Financing Constraints as Barriers to Innovation:
Evidence from R&D Grants to Energy Startups

JOB MARKET PAPER

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Abstract

Governments regularly subsidize new ventures to spur innovation, often in the form of R&D grants. This paper examines the effects of such grants in the first large-sample, quasi-experimental evaluation of R&D subsidies. I implement a regression discontinuity design using data on ranked applicants to the Small Business Innovation Research grant program at the U.S. Department of Energy. An award approximately doubles the probability that a firm receives subsequent venture capital and has large, positive impacts on patenting and the likelihood of achieving revenue. The effects are stronger for more financially constrained firms. In the second part of the paper, I use a signal extraction model to identify why grants lead to future funding. The evidence is inconsistent with a certification effect, where the award contains information about firm quality. Instead, the grant money itself is valuable, possibly because it funds proof-of-concept work that reduces investor uncertainty about the technology.

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1 Introduction

Governments regularly subsidize research and development (R&D) in new ventures.[1] One rationale for such subsidies is that the private sector does not internalize the social benefits of innovation.[2] Another is that financial frictions lead to underinvestment in early-stage R&D.[3] Yet critics contend that government R&D subsidies are ineffective because they crowd out private investment, or because they allocate funds inefficiently (Wallsten 2000, Lerner 2009). Despite opposing theoretical arguments, we have little empirical evidence about the effectiveness of R&D subsidies, nor about whether financing constraints are first-order barriers to innovative startups.

In the first quasi-experimental, large-sample evaluation of R&D grants to private firms, I show that the grants have statistically significant and economically large effects on measures of financial, innovative, and commercial success. I then provide evidence that the grants benefit firms because they ease financing constraints. Finally, I explore the specific mechanism through which grants alleviate financial frictions.

The study is based on a new, proprietary dataset of applications to the U.S. Department of Energy's (DOE) Small Business Innovation Research (SBIR) program. The data include 7,436 small high-tech firms and over $884 million in awards from 1983 to 2013. Awards typically fund testing or proof-of-concept of a new energy technology. DOE officials rank firms within competitions, and I exploit these ranks in a sharp regression discontinuity design that compares firms immediately around the award cutoff.

I show that a Phase 1 grant of $150,000 approximately doubles a firm's chance of subsequently receiving venture capital (VC) investment, increasing the long term probability by 9 percentage points from 10% to 19%. Within two years of the grant, the effect is 7 percentage points. These results imply that on average the grants do not crowd out private capital, and instead transform some awardees into privately profitable investment opportunities. I provide evidence that the effect does not reflect reallocation of capital from losers to winners within competitions.

Firms that tend to be more financially constrained receive the most benefit. First, the

[1]In addition to the federal SBIR, many U.S. states have similar programs. Parallels overseas include the UK's Innovation Investment Fund, China's Innofund, Israel's Chief Scientist incubator program, Germany's Mikromezzaninfonds and ZIM, Finland's Tekes, Russia's Skolkovo Foundation, and Chile's InnovaChile.

[2]For evidence that startups contribute disproportionately to economic growth, see Akcigit and Kerr (2011), Haltiwanger et al. (2013), and Audretsch, Keilbach and Lehmann (2006).

[3]Grants might increase investment if given to startups that face excessively costly external finance. Frictions that can lead to such costly finance and thwart privately profitable investment opportunities include information asymmetry, asset intangibility, and incomplete contracting (Akerlof 1970, Holmstrom 1989).

effect is strongest for the youngest firms, and I show that it declines with firm age. Second, the effect is larger and more robust for immature technologies, like geothermal and wave energy, that are likely the riskiest investments. Third, the effect is stronger in times when external finance is harder to access. Employing clean energy industry Tobin's Q as a proxy for investment opportunities, I find that when Q is lower, the grant effect is larger. The effect is also negatively correlated with total U.S. venture deal flow, a proxy for VC availability.

Beyond the consequences for future private financing, I also show that the Phase 1 grants influence real outcomes. A grant leads a firm to produce about 1.5 extra patents within three years, increasing the average from one patent to 2.5 patents. It is associated with greater technology commercialization, increasing the probability a firm achieves revenue from 52% to 63%. While grants do not affect firm survival, they do increase exit probability via IPO or acquisition. Like the results on future financing, these results are stronger for more constrained firms. Together, the VC, patent, and revenue results show that the early stage grants enable new technologies to go forward.

While Phase 1 grants have large, positive effects on financing and real outcomes, I find that later stage grants are ineffective. Phase 1 winners can apply for Phase 2 grants of $1 million, disbursed about two years after the Phase 1 award. Entrepreneurs' revealed preference indicates that they perceive relatively low benefits to the much larger grant. For example, among firms that get VC within two years of Phase 1, 55% opt *not to apply* to Phase 2. Regression discontinuity estimates using Phase 2 applicants yield tiny or negative effects on VC finance, and small positive effects on patents and patent citations. These findings suggest that - perhaps due to very high discount rates - Phase 2 is often not worthwhile for high-quality firms, and has little benefit among firms that do apply.

What mechanism might explain the early stage grants' impact on future financing? In a simple signal extraction model, I capture how the grant might influence investor beliefs to ease financing constraints. One mechanism is *certification*; the government's decision conveys positive information to venture capitalists about the firm's technology. Alternatively, the money itself may switch the net present value (NPV) of investing in the startup from negative to positive (a *funding* effect). The NPV may initially be negative because of financing frictions like information asymmetry and agency problems, or because the technology risk at such an early stage is too high. The funding effect has two possible channels. First, the grant could allow the entrepreneur to retain more equity; in the counterfactual, an investor might require such a large stake that entrepreneurial incentives could not be maintained. Second, the startup might use the grant to prove the viability of its technology. This *prototyping* channel could reduce investor uncertainty.

I use my empirical evidence to identify which mechanism most likely drives the grants' effect on VC. The certification test reveals an important fact about the grant program: officials seem unable to identify high-quality firms. The test asks whether applicant ranks are correlated with outcomes, conditional on award status. Rational investors should view the grant as a positive signal only if ranks are relevant to market outcomes. This is because a firm's rank within a competition, which the investor *does not* observe, maps directly to whether the firm wins, which the investor *does* observe.[4] Empirically, the ranks are uninformative about all outcomes that I observe. For example, conditional on winning, more highly ranked firms are not more likely to receive VC; the same is true conditional on losing. To the rational investor, the grant signal is pure noise. Thus certification is unlikely to explain the large jump at the discontinuity.

Instead, the evidence best supports the funding effect and is most consistent with the prototyping channel, where the grant enables proof-of-concept work that the firm cannot otherwise finance. Startups with a successful prototype can demonstrate to investors that their technology works as advertised. After Phase 1 prototyping, there is enough information for the private market to take over. At this later stage firms either prefer VC to government funds, or apply to Phase 2, in which case the larger grant crowds out private investment. In Section 5, I discuss the mechanisms and describe in detail how I tell them apart.

Seattle-based Oscilla Power, a wave energy startup, illustrates the prototyping hypothesis. Founded in 2009, Oscilla won its first DOE SBIR Phase 1 grant in May 2011 to conduct "testing activities to ensure the reliability of both the core power generation module as well as the mooring lines."[5] In an interview, CEO Rahul Shendure said that this proof-of-concept work helped Oscilla raise a \$1.6 million Series A round from venture investors in November 2011. "Phase 1 is not providing a material amount of money in terms of the investor's dollar," he said, "instead it's about running experiments, demonstrating that the idea you have works, or doesn't work." In his opinion, the grants "have no certification effect," a view shared by nearly all thirty of the venture capitalists I interviewed.

For startups like Oscilla, early stage grants appear to relieve a critical liquidity constraint on R&D investment. Such startups are an important middle ground between universities and national labs, which must undertake basic R&D, and large firms, which have the market-oriented discipline to efficiently conduct later stage applied R&D (Griliches 1998;

[4]The decision about a competition's award cutoff is exogenous to the ranking process. Officials producing the ranks do not determine the cutoff and are uncertain about the number of awards.

[5]From the application abstract.

Aghion, Dewatripont and Stein 2008).[6] My results suggest that for early stage applied R&D in capital-intensive sectors, there may be space for a hybrid model that involves both government funding and startups.

Severe financing constraints at the "seed" stage, however, contrast with evidence from Phase 2 that later stage ("Series A") projects may not suffer from the same frictions. This study's main policy implications, therefore, are that the SBIR program - and potentially similar programs - could achieve better outcomes through reallocating money (1) from larger, later stage grants (Phase 2) to more numerous small, early-stage grants (Phase 1); and (2) from older firms and regular winners to younger firms and first-time applicants. I do not address the complex questions of optimal program size or whether government should be subsidizing private R&D.

This paper builds on the costly external finance literature, which finds evidence of financing constraints but has focused on large public companies and rarely studied R&D. I provide a novel and plausibly exogenous cash flow shock that identifies a causal relationship between financing constraints and investment responses.[7] In addition, this study relates to the literature on barriers to entrepreneur entry (Chatterji and Seamans 2012, Hochberg, Ljungqvist, and Lu 2007, Black and Strahan 2002). Finally, in establishing a causal effect of grants on outcomes, I contribute to the literature evaluating R&D subsidy programs. This literature has not reached consensus. For example, while Lerner (2000) finds that SBIR awardees in the first few years of the program grew more than a matched sample, Wallsten (2000) finds that the program crowded out private funding, also using mid-1980s data. Most studies examine non-U.S. R&D programs and come to disparate conclusions, such as Lach (2002), Takalo, Tanayama and Toivanen (2013), and Almus and Czarnitzki (2003).[8]

[6] Aghion, Dewatripont and Stein (2008) present a model describing the challenge of locating basic R&D in private firms. They use scientists' demand for research control rights to demonstrate why much early-stage research must be located in academia.

[7] Financing constraints are a central issue in corporate finance. A debate beginning with Fazzari, Hubbard and Petersen (1988) and Kaplan and Zingales (1997) has for the most part found investment to be sensitive to cash flow shocks (e.g. Lamont 1997, Rauh 2006, Whited and Wu 2006). However, it is difficult to establish that financial constraints *cause* this sensitivity, and there is little evidence on small or private firms (see Hall 2010). Zwick and Mahon (2014) use a tax policy change to find evidence of financing constraints that are more severe for smaller firms. Barrot (2014) shows that financial constraints can impeded entry and competition in the context of trade credit supply. Studies of intangible asset investment under imperfect capital markets include Himmelberg and Petersen (1994), Aghion et al. (2012), Bond, Harhoff and Van Reenen (2005), Brown and Petersen (2009), Hall (1992), Carpenter and Petersen (2002), and Czarnitzki and Hottenrott (2011). See Hall (2010) for discussion of the gaps in the literature on startups and R&D.

[8] Evaluations of R&D subsidies mainly address European programs, with quite disparate findings, including Czarnitzki and Lopes-Bento (2012), Serrano-Velarde (2008), Busom (2000), Duguet (2003), González et al. (2005), González and Pazó (2008), Blasio, Fantino and Pellegrini (2014), and Henningsen et al. (2014). In the U.S., Nemet and Kammen (2007) find little evidence of crowding out in federal energy R&D, but

Much of this literature focuses not on financing constraints as a rationale for R&D subsidies but rather on the extent to which the private sector fails to internalize knowledge spillovers and other positive externalities (Arrow 1962). I do not quantify this public good aspect to the grants, but two findings provide indirect evidence. I find no Phase 1 effect on patent citations, suggesting that proof-of-concept work may not lead to large knowledge spillovers. The grants do, however, seem to help internalize positive externalities from clean energy (Nordhaus 2013). I find the strongest effect in the cleanest sub-sectors, such as solar and wind, and the weakest effects in conventional sub-sectors like natural gas and coal.

The paper is organized as follows. In Section 2, I explain the DOE SBIR setting and the applicant data. Section 3 describes the regression discontinuity design and establishes its validity in my context. Section 4 contains the empirical results on financing and real outcomes. Section 5 uses a signal extraction model to frame how grants might affect investor decisions, and evaluates the model's hypotheses in light of the empirical evidence. I test the robustness of the empirical results in Section 6. Section 7 conducts a return calculation. Section 8 concludes.

2 The Setting: Context & Data Sources

In this section, I first discuss DOE's SBIR program and my applicant dataset. Section 2.2 summarizes the private finance data and matching. Section 2.3 describes data on patenting, revenue, and survival.

2.1 The SBIR Program at the Department of Energy

In the U.S., grants are a significant funding source for high-tech entrepreneurs.[9] The largest single program is the SBIR grant program, which disburses around $2.2 billion each year. Congress first authorized the SBIR program in 1982 to strengthen the U.S. high technology sector and support small firms. Today, 11 federal agencies must allocate 2.7% of their extramural R&D budgets to the SBIR program; the required set-aside will increase to 3.2% in

Popp and Newell (2009) do. Link and Scott (2010) use SBIR Phase 2 awardee survey data to analyze the likelihood of commercialization. To my knowledge, only the working papers by Zhao and Ziedonis (2013) and Bronzini and Iachini (2011) use data on applicants to R&D incentive programs. The former evaluates a Michigan loan program (N=104), and the latter grants to large firms in Northern Italy (N=171). Both programs have private cost sharing, which SBIR does not. Other researchers have used RD to evaluate grants to university researchers, such as Jacob and Lefgren (2011) and Benavente et al. (2012).

[9] A rough estimate suggests that federal and state R&D grants to high-tech new ventures were about $3 billion in 2013, compared total VC investments in the U.S. that year of $29.6 billion (NVCA 2014).

2017 and beyond. Though important in its own right, the SBIR program is also representative of the many targeted subsidy programs for high-tech new ventures at the state level and around the world.

Akin to staged VC funding, the SBIR program has two "Phases." Phase 1 grants fund proof-of-concept work intended to last nine months. Awardees are given the $150,000 in a lump sum (the amount has increased stepwise from $50,000 in 1983). DOE does not monitor how they use the money, but firms must demonstrate progress on their Phase 1 projects to apply for $1 million Phase 2 grants. Phase 2 funds more extensive or later stage demonstrations, and the money is awarded in two lump sums over two years.[10]

There is no required private cost sharing in the SBIR program. Also, the government neither takes equity in the firm nor assumes IP rights. Eligible applicants are for-profit, U.S.-based, and at least 51% American-owned firms with fewer than 500 employees. Although the SBIR grant is non-dilutive, it is not costless. In interviews, 30 VC investors and employees at ten startups described the application and reporting process as onerous. Applying for an SBIR grant can require two months of 1-2 employees working full time.

Each year, DOE officials in technology-specific program offices (e.g. "Solar") develop a series of competitions. A firm applies to a relevant competition, proposing a project that fits within its scope. Examples of competitions include "Solar Powered Water Desalination," and "Improved Recovery Effectiveness In Tar Sands Reservoirs." My empirical strategy compares firms within competitions.

Three external experts from National Labs and universities review applications according to three criteria: 1) Strength of the scientific/technical approach; 2) Ability to carry out the project in a cost effective manner; and 3) Commercialization impact (Oliver 2012). Program officials rank applicants within each competition based on the written expert reviews and their own discretion. These ranks and losing applicant identities are strictly and indefinitely non-public information.[11] Program officials submit ordered lists to an independent, separate DOE SBIR office. The cutoff within each competition is unknown to the program officer when she produces the rankings. The SBIR office determines the competition's number of awards. This cutoff varies across competitions, so one competition may have one awardee while another has four; the average is 1.7. To the best of my knowledge

[10] Phase 2 grants are analyzed in Appendix E. Please find all appendices here: http://scholar.harvard.edu/showell/home. Phase 3 is commercialization of the technology. It is ineligible for SBIR funds except when agencies are purchasing the technology, which does not occur at DOE but is common at the Department of Defense.

[11] It is only in my capacity as an unpaid DOE employee that I am able to use this data. Throughout the paper, specific references to companies will only include winners.

the cutoff is arbitrary.[12] Figure 6 shows that there are no obvious differences among program offices in the average number of awards.[13]

In this study, I use complete data from the two largest applied offices at DOE, Fossil Energy (FE) and Energy Efficiency & Renewable Energy (EERE), which has eight technology-based program offices.[14] Together, EERE and FE awarded $884 million (2012 dollars) in SBIR grants over the course of my data from 1983 to 2013. Appendix D Figure 4 shows all applicants by office and award status. The data include, for each applicant, the company name and address, funded status, grant amount, and award notice date. I have ranking information only since 1995, so my estimation starts in that year.

Table 1 contains summary statistics about the applications and competitions, and Table 2 shows all variables used in estimation. Each competition has on average 9.8 applicants, with a standard deviation of eight. Of the 7,436 applicant firms, 71% applied only once, and a further 14% applied twice. Within my data, seven companies each submitted more than 50 applications. For discussion of "SBIR mills" and the grant effect by the number of awards, see Appendix F.

Despite the presence of "SBIR mills," startups dominate the applicant pool; the firm median age is six years, and many firms are less than a year old.[15] Consistent with this fact, scholars have used SBIR winners as representative samples of high-tech entrepreneurial firms. For example, Hsu (2006) uses a sample of SBIR awardees as a counterfactual for VC-funded startups. Gans and Stern (2003) use survey data on 71 SBIR grantees to test whether capital constraints or appropriability problems explain different performance across sectors.

[12] The number of awards is determined by topic and program budget constraints, recent funding history, office commitments to projects such as large National Laboratory grants, and the overall number of ranked applicants the central SBIR office receives (the number of applicants deemed "fundable"). My understanding of the exogeneity of the cutoff to the ranking comes from conversations with stakeholders in the DOE SBIR program, and from historical email records containing rank submissions. I cannot predict the number of awards in a competition using any observable covariates, and fluctuation in the number of awards does not differ systematically by program office, technology topic, or time.

[13] The average number of applicants per competition by program office is in Appendix D Figure 1. Appendix D Figures 2 and 3 show the number of awards per office and per competition over time.

[14] Besides EERE and FE, the other offices are: Basic Energy Science; Nuclear Energy; Environmental Management; and Electricity Delivery & Energy Reliability. Within EERE, the eight program offices are: Solar Energy Technology, Biomass Program; Fuel Cell Technologies; Geothermal Technology; Wind & Hydropower Technology; Vehicle Technology; Building Technology and Advanced Manufacturing.

[15] Among the 23 solar firms that have ever had an IPO, nine appear in my data; SBIR winners include Sunpower, First Solar, and Evergreen Solar (Cleantech Group i3). Although there is no strict definition of "startup," they must be young, small, and have location-unconstrained growth potential. This is why restaurants, plumbers, and other local small businesses are not startups.

2.2 Private Finance Data

To match as many private financing deals to applicant companies as possible, I combined the ThompsonOne, Preqin, Cleantech Group i3, CrunchBase, and CapitalIQ databases. After matching by name and state, and hand-checking for accuracy, there are 838 firms with at least one private financing deal, of which 683 had at least one VC deal. Summary statistics about the matches are in Appendix D Table 2. Note that my private finance variables include IPOs and post-IPO transactions. I use "private" in the sense of non-government, as opposed to private equity. The matched VC deals by round type over time are in Appendix D Figure 6, and all private finance deals are in Appendix D Figure 7.[16]

In Table 2, VC_i^{Post} is one if the firm ever received VC investment after its first grant award date.[17] This variable includes angel financing, which is qualitatively different from VC, but both target high-growth startups. I use binary indicators (or number of deals in robustness tests) and not dollar amounts for two reasons. First, VCs often report an investment but not the amount to survey firms, so the amount is available for a selected fraction of the deals. Second, there is rarely information about the pre-money valuation or how much the company sought to raise. A VC round of $1 million has a different value for a capital intensive battery company than for a smart phone energy efficiency app.

The variable $Exit_i$ takes a value of 1 if a firm has experienced an IPO or acquisition in the relevant time period. As in much of the literature, I am unable to distinguish acquisitions with high rates of return for investors from acquisitions that are an escape hatch, yielding modest or no returns.[18] The majority of startups fail altogether, so a "selling for parts" exit at least indicates that the human capital or IP were valuable.

2.3 Real Outcome Data

I employ firm patents and a normalized citation metric as proxies for innovation quantity and quality, respectively. The data, from Berkeley's Fung Institute for Engineering Leadership, include all patents filed between 1976 and 2014. I matched non-reissue utility patents to applicant firms, and checked most by hand. Appendix D Table 4 contains summary statistics about the 2,109 firms with at least one patent. The pre- and post- treatment variables use

[16] The paucity of matched deals before 2000 likely reflects the poorer quality of private transaction databases in earlier years and the lower volume of clean energy deals.

[17] For summary statistics on all private finance events and the number of deals, see Appendix D Table 1.

[18] Other papers that use all M&A events as positive exit outcomes include Gompers (1995), Hochberg, Ljungqvist, and Lu (2007), Puri and Zarutskie (2012), and Brander, Egan and Hellman (2008).

the patent application date rather than the issue date, as is standard in the literature.

I do not normalize the patent count by USPTO classification or year because competition fixed effects control for sub-sector and date. For citations, however, I use the normalization from Lerner, Sorensen, and Strömberg (2011). It starts with a patent's forward citation count, which is the number of citations it receives from later patents within a three year window after it was granted. I divide this count by the patent's class-year intensity.[19]

Data on firm survival and achieving revenue (commercialization) were collected by searching the internet for each firm to identify its current or historical status, website, and brief product description. Appendix D Table 3 summarizes the relevant information from this process. Roughly half of the companies in the estimation sample commercialized their technology, which I define as having ever sold their product or service. Less than a quarter are out of business as of May, 2014. The revenue variable is not date-specific relative to the award. Section 4.3 discusses how this limits the interpretation of the RD estimates. Although the real outcome metrics are crude, an advantage is that I have data for each firm in my sample.

3 Empirical Strategy

Regression discontinuity (RD) is a design that estimates a local average treatment effect around the cutoff in a rating variable - in my case the applicant's rank. The critical assumption in RD is that applicants cannot precisely manipulate their rank immediately around the cutoff. My institutional context, where firms are funded in rank order and the cutoff is exogenous to rank, permits a sharp RD comparing firms around the cutoff. As public agencies resist randomizing treatment to evaluate R&D subsidies (unlike new medicines), RD is the most plausibly exogenous variation possible (Jaffe 2002).

More specifically, a valid RD design must satisfy four conditions to be considered a local randomized experiment.[20] First, treatment cannot cause rank. This holds for the DOE SBIR program, as the award happens after ranking. To avoid contamination, I exclude applicants who previously won a grant within EERE/FE. Second, the cutoff must be exogenous to rank, which is true in my setting (Section 2.1). Third, the functional form must be correctly specified, else the estimator will be biased. I perform a goodness-of-fit test and

[19]This intensity is: $\gamma = \frac{\text{Total 3 Year Citations for a Class-Year}}{\text{Total Patents in a Class-Year}}$, where "Total 3 Year Citations for a Class-Year" are the number of citations made within 3 years to all patents in a given class-year.

[20]For more on RD, see Lee and Lemieux (2010).

show that rank is uninformative (Sections 4.1 and 7). Finally, to meet the key assumption that applicants cannot precisely manipulate their rank in the region around the cutoff, all observable factors must be shown to be locally continuous. To establish the necessary weak smoothness (see Hahn et al. 2001), I show continuity of covariates below.

Since the number of applicants and awards varies across competitions, I center the applicant ranks in each competition around zero at the cutoff. The lowest-ranked winner has centered rank $R_i = 1$, and the highest-ranked loser has $R_i = -1$. Each competition that I consider has at least this pair. As I expand the bandwidth, $[-r,\ r]$, I include higher ranked winners and lower ranked losers.[21]

I estimate variants of Equation 1, where Y_i^{Post} is the outcome and dependent variable. The coefficient of interest is τ on an indicator for treatment, and $f(R_{ic})$ is a polynomial controlling for the firm's rank within competition c.[22] The pre-assignment outcome variable is Y_i^{Prev}. I include a full set of dummies for each competition δ_c, which are date-specific. X_i indicates other controls.[23] My estimations use OLS for binary dependent variables, negative binomial for count data, and two-part models for semi-continuous data.[24] Standard errors are robust and clustered by topic-year, to account for correlation in time and sector.

$$\begin{aligned} Y_{ic}^{\text{Post}} &= \alpha + \tau\left[\mathbf{1} \mid R_{ic} > 0\right] + f\left(R_{ic}\right) + \gamma_1 Y_{ic}^{\text{Prev}} + \gamma_2 X_{ic} + \delta_c + \varepsilon_{ic} \\ & \text{where } -r \le R_{ic} \le r \end{aligned} \tag{1}$$

An important data limitation is the discreteness of my rating variable - competitions average ten applicants. Lee and Card (2008) note that discrete rating variables can require

[21] To assess composition issues, I also use percentile ranks and conduct a variety of tests, such as interacting raw rank with the number of awards in a competition.

[22] The standard RD implementation pools the data but allows the function to differ on either side of the cutoff by interacting the rank with treatment and non-treatment (Imbens and Lemieux 2008). However, I potentially have too few points to the right of the cutoff to estimate a control function separately on both sides, so I rely on global polynomials for my primary specification. I show that my results are robust to allowing the slope coefficients to differ.

[23] The RD design does not require conditioning on baseline covariates, but doing so can reduce sampling variability. Lee and Lemieux (2010) advise including the pre-assignment dependent variable as they are usually correlated. Appendix G Table 1 projects rank on observable covariates. Previous non-DOE SBIR awards are the strongest predictor of rank. A one standard deviation increase in previous SBIR wins (the mean is 11.4 and the standard deviation is 38) increases the rank by nearly one unit. Previous VC deals also have a small positive impact. I include these two variables in my primary specifications.

[24] I use OLS for binary outcomes because many of the groups defined by fixed effects (competitions) have no successes (e.g. no subsequent VC). Logit drops the groups without successes. In such situations, Beck (2011) finds that OLS is superior despite his conclusion that logit is usually preferable with binary variables. Also, OLS with a binary variable is common in applied economics, following the arguments in Angrist (2001) that regression does as well as logit in estimating marginal effects and often better with binary treatment variables. My main results are intact with a logit specification (see Section 7).

greater extrapolation of the outcome's conditional expectation at the cutoff. The fundamental econometrics are no different than with a continuous rating variable, however, as extrapolation is required in both cases. Section 7 demonstrates the robustness of my findings to this discreteness by, for example, separately considering competitions with certain numbers of awards.

To determine the appropriate polynomial, I employ Lee and Card's (2008) goodness-of-fit test for RD with discrete covariates, which compares unrestricted and restricted regressions. The former is a projection of the outcome on a full set of dummies for each of K ranks. The latter is a polynomial similar to Equation 1.[25] The null hypothesis is that the unrestricted model does not provide a better fit. If the goodness-of-fit statistic G exceeds its critical value for a certain level of confidence, then we can reject the null and turn to a higher order polynomial. The test results for each outcome metric are in Section 4.

I demonstrate smoothness in observable baseline covariates in three ways: visually, through an RD on baseline covariates, and through differences in means. First, I show at each rank the means of baseline covariates, most importantly the pre-assignment outcome variables VC investment (Figure 1A), patenting (Figure 2A), exit (Figure 5A), and all private finance (Appendix D Figure 8). For ease of comparison, these are shown adjacent to the post-treatment variables. Four additional covariates are in Appendix H Figure 1; average age as well as the probability a firm is located in a major metro area, is woman owned, and is minority owned. In none of the eight figures is there any discontinuity around the cutoff visible, nor is there any trend in rank. A ninth covariate is the exception: previous non-DOE SBIR wins (Appendix H Figure 2). Rank is clearly increasing in previous wins, but again there is no discontinuity around the cutoff.[26]

Second, I try to detect a discontinuity in the outcome predicted by the baseline covariates, following Card, Chetty and Weber (2007) and Imbens and Lemieux (2008). I use an OLS regression of the outcome of interest, Y_{ic}^{Post}, on baseline covariates and competition dummies to obtain a weighted average of the covariates by relevance to the outcome:

$$Y_{ic}^{\text{Post}} = \alpha + X_i \phi + \delta_c + \varepsilon_{ic} \tag{2}$$

For each applicant I then use the estimated coefficient vector to predict the probability of

[25]The goodness-of-fit statistic is: $G \equiv \frac{(ESS_{Restr.} - ESS_{Unrestr.})/(K-P)}{ESS_{Unrestr.}(N-K)}$, where ESS is the error sum of squares from regression, N is the number of observations, and P is the number of parameters in the restricted regression. G takes an F-distribution $F(K-P,\ N-K)$.

[26]See Appendix F for analysis of multiple SBIR wins.

subsequent VC financing: $\hat{Y}_{ic}^{\text{Post}} = \hat{\alpha} + X_i\hat{\phi} + \hat{\delta}_c$. I average the probabilities for each rank and plot them in Appendix H Figure 3. There is no obvious discontinuity around the cutoff, in striking contrast to the actual outcome in Figure 1B.

Third, I conduct a t-test for matched pair differences of means in baseline covariates immediately around the cutoff, as in Kerr et al. (2014). The null hypothesis is that the mean of the covariate for $R_i = -1$ applicants is the same as for $R_i = 1$ applicants: $H_o = \bar{X}_1 - \bar{X}_{-1} = 0$. The first alternative hypothesis is a two-tailed test: $H_1 = \bar{X}_1 - \bar{X}_{-1} \neq 0$. The second is a one-tailed test: $H_2 = \bar{X}_1 - \bar{X}_{-1} > 0$ (this is most relevant for the pre-application covariates). The results are in Appendix G Table 22. The two-tailed tests cannot reject the null at the 10% level for any covariate. The one-tailed tests find a significant difference only for previous citations (at the 10% level). However, adding or removing these covariates from the regression has essentially no effect on my results. I also estimate whether treatment can predict each covariate individually. In Appendix G Table 21, I regress each the 10 baseline covariates on treatment. None of the treatment effects have any significance.

Program officials observe more data than the econometrician, so it is impossible to fully test the assumption of no sorting on observables in the neighborhood of the cutoff. Nonetheless, this preponderance of evidence suggests the RD design is valid.

4 The Grant Impact on Firm Outcomes

I find strong effects of the grant on financial and real outcomes, summarized in Table 3. A Phase 1 award nearly doubles a firm's probability of venture capital finance and leads to almost three times as many patents. It also increases a firm's likelihood of reaching revenue and of achieving a liquidation event. The effects are consistently stronger for younger, more inexperienced firms. In contrast with the large Phase 1 impact, Phase 2 has no effect on any outcome other than patents, where it has a much weaker effect than Phase 1.

I begin with the long term effect of the Phase 1 grant on VC. Subsequent sections use variation in firm characteristics (4.1.1) and over time (4.1.2 and 4.1.3) to reinforce the case that the grant eases financial constraints. I test for reallocation of capital within competitions in Section 4.1.4. Section 4.1.5 evaluates the Phase 2 grant. Section 4.2 assesses the effect on patents and patent citations, considering heterogeneity across firms (4.2.1), the Phase 2 effect (4.2.2) and the relationship of VC finance to patenting (4.2.3). Finally, Section 4.3 examines commercialization, exit, and survival.

4.1 The Grant Impact on Venture Capital Investment

Startups' typically have little or no tangible collateral, so they often cannot initially access debt finance. VC is their main source of external capital outside of partnering with a larger corporation (Hall and Woodward 2007). VC accomplishes two important goals as an outcome metric. First, it tests whether the grants mobilize or crowd out private investment. Second, observing subsequent VC investment indicates that the company presents a privately profitable investment opportunity.

VC investment is not only a financial outcome, but is also as a good early-stage proxy for market success in a context where outcome data are difficult to collect. The literature has established that venture capitalists are important intermediaries in the U.S. innovation system.[27] They select innovative firms and bring new technologies to market quickly (Hellmann and Puri 2000, Sorenson 2007, Engel and Keilbach 2007). The VC commitment also makes debt finance easier to obtain (Hochberg, Serrano and Ziedonis 2014). VCs further provide non-monetary resources, such as intensive monitoring, improved governance, legal services, and networking. Chemmanur et al. (2011) find that VC-backed manufacturing firms have higher productivity prior to receiving VC finance, but that after controlling for this screening, VC-backed firms also subsequently experience faster growth. Kortum and Lerner (2000) exploit the 1979 pension fund policy shift and find that $1 of VC money produces 3-4 times more patents than $1 of corporate R&D. Further, DOE officials consider mobilizing private investment to be an important goal.

Visual evidence for a grant treatment effect on VC is in Figure 1B. The probability of subsequent VC jumps from about 10% to 20% around the grant cutoff. Table 4 contains this difference in regression form. The dependent variable (VC_i^{Post}) is one if a firm ever subsequently received VC investment, and zero if it did not. Column I finds that an award increases the probability of subsequent venture funding by 9.8 percentage points (hereafter pp), significant at the 1% level, with the narrowest bandwidth possible of one rank on either side of the cutoff. Subsequent columns find effects between 7.2 and 14 pp using larger bandwidths of two, three, and all my data.[28] Note that the overall likelihood of receiving VC after the grant is 10.9%; among losers it is 9.4%, and among winners it is 21.3% (with

[27]The U.S. VC industry has grown dramatically since its origins in the 1960s. Over the past decade it has invested $20-$30 billion annually in portfolio companies, up from about $8 billion in 1995 (NVCA 2014). VC firms invested between $4 and $7 billion annually in U.S. clean energy in recent years (see Appendix D Figure 5).

[28]Appendix G Figure 1 depicts the predictive margins. It shows the conditional expectation of VC^{Post} by rank, calculated at the mean of all the other independent variables. I use a linear rank specification around the cutoff with BW=all.

the bandwidth=All specification). I control for centered rank linearly with a bandwidth of two $\left(f\left(R_{ic}\right)=\beta_1 R_{ic}\right)$, and quadratically with wider bandwidths $\left(f\left(R_{ic}\right)=\beta_1 R_{ic}+\beta_2 R_{ic}^2\right)$. My preferred estimate is 9 pp (column II).[29]

The models with and without rank controls in Table 4 yield fairly similar coefficients. The ranks do not contain much information about an applicant's chances of VC financing. The Lee and Card (2008) goodness-of-fit test reveals that once I control for award, no function is too restrictive.[30] We might worry that information in the raw rank is lost when I center the ranks around the cutoff. A firm with a centered rank of two in a competition with two awards might be of different quality than in a competition with four awards. I create percentile ranks to address this possibility. Regressions controlling for quintiles in rank within a competition, instead of centered rank, are in Table 5. The coefficients on treatment range from 9.3 to 10.1 pp, all significant at the 1% level.[31]

The grant effect on VC happens quickly. This confirms that the long term effect above is indeed due to the grant, and also tells us that whatever mechanism explains the grant effect must act rapidly. Within one year of the award a grantee is 5.8 pp more likely than a loser to receive VC, significant at the 1% level, (Table 8 column 1). This is more than half the total effect. Subsequent columns show the cumulative effect over time; for example, within two years the effect is 7.5 pp and within four years it is 8.2 pp, both also significant at the 1% level.

When I include all private financing events, such as IPOs, acquisitions, and debt, I find a slightly larger effect of about 12 pp. The probability of funding jumps from 12% to 26% around the cutoff, shown visually in Appendix D Figures 8 and 9. Appendix G Tables 4-7 replicate the VC findings with all private financing (PF_i^{Post}) as the dependent variable, and find analogous results.

[29]Note that in specifications with bandwidth "all," the data are not symmetric around the cutoff. In Appendix G Table 2 I use quadratic specifications that do not restrict the slope to be the same on either side. The coefficients jump to 16.7 and 23.2 pp with BW=2 and BW=3, but return to 11.5 pp with BW=all. Compared with Table 4, the standard error increases when rank is added, indicating that rank is correlated with treatment. It is difficult to distinguish the effect of winning from the rank because of the coarseness of my rating variable. The confidence interval implied by the standard errors from Appendix G Table 2 include my preferred estimate of 9 pp. Any bias from excluding rank is downward rather than upward, which is reassuring if the concern is overstating the result.

[30]G-values from the goodness-of-fit test are tiny. With no control for rank, G =0.000028, while the critical value above which I could reject the null even with 15% confidence is 1.27. In F-tests for regressions with linear and quadratic rank, I find that the G-value remains miniscule.

[31]I find the same result using quartile ranks (Appendix G Table 3). See Section 7 for a rich array of robustness tests, including regressions estimated on subsamples with specific numbers of awards, and with dummies for the raw rank interacted with the number of awards.

4.1.1 Variation in the Effect Across Firm Age and Sector

If the grants ease financing constraints, then the estimated effect ought to be larger for more constrained firms. In this section and the next, I examine variation in the effect across firm characteristics and over time. Since these variables are not randomly assigned, the analysis is necessarily more speculative than the affirmative conclusions in the main result above.

First, young firms tend to be more financially constrained - there is less information available about them, and they generally have fewer assets (e.g. Brown, Fazzari and Petersen 2009, Whited and Wu 2006). Indeed, young firms experience much stronger grant treatment effects. Table 6 Column I includes only firms less than three years old and finds that a grant increases the likelihood of subsequent VC by 17 pp (significant at the 5% level), while for firms older than three the effect is 9.2 pp (column II). Similarly, the effect for firms less than ten years old is 14 pp, significant at the 1% level, but for firms ten years or older, it is only 4.7 pp (columns IV and V). I jointly estimate the young and old regressions by fully interacting the variables, including fixed effects, with dummies for age group. The coefficient on the difference between the treatment effect for firms younger and older than nine is 9.3 pp, significant at the 5% level (column VI).[32]

This result is in keeping with the model in Acemoglu et al. (2013), where R&D subsidies to entrants increase welfare, but subsidies to incumbents decrease welfare. Policymakers might consider targeting young firms for grants, as not only do they experience the largest grant effects, but young companies generate greater innovation and growth than simply small companies (Evans 1987, Calvo 2006).

Immature technologies without well-developed markets or supply chains, such as solar and geothermal, are riskier investments than incumbent technologies, such as coal and natural gas. I create a binary variable, $Immature_i$, which is one if the sector is solar, wind, geothermal, fuel cells, carbon capture and storage, biomass, or hydro/wave/tidal; and zero if the sector is oil, gas, coal, biofuels, or vehicles/motors/engines.[33] More ambiguous sectors are excluded. The grant effect is 18 pp for immature sectors, but only 7.2 pp for mature sectors (Table 6 columns X-XI). Both coefficients and their difference (column XII) are significant at conventional levels.[34]

Separate regressions for each clean energy technology (Table 7) confirm that the grants

[32]This is equivalent to an F-test for equality of the coefficients in the separate regressions.

[33]Most electric vehicle and hydrogen car competitions are classified as batteries or fuel cells. The sector categorizations are based on the topic to which the firm applied.

[34]The degree to which some of these sectors are mature may have changed over time, so Appendix G Table 8 considers the sample from 2007, and finds roughly the same results.

are most beneficial for emerging energy generation technologies. For example, a grant makes a solar company 25 pp more likely to get subsequent VC investment, increasing the probability from roughly 11% for losers to 35% for awardees. For wind companies, the grant increases the probability of subsequent VC from about 5% to 16%. There is no correlation between the grant effect on VC in a sector and that sector's propensity to receive VC.[35] These emerging energy sub-sectors have positive externalities from reduced pollution and greenhouse gases. Mitigating climate change does not enter most private sector return calculations, but it is one of DOE's central objectives. My results indicate that subsidies have the greatest impact when awarded to clean energy generation technologies, rather than to projects that improve efficiency in a mature sector.

4.1.2 Variation in the Effect over Time

The results thus far pool all years between 1995 and 2013, but the effect has actually changed somewhat over time. The bottom panel of Table 8 divides the sample into four five-year periods. Between 1995 and 1999, the effect is 7.6 pp. It drops to 4.7 pp between 2000 and 2004, perhaps because VC firms were focused on internet startups at the beginning of the period, then dramatically reduced investing when the internet bubble collapsed. The effect returns to 7 pp in 2005-2009. The strongest effect is between 2009 and 2013 at 19 pp. I focus on the ARRA years of 2009-2011, when DOE funding was unusually high, in columns XI and XII. Some investors I interviewed believed that in this period there was "too much government money chasing too few good projects." But the estimated grant effect is 13 pp for the whole Stimulus period. Despite a large spike in applicants in 2009, limiting the sample to that year yields the same effect as the whole sample.

The economic environment may explain these across-time period differences. Unlike large firms, startups cannot use cash reserves to smooth R&D investment over time and have little control over when their invention requires an infusion of capital (Himmelberg and Petersen 1994). If the grants mitigate entrepreneurs' financing constraints, they should be more powerful in lean times when external financing is more difficult to attain.

Tobin's Q, the ratio of a firm's market value to its book value, is widely employed in the literature to measure investment opportunities (e.g. Stein 2003, Gompers, Lerner and Scharfstein 2005). Q can also be interpreted as an indicator of financing availability, as

[35] Without controlling for treatment, I project subsequent VC on sector dummies in Appendix G Table 9. Vehicles/batteries and advanced materials are among the most likely to receive VC, but have weak treatment effects. Meanwhile, solar and efficiency are relatively likely likely to receive VC and also have strong treatment effects. Wind is unlikely to receive VC, but the grant has a dramatic impact.

in Baker, Stein and Wurgler (2003). I hypothesize that in low-Q environments firms face greater difficulty accessing external finance, making the grant more useful. But the grant could act pro-cyclically if, say, there are always more worthy startups seeking funding than willing investors, but the supply of entrepreneurs is positively elastic to hot markets.

My simplified measure of Q follows Kaplan and Zingales (2007) and Gompers, Ishii and Metrick (2003).[36] I use NAICS codes to identify companies in the clean energy sector, and calculate Q annually by company.[37] I interact the treatment variable with median sector Q_{t+1} (Q in the four quarters following the award), which I demean so that the coefficient on treatment alone reflects the impact of the grant at mean Q. The results, in Table 9, show that the grant effect decreases significantly as Q increases. A one standard deviation increase in Q is associated with a 4 pp decrease in the grant effect. I also divide the years into periods of low and high Q, and find that the difference in the effect between periods is 9.2 pp, significant at the 5% level (column III of Appendix G Table 11).

The private sector's disinterest in funding startups when industry Q is low makes sense under both Q interpretations: low Q implies poor investment opportunities or that the market undervalues the investment opportunities. Under the investment opportunities interpretation, VC firms - who are relatively unconstrained and thus Q-sensitive - should invest less in clean energy startups when industry Q is low. Market failure occurs because startups' financing constraints disrupt the linkage between Q and investment. Worthwhile startups with the bad luck (or poor choice) to commercialize their invention when industry Q is low cannot substitute other resources for venture funding. They find the grant more valuable.

A different angle on access to finance is VC investment in portfolio companies, which is quite volatile (Nanda and Rhodes-Kropf 2012, Jeng and Wells 2000). This volatility may reflect irrational herding, as in Scharfstein and Stein (2000), or it may reflect shocks to investment opportunities, as in Gompers et al. (2008). I expect that when VC availability is high, firms are less financially constrained, so the grant effect is diluted.

The right panel of Table 9 explores how the grant effect varies with the total number of

[36]Q is calculated using the equation below, where BV is book value, MV is market value (price times shares outstanding), and DT is balance sheet deferred taxes. Data is from Compustat via Wharton Research Data Services. The book value is in fiscal year t and the common stock value is at the end of calendar year t.

$$Q_t = \frac{MV_t^{Assets}}{BV_t^{Assets}} = \frac{BV_t^{Assets} + MV_t^{\text{CommonStock}} - (BV_t^{\text{CommonStock}} + DT)}{BV_t^{Assets}}$$

[37]The sector median is plotted in Appendix D Figure 10, and summary statistics are in Appendix D Table 6. See Appendix D Table 5 for NAICS codes that define the clean energy sector.

U.S. VC deals over the eight quarters following the grant.[38] The coefficient on the interaction between treatment and number of deals is negative and significant at the 5% level. It implies that a one standard deviation increase in deal flow is associated with a 5.3 pp decrease in the grant's effect. The alternative specification finds that the difference in the treatment effect between high and low deal flow periods is 6.6 pp, significant at the 10% level (column VI of Appendix G Table 12). When I perform this exercise within only one year of the grant ($\#VC_{t+1}$), I find a smaller and insignificant difference.

It seems that a grant is more valuable in times of low Tobin's Q and low VC availability. This counter-cyclicality reinforces the conclusion that energy startups face severe financing constraints, like the across-period findings in Fazzari, Hubbard and Petersen (1988). Yet this heterogeneity analysis is an exercise in theory-motivated correlations, so other economic conditions may drive the relationships.[39] However, my counter-cyclical finding accords with Tian and Wang's (2014) conclusion that being financed by a failure-tolerant VC is more important for innovation when ventures are founded in recessions. Related research finds that R&D investment is pro-cyclical, declining in recessions due to financing constraints. This body of work includes Aghion et al. (2012), Campello, Graham, and Harvey (2010), and Ouyang (2011).

4.1.3 Testing for Spillovers

Thus far I have assumed that awardees do not affect losing applicants. But a grant might increase an awardee's chance of VC by *decreasing* the losers' chance. In this section I test whether my RD estimates reflect negative spillovers. Unfortunately, I cannot test whether capital is reallocated from non-applicant firms to winning firms, or whether total VC investment in clean energy changes as a result of the grant program.

To test for reallocation of capital within the applicant pool, I conduct two tests. First, I ask whether the likelihood of a losing firm obtaining VC varies with the number of winners in the competition. Recall that within a competition firms are doing very similar activities - they are in the same narrowly defined sub-sector. Also recall that the number of awards in a competition is unrelated to the technology type, program office, time period, and ranking process. Therefore, if there are negative spillovers from winners to losers, these should be more intense when there are multiple winners in the competition. I regress the outcome on the subset of losers and include in separate models dummies for having either more than one,

[38] I use data from ThompsonOne (Appendix D Figure 10, summarized in Appendix D Table 6).

[39] I also tested the correlation of the grant effect with the business cycle using NBER recessions, but found no significant effects.

or more than two, awards in the competition. I find that these dummies have no predictive power, suggesting that spillovers do not explain the main effect (see Appendix G Table 31).

Second, I exploit the robust finding in the literature that VC firms typically invest in geographic proximity to their offices, and indeed in firms located in their city (Sorenson and Stuart 2001, Samila and Sorenson 2011). Chen et al. (2010) point out that distant monitoring is costly, which is one reason why portfolio companies typically have at least one investor in the same metro region. Cumming and Dai (2010) also find strong local bias in VC investments. They calculate the average distance between a company and its venture investor at less than 200 miles since 1998.

Geographically close firms competing for an SBIR grant are much more likely than firms far away from one another to also be competing for investment from the same VC firms. Therefore, if the grant causes reallocation, I should observe a larger treatment effect in competitions where winners and losers are from the same area. My first test identifies firms within competitions from the same metropolitan statistical area (MSA) and from different MSAs. The Phase 1 grant effect is slightly higher when competing firms are from the same MSA, at 11.9 pp compared to 9.9 pp (Table 6 columns VII and VIII). Column IX shows that the difference between these coefficients is insignificant.

In the geographical analysis (Appendix B), a second test examines specific within-region effects. I find that the grants are consistently most useful to firms in the San Francisco (SF) region, regardless of whether they are competing with firms locally or far away. Hochberg, Ljungqvist, and Lu (2007) also find that the benefits of early-stage resources are amplified in SF. Otherwise, the effect when competing firms are from the same MSA and when they are from different MSAs is not systematically different. Therefore, reallocation does not seem drive the main findings, although I cannot rule it out.

The grant effect is, however, systematically larger not just for firms from SF, but more broadly when the winner is located in a city with greater VC investment per unit of city output. I demonstrate this in Appendix B, the geographical analysis. The literature has found that firms, particularly startups, are *less* financially constrained in areas with deeper capital markets (Rajan and Zingales 1998, Berkowitz and White 2004). My other results point to the grant having a larger effect for firms that are *more* financially constrained. This is a puzzle.

One possible solution comes from Lerner (2000), who finds that SBIR awards stimulate firm growth only in regions with high venture investing, a more extreme result than mine. Lerner suggests that perhaps congressional efforts distort award allocation across regions. In

Appendix C I use delegation congressional power in the House and Senate to predict spending to a jurisdiction. The regressions reveal a statistically significant positive effect of seniority on committees with relevant authority in both chambers. However, the effect is very small, which is not surprising since these awards are small, dispersed, and bureaucratized. While its direction supports Lerner's hypothesis, it seems unable to explain the much larger grant effect in cities with greater VC intensity. Lerner also hypothesizes that long-lived research firms, which win many awards and do not seek VC finance, could be disproportionately located in areas without high venture activity. This is *not* the case in my data. The correlation of all-government SBIR awards (i.e. the degree to which a firm is an "SBIR mill") and local VC intensity is 0.01. Of the 59 firms with at least 50 all-government SBIR awards, 20% are in Boston, 10% are in LA, and 11% are in SF.

What, then, explains the regional variation? Larger knowledge spillovers may play a role. High-tech employees in Silicon Valley exhibit extreme inter-firm labor mobility (Saxenian 1994, Fallick, Fleischman and Rebitzer 2006). Rapid job-hopping can increase agglomeration economies, but it imposes costs on employers who must invest in - and expose trade secrets to - fleeting human capital. Greater spillovers from R&D investment in high-tech clusters could make the grant more valuable for startups in these areas. A second factor could be that regions with high VC per unit output have more intense competition for venture finance.

4.1.4 Phase 2 Grant Impact on VC

Roughly a year after receiving a $150,000 Phase 1 award, a firm may apply for a $1 million Phase 2 grant. Successful applicants typically receive their Phase 2 money nearly two years after the Phase 1 award. In Appendix E, I analyze the Phase 2 grant effect in depth. Here, I summarize my results and their policy relevance.

The Phase 2 grant has no consistently positive effect on subsequent VC. RD estimations using the DOE ranking of Phase 2 applicants (a subset of Phase 1 winners) produce small, positive, but imprecise coefficients. When I jointly estimate the Phase 1 and 2 effects, shown in Table 10, I find the same robust Phase 1 effects, but coefficients on Phase 2 range from -4.2 pp to -0.003 pp. These coefficients have only slightly smaller standard errors than when I estimate Phase 2 alone. While Phase 2 may be useful for some firms, it is not for others. The true average effect is almost certainly smaller than Phase 1, if not negative. I find no heterogeneity across firm age, sector or over time; the coefficients are always small or negative, and insignificant.

One reason for this Phase 2 finding is adverse selection among Phase 1 winners in the decision to apply to Phase 2. Among Phase 1 winners, 37% *did not apply for Phase 2*. Of these non-applyers, 19% received VC investment within two years of their initial award. This is only 9% for firms who applied and lost Phase 2, and 8% for firms who applied and won. From a different angle, 55% of firms who receive VC within two years of the Phase 1 grant do not apply for Phase 2. Apparently, firms do not apply for Phase 1 - and VC firms do not fund Phase 1 winners - because of the Phase 2 expected value.

In interviews, grantees told me that the grant application and reporting processes are so onerous that once they receive external private finance, it is often not worthwhile to apply for additional government funding. Similarly, Gans and Stern (2003) hypothesize that private funding is preferred to SBIR funding. Startup Oscilla Power, introduced above, did win a Phase 2 grant. CEO Shendure said that the $1 million was significant relative to what the firm sought to raise from private sources. Had Oscilla raised a $10 million VC round, he added, applying to Phase 2 may not have been worthwhile.

Extremely high discount rates could help explain why firms do not find applying to Phase 2 worthwhile. It may be that the value of the time required to apply exceeds the expected value of the $1 million Phase 2 grant. Note that roughly 40% of Phase 2 applicants win, and the Phase 2 money is split into two equal disbursements, one in the following year, and one two years after applying. At the seed stage, a VC's required rate of return is typically at least 50%, and as high as 80% (Sahlman and Scherlis 2009). If the entrepreneur uses an 80% discount rate to value his time, then if the application cost exceeds $172,000, it would not be worthwhile to apply.[40] My interviews suggest that the application cost solely in employee time is one to two full months, apart from any consulting or legal costs the firm may incur. High-tech, early stage startups often place a very high value on their time, so in conjunction with a high discount rate, it is plausible that among Phase 1 winners, high quality startups seeking venture finance tend not to apply for Phase 2.

The SBIR program spends vastly more on Phase 2 than Phase 1, so the absence of a strong Phase 2 effect is worrisome from a policy perspective. At the high end of the confidence intervals, the impact of Phase 2 is still much weaker per public dollar than Phase 1. For example, suppose that the true effect of Phase 2 on the likelihood of subsequent VC is 12 pp, which is the highest end of the estimates' 95% confidence intervals. Then the effect of Phase 1 per grant dollar is six times that of Phase 2. Consider the following thought experiment. In 2012 DOE spent $111.9 million on 111 Phase 2 grants and $38.3 million on

[40] Discounted Present Value$= .4\left[\frac{500{,}000}{(1+\delta)^1}+\frac{500{,}000}{(1+\delta)^2}\right]$

257 Phase 1 grants. If all the Phase 2 money were reallocated to Phase 1, DOE could have provided 750 additional firms with Phase 1 grants, increasing by a factor of at least 2.5 the program's impact on the probability of additional VC funding.

4.2 The Grant Impact on Patents and Patent Citations

I now turn to the grant's impact on real outcomes, starting with the best available proxy for innovation: patenting. Patents are only one way that firms protect IP, and they have an ambiguous relationship with technological progress (e.g. Arora, Ceccagnoli and Cohen 2008, Cohen, Nelson and Walsh 2000). Nonetheless, they are positively associated with economic value creation and stock market returns (Hall, Jaffe, and Trajtenberg 2005, Eaton and Kortum 1999). As explained in Section 2.4, I use raw patent counts to measure the quantity of innovation and a normalized 3-year forward citation metric to measure the quality.

A Phase 1 grant leads to at least one additional patent within three years of the Phase 1 award, depicted in Figure 2B.[41] The mean number of patents within three years of the grant is 0.79; among losers it is 0.57, and among winners it is 2.2 (with the bandwidth=All specification). Table 11 reports the results of negative binomial regressions with quadratic rank controls.[42] The table reports Poisson coefficients, but in the text I exponentiate to give incident rate ratios (IRR).[43] The award causes 2.7-2.9 times more patents at bandwidths of one, two and three firms around the cutoff, a large effect. The sample mean is 0.92 patents. My preferred specification is an IRR of 2.7 (columns I and V). There is no information in rank about subsequent patenting, but in contrast to the earlier results the coefficients on treatment decline somewhat when I remove rank controls (columns II, IV and VI).

Two issues with this result bear mention. The literature finds investment in R&D and patenting to occur simultaneously (Pakes 1985, Hall, Griliches and Hausman 1986; Gurmu and Pérez-Sebastián 2008). However, in my setting firms might plausibly conduct the key research prior to the award and file patent applications after winning. Second, the result

[41] I find no statistically significant effect of the grant on long-term patenting (all subsequent patents).

[42] For patenting, the Pearson goodness-of-fit χ^2 suggests that the data are excessively dispersed for the Poisson regression model, so I rely on the negative binomial distribution. I also tried log transformations of the patent and citation metrics, as well as a binary variable for positive patenting/citations. The former provided a similar effect to that shown here, and the latter did not yield effects with statistical significance.

[43] Poisson regression models the log of the expected count. Coefficients indicate, for a one unit change in the covariate, the difference in the logs of expected counts. If λ is the Poisson rate (the number of patents), the model is $\log(\lambda) = \alpha + \tau [\mathbf{1} \mid R_{ic} > 0]$, where covariates other than treatment are omitted. We can write $\tau = \log(\lambda_{R_{ic}>0}) - \log(\lambda_{R_{ic}<0}) = \log\left(\frac{\lambda_{R_{ic}>0}}{\lambda_{R_{ic}<0}}\right)$. Exponentiating the coefficient τ gives the incidence rate ratio (IRR). (This term comes from interpreting the patent count as a rate.) The IRR tells us how many times more patents awardees are expected to have compared to losers.

becomes less consistent when the control function is estimated separately around the cutoff (Appendix G Table 20).

To evaluate the impact on patent citations I use a two-part model, because it would be incorrect to assume normality of the errors for semicontinuous data (Duan et al. 1983, Mullahy 1986).[44] I find no short or long term effect of the Phase 1 grant on the citation metric.

4.2.1 Heterogeneity in the Effect Across Firm Characteristics

Young firms have fewer internal resources and their R&D investment is likely more affected by capital market imperfections (Hall 2008). The middle panel of Table 12 shows that the grant effect on short-term patenting falls dramatically and loses all significance for older firms. The IRR is a staggering 12 for firms no more than two years old, significant at the 1% level (column I), whereas the IRR is only 1.75 for firms more than two years old, and is highly imprecise. For firms less than 10 years old, the IRR is 4.5, whereas for firms older than 10, it is 0.62 - a negative effect - and insignificant (columns III and IV). To my knowledge this is the first direct empirical evidence that young privately held firms face greater R&D investment financing constraints than older private firms, supporting the findings on public firms in Brown, Fazzari and Petersen (2009).

As with age, we might think there is more information available about firms with patents. Hsu and Ziedonis (2008) and Conti, Thursby and Thursby (2013) show that patents improve entrepreneurs' access to finance by signaling potential investors about a firm's quality. Patents may also serve as collateral, as in Mann (2014) and Hochberg, Serrano and Ziedonis (2014). The latter paper finds that among VC-backed startups with available patents available, 36% used the patents to secure loans. The middle panel of Table 12 shows that the treatment effect declines when firms have previous patents: with no patents, the grant leads a firm to produce 3.3 times more patents than it would otherwise, significant at the 1% level. With at least one patent, the IRR is 2.7. More experienced, later stage firms who may have better access to debt finance seem to benefit less from the grants.

Last, the noisiness in the patent data (note the large confidence intervals in Figures 4 and 5) may reflect the wide variation in propensity to patent across technologies (Scherer 1983, Brouwer and Kleinknecht 1999). I create an indicator for high propensity to patent

[44]The first stage models zero versus positive citations (I use logit), and a second stage models observations with positive citations linearly assuming a log-normal distribution for the citations. The two-part model is preferred to the Tobit model, in which the same stochastic process arbitrarily censored from below determines both zero and the positive outcomes. The Tobit model nonetheless gives similar qualitative results.

from the USPTO (2012) patent intensity estimations.[45] In high propensity industries, a grantee produces 8.1 times as many patents as a loser, significant at the 1% level (Table 12 column VII). In contrast, the IRR is only 2.7, significant at the 10% level, in low propensity industries (Table 12 column VIII).[46]

4.2.2 Phase 2 Grant Impact on Patents

In contrast to the financing results, I do find a positive effect of the Phase 2 grant on patenting *and* patent citations. The IRR for the Phase 2 effect on the number of patents is 1.5, half the Phase 1 effect (and thus much smaller on a per grant dollar basis). The average patents for this sample is 2.2. The two-part model for citations finds that the odds of positive citations for Phase 2 grantees are 85% higher than the odds for non-grantees.[47] The sample mean probability of positive subsequent citations is 0.31, so the odds (probability of positive citations divided by probability of no citations) are 0.44. The second stage, a regression within observations with positive citations, finds small and insignificant coefficients. For tables, see Appendix E.

The Phase 2 grant acts on the extensive margin of innovation quality, but not the intensive margin. I also find that among firms with at least one previous DOE SBIR win, the Phase 2 grant has no measurable effect on either patents or citations. A policy implication is that if the government's objective is to generate R&D, measured by patents and more highly cited patents, then Phase 2 awards are beneficial when awarded to firms without previous patenting or citation histories.

4.2.3 Relationship of VC Finance to Patents

In light of the literature on the benefits of VC finance, I am not surprised to see a large positive coefficient on previous VC finance in the regressions with patents as the dependent variable. I explore the relationship between VC and patenting further using subsets of the data unaffected by the grant: firms prior to application, and firms that lose Phase 1. I find that VC finance is associated in both groups with more patents and higher quality patents,

[45] These are based on patents per 1,000 jobs in an industry. The indicator takes a value of 1 if the firm is in one of the following sectors: Smart Grid, Sensors & Power Converters, Advanced Materials, Solar, or Batteries, and 0 otherwise.

[46] For all three heterogeneity analyses in Table 12, I am unable to estimate difference equations due to non-convergence of the maximum likelihood function. Similarly, I cannot separately estimate regressions for each technology (sub-sector) because the sample sizes are too small for the negative binomial model.

[47] Logit coefficients give the change in the log odds of the outcome for a one unit increase in the predictor variable. This odds ratio is calculates as $OR = e^{\beta}$, where β is the logit coefficient.

shown in the top panel of Table 13. For example, prior to the grant application firms with VC finance have 2.6 times as many patents as firms without VC finance (column I). With citations, I find the inverse of the Phase 2 effect. Along the extensive margin, the odds of having positive citations is just slightly larger if a firm has VC finance (logit in column IIa). The regression part reveals that conditional on having patent citations, VC financing increases by 12 the number of citations (relative to a mean of 11.8). I observe essentially the same pattern when I consider only Phase 1 losers, in columns III and IV.

This positive impact of VC on patents raises the concern that the estimated grant effect on patents may indirectly capture VC investment after the award. Rather than the grant funding useful R&D work, the grant might simply enable VC finance, which in turn leads to patents. However, I find that among firms with no VC investment prior to their grant application and no VC investment within three years of applying, the grant effect on patents within three years remains large and robust (bottom panel of Table 13). So the grant and VC finance *both* induce patents.

Thus the patent estimates imply quick use of plausibly exogenous cash for R&D, offering an alternative to the corporate finance estimates of R&D sensitivity to cash flow shocks. The ideal experiment observes whether firms invest exogenous cash in R&D, in which case costly external finance must have prevented the firm from exploiting existing profitable investment opportunities. Empirical work typically uses investment demand equations with adjustment costs, and although studies have established that R&D is rarely financed with debt, it has been difficult to definitively identify that financial constraints cause R&D cash flow sensitivity (see Hall 2010). Here I find that profitable R&D investment would not occur in the absence of a subsidy, contributing to the body of work arguing that financial constraints inhibit investment, especially for smaller firms, such as Li (2011), Faulkender and Petersen (2012), and Zwick and Mahon (2014).

4.3 The Grant Impact on Revenue, Survival & Exit

My final outcome metrics are binary variables for achieving revenue, survival, and exit (IPO or acquisition). As with financing, I find that rank has no predictive power over revenue, survival, or exit, so my preferred specifications, reported in Table 14, omit rank controls.[48]

Visual evidence for an increase in commercialization probability around the cutoff is

[48] The G-value from the goodness-of-fit test with no control for rank is 0.0001, orders of magnitude less than the critical value of 1.47 with 5% confidence. Appendix H Table 5 suggests that there are no major discontinuities besides the award cutoff. Specifications with rank controls are in Appendix G Tables 13-18.

in Figure 3, and the top panel of Table 14 shows the regression results. A Phase 1 grant increases a firm's probability of commercialization by roughly 11 pp, from around 52% to 63%. Unfortunately, I cannot center the commercialization variable around the application date, so a firm may have reached revenue before it applied. However, if the assumptions underlying the RD are sound, this probability should be the same for firms on either side of the cutoff. The magnitude of the estimated effect is not interpretable as a direct grant effect, but offers insight into whether there is an impact.

The majority of firms survive through 2014, depicted in Figure 4. Only about 23% are known to have gone out of business, been acquired, or declared bankruptcy. Visually, there is a decline in the survival probability for losers as the cutoff approaches, and then a jump from around 70% to 85% survival. The regression results (middle panel of Table 14) yield coefficients of about 4 pp, but they are imprecise. When I add rank controls (Appendix G Tables 15-16), the coefficients further lose significance. I conclude that I cannot measure an effect on survival.

VC investors typically liquidate successful investments through an IPO or acquisition. The regression results in the bottom panel of Table 14 find a strong statistical impact of 3.3-4 pp. This is a dramatic increase in the probability of acquisition or IPO from roughly 4% to 7.5%, but it should be interpreted with some caution in light of visual inconsistency. Figure 5B suggests there may be an effect of the grant on exit probability, but it disappears for firms with $R_i = 2$.

As with financing, I find no effect of the Phase 2 grant on revenue, survival or exit (see Appendix E for results).

5 How Does the Grant Affect Investor Decisions?

DOE SBIR grants positively impact a range of relevant outcomes. This fact, established in Section 4, is relevant to policy regardless of the mechanism. Yet understanding the source of the large effect is interesting and important. In this section I explore how the grants affect investor decisions, which also helps explain the real impacts.

The most obvious explanation for the Phase 1 grant's effect on VC investment is *certification*: the government's willingness to invest conveys positive information to venture capitalists that the firm has a promising technology. Thirty interviews I conducted with venture investors, mostly in 2013, consistently rebutted this hypothesis. The investors included experienced angels, partners at conventional VC firms, and leaders of corporate ("strategic")

VC groups. Nearly all believe that while an SBIR grant can help a firm advance to an investment-grade stage, the grant itself has little informational value. "SBIRs have no signal value," Matthew Nordan, then a Vice President at Venrock, said. "We don't care - they're completely immaterial. The only time we would care is when it gives the company time to do proof-of-concept." Investors like Rachel Sheinbein, then a CMEA Capital partner, and Andrew Garman, Managing Partner at New Venture Partners, conveyed similar opinions.[49] The startups I spoke with also did not think the grants signaled the value of their technology.

With this field evidence in mind, I present a simple model in Section 5.1 containing the mechanisms that might explain the grants' impact on external investment. In Section 5.2 and 5.3 I discuss which channel is most likely in light of my empirical evidence.

5.1 A Signal Extraction Model

I consider the grant's effect on investor decision-making through the lens of a signal extraction problem, drawing from Phelps (1972) and Aigner and Cain (1977). Here I summarize the model and describe the hypotheses; the full model is in Appendix A.

Whether a technology proposal will work in practice is often inherently uncertain. Layered on the entrepreneur's own uncertainty are information asymmetries between the entrepreneur and potential investors (Gompers and Lerner 1999). Venture investors rely on noisy signals and heuristics to choose a few firms quickly out of hundreds of proposals (Metrick 2007, Kirsch, Goldfarb and Gira 2009). I do not portray this complicated process here, but seek to distill the key elements that are relevant to my reduced form evidence.

A grant might alleviate financial constraints for recipient firms through either (1) *certification*; or (2) *funding.* Certification is when informational content in the grant decision alleviates information asymmetries, and it requires DOE to identify or be perceived to identify better firms. The second channel is the money itself, which has two subcategories: (2a) *equity* and (2b) *prototyping.* In the former, the grant allows the entrepreneur to retain more equity, which reduces financial frictions. Without the grant, an investor might have to take such a large stake in the firm that maintaining entrepreneurial incentives would be impossible. The latter channel is prototyping, where grantees demonstrate their technology's viability by investing in proof-of-concept work. Prototyping reduces uncertainty about the

[49]For example, Sheinbein said: "Nothing about government due diligence is informative...They're more in business of fear." A few angel and strategic investors, notably Mitch Tyson, Partner at Clean Energy Venture Group, and Steve Taub, then Senior Investment Director for Energy at GE Ventures, said that there is a small positive signal in the grant about the technology.

technology, which can alleviate information asymmetry (a financial friction), or simply decrease the project's risk. Certification is proposed as a possible mechanism in Lerner (2000), as well as in other studies. I have devised the funding effect and its two channels to suit the present setting.

I begin with in the no-grant case. Let each startup have a uni-dimensional technology quality signal $T_i = \bar{t} + \tau_i$, where T is normally distributed with mean $\bar{t}$ and variance σ_T^2. Suppose there is a single venture capitalist. He forms rational expectations and is more likely to invest in firms with high expected technology qualities. The investor knows the T distribution but receives only a noisy signal from each startup $\tilde{T}_i = \bar{t} + \tau_i + \varepsilon_i$, where ε is normally distributed with mean 0 and variance σ_ε^2. The investor calculates the expected technology quality given the signal, $\mathbf{E}\left(T_i \mid \tilde{T}_i\right)$, putting more weight on the signal $\tilde{T}_i$ if it is reliable - σ_ε^2 is small - and more weight on the mean $\bar{t}$ if σ_ε^2 is large. The optimal weight on the signal is $\frac{\sigma_T^2}{\sigma_\varepsilon^2+\sigma_T^2} = \alpha$, so the expected technology quality is:

$$\mathbf{E}\left(T_i \mid \tilde{T}_i\right) = (1-\alpha)\bar{t} + \alpha\tilde{T}_i \tag{3}$$

The first term is a group effect and the second term is an individual effect. The line in Equation 3 is depicted in Figure 7A. Note that α is the slope coefficient of a linear regression of T on $\tilde{T}$ and a constant.

The government also receives a signal about the firm, $\tilde{T}_i^G$, which neither the investor nor entrepreneurs observe.[50] The government awards grants to a subset of firms whose $\tilde{T}_i^G$ are located above a cutoff. Whether a firm has a grant (g) or does not (n) is a truncated dichotomous version of $\tilde{T}_i^G$. The investor observes this binary signal $x \in \{g, n)$.[51] The grant might affect the mean technology quality ($\bar{t}$), the quality variance (σ_T^2), and the signal variance (σ_ε^2). Any value of the grant money that is unrelated to its technology quality is μ_x, where $\mu_n = 0$ and $\mu_g \geq 0$. After the competition entrepreneurs have technology quality $T_{i,x} = \bar{t}_x + \mu_x + \tau_{i,x}$. Now $T_x \sim N\left(\bar{t}_x + \mu_x, \sigma_{T,x}^2\right)$, and the signal error becomes $\varepsilon_x \sim N\left(0, \sigma_{\varepsilon,x}^2\right)$.

Suppose two firms have the same noisy signal $\tilde{T}_i = \tilde{T}_j = k$, but one has a grant ($x = g$) and the other does not ($x = n$). The difference between their expected qualities, Equation

[50] I need not make any functional form assumptions about $\tilde{T}_i^G$.

[51] The investor does not observe whether a non-grantee firm applied and lost or did not apply at all. The model is agnostic about whether the grant has a negative effect on losers (though this seems unlikely because the applicant firms form a small subset of the space of energy startups). In Section 4.1.3 I argue that negative spillovers seem absent.

4, should reflect the grant.

$$\mathcal{D} = \mathbf{E}\left(T_i \mid \tilde{T}_i = k, x = g\right) - \mathbf{E}\left(T_j \mid \tilde{T}_j = k, x = n\right) \tag{4}$$

There are two broad mechanisms that might drive this difference away from zero:

1. *Certification Effect:* Suppose that the award process separates applicant firms into higher and lower technology quality types, but has no other effect. Now $\bar{t}_g > \bar{t}_n$, while $\mu_g = 0$, $\sigma^2_{T,g} = \sigma^2_{T,n} = \sigma^2_T$, and $\sigma^2_{\varepsilon,g} = \sigma^2_{\varepsilon,n} = \sigma^2_\varepsilon$. The difference in expected quality, shown in Figure 7B, is:
$$\mathcal{D} = (\bar{t}_g - \bar{t}_n)\left(1 - \frac{\sigma^2_T}{\sigma^2_\varepsilon + \sigma^2_T}\right) \tag{5}$$

2. *Funding Effect*

   (a) *Equity Channel:* The grant increases the entrepreneur's internal resources, potentially making a VC deal tractable by allowing the entrepreneur to retain a larger share of the firm. This also manifests as a mean shifting effect for grantees, as the only difference between grantees and non-grantees is μ (Figure 7B).
   $$\mathcal{D} = \mu_g\left(1 - \frac{\sigma^2_T}{\sigma^2_\varepsilon + \sigma^2_T}\right) \tag{6}$$

   (b) *Prototyping Channel:* The award is invested in proof-of-concept work. This improves the signal's reliability (increasing α), which translates to a steeper line, shown in Figure 7C. Grantees with above-average signals benefit from the slope change, and I assume that these high-type signal firms constitute the investor's consideration set. Prototyping occurs through increased signal precision, such that $\sigma^2_{\varepsilon,g} < \sigma^2_{\varepsilon,n}$.[52] With all else held the same, the difference is:

$$\mathcal{D} = \left(\bar{t} - \tilde{T}_k\right)\left(\frac{\sigma^2_T}{\sigma^2_{\varepsilon,n} + \sigma^2_T} - \frac{\sigma^2_T}{\sigma^2_{\varepsilon,g} + \sigma^2_T}\right) \tag{7}$$

Given these three possible mechanisms, I shift to the government perspective, and connect the model to the empirical design. Entrepreneurs have an ultimate observable quality T_i^O, which is a function of latent quality T_i and resources provided to the entrepreneur. Figure 8 shows the correlation of this outcome with the private government signal $\tilde{T}_i^G$. Applicant

[52]See Appendix A for discussion of the alternative possibility for a higher α, which is when $\sigma^2_{T,g} > \sigma^2_{T,n}$.

firms with $\tilde{T}_i^G$ to the right of the red cutoff line are awardees, while applicants to the left are losers. My regression discontinuity design approximates the difference in outcomes between two firms that present the same signal to the government, but where one has a grant and one does not. This is shown in Equation 8.

$$\mathcal{D} = \mathbf{E}\left(T_i^O \mid \tilde{T}_i^G = k, x = g\right) - \mathbf{E}\left(T_i^O \mid \tilde{T}_j^G = k, x = n\right) \tag{8}$$

First, when the grant has no effect, the observed outcome projected on the government signal is a horizontal line, and $\mathcal{D} = 0$. This is depicted in Figure 8A. Second, if the signal is informative about outcomes, the regression line is upward sloping (Figure 8B).[53] Here, the grant acts as a binary signal about firm quality, which the market learns is informative, so we observe a jump at the discontinuity due to certification ($\mathcal{D} > 0$). Investors are more likely to finance grantees because they have higher mean expected quality ($\bar{t}_g > \bar{t}_n$), even if the money itself has no effect. Figure 8B, which describes actual investment outcomes as a function of the government signal, maps to Figure 7B, which shows how the government signal affects investor beliefs.

Finally, if $\tilde{T}_i^G$ is uninformative but the grant money itself benefits recipients through either funding or prototyping, we observe a horizontal line with a jump at the discontinuity, shown in Figure 8C. Because the funding channel is a mean-shifting effect ($\mu_g > 0$), it maps to Figure 7B from the investor perspective. With only a prototyping channel, the government signal is uninformative (Figure 8C), but prototyping changes the variance of the signal to investors and so maps to Figure 7C.

5.2 Evaluating the Certification Hypothesis

Applicant ranks permit a test for the certification effect. Although the ranks are secret, the fact that rank maps directly to award means that investors should incorporate the grant as a positive signal *only* if DOE accurately ranks firms according to technological quality. This assertion requires rational investors. Irrational investors might consider DOE awards a valuable signal even if DOE has no ability to identify high quality firms.[54] Also, to contend that uninformative ranks reflect an absence of valuable information in the award, I need to establish that the centered ranks do not conceal information in raw rank, and that DOE

[53] It is possible that the government signal is informative in the other direction; that is, it orders poor quality firms above higher quality firms on average. In this case the line will slope down, and we would expect a downward jump at the discontinuity.

[54] See Baker and Wurgler (2011) on behavioral finance.

program officials cannot predict the number of awards in a competition. This test, while requiring strong assumptions in this context, is novel and may prove useful in other settings as well.

As far as I can discern, neither of these issues are present. First, Appendix H Figures 4-6 show the probability of subsequent VC finance by rank for competitions with only one, two, and three awards. Note in competitions with one winner the large difference in outcomes between firms with raw ranks of one and two (Appendix H Figure 4). With two winners, there is no difference between firms ranked one and two, but there is an obvious gap between firms ranked two and three (Appendix H Figure 5). Appendix H Table 1 confirms this in regressions that interact dummies for raw rank with dummies for the number of awards. My assertion that program officials are unsure of precisely the number of awards in any given competition is based on email correspondence included in the ranking data, and interviews at DOE with program officials who generate the ranks and SBIR office administrators. To the best of my knowledge, winning generates an effect, not rank nor the number of awards in the competition.

The actual probability of subsequent VC finance by rank depicted in Figure 1B is most similar to Figure 8C from the toy model - the slope of quality outcome (T_i^O) projected on the government signal ($\tilde{T}_i^G$) appears to be zero. The share of firms getting VC is flat in the DOE assigned rank, except immediately around the award cutoff. Ranks are also uninformative about the other outcome metrics. The ranks may reflect social benefits that I do not capture in my outcome metrics, but I believe the ranks are essentially randomly assigned, particularly for the higher ranked firms immediately around the cutoff. In interviews, program officials told me that SBIR is a tax on their time; they view the grants as excessively small and a burdensome administrative duty imposed from outside. Their primary task is to provide the much larger university, national lab, and large firm grants, where each grant decision involves vastly more money than SBIR competitions. In any event, identifying high quality startups is no easy task (Kerr, Nanda and Rhodes-Kropf 2013). Regardless of the ranking process, the ranks appear to be pure noise from the investor perspective.

Phase 2 provides an additional argument against certification. DOE does a second round of selection to determine the Phase 2 winners, so with certification a Phase 2 grant should reveal further quality distinction. I observe no measurable Phase 2 effect on financing, suggesting that Phase 2 does not have a certification effect and therefore making it less likely that Phase 1 does. Although Phase 1 is more competitive, for the certification hypothesis to explain the Phase 1 effect would require us to assume that all Phase 1 winners are "good firms," or that the private sector believes there is something special about the Phase 1 decision.

If my understanding of the institutional setting is correct, and if we are willing to accept the rational expectations hypothesis for investors, then the grant - the public signal x - is likely pure noise. Although we cannot rule it out, certification alone seems incapable of explaining the discontinuity in the grant's effect on VC. This presents a puzzle, and we must turn to more subtle mechanisms.

5.3 Evaluating the Funding Hypothesis

If certification is not the main channel, then the money itself must be useful, either because it permits entrepreneurs to make deals with VC investors, or because entrepreneurs invest it in valuable R&D.

5.3.1 Equity

We might imagine a simple incentive constraint requiring the entrepreneur to retain a certain share of the firm, else agency problems become excessively severe. The Phase 1 grant is a positive wealth shock for the entrepreneur, and may render a deal tractable. The rapidity of the Phase 1 effect argues in favor of the equity channel; recall that two-thirds of the effect occurs within two years.[55]

Yet compared to the average VC round size in my data of $9 million, the Phase 1 grant is quite small. It is hard to imagine that on average $150,000 can shift the startups in my sample from negative NPV to positive NPV simply by decreasing the required investor stake. Indeed, my Phase 2 evidence makes the equity channel less credible. If the cash acts by resolving the underinvestment problem, I should observe a much stronger effect of the $1 million Phase 2 grant. Also, we would expect that since the Phase 1 grant is so small, it enables access to VC finance because of the expected value of the Phase 2 effect. In this case, all Phase 1 awardees should apply for Phase 2.

Yet the RD revealed that the Phase 2 effect is either negative or much smaller per grant dollar than the Phase 1 effect. More important, the revealed preference of awardees suggests that the cash as such is not critical. Surprisingly, 37% of Phase 1 winners opt not

[55]The valuation channel does not imply that the grant is a subsidy to VC firms. For example, if the VC sector is competitive, the investor gets a break-even number of shares in the portfolio company. In equilibrium the grant causes the VC to get fewer shares, not a higher rate of return. The grant could also increase the entrepreneur's bargaining power.

to apply for Phase 2. Also, the Phase 1 grant effect is much *stronger* for Phase 1 winners who choose not to apply or who lose Phase 2 than for the whole sample (see Appendix E). It seems unlikely that the large Phase 1 impact is purely an equity effect.

5.3.2 Prototyping

We are left with prototyping as the dominant channel for the Phase 1 effect on VC. The Phase 1 grant is supposed to fund the applicant's proposed small-scale testing or demonstration project. This may be how the money is actually used, on average, even though the government does not monitor expenditure. High-quality firms whose prototyping reveals positive information find it easier to secure an investor. That most of the Phase 1 impact on VC occurs within two years makes sense if the Phase 1 research is completed within the nine month time frame set by the SBIR program.[56] By the Phase 2 stage there is sufficient information about the firms that Phase 2-funded work does not provide an incremental benefit.

Consistent with prototyping, the patent analysis finds that the grants fund valuable R&D in the short term. While both Phase 1 and and Phase 2 positively impact patents, only Phase 2 impacts patent citations, which measure innovation quality. The proof-of-concept Phase 1 work does not seem to cause a change in the entrepreneur's technology quality (τ_i), while the larger Phase 2 project may do so. This story substantiates prototyping through signal precision, where $\sigma^2_{\varepsilon,g} < \sigma^2_{\varepsilon,n}$.[57] The Phase 1 grant funds valuable demonstration and testing on existing technology, alleviating uncertainty and potentially information asymmetry. Through this prototyping channel, the grant may reduce the cost of external finance.

6 Robustness Tests

This section addresses validity of the empirical results. I first present key alternative specifications, and then conduct robustness tests of the RD design. I focus on the VC results.

[56]The VC deal flow analysis also suggests that VC availability is most relevant is at least six or eight months after the award. The grant effect is countercyclical with respect to deal flow in the two years after the grant (Section 4.1.2). Within only one year of the grant, I find a smaller and less significant effect. The grantee, under the prototyping hypothesis, must conduct its proof-of-concept work before it can effectively pitch to VCs.

[57]I expect only high-type signals enter the VC's consideration set, so $\sigma^2_{\varepsilon,g} < \sigma^2_{\varepsilon,n}$ leads the grant to have a positive impact on investment. If the full space is under consideration (perhaps some low-technology types have excellent business plans) then the grant may have no impact. The other avenue to a steeper regression line for grantees is to move their technology quality (τ_i) away from the mean, so that $\sigma^2_{T,g} > \sigma^2_{T,n}$.

The appendices contain similar analyses for revenue, survival, exit, and patenting.

First, I estimate the grant effect on the number of deals, rather than on indicators for VC or all private finance (Appendix G Tables 24-25). I use a negative binomial specification to best fit the over-dispersed count data. The results imply, using a conservative estimate, that the grant generates about 2.4 additional VC deals. I also test the grant's impact on early-stage venture capital (VCE^{Post}), which is a subset of VC^{Post} including only seed, angel, and Series A deals. This gave roughly the same results as for VC, albeit slightly smaller, shown in Appendix G Table 26.

A logit specification equivalent of Table 4 in the main text is in Appendix G Table 10. The results are strongly positive, but logit drops competitions without instances of financing. When I use the standard full set of competition dummies, more than half the observations are dropped and the coefficients are quite large. The odds ratio corresponding to the logit coefficient with BW=all implies that a winner is 3.2 times more likely to get VC finance than a loser (column VII), in contrast to the doubling I find with OLS. With topic dummies, fewer observations are dropped but the odds ratio is still 2.9 (column VIII). Clearly, logit grossly overestimates the effect.

Five tests explore the issue of changing rank composition as I move away from the cutoff. First, Appendix H Table 1 interacts raw rank dummies with dummies for the competition's number of awards. This provides identification of the treatment effect as the difference between having a raw rank of two when there are, say, two awards rather than one award in the competition. It shows that the impact of raw rank does not change with the number of awards in the competition. second, Appendix H Figures 4-6 show visual evidence that the discontinuity, and absence of information in rank, does not differ when I consider competitions with only one, two, and three awards.

Third, Appendix H Table 2 separately considers competitions with only one and more than one winner. The results are similar to the main specifications, albeit slightly higher for the one-award group. Fourth, Appendix H Table 3 assesses whether the control functions differ depending on the cutoff for the award. It estimates the quadratic specification separately for competitions with various numbers of awards. The coefficients on R_i and R_i^2 are quite consistent across specifications, and usually insignificant. Fifth, Appendix H Table 6 estimates regressions including dummies for raw rank rather than centered or percentile ranks. It again shows how little information is contained in rank compared to treatment. Thus pooling across competitions and centering of ranks does not conceal differences across cutoff points. The only variation that matters is winning versus losing.

Placebo tests check whether any difference between ranks 1 and 2 could be measured as a second discontinuity. Appendix H Table 7 runs the basic specification with ranks re-centered so that 0 lies between true ranks 1 and 2. The coefficients are mostly negative, all small, and all insignificant. I test the impact of fixed effects in Appendix H Table 8. The treatment effect is unchanged, so the within-competition comparison is apparently unimportant. Lee and Card (2008) suggest clustering standard errors by rank with discrete assignment variables. Appendix H Table 9 shows that with this method estimated effects are slightly higher than in the primary specification, but they remain significant at the 1% level. Appendix H Table 14 provides permutations of rank for the VC outcome. The basic result is consistent and robust across specifications. Second and third degree polynomials in rank have tiny, insignificant coefficients. The primary results are also essentially unchanged when covariates are excluded and with additional covariates, such as location in a major metro area (Appendix H Tables 12-13).[58]

7 Back-of-the-Envelope Return Calculation

The RD analysis relies on the *probability* of financing events as a measure of success. My data on ultimate firm valuation, albeit incomplete, provides some insight into the private return to the grants. First, I ask what stake a VC firm would require in order to be willing to invest the total grant amount. This amount - Phase 1 and Phase 2 grants - totals $0.62 billion (2012 dollars) between 1995 and 2013. The return consists of liquidation, or exit, events after the award: 10 IPOs and 42 acquisitions. Unfortunately, I have dollar amounts for only 14 of the acquisitions. After extrapolating the average acquisition amount to missing deals, the total deal amounts are $3.01 billion in IPOs and $2.18 billion in acquisitions (both in 2012 dollars). The average time between the award and the liquidation event is 8.6 years. If a VC firm requires a 30% IRR, it would need to take a 114% equity stake in order to be willing to invest $0.62 billion in these firms and earn $5.19 billion 8.6 years later.

Many awardees have not had time to exit because the investment data is censored in mid-2014. Using a Cox proportional hazards model, I estimate the probability of exit at each year from the firm's first award date. Appendix G Figure 4 shows the predicted probability of an IPO or acquisition as a function of years from award. I calculate from the estimates that a total of 152 IPOs and acquisitions are expected from the awardees, rather than 52.

[58] However, when I add woman-ownership and minority-ownership, the sample size decreases precipitously and I lose significance.

The gross deal amount is $12.9 billion (based on the average deal), and the VC required investment stake with a 30% IRR is 46% - still quite high.

In order to maintain entrepreneurial incentives, it is untenable for a VC investor to take 46% of the firm for $150,000.[59] This back-of-the-envelope calculation helps explain why the subsidies might be necessary for firms to access finance, supporting the funds mechanism from Section 5. Private investment in the portfolio of grantees at the stage at which they got the grant is apparently unviable, either because the required stake is too large (equity channel) or because the company has not yet proven that its technology works (prototyping channel).

A second exercise considers the "return" from the government's perspective. The RD analysis in Section 4.1 found that the grant doubles a firm's probability of receiving any type of private finance. Therefore, I assume that DOE is responsible - as though it took a notional equity stake - for 50% of grantees' subsequent IPOs, acquisitions, and VC deals. I use VC deals only where a firm did not exit.[60] Note that while IPO and acquisition amounts are interpretable as company valuations, VC investments provide a lower bound on the valuation. I allocate an equal share of the total grant "investment," $0.62 billion, to each unique awardee firm, and calculate each deal's CAGR (also the IRR in this case).[61] Summary statistics about the process and the results are in Table 15. The average CAGR across all firms is 14%. For the 549 firms who never receive any type of private finance, the return is of course -100%. For firms with only VC deals, the average return is 282%. Average acquisition and IPO returns are 405% and 755%, respectively. The returns are highly dispersed, however, and the medians are lower.

The average overall return of 14% to the grant "investments" revealed by this crude calculation is the same order of magnitude as VC fund returns. For example, Cochrane (2005) estimates the mean return to VC investments that result in an IPO or an acquisition, correcting for selection bias, at 59% between 1987 and 2000, not including fees. Net of fees, Kaplan and Schoar (2005) calculate average VC fund IRR at 17-18%, and Preqin's database puts this figure at 13.5%.[62]

[59] Usually in syndicates, VC investors typically own 40-75% of portfolio companies (Gompers and Lerner 2004, Mehta 2011).

[60] As with acquisitions, I extrapolate from the 268 VC deals where I have amounts to the 101 where I do not. I use only observed deals rather than the hazard model prediction.

[61] The compound annual growth rate (CAGR) is the discount rate that makes the NPV of investment cash flow zero, and its formula is: $CAGR = (\text{Deal Amount}/\text{Deal Share of Total Grants})^{(1/\#\text{ years})} - 1$.

[62] Kaplan and Schoar's data span 1981-2001, and Preqin's calculation uses all funds in its database with vintage years between 1981 and 2013.

8 Conclusion

Taking the government's objectives as given, this paper establishes that on average DOE SBIR money is not wasteful - it helps propel firms to the private market. For the early-stage projects in my sample, asset intangibility and uncertainty are at their most extreme. Further, energy technology startups are more capital intensive, have longer lead times, and carry higher project finance and market risk than the startups VCs typically finance in IT and biotech (Nanda, Younge and Fleming 2013). Finally, positive externalities motivate basic R&D and entrepreneurship in clean energy, but the absence of a carbon price makes commercialization challenging (Nordhaus 2013). My setting, therefore, is fertile ground for severe financing constraints and grants that provide additionality.

My results indicate that in this context, early-stage grants can alleviate financing constraints. Phase I grants lead recipients to generate more patents and be more likely to commercialize their technologies. Grantees are also nearly twice as likely to access VC finance. The mechanism, surprisingly, does not seem to be certification. Instead, the grants are useful because they increase firms' internal resources. Specifically, my evidence best supports a prototyping effect. Armed with a prototype that reduces uncertainty about its technology, the startup presents venture capitalists with a more viable investment opportunity. The problem, as Shane and Stuart (2002) explain, is that the information funders need to assess quality emerges only after the venture has enough funds to prove its potential. I find that the grants help overcome this Catch-22.

This insight into the grant mechanism contributes to the literature. Wallsten (2000) argues that grants must crowd out private capital because the SBIR program is explicitly designed to select high quality, or inframarginal, firms. Lerner (2000) considers this selection channel, but argues that instead certification explains the positive effects of SBIR grants.[63] My ranked application data indicate that officials do not or cannot choose firms based on their likelihood of success. This supports Lerner's argument against selection, and agrees with his broader argument that officials are unable to choose the "best" firms. I find support, however, for an alternative mechanism to explain the grant effect - the cash itself.

This paper also relates to the corporate finance literature on innovation. Seru (2014) and Bernstein (2012) find that target firms prior to acquisition and private firms prior to IPO, respectively, are more innovative than after the ownership change. Diversified conglomerates have been shown to underinvest (Ozbas and Scharfstein 2009). These and other

[63] Lerner (2000) reaches this conclusion primarily because the award impact in his sample is larger for more high-tech firms, and also because he finds decreasing returns to additional awards.

studies provide grounds for locating R&D in more entrepreneurial, focused institutions. But for the economy to benefit from high-impact entrepreneurship, many startups must be given the chance to test their ideas with the expectation that most will fail (Hsu 2008). While the market effectively disciplines outcomes, initial experimentation may suffer from severe financial frictions. Gruber, MacMillan and Thompson (2008), and Hao and Jaffe (1993), among others, suggest that inadequate external financing hinders new technology development. There is limited direct empirical evidence, however. I extend the literature and provide strong evidence that high-tech startups face financing constraints, which impede innovation.

Governments, both in the U.S. and abroad, fund a large share of applied research. Since 2000, the federal government has spent between \$130 and \$150 billion per year on R&D, about 30% of total annual U.S. R&D (NSF 2012). To the extent public funds are used to subsidize applied private sector R&D, the findings in this paper suggest that one-time grants to small firms seeking to prototype their product may be more effective in stimulating innovation than large grants that seek to identify and support the "best" firms.

Table 1: Summary Statistics of DOE SBIR Applicants

1983-2013	
# Phase 1 Applications	14,522
# Unique Phase 1 Applicant Firms	7,419
# Competitions	1,633
1995-2013	
# Phase 1 Applications	9,659
# Unique Phase 1 Applicant Firms	4,545
# Phase 1 Applications with ranking data used in RD	5,021
# Phase 1 Competitions used in RD[1]	428
Average # Phase 1 Applicants per Competition	10.6 (8.3)
Average # Phase 1 Awards per Competition	1.73 (1.13)
# Phase 2 Applications used in RD	919

[1]Competitions w/ $\geq$ 1 award

Note: This table summarizes the DOE Energy Efficiency & Renewable Energy (EERE) and Fossil Energy (FE) SBIR programs.

Table 2: Summary Statistics of Baseline Covariates and Dependent Variables

Covariate	Variable Type	Mean	Std. Dev.	Min	Max	N
MSA_i	0-1	0.304	0.46	0	1	5693
Age_i	Cont.	9.6	11.6	0	106	3808
$Minority_i$	0-1	0.081	0.27	0	1	1915
$Woman_i$	0-1	0.086	0.28	0	1	1915
$Exit_i^{\text{Post}}$	0-1	0.032	0.18	0	1	5693
$Exit_i^{\text{Prev}}$	0-1	0.033	0.18	0	1	5693
$\#SBIR_i^{\text{Prev}}$	Semi-Cont.	10.7	36.6	0	555	5693
VC_i^{Post}	0-1	0.11	0.31	0	1	5693
VC_i^{Prev}	0-1	0.077	0.27	0	1	5693
$Revenue_i$	0-1	0.55	0.50	0	1	5693
$Survival_i$	0-1	0.77	0.42	0	1	5365
$\#Patent_i^{\text{3 yrs Post}}$	Count	0.80	4.17	0	112	5693
$\#Patent_i^{\text{Prev}}$	Count	1.82	7.48	0	157	5693
$Citation_i^{\text{3 yrs Post}}$	Semi-Cont.	1.20	13.34	0	769.61	5693
$Citation_i^{\text{Prev}}$	Semi-Cont.	2.45	16.97	0	766.15	5693

Note: This table summarizes the variables used in the RD estimation. "Prev" indicates the variable prior to the firm's DOE SBIR application, and "Post" indicates afterward. See Appendix D Table 1 for additional statistics. First-time winners only. Year$\geq$ 1995

Table 3: Summary of Results

Outcome Metric	*A Phase 1 award:*	*A Phase 2 award:*
Venture Capital Finance	increases firm's probability of VC investment by 9 percentage points (average 12%) effect stronger for firms that: - are young - are in immature sectors - are in lean times - have no previous SBIR awards - are in VC-intensive regions	has no effect
Number of Patents	leads firm to produce 3 times more patents within three years (average 0.92 patents); has no long term effect effect stronger for firms that: - are young - have no previous patents - are in high propensity to patent sectors - have no previous SBIR awards	leads to 1.5 times more patents (average 2.2 patents)
Number of Normalized Patent Citations	has no effect	leads awardees to be 85% more likely to have positive citations[1]
Reaching Revenue	increases firm's probability of achieving revenue by 11 percentage points (average 56%)[2] effect stronger for firms that: - have no previous SBIR awards	has no effect
Survival	has no effect	has no effect
Exit (IPO or Acquisition)	increases firm's probability of exit by 3.5 percentage points (average 4%)[3]	has no effect

Note: This table summarizes the principal robust and precisely estimated results from the RD estimation. A firm first applies for a Phase 1 award of $150,000, and may then apply a year later for a Phase 2 award of $1,000,000. For the detailed results and variable descriptions, see Section 4 for VC, Section 5.1 for Revenue, Survival, and Exit, and 5.2 for Patents.
[1]This is a strong effect along the extensive margin. However, I find no effect along the intensive margin (conditional on firms having positive citations, there is no effect of the award).
[2]This variable is not date-specific, so while the estimated effect tells us that a positive impact exists, the magnitude cannot be interpreted as causal.
[3]This result is less visually and statistically significant than the others.

Table 4: Impact of Grant on Subsequent VC with Linear and Quadratic Control Functions

Dependent Variable: VC_i^{Post}							
Bandwidth:	1	2		3		All	
	I.	II.	III.	IV.	V.	VI.	VII.
$\mathbf{1} \mid R_i > 0$	**.098*** **	**.09*** **	**.14** **	**.1*** **	**.12** **	**.11*** **	**.072** **
	(.032)	(.025)	(.058)	(.023)	(.058)	(.021)	(.033)
VC_i^{Prev}	.27***	.32***	.32***	.31***	.31***	.32***	.32***
	(.057)	(.038)	(.038)	(.036)	(.036)	(.029)	(.029)
$\#SBIR_i^{\text{Prev}}$	.0012***	.001***	.001***	.001***	.001***	.00087***	.00084***
	(.00034)	(.00029)	(.00029)	(.00027)	(.00027)	(.00024)	(.00024)
R_i			-.02		-.029		.0086
			(.021)		(.033)		(.0071)
R_i^2					.012		-.000074
					(.0088)		(.00043)
Competition f.e.	Y	Y	Y	Y	Y	Y	Y
N	1872	2836	2836	3368	3368	5021	5021
R^2	0.47	.39	.39	.34	.35	.27	.27

Note: This table reports regression estimates of the effect of the Phase 1 grant ($\mathbf{1} \mid R_i > 0$) on VC. The likelihood of receiving VC after the grant is 10.9%; among losers it is 9.4%, and among winners it is 21.3% (bandwidth=all specification). The specifications are variants of the model in Equation 1. The dependent variable $VC^{\text{Post}}{}_i$ is 1 if the company ever received VC after the award decision, and 0 if not. Specifications vary the bandwidth around the cutoff and control for rank linearly and quadratically. Standard errors are robust and clustered at the topic-year level. *** $p < .01$. Year$\geq$ 1995

Table 5: Impact of Grant on Subsequent VC with Percentile Rank Control (Quintiles)

Dependent Variable: VC_i^{Post}				
Bandwidth:	I. 1	II. 2	III. 3	IV. all
$\mathbf{1} \mid R_i > 0$	**.098*** **	**.1*** **	**.094*** **	**.1*** **
	(.032)	(.035)	(.033)	(.028)
VC_i^{Prev}	.27***	.32***	.31***	.32***
	(.057)	(.038)	(.036)	(.029)
$\#SBIR_i^{\text{Prev}}$	.0012***	.001***	.001***	.00085***
	(.00034)	(.00029)	(.00027)	(.00024)
$R_i{}^{\text{Q2}}$		.016	-.01	.011
		(.032)	(.028)	(.022)
$R_i{}^{\text{Q3}}$		.019	.0043	-.022
		(.042)	(.033)	(.022)
$R_i{}^{\text{Q4}}$		.014	-.026	-.039
		(.047)	(.036)	(.026)
$R_i{}^{\text{Q5}}$		-.026	-.05	-.044
		(.062)	(.041)	(.029)
Competition f.e.	Y	Y	Y	Y
N	1872	2836	3368	5021
R^2	.47	.39	.35	.27

Note: This table reports regression estimates of the effect of the Phase 1 grant ($\mathbf{1} \mid R_i > 0$) on VC. The specifications are variants of the model in Equation 1. The dependent variable $VC^{\text{Post}}{}_i$ is 1 if the company ever received VC after the award decision, and 0 if not. Ranks are transformed into the applicant's percentile rank within his competition. The highest quantile is omitted. Standard errors are robust and clustered at the topic-year level. *** $p < .01$. Year$\geq$ 1995

Table 6: Impact of Grant on Subsequent VC by Firm Age, Location, & Sector Maturity

Dependent Variable: VC_i^{Post}						
	I. $Age_i \leq 2$	II. $Age_i > 2$	III. I & II	IV. $Age_i \leq 9$	V. $Age_i > 9$	VI. IV & V
$\mathbf{1} \mid R_i > 0$	**.17****	**.092*****	.092***	**.14*****	**.047***	.047*
	(.069)	(.021)	(.016)	(.031)	(.024)	(.024)
$\mathbf{1} \mid R_i > 0 \cdot (\mathbf{1} \mid Age_i \leq X)$			**.076***			**.093****
			(.043)			(.039)
VC_i^{Prev}	.44***	.31***	.31***	.37***	.18***	.18***
	(.11)	(.032)	(.021)	(.041)	(.053)	(.053)
$\#SBIR_i^{\text{Prev}}$	.0043	.001***	.001***	.0012**	.0012***	.0012***
	(.0027)	(.00024)	(.00014)	(.00053)	(.00028)	(.00028)
Topic f.e.	Y	Y	Y	Y	Y	Y
Topic f.e.$\cdot$ $(\mathbf{1} \mid X)$	N	N	Y	N	N	Y
N	576	2792	3368	1574	1876	3368
R^2	.52	.22	.31	.33	.23	.34

	VII. Same MSA	VIII. Different MSAs	IX. VII & VIII	X. Mature	XI. Immature	XII. X & XI
$\mathbf{1} \mid R_i > 0$	**.12*****	**.099*****	.099***	**.072****	**.18*****	.072**
	(.04)	(.021)	(.021)	(.036)	(.04)	(.036)
$\mathbf{1} \mid R_i > 0 \cdot (\mathbf{1} \mid \text{Same MSA})$			**.02**			
			(.044)			
$\mathbf{1} \mid R_i > 0 \cdot (\mathbf{1} \mid Imm.)$						**.11****
						(.054)
VC_i^{Prev}	.3***	.33***	.33***	.23***	.39***	.23***
	(.056)	(.034)	(.034)	(.059)	(.045)	(.059)
$\#SBIR_i^{\text{Prev}}$	.001***	.00095***	.00095***	.001**	.00028	.001***
	(.00038)	(.00023)	(.00023)	(.00038)	(.00034)	(.00038)
Topic f.e.	N	N	N	Y	Y	Y
Topic f.e.$\cdot$ $(\mathbf{1} \mid X)$	N	N	N	N	N	Y
Competition f.e.	Y	Y	Y	N	N	N
Competition f.e.$\cdot$ $(\mathbf{1} \mid X)$	N	N	Y	N	N	N
N	1380	4312	5692	1330	1820	3150
R^2	.23	.26	.26	.18	.2	.2

Note: This table reports regression estimates of the effect of the Phase 1 grant ($\mathbf{1} \mid R_i > 0$) on VC. The specifications are variants of the model in Equation 1, using BW=3. The top panel divides the sample by firm age in years at the time of application. III & VI jointly estimate the two preceding regressions to obtain a standard error on the difference, which is bold. VII-IX assess the reallocation effect, using BW=all. VII includes firms on each side of the cutoff within a topic who are from the same city (MSA). VIII estimates the effect when competing firms are from different MSAs. X-XII employ an indicator for immature sectors, which is 0 in X, and 1 in XI. I use topic dummies to permit sufficient within-group observations for age and sector maturity. Coefficients on other interacted covariates are not reported for brevity. Standard errors are robust and clustered at the topic-year level. *** $p < .01$. Year$\geq$ 1995

Table 7: Impact of Grant on Subsequent VC Investment by Technology Type

Dependent Variable: VC_i^{Post}		
Technology (sub-sector)	Coefficient on treatment $(\mathbf{1} \mid R_i > 0)$	N
Geothermal	**.56*** (.24)	51
Hydropower, Wave & Tidal	**.51**** (.19)	181
Solar	**.25**** (.11)	421
Carbon Capture & Storage	**.2**** (.091)	211
Building & Lighting Efficiency	**.14**** (.057)	370
Vehicles, Motors, Engines, Batteries	**.12**** (.06)	726
Wind	**.11**** (.039)	194
Advanced Materials	**.11** (.071)	435
Biomass Production/ Generation	**.085** (.067)	308
Fuel Cells & Hydrogen	**.077** (.0723)	400
Natural Gas	**.06** (.074)	255
Recycling, Waste to energy & Water	**.045** (.053)	549
Smart Grid, Sensors & Power Converters	**.045** (.053)	634
Air & Emission Control	**.025** (.035)	300
Coal	**.024** (.053)	108
Biofuels & Biochemicals	**.014** (.054)	176

Note: This table reports regression estimates of the effect of the Phase 1 grant $(\mathbf{1} \mid R_i > 0)$ on VC by technology (sub-sector) using BW=all. Here I report only the coefficient on treatment. A full table is in Appendix G Table 10. The specifications are variants of the model in Equation 1, but each includes only competitions whose topics fall within the specific technology. Other and "Oil" are omitted due to few observations. Control coefficients are not reported for brevity. Standard errors are robust and clustered at the topic-year level. *** $p < .01$. Year$\geq$ 1995.

Table 8: Temporal Impact of Grant on Subsequent Venture Capital

Dependent Var.:	I. $VC_i^{\text{0-1 yr Post}}$	II. $VC_i^{\text{0-2 yr Post}}$	III. $VC_i^{\text{0-3 yr Post}}$	IV. $VC_i^{\text{0-4 yr Post}}$	V. $VC_i^{\text{0-5 yr Post}}$	VI. $VC_i^{\text{0-6 yr Post}}$
$\mathbf{1} \mid R_i > 0$	**.058*****	**.075*****	**.074*****	**.082*****	**.079*****	**.083*****
	(.017)	(.019)	(.019)	(.021)	(.021)	(.021)
VC_i^{Prev}	.24***	.32***	.32***	.32***	.33***	.33***
	(.029)	(.033)	(.034)	(.035)	(.035)	(.035)
$\#SBIR_i^{\text{Prev}}$	-.000027	-.00004	-.000065	.000039	.00011	.000092
	(.00016)	(.0002)	(.0002)	(.00024)	(.00024)	(.00024)
Competition f.e.	Y	Y	Y	Y	Y	Y
N	3368	3368	3368	3368	3368	3368
R^2	.36	.38	.39	.38	.37	.37
Dependent Variable: VC_i^{Post}						
	VII. 1995-1999	VIII. 2000-2004	IX. 2005-2009	X. 2009-2013	XI. 2009-2011	XII. 2009
$\mathbf{1} \mid R_i > 0$	**.076***	**.047**	**.07****	**.19*****	**.13*****	**.1***
	(.04)	(.036)	(.031)	(.047)	(.039)	(.055)
VC_i^{Prev}	.096	.3***	.41***	.34***	.42***	.43***
	(.062)	(.078)	(.045)	(.049)	(.04)	(.066)
$\#SBIR_i^{\text{Prev}}$	.0019***	.0017***	.00039	-.001***	-.001***	-.00092*
	(.00025)	(.00034)	(.00028)	(.00038)	(.00025)	(.0005)
Competition f.e.	Y	Y	Y	Y	Y	Y
N	1392	1052	1970	3160	2192	893
R^2	.23	.3	.26	.39	.31	.26

Note: This table reports regression estimates of the effect of the Phase 1 grant ($\mathbf{1} \mid R_i > 0$) on VC over time. The specifications are variants of the model in Equation 1. The dependent variables in the top panel are indicators for whether a firm received VC investment within a certain number of years from the award. For example, $VC_i^{\text{0-1 yr Post}} = 1$ if the company received VC within one year of the award. The top panel uses BW=3. The bottom panel limits the sample to certain time periods, where years are inclusive, and uses BW=all. The dependent variable ${VC^{\text{Post}}}_i$ is 1 if the company ever received VC after the award decision, and 0 if not. Standard errors are robust and clustered at the topic-year level. *** $p < .01$. Year $\geq$ 1995

Table 9: Heterogeneity of Grant Impact on Subsequent VC with Clean Energy Industry Tobin's Q & Total U.S. VC Deals

Dependent Variable: VC_i^{Post}						
Time Series Variable:		Q_{t+1}			$\#VC_{t+2}$	
	I. BW=2	II. BW=3	III. BW=all	IV. BW=2	V. BW=3	VI. BW=all
$(\mathbf{1} \mid R_i > 0) \cdot Q_{t+1}$	**-.2**	**-.26****	**-.22****			
	(.14)	(.13)	(.11)			
$(\mathbf{1} \mid R_i > 0) \cdot \#VC_{t+2}$				**-.02***	**-.03****	**-.025****
				(.011)	(.012)	(.01)
$\mathbf{1} \mid R_i > 0$	.12***	.14***	.15***	.122***	.15***	.16***
	(.03)	(.031)	(.025)	(.031)	(.032)	(.027)
Q_{t+1}	.20	.26**	-26.49***			
	(.14)	(.13)	(.95)			
$\#VC_{t+2}$				.02*	.03**	-.63***
				(.011)	(.012)	(.022)
Competition f.e.	Y	Y	Y	Y	Y	Y
N	2836	3368	5021	2836	3368	5021
R^2	.32	.28	.18	.32	.28	.18

Note: This table reports regression estimates of the effect of the Phase 1 grant $(\mathbf{1} \mid R_i > 0)$ on VC in different Q and VC flow environments. The dependent variable $VC^{\text{Post}}{}_i$ is 1 if the company ever received VC after the award decision, and 0 if not. The specifications are variants of the model in Equation 1, except I interact treatment with a time series variable. In the left panel, this is a measure of clean energy industry Tobin's Q over the four quarters following the award decision, and in the right panel, it is the total number of VC investments in U.S. companies over the eight quarters following the award decision. Both variables are demeaned, and VC deals also divided by 1,000. Standard errors are robust and clustered at the topic-year level. *** $p < .01$. Year$\geq$ 1995

Table 10: Impact of both Phase 1 and Phase 2 Grants on Subsequent Venture Capital Financing

Dependent Variable : VC_i^{Post}				
Bandwidth:	I. 1	II. 2	III. 3	IV. all
$\mathbf{1} \mid R_i^{Ph1} > 0$	**.099*** **	**.1*** **	**.11*** **	**.11*** **
	(.034)	(.027)	(.027)	(.025)
$\mathbf{1} \mid R_i^{Ph2} > 0$	**-.003**	**-.042**	**-.032**	**-.017**
	(.078)	(.054)	(.048)	(.043)
VC_i^{Prev}	.27***	.32***	.31***	.32***
	(.057)	(.038)	(.036)	(.029)
$\#SBIR_i^{\text{Prev}}$	.0012***	.001***	.0011***	.00087***
	(.00034)	(.00029)	(.00027)	(.00024)
Competition f.e.	Y	Y	Y	Y
N	1872	2835	3367	5021
R^2	.47	.39	.35	.27

Note: This table reports regression estimates of the effect of the Phase 1 grant ($\mathbf{1} \mid R_i^{Ph1} > 0$) and Phase 2 grant ($\mathbf{1} \mid R_i^{Ph2} > 0$) on subsequent VC. The dependent variable $VC^{\text{Post}}{}_i$ is 1 if the company ever received VC after the award decision, and 0 if not. The specifications are variants of the model in Equation 1, but with an additional indicator that is 1 if the firm won Phase 2, and 0 if it did not or did not apply. Standard errors are robust and clustered at the topic-year level. *** $p < .01$. Year$\geq$ 1995

Table 11: Impact of Grant on Subsequent Three Year Patenting with Linear and Quadratic Control Functions (Negative Binomial)

Dependent Variable: $\#Patent_i^{3\text{ yrs Post}}$							
Bandwidth:	1	2		3		All	
	I.	II.	III.	IV.	V.	VI.	VII.
$\mathbf{1} \mid R_i > 0$	**1.03*****	**1.18*****	**1.07*****	**1.4*****	**1.0*****	**2*****	**1.1*****
	(0.17)	(0.14)	(0.25)	(0.13)	(0.210)	(.16)	(.21)
$\#Patent_i^{\text{Prev}}$	0.16***	0.11***	0.11***	0.112***	0.11***	.14***	.13***
	(0.042)	(0.019)	(0.019)	(0.02)	(0.02)	(.018)	(.017)
VC_i^{Prev}	1.22***	1.38***	1.36***	1.34***	1.33***	1.3***	1.1***
	(0.25)	(0.17)	(0.18)	(0.17)	(0.17)	(.16)	(.15)
$\#SBIR_i^{\text{Prev}}$	0.0094***	0.011***	0.011***	0.011***	0.011***	.011***	.011***
	(0.0023)	(0.0015)	(0.0015)	(0.0016)	(0.0016)	(.0015)	(.0015)
R_i			0.044		0.018		.19***
			(0.083)		(0.0873)		(.054)
R_i^2					0.06*		-.0054
					(0.034)		(.0041)
Topic f.e.	Y	Y	Y	Y	Y	Y	Y
N	1872	2836	2836	3368	3368	5021	5021
Pseudo-R^2	0.21	0.183	0.18	0.16	0.16	.16	.16
Log likelihood	-1351.7	-2054.8	-2054.7	-2421.9	-2419.3	-3219	-3208

Note: This table reports regression estimates of the effect of the Phase 1 grant ($\mathbf{1} \mid R_i > 0$) on patents. The mean number of patents within three years after the grant is 0.79; among losers it is 0.57, and among winners it is 2.2 (bandwidth=all specification). The specifications are variants of the model in Equation 1. The dependent variable $\#Patent_i^{3\text{ yrs Post}}$ is the number of successful patents that the firm applied for within three years of the grant award. Specifications vary the bandwidth around the cutoff and control for rank linearly and quadratically. Topic fixed effects are a higher level than competition to achieve convergence of the maximum likelihood function, but still within-year. Standard errors are robust. *** $p < .01$. Year$\geq$ 1995

Table 12: Impact of Grant on Subsequent Patenting within Three Years of Application by Firm Age, Technology Propensity to Patent, and Number of Previous Patents (Negative Binomial)

Dependent Variable: $\#Patent_i^{\text{3 yrs Post}}$								
	Firm Age in Years				Firm # Previous Patents		Tech. Patent Propensity	
	I. ≤ 2	II. > 2	III. ≤ 9	IV. > 9	V. 0	VI. ≥ 1	VII. High	VIII. Low
$\mathbf{1} \mid R_i > 0$	**2.5*** **	**.56**	**1.5*** **	**-.48**	**1.2*** **	**1*** **	**2.1*** **	**.99*** **
	(.38)	(.42)	(.28)	(1.1)	(.39)	(.23)	(.46)	(.22)
$\#Patent_i^{\text{Prev}}$	.21	.13***	.16***	.12***			.32***	.3***
	(.16)	(.023)	(.049)	(.022)			(.077)	(.046)
VC_i^{Prev}	1.8***	1.1***	1.5***	.73***	2***	1.1***	.59	1.4***
	(.39)	(.2)	(.25)	(.24)	(.37)	(.18)	(.37)	(.19)
$\#SBIR_i^{\text{Prev}}$	.0073	.0097***	.012***	.011***	.017***	.0051***	.011***	.01***
	(.0083)	(.0017)	(.0031)	(.0019)	(.0063)	(.00088)	(.0043)	(.002)
R_i	-.15**	.43*	.0059	1	.14	-.022	.14	-.17*
	(.072)	(.22)	(.075)	(.68)	(.11)	(.082)	(.17)	(.094)
R_i^2	-.072	-.081	-.016	-.24	.047	-.014	-.046	.14***
	(.046)	(.064)	(.034)	(.18)	(.055)	(.033)	(.061)	(.04)
Topic f.e.	N	N	N	N	N	N	Y	Y
Year f.e.	Y	Y	Y	Y	Y	Y	N	N
N	576	2790	1410	1958	2308	1058	834	2532
Pseudo-R^2	.14	.092	.1	.1	.083	.067	.15	.2
Log likelihood	-383	-2221	-1220	-1367	-794	-1646	-719	-1640

Note: This table reports regression estimates of the effect of the Phase 1 grant ($\mathbf{1} \mid R_i > 0$) on patents using BW=3. The specifications are variants of the model in Equation 1. The dependent variable $\#Patent_i^{\text{3 yrs Post}}$ is the number of successful patents that the firm applied for within three years of the grant award. The left panel divides the sample by an indicator for high propensity to patent, which is 1 if the firm's technology sub-sector is Smart Grid, Sensors & Power Converters, Advanced Materials, Solar, or Batteries. The middle panel divides the sample by firm age, and the right panel by the firm's number of patents prior to applying for the grant. For all three, I could not estimate difference equations due to non-convergence of the Poisson maximum likelihood. Standard errors are robust. *** $p < .01$. Year$\geq$ 1995

Table 13: Relationship between VC Finance and Patenting/Citation Outcomes

Panel A: Impact of VC on Patents & Citations Prior to Applying and Among Phase 1 Losers

	All Applicants			Losers only		
Dependent Variable:	I. $\#Patent_i^{\text{Prev}}$	II. $Citation_i^{\text{Prev}}$		III. $\#Patent_i^{\text{3 yrs Post}}$	IV. $Citation_i^{\text{3 yrs Post}}$	
		IIa. Logit	IIb. Regress		IVa. Logit	IVb. Regress
VC_i^{Prev}	**.96*** **	**1.005*** **	**12.04*** **	**1.31*** **	**.78*** **	**21.66*** **
	(.12)	(.11)	(4.52)	(.16)	(.18)	(6.37)
Year f.e.	Y	Y	Y	Y	Y	Y
Sector f.e.	Y	Y	Y	Y	Y	Y
N	6324	6322	6322	5042	4677	4677
R^2			.06			.14
Pseudo-R^2	.016	.055		.094	.19	
Log lik.	-8390.7	-10101.4	-10101.4	-5098.0	-4840.1	-4840.1

Panel B: Impact of Grant on Patents for Firms with no VC before or within 3 Yrs of Applying

Dependent Variable: $\#Patent_i^{\text{3 yrs Post}}$

	V. BW=1	VI. BW=2	VII. BW=3	VIII. BW=all
$\mathbf{1} \mid R_i > 0$	**.89*** **	**.57** **	**.84*** **	**1.12*** **
	(.18)	(.29)	(.25)	(.26)
Year f.e.	Y	Y	Y	Y
Sector f.e.	Y	Y	Y	Y
N	1644	2482	2952	4424
Pseudo-R^2	.063	.064	.059	.056
Log lik.	-1248.3	-1833.8	-2129.7	-2851.1

Note: This table reports regression estimates of the relationship between VC funding and patenting/citation outcomes for Phase 1 applicants. The top panel estimates the impact of having VC finance prior to applying for the grant (VC_i^{Prev}) on outcomes. Columns I and II consider only events prior to application. Columns III and IV limit the sample to firms who applied for an SBIR and lost. For patents, I use the negative binomial model as in previous regressions. For citations I use the two-part (logit plus regression). The logit portion of estimates zero vs. positive citations (extensive margin), and then the regress part estimates the impact of the grant on observations with positive citations (intensive margin). The bottom panel estimates the effect of the Phase 1 grant ($\mathbf{1} \mid R_i > 0$) on patents as in Table 11, but includes only firms that did not previously receive VC prior to application, nor received VC finance within three years of application. Covariates omitted for brevity. Standard errors are robust. *** $p < .01$. Year$\geq$ 1995

Table 14: RD Impact of Grant on Firm Revenue, Survival and Exit with no Rank

Dependent Variable: $Revenue_i$				
	I. BW=1	II. BW=2	III. BW=3	IV. BW=all
$\mathbf{1} \mid R_i > 0$	**.11*** **	**.09*** **	**.1*** **	**.12*** **
	(.038)	(.03)	(.028)	(.025)
VC_i^{Prev}	.17***	.17***	.18***	.23***
	(.05)	(.038)	(.033)	(.024)
$\#SBIR_i^{\text{Prev}}$	.0017***	.0017***	.0018***	.002***
	(.00028)	(.00022)	(.00022)	(.00019)
Competition f.e.	Y	Y	Y	Y
N	1872	2836	3368	4812
R^2	.41	.33	.3	.23
Dependent Variable: $Survival_i$				
	I. BW=1	II. BW=2	III. BW=3	IV. BW=all
$\mathbf{1} \mid R_i > 0$	**.072** **	**.046***	**.039**	**.046** **
	(.036)	(.026)	(.024)	(.021)
VC_i^{Prev}	.086*	.11***	.096***	.1***
	(.047)	(.03)	(.028)	(.02)
$\#SBIR_i^{\text{Prev}}$	.00071***	.00072***	.00078***	.00079***
	(.00025)	(.00019)	(.00016)	(.00014)
Competition f.e.	Y	Y	Y	Y
N	1750	2660	3160	4533
R^2	.39	.32	.28	.23
Dependent Variable: $Exit_i^{\text{Post}}$				
	I. BW=1	II. BW=2	III. BW=3	IV. BW=all
$\mathbf{1} \mid R_i > 0$	**.044***	**.033***	**.041*** **	**.034*** **
	(.025)	(.017)	(.015)	(.012)
$Exit_i^{\text{Prev}}$	-.1***	-.099***	-.094***	-.084***
	(.039)	(.023)	(.018)	(.012)
VC_i^{Prev}	.14***	.12***	.13***	.13***
	(.043)	(.029)	(.025)	(.019)
$\#SBIR_i^{\text{Prev}}$	.00074**	.0007***	.00056***	.0003*
	(.0003)	(.00022)	(.00021)	(.00016)
Competition f.e..	Y	Y	Y	Y
N	1872	2836	3368	5021
R^2	.41	.31	.26	.18

Note: This table reports regression estimates of the effect of the Phase 1 grant ($\mathbf{1} \mid R_i > 0$) on revenue, survival, and exit with no rank controls. The likelihood of revenue/survival/exit after the grant is 55%/78%/3.4%; among losers it is 54%/77%/2.8%, and among winners it is 68%/82%/7.1% (bandwidth=all specification). The specifications are variants of Equation 1. Top panel: the dependent variable $Revenue_i$ is 1 if the firm ever reached revenue, and 0 if not. Unfortunately this variable is not centered around the award decision. Middle panel: the dependent variable $Survival_i$ is 1 if the firm was active as of May, 2014, and 0 if not. Bottom panel: the dependent variable $Exit_i^{\text{Post}}$ is 1 if the firm experienced an IPO or acquisition after the award decision. Standard errors are robust and clustered at the topic-year level. *** $p < .01$. Year≥ 1995. Survival is as of May, 2014.

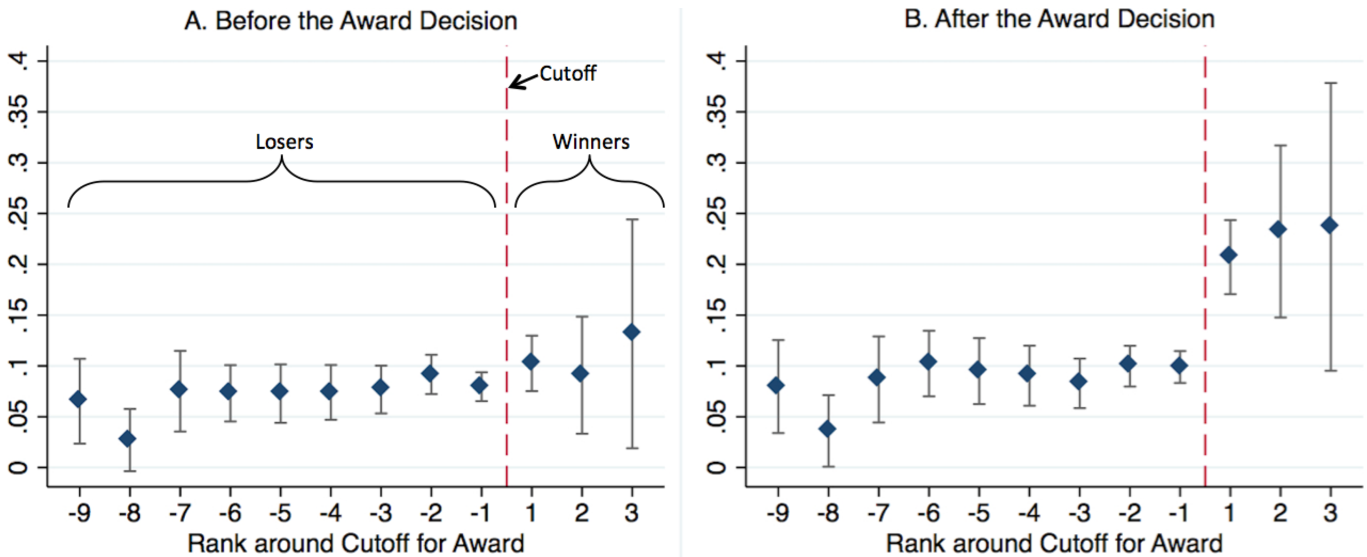
Table 15: Back-of-the-Envelope Return Calculation 1995-2013 by Deal Type

	I. IPO	II. Acquisition	III. VC only	IV. No Finance	V. All Firms
# Awardee Firms	10	43	148	549	750
# Deals	10	43	353	0	406
# Deals missing amt	0	29	90	0	119
Mean deal amt (mill)	$301	$50.6	$8.99	0	$20.60
Total deal amt w/extrapolation (mill)	$3,013	$2,175	$3,897	0	$9,084
Grant "investment" per deal (mill)[1]	$.82	$.82	$.82	$.82	$.82
Gov't "stake" in deal	50%	50%	50%	50%	50%
Mean # years between award and deal	10.46	6.87	3.10	-	3.68
Mean return (CAGR)	**755%**	**405%**	**282%**	**-100%**	**14%**
25th pctile CAGR	65%	19%	35%	-100%	-100%
50th pctile CAGR	**95%**	**73%**	**258%**	**-100%**	**-100%**
75th pctile CAGR	271%	232%	448%	-100%	-45%
Std Dev CAGR	1,845%	822%	271%	0%	600%

[1] $615 million/406

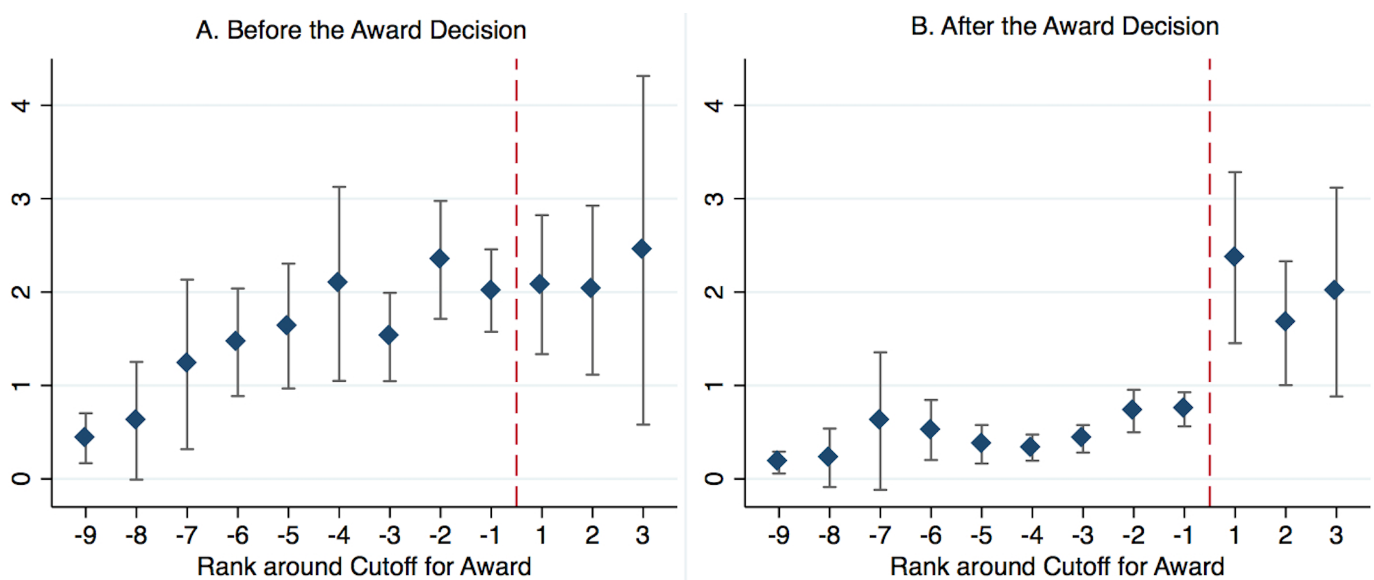
Note: This table documents a back-of-the-envelope calculation of the grant "investment" return based on ultimate company valuation. This compound annual growth rate (CAGR) is the same as the IRR in this setting. I assign each deal an equal share of the total DOE SBIR grants given to all firms between 1995-2013. Based on this "investment" of $.82 million, I calculate a CAGR for each deal. The reported mean return is the average of these deal-specific CAGRs. Column I shows the return for awardees that experienced IPOs, and column II awardees that were acquired. Where a firm does not have an IPO or acquisition, I use VC deal amounts as a lower bound on firm valuation (column III). Column IV shows the -100% return for all firms with no subsequent private finance. For deals with missing amounts, I extrapolate using the average deal amount for that category. For firms with multiple VC deals, I use the total deal amount and average the time between award and deals. I assign deals that occurred less than 365 days after the award a time period of one year. All amounts in millions of 2012 dollars.

Figure 1: Probability of Venture Capital Financing Before and After Grant Decision by Rank



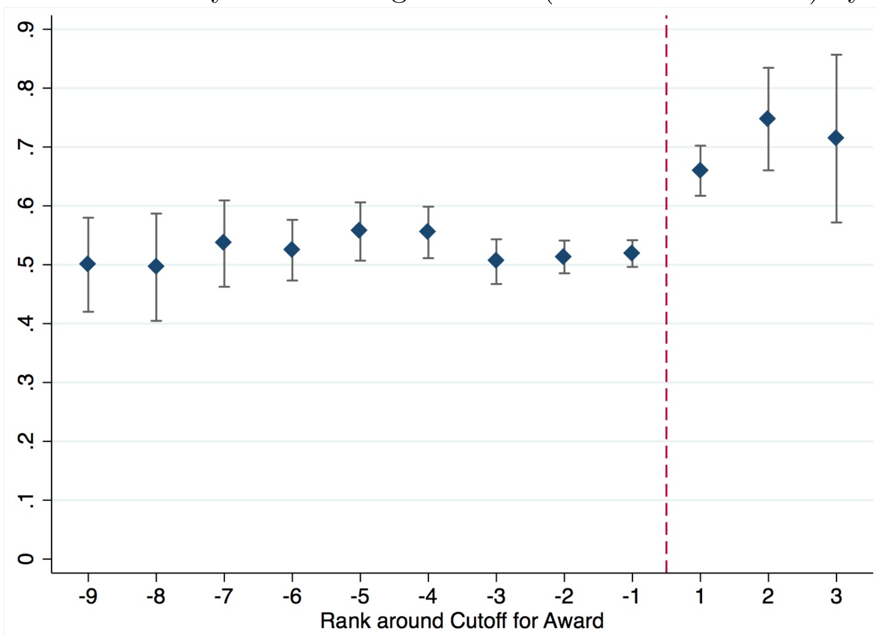
Note: This figure shows the fraction of applicants who ever received VC investment ever prior to (1A) and ever after (1B) the Phase 1 grant award decision. The applicants are binned by their DOE assigned rank, which I have centered so that Rank > 0 indicates a firm won an award. Capped lines indicate 95% confidence intervals. N=4,812.

Figure 2: Number of Patents Before and After Grant Decision by Rank



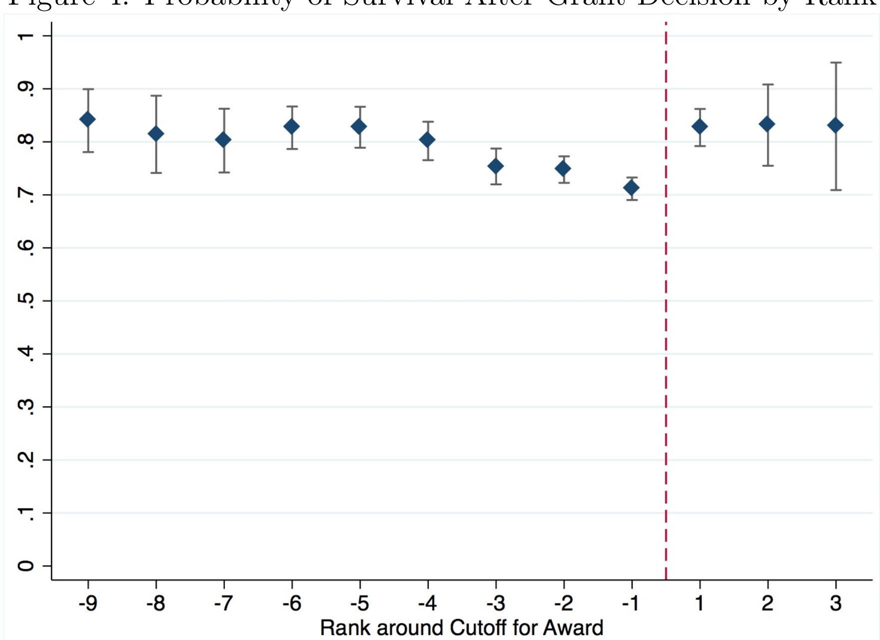
Note: This figure shows firm patents ever prior to (2A) and within three years after (2B) the Phase 1 grant award decision. The applicants are binned by their DOE assigned rank, which I have centered so that Rank > 0 indicates a firm won an award. The date associated with a successful patent is the patent application date. Capped lines indicate 95% confidence intervals. N=4,816.

Figure 3: Probability of Achieving Revenue (Commercialization) by Rank



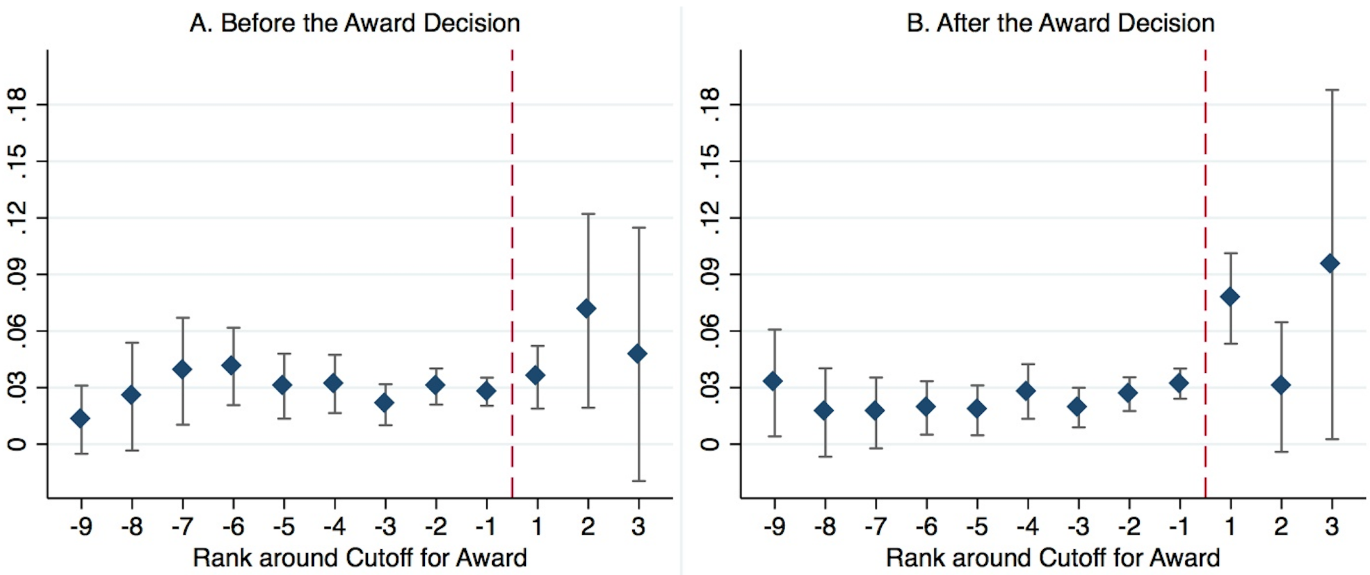
Note: This figure shows the fraction of applicants who achieved revenue. The applicants are binned by their DOE assigned rank, which I have centered so that Rank > 0 indicates a firm won an award. This variable is not dated, so I do not know if the firm achieved revenue before or after the grant. Capped lines indicate 95% confidence intervals. N=4,816.

Figure 4: Probability of Survival After Grant Decision by Rank



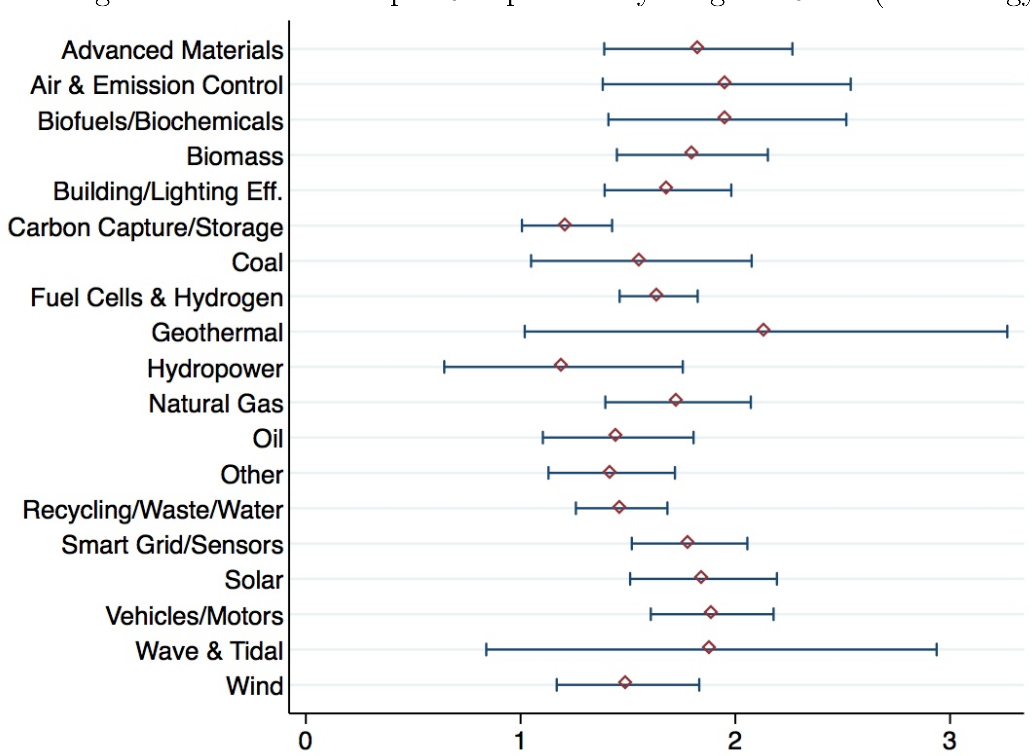
Note: This figure shows the fraction of applicants who survived (as of May 2014) after the Phase 1 grant award decision. The applicants are binned by their DOE assigned rank, which I have centered so that Rank > 0 indicates a firm won an award. Capped lines indicate 95% confidence intervals. N=4,816.

Figure 5: Probability of Exit (IPO or Acquisition) Before and After Grant Decision by Rank



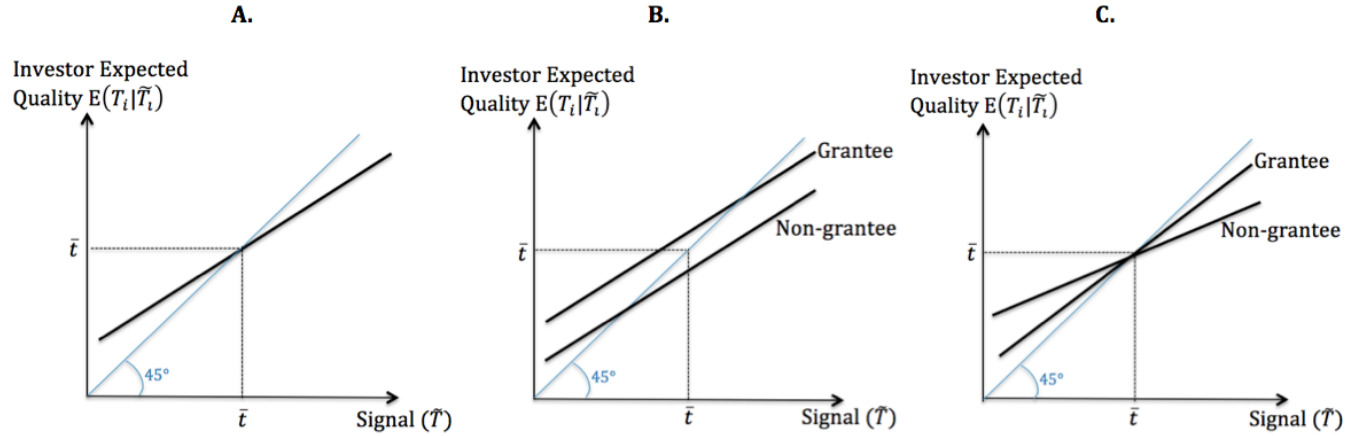
Note: This figure shows the fraction of applicants who ever experienced an exit (IPO or acquisition) ever prior to (5A) and ever after (5B) the Phase 1 grant award decision. The applicants are binned by their DOE assigned rank, which I have centered so that Rank > 0 indicates a firm won an award. Capped lines indicate 95% confidence intervals. N=4,816.

Figure 6: Average Number of Awards per Competition by Program Office (Technology Topic)



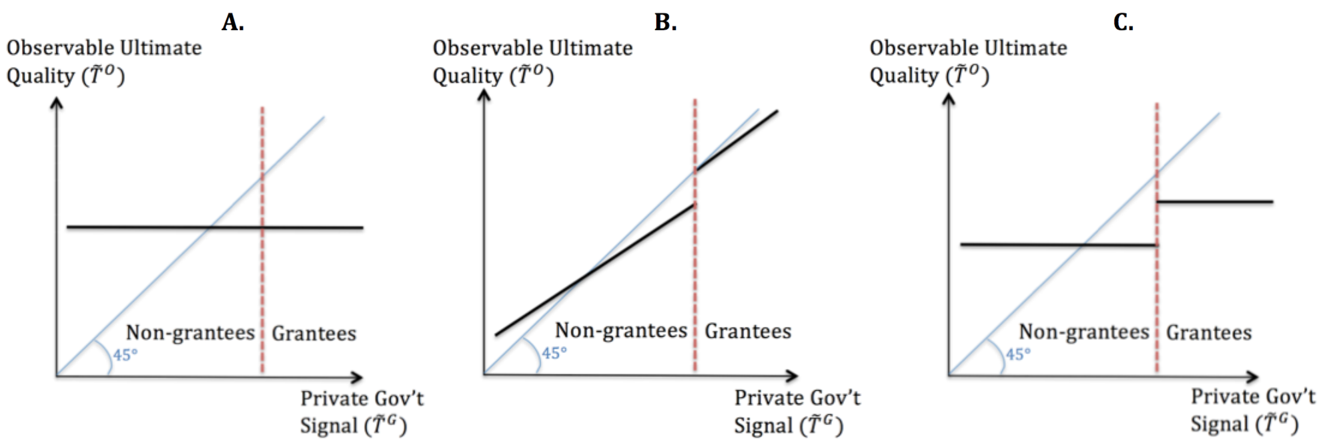
Note: This figure shows that within competitions, the average number of Phase 1 awards does not vary systematically across program offices (topics). It includes all DOE EERE & FE competitions from 1995 are included. Capped lines indicate 95% confidence intervals. For the number of awards per office and per competition over time, see Appendix D Figures 1-3. N=863.

Figure 7: Possible grant effects on investor expected quality given firms' signal to investors



Note: Figure 11.A shows the investor's expected quality of the entrepreneur (y-axis) as a function of the noisy signal that the investor observes (x-axis). Figure 11.B shows that a certification or valuation effect increases the mean expected quality of grantees relative to non-grantees ($\bar{t}_g > \bar{t}_n$). Figure 11.C shows that a prototyping effect increases the slope of the grantee line relative to the non-grantee line. This occurs because the grant causes the grantee's signal to be more reliable, which for example may occur if prototyping decreases the variance of the noisy signal $\left(\sigma^2_{\varepsilon,g} < \sigma^2_{\varepsilon,n}\right)$.

Figure 8: Possible grant effects on firm outcome given firms' private signal to government



Note: Figure 12.A shows this observable outcome (y-axis) as a function of the signal that the government receives from the firm, which is private to the government (x-axis). In this case, the government signal $\tilde{T}^G$ is wholly uninformative about outcomes, so the line is flat, and there can be no certification effect with rational investors. In Figure 12.A, there is both no certification effect and no effect of the grant money itself, so there is no jump at the discontinuity between non-grantees and grantees. Figure 11.B shows a prototyping or valuation effect increasing outcomes for grantees relative to non-grantees in the absence of certification ($\tilde{T}^G$ uninformative). Figure 11.C. shows the certification case, in which $\tilde{T}^G$ is informative and thus correlated with outcomes. In the absence of a valuation or prototyping effect, we nonetheless observe a jump at the discontinuity as the market accounts for information in the private government signal $\tilde{T}^G$.

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