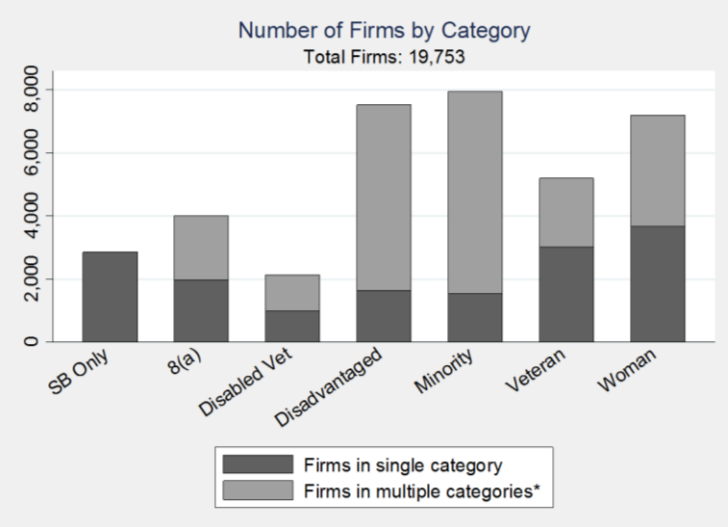

Figure 14: Number of firms in dataset by socioeconomic category. In dark grey are firms receiving obligations in one category only. In light grey are firms belonging to more than one category.
* Categories that overlap by definition are considered single-category: participants in the 8(a) program must be small disadvantaged businesses; service-disabled veteran-owned small businesses must be veteran-owned small businesses.

Cursory examination of the data suggests that the 8(a) program has strong positive effects on firm performance. Simple models and comparisons of means, however, fail to account for outliers and dissimilar characteristics across the underlying population of firms. Participants in the 8(a) program receive large amounts of funding relative to non-participating peers, making it challenging to disentangle immediate effects of contract awards from the longer-run impact on firm viability. I account for these problems by matching firms based on characteristics such as employment, sales and quantities of federal obligations. Here, a more complicated picture emerges. Relative to similar small disadvantaged businesses that are not participating in the 8(a) program, 8(a) firms perform well. Relative to matched minority- and woman-owned small businesses which do not participate in the 8(a) program, the observed 8(a) performance premium either

disappears or decreases substantially after accounting for the effects of outliers. Most strikingly, 8(a) firms underperform dramatically relative to service-disabled veteran-owned small businesses; this is the socioeconomic category most similar to 8(a) participants in terms of benefit eligibilities, which include contract set-asides and sole-source awards.

Section 2 reviews details of socioeconomic program implementation, previous research into their efficacy and the economic theory behind them. Section 3 describes the dataset and empirical approach. Section 4 presents results and tests of robustness. Section 5 discusses the significance of these findings and Section 6 concludes.

Section 2: Background and Theory

Efforts to achieve policy goals through federal contracting began in earnest with the Small Business Act of 1953, although similar programs existed in the 1940's as part of the New Deal and the war effort. The justification for activist contracting policy changed over time, as described by Anglund (2000). The earliest arguments emphasized fairness, and not until the 1960's and 1970's did the focus shift to civil rights (Kotlowski 1998). In the 1980's and 1990's the focus shifted again, this time to economic impact. With economists highlighting the dynamism of small businesses, legislators began to use job creation, innovation and exports as justifications for preference programs. Subsequent evidence caveating the economic impact of small businesses did little to curb legislative enthusiasm for subsidies (Davis, Haltiwanger and Schuh 1996; Decker, et al. 2014).

Amounts involved are not insignificant. According to the most recent scorecard published by the SBA, in fiscal year 2014 agencies awarded over $90 billion to small

businesses, including $35 billion to small disadvantaged businesses (see Figure 15), $14 billion to service-disabled veteran-owned small businesses (Figure 16), and $18 billion to woman-owned small businesses (Figure 17).

Figure 15: Contract Spending to Small Disadvantaged Businesses

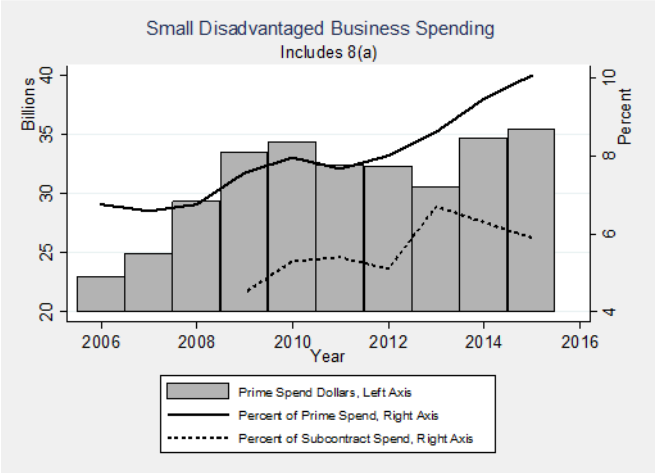

Figure 15: The federal government's goal for contract obligations to small disadvantaged businesses (SDB) is five percent of total spend. The SDB category includes 8(a) firms. Data are published at https://www.sba.gov/contracting/finding-government-customers/see-agency-small-business-scorecards.

Figure 16: Contract Spending to Service-Disabled Veteran-Owned Small Businesses

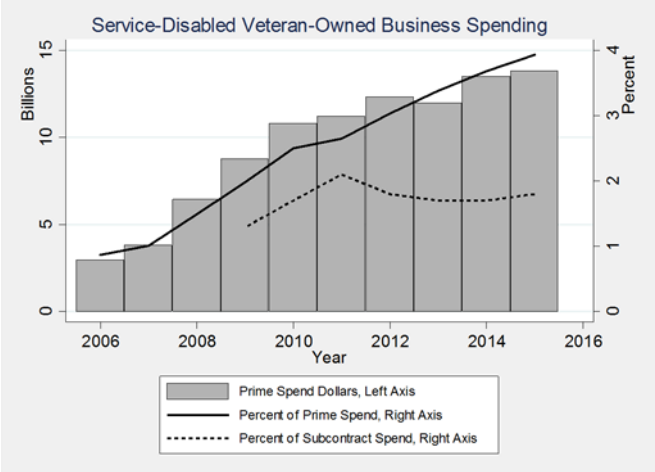

Figure 16: The federal government's goal for contract obligations to service-disabled veteran-owned small businesses is three percent of total spend. Data are published at https://www.sba.gov/contracting/finding-government-customers/see-agency-small-business-scorecards.

Figure 17: Contract Spending to Woman-Owned Small Businesses

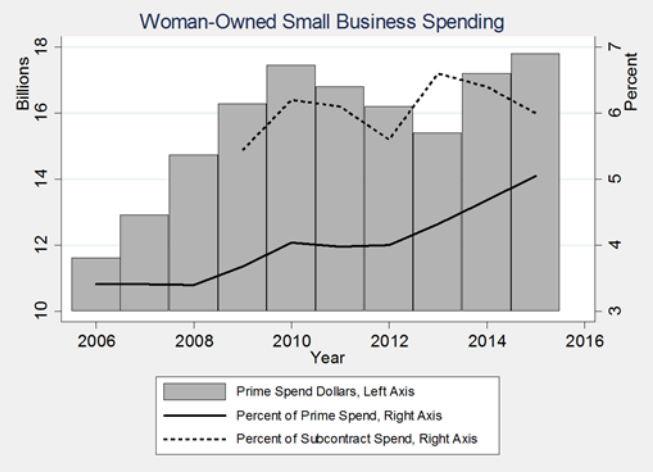

Figure 17: The federal government's goal for contract obligations to woman-owned small businesses is five percent of total spend. Data are published at https://www.sba.gov/contracting/finding-government-customers/see-agency-small-business-scorecards.

Nevertheless, remarkably little research has examined the effectiveness of these programs in stimulating growth of businesses in targeted socioeconomic groups.

Section 2.1: Regulatory Context

The United States government uses various tools to direct contracts to favored groups (Anglund 2000, Kotlowski 1998, Leiter and Leiter 2002, McVay 2009). The most basic of these are the broad agency goals outlined above. The SBA assists in coordinating the efforts of federal agencies to ensure the government as a whole meets socioeconomic procurement targets. Although there are no explicit penalties for failing to meet these goals (and the government has only begun consistently to do so in recent years), pressure is strong at the agency level to comply. Regulations offer tools to assist contracting offices in meeting their goals. Contract set-asides, the most common tool, reserve a particular award for competition only among firms in a particular socioeconomic category. Members of some favored categories of firms, including 8(a) participants and

service-disabled veteran-owned small businesses, may also receive sole source contracts without a requirement for competition.

The 8(a) Business Development Program provides the most comprehensive set of benefits to disadvantaged businesses. Participants must be small businesses at least 51% owned and controlled by socially and economically disadvantaged U.S. citizens. Social disadvantage is defined by law as those subject to racial or ethnic prejudice or cultural bias due to identification as a member of certain groups. The individual must demonstrate personal experiences of substantial disadvantage within the United States and consequent negative impact on participation in business. Proof of economic disadvantage is also required, and includes a narrative description of personal circumstances as well as submission of financial records. Personal assets may not exceed $4 million, average annual personal income may not exceed $250,000 over the previous three years, and adjusted net worth may not exceed $250,000 (these values are occasionally adjusted, and were lower at the time the data here were collected).

Upon application and admittance to the 8(a) program, participants progress through two phases. The first is a four-year developmental stage in which firms are eligible for additional benefits such as the Mentor-Protégé Program. The second is a transition phase in which firms prepare to exit the program and survive without its associated benefits. Throughout this nine-year period, participants are eligible for sole-source contracts of up to $4 million for goods and services and $6.5 million for manufacturing, up to a program cap of $100 million. The SBA coordinates with other government agencies to facilitate these awards and encourage contracting officers to

consider 8(a) participants when filling requirements. Agencies may in effect pass requirements to the SBA, which then allocates funding on a sole-source basis to 8(a) firms. Participating firms may form joint ventures to take on larger contracts, and through the Mentor-Protégé Program may partner with other established government contractors who provide assistance in contract performance. Protégé firms may form joint ventures with mentor firms or relinquish to them up to 40% of ownership in return for capital investment.

The SBA monitors participating firms throughout the program to ensure they maintain a balance between commercial and government business. District offices work with firms within their regions by conducting evaluations and annual reviews and monitoring participants' business planning activities. Administrators provide business training, counseling, marketing assistance, and executive development through government resources or contracted partner firms. Participants may gain access to surplus government property, SBA-guaranteed loans, and bonding assistance.

The Small Business Act enumerates a number of testable hypotheses regarding the intended effects of the 8(a) Business Development Program. Apart from normative issues of social justice not considered here, it specifies the following:

1. "[T]he conditions of socially and economically disadvantaged groups…can be improved by providing the maximum practicable opportunity for the development of small business concerns…"

2. "[S]uch development can be materially advanced through the procurement by the United States of articles, equipment, services, materials, and construction work from such concerns…"
3. "[S]uch procurements also benefit the United States by encouraging the expansion of suppliers for such procurements, thereby encouraging competition among such suppliers and promoting economy in such procurements…." [Small Business Act Sec. 2(f)(1)]

Thus the explicit goals of the program are to (1) provide opportunities in business for disadvantaged groups, (2) encourage expansion of participating firms, (3) encourage competition among them, and (4) economize on government spending through purchasing in a more competitive environment. This analysis directly examines the second and third of these goals and, indirectly, the fourth.

Section 2.2: Prior Evidence

Substantial analysis supports the claim that historically disadvantaged groups experience poorer outcomes in the business environment. Firms owned by them tend to by younger, more highly leveraged, and less profitable (Bates 1985). Fairlie and Robb (2007, 2008) propose that the problem is one of historical opportunities. Minorities are less likely to have self-employed family members and are thus less likely to have entrepreneurship exposure that would contribute to success. The lingering effects of discrimination are perpetuated through low family- and peer-group educational opportunities. Service-disabled veterans and women face challenges of their own,

experiencing lower success rates in business relative to comparable peers from other socioeconomic groups (Cox and Moore 2013; Rosa, Carter and Hamilton 1996).

Minorities are also harmed through discrimination in credit markets; many studies find that minorities face strong headwinds in financing business activities (Blanchard, Zhao and Yinger 2008; Blanchflower, Levine and Zimmerman 2003; Cavalluzzo and Cavalluzzo 1998; Cavalluzzo and Wolken 2005). Other analyses examine the legal challenges to awarding contracts based on socioeconomic characteristics (Hopkins 1975, Rice 1992, Reeder and Vergilio 1996, Sirmons 2004, Sakallaris 2007).

Governments around the world have tried to overcome such biases (McCrudden 2004), and many studies evaluate the efficacy of affirmative action (Ashenfelter and Heckman 1976, Holzer and Neumark 2000, Marion 2009, Myers and Yuan 2013). The method of overcoming historical bias examined here is the application of public contracting policy to markets for goods and services in which governments play a major role. Some researchers who have examined these types of programs at the state and local levels found non-existent or counterproductive effects (Bates 2009; Davila, Ha and Myers 2012; Enchautegui, et al. 1996; Myers and Chan 1996; Sweet 2006). One of the earliest examinations looked at over 4,000 minority business enterprises that sold goods and services to state and local governments in 1987 (Bates and Williams 1996). Following up on them four years later, the authors found that those with substantial portions of revenue dependent on government contracting were more likely to go out of business. The authors hypothesize that many of these businesses may have been front companies for larger business concerns and disappeared after the contract in question

concluded. La Noue (2008) examines an agency-specific race-conscious contracting program at the U.S. federal level, and finds similar evidence of poor implementation.

Other studies find positive effects. Chatterji, Chay, and Fairlie (2014) look at city-level contract set-aside programs and find that the gap in business ownership rates between blacks and whites fell by three percentage points following implementation, although there was little discernible effect on minority employment overall. Bates and Williams (1995) suggest that success of preferential contracting programs depends on their design; poorly designed policies encourage the creation of front companies that pass on contracts to established firms. Bates and Williams' finding that well-designed programs can be successful is corroborated by more recent evidence (Bates 2015).

Despite the fact that U.S. government contracting programs designed to address socioeconomic outcome disparities are the largest in the world by a wide margin, there are few large-scale empirical studies testing their effects. Using early data, Black (1983) finds that these federal programs do not substantially increase the amount of funding going to minority-owned businesses. Some qualitative assessments are critical. Bates (2004) claims that the programs are "flawed in intent, design and implementation": they assume that capital acquired through debt and proprietor human capital are substitutes rather than complements. Minorities may receive little substantive assistance, or may be encouraged to proceed with non-viable business models. However, such assessments remain speculation without broad empirical support. The analysis presented here addresses this gap.

Section 2.3: Theoretical Foundations

Two competing theories underlie most of the previous research on the efficacy of preferential contracting programs designed to support disadvantaged groups of citizens. The first builds on the hypothesis that historical patterns of discrimination based on non-market characteristics such as race, sex and veteran status exclude some individuals from full participation in the market. This theory posits that the widely observed underperformance of such firms arises from either continuing discrimination or its legacy. Ability of disadvantaged firms to prosper is limited by characteristics of their owners that are orthogonal to inherent ability; performance will rise toward the level of the general population as government policies deliberately counteract the discriminatory practices. Even past discrimination may result in business owners that are less experienced and less capable of performing in the market. The work of Fairlie and Robb (2007, 2008) supports the proposition that these traits may be intergenerational. The personal characteristics leading to discrimination are unrelated to innate ability, and opportunities to participate more fully in the market will over time provide the experience that disadvantaged business owners need to succeed. Under this hypothesis, preferential contracting programs will improve firm performance relative to the population baseline as owners gain the experience they need to compete more broadly.

Alternatively, awarding contracts based on firm characteristics other than ability to perform may lead to a host of complications. At the most basic level, preferred firms are unlikely to execute contracts as effectively as firms awarded contracts under standard competitive procedures. Preferential contracting programs may also have effects on firm performance by encouraging suboptimal behavior. Rents generated by limited

competition may cause firms to compete along regulatory rather than performance dimensions. They will spend time and effort learning to navigate the bureaucratic environment up to the point where the additional benefits to be gained are dissipated through non-productive activities (Tullock 1967). The end result will be wasted effort on the part of preferred firms and lower long-run production. Alternatively, firms for whom subsidies are unavailable will apply their efforts to improving their business offering in order to win in a competitive environment.

These competing hypotheses are not mutually exclusive. Indeed, both likely play a role in determining final outcomes. The key question remains, which has the stronger effect? Are the inefficiencies of rent seeking and lack of competition outweighed by the benefits of raising disadvantaged firms to their full potential? Can the programs achieve the goals set out by their designers, encouraging both broad-based growth of disadvantaged firms and competition among them? These questions can only be answered empirically. If a wide selection of firms participating in preferential contracting programs outperform non-participating peers, then inefficiencies generated by the more burdensome regulatory environment may be justified. If, on the other hand, exceptional growth of a few firms creates monopolies perpetuated by the regulatory environment, then the ultimate goal will not be achieved. High mean growth will come at the expense of both median growth and the greater competition that arises from a population with more successful competitors.

The analysis to follow tests these competing theories. This is accomplished by comparing the 8(a) Business Development Program with four other federal

socioeconomic preference programs: those supporting minority-owned, woman-owned, service-disabled veteran-owned, and small disadvantaged businesses. Table 16 illustrates the benefits available to each category of firm relative to 8(a) firms as well as summarizing the key findings of the analysis to follow. By examining benefit differences and their effects on relative performance levels, the analysis illuminates the complex relationships between program characteristics that promote broad-based growth and those that serve only to generate monopoly rents for favored firms. These relationships hinge on two distinct findings: (1) Do 8(a) firms, on average, outperform the comparison group? And, (2) Is the relative performance difference driven by 8(a) outliers, or by the main body of 8(a) firms? There are several possibilities:

1. If 8(a) firms outperform, but the effect is driven by outliers, then the program accomplishes the first goal of improving performance, but at the expense of the median firm. Thus it inhibits rather than fosters competition (i.e., it creates opportunities for monopoly rents).
2. If 8(a) firms outperform and the effect is not driven by outliers, then we have evidence that the program is working as designed (i.e., it encourages growth and broad-based competition).
3. If 8(a) firms underperform, then we have evidence that the program does not accomplish the first goal of encouraging growth. If the program damages growth prospects for participating firms, competition effects are largely irrelevant.

Table 16: Available Benefits by Program

	Contracting Goal (not including 23% small business goal)	Set-aside Authority	Sole-source Authority	Management Assistance and Mentorship	8(a) Relative Performance	Dominant Outliers
Minority-owned	No	No	No	No	+	8(a)
Woman-owned*	No	No	No	No	+	8(a)
Disadvantaged	Yes	Yes	No	No	+	Disadvantaged
Service-disabled Veteran-owned	Yes	Yes	Yes	No	-	Service-disabled Veteran-owned
8(a) Participant	Yes	Yes	Yes	Yes		

Table 16: Total contract funding recorded in the Federal Procurement Data System (FPDS) for sampled firms between 2007 and 2012. Funding recorded for each firm is the maximum under a single socioeconomic category in a given year; funding recorded under distinct, non-overlapping categories in the same year is not captured.
* At the time of data collection.

Three caveats are important here. First, due to heavy positive skew in growth distributions, we do not see the fourth possibility of 8(a) outliers driving underperformance. Second, it might be argued that high 8(a) performance driven by outliers simply mirrors what we see in the broader population of small businesses. One must bear in mind, however, that the matching procedures employed compare treatment firms to peers; thus, the outliers are not only causing the mean growth premium that we witness, they are doing so at a higher rate than in a comparable group of firms. Relative to the population, the group of 8(a) firms is growing more concentrated, not simply following the usual pattern. Third, the analysis relies on an important assumption: that individual business owners from various groups do not, in fact, possess fundamental differences in ability that would cause relative performance variation regardless of status, training, or any other variable that can be manipulated through policy intervention.

Section 3: Dataset and Empirical Approach

The Federal Procurement Data System maintains records of all significant government contracts and tracks a wealth of contract attributes including the socioeconomic status of awardees. Contracting officers throughout the federal government enter data into the system, which are then aggregated and used to monitor various performance metrics such as small business utilization rates. Data in the system are prone to error, but improved significantly upon passage of the Federal Funding Accountability and Transparency Act of 2006. In particular, awardees from 2007 onward are required to obtain DUNS numbers which are then used to track firms in FPDS. For this reason, the analysis here includes only contracts awarded after 2006. Dun and Bradstreet, the originator of the DUNS number system, also uses the numbers in monitoring firms' credit quality. Credit tracking information includes annual reporting of firm employment size and sales receipts, and is published in the NETS database. By linking FPDS data to NETS data by firm DUNS number, I pair federal contracts with awardee characteristics.

The matching procedure also assists in cleaning error-prone FPDS data; of more than 120,000 small business establishments active in FPDS over the period examined, I match 21,089 with DUNS numbers extracted from the NETS. To simplify the analysis, a further 505 firms consisting of more than one establishment were removed. Eliminating firms with zero employment or sales throughout the period brings the dataset to 19,855 observations. I remove firms annotated as 8(a) participants which are not also designated as small disadvantaged businesses since SDB certification is a prerequisite for the 8(a) program and the discrepancy implies erroneous data entry. I similarly remove firms with

more than 500 employees in 2007 and those with several different headquarters DUNS numbers over the period examined (implying M&A activity) to arrive at the final dataset with 19,753 observations.

Section 3.1: Dataset Description

The linked databases provide all of the variables necessary for the analysis. They identify firms as small or large (by the government's definition, which varies across different industries), minority-owned, woman-owned, disadvantaged, veteran-owned, or as 8(a) participants. They record firms' location, age, credit score and industry by 8-digit Standard Industrial Classification (SIC) and 6-digit North American Industrial Classification System (NAICS) code. Most importantly, they record annual employment and sales estimates, allowing evaluation of growth. NETS data are current as of the first day of the year and are available through 2013. The beginning of the dataset thus includes firm-level employment and sales as of January 2007, and federal obligations to those firms in the following year (2007). The end of the dataset includes obligations in 2012, and employment and sales figures as of the end of that year, or January 2013.

Many firms hold more than one socioeconomic classification. Minority-owned firms, for example, may also be woman-owned. Participants in the 8(a) program must be small disadvantaged businesses, which are usually minority-owned. To avoid confounding effects of different programs, most of the following analysis is limited to firms that carry only a single category certification (with the exception of 8(a)/small disadvantaged businesses and disabled-veteran-/veteran-owned businesses, which are embedded by definition). Figure 18, Figure 19, and Figure 20 illustrate the overlap of

various classifications by number of firms. As Figure 18 shows, all 8(a) firms are small disadvantaged businesses, and 93 percent of them are also minority-owned businesses. In Figure 19 we see that only 11 percent of 8(a) firms are owned by service-disabled veterans. Woman-owned firms make up 35 percent of 8(a) participants (Figure 20).

Figure 18: Minority-Owned, SDB and 8(a) Program Overlap

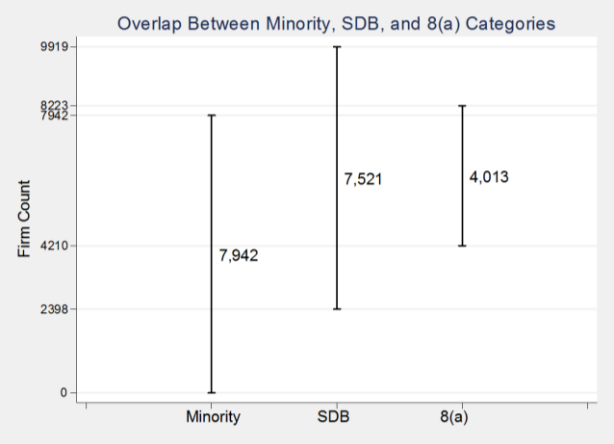

Figure 18: There are 4,013 firms in the dataset participating in the 8(a) program; certification as a small disadvantaged business is a requirement for participation. Of these, 281 firms are not minority-owned, but maintain SDB certification through other means. Of the 7,521 SDBs in the dataset, 1,812 are minority-owned and 1,977 are not.

Figure 19: Veteran-Owned, SDVOSB, and 8(a) Program Overlap

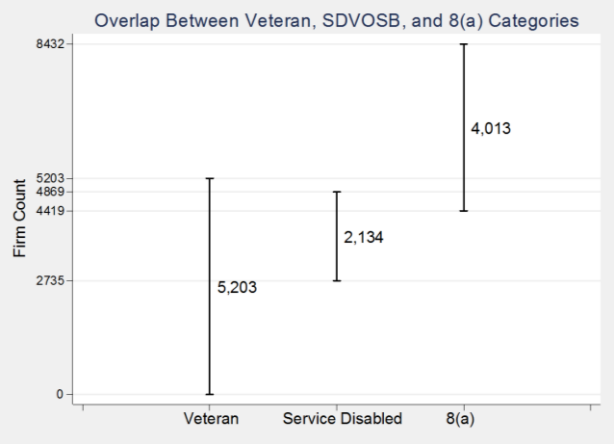

Figure 19: Of the 4,013 sampled participants in the 8(a) program, 784 are veteran-owned. Of these, 450 are service-disabled veteran-owned.

Figure 20: Woman-Owned and 8(a) Program Overlap

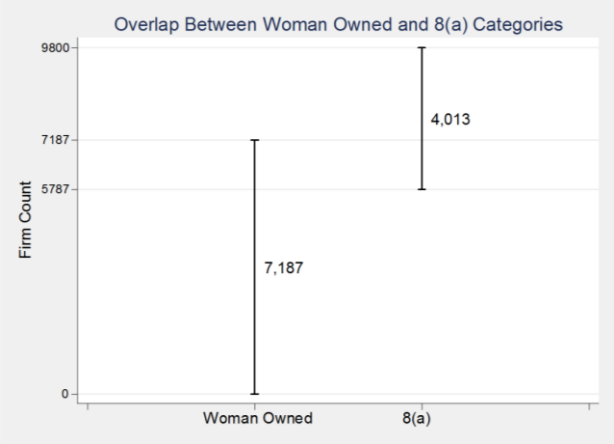

Figure 20: Of the 4,013 sampled participants in the 8(a) program, 1,400 are woman-owned.

Network effects are important to innovation and growth. Early theoretical descriptions of the phenomenon are found in Marshall (1920) and Krugman (1991), while later authors examine the effect empirically in the context of government support to R&D (Jaffe, Trajtenberg and Henderson, 1993; Audretsch and Feldman, 1996). As Figure 21 illustrates, federal small business funding is strongly concentrated in certain regions of

Figure 21: Concentration of Sampled Firms by County

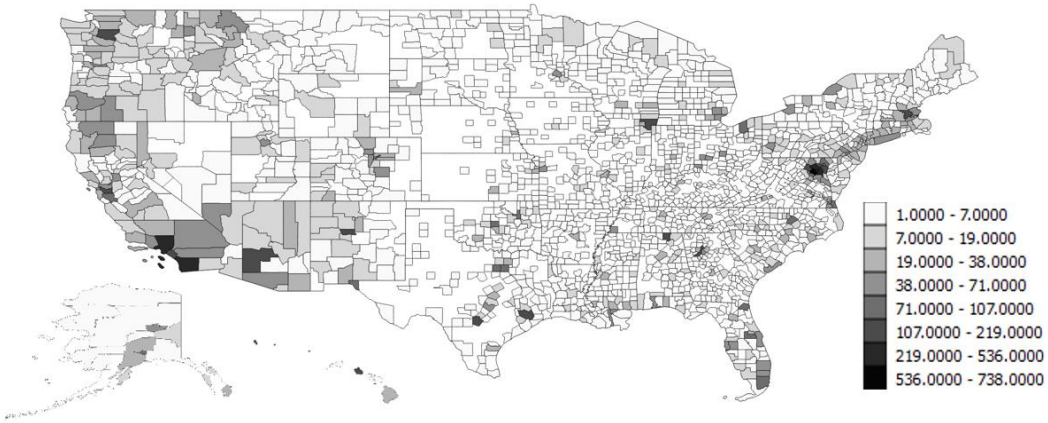

Figure 21: Counties are color-coded by total number of sampled small businesses as of 2007. Darker areas have a higher concentration of firms.

the United States, particularly near large cities and military installations. The models below assessing success rates of small-business contractors include geographic network effects by controlling for aggregate industry presence within the local area of a given firm.

Socioeconomic contracting programs in general, and the 8(a) program in particular, are widely suspected of being exploited by unscrupulous businesses to gain an advantage in competition for federal contracts. Repeated audits over the years have identified many instances of fraud and mismanagement. As an example, the firm receiving the most obligations in the dataset compiled here is MicroTechnologies, LLC, of Vienna, Virginia. The firm was an 8(a) participant and carried certifications as a minority-owned business, small disadvantaged business, and service-disabled veteran-owned small business throughout the period examined. Over the period, the firm received $1.19 billion in federal obligations, or an average of nearly $200 million per year. By law, firms may not receive more than $100 million in contracts over the course of their participation in the 8(a) program.

While these numbers seem to imply contracting practices well outside the intent of the 8(a) program, the SBA responds that the complicated nature of how the contracts were awarded, the duration of them, and the types of contracts involved make it difficult to make direct comparisons. It is also important to note that obligations do not necessarily equate to disbursed funds. Obligations may be later de-obligated if the funds are not required for execution of the contract. They do not capture work subcontracted to other firms. Data entry errors are also a consistent problem. However, obligations are the only

metric reported and must serve as a proxy for disbursements. Below I examine this assumption through the correlation between obligations and short-run firm performance. The relationship is indeed positive, suggesting obligations serve as a reasonable proxy, although the modeling below avoids using obligations as an explanatory variable of interest.

In 2013, the SBA moved to debar MicroTechnologies, LLC, from future federal contracts, saying it misrepresented its ownership and operational arrangements in order to receive its preferential contracts. As a condition of continuing to receive government contracts, the CEO was required to step aside. He was allowed to return to the firm in May 2014 after signing a code of ethics and completing contracts compliance training. As of mid-2016, the firm had left the 8(a) program, but continued to receive preferential federal contracts as a service-disabled veteran-owned small business, recording nearly $275 million in obligations in the year to April 2016. It claims to be a prime contractor on more than 100 federal projects.

Despite the large award values, the NETS recorded only $6.4 million in firm sales in 2007, rising to $14.1 million in 2013. Funds obligated to the firm may not be ultimately booked as revenue, as described above. Nevertheless, there is clearly a lot going on that requires further explanation. It requires a high level of sophistication to manage 100 federal projects and $275 million in annual obligations. That a firm can do so and continue to be classified as “small” and worthy of subsidy speaks to the nature of the regulatory environment. Given the level of scrutiny surrounding the firm after the scandal, it is unlikely that its operations continue to constitute fraudulent behavior;

however, it is illuminating to see the extent to which rules designed to help small businesses grow and compete can be turned to the advantage of firms that few impartial observers would consider small or disadvantaged.

Figure 22: Kernel Density Functions of Obligations and Sales by Firm

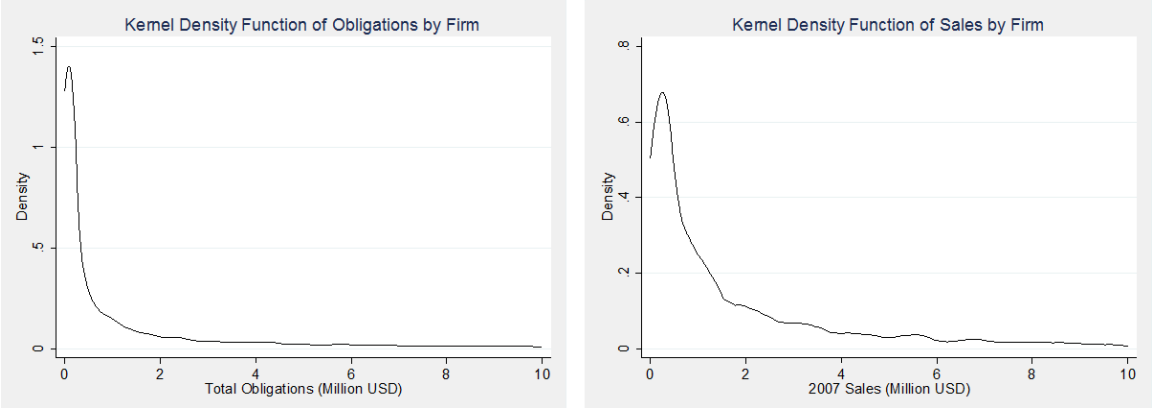

Figure 22: The median firm in the sample received $388,000 in obligations between 2007 and 2012 and had $598,000 in annual sales. The tails of the kernel density distributions continue to the maximum values of $1.2 billion in obligations (left panel) and $1.7 billion in sales (right panel), not shown here.

At the other end of the distribution are a large number of firms receiving small amounts of contract obligations. Figure 22 shows the kernel density functions of total obligations by firm (left panel) and annual sales by firm as of 2007 (right panel). The median firm received approximately $388,000 in obligations over the six-year period, while the heavily right-skewed distribution continues to $1.19 billion for MicroTechnologies, LLC (not shown in the figure). The median firm by 2007 sales took in $598,000, with the largest firm in the sample, Sprague Operating Resources, LLC, taking in $1.6 billion in 2007 (again, the figure is truncated and does not show this firm).

Sprague Operating Resources had 250 employees and received $301,000 in contracts as a woman-owned small business.

An important question is the viability of the many small firms in the sample. Are these going concerns, short-lived start-ups, or perhaps little more than shell companies used to take best advantage of the regulatory environment? Figure 23 illustrates descriptive statistics for all of the firms in the dataset segmented by Dun & Bradstreet credit rating. These credit ratings range from one (high) to four (limited). The top panel shows the number of firms in each credit rating category in each year of the sample. Approximately half of the firms did not receive a credit rating. Absence of a rating does not imply lack of viability, but only that Dun & Bradstreet did not have sufficient information to classify the company, whether for lack of historical data, a deficit net worth, or lack of sufficient payment information (all common conditions for young businesses). Of those that did receive ratings, only a small number received the lowest (4) and highest (1). Despite most of the firms in the sample not being rated, those that are rated received substantially larger contract obligations, as shown in the center panel of Figure 23. The “high” credit category received the highest average amount of obligations. The bottom panel of Figure 23 shows average employment levels by credit category. As expected, firms without a credit rating tended to have low levels of employment, with an average in 2007 of only 7.8. The largest firms by employment were those the highest credit ratings, although there is little correlation of employment with credit ratings for the lower three credit categories.

Figure 23: Firm Characteristics by Credit Rating and Year

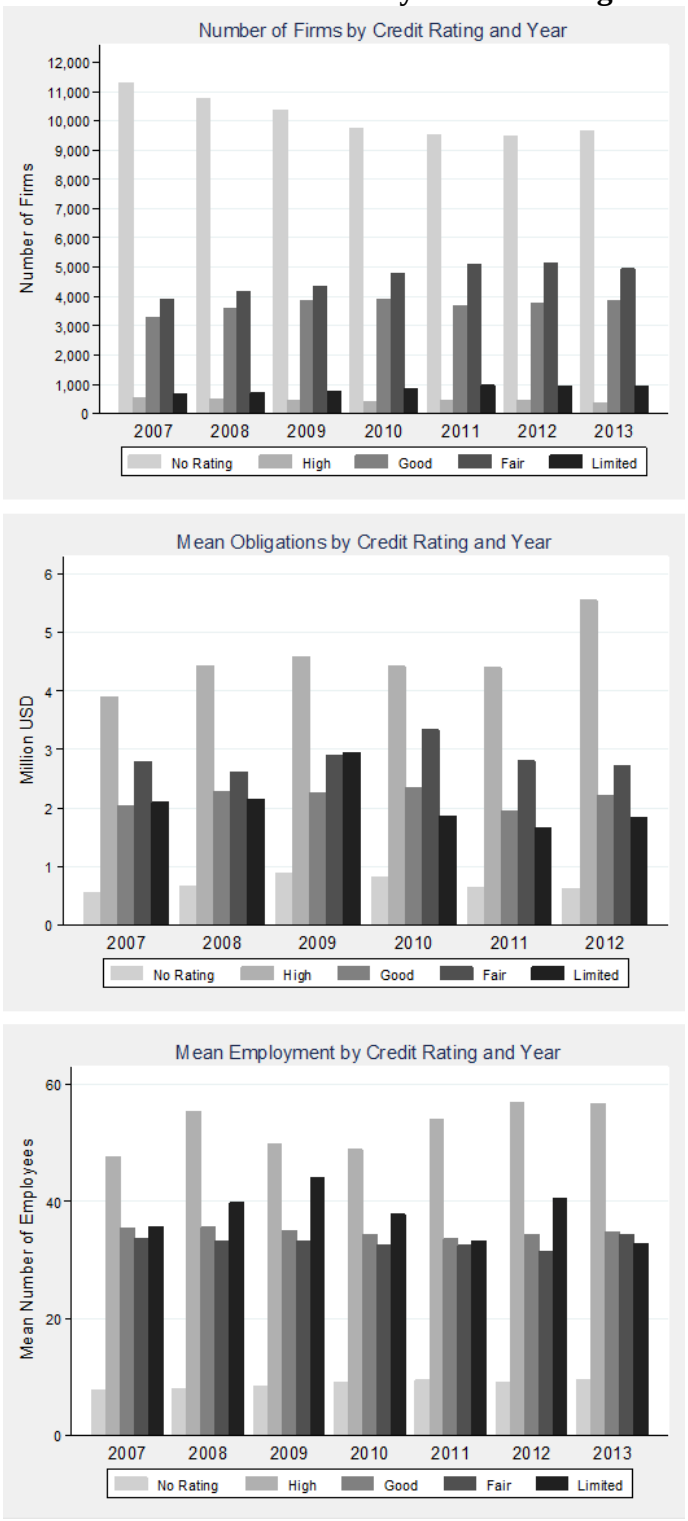

Figure 23: The top panel depicts the total number of firms in each Dun & Bradstreet credit rating category in each year of the sample. The center panel shows average contract obligations by credit rating, and the bottom panel shows mean employment levels.

I present summary statistics of variables captured in the FPDS and NETS datasets in Table 17. There are 2,851 firms in the dataset that received federal funding between fiscal years 2007 and 2012 and did not belong to any protected socioeconomic category. Each of these firms received an average of $4 million in obligations over the period (or about $670,000 per year). They began with an average of 22.6 employees, which rose to 25.7 employees by 2013. Annual sales fell from an average of $4.2 million to $3.8 million. They were 27.2 years old on average as of 2013. The firm receiving the most obligations over the six-year period ($587 million) was Petromax Refining Company of Houston, Texas. The entire obligation was awarded in 2009. While the firm has a minimal online presence and oil refining facilities in Houston marked with its brand name, Dun & Bradstreet records do not provide a credit rating and state the firm had only one employee and $861,000 in sales in that year. An objective of the matching procedures in the models to follow is to control for the effects of such questionable data points.

There are 7,942 firms in the sample that are minority-owned; the mean federal obligation amount over the six-year period for these firms was $15.8 million. They tend to be younger than non-minority firms, with a mean age of 17.2 years. Small disadvantaged businesses make up 7,521 firms in the sample with mean obligations of $17.5 million and approximately 20 employees. The sample contains 2,134 service-disabled veteran-owned businesses; these tend to be substantially younger, at 13.5 years, and received $18.2 million in awards on average. The 7,187 woman-owned businesses in the sample received an average of only $8.8 million in awards. The largest of these, at

Table 17: Summary Statistics by Socioeconomic Category

	Mean	Std. Dev.	Min	Max	Obs
	Non-Favored Firms				
Maximum Total Awards, 2007-12	$4,004,946	$22,400,000	$0	$587,000,000	2,851
Employees, 2007	22.6	42.0	0	500	
Employees, 2013	25.7	59.4	0	1,875	
Annual Sales, 2007	$4,156,857	$10,100,000	$0	$179,000,000	
Annual Sales, 2013	$3,821,637	$9,513,530	$0	$214,000,000	
Firm Age in 2013	27.2	20.7	1	213	
	Minority-Owned				
Maximum Total Awards, 2007-12	$15,800,000	$45,500,000	$1	$1,190,000,000	7,942
Employees, 2007	19.8	42.0	0	500	
Employees, 2013	24.2	58.0	0	1,800	
Annual Sales, 2007	$3,017,573	$9,540,950	$0	$327,000,000	
Annual Sales, 2013	$3,066,581	$9,094,464	$0	$410,000,000	
Firm Age in 2013	17.2	13.0	1	213	
	Small Disadvantaged Businesses (SDB)				
Maximum Total Awards, 2007-12	$17,500,000	$44,900,000	$1	$1,190,000,000	7,521
Employees, 2007	20.1	41.6	0	490	
Employees, 2013	24.5	55.3	0	1,800	
Annual Sales, 2007	$3,063,971	$10,600,000	$0	$646,000,000	
Annual Sales, 2013	$3,068,487	$7,348,313	$0	$148,000,000	
Firm Age in 2013	17.7	12.3	1	138	
	Service-Disabled, Veteran-Owned Small Businesses (SDVOB)				
Maximum Total Awards, 2007-12	$18,200,000	$53,400,000	$105	$1,190,000,000	2,134
Employees, 2007	13.2	32.2	0	440	
Employees, 2013	20.7	47.1	0	650	
Annual Sales, 2007	$2,111,161	$15,000,000	$0	$646,000,000	
Annual Sales, 2013	$2,765,548	$7,532,803	$0	$175,000,000	
Firm Age in 2013	13.5	11.0	1	120	
	Woman-Owned Small Businesses (WOSB)				
Maximum Total Awards, 2007-12	$8,835,454	$31,400,000	$1	$979,000,000	7,187
Employees, 2007	16.9	35.1	0	500	
Employees, 2013	20.1	51.4	0	1,800	
Annual Sales, 2007	$3,205,407	$25,000,000	0	$1,660,000,000	
Annual Sales, 2013	$3,022,778	$25,800,000	0	$1,870,000,000	
Firm Age in 2013	18.7	15.5	1	209	
	8(a) Participants				
Maximum Total Awards, 2007-12	$25,700,000	$51,900,000	$1	$1,190,000,000	4,013
Employees, 2007	20.0	38.9	0	490	
Employees, 2013	27.0	57.8	0	1,800	
Annual Sales, 2007	$2,718,743	$6,174,846	$0	$141,000,000	
Annual Sales, 2013	$3,420,923	$7,450,786	$0	$133,000,000	
Firm Age in 2013	16.1	8.5	1	132	

Table 17: Summary statistics for socioeconomic groups. Non-favored firms at the top of the table do not belong to any socioeconomic category.

$979 million in awards, was Kipper Tool Company of Gainesville, Georgia, which maintained credit ratings of 1 or 2 over the period examined. Despite receiving an average annual obligation of over $160 million, the firm recorded sales of approximately $18 million per year over the period. This example again demonstrates that obligations do not necessarily imply similar levels of sales. While obligations are used in this study to divide firms into socioeconomic categories and to generate matched quasi-experimental control groups, their magnitudes are not independent variables of interest in the models below. Differences in the sample of 8(a) firms are apparent in the summary statistics for these firms. There are 4,013 of them in the dataset and they have the highest level of average obligations by a wide margin, at $25.7 million.

Section 3.2: Empirical Approach

I use several distinct modeling techniques to assess the degree to which 8(a) program characteristics encourage growth in participating firms. In all cases, the unit of analysis is the individual firm. The first and most basic model uses ordinary least squares (OLS) to estimate the impact of program participation on firm-level employment and sales growth between 2007 and 2013. I model the relationship as

Equation 6: Socioeconomic Ordinary Least Squares Model

$$y_i = \alpha + \beta T_i + \gamma X + \varepsilon_i$$

where y_i is either growth in employment or growth in annual sales for firm *i* between 2007 and 2013. Growth is calculated according to the formula (value2013 – value2007)/value2007. The set of treatment dummies, T_i, indicate participation in each of the various socioeconomic programs, including 8(a), with β a vector of the coefficients of interest. Participants in the Small Business Innovation Research Program, which supports

small entrepreneurs, are also indicated with a dummy variable. The vector X contains other independent variables as controls, including obligations received, total employment and sales at the beginning of the period, firm age, and the number of other firms of the same 8-digit SIC and city (i.e., core-based statistical area). The latter variable controls for agglomeration effects of localized ecosystems of firms that implicitly or explicitly share information, expertise, and perhaps employees. One specification also includes as a control variable firms' mean credit score over the period. Inclusion of this variable unfortunately eliminates many relevant firms from the analysis: those without credit scores. Error is captured in ε_i and is assumed orthogonal. Robust standard errors are clustered by 6-digit NAICS code.

Following OLS, I examine performance of 8(a) participants using a difference-in-difference specification directly comparing levels of employment and sales of 8(a) participants and non-participants at the beginning and end of the six-year period. 8(a) treatment firms are first matched to control firms from a select alternative socioeconomic program according to their total level of federal obligations between 2007 and 2012. This results in treatment groups of 8(a) firms and equally sized control groups of minority-owned, small disadvantaged, service-disabled veteran-owned, or woman-owned small businesses that received comparable levels of federal funding. The model is specified as

Equation 7: Socioeconomic Difference-in-Difference Model

$$y_{i\tau} = \alpha + \beta t_i + \gamma a_\tau + \delta t_i a_\tau + \varepsilon_{i\tau}$$

where $y_{i\tau}$ is the level of sales or employment for firm *i* in period τ, t_i is a dummy variable indicating treatment status of firm *i*, a_τ is a dummy variable indicating either 2007 or 2013 values of the dependent variable, and $\varepsilon_{i\tau}$ is an error term that is assumed

orthogonal. The coefficient of interest is δ, which is the difference-in-difference estimator

Equation 8: Socioeconomic Difference-in-Difference Estimator

$$\hat{\delta} = \left(\bar{y}_{(treat)(after)} - \bar{y}_{(treat)(before)}\right) - \left(\bar{y}_{(control)(after)} - \bar{y}_{(control)(before)}\right)$$

Robust standard errors are again clustered by 6-digit NAICS code.

A final set of tests uses propensity score matching to generate control groups from among the group of non-8(a) firms belonging to the socioeconomic category of comparative interest. This method allows matching along a wide array of firm characteristics beyond simply total federal contract obligations (as applied in the difference-in-difference model above). A probit model of the form

Equation 9: Socioeconomic Probit Estimation of Propensity Scores

$$t_i = \alpha + \beta X_i + \varepsilon_i$$

estimates the probability that a given firm would be an 8(a) participant (t_i) given the matrix X_i of independent variables, including logarithms of the number of firms in the same city/industry, employment in 2007, sales in 2007, firm age in 2013 and total federal obligations to the firm over the period. A model including mean credit score is also included here for comparison, although this disqualifies a large portion of the relevant population. The error term ε_i again captures unexplained variation. Estimated coefficients β are used to estimate a probability of treatment for each firm, and treated firms are matched to control firms by this value.

After 8(a) program participants are matched to untreated firms (i.e., non-participating contractors from the select socioeconomic category) by propensity score, I estimate the average treatment effect on the treated (ATT) by the formula

Equation 10: Socioeconomic Average Treatment Effect on the Treated

$$E[Y_{1i} - Y_{0i} \mid D_i = 1] = E[Y_{1i} \mid D_i = 1] - E[Y_{0i} \mid D_i = 1]$$

where $E[Y_{1i} \mid D_i = 1]$ is the value of the treatment group outcome (change in employment or annual sales) for firm i given treatment and $E[Y_{0i} \mid D_i = 1]$ is the counterfactual control group outcome assuming treatment. Matched controls supply counterfactual outcome estimates. Robust standard errors in the models employ the methodology of Abadie and Imbens (2012), in which error from the first stage probit matching procedure is accounted for in final estimation of the ATT.

The OLS specification examines all of the socioeconomic contracting programs in a single model, while the subsequent difference-in-difference and propensity score matching models compare 8(a) firms to each of the other socioeconomic categories in turn. To assess the overall effect of 8(a) Business Development Program participation, I estimate the effect of treatment on firm performance compared to that of minority-owned small businesses (which do not benefit from any contracting preferences beyond the overall small business contracting goal that affects all firms in the sample) and woman-owned small businesses (which also did not receive special benefits as of the time of the study). I similarly assess 8(a) program participants by comparing them with non-8(a) small disadvantaged businesses, which benefit only from the federal spending goal of five percent. This assesses the proportionate impact of the remaining benefits available to 8(a) participants (but not non-participating small disadvantaged businesses), including sole-source authority, management assistance, and mentor relationships. Finally, I compare 8(a) firms to service-disabled veteran-owned small businesses, which benefit from a spending goal and sole-source contract authority. This comparison assesses the

relative impact of management assistance, mentorship, business plan monitoring, and other such non-monetary benefits enjoyed by 8(a) firms.

Small business growth distributions are strongly affected by outliers; a small group of exceptional performers have an outsized effect on the group's average growth rate. To examine the impact of these outliers, each model is also executed after Winsorizing the sample growth rates. In combination, these models provide multifaceted and robust evidence of relative performance differences between programs with different characteristics. The following section enumerates these differences in more detail.

Section 4: Results

Prior to estimating the models outlined in Section 3, I examine relative differences between the means of relevant variables in the socioeconomic groups of interest. Table 18 displays these differences for the four comparisons outlined above: 8(a) firms relative to minority-owned, small disadvantaged, service-disabled veteran-owned, and woman-owned firms. The table shows the mean change in employment and sales between 2007 and 2013 for each group and the difference in these changes between 8(a) firms and the applicable comparison category. I test the statistical significance of the difference in means with a standard t-test. In order to carefully explore the effects of distribution on the results, I separately calculate mean growth for the unaltered population in addition to Winsorizations at the $1^{st}/99^{th}$, $5^{th}/95^{th}$, and $10^{th}/90^{th}$ percentiles. I similarly compare differences in group means for the control variables of total contract obligations and firm age.

Table 18: Differences in Means, Full Sample

		Employment Change (Mean)				Sales Change (Mean)				Other	
		--	W1	W5	W10	--	W1	W5	W10	Total	Firm Age
	Obs.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
8(a)	1,968	1.58	1.14	0.509	0.238	2.32	1.65	0.719	0.328	23,700,000	16.0
		(0.15)	(0.077)	(0.027)	(0.015)	(0.28)	(0.12)	(0.040)	(0.021)	(1,022,943)	(0.20)
Minority	1,543	0.466	0.406	0.288	0.226	0.797	0.590	0.342	0.218	3,169,960	19.5
		(0.062)	(0.040)	(0.021)	(0.015)	(0.15)	(0.071)	(0.030)	(0.020)	(793,785)	(0.50)
Difference:		**1.11**	**0.738**	**0.221**	**0.012**	**1.52**	**1.06**	**0.378**	**0.111**	**-20,500,000**	**-3.55**
		(0.16)	(0.068)	(0.035)	(0.021)	(0.31)	(0.14)	(0.050)	(0.029)	(1,294,800)	(0.54)
Pr(T > t)		**1.00**	**1.00**	**1.00**	**0.72**	**1.00**	**1.00**	**1.00**	**1.00**	**1.00**	**0.00**
8(a)	1,968	1.58	1.14	0.509	0.238	2.32	1.65	0.719	0.328	23,700,000	16.0
		(0.15)	(0.077)	(0.027)	(0.015)	(0.28)	(0.12)	(0.040)	(0.021)	(1,022,943)	(0.20)
SDB	1,646	0.775	0.449	0.236	0.151	1.10	0.546	0.255	0.130	8,217,794	21.4
		(0.17)	(0.052)	(0.022)	(0.015)	(0.30)	(0.076)	(0.030)	(0.019)	(950,118)	(0.40)
Difference:		**0.804**	**0.695**	**0.273**	**0.087**	**1.21**	**1.10**	**0.464**	**0.198**	**16,600,000**	**-5.43**
		(0.23)	(0.093)	(0.035)	(0.021)	(0.28)	(0.14)	(0.050)	(0.029)	(1,396,115)	(0.45)
Pr(T > t)		**1.00**	**1.00**	**1.00**	**1.00**	**1.00**	**1.00**	**1.00**	**1.00**	**1.00**	**0.00**
8(a)	1,968	1.58	1.14	0.509	0.238	2.32	1.65	0.719	0.328	23,700,000	16.0
		(0.15)	(0.077)	(0.027)	(0.015)	(0.28)	(0.12)	(0.040)	(0.021)	(1,022,943)	(0.20)
SDVOSB	986	1.55	1.34	0.709	0.459	3.88	1.87	0.917	0.549	14,000,000	13.4
		(0.16)	(0.10)	(0.035)	(0.020)	(1.22)	(0.17)	(0.052)	(0.028)	(1,508,437)	(0.38)
Difference:		**0.029**	**-0.193**	**-0.199**	**-0.221**	**-1.56**	**-0.215**	**-0.198**	**-0.221**	**9,705,973**	**2.61**
		(0.21)	(0.13)	(0.045)	(0.025)	(1.25)	(0.20)	(0.065)	(0.035)	(1,822,579)	(0.43)
Pr(T > t)		**0.55**	**0.067**	**0.00**	**0.00**	**0.11**	**0.14**	**0.00**	**0.00**	**1.00**	**1.00**
8(a)	1,968	1.58	1.14	0.509	0.238	2.32	1.65	0.719	0.328	23,700,000	16.0
		(0.15)	(0.077)	(0.027)	(0.015)	(0.28)	(0.12)	(0.040)	(0.021)	(1,022,943)	(0.20)
WOSB	3,673	0.489	0.395	0.261	0.197	0.694	0.574	0.326	0.201	3,623,766	20.1
		(0.060)	(0.028)	(0.013)	(0.009)	(0.081)	(0.046)	(0.019)	(0.013)	(395,425)	(0.30)
Difference:		**1.09**	**0.748**	**0.248**	**0.042**	**1.62**	**1.08**	**0.393**	**0.127**	**20,000,000**	**4.09**
		(0.16)	(0.081)	(0.031)	(0.018)	(0.29)	(0.12)	(0.044)	(0.025)	(1,096,710)	(0.36)
Pr(T > t)		**1.00**	**1.00**	**1.00**	**0.99**	**1.00**	**1.00**	**1.00**	**1.00**	**1.00**	**0.00**

Table 18: t-tests of differences in means. Variable means for each program in the left column are tested against mean values for the 8(a) Business Development Program in the row above. Each socioeconomic category is limited to firms participating in that category alone, with the exception of categories that are subsets by definition (see Table 15). 8(a) firms may be minority owned due to the substantial overlap.

The results in Table 18 show strong relative performance of 8(a) firms relative to all of the control groups with the exception of service-disabled veteran-owned small businesses, with basic differences in means showing 8(a) growth rates two to three times higher than comparison groups. The effects remain significant, although at dramatically lower magnitudes, for Winsorized samples. Service-disabled veteran-owned businesses are a notable exception to the trend. Despite receiving less obligations on average, these businesses perform similarly to 8(a) firms in the un-Winsorized sample and significantly better after Winsorization. Examination of control variables, however, suggests that the treatment and control groups are not similar. Firms participating in the 8(a) program received far more contract obligations, on average, than did any other socioeconomic category of small business (Table 18, Column 9).

To alleviate the discrepancy in relative funding amounts, I next match 8(a) firms with control firms in each socioeconomic comparison group by the amount of total obligations received. Results of tests using these matched samples are displayed in Table 19, with Column 9 showing the equivalent mean funding levels of the comparison groups. Through matching, outlying obligation values that remain in the dataset are balanced by comparable obligations at other firms, equalizing the likely degree of discrepant values across the compared groups.

By only retaining firms in the sample that have a matching firm from the comparison group, I generate samples of approximately equal size: 594 pairs of 8(a) firms with minority-owned firms, 816 pairs with small disadvantaged businesses, 735 pairs with service-disabled veteran-owned businesses, and 997 pairs with woman-owned

Table 19: Differences in Means, Samples Matched by Total Funding

		Employment Change (Mean)				Sales Change (Mean)				Other	
		--	W1	W5	W10	--	W1	W5	W10	Total Funding	Firm Age
	Obs.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
8(a)	594	0.869	0.649	0.296	0.146	1.19	0.941	0.428	0.183	6,851,802	16.4
		(0.19)	(0.11)	(0.043)	(0.027)	(0.26)	(0.16)	(0.062)	(0.035)	(1,283,864)	(0.43)
Minority	594	0.387	0.343	0.258	0.180	0.854	0.661	0.402	0.225	7,945,962	18.8
		(0.087)	(0.056)	(0.035)	(0.024)	(0.21)	(0.12)	(0.054)	(0.033)	(2,047,692)	(0.62)
Difference:		**0.483**	**0.306**	**0.039**	**-0.034**	**0.341**	**0.280**	**0.026**	**-0.041**	**-1,094,160**	**-2.36**
		(0.21)	(0.12)	(0.056)	(0.036)	(0.33)	(0.20)	(0.082)	(0.049)	(2,416,888)	(0.75)
Pr(T > t)		**0.99**	**0.99**	**0.76**	**0.17**	**0.85**	**0.92**	**0.62**	**0.20**	**0.33**	**0.00**
8(a)	816	1.13	0.851	0.362	0.159	1.42	1.24	0.512	0.214	12,900,000	16.4
		(0.18)	(0.11)	(0.040)	(0.023)	(0.31)	(0.16)	(0.057)	(0.031)	(1,285,699)	(0.35)
SDB	816	1.19	0.567	0.253	0.147	1.70	0.687	0.278	0.123	13,000,000	21.8
		(0.34)	(0.089)	(0.033)	(0.021)	(0.59)	(0.13)	(0.045)	(0.028)	(1,314,089)	(0.55)
Difference:		**-0.061**	**0.285**	**0.109**	**0.012**	**-0.285**	**0.549**	**0.235**	**0.091**	**-110,714**	**-5.40**
		(0.38)	(0.14)	(0.052)	(0.031)	(0.63)	(0.20)	(0.072)	(0.042)	(1,838,437)	(0.66)
Pr(T > t)		**0.44**	**0.98**	**0.98**	**0.65**	**0.32**	**0.99**	**1.00**	**0.98**	**0.48**	**0.00**
8(a)	735	1.13	0.931	0.398	0.188	1.38	1.26	0.559	0.259	15,500,000	15.7
		(0.17)	(0.12)	(0.042)	(0.025)	(0.20)	(0.16)	(0.060)	(0.033)	(1,380,832)	(0.37)
SDVOSB	735	1.78	1.54	0.769	0.472	4.60	2.14	1.03	0.590	15,570,000	13.4
		(0.20)	(0.13)	(0.043)	(0.024)	(1.62)	(0.20)	(0.064)	(0.033)	(1,429,745)	(0.42)
Difference:		**-0.652**	**-0.607**	**-0.371**	**-0.284**	**-3.22**	**-0.883**	**-0.470**	**-0.331**	**-147,808**	**2.36**
		(0.26)	(0.18)	(0.060)	(0.034)	(1.64)	(0.26)	(0.087)	(0.047)	(1,987,679)	(0.56)
Pr(T > t)		**0.01**	**0.00**	**0.00**	**0.00**	**0.025**	**0.00**	**0.00**	**0.00**	**0.47**	**1.00**
8(a)	997	1.10	0.819	0.362	0.178	1.76	1.15	0.491	0.221	11,100,000	16.2
		(0.17)	(0.094)	(0.035)	(0.021)	(0.42)	(0.14)	(0.049)	(0.028)	(1,007,883)	(0.31)
WOSB	998	0.677	0.543	0.285	0.168	0.979	0.886	0.394	0.198	11,100,000	21.7
		(0.11)	(0.071)	(0.029)	(0.018)	(0.15)	(0.12)	(0.043)	(0.026)	(1,034,696)	(0.52)
Difference:		**0.424**	**0.276**	**0.077**	**0.010**	**0.781**	**0.261**	**0.097**	**0.023**	**80,048**	**-5.43**
		(0.20)	(0.12)	(0.045)	(0.027)	(0.45)	(0.19)	(0.066)	(0.038)	(1,444,446)	(0.61)
Pr(T > t)		**0.98**	**0.99**	**0.96**	**0.64**	**0.96**	**0.92**	**0.93**	**0.73**	**0.48**	**0.00**

Table 19: t-tests of differences in means. Variable means for each program in the left column are tested against mean values for the 8(a) Business Development Program in the row above. Each socioeconomic category is limited to participants in that category alone, with the exception of categories that are subsets by definition (see Table 15). Firms in each comparison program are matched to 8(a)-participating firms by total contract funding (Column 11) to create treatment and control groups of equal size.

small businesses. Differences in means in the un-Winsorized samples continue to show significant outperformance of 8(a) firms relative to minority-owned and woman-owned firms; however, these effects diminish and disappear at higher levels of Winsorization, suggesting the effect is driven by outliers rather than the broad population of program participants. There is no difference between the unmodified samples of 8(a) and small disadvantaged businesses, although 8(a) firms appear to outperform after Winsorization; this finding suggests that outliers predominantly affect the mean performance of firms in the small disadvantaged group and that the group of 8(a) firms experiences more broad-based growth. Service-disabled veteran-owned small businesses outperform across all levels of Winsorization as well as in the unaltered sample, although the size of the effect diminishes as outlying values are adjusted through Winsorization. Results of these simple tests foreshadow the findings of more robust models to come.

Section 4.1: Ordinary Least Squares

Presented in Table 20, ordinary least squares is used to model the effects of program participation on employment and sales growth. The set of firms used in this analysis includes overlapping categories, as depicted in Figure 18, Figure 19, and Figure 20. Program participation is indicated by dummy variables. Remarkable trends emerge across the various socioeconomic programs. In the un-Winsorized sample there are no significant effects for minority-, disadvantaged-, veteran-, woman-, or service-disabled veteran-owned small businesses. Employment changes become more significantly negative in the Winsorized sample for all but service-disabled veterans, suggesting that these groups generally perform poorly, but that outliers have large effects on the means.

Table 20: Ordinary Least Squares with Socioeconomic Categories

Dependent Variable:	Empl. Chg.	Empl. Chg. (Winsorized)	Empl. Chg. (Win., Credit)	Sales Chg.	Sales Chg. (Winsorized)	Sales Chg. (Win., Credit)
	(1)	(2)	(3)	(4)	(5)	(6)
8(a) Participant	**0.793*** **	**0.219*** **	**0.173*** **	**1.54*** **	**0.377*** **	**0.345*** **
	(0.223)	**(0.0339)**	**(0.0349)**	**(0.567)**	**(0.0459)**	**(0.0505)**
Minority	-0.0171	-0.0333**	-0.0223	-0.274	-0.0279	-0.0241
	(0.0944)	(0.0148)	(0.0224)	(0.236)	(0.0253)	(0.0349)
Small Disadvantaged Business	0.117	-0.0299*	-0.0704***	-0.256	-0.0381	-0.0713*
	(0.102)	(0.0168)	(0.024)	(0.264)	(0.0257)	(0.038)
Veteran-Owned Business	0.155	-0.0659***	-0.0306	0.424	-0.0433	-0.012
	(0.239)	(0.0199)	(0.0271)	(0.756)	(0.0273)	(0.0362)
Woman-Owned Small Business	0.0261	-0.043***	-0.0238	0.0789	-0.0103	0.00505
	(0.149)	(0.015)	(0.0178)	(0.313)	(0.0246)	(0.0306)
SDVOSB	0.334	0.186***	0.175***	0.673	0.292***	0.29***
	(0.26)	(0.0232)	(0.0358)	(1.04)	(0.0456)	(0.0734)
SBIR/STTR	-0.327***	-0.00289	-0.0972***	-0.828**	-0.0274	-0.125*
	(0.0963)	(0.0251)	(0.0346)	(0.375)	(0.0454)	(0.0686)
Obligations (log)	0.152***	0.047***	0.0414***	0.359***	0.0625***	0.0585***
	(0.0176)	(0.00282)	(0.00252)	(0.0945)	(0.00396)	(0.00404)
Firm Count by CBSA and 8-digit SIC (log)	0.246***	0.0438***	0.052***	0.487**	0.0528***	0.0536***
	(0.0714)	(0.0103)	(0.00959)	(0.228)	(0.0169)	(0.0163)
Employment, 2007 (log)	-0.872***	-0.168***	-0.257***	-0.211	-0.0888***	-0.128***
	(0.114)	(0.0211)	(0.028)	(0.677)	(0.023)	(0.0344)
Sales, 2007 (log)	0.114***	-0.0311***	-0.000636	-0.146	-0.0536***	-0.0584***
	(0.0253)	(0.00567)	(0.00747)	(0.197)	(0.00647)	(0.011)
Firm Age (log)	-0.0533	-0.0154	-0.17***	0.371	-0.0262	-0.272***
	(0.076)	(0.0142)	(0.02)	(0.36)	(0.019)	(0.0295)
Mean Credit Rating			-0.0571***			-0.0955***
			(0.0165)			(0.0249)
Constant	-0.92***	0.446***	1.06***	-2.41**	0.425***	1.78***
	(0.269)	(0.0382)	0.173***	(0.967)	(0.0588)	(0.133)
Observations	19,753	19,753	12,176	19,753	19,753	12,176
R-squared	0.0332	(0.180)	(0.187)	0.0049	(0.115)	(0.136)

Notes: *** Significant at the 1 percent level
** Significant at the 5 percent level
* Significant at the 10 percent level
(Robust standard errors clustered by NAICS)

Table 20: Dependent variables are the change in employment or change in sales between 2007 and 2013, measured as (value2013 – value2007) / value2007. The independent variables *8(a) Participant* through *Small Business Innovation Research/Small Business Technology Transfer (SBIR/STTR)* are indicator variables coded to 1 for participants in the given program. Alternative specifications in Columns (2) and (5) are Winsorized at 5% and 95% while those in (4) and (6) include the additional mean credit rating control variable.

As in the basic comparisons of means above, service-disabled veteran-owned businesses grow at above-average rates in the Winsorized sample.

The table also presents a model that includes each firm's mean credit score over the period as an additional control variable. This specification presents problems, since firms without credit scores (which make up a substantial portion of the population of interest) must be dropped from the sample. Nevertheless, the results are similar to those in the Winsorized sample.

Participants in the 8(a) program perform better than any other category of firm, with employment growth 79.3 percentage points higher than baseline firms and sales growth 154 percentage points higher (measured not annually, but from the beginning to the end of the period). The effect is dramatically smaller (21.9 and 37.7 percentage points, respectively) in the Winsorized samples, again suggesting the effect is driven largely by outliers.

The OLS models control for differential levels of contract obligations and industry clustering, and examination of the coefficients on these control variables also provides interesting insight. Despite the problems associated with using obligations as a proxy for disbursed funds, the variable is strongly correlated with increased growth across all specifications. Positive agglomeration effects are visible in the coefficient of the clustering variable. Firms with more peers in the same industry within their local area see significantly stronger growth in employment and sales relative to those which are geographically isolated.

Section 4.2: Individual Program Comparisons – Difference-in-Difference

I next turn to examining pairwise relationships between firms participating in the 8(a) program and the comparison groups. These comparison groups here exclude firms belonging to overlapping categories, such as 8(a) firms owned by women or service-disabled veterans. Having constructed treatment and control groups of 8(a) and non-8(a) firms that received comparable levels of contract awards (the same groups as in Table 19), I construct a difference-in-difference model comparing levels of employment and sales before and after treatment. Results are presented in Table 21 and Table 22. In most specifications, 8(a) firms matched with similar peers fail to show any substantial growth premium. There are weak positive effects for sales growth relative to small disadvantaged businesses, an effect also visible in the basic differences in means calculated in Table 19. Once again, service-disabled veteran-owned small businesses perform dramatically better than 8(a) firms, with a growth premium of four to five employees and approximately $800,000 in sales over the six-year period.

Section 4.3: Individual Program Comparisons – Propensity Score Matching

A final test of the robustness of these findings comes from estimation of the ATT using treatment and control groups constructed through propensity score matching. The probit model in Table 23 generates propensity scores used to match treated firms with control firms for estimation of the ATT in Table 24. These models again exclude from consideration firms from overlapping socioeconomic categories. Results are presented for the complete and Winsorized samples, as well as for a model which includes mean credit rating as a matching variable in the probit model; inclusion of this variable again limits the sample to firms with credit scores.

Table 21: Difference-in-Difference Estimates, Matched by Total Funding (1)

Treatment Group:	8(a) Business Development Program Participants				8(a) Business Development Program Participants			
Control Group:	Minority-owned Businesses				Small Disadvantaged Businesses			
Dependent Variable:	Empl.	Empl. (W5)	Sales	Sales (W5)	Empl.	Empl. (W5)	Sales	Sales (W5)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment * Period	**-1.57**	**-1.12**	**413,000**	**1920**	**1.54**	**1.01**	**837,000***	**542,000*****
	(2.8)	**(1.25)**	**(665,000)**	**(211,000)**	**(1.96)**	**(1.05)**	**(444,000)**	**(150,000)**
Treatment	-7.47***	-3.29**	-2,490,000***	-677,000***	-7.41***	-3.49**	-1,640,000***	-896,000***
(Treat = 1, Control = 0)	(2.43)	(1.34)	(897,000)	(242,000)	(2.53)	(1.37)	(447,000)	(201,000)
Period	2.24	1.24**	-588,000	-232,000*	0.424	0.424	-880,000**	-591,000***
(After = 1, Before = 0)	(1.46)	(0.626)	(628,000)	(122,000)	(1)	(0.577)	(357,000)	(110,000)
Obligations	1.74e-7***	8.88e-8***	0.0513***	0.0176***	2.75e-7***	1.35e-7***	0.0508***	0.0231***
	(4.19e-8)	(1.07e-8)	(0.0128)	(0.00324)	(8.54e-8)	(2.54e-8)	(0.0133)	(0.0036)
Firm Age	1.09***	0.622***	220,000***	92,800***	0.914***	0.563***	132,000***	79,900***
	(0.204)	(0.0781)	(54,700)	(11,400)	(0.135)	(0.0603)	(28,200)	(8,270)
Constant	3.4	5.89***	453,000	794,000***	6.12**	7.43***	1,090,000*	1,270,000***
	(3.28)	(1.65)	(1,020,000)	(259,000)	(2.82)	(1.36)	(618,000)	(235,000)
Observations	2,376	2,376	2,376	2,376	3,264	3,264	3,264	3,264
R-squared	0.125	0.147	0.0655	0.175	0.126	0.149	0.132	0.166

Notes: *** Significant at the 1 percent level
** Significant at the 5 percent level
* Significant at the 10 percent level
(Robust standard errors clustered by NAICS)

Table 21: The models are of the functional form $y_{i\tau} = \alpha + \beta t_i + \gamma a_\tau + \delta t_i a_\tau + \theta X_{i\tau} + \varepsilon_{i\tau}$, where $y_{i\tau}$ is the level of sales or employment for firm *i* in period τ, t_i is a dummy variable indicating treatment status of firm i, a_τ is a dummy variable indicating either 2007 or 2013 values of the dependent variable, and $\varepsilon_{i\tau}$ is an error term that is assumed orthogonal. $X_{i\tau}$ is a matrix of control variables including competitive and non-competitive obligations and firm age. The variable of interest is δ, which is the coefficient on the interaction term between t_i and a_τ. The values in the bold-text row of the table above represent estimated values of the difference-in-difference estimator: $\hat{\delta} = \left(\bar{y}_{(treat)(after)} - \bar{y}_{(treat)(before)}\right) - \left(\bar{y}_{(control)(after)} - \bar{y}_{(control)(before)}\right)$. For columns annotated (W5) , data were Winsorized at 5% and 95% prior to estimation. Treatment and control firms were matched by total contract funding.

Table 22: Difference-in-Difference Estimates, Matched by Total Funding (2)

Treatment Group:	8(a) Business Development Program Participants				8(a) Business Development Program Participants			
Control Group:	Service-disabled, Veteran-owned Small Businesses				Woman-Owned Small Businesses			
Dependent Variable:	Empl.	Empl. (W5)	Sales	Sales (W5)	Empl.	Empl. (W5)	Sales	Sales (W5)
	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Treatment * Period	**-4.97*****	**-4.08*****	**-811,000****	**-716,000*****	**0.0501**	**0.898**	**451,000**	**275,000**
	(1.4)	**(1.06)**	**(333,000)**	**(203,000)**	**(1.78)**	**(0.93)**	**(350,000)**	**(187,000)**
Treatment	2.89	3.13***	133,000	347,000**	-4.2*	-3.55***	-2,190,000**	-736,000***
(Treat = 1, Control = 0)	(1.85)	(1.11)	(289,000)	(174,000)	(2.3)	(1.16)	(925,000)	(183,000)
Period	7.44***	6.14***	1,100,000***	803,000***	1.99	1.01*	-456,000	-281,000**
(After = 1, Before = 0)	(1.18)	(0.889)	(326,000)	(166,000)	(1.39)	(0.585)	(324,000)	(133,000)
Obligations	1.46e-7***	1.10e-7***	0.0347***	0.0211***	2.78e-7***	1.69e-7***	0.0488***	0.0284***
	(3.87e-8)	(2.40e-8)	(0.00716)	(0.00405)	(5.32e-8)	(2.61e-8)	(0.0117)	(0.00459)
Firm Age	0.553***	0.424***	82,700***	62,200***	0.69***	0.529***	152,000***	83,700***
	(0.12)	(0.0722)	(16,700)	(11,400)	(0.0828)	(0.0504)	(28,400)	(7,440)
Constant	2.43*	2.96***	92,400	252,000	6.39***	6.88***	1,320,000*	910,000***
	(1.37)	(1.05)	(261,000)	(166,000)	(1.94)	(1.26)	(692,000)	(230,000)
Observations	2,940	2,940	2,940	2,940	3,990	3,990	3,990	3,990
R-squared	0.0650	0.0996	0.0871	0.125	0.0993	0.159	0.0183	0.192

Notes: *** Significant at the 1 percent level
** Significant at the 5 percent level
* Significant at the 10 percent level
(Robust standard errors clustered by NAICS)

Table 22: The models are of the functional form $y_{i\tau} = \alpha + \beta t_i + \gamma a_\tau + \delta t_i a_\tau + \theta X_{i\tau} + \varepsilon_{i\tau}$, where $y_{i\tau}$ is the level of sales or employment for firm i in period τ, t_i is a dummy variable indicating treatment status of firm i, a_τ is a dummy variable indicating either 2007 or 2013 values of the dependent variable, and $\varepsilon_{i\tau}$ is an error term that is assumed orthogonal. $X_{i\tau}$ is a matrix of control variables including competitive and non-competitive obligations and firm age. The variable of interest is δ, which is the coefficient on the interaction term between t_i and a_τ. The values in the bold-text row of the table above represent estimated values of the difference-in-difference estimator: $\hat{\delta} = \left(\bar{y}_{(treat)(after)} - \bar{y}_{(treat)(before)}\right) - \left(\bar{y}_{(control)(after)} - \bar{y}_{(control)(before)}\right)$. For columns annotated (W5) , data were Winsorized at 5% and 95% prior to estimation. Treatment and control firms were matched by total contract funding.

Table 23: Probit Regression for Estimation of Propensity Scores

Comparison Group:	Minority	SDB	SDVOSB	WOSB
	(1)	(2)	(5)	
Firm Count by CBSA	0.0658***	0.0608***	0.00771	0.0382**
and 8-digit SIC (log)	(0.0216)	(0.0187)	(0.0178)	(0.016)
Employment, 2007 (log)	-0.127***	-0.139***	0.0442	-0.0557**
	(0.0292)	(0.0269)	(0.0314)	(0.0238)
Sales, 2007 (log)	0.0667***	0.0637***	0.0389***	0.0481***
	(0.00858)	(0.00853)	(0.00857)	(0.00708)
Firm Age (log)	-0.202***	-0.433***	0.161***	-0.35***
	(0.0538)	(0.0505)	(0.0607)	(0.0418)
Obligations (log)	0.32***	0.236***	0.109***	0.299***
	(0.0132)	(0.0111)	(0.0102)	(0.0109)
Constant	-4.24***	-2.58***	-2.11***	-4.01***
	(0.201)	(0.177)	(0.186)	(0.156)
Observations	3,511	3,614	2,954	5,641
Pseudo R-squared	0.327	0.200	0.106	0.282

Notes: *** Significant at the 1 percent level
** Significant at the 5 percent level
* Significant at the 10 percent level
(Robust standard errors clustered by NAICS)

Table 23: Probit models for estimating propensity scores in Table 24. The dependent variable is coded to zero or one for non-participants/participants in the 8(a) program. All non-8(a) participants in each sample belong to the socioeconomic category indicated. Models with the credit score control variable are not included here.

Table 24: PS Matching Estimates of Average Treatment Effect on the Treated

8(a) vs:		Coef.	AI Robust Std. Error	z	P > | z |	Obs.
Minority	Empl.	**1.13**	0.166	6.80	0.000	3,511
	Empl. (W5)	**0.168**	0.0677	2.48	0.013	3,511
	Empl. (W5, Cred)	**0.201**	0.105	1.91	0.057	2,172
	Sales	**1.72**	0.308	5.60	0.000	3,511
	Sales (W5)	**0.334**	0.101	3.31	0.001	3,511
	Sales (W5, Cred)	**0.331**	0.167	1.98	0.048	2,172
SDB	Empl.	**0.151**	0.444	0.34	0.734	3,614
	Empl. (W5)	**0.184**	0.0530	3.48	0.001	3,614
	Empl. (W5, Cred)	**0.161**	0.0699	2.3	0.021	2,519
	Sales	**0.204**	0.847	0.24	0.810	3,614
	Sales (W5)	**0.378**	0.0761	4.97	0.000	3,614
	Sales (W5, Cred)	**0.386**	0.0959	4.03	0.000	2,519
SDVOSB	Empl.	**-0.202**	0.237	-0.85	0.393	2,954
	Empl. (W5)	**-0.158**	0.0575	-2.75	0.006	2,954
	Empl. (W5, Cred)	**-0.202**	0.0708	-2.85	0.004	1,993
	Sales	**-2.71**	1.20	-2.26	0.024	2,954
	Sales (W5)	**-0.305**	0.0897	-3.40	0.001	2,954
	Sales (W5, Cred)	**-0.242**	0.113	-2.13	0.033	1,993
WOSB	Empl.	**0.266**	0.419	0.63	0.526	5,641
	Empl. (W5)	**0.143**	0.050	2.84	0.004	5,641
	Empl. (W5, Cred)	**0.099**	0.0823	1.20	0.230	3,334
	Sales	**0.395**	0.488	0.81	0.419	5,641
	Sales (W5)	**0.165**	0.0824	2.00	0.046	5,641
	Sales (W5, Cred)	**0.176**	0.117	1.51	0.131	3,334

Table 24: Estimates of the average treatment effect on the treated (ATT) using propensity score matching models. The treatment group is composed of 8(a) participants and the control group is composed of members of the program annotated in the left-most column. Reported values are differential growth in employment and annual sales computed as (value2013 – value2007) / value2007. Estimates use Abadie and Imbens (2012) robust standard errors. W5 indicates dependent variable Winsorization at the 5% level, and "Cred." indicates samples in which mean credit scores were included as variables in the first-stage probit model.

Propensity score matching ATT models again show stronger performance of 8(a) firms relative to peers in other programs, although the findings are more nuanced. Relative to minority-owned firms, 8(a) participants outperform at statistically significant levels for both employment and sales in Winsorized and un-Winsorized samples. However, for the Winsorized samples the magnitude of the employment growth premium drops nearly seven-fold and the sales growth premium five-fold. Findings are similar in

the credit rating model. This consistent finding suggests the top firms in the sample exert substantial influence. Relative to woman-owned small businesses, the magnitude of the performance difference is approximately half the size in the Winsorized sample relative to the un-Winsorized sample; notably, it also increases in statistical significance, suggesting substantial noise is eliminated from the sample by attenuating the outliers. In the credit rating model, relative performance of 8(a) firms remains positive, but loses statistical significance with elimination of the smallest (non-rated) firms from the sample.

The comparison with small disadvantaged businesses shows results even stronger than those in Table 19. While there is no statistically significant difference in mean growth rates in the un-Winsorized sample, after Winsorization the magnitude of the 8(a) growth premium increases and becomes highly significant. The effect remains in the credit rating model. This supports the finding above from the comparison of means in the matched sample: outliers have a strong effect on non-participating small disadvantaged businesses as well as their 8(a) counterparts.

Findings for comparison of 8(a) firms with service-disabled veteran-owned small businesses are again confirmed in this model. 8(a) firms perform substantially worse in all but one of the specifications (which has the expected sign but fails to achieve statistical significance). Magnitudes of the difference are again reduced in the Winsorized and credit rating models, but remain significantly negative.

Section 5: Discussion

Results are broadly consistent across a wide range of model specifications, alternative treatment and control groups, and varying levels of sample Winsorization. The

basic findings are these: 8(a) firms appear to perform very strongly at first look relative to firms which enjoy no comparable benefits (minority- and woman-owned firms). However, the performance gap is mostly driven by a small number of firms. Upon Winsorization of the samples, performance differences are dramatically reduced, or in many cases eliminated. Careful consideration of magnitude differences is informative: in Table 24 we see that 8(a) firms outperform minority-owned businesses in employment growth over six years by 113 percentage points in the basic sample and 16.8 percent after Winsorization; these numbers equate to annual growth premiums of 13.4 percent and 2.6 percent, respectively. Small disadvantaged businesses, on the other hand, enjoy the same five percent contracting goal as do 8(a) firms, but not sole-source authority or the range of management assistance and mentorship benefits. 8(a) firms continue to outperform, only with effects that are stronger after Winsorization of the sample. This suggests the effect of outliers is stronger for small disadvantaged businesses.

In most of these cases, 8(a) firms show consistent, if not consistently strong, levels of growth that exceed those of comparable firms in other categories. This effect reverses when they are compared with service-disabled veteran-owned firms. The effects of the additional benefits enjoyed by 8(a) firms and not this contrasting peer group, namely management assistance and mentorship, are either negative or are overcome by characteristics of service-disabled veterans that are not accounted for in this analysis.

The expressed purpose of the 8(a) Business Development Program is to “improve the conditions of socially and economically disadvantaged groups” by deliberately steering federal contracts to their firms. This support ostensibly encourages firm-level

growth and competition among government suppliers which "promotes economy in such procurements." These goals do not follow necessarily from one another, but must be examined in isolation: (1) Does the 8(a) program improve the conditions of disadvantaged groups? (2) Does it encourage growth in the small businesses they own? (3) Does this encouragement then foster competition among the broader population of government suppliers? Finally, (4) does this increase in competition, in the end, save the government money? This analysis addressed the second and third of these questions. (Note, however, that if the third goal is not met then the fourth is also unlikely.)

It examined them in light of competing theories. Under the first hypothesis, historical discrimination has left talent underutilized in the population of potential government suppliers. By deliberately counteracting this discrimination, the government can help minorities and other disadvantaged groups overcome biases and grow to their full potential. Under the second hypothesis, diminished competition produces rent-seeking and competition along non-productive lines. In comparing these hypotheses, I employ the following assumption: underlying levels of ability across various socioeconomic groups are essentially equal. The implications of this assumption in light of the study's findings are as follows, as summarized in Table 16.

Firms that benefit from the wide array of advantages available through the 8(a) program (i.e., socioeconomic contracting goals, sole-source contracts, management assistance, and mentor-protégé relationships) outperform minority- and woman-owned small businesses, which do not so benefit. However, this average level of outperformance is not robust to alternative specifications. It grows substantially weaker and even reverses

on occasion with high levels of sample Winsorization (Table 19), and disappears in difference-in-difference estimates with firms matched by levels of contract obligations (Table 21 and Table 22). It grows substantially weaker in quasi-experimental estimation of the ATT (Table 24). These findings imply that, while the full array of benefits may indeed help firms to grow, they do not do so uniformly. They accomplish the government's goal of stimulating growth, but at the expense of the goal of encouraging competition. Small numbers of firms see strong growth while most remain stagnant, which inhibits competition by encouraging concentration.

Next, I compare 8(a) firms to non-participating small disadvantaged businesses. 8(a) firms must be certified as SDBs prior to entering the program, and both groups benefit from a five percent federal spending target and contract set-asides. The relative regulatory advantage provided to 8(a) firms consists of sole-source contracts, management assistance, and mentor relationships. This comparison provides the strongest evidence in support of the 8(a) program. Performance of the two groups is generally equal in comparisons of matched samples (Table 19, Columns 1 and 5; Table 21, Columns 5 and 7; Table 24), but groups of 8(a) firms perform better with Winsorization of the samples – at increasing levels of statistical significance, and even coefficient magnitude in the ATT model (Table 24). This suggests that the group of 8(a) firms achieves more broad-based growth, while non-participating small disadvantaged businesses see a stronger effect of outliers. However, the finding is not as positive as it may seem: the intent of the 8(a) program is to increase performance relative to non-disadvantaged firms, not relative to firms drawn from the same peer group. It may also be

indicative of selection effects since small disadvantaged businesses are, by definition, eligible for the 8(a) program. The results must be interpreted with caution.

A final comparison with service-disabled veteran-owned businesses produces evidence that is more critical. Across virtually all model specifications, 8(a) firms perform substantially worse than this peer group. Service-disabled veteran-owned businesses are the most comparable control group in terms of regulatory benefits, with similar contracting goals and sole-source authority. The only differences lie in the management and mentorship assistance available to participants, which appear to be a hindrance rather than a help. This negative effect grows weaker as the sample is Winsorized, suggesting that the service-disabled veteran-owned small business control group sees a stronger effect of outliers on growth. Implications of this finding for competition among 8(a) participants is largely irrelevant since their growth prospects are diminished.

Section 6: Conclusion

To summarize, the 8(a) program drives industry concentration among participants relative to firms that do not receive any of the wide array of benefits they enjoy. It accomplishes its goals of fostering growth and encouraging competition only relative to non-participating small disadvantaged businesses. Participants fare worse than firms which receive the same set of benefits with the exception of management assistance and mentorship. Thus the program seems to fail along both of the key parameters examined here. While participants indeed do better relative to non-participants from similar socioeconomic categories (i.e., other small disadvantaged businesses), they do not

perform better relative to more disparate socioeconomic groups, which is the intent of the program. On top of this, participants perform substantially worse than peers most closely matched in terms of available benefits (i.e., service-disabled veteran-owned small businesses).

Of the competing theories of program effects, the rent-seeking hypothesis receives greater support. Providing more benefits to disadvantaged groups via the 8(a) program does not produce the inter-group growth premium predicted by the "money-on-the-table" model. Broad-based growth is in fact inhibited relative to other socioeconomic groups receiving comparable benefits while what supernormal growth is achieved through the widest array of relative benefits accrues to a select few firms. The findings here support a growing body of anecdotal evidence that the program does not achieve its stated objectives.

These findings have clear policy implications. Above all, they point to adverse effects of a complex regulatory environment. Relative to the most lightly regulated groups examined (minority- and woman-owned small businesses), the sample of 8(a) firms grows substantially more concentrated over the period examined. This suggests that industry concentration is encouraged by the multitude of rules that allow some firms to achieve monopolistic or oligopolistic positions by using the regulatory environment to their advantage. While preferential contracts in the form of set-asides and sole-source awards indeed encourage growth, the growth is counterproductive in light of the program goals since it is captured by so few firms.

Where the relative advantage of set-asides and sole-source awards is not present, as in comparison with service-disabled veteran-owned businesses, the multitude of rules surrounding the program has a sharply adverse effect on growth. This implies the system set up to provide management assistance, mentorship, and other such benefits is not functioning as designed and re-examination of the regulatory structure is necessary. As previous authors have found, program design is crucial in determining whether a given affirmative action program helps or hinders subsidy recipients. Policy makers may be able to improve the functioning of the 8(a) program by simplifying the regulatory environment and re-examining the system of scrutiny and support provided by regional SBA administrators.

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CHAPTER 3: EFFECTS OF SMALL BUSINESS THRESHOLDS ON FIRM-LEVEL STRATEGIC BEHAVIOR

The United States government sets a goal of 23 percent of the value of its goods and services contracts awarded to small businesses. To further this objective, it uses mechanisms such as contract set-asides to shield small businesses from competition with large contractors. This analysis examines strategic behavior of firms near the threshold of eligibility for small business contracting preferences. Firms approaching the size threshold face a discontinuous decrease in competitiveness with continued growth. Contrary to common assumptions, they display no substantial propensity to limit employment growth as they approach the threshold. Rather, they mitigate the discontinuous loss of competitiveness in other ways such as mergers, acquisitions, and reporting under alternative industry categories which carry different size restrictions.

(*JEL* H32, H57, L25, L53)

Keywords: Entrepreneurship, Firm Sales, Firm Size, National Subsidies, Policy, Public Economics, Public Expenditure

Section 1: Introduction

Promoting the interests of small businesses in the United States has long been important to congressional policymakers, although justification for the policies has

changed since they took shape in the years following World War II. Reasons for favoring small businesses have ranged from fairness, to civil rights, to enhancing the dynamism of the national economy (Anglund 2000, Kotlowski 1998). Since the beginning, however, legislators have faced the challenge of supporting favored categories of firms while avoiding the unintended consequences that accompany such regulations. The most basic problem is determination of what constitutes a small business. While we seem to know it when we see it, codification of this intuition is remarkably difficult (Moore, Grammich and Mele 2014b). Size thresholds cause behavioral changes that shape the market on which they operate, re-arranging relationships of relative advantage and disadvantage such that simple employee counts or revenue totals no longer serve the purpose. Firms compete along regulatory lines rather than those measured by traditional economic indicators. It seems clear that rent seeking is a necessary consequence of competitive advantage created through regulatory fiat. Nevertheless, few empirical analyses have succeeded in quantifying the effects of the largest preferential contracting program in the world: that overseen by the U.S. Small Business Administration.

It is not surprising that researchers have generally avoided large-scale empirical analysis of SBA-administered programs. While anecdotal evidence of strategic behavior (let alone mismanagement and fraud) is common, the nature of the environment makes it challenging to quantitatively identify systemic market distortions. Complex business relationships arise in the presence of complex regulatory structures, and every new effort to close loopholes results in new strategies for government contracting incumbents to maintain favored status. What would seem to be clear regulatory discontinuities, such as

size thresholds, are obscured by the consequent set of reactions on the part of those affected. As audit after audit has been highly critical of federal small business programs, researchers have seldom succeeded in identifying systemic effects.

The paucity of evidence and prevalence of subjective analysis arise from the nature of the problem at hand. It would seem clear, for example, that a size threshold above which firms are ineligible for preferential treatment would lead to strategic behavior. However, when large firms are able to win contracts ostensibly set aside for small businesses, or when small businesses are awarded contracts and pass on the bulk of the work through opaque subcontracting arrangements, the apparent discontinuity in treatment will not necessarily result in a visible discontinuity of outcomes. Attempts to quantify the impact of regulatory structures thus flounder on a byzantine regulatory environment in which winners, whatever their size, thrive by understanding the complexities of the system and the means of using the rules to their advantage.

This paper fills the gap by quantifying the effects of several strategic behaviors commonly employed to circumvent federal small business regulations. In particular, it examines activities of firms near the size threshold for preference eligibility. Participants in and observers of the federal contracting system note many ways that firms may avoid a sudden loss of competitiveness caused by losing eligibility for small business contract preferences as a firm grows through the threshold. Four of these will be examined here. First, and most commonly discussed, is deliberately restricting hiring to remain below the threshold. While this behavior may be employed on a limited basis, theory suggests it is unlikely on a large scale and its effects are not visible across a broad sample of firms.

Three other methods of circumventing regulatory discontinuities have effects that are more apparent in the data. Firms approaching the eligibility threshold may exit the market, either merging with larger firms or in some other way re-incorporating such that benefits are maintained. In this way, they avoid the difficult situation faced by firms barely above the small business size threshold: too large to receive subsidies and too small to possess competitive economies of scale. Third, firms may avoid the situation of bare ineligibility by growing discontinuously. By purchasing other businesses or business units they can rapidly achieve the knowledge base and economies of scale required to compete successfully. Finally, firms can change the nature or description of the goods or services they provide in order to remain eligible. Different market dynamics across industries cause regulatory size thresholds to be set at different levels for different classifications of goods and services, meaning that reclassification of a business establishment under another industry code can result in continued eligibility.

An important caveat to this analysis is that firms facing a dramatic decrease in competitiveness as they cross a size threshold are relatively rare. Of the nearly 10,000 firms examined here, no more than a few hundred are in a position to feel the direct regulatory effects of marginal changes in size in any given year. However, there remain many ways that firms far from the regulatory discontinuity can manipulate the system to their advantage. As examples, firms awarded multi-year contracts as small businesses were until recently not required to re-certify in later years despite employment growth. Small and large businesses often exist in symbiotic relationships which skirt the boundaries of legal affiliation, allowing firms with only a few employees to win contracts

far larger than their capabilities by subcontracting to large firms which are able to execute the work. Another common problem is simple dishonesty on the part of sellers and mismanagement on the part of buyers; audits of small business contracting practices continue to uncover widespread fraud and inaccuracies in contract records (see the numerous reports published by the Government Accountability Office and SBA Audit Program). Apart from alluding to such problems through obvious cases, the analysis to follow does not directly address them. Instead, it examines the narrower subset of firms which are directly affected by the regulatory discontinuity.

It does so through a variety of means, each suited to the strategic behavior in question. Comprehensive models are unable to capture the wide range of strategic behaviors that result from small business regulations. This is a necessary consequence of the environment. Rules put in place to create favorable conditions for select categories of market participants are imposed on a dynamic environment in which firms adjust their behavior to maintain competitiveness in the face of every obstacle, whether imposed by traditional market forces or the dictates of a monopsonistic buyer. The non-linearity of the system defies parsimonious exposition. Effects can, however, be quantified. In accomplishing this goal, the most significant innovation employed here is construction of the dataset. I link government spending data from the Federal Procurement Data System with firm-level credit history data compiled by Dun & Bradstreet and reported in the National Establishment Time Series. I use the resulting set of input and outcome variables to identify firms which received preferential contracts and were near the threshold of preference eligibility.

Once these firms are identified in such a manner, regulatory effects on firm behavior are readily apparent in the data. Firms show no strong propensity to adjust employment levels to remain below regulatory thresholds. They are, however, substantially more likely to participate in mergers or acquisitions and to change the nature or description of the products they sell to maintain eligibility. I employ simple econometric models to quantify the size and significance of each of these effects. Ordinary least squares, regression discontinuity, and fixed effects specifications each serve, in their place, to shed light on different aspects of these behaviors.

The analysis begins in Section 2 with a review of small business contracting regulations, past research on their effects, and theoretical implications of various characteristics of the regulatory environment. Section 3 describes the construction of the dataset and its features that allow exploration of strategic behavior, including employment levels over time, affiliation relationships, and industry category reporting. Section 4 presents the analysis quantifying levels of strategic behavior and Section 5 discusses the practical significance of the findings. Section 6 concludes.

Section 2: Background and Theory

Although various programs to encourage small businesses emerged during the Great Depression and World War II, the regulatory regime as it exists today began with the Small Business Act of 1953, which established the Small Business Administration (SBA) and was later codified in the Federal Acquisition Regulation (FAR) Part 19. The SBA was designed as a source of loans, capital, and management and procurement assistance for small business owners in the United States. Anglund (2000) and Moore,

Grammich, and Mele (2014a) describe the history of the program in the following years. What began as a program to help entrepreneurs overcome the perceived predations of large businesses and the bias of government procurement policies changed in the civil rights era of the 1960's and 1970's to champion minorities and other disadvantaged groups (Kotlowski 1998). Not until the Reagan era did the justification for such policies turn to encouraging small business as the great engine of U.S. job creation and economic dynamism. The government applies a wide variety of tools in the effort to advance these goals (Leiter and Leiter 2002, McVay 2009).

All of these traditions persist to the present day despite research that has challenged core assumptions of each. Evidence that businesses are helped by contracts awarded based on the racial characteristics of their owners is mixed, with findings in support of such practices (Bates 2015; Chatterji, Chay and Fairlie 2014) caveated and even contradicted by others (Bates and Williams 1996, Myers and Chan 1996, Sweet 2006). Support for the claim that small business drives the U.S. economy is similarly tenuous. Researchers have largely settled on an understanding that the perception of small business as a key driver of job creation is based on failure to account for correlation between firm age and size as well as confusion between gross and net rates of employment growth (Davis, Haltiwanger and Schuh 1996; Decker, et al. 2014). Hurst, et al. (2011), find that the entire notion of small business as dynamic and entrepreneurial is based on outlying examples and not the great majority of firms providing an existing service to an existing market with primary goals that are non-pecuniary in nature (e.g., autonomy, flexibility).

Section 2.1: Program Execution and Monitoring

As the justification for small business contracting preferences grew threadbare, it also became apparent that execution of the programs leaves as much to be desired as the theory behind them (Bean 2001). Cullen (2012) documents a number of these problems, including large businesses qualifying for small business awards. She points to an unconfirmed report by the American Small Business League claiming that nearly two-thirds of federal procurement targeted to small businesses in fact went to large, established contractors. Another watchdog reported that $47 billion in small business contracts between 1998 and 2003 went to companies earning over $100 million in Department of Defense contracts. Anecdotes abound of individual cases suggesting program mismanagement and possibly fraud.

Many of these cases arise from loopholes in the regulatory code, such as a provision that allowed small businesses to continue receiving preferential awards under a multi-year contract vehicle after surpassing size limitations or being acquired by a larger concern. Despite fixes such as the "recertification rule" (Seabrooks 2005), which required more frequent updates to eligibility status, the problem continued. The SBA's own *Report on the Most Serious Management and Performance Challenges Facing the Small Business Administration in Fiscal Year 2015* (Gustafson) points out that $400 million in 2013 small business contract actions may have been awarded to ineligible companies. The report states, "While some contractors may misrepresent or erroneously calculate their size, most of the incorrect reporting results from errors made by Government contracting personnel, including misapplication of small business contracting rules." In response to this audit report, the SBA noted substantial progress in addressing the

findings after changing a rule that formerly allowed companies to be awarded task orders under incorrect size standards (a change distinct from the earlier recertification rule noted above) and declaring that "The SBA should ensure that procuring agencies accurately report contracts awarded to small businesses when representing their progress in meeting small business contracting goals, and that contracting personnel are reviewing on-line certifications prior to awarding contracts."

Inaccuracy of data used to track achievement of small business goals and monitor performance of contracting officers is a significant problem both in evaluating system performance and in reforming it. Another internal audit from 2010 found that the SBA had certified the accuracy of its contracting data in Fiscal Year 2008 although 92 percent of the actions in the audited sample contained inaccurate or incomplete elements. After implementation of an improvement plan, a sample taken the following year contained an even higher error rate of 97 percent (Ritt 2010). Data quality is a significant problem in assessment of small business programs, and must be considered throughout the analysis to follow. While aggregation and cleaning can go some way toward mitigating inaccuracies, substantial variation remains that must be assumed orthogonal to noted effects.

Section 2.2: Program Effects

Apart from documenting problems in execution of the regulatory framework, some research points to inefficiencies created by efforts to modify economic outcomes through non-market intervention. Most quantitative assessments focus on the cost to the government of awarding contracts based on attributes other than cost and performance.

Several studies find substantial deleterious effects (Marion 2007; Athey, Coey and Levin 2013) while others find them to be small or non-existent (Denes 1997, Krasnokutskaya and Seim 2011, Nakabayashi 2013, Reis and Cabral 2015). A common theme in the literature is that higher costs associated with small business contract incentives are offset by the increased competition they encourage. However, the small state and local programs evaluated in these studies lack the scale and regulatory complexity of the federal small business contracting environment and therefore avoid broader issues of strategic behavior by participants. They often identify microeconomic outcomes without regard to broader inefficiencies in the market as a whole.

The line of research more directly addressing large-scale strategic behavior instigated by small business regulation is not well developed. Sakallaris (2007) points out that a likely effect of subsidizing small businesses is to keep them small by punishing growth. This line of reasoning, however, confuses individual human motivation with incentives at the organizational level. Were a business an immutable entity unto itself, it would indeed have an incentive to remain small in the face of such regulations. A business owner, however, does not face the same incentives as would the organizational entity as a whole. An owner may sell his or her business, purchase competitors or change the firm's core activities. Limiting the size of the concern would limit the long-run earnings potential of the owner. In practice there are many ways to continue building individual earnings while sacrificing the independence or very nature of the associated business. Thus the theoretical foundation suggesting firms deliberately limit growth in response to size thresholds is weak.

Reeder and Vergilio (1984) outline another means of maintaining competitiveness in their analysis of contract law as it applies to corporate affiliation. As they put it, "When a dispute arises as to the size of a firm for purposes of small business set-aside eligibility, the central inquiry often is whether any third party corporation or person shares control of the firm, and if so, whether the aggregate size of all affiliated firms exceeds the small business size standard established by the SBA." In other words, businesses can circumvent size standards by remaining small on paper while leveraging the capabilities of other corporate entities to fulfill the task in question. While such activities can take forms that contradict the spirit or letter of the law, they can also be entirely legal and even encouraged, as in the mentor-protégé relationships brokered by the 8(a) Business Development Program.

As discussed above and examined in more depth in the analysis to follow, firms face a sharp loss of competitiveness as they grow beyond the size threshold and lose the benefits that allow them to compete with larger firms possessing stronger experience, interpersonal networks, and economies of scale. A clear means of sidestepping this problem is to avoid the middle ground. Upon reaching the size threshold, a business owner can sell out or can purchase other going concerns in order to create a growth discontinuity commensurate with the regulatory discontinuity. Such options open the possibility for continued personal income growth not afforded by deliberately restricting the firm's size.

In a separate context, Bennear (2005) examines such strategic behavior in the face of regulatory discontinuity. The Massachusetts Toxics Use Reduction Act requires firms

above a given size emitting toxic materials into the environment at greater than certain thresholds publicly to disclose their activities. Bennear finds that as much as 40 percent of the observed decline in reported releases of toxic substances following passage of the law was due to strategic responses on the part of businesses rather than an actual reduction in the behavior. Manufacturing facilities deliberately manipulated their operations to ensure they remained below the reporting threshold for as many chemicals as possible. This behavior requires that two conditions be met: (1) management must be able to manipulate the relevant metrics and (2) the facility must be close enough to the threshold for such manipulation to matter.

These conditions apply to the case examined here. Small business regulatory thresholds vary from 100 employees to 1,500 employees depending on the industry in question (revenue thresholds are also used for some industries, but these are not examined in depth here for reasons discussed below) and there is substantial flexibility to reporting requirements. For example, while storage battery manufacturers (threshold: 500 employees) could not feasibly reclassify their activities as aircraft manufacturers (threshold: 1,500 employees), they may be able to report as primary battery manufacturers (threshold: 1,000 employees) or simply make sufficient modifications to their product to qualify. As with manufacturers in Massachusetts adjusting their operations at the margin to avoid scrutiny in releasing toxic materials, government contractors can manipulate relevant metrics within a certain range to avoid regulatory thresholds. As with mergers and acquisitions discussed above, such an outcome is more likely than choosing to permanently limit earnings potential through an employment cap.

Quantifying the prevalence of these behaviors necessarily captures only a small slice of the larger picture. Effects notable in the data are for the most part limited to legal practices. Anecdotal evidence, however, suggests that a large amount of activity violates either the letter or the spirit of the law. For example, a small business and a large business which are legally unaffiliated may in fact work closely together to take advantage of limited-competition contracts. Such a relationship will not be apparent in the data. Effects noted below thus represent a lower bound to strategic behavior that is undoubtedly far more widespread.

Section 3: Data Compilation and Description

Of the range of strategic behaviors discussed, I examine four here: hiring restrictions, mergers, acquisitions, and alternate industry reporting. Each of these behaviors is shaped by the core size thresholds delineating eligibility for small business preferences. The US government set a goal for itself that 23 percent of contract dollars will be awarded to small businesses each year. While the goal is not binding (and was not achieved prior to 2013), there is substantial administrative pressure across federal agencies to meet it. Each agency negotiates with the SBA every two years to establish an agency-wide goal while the SBA ensures that all agencies combined meet the overall target. Agency goals for 2015 varied from six percent small business utilization at the Department of Energy to 53 percent at the Departments of Agriculture and the Interior. The goal was 69 percent for contracts administered directly by the SBA. At the working level, interagency committees coordinate small business contracting activities and

departmental leaders face pressure from superiors and congressional appropriators to conform.

To help them meet these goals, the FAR Subpart 19.5 allows contracting officers to set aside contracts for competition among only small businesses. By law, every contract above the simplified acquisition threshold of $3,500 and less than $150,000 (as of 2016) is automatically set aside for competition only among small businesses unless the contracting officer determines that it will be impossible to obtain offers from at least two reputable businesses at a fair market price. Although it is not the default position, contracts above $150,000 meeting these criteria should also be set aside (the practical difference between these alternative regimes is unclear in FAR language). In addition, any firm supplying a product to the government that it did not manufacture must purchase the product from a small business manufacturer in the absence of a waiver from the SBA. Large businesses must, in submitting their bids, include a subcontracting plan that delineates how much of the work will be accomplished by small businesses. The SBA may upon request review acquisitions at a given contracting office to ensure a reasonable effort was made to support small businesses. If a contracting officer rejects the SBA's recommendation to award a particular contract to a small business, an appeal process is in place which terminates with the secretary of the respective department. By law the appeal process can take as much as two months, during which time the contracting officer must suspend the acquisition.

Section 3.1: Dataset Compilation

The Federal Procurement Data System (FPDS) is the primary database of record for US government contracts and for the SBA's monitoring and analysis of agency performance in meeting small business contracting goals. The database records extensive information for each contract action (although at questionable levels of accuracy). Data fields include the amount obligated to a particular contractor through a given contract vehicle (note that obligations serve as a proxy for actual disbursement of funds), the status of the contractor as a large or small business, the level of competition associated with the award process, and the North American Industry Classification System (NAICS) code reported by the contractor.

While the government does not collect firm-level details such as employment levels or annual revenue in FPDS, it has since passage of the Federal Funding Accountability and Transparency Act of 2006 required each federal contractor to obtain and report a Dun & Bradstreet Data Universal Numbering System (DUNS) number for tracking purposes. I use these DUNS numbers to match FPDS contractors with credit history data compiled by Dun & Bradstreet and reported in the National Establishment Time Series (NETS). This pairing completes the picture of each contractor in the dataset by providing annual employment, sales, geographic location, credit rating, NAICS code, and affiliations with other business establishments (i.e., stand-alone, headquarters, or subsidiary).

Firms are classified as small or large businesses based on the size limitation associated with their reported industry. The SBA sets these thresholds and updates them periodically. Compiling the dataset used in this analysis began with reviewing annual size

standards each year beginning in 2007, the first year in which it is possible comprehensively to match firms in FPDS to NETS data by DUNS number. To simplify the analysis, I limit the investigation to industries with small business size thresholds remaining constant between 2007 and 2012. Since revenue thresholds are regularly updated for inflation, the only industries included in the study have size standards based on employment. Size thresholds are set at 100, 500, 750, 1,000, and 1,500 employees depending on industry.

All firms recorded in FPDS under these industries between 2007 and 2012 were then compared to records in the NETS to assess their proximity to the applicable small business threshold. Each establishment with a distinct DUNS number in FPDS was aggregated with all affiliated NETS establishments and maintained in the dataset if the firm as a whole was within approximately 50 employees of the nearest small business threshold during the period of study and going back through 2004. Also included in the dataset are any firms that were affiliated with such small businesses through merger and acquisition activity at any point in the time period examined. The resulting list contains 10,733 headquarters firms comprising 48,022 total establishments. While most of these firms (6,006) are standalone establishments, many incorporate a number of distinct facilities, with the largest firm in the dataset by this measure comprising 1,563 separate establishments. (This firm, an auto parts retailer, had $1.7 billion in revenue but made only $4,000 in sales to government customers in 2007.) Such a large firm may be included in the dataset for reasons of rule manipulation or reporting inaccuracies described above, or because it merged with or acquired another small establishment at

some point during the period examined and thus met the criteria for inclusion. Figure 24 illustrates the distribution of parent firms by the number of establishments composing them as of the start of the observed period in 2007.

Figure 24: Establishments per Parent Firm, 2007

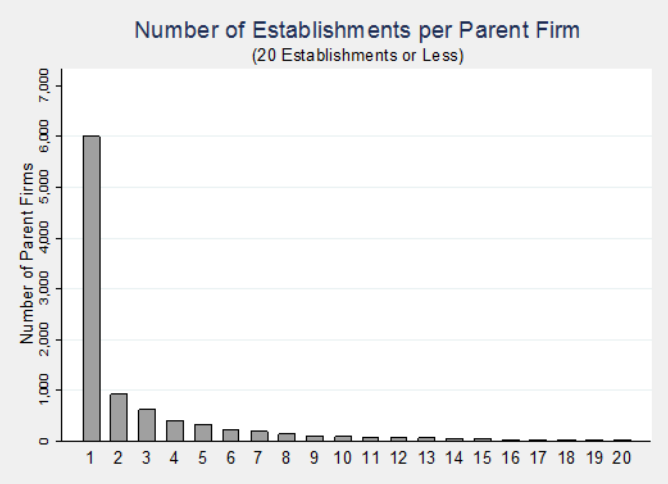

Figure 24: Number of parent firms with a given number of establishments for the sample of 9,854 firms (incorporating 47,145 total establishments) as of 2007. The figure shows firms with 20 or fewer establishments; there are 287 firms with more than this, with the largest reporting 1,563 establishments.

Section 3.2: Data Validation and Description

An important concern in examining federal procurement data is the validity of the business establishments recorded in FPDS. As described above, participants in this market are incentivized to arrange complicated relationships of firm affiliation (or non-affiliation) in order to maintain eligibility for small business set-asides. One means of accomplishing this is setting up what amount to little more than shell companies which then pass a significant portion of contract work on to larger businesses capable of executing it. I examine credit scores to provide a more complete picture of the

reputability of establishments in the dataset. Dun & Bradstreet reports creditworthiness on a scale of one to four, with one being the best rating. In the dataset there are 4,933 headquarters-level firms (comprising 21,738 establishments) which have a yearly and establishment-level average credit score of three or better (or ten or more employees for those reporting only an employment range and not a credit score). The number of single-establishment firms with good credit scores is 2,364, approximately 40 percent of the good-credit sample. As expected, firms with two or more establishments are more likely to have good credit scores.

While credit scores are informative, they are not useful for cleaning the data. Firms without Dun & Bradstreet credit scores are common, and lack of a score does not indicate that a firm is not viable. A firm may be unrated due to inadequate historical information, a deficit net worth, or missing payment information, all of which are characteristic of young, small firms. Removing these observations from the dataset would be counterproductive.

Total obligations to sampled firms grew from approximately $5 billion at the beginning of the period in 2007 to $7.5 billion by 2012. Firms with good credit scores captured $3.6 billion (71 percent) of this funding in 2007, rising to $6 billion (80 percent) in 2012 (the causal feature in this relationship is likely firm size rather than credit score). Figure 25 illustrates these spending trends among sampled firms. Obligations going to small businesses (as reported by contracting officers in FPDS) rose from $2.7 billion to $3.2 billion, but fell as a percentage of total spending from 53 percent to 42 percent. Of the aggregate spending on small businesses in the dataset, approximately 45 percent of

dollars were awarded under conditions of limited competition, including sole-source contracts and small business set-asides. This percentage remained roughly constant over the period.

Figure 25: Federal Obligations by Category and Firm Size

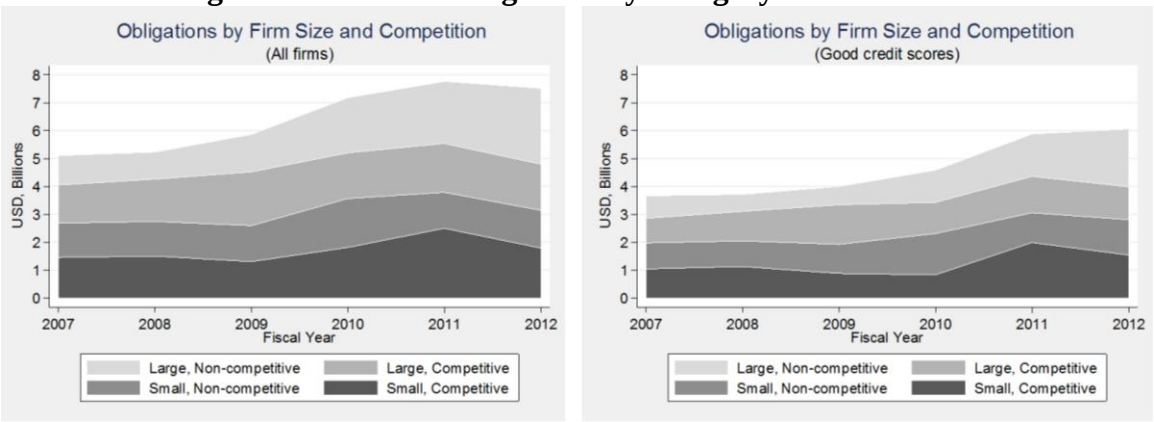

Figure 25: Aggregate federal obligations to relevant firms by year. The figure on the left includes all firms in the dataset while the figure on the right includes only those with Dun and Bradstreet credit scores of three or better.

Remarkably, the percentage of non-competitive funds obligated to large businesses was comparable to that awarded to small businesses at the beginning of the period, at 44 percent, but grew to 62 percent by 2012. This trend may seem to suggest that maintaining certification as a small business carries few benefits. The reality, however, is more complex. The types of contracts executed by large businesses are different in character, making direct comparisons difficult. For example, an aircraft manufacturer may execute a contract worth many billions of dollars that spans years or even decades. Such work cannot feasibly be re-competed during the life of the system and results in innumerable supporting contracts with only one possible bidder. This

results in large amounts of spending that may show up in FPDS as non-competitive despite a single large-scale competition at the beginning of the program. These types of contracts are not of interest here since a small business would be unable to execute such a large requirement. In fact, they increase the importance of small business rules by forcing certain categories of spending to include high proportions of small contractors to balance the very large contracts and allow agencies to meet their overall small business goal.

Figure 26: Establishments by U.S. County, 2007

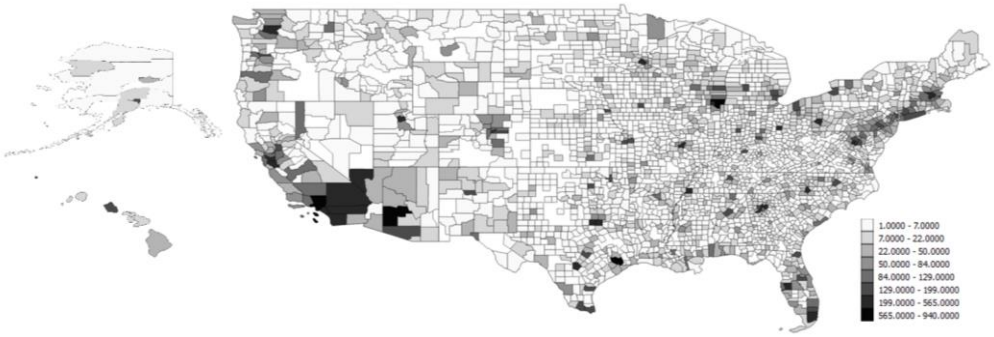

Figure 26: Number of evaluated business establishments by U.S. county. Includes 47,091 establishments with FIPS county codes reported in the NETS. The top five counties are Los Angeles County, California (947 establishments); Harris County, Texas (704); Cook County, Illinois (646); Maricopa County, Arizona (591); and Orange County, California (574).

Business establishments represented in the dataset compose a broad cross-section of the economy. Figure 26 illustrates their geographic distribution across the United States by county. The top five counties are Los Angeles County, California (947 establishments); Harris County, Texas (704); Cook County, Illinois (646); Maricopa County, Arizona (591); and Orange County, California (574). Significant agglomerations of government contractors exist in Southern California and the Northeastern seaboard, as well as a variety of locations across the country often coinciding with military facilities.

Table 25: Descriptive Statistics, 2007

	Min	Median	Mean	99th Perc.	Max	Std. Dev.	Obs
				All Firms			
Employment	1	65	183	1,699	44,593	722	
Sales	$12,795	$10.7m	$32.2m	$310m	$3.98b	$99.6m	
Obligations	$0	$0	$632,435	$11.2m	$448m	$7.2m	7,977
SB Oblig.	$0	$0	$336,236	$5.7m	$163m	$3.7m	
Establishments	1	1	5.8	51	1,563	32.4	

Table 25: There are 7,977 firms in the dataset with positive employment and sales in 2007. Descriptive statistics for 2007 included here are total firm employment, annual sales, total obligations from the federal government, obligations to firms categorized as small businesses (SB), and total number of establishments by firm.

After removing from the dataset those establishments reporting no employees or no revenue, the number of remaining firms falls to 7,977, a decline of 19 percent. Table 25 displays descriptive statistics for these groups. Notable is the strongly skewed distribution, with the median firm having 65 employees and $10.7 million in annual revenue and the mean firm having 183 employees and $32.2 million in revenue. One firm in the dataset received $163 million in small business set-aside obligations in 2007 alone. In the same year, Dun & Bradstreet reported the firm had 740 employees and $198 million in total sales across 12 establishments. The firm's various establishments reported under six distinct two-digit NAICS codes, in industries ranging from personal service agents to aircraft parts to security control systems. Despite being exemplary of the strategic behavior examined here, this firm was a significant outlier. The firm at the 99th percentile of the distribution received only $5.7 million in small business obligations in 2007. This firm had 10 employees, $10 million in total annual sales, and a single establishment.

Section 3.3: Industry Reporting

As reported in the NETS, firms in the dataset operated in a total of 867 distinct six-digit NAICS codes and all 24 two-digit codes in existence. As Figure 27 shows, a large majority of firms (6,839) operated under only a single six-digit NAICS code. An even larger number, 7,348, confined their operations to a single two-digit code. (Two-digit codes constitute the broadest industry categories such as construction or manufacturing.) Nevertheless, some firms engaged in a wide range of activities. The most diverse firm in the dataset, a large construction conglomerate, operated under 45 separate six-digit industries across 614 establishments.

Figure 27: NAICS Codes per Parent Firm, 2007

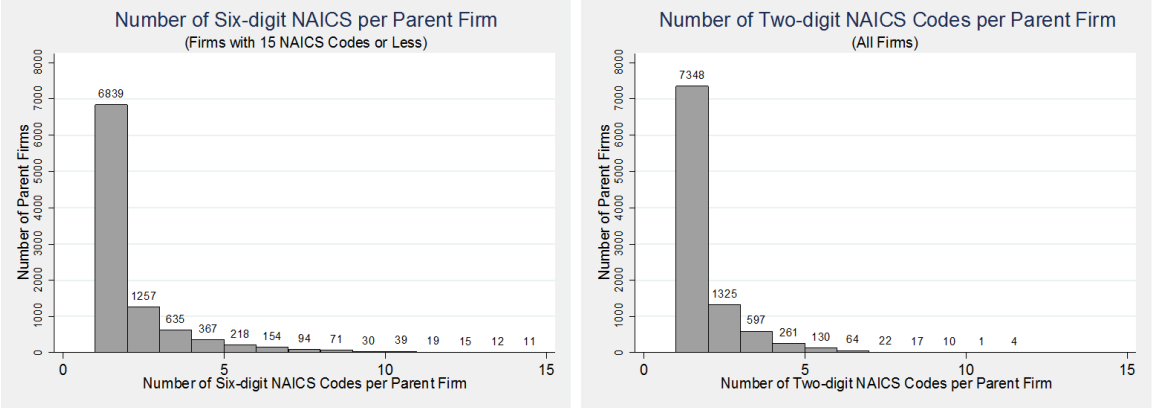

Figure 27: Number of parent firms selling to the government under a given number of NAICS codes. The figure on the left (six-digit) excludes 19 firms selling under more than 15 NAICS codes, with the largest using 45 different six-digit codes. The dataset contains 867 six-digit codes and 24 two-digit codes.

Each establishment in the NETS is identified by a single NAICS code, but there is no such standardization in reporting industry categories to the government. Reporting in FPDS on contractor industries is thus far more diverse, with individual establishments commonly identifying with different industries for different government contracts. Figure

28 illustrates this practice across six-, four-, and two-digit industry codes. In 2007 there were only 540 establishments in the dataset that reported the same six-digit NAICS code to the government as was reported in the NETS. More than 2,000 reported a single industry code to the government that differed from their credit records. A full 137 firms reported five or more industry codes to the government that differed from NETS

Figure 28: Number of FPDS NAICS Codes per D&B NAICS Code

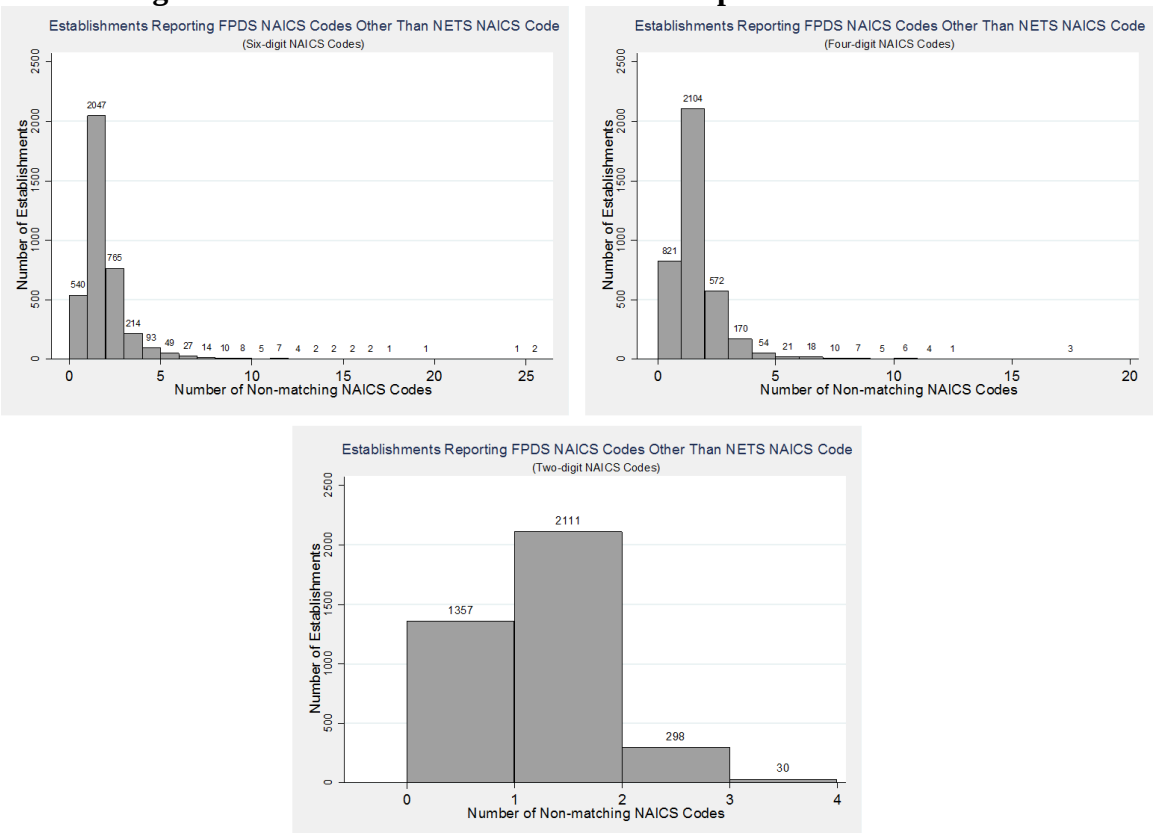

Figure 28: Number of NAICS codes reported for government purchases that do not match the single establishment NAICS code reported to Dun & Bradstreet. Zero represents matching NAICS codes.

reporting. This practice may be understandable for six-digit codes, for which gradations of difference between industry categories are minor, but the practice continues at the level of four- and two-digit codes as well. More than 300 establishments are reported in FPDS under two or more two-digit codes that differ from NETS-reported codes. Some differences, such as Information (NAICS 51) and Professional, Scientific, and Technical Services (NAICS 54) are inconspicuous. Others are more remarkable, such as Arts, Entertainment, and Recreation (NAICS 71) and Wholesale Trade (NAICS 42), a combination managed by two different establishments in the dataset.

Thus at a purely descriptive level, a picture emerges of a complex environment in the market for government contracts. Firms spread their activities across large numbers of establishments, many of which report no employees and no revenue. Large and small businesses alike are awarded contracts through non-competitive procedures, although the reasons often vary. Firms are concentrated in particular areas of the United States, and have a strongly skewed size distribution. They report to the government under a wide range of industry codes, apparently to suit the case at hand. In the section to follow, I use these features of the dataset to examine the four hypothesized means that firms may employ to avoid a sharp loss in competitiveness as they grow beyond the applicable small business size threshold.

Section 4: Analysis

The nature of the regulatory environment for government contractors does not accommodate modeling within a single, comprehensive framework, a characteristic that has perhaps inhibited past research into the market effects of various rules. While

regulations such as small business eligibility thresholds appear to set clear guidelines, there is in practice a wide range of ways to work around them and even turn them to advantage. One of the most widespread means of doing so is through subcontracting relationships. Regrettably, however, the government has only recently begun collecting and publishing data on subcontracts and, much like FPDS in its early years, these data are not accurate or comprehensive enough to be useful. Nevertheless, the means employed here of combining the FPDS and NETS allows for limited exploration of a narrow set of strategic behaviors.

Another important issue is the validity of firms and establishments reported in FPDS. Firms with poor or non-existent credit scores make up roughly half of the dataset. However, eliminating these questionable business entities from observation runs contrary to the purposes of the study. Such companies are part and parcel of corporate strategies to take advantage of regulatory features of the environment. Additionally, many startups have poor credit scores because they are young and small, the very characteristics of interest in this investigation. These firms are therefore included in the analysis to follow. Each model, however, examines only a limited subset of firms affected in a particular way; for example, examining firms with employment levels near a size threshold. This necessarily eliminates outliers reporting zero employees or so many employees that their status as a legitimate small business is questionable. Except where otherwise specified, the unit of analysis is the firm, with employment, sales, and federal obligation metrics aggregated across all affiliated establishments.

4.1 Hiring Restrictions

I begin with the most commonly cited effect of small business thresholds: deliberately restricting hiring to retain small business certification. The theory behind this hypothesis is clear: firms punished for growing larger will choose not to grow. Despite the appeal of this theory in light of basic economic precepts, it neglects aspects of the situation critical to the final outcome. Firms are not people; they are instruments used by people to create value and wealth. As such, a firm can be bought, sold, or modified as circumstances dictate to suit the needs of the owners and managers. Those owners are unlikely to restrict future earnings potential indefinitely in order to preserve the integrity of a purely legal entity.

To test whether firms deliberately limit growth as they approach a small business threshold, I examine employment levels at the beginning and end of the six-year period between 2007 and 2013. If firms engage in such behavior, we would expect to see a discontinuity in growth rates at or around the threshold as hiring slows or stops just below it and then expands rapidly above it as firms attempt to expand organically past the competitiveness gap (i.e., the space between regulatory preferences enjoyed by small firms and economies of scale enjoyed by large firms) as rapidly as possible. There are five different employment thresholds which apply to the industries examined here: 100, 500, 750, 1,000, and 1,500 employees. I standardize employment levels across industries such that zero coincides with the respective threshold. This metric is referred to as "over/under" employment: a firm with 450 employees falling in an industry category with a 500-employee threshold has an over/under employment level of -50 and is comparable to a firm of 700 employees which falls in a 750-employee-threshold industry.

For the purposes of this model, firms are assigned to industries based on the NAICS code under which they received the largest amount of contract obligations.

Figure 29: Employment Relative to SB Threshold, 2007 vs 2013

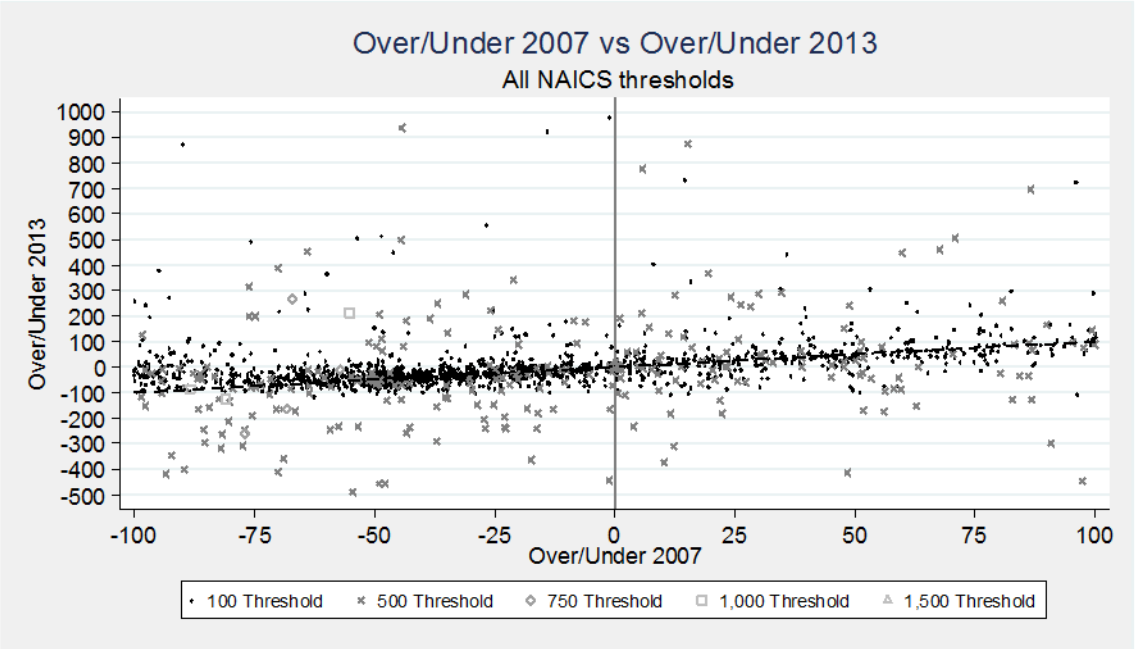

Figure 29: Firm employment over or under the small business threshold at the start of the period in 2007 relative to the end of the period in 2013. Firms are assigned to a small business threshold according to the NAICS code under which they sold the highest value of goods or services. Illustrated is a subset of the 5,638 firms with complete data that sold to the government between 2007 and 2013.

Figure 29 shows the subset of firms falling near the unified over/under threshold at the start of the period, plotting their 2007 employment against their employment at the end of the period in 2013. Firms that do not grow over the six-year period will fall on the dashed zero-growth line at which beginning employment is equal to ending employment. (Note that NETS employment levels are reported as of the first of the year, so 2013 employment represents the level at the end of 2012.) Several characteristics of the

distribution are important. First, the only industries of significance in the analysis are those with employment thresholds of 100 and 500 employees. Not enough firms in the other categories fall near enough to the threshold for the restriction to bind. Second, over/under comparisons are only relevant close to the common threshold of zero. A firm at -99 on the over/under scale which falls in a 100-threshold industry has one employee while a -99 firm in a 500-threshold industry has 401 employees. We expect very different employment growth behavior from these firms, and indeed this difference is apparent in Figure 29. Very small firms in 100-threshold industries tend to experience positive growth by construction (see magnification of the relevant area in Figure 30). Since they cannot have less than -100 over/under employment in 2013, they cluster above the zero-growth line (firms going out of business are not included in the figure). Such an effect is not visible for 500-threshold firms since it occurs far to the left of the region pictured.

Figure 30: Magnification of Figure 29 (1)

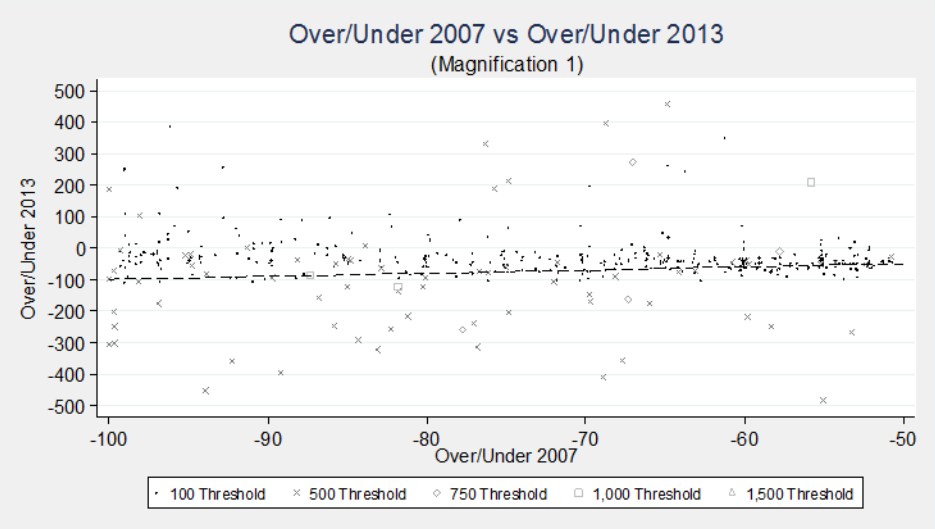

Figure 30: Magnification of Figure 29 at low levels of 2007 employment. By construction, firms in 100-threshold industries fall above the zero-growth line in this region. Firms in 500-threshold industries are not so restricted.

A final notable characteristic of the distribution in Figure 29 is the lack of any discernible discontinuity around the zero over/under threshold. If firms responded to the threshold by deliberately adjusting employment levels, those at or below the threshold in 2007 would be unlikely to rise above it in 2013, resulting in clustering in the southwest quadrant of Figure 31. Similarly, firms choosing to grow organically through the employment threshold would be forced to grow quickly or face an uncompetitive market position. These firms should cluster in the northeast quadrant of Figure 31, above the zero-growth line. There is no clear evidence of such trends, with firms distributed evenly throughout the figure.

Figure 31: Magnification of Figure 29 (2)

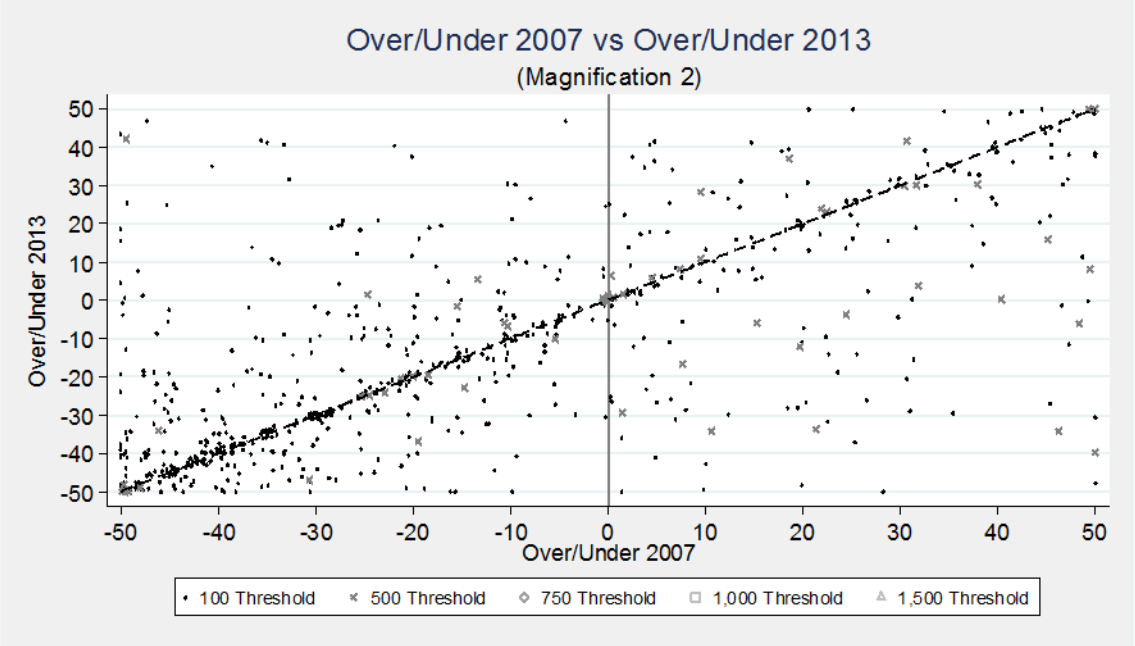

Figure 31: Magnification of Figure 29 near the over/under threshold. If firms deliberately adjust organic employment growth near the threshold, we would expect to see clustering in the southwest and northeast quadrants.

I test this observation with a regression discontinuity model using over/under employment in 2007 as a predictor of over/under employment at the start of 2013. A treatment dummy variable takes on a value of one for firms falling above the employment threshold. The model takes the form

Equation 11: Growth Regression Discontinuity Model

$$y_i = \alpha + \beta t_i + \gamma a_i + \delta t_i a_i + \varepsilon_i,$$

where y_i is relative employment at the end of the period for firm i, a_i is relative employment at the start of the period, and t_i is a treatment status dummy coded to one if the firm size exceeds the employment threshold in 2007. Error is captured in ε_i. The regression is limited to an over/under range of -50 to +50 to both eliminate the variable-size-threshold problem described above and to focus the analysis on the region of interest around the regulatory discontinuity. The model confirms the visual observation of no significant effect, as reported in Column 1 of Table 26 and illustrated in greater detail in Figure 32.

A potential challenge to the method employed is that many firms, small or large, do not rely on limited-competition contracts for survival. Some industries and market segments may possess few characteristics empowering economies of scale and distinguishing small competitors from large ones. Firms in these industries which do not rely on set-asides are unlikely to demonstrate strategic behavior near the threshold. Column 2 of Table 26, corresponding with the illustration in Figure 33, re-calculates the model including only those firms that received more than half of their federal government funding through non-competitive obligations. Again there is no distinguishable difference between firms above and below the threshold. Indeed, the clustering of firms just below

Table 26: Employment Regression Discontinuity Models, 2007 vs 2013

	All Firms	Firms with >50% preferential obligations
	(1)	(2)
Dependent Variable:	Over/Under2013	Over/Under2013
Treat	**-2.49**	**-3.78**
	(4.76)	**(6.11)**
Over/Under2007	0.854***	0.834***
	(0.0786)	(0.102)
*Over/Under2007 * Treat*	0.0779	0.0739
	(0.178)	(0.229)
Constant	-6.61**	-6.1*
	(2.81)	(3.6)
Observations	766	476
R-squared	0.415	0.386

Notes: *** Significant at the 1 percent level
** Significant at the 5 percent level
* Significant at the 10 percent level
(Robust standard errors)

Table 26: Regression discontinuity model comparing employment over or under the relevant threshold in 2007 to employment over or under the relevant threshold in 2013. The model takes the functional form $y_i = \alpha + \beta t_i + \gamma a_i + \delta t_i a_i + \varepsilon_i$, where y_i is relative employment at the end of the period, a_i is relative employment at the start of the period, and t_i is a treatment status dummy coded to 1 if the firm size exceeds the employment threshold in 2007. See illustrations in Figure 32 and Figure 33 below.

the zero over/under line that would be expected in a dynamic environment is notably absent. While several firms do fall precisely on the zero over/under line, this is likely due primarily to the common reporting practice of rounding off employment levels rather than to strategic behavior. Similar clustering around other round numbers which have no regulatory significance is also visible in the dataset. Many other reasonable variations on this model that are not reported here, such as alternative functional forms, time periods, and firm-level requirements for inclusion in the sample, all fail to detect a substantial effect of the threshold on firm hiring behavior.

Figure 32: Employment RD Model, All Firms

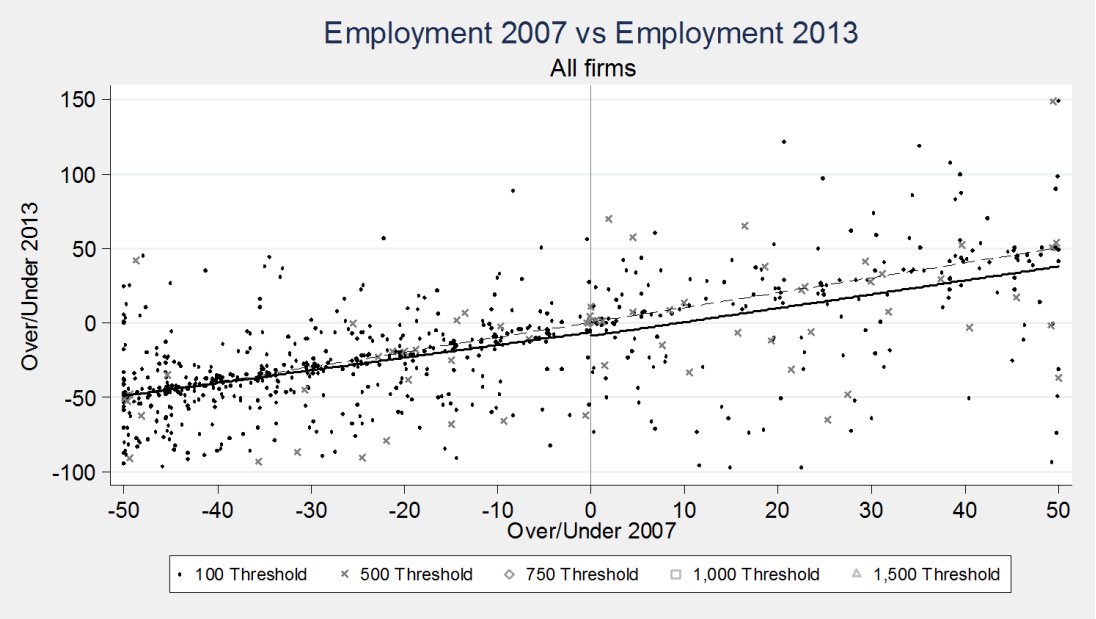

Figure 32: Illustration of regression discontinuity model in Table 26, Column 1 (all firms). There is no apparent substantial propensity of firms to delay employment growth at the small business eligibility threshold.

Figure 33: Employment RD Model, Firms Receiving Non-competitive Funds

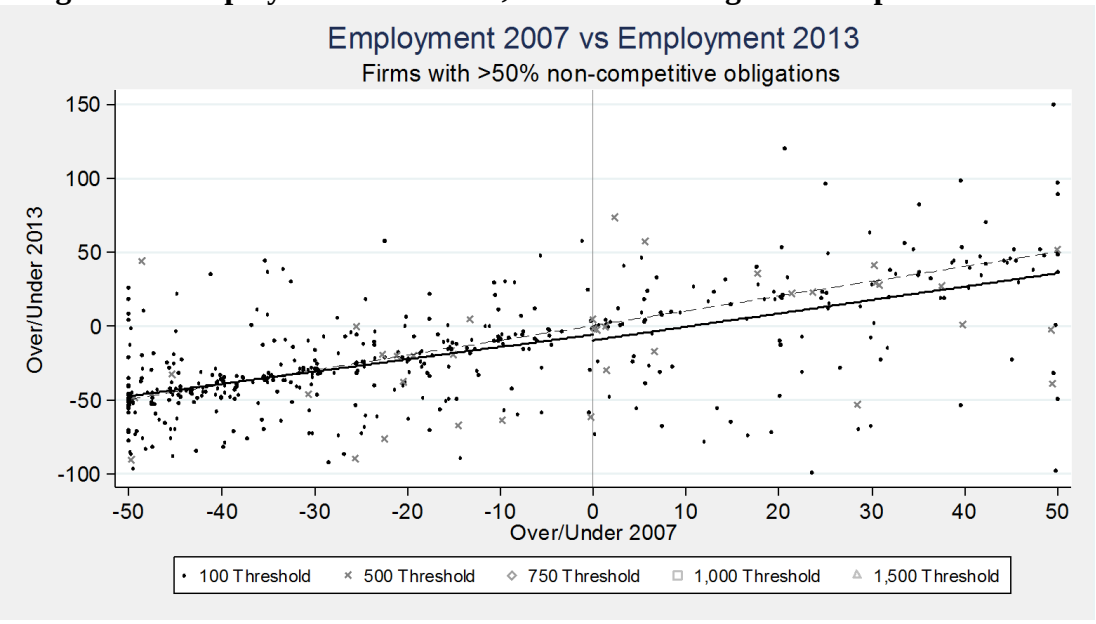

Figure 33: Illustration of regression discontinuity model in Table 26, Column 2 (firms receiving more than 50 percent of total obligations through non-competitive contracts). There is no apparent substantial propensity of firms to delay employment growth at the small business eligibility threshold.

4.2 Probability of Exit

A more likely method of avoiding a drop in competitiveness as a firm crosses the size threshold is to sell. Unlike the many small business owners described by Hurst, et al., (2011) who are content to be their own boss and mind the store, an entrepreneur ambitious enough to build a company up to the substantial size implied by binding small business regulatory restrictions is unlikely to be content with permanently limiting his or her income potential due to the employment cap. Selling the company presents a ready means of capitalizing the organization's future potential for value creation. A buying firm could combine the operation with its own, enabling the purchased concern to bypass the low-competitiveness region between eligibility and scale.

To test this hypothesis, I examine the annual probability of exit from the dataset relative to employment levels in the previous year. For example, all firms with an over/under employment level of -50 in 2007 are examined in 2008 to determine how many remain in business. The dependent variable is calculated as the number of firms exiting the dataset divided by the total number in existence the prior period. A similar calculation is performed for each year through 2013 and the results aggregated to a combined probability measure attached to the independent variable of over/under employment: in the example, -50. Figure 34 illustrates the resulting relationship, with the overall probability measure in the top panel and the raw numbers used to calculate the probability for each employment level on the bottom. The probability of exit is high for small firms and decreases as firm size increases. This trend reverses just prior to the eligibility threshold, rising sharply to nearly 15 percent probability of exit for firms one

Figure 34: Employment vs Probability of Exit in the Following Year

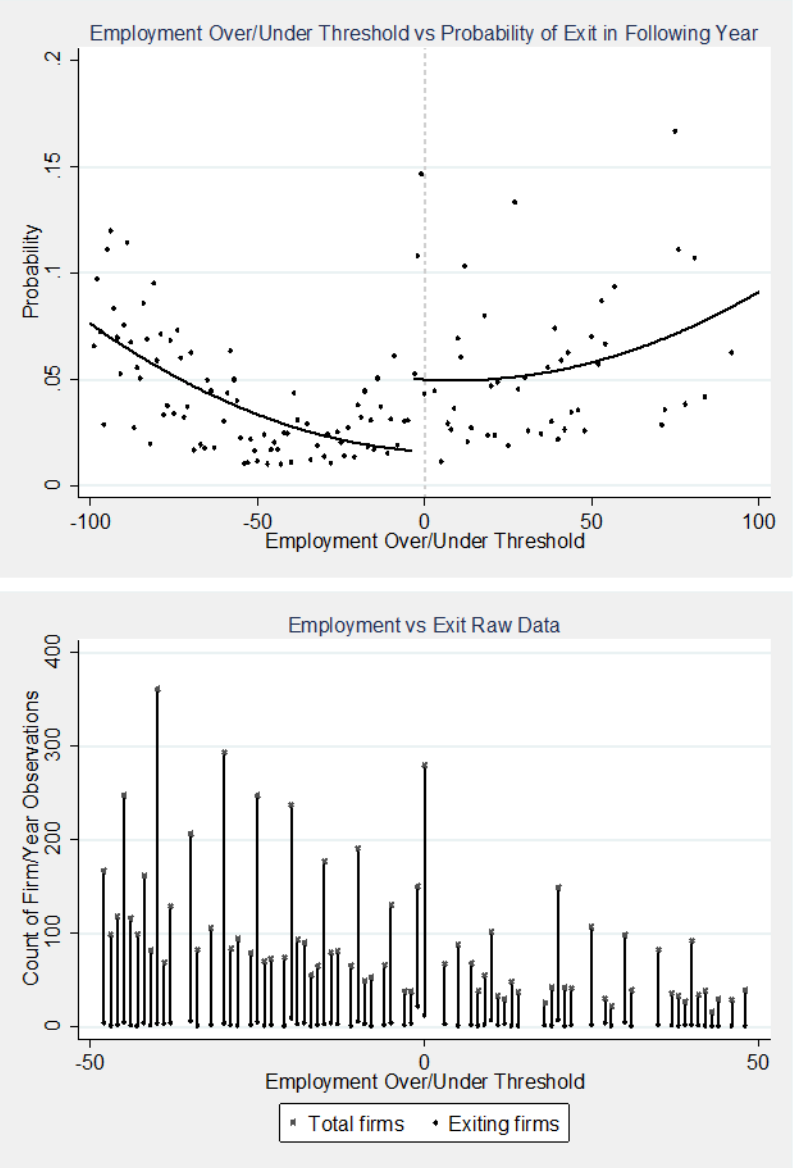

Figure 34: Illustration of regression discontinuity model in Table 27, Column 1. The probability of exit clearly begins rising prior to the threshold, suggesting the discontinuity is not sharp. The panel on the bottom shows raw data for the probability calculations, with data pairs (exiting firms/total firms) connected.

employee below the threshold (22 of 149 firm/year observations meeting this criterion exited the dataset the following year). The bottom panel in Figure 34 shows the raw data for probabilities displayed in the top panel. The dispersion of probabilities above the

employment threshold appears to be affected by smaller numbers of firms falling in this employment range.

Results of a regression discontinuity model estimating the size of the effect are presented in Table 27 and illustrated in Figure 34. The functional form is:

Equation 12: Small Business Probability of Exit Model

$$y = \alpha + \beta t + \gamma a + \delta a^2 + \varepsilon$$

where y is the probability of subsequent exit, a is employment relative to the small business threshold, and t is a treatment dummy variable taking the value of one for firms above the indicated threshold. The break in probability of exit occurs slightly prior to the

Table 27: Employment vs Probability of Exit in the Following Year

Dependent Variable:	Probability of exit in following year
Treat	**0.0342*** **
	(0.0111)
Over/Under	-0.0001
	(0.000132)
Over/Under ^{2}	0.000005***
	(0.00000126)
Constant	0.0159***
	(0.00447)
Observations	131
R-squared	0.303

Notes: *** Significant at the 1 percent level
** Significant at the 5 percent level
* Significant at the 10 percent level
(Robust standard errors)

Table 27: RD model comparing employment relative to the -3 over/under threshold in any given year to the probability of exit from the dataset in the following year. The model takes the functional form $y_i = \alpha + \beta t_i + \gamma a_i + \delta a_i^2 + \varepsilon_i$, where y_i is the probability of subsequent exit, a_i is employment relative to the small business threshold, and t_i is a dummy variable taking the value of one for firms above -3 in over/under employment. See illustration in Figure 34 above.

zero value on the over/under employment scale. At an employment level three employees prior to the threshold (e.g., 97 employees for firms in 100-threshold industries and 497 employees for firms in 500-threshold industries), firms are approximately 3.4 percent more likely to exit in the following year relative to firms with one employee fewer as depicted in Table 27. The result shows strong statistical significance. This effect is substantial relative to the overall mean annual probability of exit of 4.6 percent.

4.3 Acquisitions

Selling a firm is one option for avoiding the size threshold's effects on competitiveness, but equally appealing to a business owner may be rapid expansion. Despite losing the benefit of small business set-asides, the acquiring firm gains economies of scale that enable it to compete with other large contractors. Given than the regulatory threshold creates a growth-induced discontinuity in competitiveness, we would expect to see a comparable discontinuity in growth as firms pass it. The only feasible means of accomplishing such a size discontinuity is acquiring another business establishment or firm.

I test this hypothesis by examining the behavior of a subset of firms which continue in operation as they pass the employment threshold. Much as employment levels were standardized around the size threshold in previous models, here I standardize time to zero in the year of threshold crossing. The time -5, for example, represents five years before a firm crosses its respective employment threshold and 5 represents five years after. As different industries have different characteristics making economies of scale more or less important, we would expect to see differences in the importance of small

business set-asides between firms in different NAICS codes. In sectors where small firms have no trouble competing with large firms (landscaping, perhaps), competitors likely see little value in obtaining small business certification and complying with all of the regulatory requirements it entails. The sample is therefore limited to firms receiving a positive amount of non-competitive obligations over the period (it is not limited to firms receiving 50 percent or more of funding in non-competitive obligations due to sample size limitations).

Figure 35: Establishments and Employees Before and After Threshold Crossing

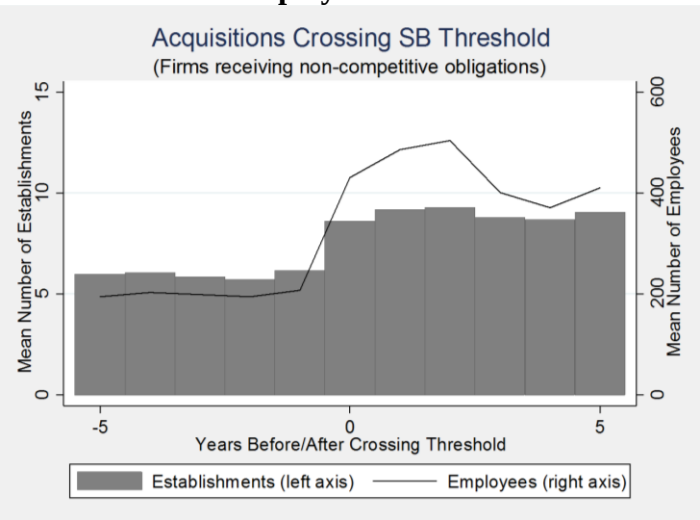

Figure 35: Illustration of model in Table 28. Firms show a discontinuous jump in establishment and employee counts in the year they cross the small business threshold.

Figure 35 illustrates the time series of average numbers of establishments and employees composing each firm in the sample. The discontinuity as firms grow past the threshold is clear, with establishment and employee counts both jumping sharply in the year firms pass from below the threshold to above it. The linear regression model in

Table 28 shows that the average number of subsidiary establishments rises by 2.85 in the year of crossing, and the number of employees by a substantial 261. Unlike the aggregated probability of exit model above, this model of the form

Equation 13: Small Business Acquisitions Time Trend Model

$$y_i = \alpha + \beta t_i + \gamma \tau_i + \delta t_i \tau_i + \varepsilon_i$$

Table 28: Establishments and Employees Before and After Threshold Crossing

	(1)	(2)
Dependent Variable:	Establishments	Employees
Treat	**2.85*** **	**261*** **
	(0.563)	**(26.6)**
Before/After	0.0245	1.67
	(0.161)	(5.48)
*Before/After * Treat*	0.0112	-13.6
	(0.227)	(10.2)
Constant	6.01***	205***
	(0.428)	(13.9)
Observations	2,716	2,716
R-squared	0.029	0.057

Notes: *** Significant at the 1 percent level
** Significant at the 5 percent level
* Significant at the 10 percent level
(Robust standard errors)

Table 28: Time series discontinuity of establishment and employee counts as firms cross the small business threshold. Time is standardized to zero in the year a firm crosses the threshold; this before/after measure predicts the total number of establishments incorporated under a given firm according to the model $y_i = \alpha + \beta t_i + \gamma a_i + \delta t_i a_i + \varepsilon_i$ where y_i is the number of establishments, t_i is a dummy coded to 1 for firms which have passed the threshold, and a_i is the standardized year.

includes all of the firms, i, in the sub-sample as distinct data points, with y_i being their corresponding annual number of establishments or employees and τ_i the standardized year. Again, t_i is a dummy coded to one for firms which have passed the threshold. The model assumes a linear time trend. The sample contains 388 firms that remained in business over the seven years examined. Of these, 206 operated in industries with a 100-

employee threshold, 161 faced a 500-employee threshold, 12 faced a 750-employee threshold, eight faced a 1,000-employee threshold, and one faced a 1,500-employee threshold. The likelihood of exit from the competitive space (demonstrated above) along with this evidence of discontinuous growth suggest strong importance of both merger and acquisition activity for firms affected by small business regulations.

4.4 Alternate Industry Reporting

A final hypothesized strategic behavior employed to circumvent the regulatory discontinuity in the market space occupied by small government contractors is competing for work under alternative industry codes. As described in Section 3 and illustrated in Figure 27 and Figure 28, contractors report to government contracting officers under many different NAICS codes. While some of this behavior may be driven by published requirements that specify certain industries for those submitting proposals, contractors also take advantage of vague requests for proposal to report under codes most advantageous to them from a regulatory perspective. They can also make small adjustments to their product mix, perhaps changing from "storage" batteries to "primary" batteries as in the example above.

To test this hypothesis, I limit the sample to approximately 74 firms which began the period with less than 100 employees and ended it with more than 100 employees. Again looking at an annual panel of firm-level data between 2007 and 2012, I examine the NAICS codes reported by these firms in FPDS. For each firm, I calculate the amount of obligations received in NAICS codes associated with the 100-employee threshold as a percentage of total obligations under all NAICS codes. In a 2007 to 2012 panel

regression model controlling for fixed effects by firm and year as well as the level of total obligations received, the level of employment predicts the percentage of 100-threshold NAICS code funding according to the following functional form:

Equation 14: Alternate Industry Reporting Fixed Effects Model

$$y_{it} = \alpha + \beta a_{it} + \gamma b_{it} + \delta_i + \nu_t + \varepsilon_{it}$$

Here y_{it} is the percentage of 100-threshold obligations in firm *i* and year *t*, a_{it} is the level of employment, b_{it} is annual obligations, and δ_i and ν_t are fixed effects by firm and year.

As in the previous examination of acquisition activity, I also estimate the model using only firms receiving some amount of limited-competition funding (the threshold for inclusion is again not set at 50 percent of funds due to sample size). This restriction cuts the sample to approximately 60 firms. Figure 36 provides illustrations of the data for both the full and limited samples. By construction, employment rises over the period. As this trend continues, the percentage of funds obligated to firms reporting 100-threshold

Figure 36: Time Series of Employment and Threshold Funding Percentage

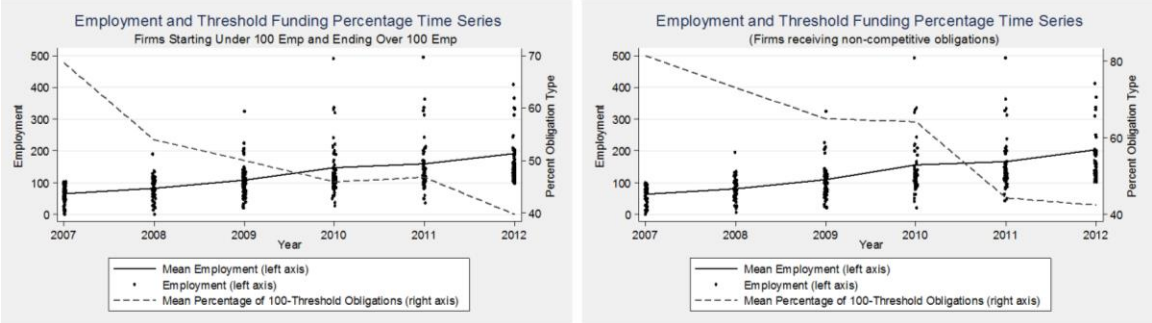

Figure 36: Illustration of the FE model in Table 29. All firms in the sample start the period with less than 100 employees and end it with more than 100 employees. The dashed line shows the average percentage of obligations awarded under NAICS codes associated with the 100-employee threshold. The panel on the left contains all firms for which the criteria apply (74 begin and 71 finish). The panel on the right contains only firms receiving non-competitive funding (60 begin and 57 finish).

NAICS codes drops from nearly 70 percent to 40 percent, suggesting firms switch to reporting under NAICS codes allowing continued eligibility for small business set-asides as their level of employment rises past the threshold. The change in the average threshold percentage is even stronger among firms receiving limited-competition funding, starting the period at 80 percent.

I test the causal relationship using the fixed effects model depicted in Table 29. Column 1 shows results for the full sample of approximately 74 firms. One additional employee in a given year is associated with a decline in the percentage of 100-threshold funding of 0.047 percentage points, significant at the 10 percent level. Despite the smaller

Table 29: Model of Employment and Threshold Funding Percentage

	(1)	(2)
Sample:	All firms	Firms receiving preferential funds
Dependent Variable:	Percent 100-Threshold Obligations	
Employment	**-0.0465***	**-0.11****
	(0.0269)	**(0.0426)**
Obligations	0.00000402**	-0.00000691
	(0.00000157)	(0.00000457)
Constant	52.9***	79.4***
	(3.26)	(4.66)
Observations	348	234
R-squared within	0.0462	0.0524
R-squared between	0.0323	0.0529
R-squared overall	0.0004	0.0292
Notes: *** Significant at the 1 percent level ** Significant at the 5 percent level * Significant at the 10 percent level (Robust standard errors)		

Table 29: Fixed effects model comparing employment in a given year to the percentage of total obligations allocated under NAICS codes with a 100-employee threshold. The sample is limited to approximately 70 firms that began the period below the 100-employee threshold and ended it above. The model is specified as $y_{it} = \alpha + \beta a_{it} + \gamma b_{it} + \delta_i + \varepsilon_{it}$ where y_{it} is the percentage of 100-threshold obligations in firm i and year t, a_{it} is the level of employment, b_{it} is total obligations, and δ_i are fixed effects by firm. See Figure 36 above.

sample size, the model including only firms receiving preferential funding shows an effect more than twice as strong, with a p-value of 0.012. Firms indeed seem to respond to regulatory incentives to shift sales into industry categories allowing continued eligibility for small business set-asides.

While no single model can capture the entire effect of strategic behavior at the regulatory threshold, the accumulation of evidence is striking. Business owners show little inclination to limit employment growth in their firms in response to small business thresholds. They do, however, respond with abnormally high levels of merger and acquisition activity and by shifting sales into industry categories with higher employment thresholds. The parsimony of the models used to test these relationships is a testament to the clarity of the evidence, which is generally visible to the naked eye in basic illustrations of the data. The question remains as to the significance of these effects. What are the economy-wide efficiency losses created by such behavior, and do they compare with purported benefits of stimulating the growth of entrepreneurial enterprises? This question is far more difficult to answer, but I turn to it now.

Section 5: Discussion

Do the effects described above result in substantial reductions in economic efficiency? It is impossible to quantify precisely the costs involved due to the nature of the problem. Business owners employ any means available to decrease costs and, as made clear in this analysis, resulting strategic behavior can take many forms. Many of these are not modeled here. One such form is beneficial subcontracting relationships, perhaps the most prevalent means of leveraging small business regulations to one's

advantage. Another clear example of the difficulty of quantifying costs is merger and acquisition activity. Estimating legal fees, lost productivity, organizational efficiencies (or inefficiencies), and any number of other costs that may come with a corporate merger are well beyond the scope of this analysis. Were such a calculation possible across all affected firms, one must still determine whether such a merger would have happened anyway, either immediately or at some time in the future. Convincingly quantifying such a counterfactual is impossible.

Nevertheless, some basic statistics are illuminating. The sample assessed here contains 10,733 firms comprising 48,022 establishments, all of which were directly or indirectly affected by federal small business regulations. More than half, or 5,638 firms, received some amount of government spending over the period, with 1,417 receiving more than $1 million in obligations. In aggregate, these firms received an average of $6.4 billion each year in revenue from federal government customers, nearly half of which went to firms reporting as small businesses. Total annual sales for the group averaged $262 billion, or 1.8 percent of US gross domestic product in 2009. They employed 1.5 million people, or 1.4 percent of the employed labor force, and conducted operations in 84 percent of the counties in the nation.

Approximately 800 firms in each year of the sample period found themselves within 50 employees of reaching their small business eligibility threshold. These firms comprised a total of 3,400 establishments and over 100,000 employees, bringing in more than $20 billion in aggregate annual revenue. There is no evidence that employment growth in these firms was restricted by eligibility thresholds. However, over the period

examined 1,218 of them would cease to be independent operations. There were only 388 firms that remained independent concerns as they grew through a binding eligibility threshold. The total number of establishments at these firms grew from 2,288 to 3,404 and their aggregate employment grew from 75,000 to over 177,000. Given that total US private employment over the period fell from 115.4 million to 113.2 million over the same time frame (even dropping to 107.3 million at the bottom of the intervening recession), it is unlikely that the innovative dynamism of the sector led to this remarkable performance.

We cannot be certain of the costs incurred by the more than 1,600 firms that engaged in merger and acquisition activity over the period while being simultaneously affected by growth limits imposed by small business regulations. The number is undoubtedly large, particularly in relation to the limited confines of the market sector. To this value we must add the inefficiencies generated by firms adjusting their industry reporting, or perhaps product itself, to remain eligible for small business preferences. This study examined only a narrow subset of the market in its investigation of the practice. The 74 firms that grew past the 100-employee threshold between 2007 and 2012 did not likely incur significant costs in changing their proposals to read, for example, NAICS 335912 rather than 335911. Implications for the broader market, however, are more substantial. How many firms operating above a threshold chose to manufacture a product not ideally suited to economy-wide demand due to the effective subsidy offered by the government to produce in a suboptimal industrial category? How many man-hours were spent researching applicable regulations to ensure firms retained small business

certification? On the other side, what were the costs of enforcement? The budget request of the SBA for fiscal year 2016 was $701.3 million.

The numbers estimated here would be larger if we were to include the many other industries that operate under revenue (rather than employment) limits or have otherwise seen their threshold change over the period in question. These categories were excluded from the analysis for the sake of parsimony. Considering them does more than increase the size of the sample. It complicates the challenge firms face in navigating the regulatory environment as they must weigh both employment and revenue figures in calculating optimum strategies. It increases the potential for lobbying to secure more favorable thresholds in future regulatory revisions.

From this perspective, the cost of federal small business regulations is significant. But while microeconomic analysis can suggest the relative importance of various mechanisms through which resources are consumed, but it must ultimately be unsuccessful in tallying a total cost. Theory, however, offers another means of arriving at an estimate of the aggregate social value of small business policies. Tullock (1967) demonstrates that market participants in competition for rents generated by artificial barriers to commerce have an incentive to expend resources on otherwise unproductive activities up to the point where the benefits to be gained are canceled out. Thus in the long run (and small business policies in their present form have been in place since 1953), the net social benefit must approach a negative number. Supernormal profits accruing to the favored group are spent in competing with one another to take best advantage of the needlessly complicated situation. The effort expended in the process is

diverted from productive activity. If firms achieve a situation of earning sustained rents only with some probability (an apt description of the situation enjoyed by the largest government contractors) then the competition among firms seeking to join the elite club will in aggregate expend more resources than the potential profit to be gained by the eventual winners.

The analysis here identifies several means by which this competition occurs. The most important appears to be sales of businesses which would otherwise continue to grow and innovate in the absence of a binding size threshold. Business owners seeking to join the Lockheed Martins and Boeings of the world must do so not by nurturing a competing enterprise, but by leveraging preferential treatment in order to grow to a size at which a merger is worthwhile for the buying firm. This effect runs contrary to the express purpose of small business regulations of encouraging the growth of competitors to large, established government contractors.

Similarly, businesses which choose to remain independent must find a way to overcome the sharp loss in competitiveness that occurs as they pass the regulatory threshold. Doing so requires a growth discontinuity comparable to the discontinuity in revenue caused by removal of the effective subsidy. Since organic growth of this scale and speed is not likely to be successful, the only feasible means of accomplishing the transition is through acquisitions of other going concerns. The sharp discontinuity visible in Figure 35 suggests that a substantial amount of acquisition activity occurs as a result of the regulatory environment, and not because it would be otherwise profitable. Although the number of firms attempting such a transition is smaller than those who choose to sell,

the theoretical welfare implications are greater. Firms choosing this path are effectively buying Tullock's lottery ticket in the hope of earning sustained monopoly rents. Theory suggests that, in aggregate, their non-productive competition is costlier than the prize to be gained.

Section 6: Conclusion

Modeling strategic behavior associated with small business regulation is challenging. There are innumerable ways that firms can avoid uncompetitive situations caused by the rules, and many ways they can turn the rules to their advantage. As regulators adjust to limit such behavior, firms accordingly re-assess the situation. They anticipate regulatory obstacles as they would a competitor or supply restriction in the private marketplace. The apparent discontinuity in the regulatory environment created by the small business threshold creates a wide range of small behavioral changes along many dimensions rather than a single noticeable market distortion. Nevertheless, it generates clearly visible behavioral changes on the part of small government contractors.

Researchers have hypothesized about these effects; only some of these hypotheses bear scrutiny. That firms deliberately limit growth to maintain small business eligibility is both theoretically unlikely and not supported by the data. They do, however, respond in other ways such as mergers, acquisitions, and defining themselves and their products in ways that are privately beneficial. These behaviors are clearly visible in sets of firms carefully constructed to include those affected by the situations in question. Simple econometric models help to illustrate the relative importance of various methods of gaming the system.

Identifying evidence of strategic behavior requires winnowing a group of over 10,000 business entities down to, in some cases, less than 100 firms most effected by a rule. As the discussion above makes clear, however, this does not imply that the overall magnitude of rent seeking is insignificant. It rather suggests that, wherever you look, you find evidence of firms behaving in suboptimal ways. Small business regulations may have functioned as designed in the early years, when the population of private business owners was used to a different set of rules. More than sixty years later, the rules have become an integral part of the business environment, and modifying otherwise optimal behavior to avoid their effects is a necessary part of competition with other firms which are doing the same. Those best able to take advantage of the system emerge winners, and are further entrenched as incumbents by the added complexity that must be navigated to reach the position.

The policy implications of the analysis are clear. Small business rules do not exist in a static environment. More than sixty years after their introduction, new business practices have emerged that dissipate through non-productive activities the benefits that accrued to the favored group. It is possible that many very small firms far from the regulatory discontinuity benefit from the situation and counterbalance the large amount of unproductive activity generated near the threshold. This possibility must be examined in greater detail as nascent databases of subcontracting activity grow to useful size. Theory suggests, however, that supernormal profits are dissipated in unproductive activities. The evidence presented here supports that theory for a narrow subset of the market. Despite the urge to introduce more rules to counter each new attempt by

businesses to circumvent existing limitations, legislators and the SBA would be better served by simplifying the system to limit the scope of unproductive competition.

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