Are New Venture Competitions Useful?

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Abstract

This paper evaluates whether and how new venture competitions are useful to startups with administrative data from 87 new venture competitions in 17 U.S. states. Regression discontinuity estimates find that winning has large, positive effects on subsequent financing, employment, and successful exit (acquisition/IPO). Winning is useful in preliminary rounds and is most useful for marginal, non-cash prize winners. Information effects best explain why winning is useful; by signaling quality to the market, winning can alleviate financial constraints. Competitions also appear to facilitate entrepreneur learning in the sense of type revelation, as receiving negative feedback increases venture abandonment.

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1. Introduction

New venture competitions, sometimes called "business plan" or "pitch" competitions, have become a common tool for universities, governments, and other institutions to promote high-growth entrepreneurship. In these competitions, early stage startup founders present their businesses to a panel of expert judges, whose scores determine which ventures win each round. At least some final round winners receive cash prizes. This paper provides the first systematic, causal evaluation of whether and how U.S. new venture competitions are useful to participating startups.

There are three central mechanisms for competitions to be useful. First, cash prizes may directly alleviate financial constraints (Chatterji and Seamans 2012, Howell 2017). Second, winning may serve a certification function, signaling quality to the market and reducing search frictions between venture capitalists and entrepreneurs, in the sense of matching models such as Inderst and Müller (2004), Sørensen (2007), and Ewens, Gorbenko and Korteweg (2018). Third, competitions may facilitate founder learning, including about their own startups' quality. Such type revelation is consistent with entrepreneurship as a process of experimentation, as in Kerr, Nanda and Rhodes-Kropf (2014) and Manso (2016).

This paper uses novel data on 4,328 new ventures participating in 87 competitions in 17 states between 2007 and 2015. The ventures are linked to employment, financing, and survival outcomes, with care taken to account for name changes. Founders are linked to education and career histories. There are no local subsistence businesses – such as restaurants or landscapers – that often contaminate efforts to study high-growth entrepreneurship (Levine and Rubinstein 2016).

Private ranking data permit a regression discontinuity design to estimate the effect of winning. Winning a round increases a venture's chances of raising subsequent external finance by about 13 percentage points, relative to a mean of 24 percent, after controlling for any cash prize. The effect is robust to an array of alternative specifications, including one with judge fixed effects. Winning also increases survival and having at least 10 employees.[1] These effects are stronger in preliminary rounds, where winning also nearly doubles the chances of a successful exit (acquisition/IPO). Winning a preliminary round but not a final round is at most only slightly less useful than winning a final round. The effect of winning is larger when the sample is restricted to bandwidths of one or two ventures around the cutoff for winning, and winning a final round is most useful to those winners that do not receive a cash prize. These results demonstrate that winning is most useful for those ventures that the judges deem marginal.

The data permit separately identifying the effect of the cash prize. An additional $10,000 in prize money increases the probability of subsequent financing by about one percentage point, which is four percent of the sample mean, and also increases survival and employment. The cash prize is less useful for founders with degrees from elite schools and for serial entrepreneurs, who are likely less financially constrained. Importantly, the main effect of the cash prize is not robust to all specifications, and the average prize of $73,000 has a smaller effect than winning only a preliminary round.

Consistent with an important role for information, the judge ranks are strongly predictive of success, even in competitions where ventures do not learn their ranks and so cannot be affected by them. Overall ranks are aggregated from specific criteria ranks. Of these, the team criterion best predicts initial venture success, consistent with Bernstein, Korteweg and Laws (2017) and Gompers, Gornall, Kaplan and Strebulaev (2016). However, technology/product scores are strongly predictive – and are the only predictor – of long run, high-level success (acquisition/IPO). This speaks to the "horse vs. jockey" debate; team may matter most initially, but the business may matter most in the long run (see Kaplan, Sensoy and Strömberg 2009).

[1]Survival and employment are measured as of August 2016. Truncation bias induced by this approach is mitigated by competition fixed effects that control for the date.

If certification is the primary mechanism, five predictions arise: (a) ranks should be informative about outcomes; (b) winning should be useful even in the absence of a cash prize; (c) winning should be more useful in final rounds; (d) winning should be more useful in prestigious competitions; and (e) founders with stronger independent signals should benefit less. The data are quite consistent with the first two, which are the better tests, but are less consistent with the last three. Therefore, the large effects of winning can be at least in part ascribed to a certification channel, but it is worthwhile to explore an additional way that rank signals can be useful to entrepreneurs.

Winning is a binary transformation of rank, and thus winning (losing) may send a positive (negative) signal to the founder about her startup's quality. Testing this requires isolating the effect of information in rank. In two-fifths of the competitions, ventures learn only that they won or lost. In the remainder, ventures are privately informed of their own ranks in the round (but not individual judge ranks). Ex-ante, neither ventures nor judges know that ranks would be disclosed to ventures. The effect of negative feedback on venture abandonment is identified in a difference-in-differences model among losing ventures. The first difference is within round, comparing below- and above-median losers. The second difference is across rounds, comparing feedback and non-feedback competitions. Receiving negative feedback increases abandonment by about 12.5 percent of the mean. The effect primarily occurs in the first six months after the competition. It is also roughly symmetrical among winners.

The empirical concern is whether the effect of negative feedback reflects systematically different distributions among losers in the two types of competitions (differences in levels are absorbed). Three tests and five robustness exercises help to allay this concern. The three tests show that observables are similarly distributed ex-ante across the two types of competitions and that entrepreneurs do not select into feedback. One robustness test measures the effect of feedback as the difference between ordinal and nominal scores, within the feedback competitions. The intuition is that two ventures in different competitions may have the same rank but different distances in score to the next highest rank. After accounting for the venture's quality in the eyes of the judges, there continues to be a strong effect of feedback. A second test finds a similar effect within a single competition that gave feedback in one year but not others.

In Manso (2011)'s optimal contract for motivating innovation, feedback is timely and failure tolerant. New venture competitions with feedback implement this guidance: while they reward top performers, they do not penalize especially poor performance (feedback is private, and loser identities are usually not publicized). Under conditions in which it is not socially costly to deter low quality startups, giving entrepreneurs private, expert feedback may improve resource allocation and the efficiency of innovation. For example, faster type revelation could reduce the number of poor-quality startups seeking financing, allowing venture capitalists to more carefully consider the remainder.

In sum, competitions appear useful through at least three channels: the cash prize, certification, and facilitating learning in the sense of type revelation (where feedback is either binary or more granular in the form of rank). The cash prize, which is awarded to the highest ranked winners, seems the least impactful channel, and may to some degree crowd out private investment. The large effects among marginal winners support a strong role for information effects; certification alleviates information asymmetry, and feedback assists with founder type revelation. Conversely, top-ranked winners are less financially constrained because they can send strong signals independently of the competition.

The relatively small effect of the cash prize is somewhat inconsistent with related studies in developing countries. For example, McKenzie (2017) finds that a Nigerian business plan competition cash prize positively effects firm outcomes.[2] In the U.S. setting, while cash prizes are useful, they appear second-order to information effects. This may reflect the importance of information asymmetry between investors and entrepreneurs in the

[2] Also see Klinger and Schündeln (2011) and Fafchamps and Quinn (2017).

U.S. context. In developing countries, startups may have to rely on internally generated funds because venture capital is absent altogether.

This paper's findings contribute to existing work evaluating programs for early stage entrepreneurs. In a contemporaneous paper, Barrows (2018) evaluates accelerators and competitions that use the YouNoodle platform. Comfortingly, he also finds positive effects of winning on firm outcomes.[3] Accelerator and mentorship programs are studied by Hallen et al. (2014), Fehder and Hochberg (2014), Scott, Shu and Lubynsky (2016), Fehder (2016), and Gonzalez-Uribe and Leatherbee (2017). Xu (2017) and Wagner (2017) examine feedback in crowdfunding and the Startup Chile accelerator program, respectively. Also related to this paper is the broader literature on information in entrepreneurship. For example, Nanda and Sørensen (2010) and Lerner and Malmendier (2013) study peer effects.

The following section discusses the data, sample representativeness, and summary statistics. Section 3 analyzes the effect of winning and assesses the cash prize and certification as possible channels for its effect. Section 4 examines whether venture survival may reflect responsiveness to feedback. Section 5 concludes.

2. New venture competition data

This section introduces the new venture competition data (Section 2.1) and presents summary statistics (Section 2.2). Startups and founders in the data are compared to the U.S. startup ecosystem in Section 2.3.

2.1. The competitions

New venture competitions have proliferated in the past decade. Sponsored by universities, foundations, governments, and corporations, among other institutions, competitions usually

[3] An advantage of the data in this paper is that they permit a sharp regression discontinuity design. For the subset of firms with observed outcomes in Barrows (2018), there is no discontinuity in the probability of winning at the cutoff.

aim to serve convening, certification, education, and financing functions. In a competition, new venture founders present their technologies and business models to a panel of judges. These competitions appear to now be an important part of the startup ecosystem, particularly for first-time founders. For example, among the 16,000 ventures that the data platform CB Insights reports received their first seed or Series A financing between 2009 and 2016, 14.5 percent won a competition. There are no data on the number of business plan or new venture competitions in the world, but casual observation suggests that nearly every non-profit university sponsors at least one, and most U.S. state governments and many national governments provide public funds to support competitions.[4]

This paper uses data from 87 competitions between 2007 and 2016, summarized in Table 1. Data from these competitions permit observing startups and their founders at an earlier stage, with greater granularity, and in a larger sample than prior studies. Further, unlike many data sources commonly used to study entrepreneurship, such as the Survey of Consumer Finances or the Panel Study of Income Dynamics, local subsistence businesses do not appear. The data were obtained individually by the author from program administrators who were either cold-called or previously known to the author and from Valid Evaluation, a company that provides application and judging software as a service. The competitions are therefore not randomly selected, but effort was made to include a variety of competition types, including those organized by universities, which are clearly a major sponsor, and competitions from a variety of U.S. locations, including major hubs and areas without substantial entrepreneurial activity. There is no sense in which it is possible to assess whether the competitions are "representative," as there is no existing data on competitions (note that accelerator programs are substantively different).

[4]For example, New York has at least three publicly funded competitions. Two examples of publicly funded competitions in this paper are the Arizona Innovation Challenge, which awards $3 million annually, and the National Clean Energy Business Plan Competition, with $2.5 million in allocated funding. On NY, see https://www.nypl.org/help/services/startup, http://queensstartup.org/, and http://www.binghamton-ny.gov/binghamton-local-development-corporation-bldc.

The individual competitions are listed in Appendix Table A.1. Competitions consist of rounds (e.g. semifinals), and sometimes judging occurs in panels within a round. The number of ventures in a preliminary (final) round averages 45 (19). Importantly for the regression discontinuity design, there are usually multiple winners in a round, including in final rounds (the average is 8.4). There are 558 ventures that participate in multiple competitions. The mean award amount is $73,000. The competitions are usually open to the public, but typically there are few people besides the judges in the room, except in the final round.

All the competitions have the following features: (1) They include a pitch event, where the venture presents its business plan for 5-15 minutes; (2) Volunteer judges privately score participants; (3) Venture ranks in the round determine which ventures win; (4) Ranks and scores are secret, except when a feedback competition informs a venture of its rank; (5) The organizer does not take equity in any participating ventures; (6) The organizer explicitly seeks to enable winners to access subsequent external finance. In most competitions, judges score or rank based on six criteria: Team, Financials, Business Model, Market Attractiveness, Technology/Product, and Presentation. These criteria scores or ranks are aggregated into a judge-specific venture score or rank. When scores are used, they are ordered to produce ranks. Judge ranks are then averaged to create an overall rank, which determines round winners.

The econometrician observes all ranking and scoring information. This includes overall ranks and individual judges' scores and ranks. In no case do founders observe individual judge scores or ranks. Judges score independently and observe only their own scoring, and never overall ranks.[5] Only winning participants are typically listed on a program website, and judges and outside investors do not generally closely monitor competitions to identify non-winners. Neither entrepreneurs nor judges perceive that losing leads the market to penalize

[5]Judges could in theory report their scores to each other. This is unlikely, as 17 judges score a venture on average.

a venture.[6]

This paper uses four transformations of the rank and score data.[7] One is decile ranks calculated for the round, and also within non-winners and winners separately. Decile ranks divide the group into ten equal bins, with the best ranks in decile one, and the worst in decile 10. Second, rank is centered around the cutoff for winning, so that a rank of one indicates the lowest-ranked winner, and a rank of negative one indicates the highest ranked loser. For example, if there are two winners, the first-place winner will have a centered rank of two, and the second place winner will have a centered rank of one. The third rank measure is is judge decile ranks, calculated among ventures that the judge scored. Finally, z-scores are calculated for the subset of competitions that begin with raw scores. The z-score indicates how far, in terms of standard deviations, a given absolute score falls relative to the sample mean. A higher z-score is better. Informal verbal feedback, which the econometrician does not observe, may take two forms. First, judges may ask questions, and second, the competition usually includes dedicated networking time, such as a post-competition reception.

2.2. Summary statistics

The ventures are described in Table 1 Panel 2. In researching the ventures, 765 name changes were identified. Average venture age is 1.9 years.[8] Forty-four percent of the ventures are incorporated at the round date as a C- or S-corp. Ventures were matched to investment events and employment using CB Insights, Crunchbase, AngelList, and LinkedIn.[9] Raising private investment (angel or venture capital) after the round is a measure of early stage venture

[6]Based on the author's conversations with participants.

[7]The number of ventures varies across rounds, and to determine which ventures win a round, most of the competitions use ordinal ranks while a few use scores.

[8]Age is determined by the venture's founding date in its application materials. Ventures that describe themselves as "not yet founded" are assigned an age of zero.

[9]The match rates for companies were 19 percent, 15 percent, 36 percent, and 45 percent, respectively. The match rate of founders to LinkedIn was 79 percent. For LinkedIn, only public profile data is used by non-logged-in users, based on Google searches for person and school or firm. VentureXpert was not used as it has poor coverage of very early stage investment, and has not been found by the author to outperform the combination of the three datasets used here to identify external financing events.

success. The average is 24 percent.

Venture survival, which averages 34 percent, indicates that the venture had at least one employee besides the founder on LinkedIn as of August 2016. While some startups may not initially appear on LinkedIn, if they are ultimately successful they almost certainly will, because their employees will identify themselves as working at the company. That is, companies rarely remain in "stealth" mode forever. This measure of survival is not ideal and induces truncation bias (mitigated by time fixed effects). However, it is the best available measure for very early stage ventures. An obvious alternative, the presence of a website, is a poor survival measure because websites often stay active long after a venture has failed. An outcome variable that proxies for meaningful real economic activity is having at least 10 employees on LinkedIn as of August 2016, which averages 20 percent.

Founders are described in Table 1 Panel 3, using data from the competitions and LinkedIn profiles. Founders are mostly first-time entrepreneurs. Twenty-two percent of founders are women, and 73 percent are men (the remaining five percent have ambiguous names and no clear LinkedIn match).[10] Age is calculated based on birth year, which is approximated as the college graduation year less 22. Time to abandonment is the number of days between the competition and the founder's next job start date. About half of abandoned ventures are abandoned within six months (shown in Panel 2).

Judges participate to source deals, clients, job opportunities, or as volunteer work. There are 2,514 unique judges, described in Appendix Table A.2, of whom 27 percent are VCs, 20 percent are corporate executives, and 16 percent are angel investors. Ventures and judges are assigned to 16 sectors. Ventures sector assignations come from competition data, and each venture is assigned only one sector. Judge sectors are drawn from LinkedIn profiles or firm webpages, and judges may have expertise in multiple sectors. Ventures and competitions are

[10]Genders were assigned to founder names using the Blevins and Mullen (2015) algorithm, based on gender-name combinations from the U.S. Social Security Administration. Unclear cases, such as East Asian names, were coded by hand.

sorted by state in Appendix Table A.3. There is concern that the judges investing themselves might contaminate any impact of the competitions on venture financing. Careful comparison of funded ventures' investors and judges revealed 95 instances of a judge's firm invested in the venture, and three instances of the judge personally investing.

These data permit observing startups and their founders at an earlier stage, with greater granularity, and in a larger sample than many prior studies in entrepreneurial finance. This sheds new light on venture and founder characteristics associated with startup success. In Appendix Table A.4 Panel 1, subsequent financing and having at least 10 employees are projected on characteristics. More founder job experience, being an IT/software (rather than hardware) venture, being located in a VC hub state, and having prior financing are all strongly associated with success. Having an MBA is weakly negatively associated with success. Attending a top 10 college is associated with a higher likelihood of investment. Kaplan, Klebanov and Sorensen (2012) find a similar relationship between college selectivity and success for CEOs of VC-backed companies. Associations between sector and success are in Table A6 Panel 2. Software and education ventures are more likely to succeed, while social enterprise and biotech ventures are less so. Media and entertainment ventures are far more likely to raise Angel/VC.[11]

2.3. Sample representativeness

To the author's knowledge, this is the first paper to assess multiple U.S. new venture competitions, and more generally, there is little empirical analysis of startups prior to their first external funding event. Therefore, there are no benchmarks against which to assess whether the competitions themselves or the participating ventures are representative. However, an attempt is made to compare the startups in this sample to other data about first-time, early stage U.S. startups and their founders. Appendix Table A.5 compares the

[11] A similar exercise using founder college majors does not find strong variation. Majoring in either entrepreneurship or political science/international affairs is weakly associated with success.

distribution of ventures to overall U.S. VC investment. The share of software startups, 37 percent, is close to the national average of 40 percent in both deals and dollars. In part because VC investment in clean energy has declined dramatically in recent years (Saha and Muro 2017), as well as the presence of the Cleantech Open in my sample, the data are skewed towards clean energy.

The competitions take place in 17 U.S. states. With the exception of Arizona, the top 20 states for venture location in the data almost entirely overlap with the top 20 states for VC investment, though the data has fewer ventures from California and more from Massachusetts. This may be expected from such early stage ventures, as startups often move to Silicon Valley to raise VC. The probability of an IPO or acquisition, 3 percent, is comparable to the 5 percent found in Ewens and Townsend (2017)'s sample of AngelList startups. Ventures average three team members, similar to the 2.6 founders on the AngelList platform in Bernstein, Korteweg and Laws (2017). The median founder age is 29 years, which is roughly representative of startup founders.[12]

2.4. Feedback

Competitions were selected for inclusion in the data such that they would be broadly similar but provide systematically different feedback. Competition organizers generally do not prioritize feedback. Instead, they are concerned with facilitating networking and identifying the "best" ventures as winners. However, 34 of the competitions in the data use a third-party company, Valid Evaluation, to provide application and judging software. The managers at Valid Evaluation thought that formal feedback might be useful and sent each venture an email after the round containing their overall and criteria ranks. Ventures learn only their own ranks, and not those of other participants. Interviews with competition

[12]The average Y-Combinator founder is just 26, and the average entrepreneur age at company founding among startups with at least a $1 billion valuation between 2003 and 2013 was 34 (https://techcrunch.com/2010/07/30/ron-conway-paul-graham/ and https://techcrunch.com/2013/11/02/welcome-to-the-unicorn-club/).

organizers indicated that they do not share an interest in feedback, and in fact sometimes discontinued use of Valid Evaluation in part because it seemed more concerned with feedback than with features the organizers valued more, such as the user interface.

The remaining 53 no-feedback competitions use different software, and participants do not observe any rank information. There are no systematic differences in the way judges score or in services (e.g. mentoring, networking, or training) across the two competition types. In no case did a competition with feedback advertise itself as providing relative ranks or more feedback in general, so ventures with greater informational needs could not have selected into them. There is an explicit test for selection into feedback in Section 4.2. Judges were not informed that feedback would be provided, so there is no reason to believe they would exert greater effort in the feedback competitions. Judges cannot learn from the feedback, as they observe only their own scoring.

3. Is winning useful?

This section first describes the regression discontinuity design. Section 3.2 contains the main effects of winning, including those of round type, rank, and cash prize. The importance of the cash prize and certification as channels for the main effect are discussed in Section 3.3.

3.1. Estimation strategy

A regression discontinuity (RD) design permits establishing a causal effect of winning a competition.

$$Y_i^{Post} = \alpha + \beta_1 WonRound_{i,j} + f\left(Rank/Zscore_{i,j/k}\right) + \beta_2 Prize_i + \gamma_{j/k} + \delta'\mathbf{X}_i + \varepsilon_{i,j} \qquad (1)$$

In Equation 1, the dependent variable Y_i^{Post} is a binary measure of venture i's success. A function of rank or z-score is at the competition-round-panel (j) or judge (k) level. $Prize_i$ is the dollar amount that the venture won, if any. Fixed effects for either the competition-round-panel (γ_j) or judge (γ_k) are included. The former absorb the date and location. Venture controls $\mathbf{X_i}$ include whether the company received investment before the round, whether any of the venture's judges or those judges' firms ever invested in the venture, 17 sector indicator variables, company age, and whether the founder is a student. These, especially age, reduce the sample size and are not included in most specifications. Standard errors are clustered by competition-round-panel or by judge.

A valid RD design requires that treatment not cause rank. This is not a problem here, as the award decision happens after ranking. In the primary specification, ranks are ordinal, rather than cardinal as in most RD contexts.[13] 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. To address any concerns with a discrete, ordinal running variable, z-scores based on nominal scores are employed in a robustness test (the set of competitions that provide nominal scores is slightly smaller than the overall sample).

One way this setting differs from conventional RD is that the ranking is ordinal rather than cardinal. However, 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 out. An important data limitation is the rating variable's discreteness; the average number of participants is 45 for preliminary rounds, and 19 for final rounds (though this is much larger than the average of 10 applicants that Howell (2017) uses in RD design). 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 the goodness-of-fit test for RD with discrete covariates in Lee and Card (2008), which compares unrestricted and restricted regressions.[14] Also note that a McCrary (2008) test for density around the cutoff

[13]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.

[14]The unrestricted projects the outcome on dummies for each of K ranks. The restricted is a polynomial like

is not relevant to this setting, since by definition the ranks around the cutoff are populated equally.

The primary empirical concern is whether ranks are manipulated around the cutoff, because the cutoff in a valid RD design must be exogenous to rank (Lee and Lemieux 2010). That is, the identification strategy is threatened if judges or organizers sort ventures on unobservables around the cutoff. This is extremely unlikely because judges score independently and typically only score a subset of participating ventures. Scores are then averaged and sorted to create ranks.

Reassuringly, observable baseline covariates and pre-assignment outcome variables are smooth around the cutoff, using both decile ranks and centered ranks. This is shown with decile ranks in Figure 1 for venture variables observable at the time of the competition, such as previous financing and whether the venture is incorporated. Similarly, Figure 2 shows that founder characteristics observable at the time of the round, such as having a BA from a top 10 college, being female, and the number of previous jobs, are continuous across winners and losers. The figures use final rounds and decile ranks. They are similar when preliminary rounds or centered rank are used instead. Note that with decile rank, the winner and loser local polynomial lines overlap because the winning share varies across rounds.[15]

3.2. Effect of winning

3.2.1. Effect of winning on subsequent financing

Visual evidence of the effect of winning is in Figures 3 (using decile ranks) and 4 (using centered ranks around the cutoff). In each case, the top two graphs show the probability of subsequent external financing in preliminary and final rounds. The middle two graphs show

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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 from regression, 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. Results in Section 4.

[15]There are no losers in the top bin, and winners are truncated at the fifth decile.

the probability of venture survival, and the bottom two graphs show the probability of having at least ten employees. The positive effect of winning is apparent in all cases, especially for preliminary rounds. In the decile graphs, the winner line lies above the non-winner line, and in the cutoff graphs, there is a clear discontinuous jump at the cutoff. This provides strong evidence of a substantial raw effect of winning.

Regression estimates of the effect of winning are in Table 2, using variants of Equation 1. The dependent variable is subsequent external financing, which is a proxy for early stage startup success and an explicit goal of the competition organizers. Both final and preliminary rounds are included. Further, some ventures participate in multiple competitions, and all observations are included. Thus, in this main specification, a venture can appear multiple times. Using overall decile rank in the round, winning a round increases a venture's chances of subsequent external finance by 8.8 percentage points (pp), relative to a mean of 24 percent. The preferred specification in column 2, where decile ranks on either side of the cutoff are used, finds an effect of 13 pp. When a rich array of venture controls are added (substantially decreasing the sample size), the effect is 7.9 pp (column 3).

Equation 1 is estimated at the judge-venture level in Table 2 Panel 1 column 7. That is, each observation is a judge's rank of the venture within a competition-round-panel. This model includes judge fixed effects and controls for the venture's decile rank within ventures that the judge scored. Year fixed effects are also included. Note that some judges are present at multiple competitions, so these fixed effects are quite different from the competition-round-panel fixed effects used in other specifications. This model finds a larger effect of winning, at 17 pp. Standard errors are clustered by judge, but the standard error is essentially unchanged when clustering by venture.

Subsequent specifications use alternative controls. Table 2 Panel 1 column 5 uses z-scores and so is restricted to the subsample of competitions that use scores before force-ranking participants. Panel 2 columns 1-3 use various forms of centered rank around the cutoff. In columns 1-2, linear and quadratic centered rank yield the same effect as the main specification. When centered rank is controlled for separately among winners and losers, the effect is somewhat larger (column 3).

To assess the effect of winning near the cutoff, Table 2 Panel 2 columns 4 and 5 use narrow bandwidths of just one and two ventures on either side of the cutoff, respectively. They find somewhat larger effects than the main specification, at 16 and 17 pp. This indicates that ventures of more marginal quality in the vicinity of the cutoff benefit most from winning, rather than the effect being consistent among lower- and higher-ranked winners. This also highlights the inherently local nature of regression discontinuity design results.

3.2.2. Effects by round type and importance of cash prize

In order to shed light on the mechanism, it is useful to decompose the overall effect of winning into that of the cash prize and the type of round (preliminary or final). It is possible to identify the prize separately from winning because not all winners receive cash prizes in final rounds, and the prize amount typically varies across winners that do win cash prizes within a final round. While prize amounts may vary with competition characteristics, competition fixed effects absorb this variation. The regressions in Table 2 consistently find that an extra $10,000 increases the probability of financing by nearly 1 pp. This implies that the average prize of $73,000 increases the chances of financing by 7.3 pp. The effect of cash prizes appears linear, as there is no effect of higher order functions of the prize, such as the prize squared.

This positive effect of a cash prize is smaller than the effect of winning even a preliminary round, and is not as robust. Table 3 considers round type using subsequent financing as an outcome. Column 1 includes dummies for winning a preliminary round, a final round, and a prize. Note that for prize winners, all three of these will equal one. The effect of winning a preliminary round after controlling for winning the final round is 8 pp. Once controls for winning preliminary and final rounds are included, there is no effect of an indicator for wining a prize. Columns 2 and 3 establish this more rigorously by restricting the sample to final rounds. The effect of winning is 12 pp, and there is no effect of an additional $10,000 in cash prize (column 2). In column 3, prize winners are excluded from the sample, and the point estimate of winning a final round rises to 17 pp. This indicates that the effect of winning a final round is driven by marginal winners that do not win a cash prize. In sum, winning a cash prize is useful, but is not nearly as useful as winning the round.

The sample is restricted to preliminary rounds in Table 3 columns 4-6. The effect of winning a preliminary round is 14 pp (column 4), which is slightly higher than the overall effect using all rounds with the same specification in Table 2 column 2. The effect of a preliminary win is 8.6 pp after controlling for whether the venture won the final round (column 5), and 8.5 pp when final round winners are excluded from the sample (column 6). The difference between the coefficients on winning final and preliminary rounds is significant at the .05 level in column 5. Note, however, that both coefficients on winning are relative to preliminary round losers. Winning a preliminary round but not a final round is at most only somewhat less useful than winning a final round (8.6 pp vs. 12 pp, which are not significantly different from one another). These effects are paralleled for the other measures of venture success (regressions available on request).

3.2.3. Effect of winning on additional outcomes

Three additional outcomes – survival, having at least 10 employees, and being acquired or going public – are considered in Table 4. The first four columns use the whole sample, the next three preliminary rounds, and the final three a bandwidth of two ventures on either side of the cutoff. First, consider the probability of venture survival. Winning has a 4.7 pp effect across all rounds, though this is significant only at the .1 level (column 1). It loses significance in preliminary rounds but is a robust 7.7 pp with the narrow bandwidth (column

8). There may be concern that dead ventures are miscoded and in fact reflect a "pivot" and venture name change. As mentioned above, care was taken to identify name changes. Nonetheless, some miscoding may remain. Therefore, column 2 considers founders that subsequently founded or were the CEO of a different company, in case these other companies are in fact the original ventures with new names. There is no effect of winning. This not only serves as a robustness test, but also indicates that winning does not affect entrepreneurship as a career for for the founder, despite being useful for the venture.

Having at least 10 employees is a measure of real economic activity. In in the context of very early stage ventures, is a meaningful marker of success. The bottom two graphs in Figures 3 and 4 show a clear jump at the cutoff in preliminary rounds, indicating a significant effect of winning. Winning increases the chances of having at least 10 employees by 5 pp across all rounds, and 6.3 pp in preliminary rounds (Table 4 columns 3 and 6). It has a similar effect using the narrow bandwidth, of 5.9 pp (column 9). There are not many acquisitions or IPOs in the data; the mean is just three percent. The effect of winning on these successful exits is not quite significant in all rounds or with the narrow bandwidth (columns 4 and 10), but it is 2.6 pp in preliminary rounds (column 7). At almost 100 percent of the mean, this effect is large in economic magnitude. For all outcomes in Table 4, the results are similar with controls for centered rank.

3.2.4. Heterogeneity in effect of the cash prize

There is no statistically significant and economically meaningful heterogeneity in the effect of winning for any observable venture, competition, or founder variables. However, there is heterogeneity in the effect of the cash prize for two variables. Table 5 interacts all covariates (except the competition fixed effects) with a characteristic C. Column 1 shows that the cash prize is significantly less useful for founders with elite college degrees.[16] Column 2 shows

[16]The definition of "elite" is the top ten colleges (Appendix Table A.6), but the result is robust to only using Harvard-Stanford-MIT, or the top twenty colleges.

that the cash prize is significantly less useful for founders who were previously was the CEO or founder of a different venture (i.e., serial entrepreneurs).

3.2.5. Effect of rank

A striking finding from Tables 2-5 is that rank and score strongly predict success after controlling for winning and competition fixed effects. For example, a one decile improvement in rank is associated with a 1.8 pp increase in the probability of external financing, which is 7.5 percent of the mean (Table 2 Panel 1 column 2). Individual judge ranks are also predictive within judge (Table 2 Panel 1 column 4). Importantly, the effect of rank persists within the no-feedback competitions, where ranks cannot directly affect venture outcomes (Table 7 column 7).

The criteria ranks are also informative. Table 6 shows the association between criteria ranks and outcomes, controlling for win status. A higher team rank (i.e. the quality of the founders) is the strongest predictor of success for all outcomes other than IPO/acquisition. Similarly, Bernstein et al. (2017) and Gompers et al. (2016) find that early stage investors care most about information regarding founder team quality. For IPO/acquisition, the only criterion with predictive power is product/technology, and this is quite robust. Therefore, in these data, team is most relevant for low-level, early stage success, while technology matters most for high-level, late stage success. This speaks to the "horse vs. jockey" debate, suggesting that the team matters initially, but the business matters in the long run. It is consistent with Kaplan, Sensoy and Strömberg (2009), who examine 50 public firms and find that business lines but not management remain stable from startup to IPO.

The strong predictive power of rank found here contrasts with the U.S. Department of Energy ranks of SBIR grant applicants in Howell (2017), which are uninformative about firm outcomes. There are a number of differences between the SBIR grant process and new venture competitions. One is that competition judges tend to be expert market participants rather than government officials. Nearly half of the judges in these data are angle or venture capital investors (Appendix Table A.2). Unreported regressions examine the predictive power of rank by judge occupation. There is little difference across investor, lawyer/consultant/accountant, and corporate executive judges. Perhaps surprisingly, entrepreneur judges are the exception: their scores have no predictive power.

3.2.6. Robustness tests

Robustness tests confirm the main effect of winning and find it to be consistent across relevant subsamples. Table 7 column 1 clusters errors by competition rather than competition-round-panel. Venture or judge clusters also yield similar results to the main model (unreported). In column 2, ventures in which a judge or judge's firm invested are excluded, in case these judges' favorable opinion of the ventures mechanically causes winning or rank to predict financing. Column 3 restricts observations to a venture's first competition. Column 4 uses a logit model instead of OLS, and that winning doubles the odds of receiving financing. Note that logit is not preferred as it drops groups without successes (i.e. panels without ventures that subsequently received financing). Column 5 restricts the sample to competitions that gave participants feedback (informed them of their rank in the round), while column 6 restricts the sample to competitions that did not provide feedback. The effect is somewhat larger in the no-feedback competitions, at .17 pp, though the differences is not statistically significant.

The main model uses competition fixed effects, so the results should not be affected if participants are on average higher quality in some competitions. However, to ensure robustness and explore potential heterogeneity, columns 1-5 of Appendix Table A.7 divide the sample by competition type. The effect is 12 (15) pp in competitions not held at (held at) universities (columns 1-2). The effect is unchanged when the two largest competitions are excluded (HBS New Venture Competition in column 3 and Arizona Innovation

Challenge in column 4). The effect is very similar to the main result when small competitions (less than 30 participants) are excluded (column 5).

The final columns of Appendix Table A.7 divide the sample by venture and founder characteristics. The effect is robust to restricting the sample to ventures located in California, Massachusetts, and New York (column 6), to incorporated ventures (column 7), to founders with MBAs (column 8), and to student founders (column 9). All of the above robustness tests for winning go through for survival and 10+ employees; these are available on request. The remarkable consistency of the effect across subsamples and the absence of significant variation mentioned in Section 3.2.4 indicate that conditional on selecting into participating in a competition, winning provides ventures with roughly homogenous benefits.

3.3. Interpretation

3.3.1. Channel 1: Cash

Non-dilutive cash may directly alleviate financing constraints. Founders could, for example, use it to build initial prototypes of their products, which might reduce uncertainty about the startup among prospective investors. Cash could also improve the bargaining position of the entrepreneur or reduce the amount of outside equity needed. Indeed, independently of winning, the cash prize is useful, with positive effects on financing, survival, and employment (Tables 2 and 4).

Yet the effect is economically small relative to the effects of winning either a preliminary or final round, and the effect of winning a final round is stronger among winners that do not receive a prize. The effect of the cash prize is also small relative to the predictive power of rank. Even in the specification where it has the largest, most robust coefficients, the effect of an additional $10,000 is similar to or smaller than one decile of rank's predictive power. Further, it is smaller in economic magnitude than the effect of U.S. Department of Energy SBIR grants found in Howell (2017). The effect of an additional $10,000 in SBIR grants on the probability of subsequent financing is 0.66 pp, or eight percent of the sample mean, compared to about one pp, or four percent of the sample mean, for the same amount of competition prize money.[17]

The heterogeneity results suggest that cash awards are more useful for founders that are likely more financially constrained. Recall that the cash prize is significantly less useful for elite school founders and for serial entrepreneurs (Table 5). Founders with top college degrees are likely wealthier (Chetty, Friedman, Saez, Turner and Yagan 2017) and may have superior access to investor networks. Serial entrepreneurs also may have better access to investor networks and may have accumulated capital from the previous venture. Both these types of founders are likely less financially constrained. The sensitivity of non-elite founders' venture outcomes to cash suggests that the cash prize can perhaps help to level the entrepreneurship playing field.

3.3.2. Channel 2: Certification

A second channel through which competitions may be useful is if they certify winners as high quality. That is, winning may be an informative signal to the market, especially to early stage investors. If certification is the primary mechanism, five natural predictions arise: (a) ranks should be informative about outcomes; (b) winning should be useful even in the absence of a cash prize; (c) winning should be more useful in final rounds; (d) winning should be more useful in prestigious competitions; and (e) founders with stronger independent signals should benefit less.

First, the large effect of winning and fact that ranks are informative make it likely that competitions do produce signals about venture quality, supporting a certification channel. This contrasts with the finding in Howell (2017) that SBIR grants do not serve a

[17] A $150,000 SBIR grant increased the probability a venture subsequently received external financing by about 10 pp. Thus an extra $10,000 in SBIR grants was associated with a 0.66 pp increase in financing, while in the competition context an extra $10,000 is associated with about a 1 pp increase. The sample means are eight and 24 percent, respectively.

certification function, and instead appear useful because the cash award funds prototyping. Second, winning is more useful for winners who do not receive a cash prize. Intuitively, winning is more useful for the marginal winners. It suggests that the top-ranked winners can send strong signals independently of the competition and are thus less financially constrained. This highlights the inherently local nature of regression discontinuity results.

Third, winning should be more useful in final rounds because competitions usually publicize only ultimate winners, and ventures that win only preliminary rounds do not typically mention this in their marketing, as it draws attention to their ultimate loss.[18] Winning a final round appears only slightly more useful than winning only a preliminary round, and not significantly so (the difference between 12 and 8.6 pp, Table 3). Note this test assumes that information asymmetry between ventures and the market is the same across rounds. There could be more uncertainty about quality among preliminary round participants. In this case, and if the market can observe preliminary winners that did not win final rounds, certification could be stronger in preliminary rounds.

Fourth, in prestigious competitions, winning should send a stronger signal. One proxy for prestige is being selective: not allowing all applicants to compete.[19] Yet winning is not more useful in selective competitions (Table 5 column 3). Last, founders with stronger signals independently of winning should benefit less from certification. In this case, winning might be less useful for founders with elite college degrees, who likely send better signals, or for founders with entrepreneurial track records, who likely send more precise signals. Conversely, there is no differential effect of winning in either case (Table 5 columns 1-2).

In sum, the large effects of winning can be at least in part ascribed to a certification channel. However, since the evidence is not entirely consistent with certification as a

[18]Based on conversations with competition participants and early stage investors.

[19]This applies to 38 percent of competitions and 33 percent of company-competition observations. The HBS' New Venture Competition is included as selective, because participating teams must include at least one HBS MBA student, and attending HBS is quite selective. The competition is also regarded as prestigious by local venture capitalists.

primary mechanism, it may be fruitful to look to additional ways in which the informative signal generated by rank (of which winning is a binary transformation) could be useful to entrepreneurs.

4. Responsiveness to feedback

Judge ranks are informative about outcomes (Section 3.2.5), which in addition to supporting a certification mechanism, points to competitions offering learning opportunities. It is natural that participants will draw lessons from a competition about how to improve the quality of their business models and pitches. Less obvious and more interesting is learning in the sense of type revelation. If entrepreneurs are uncertain about their startups' quality, they might react to signals from the competition when deciding whether to continue with or abandon their ventures. To test this possibility, it is necessary to isolate the effect of the rank signal on venture continuation.

This section first proposes the main design for estimating the effect of feedback on venture continuation (Section 4.1). The challenge to causal identification is addressed in Section 4.2. The main effect of negative feedback on abandonment is in Section 4.3. Section 4.4 summarizes robustness tests.

4.1. Estimation strategy

The effect of feedback can be measured by comparing competitions where ventures learn their rank relative to other participating ventures with competitions where ventures learn only that they won or lost. Learning the rank is equivalent to learning the order statistic, so the peer group is relevant. The analysis asks whether founders who receive especially negative feedback about their position relative to their peers are more likely to abandon their ventures. The primary empirical design is a difference-in-differences model among losers, which comprise 75 percent of the data. The first difference is between above- and below-median losers in a given competition ($Low\ Rank_{i,j}$). The second difference is across feedback and no-feedback competitions ($Feedback_j$). In Equation 2, i indexes ventures, and j indexes competition-round-panels.

$$Abandonment_i = \alpha + \beta_1 Low\ Rank_{i,j} \cdot Feedback_j + \beta_2 Low\ Rank_{i,j} \\ + \gamma_j + \delta' \mathbf{X}_i + \varepsilon_{i,j,t} \text{ if } i \in Losers_j \tag{2}$$

The dependent variable is abandonment, which is defined as the inverse of Survival. This is the outcome variable most immediately relevant to type revelation. (In fact, there is no effect of feedback on financing or the other outcomes from Section 3). Note that "failure" is an equivalent term in this context ; it is impossible to distinguish whether non-survival reflects a perceived choice on the part of the founder.

Competition-round-panel fixed effects, γ_j, absorb the average effect of feedback and also control for date and location, as in Equation 1. When a venture participated in multiple competitions, only the first instance is included. While this exercise is framed as estimating the effect of negative feedback, it could alternatively reflect relatively positive feedback inducing continuation. An alternative model considers positive feedback among winners. In neither case, however, is it possible to establish whether an effect is driven by inducing higher rates of abandonment among low-ranked ventures or continuation among high-ranked ventures.

4.2. Identification challenge

Feedback was not randomly allocated across competitions, and the two groups differ systematically along some criteria. The most striking is venture maturity: ventures are older and more likely to have previous financing in feedback competitions (see Appendix Table

A.8 Panel 2). The first difference in Equation 2 absorbs average differences across the types of competitions. Nonetheless, Equation 2 is not a fully causal test, even though applicants did not know whether the competition would inform them of their ranks in the round. The concern is that the distribution of losers around the median may be systematically different in the two types of competitions (i.e., a different mapping from quality to rank). There are two main sources of bias. First, suppose that ranks in the feedback competitions better correlate to true quality than ranks in the no-feedback competitions. Then feedback might be inherently correlated with continuation without any effect of information. Second, feedback competitions could have diverse participants while the no-feedback competitions have participants with similar quality. This could also lead to more abandonment in response to a lower rank in the feedback competitions.

Compelling evidence for causality is presented in five robustness exercises (Section 4.4). Further, Appendix Section 1.1 presents three specification tests. First, there are no meaningful distributional differences in ex-ante characteristics between feedback and non-feedback competitions. Second, rank reflects ex-ante quality equally across feedback and non-feedback competitions. Third, there is no evidence of selection into feedback using ventures in multiple competitions. Nonetheless, the fact that feedback was not randomly allocated means that this exercise should be interpreted as providing only tentative evidence and further research is warranted.

4.3. Main effect of feedback

Equation 2 is estimated in Table 8. The main specification in Panel 1 column 1 finds that negative feedback increases the probability of abandonment by 8.3 pp, relative to a mean of 66 percent. This 12.5 percent increase in the probability of failure is economically large, especially given the subtle, low stakes nature of the feedback. It implies that had the 1,603 unique below-median losers in the no-feedback competitions received feedback, an additional 137 would have been abandoned, beyond the 1,186 that were abandoned. Column 2 adds venture controls and finds an effect of 6.7 pp, albeit in a smaller sample. There is no effect of feedback on subsequent entrepreneurship (column 3). Abandonment in response to negative feedback occurs soon after the competition. In columns 4 and 5, the dependent variables are indicators for abandoning within six months and one year. They show that the effect occurs quickly, mostly in the first six months.

The effect is roughly symmetrical among round winners. In column 6, positive feedback is measured as the interaction between an above-median rank and feedback within round winners. Positive feedback leads to a 13 pp decrease in the probability of abandonment. The effect is also roughly linear and is not sensitive to using median rank as the cutoff (Appendix Table A.9 Panel 1 columns 1-3). Given these results, and as mentioned in Section 4.1, it is not possible to establish whether the main effect reflects negative feedback inducing more abandonment or positive feedback inducing more continuation. The contribution of this exercise is to establish that feedback, in the sense of learning relative rank, matters.[20] Motivated by this evidence, Appendix Section 2 presents a simple model of how Bayesian entrepreneurs respond to structured feedback in this setting. The empirical results are used to calibrate the model and show how, for example, improving signal precision by increasing the number of judges would affect entrepreneur responsiveness.

In sum, private, costless, informative signals at an early stage may lead poor quality startups to fail faster. It is possible that this sort of feedback can improve efficiency in the innovation process. However, the data do not permit a welfare assessment of feedback, nor is it apparent whether participants' prior beliefs were biased. Three potential caveats also bear mentioning. First, it is possible that inducing abandonment could be socially costly if a few highly successful outcomes are foregone. Within the data used here, there is no

[20]It is possible that the effect on survival operates through financing. Highly ranked losers with feedback may be better able to raise financing than their uninformed counterparts. However, in unreported tests negative feedback has no significant effect on subsequent external financing, though the coefficient is positive. This suggests that feedback does not measurably affect ultimate success conditional on continuation.

evidence of lost right-tail outcomes.[21] Second, encouraging entrepreneurial entry may always be socially beneficial, regardless of startup quality. Third, learning may be privately inefficient if abandoning after negative feedback leads to poorer long run labor market performance. Founders have a revealed taste for leadership, so leadership in other domains is a reasonable proxy for non-entrepreneurial success.[22] There is no effect of negative feedback on either serial entrepreneurship or subsequent non-entrepreneurial leadership among founders that abandoned their ventures, so feedback does not seem to cost abandoners leadership positions. In sum, there is no evidence of large social or private costs to feedback, suggesting that it is likely weakly more efficient.

4.4. Robustness tests

4.4.1. Effect of feedback within a single competition

A single program in the data, the Cleantech Open (CTO), gave feedback in 2011 but in no other year. As the CTO did not otherwise change in 2011, there is no reason that the distribution of quality among losers was different in 2011. Comparing the effect of having a low rank in 2011 relative to other years provides a useful robustness test. The results are in Appendix Table A.10. In columns 1, 2 and 5, the sample is restricted to 2010-12. In the remaining columns, all CTO years are included (2008-14). Negative feedback reduces the probability of survival by 11-13 pp in 2011 relative to the surrounding years, which is quite similar to the main specification.

[21] Among below-median ventures in the feedback competitions, 2.1 percent were acquired, compared to 3.2 percent in the no-feedback competitions. All appear to be minor acquisitions, as valuation is in no case available. There were no IPOs in either group.

[22] The specific variable that is used is based on the latest job title of founders who abandoned their ventures. It is an indicator for the title containing any of the following words: CEO, CFO, CTO, Chief, Managing Director, Manager, Senior, President, Partner, Director.

4.4.2. Alternative estimation with nominal scores

Nominal scores can be used to approximate the random allocation of ranks, providing an alternative estimate of the effect of feedback. In all but two of the competitions, the conference organizers arrive at ranks by ordering nominal scores. These nominal scores are never revealed to ventures. If scores measure latent quality, then residual variation in rank reflects noise in transforming nominal scores to forced ranks. To illustrate the approach, consider a pair of ventures in one round with ranks five and six, and a second pair in a different round that also has ranks five and six. Now suppose that the first pair had very similar scores, while the second pair had more distant scores. As perceived by the judges, the quality difference of the second pair is larger than that of the first pair. If all four ventures are informed of their rank, their feedback is the same, but their quality is different. The venture ranked sixth in the second pair got randomly higher feedback.

This source of variation within feedback competitions is used in Table 8 Panel 2. First note that across all columns, score (where one is worst and five is best) strongly predicts survival after controlling for decile or centered rank. The coefficients of interest are on rank. Columns 1 and 2 estimate the effect of rank after controlling for nominal score. Decreasing a venture's rank by one decile increases the probability of abandonment by 1.3 pp (recall that lower ranks are worse), and decreasing the centered rank by one increases abandonment by 0.19 pp (recall that among losers the best centered rank is -1, and lower centered ranks are worse). Columns 3 and 4 show more precisely how the residual information in rank after accounting for score predicts survival. First, the decile or centered rank is projected on score as well as competition-round-panel fixed effects (errors are clustered by competition-round-panel). The residuals from these regressions strongly predict abandonment. This supports the hypothesis that founders respond to rank information, and ex-ante distributional differences across feedback and non-feedback competitions do not explain the main result.

4.4.3. Matching estimators

An alternative approach to controlling for possible distributional confounders is a matching estimator. Exact and propensity score matching estimators adjust for "missing" potential outcomes by matching subjects in a treatment group to their closest counterparts in the untreated group. The difference between observed and predicted outcomes is the average treatment effect. Participants are matched on characteristics likely to predict survival.

Exact matching is preferable as there is no conditional bias in the estimated treatment effect (Abadie and Imbens 2006). The samples of above- and below-median losers were matched exactly on 13 sectors, competition year, student status, and company incorporation status. Balance tests of variables not used in matching are shown in Appendix Table A.8; the match dramatically reduces the differences. The exact matching result is in Appendix Table A.9 Panel 1 column 6, and yields nearly the full sample result, at 7.6 pp, significant at the .01 level. The second method is propensity-score matching, which first estimates the probability of treatment using a logit model and then for each treated participant identifies the untreated one with the closest probability.[23] Appendix Table A.11 shows that the matching brings the samples almost entirely in line. The estimate in Appendix Table A.9 Panel 1 column 7 finds an effect of 5.6 pp, significant at the .05 level.

4.4.4. Interacting feedback with distribution characteristics

As mentioned above, the primary concern is that the distribution of participants is correlated with feedback. Feedback could be more informative or impactful if ventures in feedback competitions have inherently more precise signals. To test this, it is useful to examine interactions between feedback characteristics likely associated with signal quality, venture survival, and participant diversity. Regressions that include interactions between

[23] Matching is without replacement, so variance estimation is uncomplicated by duplicates. Matching is also only on binary covariates, which reduces bias. The covariates those from the exact match plus prior external financing. Matches without common support are omitted, which reduces the matched sample by 408 ventures.

feedback and characteristics associated with venture survival are in Appendix Table A.12 Panel 1. Interactions with competition diversity are in Panel 2, and interactions at the venture level with predictors of success are in Panel 3. In all cases, the effect of $Low\ Rank \cdot Feedback$ persists.

4.4.5. Subsamples and functional form

The last set of robustness tests consider functional form and subsamples. First, Appendix Table A.9 Panel 1 column 4 shows that the result is robust controlling for score. A logit model in column 5 finds that feedback increases the odds of abandonment by about 39 percent, consistent with the main result. To ensure that higher average venture maturity in feedback competitions does not somehow explain the effect, Panel 2 column 2 restricts the sample to unincorporated ventures, and finds an effect of 13 pp.[24] The effect also persists among ventures from VC hub states (column 1). The coefficients are similar in magnitude to the main model within the population of founders with MBAs and among student-led ventures, but these are not significant, which may reflect smaller samples (columns 3-4). Column 5 restricts the sample to preliminary rounds and finds an effect of 11 pp.

5. Conclusion

This paper provides the first systematic, causal evaluation of whether and how new venture competitions are useful to participating startups. Winning has economically significant positive effects on subsequent financing, employment, and successful exit (acquisition/IPO). Notably, winning is quite useful in preliminary rounds and is most useful among those final round winners that do not receive cash prizes. The marginal winners appear to benefit most. Winning seems to serve a certification function, signaling venture

[24]In further unreported tests, the result remains roughly similar when competitions held at universities are excluded, and when ventures can enter the sample multiple times.

quality to the market. Cash prizes are useful, but their effect is small relative to the effect of winning and the predictive power of rank.

Competitions also hasten founders' type revelation about the quality of their ventures, which may be an additional reason that winning is useful and is also helpful to losers. The large effect of feedback is inconsistent with a characterization of entrepreneurs as so extremely overconfident that they do not learn about their own probability of success. Models that have made this assumption include Bernardo and Welch (2001), Bergemann and Hege (2005), and Landier and Thesmar (2009). These theories' behavioral perspective comes from evidence of cognitive biases such as over-precision and optimism in entrepreneurial decision-making.[25] The results in this paper are more consistent with models of firm dynamics in which learning plays a pivotal role, including Jovanovic (1982), Aghion, Bolton, Harris and Jullien (1991), and Ericson and Pakes (1995). New information determines entry and exit decisions in these models, implying that entrepreneurs should be sensitive to external signals about their project quality.

Finally, this paper has implications for competition organizers. Competitions seem to be most useful in generating information; signaling quality to the market, and own type to nascent entrepreneurs. The results suggest that rather than focusing on large cash prizes, competitions might consider directing resources to improving the quality of judging, feedback, and market signaling.

[25] See Astebro, Jeffrey and Adomdza (2007), Cooper et al. (1988), Camerer and Lovallo (1999), Arabsheibani et al. (2000), Koellinger et al. (2007), Kogan (2009), and Bloom et al. (2014). Financial contracting theory typically assumes that the entrepreneur knows his type or has static beliefs about it (Aghion and Bolton 1992, Admati and Pfleiderer 1994, Clementi and Hopenhayn 2006, Sørensen 2007, Hellmann 1998, Cagetti and De Nardi 2006, and Ewens, Jones and Rhodes-Kropf, 2013).

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Table 1: Summary Statistics

Panel 1: Competitions

	N	Mean	Median	S.d.	Min	Max
# competitions	87					
# competition-rounds	176					
# competition-round-panels	454					
# competitions with feedback	34					
# rounds per competition	87	2	2	.69	1	3
# ventures in preliminary rounds	113	45	35	43	6	275
# ventures in final rounds	86	19	12	21	4	152
# winners	176	8.4	6	7.2	1	37
Prize$\mid$ Prize>0 (thousand nominal $)	167	73	30	86	2	275
Days between rounds within competition	88	23	17	31	0	127
# judges in round-panel	543	17	9	23	1	178

Panel 2: Ventures

	N	Mean	Median	S.d.	Min	Max
# unique ventures	4,328					
# unique ventures in feedback competitions	1,614					
Venture age at first competition (years)	2073	1.9	0.77	3	0	20
Incorporated at round	4328	0.44	0	0.5	0	1
In hub state (CA, NY, MA)	4,328	.35	0	.48	0	1
Survival (Has >1 employee as of 8/2016)	4328	0.34	0	0.47	0	1
Abandonment (Does not have >1 employee as of 8/2016)	4328	0.66	0	0.47	0	1
Abandoned within 6 months†	3228	0.51	1	0.5	0	1
Abandoned within 1 year	3228	0.57	1	0.5	0	1
Abandoned within 2 years	3228	0.64	1	0.48	0	1
Has ≥ 3 employees as of 8/2016	4328	0.3	0	0.46	0	1
Has ≥ 10 employees as of 8/2016	4328	0.2	0	0.4	0	1
Raised external private investment before round	7099	0.16	0	0.36	0	1
External private investment after round	7099	0.24	0	0.43	0	1
Angel/VC series A investment before round	7099	0.09	0	0.29	0	1
Angel/VC series A investment after round	7099	0.15	0	0.36	0	1
Acquired/IPOd as of 9/2016	4328	0.03	0	0.18	0	1
Ventures in multiple competitions (#$\mid>1$)	558	2.52	2	0.98	2	9
# founders/team members at first competition	2305	3.1	3	1.6	1	8

Panel 3: Founders (Venture Leader - One Per Venture)‡

	N	Mean	Median	S.d.	Min	Max
# founders	3228					
# founders matched to LinkedIn profile	2554					
Age (years) at event (college graduation year-22)	1702	32.8	29	10.2	17	75
Female±	3,228	0.22				
Male	3,228	0.73				
Number of total jobs	2554	6.63	6	3.93	0	50
Number of jobs before round	2547	4.41	4	2.66	0	10
Number of locations worked in	2554	2.71	2	2.27	0	29
Days to abandon venture if abandoned**	1190	313	148	420	1	4810
Is student at round	2554	0.2				
Graduated from top 20 college	2554	0.27				
Graduated from top 10 college	2554	0.18				
Graduated from Harvard, Stanford, MIT	2554	0.1				
Has MBA	2554	0.48				
Has MBA from top 10 business school	2554	0.33				
Has Master's degree	2554	0.17				
Has PhD	2554	0.13				
Previous founder (founded different company before competition)	2554	.02				
Founder or CEO of subsequent venture after round	2554	0.17				

Note: This table contains summary statistics about the competitions (panel 1), ventures (panel 2), and founders/team leaders (panel 3) used in analysis. Data on ventures post-competition data is based on matches to CB Insights (752 unique matches), Crunchbase (638), AngelList (1,528), and LinkedIn (1,933). †1 if the number of days between the competition's end date and the first subsequent new job start date for the founder is less than 180, among ventures that did not survive and where the founder was matched to a LinkedIn profile. ‡From LinkedIn profiles. Not all competitions retained founder data, so the number of venture leaders is less than the number of ventures. ±Gender coding by algorithm and manually; sexes do not sum to one because some names are both ambiguous and had no clear LinkedIn match. **This is the number of days between the competition's end date and the first subsequent new job start date, among ventures that did not survive.

Table 2: Effect of Rank and Winning on Subsequent External Financing

Panel 1

Dependent variable: Financing after round

	(1)	(2)	(3)	(4)	(5)
Won Round	.088***	.13***	.079**	.17***	.15***
	(.018)	(.026)	(.036)	(.015)	(.019)
Decile rank	-.02***				
	(.0027)				
Decile rank winners		-.011***	-.0059		
		(.0044)	(.0054)		
Decile rank losers		-.018***	-.013***		
		(.0025)	(.0031)		
Within-judge decile rank				-.006***	
				(.0011)	
Z-score winners					.0074
					(.024)
Z-score losers					.031***
					(.011)
Prize (10,000$)	.0089***	.0085***	.0085***	.011***	.012**
	(.0023)	(.0024)	(.0029)	(.0034)	(.0055)
Venture controls	N	N	Y	N	N
Comp.-round-panel f.e.	Y	Y	Y	Y	Y
Judge & year f.e.	N	N	N	Y	N
N	6023	6023	3487	26663	3973
R^2	.16	.16	.4	.4	.19

Note: This panel shows regression estimates of the effect of winning, rank, and cash prize on whether the venture raised external financing after the competition using variants of Equation 1. The level of observation is a venture-round. Some rounds divide ventures into panels. Financing after round is an indicator for the venture raising private external investment after the round. "Decile rank" is the overall decile rank in the round, while "decile rank winners" and "decile rank losers" are, respectively, the decile rank within the round's winners and losers. A smaller rank is better (one is best decile, 10 is worst decile). Venture controls include whether the company received investment before the round, whether any of the venture's judges or those judges' firms ever invested in the venture, 17 sector indicator variables, company age, and whether the founder is a student. Competition fixed effects control for the date. Column 4 uses judge fixed effects, and the level of observation is judge-venture-round. Column 5 restricts the sample to competitions that did not provide feedback. Column 6 uses z-scores instead of ranks, and is restricted to the subsample of competitions that use scores before force-ranking participants. Errors clustered by competition-round-panel except in column 4, where they are clustered by judge. *** indicates p-value<.01.

Panel 2

				Bandwidth around cutoff	
				1	2
	(1)	(2)	(3)	(4)	(5)
Won Round	.13***	.13***	.17***	.16***	.17**
	(.017)	(.019)	(.019)	(.046)	(.077)
Centered rank	.0018***	.0018***			-.014
	(.00032)	(.00056)			(.026)
Centered rank^2		-2.9e-07			
		(3.9e-06)			
Centered rank winners			-.0066***		
			(.0019)		
Centered rank losers			.0023***		
			(.00042)		
Prize (10,000$)	.0095***	.0095***	.0088***	.0037	.007
	(.0024)	(.0024)	(.0024)	(.0095)	(.0062)
Comp.-round-panel f.e.	Y	Y	Y	Y	Y
N	6023	6023	6023	971	1712
R^2	.16	.16	.16	.52	.35

Note: This panel shows regression estimates of the effect of winning, rank, and cash prize on whether the venture raised external financing after the competition using variants of Equation 1. The level of observation is a venture-round. Some rounds divide ventures into panels. Financing after round is an indicator for the venture raising private external investment after the round. Centered rank is the venture's rank in the round centered around the cutoff for winning, such that a rank of 1 is the lowest-ranked winner, and -1 is the highest ranked loser. If there are three winners, the highest-ranked winner will have centered rank of 3. If there are 20 losers, the lowest-ranked loser will have centered rank of -20. Columns 4 and 5 restrict the sample to ranks immediately around the cutoff. Column 4 uses only one venture on either side of the cutoff, and column 5 uses two ventures on either side of the cutoff. Competition fixed effects control for the date. Errors clustered by competition-round-panel. *** indicates p-value<.01.

Table 3: Effect of Award and Round on Subsequent External Financing

Dependent variable: Financing after round

Sample:	All	Final rounds		Preliminary rounds		
			no prize winners			no final winners
	(1)	(2)	(3)	(4)	(5)	(6)
Won prelim round	.081***			.14***	.086***	.085**
	(.027)			(.03)	(.03)	(.034)
Won final round	.2***	.12**	.17*		.2***	
	(.033)	(.05)	(.085)		(.044)	
Decile rank winners	-.0096**	-.00047	-.01	-.015***	-.012**	-.011*
	(.0039)	(.0076)	(.012)	(.0052)	(.0049)	(.0058)
Decile rank losers	-.018***	-.019***	-.019***	-.018***	-.017***	-.017***
	(.0025)	(.004)	(.0041)	(.0031)	(.0031)	(.0031)
Prize (dummy)	.00079					
	(.032)					
Prize (10,000$)		.0052		.012***	.0032	
		(.0033)		(.0032)	(.0037)	
Comp.-round-panel f.e.	Y	Y	Y	Y	Y	Y
N	6023	1617	1286	4406	4406	4148
R^2	.17	.17	.12	.16	.17	.14

Note: This table shows regression estimates of the effect of winning, rank, and cash prize on whether the venture raised external financing after the competition using variants of Equation 1. The level of observation is a venture-round. Some rounds divide ventures into panels. Financing after round is an indicator for the venture raising private external investment after the round. Column 2 restricts the sample to final rounds, and column 3 to ventures in final rounds that did not win a cash prize. Columns 4-7 restrict the sample to preliminary rounds. Columns 6-7 further restrict the sample to ventures in preliminary rounds that did not ultimately win a prize. "Decile rank" is the overall decile rank in the round, while "decile rank winners" and "decile rank losers" are, respectively, the decile rank within the round's winners and losers. A smaller rank is better (one is best decile, 10 is worst decile). Competition fixed effects control for the date. Errors clustered by competition-round-panel. *** indicates p-value<.01.

Table 4: Effect of Rank and Winning on Additional Outcomes

Sample:	All				Preliminary rounds			Bandwidth of 2 around cutoff		
Dependent variable:	Survival	Founder subsequent entrep.	10+ employees	Acquired/ IPO	Survival	10+ employees	Acquired/ IPO	Survival	10+ employees	Acquired/ IPO
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Won Round	.047*	-.00053	.051*	.018	.05	.063**	.026**	.077**	.059**	.0091
	(.028)	(.02)	(.027)	(.012)	(.031)	(.03)	(.012)	(.03)	(.029)	(.013)
Decile rank winners	-.006	-.0013	-.0041	-.0028*	-.0059	-.0045	-.0035**			
	(.0043)	(.0027)	(.0044)	(.0017)	(.0052)	(.0051)	(.0018)			
Decile rank losers	-.023***	.0012	-.017***	-.0011	-.024***	-.016***	-.0008			
	(.0028)	(.0016)	(.0023)	(.001)	(.0031)	(.0027)	(.0012)			
Prize (10,000$)	.0062*	-.00059	.0074***	.0002				.0053	.0082*	-.0019
	(.0032)	(.0013)	(.0026)	(.0013)				(.0051)	(.005)	(.0012)
Comp.-round- panel f.e.	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
N	6023	6023	6023	6023	4406	4406	4406	1712	1712	1712
R^2	.17	.32	.14	.083	.16	.13	.094	.4	.36	.3

Note: This table shows regression estimates of Equation 1. Survival is one if the venture had ≥ 1 employee besides the founder on LinkedIn as of 8/2016. 10+ employees is defined analogously. Acquired/IPO indicates that the venture was acquired by another company or went public. In columns 1-4, the whole sample is used. In column 2, the dependent variable is one for founders that subsequently founded or were the CEO of another company. This could reflect unidentified name changes, but if not, it is a measure of serial entrepreneurship. In columns 5-7, only preliminary rounds are included. In columns 8-10, the sample is restricted to a bandwidth of 2 ventures on either side of the cutoff for winning. The level of observation is a venture-round. Some rounds divide ventures into panels. Rank is defined as in Table 3. Competition fixed effects control for the date. Errors clustered by competition-round-panel. *** indicates p-value<.01.

Table 5: Heterogeneity in Effect of Rank and Winning on External Finance

Dependent variable: Financing after round

C :	Founder BA from top 10 college	Founder founded previous company	Selective competition
	(1)	(2)	(3)
Won Round	.13***	.097***	.13***
	(.028)	(.035)	(.037)
Won Round$\cdot C$	-.0027	.049	.012
	(.081)	(.058)	(.053)
Decile rank winners	-.011**	-.0057	-.012**
	(.0046)	(.0059)	(.0055)
Decile rank winners$\cdot C$	-.0039	-.013*	.0042
	(.013)	(.0077)	(.01)
Decile rank losers	-.018***	-.015***	-.02***
	(.0026)	(.003)	(.0029)
Decile rank losers$\cdot C$	.0068	-.0075	.0094*
	(.0083)	(.0048)	(.0054)
Prize (10,000$)	.0098***	.013***	.0076***
	(.0024)	(.0029)	(.0026)
Prize (10,000$)$\cdot C$	-.013**	-.0084**	.0079
	(.0056)	(.004)	(.007)
C	.076	.14***	-
	(.049)	(.03)	
Comp.-round- panel f.e.	Y	Y	Y
N	6023	6023	6023
R^2	.17	.18	.16

Note: This panel shows OLS regression estimates of heterogeneity in the effect of winning, rank, and cash prize on whether the venture raised external financing after the competition. The characteristic in the column header is interacted with all covariates except the competition fixed effects. Financing after round is an indicator for the venture raising private external investment after the round. A smaller rank is better (one is best decile, 10 is worst decile). Competition fixed effects control for the date. Errors clustered by competition-round-panel. *** indicates p-value<.01.

Table 6: Effect of Criteria Rank on Venture Outcomes

Dependent variable:	Financing after round		10+ Employees		Acquired/IPO	
	(1)	(2)	(3)	(4)	(5)	(6)
Decile rank in round:						
Team	-.021***	-.023***	-.0091	-.017***	.00069	-.0012
	(.0057)	(.0053)	(.0063)	(.0049)	(.0026)	(.0024)
Financials	-.014**	-.0079	-.036***	-.026***	.0034	.0023
	(.0067)	(.005)	(.0083)	(.0057)	(.0031)	(.0027)
Business Model	.0032	.002	.0024	.0035	.0046	-.0059
	(.016)	(.011)	(.014)	(.011)	(.0074)	(.0074)
Market	.01	-.0091	.0075	-.011	-.00047	.0039
	(.015)	(.011)	(.013)	(.011)	(.0072)	(.0074)
Tech./Product	.0098	.0031	-.0015	-.0081	-.0062**	-.0056**
	(.0078)	(.0054)	(.0069)	(.0054)	(.0024)	(.0024)
Presentation	-.015**	-.0098**	.0074	.008	-.0032	-.0013
	(.0059)	(.0043)	(.0071)	(.0052)	(.0024)	(.0022)
Won Round	.14***	.2***	.1***	.17***	.011	.023***
	(.024)	(.013)	(.032)	(.015)	(.013)	(.0068)
Judge/judge co invested	.47***	.56***				
	(.11)	(.027)				
Comp,-round-panel f.e.	Y	N	Y	N	Y	N
Judge f.e.	N	Y	N	Y	N	Y
N	1926	8794	1926	8794	1926	7043
R^2	.15	.14	.13	.12	.065	.066

Note: This table contains OLS regression estimates of the effect of criteria-specific ranks on indicators for venture outcomes. The level of observation is a venture-round. Some rounds divide ventures into panels. Financing after round is an indicator for the venture raising private external investment after the round. 10+ employees is one if the venture had $\geq$ 10 employees besides the founder on LinkedIn as of 8/2016. The criteria scores are averaged to produce the overall ranks used in other tables. A smaller decile rank is better. Competition fixed effects control for the date. Errors clustered by competition-round-panel or judge, depending on fixed effects. *** indicates p-value<.01.

Table 7: Robustness Tests of Effect of Winning

Dependent variable: Financing after round

				Logit	Feedback	No feedback
	(1)	(2)	(3)	(4)	(5)	(6)
Won Round	.13***	.13***	.13***	.71***	.13***	.17***
	(.03)	(.026)	(.027)	(.14)	(.034)	(.04)
Decile rank winners	-.011**	-.012***	-.012**	-.069***	-.0091	-.017***
	(.0043)	(.0043)	(.0047)	(.021)	(.0061)	(.0063)
Decile rank losers	-.018***	-.018***	-.017***	-.13***	-.011***	-.025***
	(.0027)	(.0025)	(.0026)	(.017)	(.0033)	(.0033)
Within-judge decile rank						
Prize (10,000$)	.0085***	.0088***	.0067*	.036***	.011**	.0068**
	(.0023)	(.0023)	(.0039)	(.011)	(.0055)	(.0027)
Comp.-round-panel f.e.	Y	Y	Y	Y	Y	Y
Judge & year f.e.	N	N	N	N	N	N
N	6023	5998	4920	5484	3422	2601
R^2	.16	.16	.17	.12	.2	.13

Note: This table shows regression estimates of the effect of winning, rank, and cash prize on whether the venture raised external financing after the competition using variants of Equation 1. The level of observation is a venture-round. Some rounds divide ventures into panels. Financing after round is an indicator for the venture raising private external investment after the round. "Decile rank" is the overall decile rank in the round, while "decile rank winners" and "decile rank losers" are, respectively, the decile rank within the round's winners and losers. A smaller rank is better (one is best decile, 10 is worst decile). Column 1 clusters errors by competition. Column 2 omits ventures in which a judge or judge's firm invested. Column 3 restricts observations to a venture's first competition. Column 4 uses a logit model instead of OLS. Column 5 restricts the sample to competitions that gave participants feedback (informed them of their rank in the round), while column 6 restricts the sample to competitions that did not provide feedback. Competition fixed effects control for the date. Errors clustered by competition-round-panel except in columns 1 and 4, where they are clustered by competition and judge, respectively. *** indicates p-value<.01.

Table 8: Effect of Negative Feedback on Venture Abandonment

Panel 1

Sample:	Losers					Winners
Dependent variable:	Abandonment		Founder subsequent entrep.	Abandoned within 6 months	1 year	Abandonment
	(1)	(2)	(3)	(4)	(5)	(6)
Low rank·Feedback	.083**	.067*	-.017	.076*	.081*	
	(.04)	(.04)	(.02)	(.044)	(.045)	
Low rank	.06***	.066***	.027	.049**	.056**	
	(.021)	(.022)	(.017)	(.022)	(.023)	
High rank·Feedback						-.13*
						(.073)
High rank						-.019
						(.061)
Venture controls	N	Y	N	N	N	N
Comp.-round-panel f.e.	Y	Y	Y	Y	Y	Y
N	4405	3928	4405	4405	4405	1571
R^2	.17	.21	.34	.15	.16	.39

Note: This panel shows estimates of the effect of negative feedback within the sample of losers (having a below-median rank among losers when participating ventures learn their ranks, relative to competitions where they do not learn their ranks). The level of observation is a venture-round. "Low rank" is one if the venture's rank is below median among losers. Sample restricted to losers of rounds, except in column 7. The dependent variable in columns 1-2 and 7 is abandonment, which is 0 if the venture had ≥ 1 employee besides the founder on LinkedIn as of 8/2016, and 1 otherwise. Abandonment is indistinguishable from failure. In column 3, the dependent variable is an indicator for founding a subsequent venture. The dependent variables in columns 4-5 are based on time to abandon, which uses the founder's next job start date conditional on abandonment. For example, in column 4, the dependent is one if the venture is abandoned and the founder has a new job within six months. Column 6 restricts the sample to winners and tests the effect of positive feedback (above median rank). It also controls for cash prize amount. Venture controls include whether the company received investment before the round, whether any of the venture's judges or those judges' firms ever invested in the venture, 17 sector indicator variables, company age, and whether the founder is a student. Errors clustered by competition-round-panel or judge, depending on fixed effects. *** indicates p-value<.01.

Panel 2

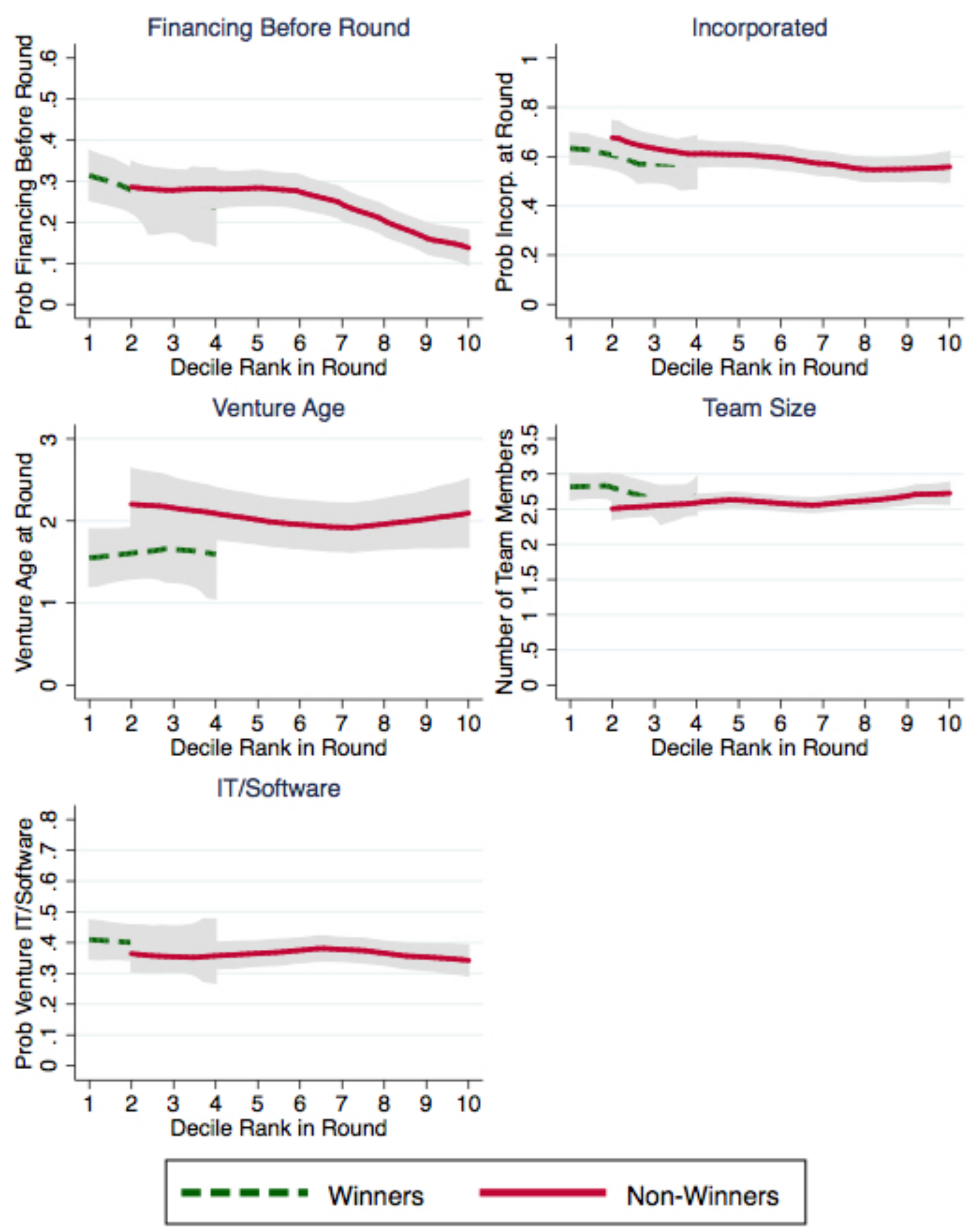
Dependent variable: Abandonment

Sample: Losers, feedback competitions only

	(1)	(2)	(3)	(4)
Nominal score	-.08***	-.056**		
	(.02)	(.021)		
Decile rank	.013*			
	(.0072)			
Centered rank		-.0019**		
		(.00075)		
Decile rank residual			.048***	
			(.0059)	
Centered rank residual				-.0036***
				(.00068)
Venture controls	Y	Y	Y	Y
Year f.e.	Y	Y	Y	Y
N	2044	2044	2044	2044
R^2	.045	.081	.077	.075

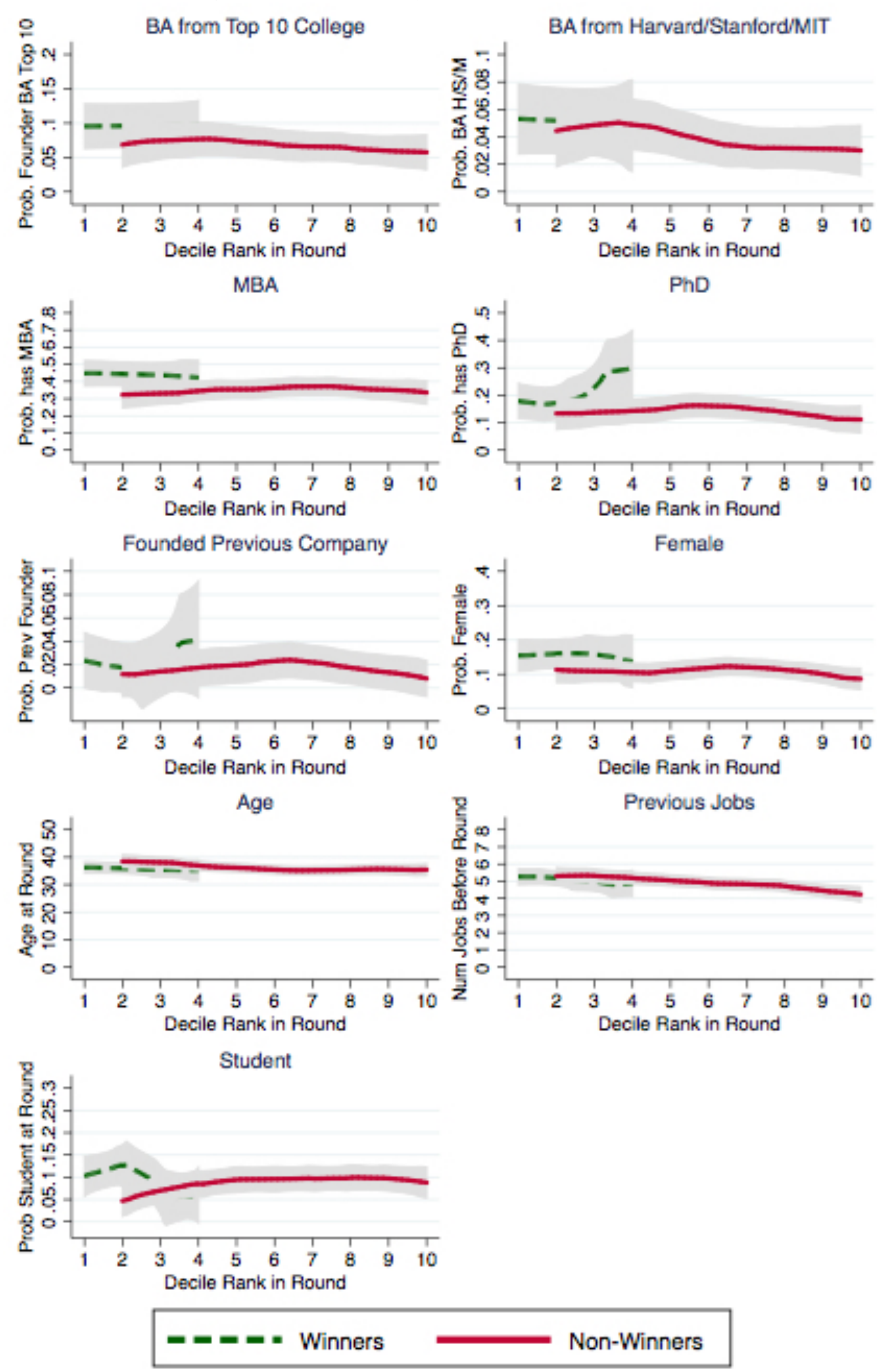
Note: This panel exploits nominal scores to estimate the effect of negative feedback (captured by information in rank) within the sample of losers in feedback competitions. The level of observation is a venture-round. The dependent variable is abandonment, which is 0 if the venture had ≥ 1 employee besides the founder on LinkedIn as of 8/2016, and 1 otherwise. Abandonment in this context is indistinguishable from failure. Nominal score is the underlying venture score that is ordered by the competitions to produce ordinal ranks, and varies from 1 (worst) to 5 (best). It is not available for some competitions in which judges rank and do not score ventures. Columns 1-2 identify the effect of feedback as the effect of rank after controlling for nominal score. Columns 3-4 use decile and centered rank residuals. These residuals are taken from a projection of rank on score (which also includes competition-round-panel fixed effects and clusters errors by competition-round-panel). Venture controls as in previous tables. Errors clustered by competition-round-panel. *** indicates p-value<.01.

Figure 1: Continuity of Venture Covariates



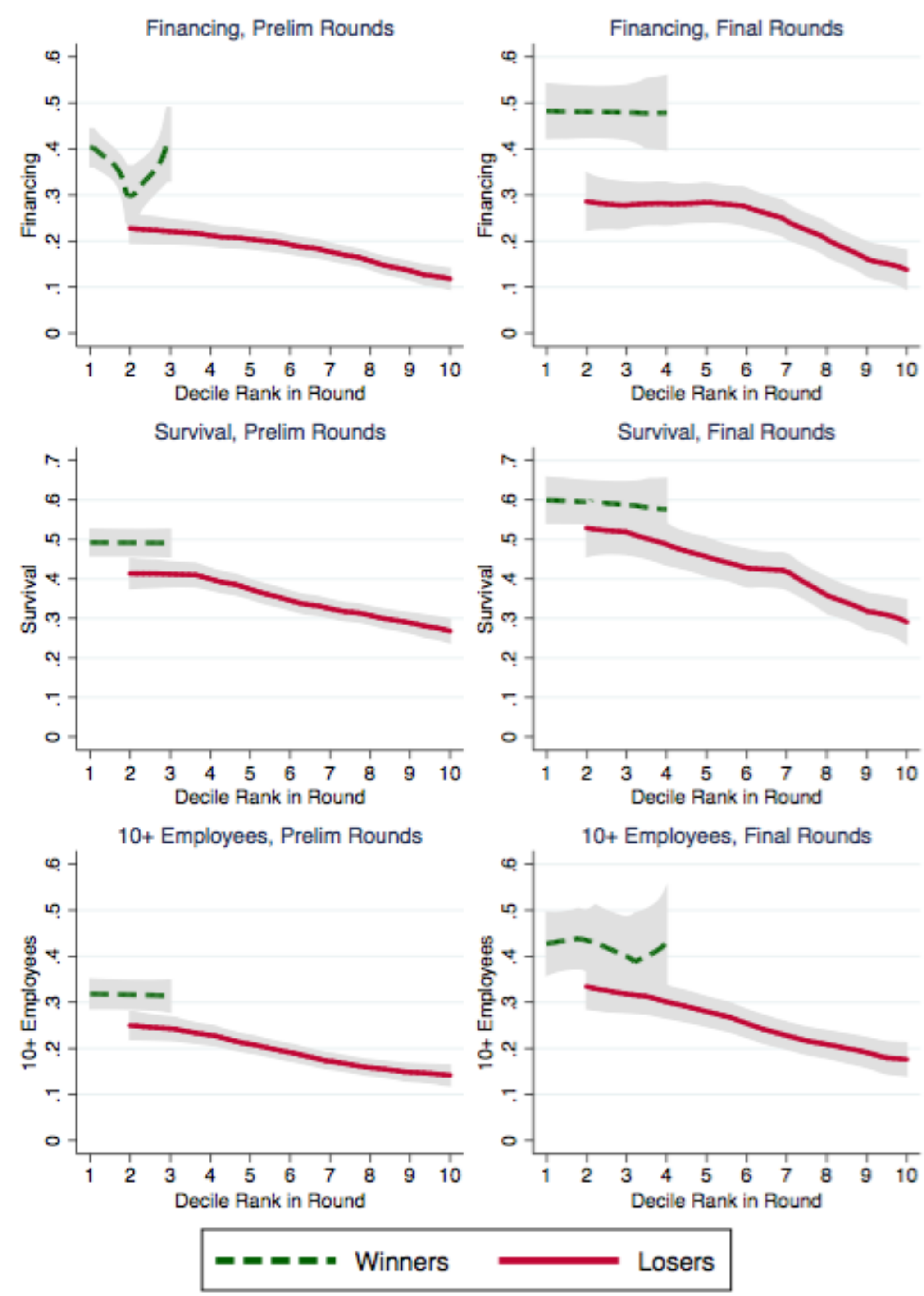
Note: This figure shows probabilities of venture-specific covariates observed at the time of the competition by percentile rank in the round (lower percentile rank is better). Final rounds are used. There are no losers in the top bin, and winners are truncated at the fifth decile. The lines overlap because the share of participants that win varies across rounds. Local polynomial with Stata's optimal bandwidth. 95% CIs shown.

Figure 2: Continuity of Founder Covariates



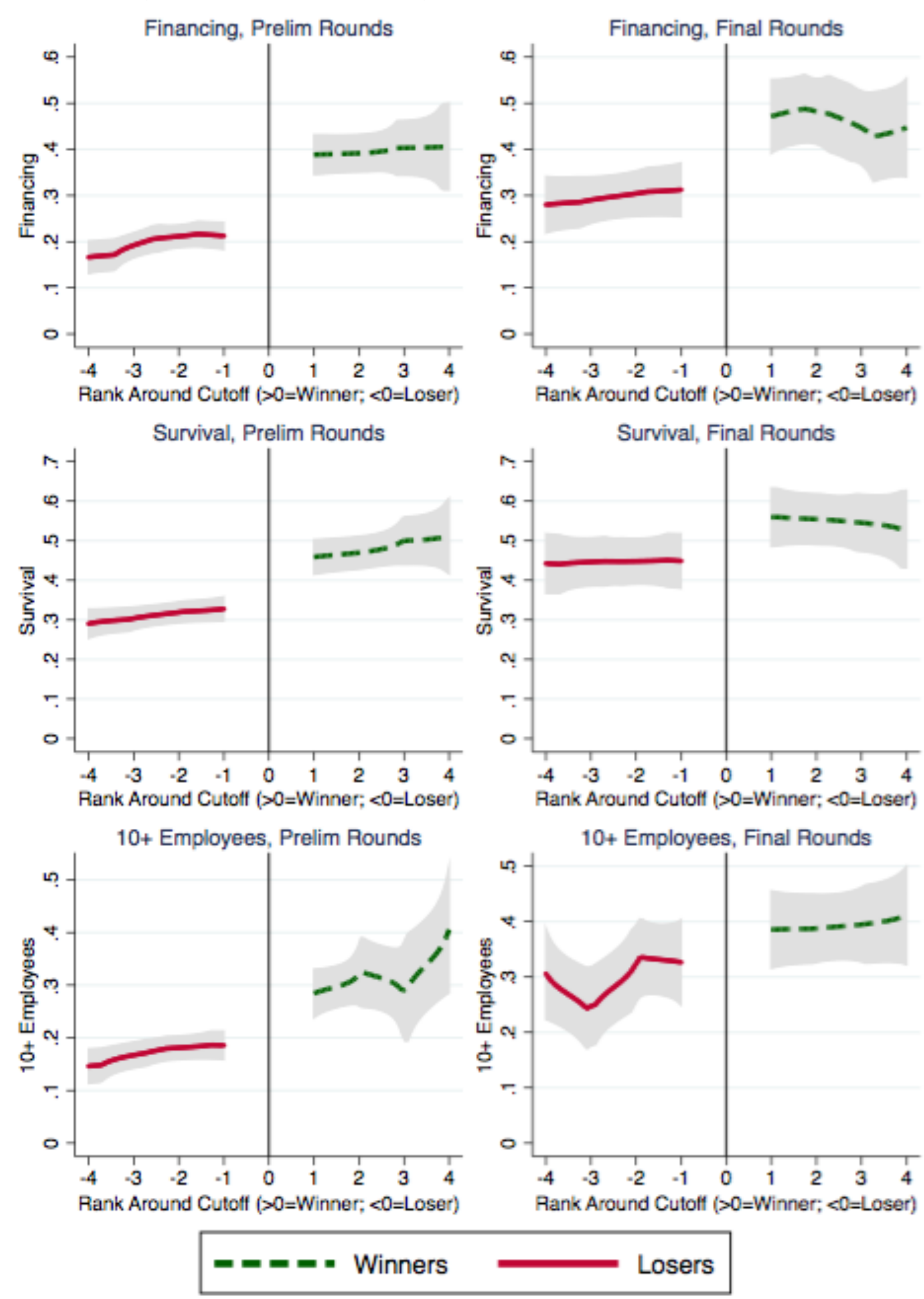
Note: This figure shows probabilities of founder-specific covariates observed at the time of the competition by percentile rank in the round (lower percentile rank is better). Final rounds are used. There are no losers in the top bin, and winners are truncated at the fifth decile. The lines overlap because the share of participants that win varies across rounds. Local polynomial with Stata's optimal bandwidth. 95% CIs shown.

Figure 3: Effect of winning with percentile rank



Note: This figure shows probabilities of any subsequent financing (top), survival (middle), and having 10+ employees (bottom) by percentile rank in the round (lower percentile rank is better). There are no losers in the top bin, and winners are truncated at the fourth and fifth decile for preliminary and final rounds, respectively. The lines overlap because the share of participants that win varies across rounds. Local polynomial with Stata's optimal bandwidth. 95% CIs shown.

Figure 4: Effect of winning with centered rank around cutoff



Note: This figure shows probabilities of any subsequent financing (top), survival (middle), and having 10+ employees (bottom) by the venture's centered rank around the cutoff for winning. Centered rank improves from left to right. A rank of 1 indicates the lowest ranked ranked winner (the winner with the worst rank, which just barely won). A rank of -1 indicated the highest ranked loser (the loser which just barely lost). Local polynomial with Stata's optimal bandwidth. 95% CIs shown.