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Financing Innovation: Evidence from R&D Grants†

By Sabrina T. Howell*

Governments regularly subsidize new ventures to spur innovation. This paper conducts the first large-sample, quasi-experimental evaluation of R&D subsidies. I use data on ranked applicants to the US Department of Energy's SBIR grant program. An early-stage award approximately doubles the probability that a firm receives subsequent venture capital and has large, positive impacts on patenting and revenue. These effects are stronger for more financially constrained firms. Certification, where the award contains information about firm quality, likely does not explain the grant effect. Instead, the grants are useful because they fund technology prototyping. (*JEL* D22, G24, G32, L53, O31, O34, O38)

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. Another is that financial frictions cause small firms to underinvest in early-stage R&D.[2] Yet, critics contend that R&D subsidy programs crowd out private investment or allocate funds inefficiently (Lerner 2009). Despite opposing theoretical arguments, we have little empirical evidence about the effectiveness of R&D subsidies. There is also little work on the extent to which small, innovative firms suffer from financing constraints.

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 innovative, financial, and commercial success. The study is based on a proprietary dataset of applications to the US Department of Energy's

* New York University Stern School of Business, 44 West 4th Street, New York, NY 10012 (e-mail: sabrina.howell@nyu.edu). I thank David Scharfstein, Josh Lerner, Ramana Nanda, Raj Chetty, and Joseph Aldy. I am also grateful to David Yermack, Adi Sunderam, Jeremy Stein, John Van Reenen, Saul Lach, Larry Katz, Deepak Hegde, Edward Glaeser, Jeffrey Furman, Lee Fleming, Gary Chamberlain, seminar participants, and anonymous referees. Finally, I thank current and former Department of Energy employees Jamie Vernon, Teryn Norris, Tina Kaarsberg, Carl Hebron, Carla Frisch, Matthew Dunne, Jeffrey Dowd, and Ken Alston. This project received funding from the Harvard Lab for Economic Applications and Policy and a NSF Graduate Research Fellowship. The author declares that she has no relevant or material financial interests that relate to the research described in this paper.

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[1] In addition to the federal SBIR, many US 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 Audretsch, Keilbach, and Lehmann (2006); Akcigit and Kerr (2010); and Haltiwanger, Jarmin, and Miranda (2013). 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 (Holmstrom 1989).

(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.

The SBIR program, which exists at ten federal agencies besides DOE, has two stages. Firms first apply to a subsector-specific Phase 1 competition for awards of $150,000. Phase 1 winners may apply nine months later for a $1 million Phase 2 award, which successful applicants receive two to three years after Phase 1. DOE officials rank firms within competitions. While the grant application describes precisely how firms plan to use the money, this use is not enforced ex post. The cutoff decision is exogenous to these ranks. I exploit the ranks in a sharp regression discontinuity design that compares firms immediately around the award cutoff. Jaffe (2002) proposes this approach to resolve the selection problem in R&D grant evaluation.

The Phase 1 award has powerful effects. First, it increases a firm's subsequent cite-weighted patents by at least 30 percent. Second, a Phase 1 grant increases a firm's chance of receiving venture capital (VC) investment from 10 percent to 19 percent, and also increases the amount of money raised and the number of deals. Within two years of the grant, the effects on cite-weighted patents and VC are just over one-half of their long-term effect. Third, a Phase 1 grant almost doubles the probability of positive revenue and, conditional on positive revenue, increases it by 30 percent. Finally, Phase 1 increases the probability of survival and successful exit (IPO or acquisition).

Thus, on average the early-stage grants do not crowd out private capital. Instead they enable new technologies to go forward, transforming some awardees into privately profitable investment opportunities. In contrast, the larger Phase 2 grant has no measurable effect, except for a small positive effect on cite-weighted patents. Almost 40 percent of Phase 1 winners do not apply to Phase 2, and these are disproportionately VC recipients. Phase 2 eligibility criteria apparently generate this adverse selection in application rates.

Heterogeneity across firm types in the Phase 1 grant impact is consistent with the grant easing financing constraints. Firms that require more upfront capital should be more financially constrained, and I show that the grant is more useful for hardware than software firms in raising VC. Young firms, firms with little experience, and firms in emerging sectors should also face greater financing constraints. Indeed, the grant effects on VC and survival decline with age, previous cite-weighted patents, and sector maturity.

The Phase 1 grant might ease financing constraints through a certification mechanism. The government's decision could convey positive information to investors about the firm's technology. Alternatively, the money itself may be useful in transforming a project from negative to positive net present value. Such a funding mechanism 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 test for certification by asking 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. Conditional on win status, ranks are relatively uninformative about outcomes. Interviews with investors and further empirical evidence indicate that the grant neither serves as a signal nor acts through the equity channel.

Instead, the evidence is most consistent with the prototyping channel, where the grant enables proof-of-concept work that the firm cannot otherwise finance. Prototyping depends on firms using the grant for R&D (use is not monitored or enforced). In a survey of post-2004 grantees, respondents overwhelmingly reported using Phase 1 money for either basic research on a new technology or testing and demonstrating an existing technology. Phase 2 grants were more often used for patent applications and overhead than Phase 1 grants.

This study's main policy implication is that the SBIR program—and perhaps similar programs—could achieve better outcomes by reallocating money (i) from larger, later stage grants to more numerous small, early-stage grants; and (ii) from older firms and regular winners to younger firms and first-time applicants. Beyond the scope of this paper are optimal program size and whether the government should in general subsidize private R&D.

The R&D subsidy program evaluation literature includes Lerner (2000) and Wallsten (2000), who examine SBIR awardees. Bloom, Schankerman, and Van Reenen (2013) study R&D spillovers using US tax credit changes. Other studies address non-US R&D programs, such as Lach (2002); Almus and Czarnitzki (2003); Bronzini and Iachini (2014); Jaffe and Le (2015); and Bronzini and Piselli (2016).[3] This paper also 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.[4] An exception is Bond, Harhoff, and Van Reenen (2005), who find evidence consistent with financial constraints discouraging investment in R&D.

Universities and national labs must undertake basic R&D. Startups are an important middle ground between these institutions and large firms, which can efficiently conduct applied, market-oriented R&D (Griliches 1998 and Aghion, Dewatripont, and Stein 2008). Early-stage grants to small firms relieve a critical liquidity constraint on R&D investment in a capital-intensive sector.

More specifically, the energy context of this paper relates to an important research agenda on innovation to mitigate climate change. Acemoglu et al. (2012, 2016) model the competition between clean and dirty technologies in production and innovation, and ask whether the social planner should use carbon taxes or research subsidies. Acemoglu et al. (2016, p. 101) find "that optimal policy heavily relies on research subsidies." Their estimates are sensitive to the choice of R&D elasticity. I provide evidence that R&D subsidies can increase clean innovation. The Phase 1 grant is most useful in sectors likely to have large positive spillovers: hydropower, carbon capture and storage, building and lighting efficiency, and automotive

[3] Other work includes González and Pazó (2008); Link and Scott (2010); and Fracassi et al. (2016). Econometrically, my method is similar to Jacob and Lefgren (2004) and Li (2015), who examine National Institutes of Health research grant applications.

[4] 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., Rauh 2006; Whited and Wu 2006). However, identification has been challenging, and there is little evidence on small or private firms (see Hall 2010).

technologies. There is no measurable effect for conventional energy technologies, like natural gas and coal, suggesting that they are not as financially constrained.

The paper proceeds as follows. In Section I, I describe the data. Section II explains the regression discontinuity design and establishes its validity. The results for Phase 1 and 2 are in Sections III and IV, respectively. Section V examines the grant mechanism. Robustness tests are in Section VI.

I. Context and Data Sources

Grants are a significant funding source for high-tech entrepreneurs. The largest single provider in the United States is the SBIR grant program, which disburses around $2.2 billion each year. Congress first authorized the SBIR program in 1982 to strengthen the US high technology sector and support small firms. Eleven federal agencies must allocate 2.7 percent of their extramural R&D budgets to the SBIR program; the required set-aside increases to 3.2 percent in 2017. Though important in its own right, the SBIR program is also representative of many targeted subsidy programs for high-tech new ventures at the state level and around the world.

The SBIR program has two Phases. Phase 1 grants of $150,000 are supposed to fund nine months of proof-of-concept work (the amount increased in two steps from $50,000 in 1983). Phase 2 grants of $1 million, awarded about two years after Phase 1, aim to fund later stage demonstrations. The application process is onerous, taking a full-time employee one to two months.[5] This application cost likely biases the applicant pool, potentially toward firms with positive NPV investment opportunities that do not have access to low-cost external finance.

The firm proposes to use the grant for R&D in its application, but there is no monitoring or enforcement once the firm receives the lump sum. However, to apply for Phase 2 a firm must (i) demonstrate progress on the Phase 1 project; and (ii) not be more than 50 percent owned by a single outside private equity investor. For both phases, eligible firms are for-profit, US-based, and majority US-owned. There is no required private cost sharing, and the government takes no equity and demands no rights to IP.

Each year, DOE officials in technology-specific programs (e.g., Solar) announce competitions in granular subsectors like Solar Powered Water Desalination and Improved Recovery Effectiveness In Tar Sands Reservoirs. Program officials rank applicants within each competition based on written expert reviews and their own discretion, according to three criteria: (i) strength of the scientific/technical approach; (ii) ability to carry out the project in a cost effective manner; and (iii) commercialization impact (Oliver 2012). The program official does not know the award cutoff (the number of grants in a competition) when she conducts the ranking. She submits ordered lists to a central DOE SBIR office, which determines the cutoff.[6]

[5] Applicants must describe the project and firm in detail, and provide an itemized budget for the proposed work. There are over 100 pages of instructions on DOE's SBIR Phase 1 application website. Interviews with grantees confirmed the 1–2 month time-frame.

[6] The cutoff in a competition is based on budget constraints. Ranking occurs before the SBIR office determines how many awards to allocate to each program and competition. Interviews with DOE officials indicated that the cutoff decision is exogenous to the ranking process. Some ranking data provided in the form of e-mails from program officials to the SBIR office also support exogeneity. Observable variables do not predict competition cutoffs. Average award numbers do not vary systematically by office or competition subsector. The budget for

Table 1—Summary Statistics

Panel A. Application data from DOE

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 ($\geq$ 1 award)	428
Average Phase 1 applicants per competition	11 (8.3)
Average Phase 1 awards per competition	1.7 (1.1)
Phase 2 applications used in RD	919

Panel B. Variables used in analysis from non-DOE sources

	Type	Mean	SD	Median	Observations
Pre-award venture capital (VC) investment	0–1	0.083	0.27	0	5,021
Pre-award venture capital deals	Count	0.25	1.3	0	5,021
Pre-award cite-weighted patents	Count	21	122	0	5,021
Pre-award patents	Count	1.9	7.5	0	5,021
Pre-award acquisition or IPO	0–1	0.033	0.18	0	5,021
Post-award VC $\left(VC_i^{post}\right)$	0–1	0.11	0.31	0	5,021
Post-award VC (mill 2012\$) $(VC\,Amt_i^{post})$	Cont.	2.7	26	0	4,964
Post-award VC deals $(VC\,Deals_i^{post})$	Count	0.32	1.4	0	5,021
Post-award cite-weighted patents $\left(Cites_i^{post}\right)$	Count	12	117	0	5,021
Post-award patents	Count	2	11	0	5,021
Post-award acquisition or IPO $(Exit_i^{post})$	0–1	0.034	0.18	0	5,021
Revenue as of 2016 in \$ millions $(Revenue_i)$	Cont.	2.0	6.6	0.20	3,583
Survival as of 2016 $(InextrmBus_i^{post})$	0–1	0.67	0.47	1	3,880
Probability in major metro area (top 6)	0–1	0.30	0.46	0	5,021
Age (years)	Count	9.5	11	6	3,427
Probability tech is hardware $(Hardware_i)$	0–1	0.43	0.49	1	2,571
Probability new subsector $(Emerging\ Sector_i)$	0–1	0.58	0.49	1	2,571
Probability minority owned	0–1	0.077	0.27	0	1,722
Probability woman owned	0–1	0.084	0.28	0	1,722
All-government SBIR wins $(SBIR_i)$	Count	10	36	0	5,021
Future patents in modal class	Count	9,758	11,809	5,453	1,583
MSA VC investment 2011 (\$millions)	Cont.	851	1,570	0	4,950
MSA median per cap. income 2011 (in \$thousands)	Cont.	56	14	56	4,603

Note: This table summarizes the DOE SBIR application data in panel A, and variables used in the regression analysis in panel B.

I use complete data from the two main applied offices at the DOE: Fossil Energy (FE) and Energy Efficiency and Renewable Energy (EERE). Together, they awarded \$884 million (in 2012 US\$) in SBIR grants between 1983 and 2013. Online Appendix Figure 1 shows the number of applicants by office and award status. The data include the applicant's company name, address, funded status, and award notice date. While awards are public information, the ranks and losing applicant identities are indefinitely secret.[7] I have ranking data from 1995, so analysis begins then.

Panel A of Table 1 contains summary statistics about the applications and competitions. Each competition has on average 11 applicants. Over 70 percent of firms applied only once, and 14 percent applied twice. Panel B shows summary statistics

each contest is set at the beginning of the year based on the budget for the program office (e.g., Solar), which overwhelmingly goes to other line items, like the national labs. The SBIR program is just under 3 percent of each office's budget, as mandated by Congress.

[7] Only in my capacity as an unpaid DOE employee was I able to use this data.

used in estimation. $Emerging\ Sector_i$ is an indicator for the technology being relatively new, without a well-developed supply and demand infrastructure.[8]

Patents and their citations are proxies for innovation. Using comprehensive patent data from 1976 to 2014 from Berkeley's Fung Institute, 1,471 post-1995 applicant firms were matched to at least one nonreissue utility patent.[9] To control for patent quality, I weight patents by their future citations, as in Aghion, Van Reenen, and Zingales (2013) and Bloom, Schankerman, and Van Reenen (2013). I do not normalize the patent count by classification or year because competition fixed effects control for subsector and date.

Of the post-1995 applicant companies, 838 were matched to private financing deals (online Appendix Table 1).[10] The primary outcome variable is an indicator for whether the firm received angel or VC investment after the competition's award date (VC_i^{post}). The amount exists for 57 percent of deals, but valuation data are rare. One issue in using dollars without the equity stake is that it would conflate the firm's success at raising private investment with its capital intensity.

Data were manually collected on firm technology type, operating status, and most recent year revenue as of January 2016 for all post-2000 applicants (2,994 firms).[11] Of these, 1,737 were still in business (survived), 1,721 had positive revenue, 1,000 were out of business and assigned zero revenue, 744 produced primarily hardware, and 971 primarily software ($Hardware_i = 0$). Companies with ambiguous technologies or that produced both were omitted. Finally, I use metropolitan statistical area (MSA)-level data from the Federal Reserve Economic Data research center.

II. Empirical Strategy

The ideal experiment would observe whether young, small, energy technology firms invest exogenous cash in R&D. If they do, costly external finance must have prevented the firm from exploiting existing profitable investment opportunities. I provide a novel and plausibly exogenous cash flow shock to identify a causal relationship between financing constraints and investment responses.

Specifically, I employ a regression discontinuity (RD) design, which estimates a local average treatment effect around a cutoff in a rating variable. Since the number of applicants and awards varies across competitions, I center ranks around zero or use percentile ranks. The lowest-ranked winner i in competition c has centered rank $(rank_{ic}) = 1$, and the highest-ranked loser has $rank_{ic} = -1$. 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.

[8] Categorization is based on competition topics. $Emerging\ Sector_i = 1$ for solar, wind, geothermal, fuel cells, carbon capture and storage, biomass, or hydro/wave/tidal (886 companies); 0 for oil, gas, coal, biofuels, or vehicles/motors/engines (481 companies). Unclear firms and/or topics are excluded.

[9] I use the patent application date. Subsidiary patents are not a problem as the firms are overwhelmingly small, private, and single-unit.

[10] Companies were matched on name and state using ThompsonOne, Preqin, Cleantech Group i3, CrunchBase, and CapitalIQ, and then hand-checked for accuracy. The $VC\ Amt_i^{post}$ variable omits firms that raise VC after but not before the award, for which there is no deal amount available.

[11] This data was collected from D&B and company websites. A company is in business (survived) if it has an active website. Initial searches suggested that pre-2000 data were rarely available, so for economy of effort the research began for applicants in 2000.

I estimate variants of equation (1), where Y_i^{Post} is regressed on grant award status and a polynomial $f(\cdot)$ in the firm's rank within the competition.[12] I include competition fixed effects δ_c, which are date-specific. Some specifications use controls X_i.[13] My estimations use ordinary least squares (OLS) where possible.[14] When appropriate for count data, I use other models. I cluster standard errors by sector-year in the main specification

$$(1) \qquad Y_i^{Post} = \alpha + \tau[\mathbf{1} \mid rank_{ic} > 0] + f(rank_{ic}) + \gamma X_{ic} + \delta_c + \varepsilon_{ic}$$

$$\text{where } -r \leq rank_{ic} \leq r.$$

A valid RD design requires that treatment not cause rank. This is not a problem here, as the award decision happens after ranking and I exclude previous winners in the primary specification. Ranks are ordinal in my data, rather than cardinal as in most RD contexts. On average the differences in the true distance between ranks should be the same. That is, errors in differences on either side of cutoff in any given competition should average zero. An important data limitation is the rating variable's discreteness. Lee and Card (2008) note that discrete rating variables can require greater extrapolation of the outcome's conditional expectation at the cutoff, though the fundamental econometrics are not different. 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.[15]

The primary concern is whether firm ranks are manipulated around the cutoff. The cutoff in a valid RD design must be exogenous to rank (Lee and Lemieux 2010). In my setting, manipulation could happen either if (i) higher quality firms put more effort into their applications; or (ii) the program official manipulated applicants around the cutoff. Both are difficult, since neither the applicant firms nor the program officers (DOE civil servants) know the selection rule (see Section I).

I deploy five tests for manipulation. First, online Appendix Figure 2 shows the density of applicants by normalized rank. There is no obvious discontinuity around the cutoff.[16] Second, I visually demonstrate smoothness in observable baseline covariates (Figure 1) and pre-assignment outcome variables (Figures 2A and 3A). Third, I predict the probability of subsequent VC with baseline covariates and sort

[12] Standard RD implementation pools the data but allows the function to differ on either side of the cutoff (Imbens and Lemieux 2008). My main results are robust to different slopes on either side of the cutoff, but the primary specification relies on global polynomials due to the small number of ranks on the award side.

[13] The RD design does not require conditioning on baseline covariates, but doing so can reduce sampling variability. Online Appendix Table 2 projects rank on observable covariates. Previous non-DOE SBIR awards and VC predict rank, so they are included in some specifications. Lee and Lemieux (2010) advise including the pre-assignment dependent variable as they are usually correlated.

[14] I use OLS in my primary specification because many of the groups defined by fixed effects (competitions) have no successes (e.g., no subsequent VC). Logit drops these groups. Also, OLS does as well as logit if not better in estimating marginal effects (Angrist 2001).

[15] The unrestricted regression projects the outcome on dummies for each of K ranks. The restricted regress is a polynomial like equation (1). 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, N is the number of observations, and P is the number of restricted parameters. G takes an F-distribution. The null hypothesis is that the unrestricted model does not provide a better fit. If G exceeds its critical value, I reject the null and turn to a higher order polynomial.

[16] Unfortunately, the discreteness of the running variable prevents a McCrary density test.

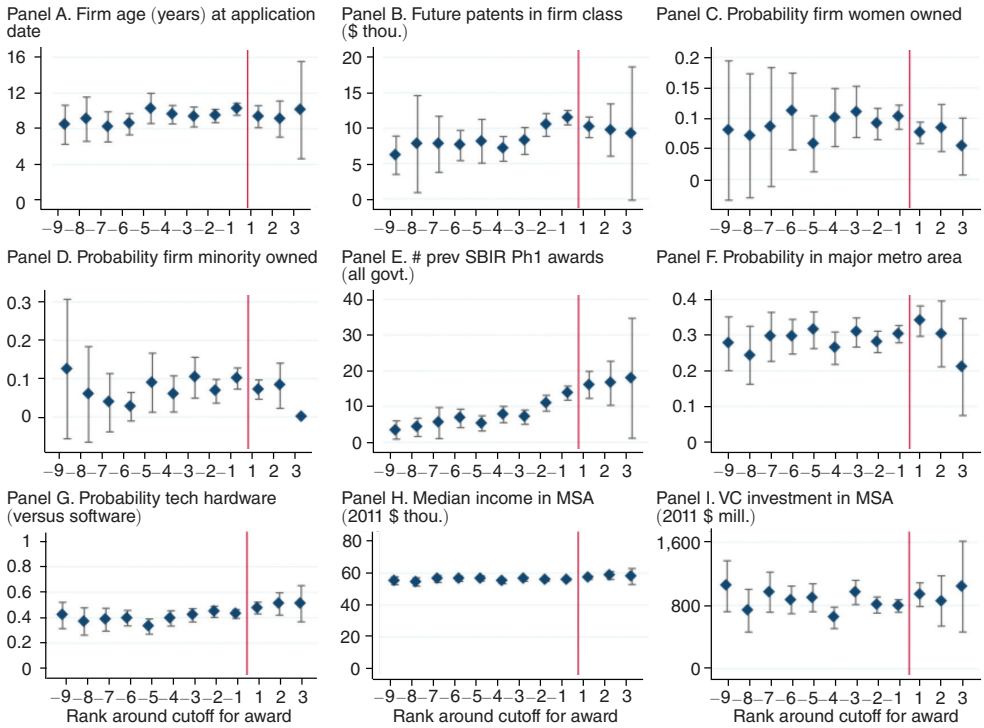

FIGURE 1. CONTINUITY IN OBSERVABLE COVARIATES

Notes: This figure shows covariates at the award date. Ninety-five percent confidence intervals shown.

the resulting coefficients by the applicant's rank.[17] There is no discontinuity. Fourth, I conduct a t-test for matched pair differences of means in baseline covariates immediately around the cutoff. In no case can I reject the null hypothesis that the means are the same for both one- and two-tailed tests, except for the one-tailed test on previous citations, which is significant at the 10 percent level (online Appendix Table 3).

Fifth, I test whether awardees' underlying technologies are more predisposed to growth than non-awardees'. The proxy for future innovation growth in a sector is the number of future patents in the firm's modal patent subclass. Online Appendix Table 4 and online Appendix Figures 4 and 5 show that future patents do not predict rank or award. The latter figure is a 3D graph depicting the number of firms with a certain modal subclass and future patents in that subclass around the cutoff for award.

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.

[17] I regress the outcome on covariates using $Y_{ic}^{Post} = \alpha + X_i\phi + \delta_c + \varepsilon_{ic}$. I use the estimated coefficient vector to predict an applicant's probability of subsequent VC financing: $\hat{Y}_{ic}^{Post} = \hat{\alpha} + X_i\hat{\phi} + \hat{\delta}_c$. The averaged probabilities by rank are in online Appendix Figure 3.

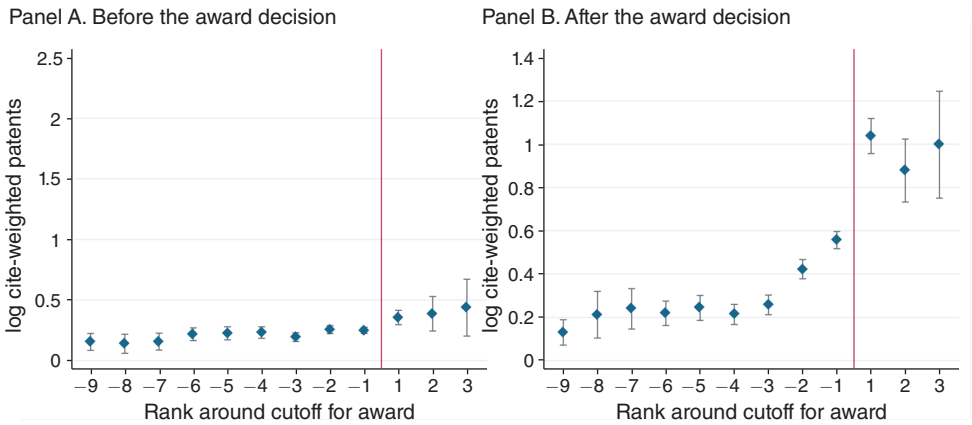

FIGURE 2. CITE-WEIGHTED PATENTS BEFORE AND AFTER PHASE 1 GRANT BY RANK

Notes: This figure shows $\ln(1 + Cites_t^{post})$ before and after the Phase 1 grant award decision, using the patent application date. DOE's rank is centered so $rank_{ic} > 0$ indicates a firm won an award. Ninety-five percent confidence intervals shown.

III. The Phase 1 Grant Impact on Firm Outcomes

A. *Innovation*

The best available measure for innovation is patenting. Though patents are not the only way that firms protect IP, they are positively associated with economic value creation and stock market returns (Hall, Jaffe, and Trajtenberg 2005). Following Aghion, Van Reenen, and Zingales (2013), I focus on cite-weighted patents using negative binomial and OLS models (the Pearson goodness-of-fit χ^2 suggests that the data are excessively dispersed for the Poisson regression model). Figure 2 shows log cite-weighted patents ever before and ever after the Phase 1 grant.

DOE does not rank based on previous patenting, but ranks among high-ranking losers are predictive of future patenting. This relationship disappears for winners. The goodness-of-fit test identifies the appropriate polynomial in rank as quadratic.[18]

Results from estimating variants of equation (1) are in Table 2. I use the narrowest bandwidth of one rank around the cutoff in each competition, except in column 3 where I use all the data with quadratic rank controls. In the primary sample of no previous winners and using the negative binomial specification, a Phase 1 award increases cite-weighted patents by 2.5 times (panel A, columns 1–4).[19] The OLS specification finds that the grant increases log cite-weighted patents by about 30 percent (panel B, columns 1–3). Note that the linearization reduces the effect. All successful patents filed after the competition's award date are used, as competition

[18] The critical value for rejecting the null with 85 percent confidence (a conservative approach) is 1.28. Without rank controls, the G-statistic is 1.44. With quadratic controls it is 1.05.

[19] Coefficients indicate, for a one unit increase in regressor, the difference in the logs of expected counts. If λ is the Poisson rate (# patents), $\tau = \log\left(\frac{\lambda_{R_{ic}>0}}{\lambda_{R_{ic}<0}}\right)$. Exponentiating gives the incidence rate ratio (how many times more patents winners get than losers).

Table 2—Impact of Phase 1 Grant on Cite-Weighted Patents

Sample:	No previous winners				All	No prev. apps	>2 prev. wins
Bandwidth:	1	1	All	All	1	1	1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Negative binomial model; dependent variable is $Cites_i^{post}$							
Award	0.93	0.91	0.92	0.94	0.82	2.1	0.40
	(0.21)	(0.19)	(0.33)	(0.26)	(0.13)	(0.34)	(0.14)
Normalized rank			0.052				
			(0.074)				
Normalized rank^2			−0.0072				
			(0.0045)				
Rank quintiles	No	No	No	Yes	No	No	No
Controls[a]	No	Yes	No	No	No	No	No
Sector-year fixed effects[b]	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,871	1,871	5,021	5,021	2,714	972	1,477
R^2	0.056	0.084	0.053	0.053	0.034	0.080	0.035
Panel B. OLS models; dependent variable is $\ln(1 + Cites_i^{post})$							
Award	0.33	0.27	0.29	0.22	0.29	0.49	0.22
	(0.15)	(0.13)	(0.11)	(0.087)	(0.087)	(0.21)	(0.13)
Normalized rank			−0.026				
			(0.02)				
Normalized rank^2			7.8e-4				
			(0.0011)				
Rank quintiles	No	No	No	Yes	No	No	No
Controls[a]	No	Yes	No	No	No	No	No
Competition fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,872	1,872	5,021	5,021	2,714	972	1,477
Pseudo-R^2	0.46	0.60	0.53	0.351	0.63	0.71	0.75

Notes: This table reports regression estimates of the Phase 1 award effect on cite-weighted patents using variants of equation (1). The model is negative binomial in panel A and OLS in panel B. 1–4 limit the sample to firms that have not previously won an award, but may have previously applied. 5–7 respectively use the whole sample, only firms that have never previously applied, and only firms that have more than two previous wins.

[a]Controls are $Cites_i^{prev}$ or $\ln(1 + Cites_i^{prev})$ and previous non-DOE SBIR awards.

[b]Competition fixed effects in the NB model do not permit convergence. Standard errors are clustered by sector-year. Year $\geq$ 1995.

fixed effects control for years since the grant. However, the time fixed effects do not necessarily control for when the firm patents. In unreported specifications, I find similar results when citations are limited to three years after the patent. In an additional unreported regression, I find that the grant increases the probability of positive patenting by 9 percentage points (pp). Rank is uninformative in all models.

Centering ranks might obscure information in the raw rank. For example, firms with centered ranks of two might have different qualities in competitions with two and four awards. I address this by using dummies for the firm's rank quintile within the competition (panels A and B, column 4).[20] Conditional on award status, there is no information in rank visually or in regressions, regardless of bandwidth. The goodness-of-fit test reveals that once I control for award, no function is too restrictive.

[20]I find similar results with the slope controlled for separately on each side of the cutoff (with a bandwidth of all specification) and using quartile ranks.

Column 6 limits the sample to first-time applicants, and yields a significantly larger effect than the standard sample. When only firms with more than two wins are included (column 7), the effect declines by more than one-half. Further, the interaction between award and the number of previous non-DOE awards is strongly negative (online Appendix Table 5). For each previous SBIR award, the effect of an award on log cite-weighted patents declines by 20 percent. I find a similar pattern for other outcome variables, but it is especially troubling for patenting. A small subset of applicants wins many awards and may be dependent on grants. While such firms might naturally not be seeking VC or direct sales, if their R&D is productive it should yield patents. Instead, there is a steeply declining benefit of additional grants to the same firm.

B. *Finance*

Not all SBIR applicants are startups, and not all seek external finance. However, a majority of the firms analyzed here qualify as startups; they are young, small, and have significant growth potential that is not limited to a single geographic location.[21] The median firm age is six years, with many less than one year old. The literature has used SBIR winners as representative samples of high-tech entrepreneurial ventures (Gans and Stern 2003; Hsu 2006).

Venture capital is an important source of external funding for startups; lesser sources include partnering with large companies and venture debt. The literature has established that venture capitalists are important intermediaries in the US innovation system. They select innovative firms and bring new technologies to market quickly (Hellmann and Puri 2000; Sørensen 2007). They facilitate access to debt finance and provide nonmonetary resources like governance, and networking (Hochberg, Serrano, and Ziedonis 2014). Also, the DOE aims to mobilize private investment. VC is therefore an appropriate outcome metric. Observing VC tests whether the grants crowd out private investment, indicates that the company presents a privately profitable opportunity, and is a good early-stage proxy for market success in a context where outcome data are difficult to collect.

Visual evidence for the Phase 1 grant's effect on VC is in Figure 3. The probability of subsequent VC jumps from about 10 percent to 20 percent around the grant cutoff. Estimates in panel A of Table 3 range from 7 to 12 pp; my preferred estimate of 10 pp uses the narrowest bandwidth (columns 1–2). The goodness-of-fit test shows that once I control for award, no function is too restrictive.[22] Quintile ranks yield stable grant effects of 9–10 pp regardless of bandwidth.

Including previous winners and excluding previous applicants have little effect on the results (panel B, columns 1–4), but when I include firms with more than two previous wins (columns 5–6) the effect decreases and loses significance. Online Appendix Table 5 shows that the interaction between award and previous wins is −4 pp. I also estimate the grant effect on the log amount of VC and on the number

[21] For example, among the 23 solar firms that have ever had an IPO, 9 appear in my data; SBIR winners include Sunpower, First Solar, and Evergreen Solar.

[22] The G-value with no control for rank is 0.0014, three orders of magnitude less than the critical value.

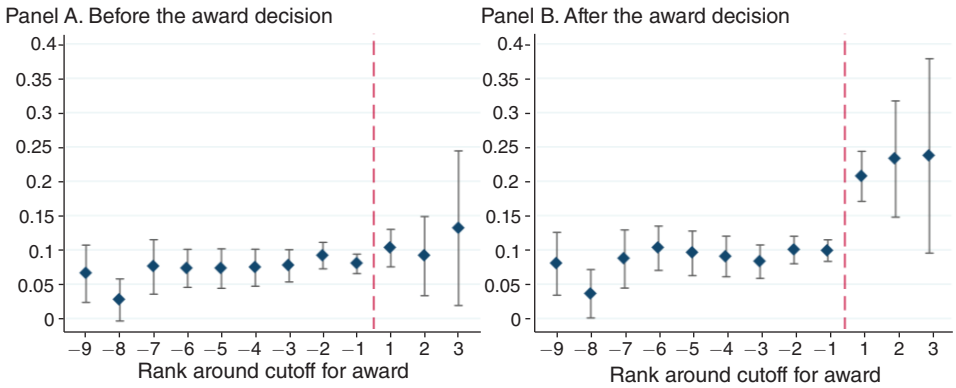

FIGURE 3. PROBABILITY OF VENTURE CAPITAL BEFORE AND AFTER GRANT BY RANK

Notes: This figure shows the fraction of applicants who received VC before and after the Phase 1 grant. Ninety-five percent confidence intervals shown.

TABLE 3—PHASE 1 GRANT IMPACT ON SUBSEQUENT VENTURE CAPITAL INVESTMENT

Panel A							
Dependent variable:	VC_i^{post}					$\ln(1 + VC\ Amt_i^{post})$	
Bandwidth:	1		3		All	1	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Award	0.098	0.10	0.12	0.094	0.072	0.10	1.40
	(0.032)	(0.033)	(0.058)	(0.033)	(0.033)	(0.028)	(0.5)
Normalized rank			−0.029		0.0086		
			(0.033)		(0.0071)		
Normalized rank^2			0.012		−7.4e-5		
			(0.0088)		(4.3e-4)		
Rank quintiles	No	No	No	Yes	No	Yes	No
Controls[a]	Yes	No	Yes	Yes	Yes	Yes	No
Competition fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,872	1,872	3,368	3,368	5,021	5,021	1,843
R^2	0.47	0.42	0.35	0.35	0.27	0.27	0.42

Panel B							
Dependent variable:	VC_i^{post}						$VC\ Deals_i^{post}$
Sample:	All applicants		No prev. apps		>2 prev. wins		No prev. wins
Bandwidth:	1	All	2	All	1	All	1
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Award	0.11	0.075	0.11	0.10	0.080	0.074	0.93
	(0.023)	(0.026)	(0.037)	(0.048)	(0.048)	(0.053)	(0.19)
Normalized rank, normalized rank^2	No	Yes	No	Yes	No	Yes	No
Competition fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	No[b]
Observations	2,714	6,400	1,514	2,951	1,246	2,322	1,872
R^2	0.37	0.19	0.50	0.28	0.51	0.35	0.051 (Pseudo- R^2)

Notes: This table reports regression estimates of the Phase 1 award effect on subsequent VC investment using variants of equation (1). The model is OLS, except for column 7 in panel B, which is negative binomial. Panel A varies the bandwidth around the cutoff and controls in columns 1–6, and uses the amount of VC investment as dependent variable in 7. Panel B varies the sample (as in Table 2) in 1–6, and uses the number of VC deals as dependent variable in column 7.

[a] Controls are VC_i^{prev} and previous non-DOE SBIR awards.

[b] This model uses year-sector fixed effects. Standard errors are clustered by sector-year. Year $\geq$ 1995.

of deals (panels A and B, column 7).[23] The grant generates over 100 percent more VC investment in dollars and 2.4 additional VC deals. When I include all private financing events, such as IPOs, acquisitions, and debt, I find a slightly larger effect of about 12 pp (online Appendix Table 6).[24]

A grant might increase a winner's chance of VC by decreasing the losers' chance. To test for such reallocation of capital, I first ask whether the effect varies with the competition's number of awards. Negative spillovers should increase with the number of winners, because competitions are defined by narrow subsectors. When the sample is limited to losers, dummies for the number of awards have no predictive power, suggesting that spillovers do not explain the main effect (online Appendix Table 7, columns 1–4).

Second, I exploit the stylized fact that VC firms typically invest in geographic proximity to their offices, and indeed in ventures located in their city (Chen et al. 2010; Cumming and Dai 2010). When applicants are near each other, they are more likely to seek investment from the same VC firms, so reallocation should increase the grant effect. In fact, when competing firms are from the same metro area, the effect is not significantly higher (online Appendix Table 7, columns 5–7). Negative spillovers within competitions do not appear present, but future research is needed to assess whether capital is reallocated from non-applicant firms.

C. *Revenue*

The most direct measure of technology commercialization is revenue. Figure 4 shows most recent year revenue as of January 2016.[25] There is no slope in rank, but there is a clear jump at the cutoff. Results using OLS, zero-inflated negative binomial (ZINB), and Tobit models are in Table 4. The regressions include time controls so recent competitions are not advantaged. With OLS, the effect of Phase 1 is \$1.3–\$1.7 million, relative to a mean of \$2 million (panel A, columns 1–2). With log revenue as the dependent variable and zeros omitted, the effect is 19 percent (column 3).

Although this result is less compelling because there is no pre-award data, the grant appears to propel firms from zero revenue to positive revenue. This is clear from the ZINB model, which provides two estimates of the award effect: first a logistic portion predicting the likelihood of a zero, and then a full model predicting revenue.[26] Columns 4–6 of Table 4, panel A show that the grant decreases the odds that a firm has zero revenue by two times (exponentiating the most conservative coefficient, -0.67). If a firm is not in the certain zeros group, the award increases revenue by about 30 percent (exponentiating 0.25). The Tobit model in panel B,

[23] Panel A, column 7 omits firms that raise VC after but not before the award, where there is no deal amount available. I find similar results in a zero-inflated model. Panel B, column 7 uses a negative binomial model because the dependent count variable is overdispersed.

[24] Sample splits by time period finds the largest effects between 2009 and 2013 (19 pp) and the smallest between 2000 and 2004 (5 pp), perhaps because VC firms reduced investing when the Internet bubble collapsed. Between 2009 and 2011, when DOE funding was unusually high, the estimated grant effect is 13 pp.

[25] I would deduct the grant amount from total postgrant revenue if I observed annual revenue each year. Unfortunately this is not meaningful with only most recent year revenue.

[26] Negative binomial because the dependent variable is again overdispersed, and zero-inflated because the Vuong test statistic indicates it is preferred to the standard version.

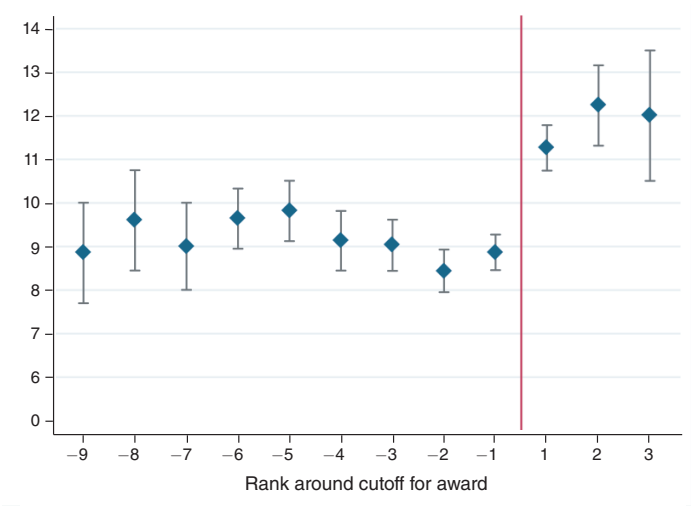

FIGURE 4. LOG REVENUE (*in $millions*)

Notes: This figure shows most recent year revenue as of January 2016 among post-2000 applicants. Out-of-business firms assigned zero revenue. Ninety-five percent confidence intervals shown.

TABLE 4—PHASE 1 GRANT IMPACT ON REVENUE (*in $millions*)

Panel A Model:	OLS			Zero-inflated negative binomial (ZINB)		
Bandwidth:	1	All	2	1	All	All
	(1)	(2)	(3)	(4)	(5)	(6)
Award[a]	1.7	1.3	0.19	0.25	0.29	0.34
	(0.93)	(0.58)	(0.11)	(0.12)	(0.13)	(0.096)
Award (logistic ZINB)				−0.67	−0.82	−0.76
				(0.16)	(0.17)	(0.13)
Norm. rank, Norm. rank^2	No	No	No	No	Yes	No
Rank quintiles	No	Yes	No	No	No	Yes
Controls[b]	No	No	No	No	Yes	No
Competition fixed effects	Yes	Yes	Yes	No	No	No
Sector and year fixed effects	No	No	No	Yes	Yes	Yes
Observations	1,108	3,942	1,176	1,108	3,942	3,942
R^2 /Dev.-based R^2	0.40	0.14	0.16	0.11	0.10	0.06

(*Continued*)

columns 1–3 finds a treatment effect of $2.3 million; the larger effect is because zero values are treated as censored, rather than true zeros. As with financing, rank has no predictive power.

The relationship between previous awards and revenue is less clear than it is for the other outcomes. Panel B, columns 4–6 split the sample. For firms with more than two previous wins, the logistic portion is slightly larger and remains highly significant; online Appendix Table 5 shows that the interaction between the grant effect and previous SBIR awards is negative but not significantly so.

Table 4—Phase 1 Grant Impact on Revenue (*in $millions*) (*Continued*)

Panel B Model:	Tobit			Zero-inflated negative binomial (ZINB)		
Bandwidth:	1	All	All	1	1	1
Sample:	No previous winners			All apps	No prev. apps	>2 prev. wins
	(1)	(2)	(3)	(4)	(5)	(6)
Award[a]	2.3	2.5	2.5	0.53	0.39	0.34
	(0.72)	(0.72)	(0.59)	(0.10)	(0.16)	(0.26)
Award (logistic ZINB)				−0.91	−1.0	−1.0
				(0.14)	(0.20)	(0.33)
Norm. rank, Norm. rank^2	No	Yes	No	No	No	No
Rank quintiles	No	No	Yes	No	No	No
Sector and year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,108	3,942	3,942	1,384	657	329
Pseudo R^2/Dev-based R^2	0.002	0.001	0.001	0.10	0.17	0.18

Notes: This table reports regression estimates of the Phase 1 award effect on revenue (in millions of nominal $) using variants of equation (1). Panel A varies the bandwidth around the cutoff and uses OLS (columns 1–3) and negative binomial (columns 4–6) models. Panel B, columns 1–3 use Tobit models. Panel B, columns 4–6 vary the sample (as in Table 2).
[a]Controls are VC_i^{prev} and previous non-DOE SBIR awards. * Sample is restricted to firms with revenue > 0.
[b]In columns 4–6, this coefficient is on the full ZINB model. Sector and year fixed effects used where ML estimator does not converge with competition fixed effect. Standard errors are clustered by sector-year. Year $\geq$ 1995.

D. *Successful Exit and Survival*

Last, I examine exit and survival. VC investors usually liquidate successful investments through an IPO or acquisition.[27] The treatment effect on such exits is 4.5 pp, indicating that the probability of exit increases from about 3 percent to 7.5 percent (Table 5, columns 1–4). However, this result is less visually consistent than the others (see online Appendix Figure 6B).

I find that a grant increases the likelihood a firm remains in business as of early 2016 by 12–14 pp, relative to a mean of 67 percent. Survival is measured as of 2016. For both exit and survival, rank has no predictive power. Across all outcomes, the rank data indicate that officials do not or cannot choose firms based on their likelihood of success. This supports Lerner's (2000) argument against selection in SBIR grant making.

In unreported tests, I ask whether the grant's effect on survival decays with the number of years since the award. I split the sample by time period and interact the award with the number of years since the grant. The former approach does not find significant differences in the grant effect over time. The magnitude is somewhat larger in earlier years, in contrast to the VC sample splits (see footnote 24). The latter approach yields a zero coefficient on the interaction, and leaves the grant effect itself unchanged.

[27] As in much of the literature, I am unable to identify whether acquisitions are high return events. However, even an acquisition that is unsuccessful from an investor's perspective indicates that the human capital or IP were valuable. Hochberg, Serrano, and Ziedonis (2014) and Puri and Zarutskie (2012), among others, employ all M&A events as positive exit outcomes.

Table 5—Phase 1 Grant Impact on Survival and Successful Exit

Dependent variable:	$Exit_i^{post}$				$InextrmBus_i^{Post}$			
Bandwidth:	1	All	All	All	1	All	All	All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Award	0.046	0.05	0.046	0.044	0.15	0.14	0.14	0.12
	(0.025)	(0.022)	(0.022)	(0.016)	(0.061)	(0.046)	(0.048)	(0.034)
Normalized rank		−3.6e-4	−9.1e-4			−0.0027	5.2e-4	
		(0.0047)	(0.0045)			(0.0093)	(0.01)	
Normalized rank^2		5.9e-5	1.1e-6			3.6e-4	3.1e-4	
		(2.9e-4)	(2.8e-4)			(4.5e-4)	(5e-4)	
Rank quintiles	No	No	No	Yes	No	No	No	Yes
Controls[a]	No	Yes	No	No	No	Yes	No	No
Competition fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,872	5,021	5,021	5,021	1,212	3,880	3,880	3,880
R^2	0.38	0.20	0.17	0.17	0.46	0.27	0.25	0.25

Note: This table reports regression estimates of the Phase 1 award effect on survival (remaining in business, columns 1–4) and successful exits (IPOs and acquisitions, columns 5–8) using variants of equation (1).
[a]Controls are VC_i^{prev} and previous non-DOE SBIR awards. Standard errors are clustered by sector-year. Year $\geq$ 1995.

IV. The Phase 2 Grant Impact on Firm Outcomes

About nine months after receiving Phase 1 award, a firm may apply for Phase 2. If successful, the firm receives Phase 2 in two increments of $500,000 roughly two and three years after the Phase 1 award. Any Phase 2 effect is local to the subset of Phase 1 winners. Many Phase 1 competitions have two or fewer Phase 2 applicants, so I use sector and year fixed effects, and do not control for rank. Phase 2 is therefore analyzed as though randomly assigned. If the DOE is selecting on quality, Phase 2 results should be biased upward.

Using the negative binomial model and the standard sample of no previous winners, columns 1–3 of Table 6, panel A show that Phase 2 doubles a firm's cite-weighted patents, relative to a mean of 20. Column 3 jointly estimates both Phases. The coefficient on Phase 2 drops to about 1.4 times as many cite-weighted patents. OLS results using $\ln\left(1 + Cites_i^{post}\right)$ in columns 7–9 find that Phase 2 increases cite-weighted patents by about 30 percent.

Over half of Phase 2 applicants have won multiple DOE SBIR awards and so are excluded from the primary sample. Consistent with the Phase 1 findings, the positive Phase 2 effect on cite-weighted patents falls and loses significance when the sample includes previous winners. There is no measurable Phase 2 effect on VC, regardless of the sample (panel B, columns 1–5). Estimates are near-zero and imprecise. The revenue estimates suggest a negative effect, and the survival and exit impacts are also imprecise and near-zero.[28]

The Phase 2 sample is small because 37 percent of Phase 1 winners do not apply for Phase 2. There are three reasons for this, based on interviews with grantees and

[28]In the revenue ZINB model in column 7, the logistic portion coefficient is insignificant and not reported for brevity.

Table 6—Impact of Phase 2 Grant

Panel A. Cite-weighted patents

Model:	Negative binomial						OLS		
Dependent variable:	$Cites_i^{post}$						$\ln(1 + Cites_i^{post})$		
Sample:	No previous winners			All applicants		>1 prev. win	No previous winners		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Phase 2 award	0.77	0.69	0.34	0.28	0.25	0.17	0.29	0.32	0.22
	(0.29)	(0.30)	(0.17)	(0.15)	(0.17)	(0.15)	(0.13)	(0.15)	(0.12)
Phase 1 award			0.33						0.17
			(0.10)						(0.061)
Controls[a]	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes
Sector and year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	408	408	5,021	867	867	459	408	408	5,021
[Pseudo-] R^2	0.13	0.091	0.21	0.092	0.042	0.10	0.51	0.35	0.42

Panel B. VC, revenue, survival, and exit

Model:	OLS					OLS	ZINB	OLS	
Dependent variable:	VC_i^{post}					$Revenue_i$		$InBus_i^{post}$	$Exit_i^{post}$
Sample:	No previous winners			All apps	>1 prev. win	No previous winners			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Phase 2 award	0.027	0.027	−0.016	0.049	0.057	−0.06	−0.25	0.050	−0.0025
	(0.036)	(0.040)	(0.034)	(0.032)	(0.051)	(0.21)	(0.15)	(0.053)	(0.027)
Phase 1 award			0.11						
			(0.021)						
Controls[a]	Yes	No	Yes	No	No	No	No	No	No
Sector and year FE	Yes	Yes	Yes	Yes	Yes	Yes	No	Yes	Yes
Observations	408	408	5,021	867	459	226	474	311	408
Pseudo- R^2	0.25	0.14	0.13	0.080	0.12	0.16	0.004	0.19	0.13

Notes: This table reports estimates of the Phase 2 grant effect on all outcomes, using variants of equation (1). The model varies based on the dependent variable. There is no rank control due to the small number of applicants in each competition.

[a]Controls are $\ln(1 + Cites_i^{prev})$ in panel A; VC_i^{prev} in panel B, columns 1–5; and $SBIR_i^{prev}$ in both. Standard errors clustered by sector-year. Year $\geq$ 1995.

DOE officials. First, a firm is ineligible to apply if an outside investor owns more than 50 percent. Some firms that raise equity after Phase 1 may sell too much of the firm to be eligible to apply for Phase 2. Consistent with this possibility, among firms that receive VC within two years of the Phase 1 grant, 55 percent do not apply for Phase 2. Put another way, 19 percent of non-Phase 2 applicants received VC investment within two years of their initial Phase 1 award, but only 8 percent of Phase 2 applicants did.[29]

Second, firms might not apply if they changed business strategies. Phase 1 activities are monitored only if a firm applies for Phase 2. Third, the application and reporting processes are so onerous that once a firm receives external private finance, it may sometimes not be worthwhile to apply to Phase 2.[30] Relatedly, Gans and Stern (2003) suggest that private funding is preferred to SBIR funding.

[29] A t-test of the difference of means strongly rejects the hypothesis that non-appliers and appliers have the same mean probability of VC investment within two years, with a t-statistic of 5.44.

[30] This was given as a reason for not applying in interviews with grantees and investors.

A firm's discount rate may increase once its initial R&D is successful, so that applying to Phase 1 is worthwhile but applying to Phase 2 is not, despite the larger sum at stake. This is consistent with the sharply decreasing risk premium in Berk, Green, and Naik (2004) as an R&D project moves from initiation to completion. The adverse selection in the Phase 2 sample suggests that startups seeking to raise external finance whose Phase 1 R&D revealed positive information often secured private investment without needing further government support.

The Phase 2 grant does generate new inventive activity in the form of cite-weighted patents. But on a public dollar basis, its effects are much smaller than Phase 1. High estimates for the Phase 2 impact are 2 cite-weighted patents and at most 6 pp in the probability of VC. Phase 1 generates about 1.4 cite-weighted patents and a 10 pp increase in the probability of VC. In 2012, the DOE spent $38 million on 257 Phase 1 grants and $112 million on 111 Phase 2 grants. If all the Phase 2 money were reallocated to Phase 1, the DOE could have provided 750 additional firms with Phase 1 grants, increasing by a factor of about 2.5 and 3.1 the program's impact on cite-weighted patents and VC, respectively.

A test of such reallocation would be to randomly remove Phase 2 from certain competitions, notifying the public in advance which competitions will be affected. (Congress controls the SBIR program structure, so this would require legislative action.) Experimentation is essential because there may be option value in the right to apply to Phase 2. Eliminating Phase 2 would likely alter the Phase 1 applicant pool. It is also important to stress that my results for the DOE SBIR program do not necessarily generalize to other US SBIR programs or to staged grants more broadly.

V. The Grant Mechanism

A. *Evidence That Grants Ease Financing Constraints*

Absent financial frictions, a grant should have no effect when grant use is not enforced ex post. Suppose a firm in a frictionless world has a project whose return does not exceed the marginal cost of capital (also the opportunity cost of capital or market rate of return). Without a grant, the firm does not invest in the project. A grant is a cash windfall and should be put to work earning the market return, which is greater than the project's return. The firm could accomplish this by distributing the cash to shareholders, who can then invest at the market return, or lending it to another firm that has a positive net present value project. Conversely, if the project's return exceeds the marginal cost of capital, the firm will invest in it with or without a grant.

The Phase 1 grant *does* have a causal effect, so some friction must prevent that investment in the absence of a grant. The outcomes considered are measures of productive investment (patents, VC, revenue), rather than the amount of R&D investment itself. This limits the extent to which only marginally valuable projects (such as those with profit no greater than the grant amount itself) might drive the estimated grant impact. Instead, firms appear to face financing constraints. The rate of return required by external financiers is higher than what the entrepreneur would require if the project could be funded with internal resources. If Phase 1 grants ease financing

Table 7—Heterogeneity in Phase 1 Grant Impact across Firm Characteristics

	Zero versus $\geq$ 1 cite-weighted patents		Hardware versus software	
Dependent variable:	VC_i^{post}	$InBus_i^{post}$	VC_i^{post}	$InBus_i^{post}$
Bandwidth:	3	3	2	2
	(1)	(2)	(3)	(4)
Award $\cdot$ $(\mathbf{1} \mid No\ Cites_i^{Prev})$	0.057	0.31		
	(0.033)	(0.1)		
Award $\cdot$ $(\mathbf{1} \mid Hardware_i)$			0.32	−0.038
			(0.19)	(0.055)
Award	0.1	−0.08	0.083	0.13
	(0.023)	(0.089)	(0.14)	(0.055)
$\mathbf{1} \mid No\ Cites_i^{Prev}$	−0.11	0.23		
	(0.01)	(0.42)		
$\mathbf{1} \mid Hardware_i$			0.0064	0.56
			(0.18)	(0.021)
Sector-year fixed effect and normal rank	Yes	Yes	Yes	Yes
Observations	3,368	2,378	1,209	1,209
R^2	0.209	0.107	0.529	0.530

	Age $\leq$ 2 yrs versus $>$ 2 yrs		Emerging versus mature sector	
Dependent variable:	VC_i^{post}	$InBus_i^{post}$	VC_i^{post}	$InBus_i^{post}$
Bandwidth:	3	3	3	3
	(5)	(6)	(7)	(8)
Award $\cdot$ $(\mathbf{1} \mid Age_i \leq 2)$	0.14	0.15		
	(0.074)	(0.085)		
Award $\cdot$ $(\mathbf{1} \mid Emerging\ Sector_i)$			0.24	0.29
			(0.098)	(0.16)
Award	0.033	0.09	−0.041	0.039
	(0.027)	(0.054)	(0.062)	(0.13)
$\mathbf{1} \mid Age_i \leq 2$	0.26	−0.53		
	(0.12)	(0.18)		
$\mathbf{1} \mid Emerging\ Sector_i$			−0.055	0.17
			(0.063)	(0.1)
Sector-year fixed effect and normal rank	Yes	Yes	Yes	Yes
Observations	3,368	2,378	1,772	1,238
R^2	0.092	0.096	0.160	0.234

Notes: This table reports estimates of the difference by firm type in the Phase 1 grant effect on VC investment and survival using variants of equation (1) (e.g., coefficient on Award $\cdot$ $(\mathbf{1} \mid Age_i \leq 2)$ means the grant effect is 14 pp higher for younger firms). All regressors, including fixed effects and rank, are interacted with the firm type indicator. Rank controls and sector-year fixed effects are included. Standard errors robust and clustered at the sector-year level. Year $\geq$ 1995.

constraints, they should be most useful for firms with few collateralizable assets, firms that require the most capital, and firms about which the market has less information. Proxies for these characteristics are patent holdings, technology type, and age. I focus on how these characteristics affect subsequent financing and survival.

First, patents may increase the information available about a firm, and can also serve as collateral (Hochberg, Serrano, and Ziedonis 2014). Table 7, columns 1–2 show that the grant effects on VC and survival are 6 pp and 31 pp higher,

respectively, for firms with no cite-weighted patents relative to firms with at least one cite-weighted patent.[31] Second, hardware startups typically require more capital up-front than software startups and thus should face more daunting financing constraints. The grant effect on VC is much higher for hardware than for software firms (Table 7, column 3), but there is no comparable effect on survival.

Young firms are more constrained because there is less information about them and they have fewer internal resources or assets to serve as collateral (Cabral and Mata 2003; Angelini and Generale 2008; and Hall 2008). In Table 7, columns 5–6, I show that the grant effect is significantly larger for young firms (less than two years old) than for older firms. 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, consistent with the public firm results in Brown, Fazzari, and Petersen (2009) and the private firm results in Farre-Mensa and Ljungqvist (2015).[32] Haltiwanger, Jarmin, and Miranda (2013) show that young firms rather than small firms are responsible for innovation and growth. In combination with this literature, my results suggest that the government might consider limiting eligibility to young firms.

Dirty, incumbent technologies benefit from being at the technological frontier and from the absence of a carbon price (e.g., Aghion et al. 2016). This puts new, clean technologies at a double disadvantage; they are more difficult to fund *both* because their positive externalities on climate change are unpriced and because they are more immature, with greater information asymmetry between entrepreneurs and investors. Table 7, columns 7 and 8 show much higher effects for emerging sectors than for mature sectors.

Separate estimates by subsector in Table 8 confirm this result. The grant is most consistently effective across VC, patenting, and survival for hydropower (mostly wave and tidal technologies), carbon capture and storage, building and lighting efficiency, and automotive (mostly electric vehicle and new engine technologies). The grant is ineffective for coal, natural gas, biofuels, and recycling technologies; while the latter two may be clean, all are older, incumbent technologies. For solar, the grant has a large and significant impact on VC and survival but an imprecise effect on cite-weighted patents. There are also large measurable effects on survival for advanced materials, biomass, and fuel cells/hydrogen technologies.

The grant appears, therefore, most effective for the most immature technologies (as opposed to all clean technologies).[33] Note that cleaner subsectors may be technologically riskier, but this is not a financial friction. In the absence of a friction like information asymmetry, where the entrepreneur knows something that the investor does not, projects too risky to receive private investment without a grant should not receive it with a grant.

In sum, firms that benefit most from the Phase 1 grant are those expected to face acute financing constraints. That Phase 2, which arrives two to three years after

[31] The characteristics used to analyze heterogeneity are not well correlated. A correlation matrix is in online Appendix Table 8.

[32] Naturally, young firms are more likely than old firms to receive VC and less likely to have many cite-weighted patents, explaining the coefficients on $\mathbf{1} \mid Age_i \leq 2$.

[33] There is no correlation between the grant effect in a sector and that sector's propensity to receive VC. However, solar companies have received much of clean energy VC.

Table 8—Impact of Phase 1 Grant by Technology (*Coefficient on award shown*)

	VC_i^{post} (1)	$\ln(1 + Cites_i^{post})$ (2)	$InBus_i^{post}$ (3)	Observations
Geothermal	0.56 (0.24)	−0.58 (0.30)	0.14 (0.13)	51
Hydropower, wave and tidal	0.51 (0.19)	0.50 (0.20)	0.26 (0.025)	236
Solar	0.25 (0.11)	0.15 (0.13)	0.20 (0.077)	421
Carbon capture and storage	0.20 (0.091)	0.73 (0.34)	0.25 (0.13)	211
Building and lighting efficiency	0.14 (0.057)	0.39 (0.16)	0.21 (0.048)	370
Vehicles, motors, engines, batteries	0.12 (0.060)	0.23 (0.12)	0.16 (0.059)	726
Wind	0.11 (0.039)	0.030 (0.10)	0.15 (0.093)	194
Advanced materials	0.11 (0.071)	−0.081 (0.18)	0.23 (0.077)	435
Biomass production/generation	0.085 (0.067)	0.036 (0.29)	0.21 (0.079)	308
Fuel cells and hydrogen	0.077 (0.072)	0.17 (0.18)	0.27 (0.10)	400
Natural gas	0.060 (0.074)	0.54 (0.45)	−0.15 (0.17)	255
Recycling, waste-to-energy, and water	0.045 (0.053)	0.094 (0.21)	0.061 (0.08)	549
Smart grid, sensors, converters	0.045 (0.053)	0.36 (0.21)	0.056 (0.069)	634
Air and emission control	0.025 (0.035)	0.20 (0.24)	0.20 (0.11)	300
Coal	0.024 (0.053)	0.79 (0.51)	0.11 (0.41)	108
Biofuels and biochemicals	0.014 (0.054)	0.036 (0.29)	0.051 (0.13)	176

Notes: This table reports regression estimates of the Phase 1 grant effect on VC investment, log cite-weighted patents, and survival by technology type using variants of equation (1). Each row represents a separate regression whose sample is limited to competitions in the technology type. Only the coefficient on treatment is shown. All models are OLS and use all the data. Other and Oil are omitted due to few observations. Standard errors robust and clustered at the sector-year level. Year $\geq$ 1995.

Phase 1, has no impact on commercial outcomes is also consistent with Phase 1 reducing early-stage financing constraints. Among Phase 2 applicants, there remain some constraints to R&D investment—hence the positive effect on cite-weighted patents.[34] Yet high quality firms that seek external finance have already received it, and there is sufficient information available about Phase 2 applicants that Phase 2-funded R&D does not incrementally affect investment or commercialization.

B. *How Do Phase 1 Grants Affect Investor Decisions?*

What mechanism explains the large Phase 1 grant effect on VC investment, cite-weighted patents, and commercialization? An early-stage grant might alleviate financial constraints for recipient firms through (i) *certification* or (ii) *funding*. In certification, the award serves as a signal, conveying market-relevant information about grantee quality. Certification requires investors to perceive DOE selection as discriminating. The second mechanism, the money, has two channels: *equity* and *prototyping*. In the equity channel, the grant allows the entrepreneur to retain more equity when funding his project. Without the grant, he would have to sell so large a stake in exchange for the needed investment that entrepreneurial incentives would languish. In the prototyping channel, grantees demonstrate an early-stage technology's viability by investing in proof-of-concept work. Prototyping reduces uncertainty about the technology, which reduces risk for the investor and mitigates information asymmetry.[35]

[34] While use of the grant is not enforced ex post, firms who expect to apply to Phase 2 might feel more pressure to use the grant as originally planned.

[35] Lerner (2000) and others have proposed certification. I devised the funding effect and its two channels.

The secret applicant ranks permit a test for certification. Since rank maps directly to award, rational investors should treat the grant as a quality signal if rank predicts measures of ultimate success. Instead, the slopes of outcomes projected on rank are indistinguishable from zero; ranks are relatively uninformative.[36] Therefore, the Phase 1 grant should not serve as a signal to rational investors.

DOE does a second round of selection to determine Phase 2 winners, so with certification a Phase 2 grant should reveal further quality distinction. The absence of a Phase 2 effect on financing indicates that it does not serve as a signal, making it less likely that Phase 1 does.[37] Thirty interviews with venture investors, conducted in 2013 and 2014, corroborate this conclusion. The investors mostly agreed that SBIR grants sometimes help firms advance, but indicated that the grant itself has little if any informational value.[38] Grantees I interviewed also did not think the grants signaled their technology's value.

Identifying high quality startups is no easy task, even for expert investors (Kerr, Nanda, and Rhodes-Kropf 2014). Further, interviews with program officials indicated that SBIR administration is often viewed as a poor use of time relative to allocating much larger grants to universities, national labs, and corporations. It should not be surprising, then, that the grant decision is relatively random from a private outcome perspective. Yet the grant itself is useful, requiring an explanation beyond signaling.

The equity channel, in which the grant reduces agency problems because it enables the entrepreneur to retain a larger share of the firm, is farfetched. The Phase 1 grant is tiny compared to the average VC round size in my data of $9 million. It is unlikely that $150,000 shifts many firms from negative to positive net present value solely by decreasing the required investor stake. In the equity channel, the Phase 2 effect should be much larger than Phase 1, and the expected value of Phase 2 should drive the Phase 1 effect. Instead, firms that receive VC tend not to apply for Phase 2.

The certification and equity mechanisms imply an immediate grant effect that weakens and grows less precise over time. This is true even in a sunspot coordinating equilibrium, which permits rational investors and random grant allocation. Figure 5 graphs coefficients on VC and cite-weighted patents by number of years after the grant. The effect builds over the first four years, a pattern inconsistent with equity or certification.

Prototyping, instead, appears to be the primary channel. The grant's large effect on cite-weighted patents suggests that it funds proof-of-concept R&D. In certification or funding, the grant could affect VC and revenue while having no effect on cite-weighted patents. However, in certification any effect on patenting might go through the VC effect. In unreported tests, I find that the grant has a positive effect

[36] The one exception is that among losers near the cutoff, ranks are predictive of future cite-weighted patents, though this relationship disappears for winners.

[37] Although Phase 1 is more competitive, if certification explained Phase 1, then the Phase 2 results require the assumption that all Phase 1 winners are "good firms," or that the private sector believes the Phase 1 decision is different in some way.

[38] The investors included experienced angels, partners at conventional VC firms, and leaders of corporate (*strategic*) VC groups. "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 (CMEA Capital Partner) and Andrew Garman (New Venture Partners Managing Partner) conveyed similar opinions.

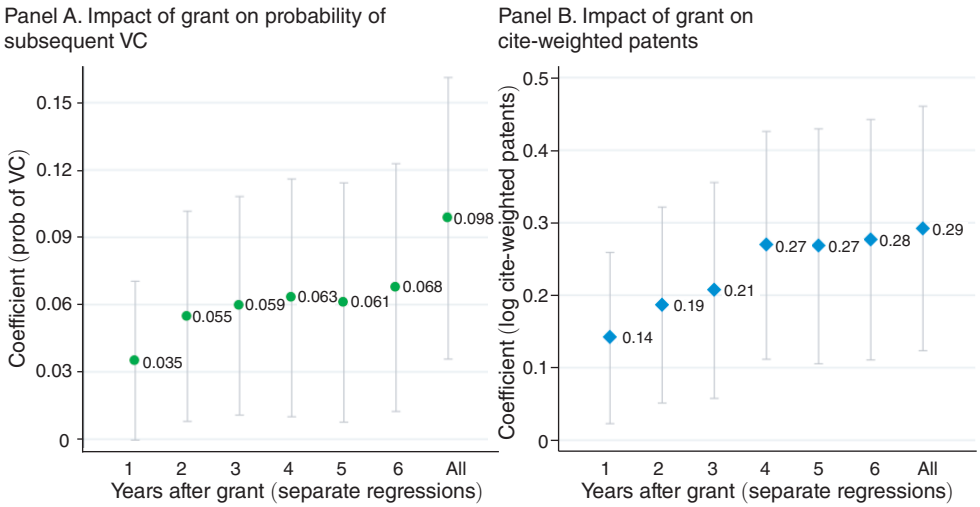

Figure 5. Phase 1 Grant Impact on VC and Cite-Weighted Patents over Time

Note: This figure shows the coefficients from regressions with the dependent variable $(VC_i^{post}, \ln(1 + Cities_i^{post}))$ limited to a certain number of years from the grant.

on cite-weighted patents even among firms that never receive VC. Among firms that do raise VC, the nine months of testing and demonstration that the grant is intended to fund plausibly affect investor decisions within the first two years. This is because roughly one-half of the long-term effect on cite-weighted patents occurs in the first year (Figure 5).

In prototyping, a larger Phase 1 grant should enable more useful testing and demonstrating, at least up to a point. Conversely, if certification were the main channel, a larger Phase 1 grant would have the same effect as a smaller grant (note that all grantees in a given year get the same size award). Phase 1 increased in 1999 from $75,000 to $100,000, and then in 2010 to $150,000. I examine how the effect changed around these transitions in online Appendix Table 9. An additional $50,000 in grant increases the probability of VC by 10 pp, significant at the 5 percent level. I also find a large increase in the effect immediately around the 2010 change; the coefficient after the change is roughly double the coefficient before, and the difference between the two is statistically significant (online Appendix Table 9, column 3).[39] Certification cannot fully explain the Phase 1 effect if the grant amount matters.

The prototyping channel depends on firms spending the grant money on R&D. Recall that while this is the program's intent, firms receive a lump sum cash payment and the DOE does not monitor or enforce how the money is used. Of particular concern is that firms may use the grant money to apply for patents, in which case the positive effect on patents is not evidence for the prototyping channel.

I conducted a grant use survey of all 347 post-2004 Phase 1 grantees that are still in business. Representatives from the firms were given the following none-mutually

[39] I find large positive coefficients in similar analyses for patenting, revenue, and survival, but they are just barely insignificant.

Table 9—Grant Use Survey Results

Survey response:	Fraction			Frequency		
	Yes (1)	No (2)	If random (3)	Yes (4)	No (5)	χ^2 p-value[a] (6)
Panel A. Phase 1 (94 *respondents*)						
(a) Basic R&D on new technology	0.71	0.29	0.40	67	27	0.00
(b) Patent applications	0.08	0.92	0.40	8	86	0.00
(c) Testing existing technology	0.65	0.35	0.40	61	33	0.00
(d) Overhead/salaries	0.47	0.53	0.40	44	50	0.18
(e) Something else	0.10	0.90	0.40	9	85	0.00
Only (a)	0.32	0.68	0.088	30	64	0.00
Only (c)	0.26	0.74	0.088	24	70	0.00
Neither (a) nor (c)	0.032	0.97	0.36	3	91	0.00
Panel B. Phase 2 (38 *respondents*)						
(a) Basic R&D on new technology	0.42	0.58	0.50	16	22	0.33
(b) Patent applications	0.21	0.79	0.50	8	30	0.00
(c) Testing existing technology	0.79	0.21	0.50	30	8	0.00
(d) Overhead/salaries	0.63	0.37	0.50	24	14	0.11
(e) Something else	0.18	0.82	0.50	7	31	0.00
Only (a)	0.026	0.97	0.042	1	37	0.00
Only (c)	0.16	0.84	0.042	6	32	0.00
Neither (a) nor (c)	0.11	0.89	0.25	4	34	0.04

Notes: This table shows the fraction (columns 1–2) and frequency (columns 4–5) of responses to the grant use survey. For example, the first row of panel A indicates that 71 percent of respondents reported using the grant for basic R&D.

[a] χ^2 goodness-of-fit test: asks whether observed proportion differs from random assignment. The null for Phase 1 is that 40 percent pick any given option (respondents averaged 2 choices). If the observed fraction is different from 40 percent with 95 percent confidence, then p-value < 0.05. For Phase 2, the null is that 50 percent pick an option (respondents averaged 2.5 choices). 44 percent (21 percent) picked only one option for Phase 1 (2), so we expect to observe 8.8 percent (4.2 percent) pick a given option at random.

exclusive options for how they used the Phase 1 money: (a) basic research on a very new technology or idea; (b) patent applications; (c) testing/demonstrating/prototyping an existing technology or idea; (d) overhead or salaries; (e) something else.[40] The response rate was 27 percent (94 firms). These respondents are not a random sample of grantees; the results are informative about how firms that ultimately succeed use the grants.[41]

The Phase 1 grant is overwhelmingly used for R&D; Table 9, columns 1–2 show that 71 percent of respondents use the grant for basic research (i), and 65 percent for testing an existing technology (c). Only 3 percent of respondents picked neither (a) nor (c). About one-half also reported using the grant for overhead/salaries, 10 percent for something else, and 8 percent for patent applications.

[40] The survey was conducted in early 2016 by research assistants over e-mail and phone. The question was repeated for Phase 2 grants where applicable. For firms that won multiple Phase 1 or 2 grants, the question was asked about the most recent grant. Respondents were typically senior executives or heads of R&D within the firm. As the chance of contacting someone who personally experienced or directed the grant's use decreases in time since the grant, we began the survey with 2005 applicants (the year was chosen arbitrarily). Please contact the author for survey data and e-mail text (grantees are public information).

[41] Online Appendix Table 10 compares respondents to nonrespondents in panel A, and surveyed firms to all grantees in panel B. Respondents won their awards somewhat more recently, but the two groups do not have statistically different numbers of awards, cite-weighted patents, or probability of VC. Surveyed firms have fewer awards and cite-weighted patents but a higher chance of VC than nonsurveyed firms. Respondents are also likely different in unobservable ways.

Each respondent chose on average two options. If choices were randomly assigned, 40 percent of firms should answer each option affirmatively. A chi-square goodness-of-fit test (column 6) finds that the observed proportions for all but overhead/salaries (d) are different than what we would expect under random assignment with 99 percent confidence.[42]

If the prototyping channel dominates, then firms that choose (c) should be most likely to raise VC. The correlations between choosing an option and receiving VC support this hypothesis. They are: (a) −0.14; (b) 0.16; (c) 0.20; (d) 0.11; (e) 0.05. The correlation with (a) becomes zero when firms that won multiple previous awards are excluded (they are more likely to depend on grants and not seek VC).

Among respondents who also received a Phase 2 grant, 79 percent reported using the money for (c). However, Phase 2 respondents use the grant for purposes that the SBIR program does not intend more often than Phase 1 respondents. Seventy percent of Phase 2 grantees chose (b), (d), or (e), compared to 50 percent of Phase 1 grantees.

In sum, the evidence from the grant data and the survey indicate that firms use the money—particularly in Phase 1—to test or demonstrate an early-stage technology. When this work reveals positive information, they more easily secure investment. Through the prototyping channel, the grant alleviates uncertainty and thus the cost of external finance.

VI. Robustness Tests

This section addresses validity of the RD results. The primary concerns are that (i) rank, not the grant, causes the effect; and (ii) officials manipulate firms around the cutoff. I address (ii) in Section II. The most stringent test for (i) is a regression in which variation in the number of awards across competitions identifies the grant effect. I interact dummies for raw rank with dummies for the number of awards in the competition. This estimates the treatment effect as, for example, the difference in impact on a firm with a raw rank of two between competitions with one and two awards. Table 10 shows that only winning ranks have positive and significant effects on VC. This test with cite-weighted patents as the dependent variable (not reported) provides similar confirmation.

Online Appendix Figure 7 shows a consistent treatment effect within subsets of competitions having only one, two, or three awards. Online Appendix Table 11 confirms this in regression form, and further shows that the number of awards do not affect coefficients on rank. Online Appendix Table 12 uses dummies for raw rank rather than centered or percentile ranks. It again shows how little information is contained in rank. Together, these tests confirm that the treatment effect is explained by being above the cutoff, not rank.

Table 11 contains placebo tests in which ranks are recentered so that 0 lies between true ranks 1 and 2 in panel A and between true ranks −2 and −1 in panel B. The coefficients are mostly negative, small, and insignificant. The exception is cite-weighted patents in panel B; high ranks among losers are predictive of future patenting. This

[42] See note to Table 9 for details on the goodness-of-fit test and expected values under random assignment.

Table 10—Identifying Effect with Number of Awards in Competition

Dependent variable: VC_i^{post}	(1)		(1) *Continued*
$(R_i^{Raw} = 1) \cdot (\mathbf{1} \mid \#Awards = 1)$	0.093	$(R_i^{Raw} = 1) \cdot (\mathbf{1} \mid \#Awards = 3)$	0.095
	(0.042)		(0.087)
$(R_i^{Raw} = 2) \cdot (\mathbf{1} \mid \#Awards = 1)$	−0.055	$(R_i^{Raw} = 2) \cdot (\mathbf{1} \mid \#Awards = 3)$	0.16
	(0.093)		(0.12)
$(R_i^{Raw} = 3) \cdot (\mathbf{1} \mid \#Awards = 1)$	0.060	$(R_i^{Raw} = 3) \cdot (\mathbf{1} \mid \#Awards = 3)$	0.055
	(0.036)		(0.090)
$(R_i^{Raw} = 4) \cdot (\mathbf{1} \mid \#Awards = 1)$	0.015	$(R_i^{Raw} = 4) \cdot (\mathbf{1} \mid \#Awards = 3)$	−0.12
	(0.034)		(0.10)
$(R_i^{Raw} = 1) \cdot (\mathbf{1} \mid \#Awards = 2)$	0.13	$(R_i^{Raw} = 1) \cdot (\mathbf{1} \mid \#Awards = 4)$	0.087
	(0.063)		(0.16)
$(R_i^{Raw} = 2) \cdot (\mathbf{1} \mid \#Awards = 2)$	0.15	$(R_i^{Raw} = 2) \cdot (\mathbf{1} \mid \#Awards = 4)$	0.18
	(0.081)		(0.22)
$(R_i^{Raw} = 3) \cdot (\mathbf{1} \mid \#Awards = 2)$	0.064	$(R_i^{Raw} = 3) \cdot (\mathbf{1} \mid \#Awards = 4)$	−0.017
	(0.065)		(0.17)
$(R_i^{Raw} = 4) \cdot (\mathbf{1} \mid \#Awards = 2)$	0.027	$(R_i^{Raw} = 4) \cdot (\mathbf{1} \mid \#Awards = 4)$	0.14
	(0.049)		(0.18)
		Observations	3,206
		R^2	0.29

Notes: This table reports regression estimates of the Phase 1 grant effect on subsequent VC investment in which identifying variation comes from having a given raw rank in a competition in which the number of awards is at least that rank. Raw rank dummies are interacted with dummies for the competition's number of awards. The omitted dummy in each group is $(R_i^{Raw} = 5) \cdot (1 \mid \#Awards = x)$. Only competitions with 1–4 awards and firms with raw ranks of 1–5 included. Standard errors robust and clustered at sector-year level. Year $\geq$ 1995.

Table 11—Placebo Tests

	VC_i^{post} (1)	$\ln(1 + Cites_i^{post})$ (2)	$Revenue_i$ (3)	$InBus_i^{post}$ (4)	$Exit_i^{post}$ (5)
Panel A. Artificial cutoff between ranks 1 *and* 2					
Award	−0.19	0.044	−3.0	0.0	−0.021
	(0.19)	(0.55)	(0.61)	(0.23)	(0.1)
Competition fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	599	599	420	450	599
R^2	0.78	0.80	0.80	0.85	0.75
Panel B. Artificial cutoff between ranks −1 *and* −2					
Award	0.023	0.26	−0.32	0.074	0.0033
	(0.023)	(0.1)	(0.53)	(0.052)	(0.014)
Competition fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	2,261	2,261	1,370	1,486	2,261
R^2	0.36	0.41	0.41	0.39	0.341

Notes: This table reports regression estimates of the placebo Phase 1 grant effect on all the outcomes using a OLS model and a bandwidth of 1 firm around the cutoff. The regressions are variants of equation (1), except the cutoff for Award is moved from between ranks -1 and 1 to between 1 and 2 (panel A) or −1 and −2 (panel B). Standard errors are robust and clustered at sector-year level. Year $\geq$ 1995.

does not invalidate the RD design, but rather indicates that controlling for rank is important in the patent regressions.

Logit specifications for the baseline results are in online Appendix Table 13. The model biases coefficients upward because it drops competitions where no

applicant has $VC_i^{post} = 1$. The main results are robust to alternative fixed effects (online Appendix Table 14) and to alternative standard error clustering, including by rank and competition (online Appendix Table 15). Results are also robust to specifications controlling for firm characteristics such as minority ownership, age, previous amount of VC, and location in a major city (online Appendix Table 16, columns 1–3). Finally, results are robust to controlling for rank separately on either side of the cutoff and to using different bandwidths (online Appendix Table 16, column 4–6).

VII. Conclusion

In studies of public companies, Seru (2014) and Bernstein (2015) provide grounds for locating R&D in more entrepreneurial, focused institutions. Early-stage startups are arguably the most focused and entrepreneurial institutions doing R&D. Yet their initial experimentation with new technologies may suffer from severe financial frictions. Asset intangibility and uncertainty are especially problematic for energy technology startups. These firms are typically capital intensive with long development cycles and high financing and market risk (Nanda, Younge, and Fleming 2015). Also, while positive externalities motivate basic R&D and entrepreneurship in clean energy, the absence of a carbon price stymies commercialization.

I find that early-stage grants have large, positive effects on cite-weighted patents, finance, revenue, survival, and successful exit. The evidence is consistent with high-tech energy startups facing financing constraints that impede innovation. The mechanism does not seem to be certification. Instead, a grant is useful because it enables the firm to invest in reducing technological uncertainty, which makes the firm a more viable investment opportunity.

Governments, both in the United States and abroad, fund a large share of R&D. Since 2000, the US federal government has spent between $130 and $150 billion per year on R&D, about 30 percent of total annual US R&D (National Science Foundation 2012). To the extent public funds are used to subsidize applied private sector R&D, the findings in this paper suggest that more grants to small, young firms on a one-time basis may be more effective in stimulating innovation than fewer larger grants that follow firms through multiple stages of technology development.

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