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ABSTRACT

This paper uses administrative data from 87 new venture competitions in 17 U.S. states to show that winning has large, positive effects on measures of subsequent venture success, including employment and financing. While cash prizes are valuable, especially for founders who are likely financially constrained, winning is independently useful. Certification may be one mechanism, but it does not seem to be the primary one. An alternative is that competitions help entrepreneurs learn about their projects' quality. Receiving negative feedback is shown to increase venture abandonment, suggesting that competitions are useful in part because they facilitate faster type revelation.

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An online appendix is available at http://www.nber.org/data-appendix/w23874

1. Introduction

New venture competitions, sometimes called business plan or "pitch" competitions, have become a ubiquitous feature of the high-growth entrepreneurship ecosystem. In these competitions, early stage startup founders present their businesses to a panel of expert judges. Judge scores determine which ventures win, and at least some winners receive cash prizes. Sponsored by universities, foundations, governments, and corporations, among other institutions, competitions aim to serve convening, certification, education, and financing functions.

This paper asks whether and how these competitions are useful to entrepreneurs, using novel data on 4,328 new ventures participating in 87 competitions in 17 states between 2007 and 2015. 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. The ventures are linked to employment, financing, and survival outcomes, with care taken to account for name changes. They are roughly representative of the U.S. startup population, with no local subsistence businesses – such as restaurants or landscapers – that often contaminate efforts to study high-growth entrepreneurship (Levine and Rubinstein 2016). Data are also collected on founders' education and career histories, which sheds new light on founder characteristics that are associated with startup success. For example, founder job experience or having a software venture are associated with success, while having an MBA or a hardware venture are not.

The effect of winning can be measured using a regression discontinuity design. 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 and rank. Winning also increases survival, having at least 10 employees, and being acquired or going public.[1] All of these effects are stronger in preliminary rounds among ventures that ultimately won no prize, relative to final rounds. The effect on acquisition/IPO is strong in preliminary rounds but loses significance when all rounds are included.

There are three primary ways in which competitions may be useful to startups: cash prizes, certification, and learning. The results indicate that while cash prizes are useful, the effect is small in economic magnitude relative to the overall effect of winning and the predictive power of rank. It is roughly half the magnitude of the effect of U.S. Department of Energy SBIR grants found in Howell (2017). Consistent with the cash prize alleviating financial constraints, it is less useful for elite founders and for serial entrepreneurs, who may be wealthier or have better access to investor networks.

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 dimension scores in most competitions. Of these, the team rank is the strongest predictor of initial 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).

The large effect of winning and the predictive power of rank suggest that competitions produce useful information about venture quality, supporting a certification channel. However, three tests point away from certification as the primary mechanism. It is therefore worth exploring the third channel: Competitions may be useful because they create learning opportunities. Of particular interest is learning in the sense of entrepreneur type revelation. (Learning in the sense of improvement is more straightforward.) Winners may push

[1] The primary measure of survival is whether a venture has at least one employee besides the founder as of August 2016. Similarly, having at least 10 employees is as of August 2016. These measures contain truncation bias, but it is at least partially mitigated by year fixed effects. Unfortunately the source of the data, LinkedIn, does not permit observing historical firm data.

forward with their ventures because they correctly interpret winning as a positive signal.

To test this possibility, it is necessary to isolate the effect of the rank signal. In 53 of the competitions, ventures are informed only that they won or lost. In 34 of the competitions, ventures are privately informed of their overall and dimension ranks in the round (but never individual judge ranks). The competitions are otherwise similar, and in the feedback competitions neither ventures nor judges are informed that ventures would subsequently learn their ranks. The effect of negative feedback on venture continuation is identified with a difference-in-differences model among non-winning ventures. The first difference is within round, comparing below-median and above-median non-winners. The second difference is across rounds, comparing ventures that were informed of their rank with those that were not. This estimates the effect of a low rank with knowledge of that rank, relative to a low rank without such knowledge. Receiving negative feedback increases abandonment by about 14 percent of the mean. The effect primarily occurs in the first six months after the competition. It is also roughly symmetrical among winners without cash prizes.

The empirical concern is whether this effect reflects systematically different distributions among non-winners in the two types of competitions (differences in levels are absorbed). To address this concern, I use three tests and five robustness exercises. The three tests show that the distributions of observables across the two types of competitions are similar ex-ante, and that entrepreneurs do not select into feedback. One example of the robustness tests 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 example is finding a similar effect within a single competition that gave feedback in one year but not others.

Heterogeneity in responsiveness to feedback is consistent with two interpretations (though it does not rule out alternatives). First, if founders treat their ventures as real options, they should be less responsive – delaying abandonment despite negative feedback – when the venture is more uncertain and has more asset specificity, or irreversibility of investment (Dixit and Pindyck 1994). Indeed, riskier ventures are less responsive, as are ventures with prior external financing, which likely have higher sunk costs and thus greater investment irreversibility. Second, founders update in a manner consistent with Bayes' rule, which dictates how rational agents update their beliefs. They are more responsive when the signal is more precise, proxied with the number of judges. Feedback also matters less when they have a more precise prior. Finally, non-linearity in the effect could be consistent with cognitive biases, because rank predicts success in a linear way. Instead, the effect of feedback is roughly linear. Motivated by this evidence, a simple Bayesian framework is used to model and calibrate sensitivity to feedback.

Understanding how competitions are useful and which entrepreneurs learn can help inform the theory of entrepreneurship. Competitions may reduce 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). Certification most obviously facilitates matching by reducing information asymmetry, but cash and learning could also do so. Ventures can use cash to generate more informative signals, for example by prototyping their products. Learning in the sense of type revelation may reduce the number of poor quality startups seeking financing, allowing venture capitalists to more carefully consider the remainder. More generally, the results are consistent with entrepreneurship being a process of experimentation, as in Kerr, Nanda and Rhodes-Kropf (2014) and Manso (2016).

This analysis provides, to my knowledge, the first evaluation of the effect of winning new venture competitions in the developed world. This is relevant for policy, as many competitions are publicly funded.[2] Governments view these programs as a means to foster high-growth entrepreneurship either in a specific region or in a sector perceived to have high social benefits. A related evaluation is McKenzie (2017)'s analysis of a competition in Nigeria. Other work studies accelerator and mentorship programs, including 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 literature on peer effects in entrepreneurship, including Nanda and Sørensen (2010) and Lerner and Malmendier (2013).

2. New venture competition data

This section first introduces the new venture competition data. Section 2.2 presents summary statistics. 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, sometimes called business plan or "pitch" competitions, have proliferated in the past decade. In a competition, new venture founders present their technologies and business models to a panel of judges. New venture competitions are now 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. 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

[2] Two examples 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.

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.

This paper uses data from 87 competitions between 2007 and 2016, summarized in Table 1.[3] 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). There are 558 ventures that participate in multiple competitions. The mean award amount is $73,000. The individual competitions are listed in Appendix Table A1. 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 dimensions (or "criteria"): Team, Financials, Business Model, Market Attractiveness, Technology/Product, and Presentation. These dimension 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.[4] Only winning participants are typically listed on a program website, and

[3]The data were obtained individually from program administrators and from Valid Evaluation.

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

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 a venture.[5]

This paper uses three transformations of the rank and score data.[6] 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. The second transformation is judge decile ranks, calculated among ventures that the judge scored. The third is z-scores 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. Average venture age is 1.9 years.[7] Forty-four percent of the ventures are incorporated at the round date as a C- or S-corp. Ventures are matched to investment events and employment using CB Insights, Crunchbase, AngelList, and LinkedIn.[8] In researching the ventures, 765 name changes were identified. Ventures are matched to private investment on both original and changed names.

Venture survival is a binary indicator for the venture having at least one employee besides the founder on LinkedIn as of August 2016. While some startups may not initially

average.

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

[6]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.

[7]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.

[8]For LinkedIn, only public profile data is used by non-logged-in users, based on Google searches for person and school or firm.

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, as it induces truncation bias (though this will be at least partially mitigated by time fixed effects). The source of the data, LinkedIn, does not permit observing historical firm data. However, the available alternative, the presence of a website, is a a poor survival measure because websites often stay active long after a venture has failed.

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).[9] Elite degree status is tabulated using the university ranking in Appendix Table A2. 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 A3, 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 sorted by state in Appendix Table A4. 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.

[9] 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.

2.3. Sample representativeness

There is little empirical analysis of startups prior to their first external funding event, but the data are roughly representative of first-time, early stage startups and their founders in the U.S. Appendix Table A5 compares the 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. Each venture team averages three members. This is similar to Bernstein, Korteweg and Laws (2017), who note that on the AngelList platform, the average number of founders is 2.6. The median founder age, based on subtracting 22 from the college graduation year, is 29 years. This is roughly representative of startup founders.[10]

Associations between venture characteristics and success accord with common knowledge. In Appendix Table A6 panel 1, two measures of success, subsequent angel/VC investment and having at least 10 employees as of August 2016, are regressed on venture and founder characteristics. More founder job experience, being an IT/software (rather than

[10]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/).

hardware) venture, being located in a VC hub state, and having prior financing are all strongly associated with both measures of 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]

2.4. 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, Valid Evaluation, to manage their judging software. Valid Evaluation believes that formal feedback might be useful and sends each venture an email after the round containing their overall and dimension ranks. Ventures learn only their own ranks, and not those of other participants. Interviews with competition 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

[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.

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?

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.[12] On average the differences in the true distance

[12]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.

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).

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 very unlikely because while the number of awards is generally known ex-ante, judges score independently and typically only score a subset of participating ventures. Reassuringly, observable baseline covariates and pre-assignment outcome variables are smooth around the cutoff. Figure 1 uses local polynomials to show that venture variables observable at the time of the competition, such as previous financing and whether the venture is incorporated, are continuous across winners and losers in final rounds. Similar continuity exists for preliminary rounds. In these graphs, the venture's decile rank in the round is on the x-axis. The lines overlap because the share of participants that win varies across rounds.[13] 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.

3.2. Main effect of winning

Visual evidence of the effect of winning is in Figure 3, which contains the same local polynomials as the previous figures, but with post-competition outcomes on the y-axis. The top two graphs show the probability of subsequent external financing in preliminary and final rounds.[14] The bottom two graphs repeat this exercise for having at least ten employees.

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

In all four cases, the winner line lies above the non-winner line, indicating a substantial raw effect of winning.

Estimates of Equation 1 are in Table 2. The dependent variable is subsequent external financing, which is a proxy for early stage startup success and an explicit goal of the competition organizers. In panel 1, final and preliminary rounds are included, so a venture can appear multiple times. Columns 1 and 2 use overall decile rank, while columns 3-5 separately control for decile ranks within winners and non-winners. Starting in column 2, the prize amount if any is included as a control. The preferred specification in column 3 finds that winning a round increases a venture's chances of subsequent external finance by 13 percentage points (pp), relative to a mean of 24 percent, significant at the .01 level. To assess the effect of winning near the cutoff, in column 4 only the bottom quintile among winners (quintile 5) and the top quintile among losers (quintile 1) are included. The effect increases to 17 pp. Adding venture controls in column 5 reduces the effect to 8 pp, though the sample is much smaller. A logit model in column 6 finds roughly a doubling, because it drops groups without successes (i.e. panels without financing events). Observations in column 7 are at the judge-venture level. This model includes judge fixed effects and controls for the venture's decile rank within ventures that the judge scored. It finds a larger effect of winning, at 17 pp.

Preliminary and final rounds are distinguished in Table 2 panel 2. In columns 1-2, the sample is restricted to preliminary rounds, and further to ventures that ultimately won no prize in column 2. The effect increases in both cases; to 14 and 15 pp respectively. In column 3, the sample is restricted to final rounds, and finds an effect of 8.9 pp significant only at the .1 level. The remaining columns of Table 2 panel 2 contain robustness checks. In column 4, 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. The sample is restricted to ventures participating in their first competition in column 5. In order to ensure that feedback does not cause the effect of winning, column 6 restricts the sample to competitions without feedback. All three of these (columns 4-6) yield precisely the same effect of winning as the primary specification. In unreported specifications, errors are clustered at the competition and competition-round level rather than the competition-round-panel level, because a venture's ranks across different rounds might be correlated. The precision of β_1 in these models does not fall below the .01 level. Finally, column 7 finds a similar effect controlling for z-scores (based on nominal scores) rather than percentile ranks.

Three additional outcomes – survival, having at least 10 employees as of August 2016, and being acquired or going public – are considered in Table 3. Columns 1-3 include preliminary and final rounds, while columns 4-6 limit the sample to ventures in preliminary rounds that won no prize. Preliminary rounds drive the positive effects of winning on all three outcomes. Survival is not necessarily a measure of success, but it is included here because it is central to the feedback analysis in Section 4. Across all rounds, winning increases the chance of survival by 4.7 pp, significant at the .1 level (column 1). Winning increases survival within preliminary rounds by 8.7 pp, significant at the .05 level (column 4). It similarly increases the chances of having at least 10 employees by 5 pp across all rounds, and 10 pp in preliminary rounds (columns 2 and 5). The effect on acquisition/IPO is not quite significant in all rounds (column 3) but is 3.7 pp and significant at the .05 level in preliminary rounds. This effect is large in economic magnitude; it is more than 100 percent of the mean.

There is no meaningful or robust heterogeneity in the effect of winning across venture, competition, or founder types. For the purposes of tests below, Table 4 considers three sources of variation: whether the competition is selective, whether the founder graduated from a top 10 college, and whether the founder previously was the CEO or founder of a different venture (i.e., serial entrepreneurs). All covariates are interacted with the characteristic indicator (C). The coefficients on the interaction between winning and C are all near zero and imprecise. The independent coefficient, giving the effect when $C = 0$, is generally about the same as in the primary specification.

3.3. Channel 1: Cash

Non-dilutive cash may be helpful if ventures use it to build initial versions or prototypes of their products before seeking external financing. This will give prospective investors more precise signals about venture quality. Cash may also improve the bargaining position of the entrepreneur or reduce the amount of outside equity needed.

Independently of winning, the cash prize is also useful, with positive effects on financing, survival, and employment (Tables 2 and 3). 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 (e.g., more prestigious competitions may give larger prizes), competition fixed effects should absorb this variation. Table 2 panel 1 columns 2-3 shows that an extra $10,000 increases the probability of financing by nearly 1 pp. This effect seems small in economic magnitude relative to the overall effect of winning and the predictive power of rank, discussed below.[15] It is also 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 8 percent of the sample mean, while the effect of an additional $10,000 in competition prize money is 1 pp, or 4 percent of the sample mean.[16]

The cash prize is significantly less useful for elite founders and for serial entrepreneurs. This is shown in Table 4 columns 2-3 through the interaction Prize (10,000$)$\cdot C$. These

[15] Depending on the specification, winning is separately identified because of the variation in prize amount, because not all competitions have prizes, and because in some competitions not all winners receive cash prizes.

[16] 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.

results suggest that cash awards are more useful for founders that are likely more financially constrained. 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.

3.4. 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 way that competitions are useful, winning is likely to exhibit three types of heterogeneity.

First, in a certification mechanism, winning should be more useful in final rounds. 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.[17] Yet Section 3.2 showed that winning is most useful in preliminary rounds, and when it does not involve prize money. Note this test assumes that information asymmetry between ventures and the market is the same across rounds. Final rounds likely have higher quality ventures, and 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.

A second test for certification comes from variation in competition prestige. Winning a selective competition in which not all prospective participants are allowed to compete may be a stronger signal. However, there is no difference for selective competitions (Table 4 column 1).[18] Third, founders with stronger or more precise signals independently of winning should

[17]Based on conversation with competition participants and early stage investors.

[18]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

benefit less from certification. In this case, winning should be less useful for founders with elite backgrounds, who likely send stronger 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 4 columns 2-3).

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 clearly do not serve a certification function, and instead are useful solely because the cash award funds prototyping. However, the three tests presented in this section all point away from certification as a primary mechanism, suggesting that it may be fruitful to look to other ways in which informative signals could be useful to entrepreneurs.

3.5. Channel 3: Informative signals

A striking finding from Tables 2-4 is that the coefficients on rank and z-score are large and robust. Particularly within non-winners – a much larger sample – rank and z-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 3). Rank is also predictive within judge and persists within the no-feedback competitions, where it is impossible that the judge's ranks directly affect venture outcomes (Table 2 panel 1 column 7 and Table 2 panel 2 column 6). Further, Appendix Table A7 uses indicator variables for each decile of rank, while also controlling for winning. The top decile dummy is omitted, and the others all have large, negative coefficients that increase stepwise from -.065 for the second decile to -.18 for the tenth decile. All are significant at the .01 or .05 level.

This predictive power of rank contrasts with the uninformative SBIR grant ranks in

prestigious by local venture capitalists.

Howell (2017). 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. 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.

The dimension ranks are also informative. Table 5 shows the association between dimension 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 dimension 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.

In sum, this section has shown that winning is useful, and that through the judge ranks, competitions generate valuable signals. While certification and the cash prize are likely useful to winners, the larger benefit of winning an early round and the predictive power of rank signals suggest that participation (more broadly than simply winning) may be useful because of the opportunity to learn from the judges' expert opinions. The next section tests this hypothesis directly.

4. Responsiveness to feedback

Competitions may be useful because they create learning opportunities. Winning is a binary transformation of the underlying ranking information, which is not observed in the no-feedback competitions, where it is still informative about startup outcomes, including survival. Winners may push forward with their ventures because they correctly interpret winning as a positive signal. To test this possibility, it is necessary to isolate the effect of the rank signal.

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 contains five robustness tests. Section 4.5 explores whether learning is efficient, and Section 4.6 examines which types of founders are more responsive.

4.1. Estimation strategy

The effect of feedback can be measured by comparing competitions where ventures receive feedback – they learn their rank relative to other participating ventures – with competitions where ventures learn only that they won or lost. This feedback is relative: ventures learn their 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 empirical design is a difference-in-differences model among non-winners, which comprise 75 percent of the data. The first difference is between above- and below-median non-winners in a given competition ($Low\ Rank_{i,j}$). The second difference is across feedback and no-feedback competitions ($Feedback_j$).

$$Y_i^{Post} = \alpha + \beta_1 Low\ Rank_{i,j} \cdot Feedback_j + \beta_2 Low\ Rank_{i,j} \qquad (2)$$
$$+ \beta_3 Feedback_j + \gamma_t + \delta' \mathbf{X}_i + \varepsilon_{i,j,t} \text{ if } i \in Losers_j$$

In Equation 2, i indexes ventures, and j indexes competition rounds. The dependent variable is continuation, measured as having at least one employee besides the founder as of August 2016. Year fixed effects, γ_t, address censoring issues with the survival outcome. The controls are sector dummies, whether the founder is a student at the time of the competition, and whether the venture is incorporated at the time of the competition. Some models also include company age and whether the company received investment before the round. When a venture participated in multiple competitions, only the first instance is included.

4.2. Identification challenge

In Equation 2, above-median non-winners comprise the control group. Therefore, average differences across the types of competitions are differenced out. The concern is that the distribution of non-winners around the median may be systematically different in the two types of competitions, even though applicants did not know whether the competition would inform them of their ranks in the round. More formally, the concern is that the mapping from quality to rank is systematically different.

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.

Three tests and five robustness exercises address this concern. The three tests are: (1) Test for ex-ante differences in the distributions of observables across the two types of competitions; (2) Test whether rank reflects measures of ex-ante quality equally in both types of competitions; (3) Exploit ventures in multiple competitions to test for selection into feedback. The first part of the Appendix describes these three tests in detail. The summary is that across the two types of competitions, the distributions are not meaningfully different, rank reflects observable quality at the time of the competition equally, and there is no evidence of selection into feedback. The five robustness exercises are described in Section 4.4.

4.3. Main effect of feedback

The raw effect of feedback is in Figure 4, which shows demeaned survival on the y-axis and decile rank on the x-axis. Rank is more predictive of continuation in the feedback competitions, though it is also predictive in the no-feedback competitions, as shown in the RD analysis in Section 3.5. This is important, as it demonstrates that ranks are inherently informative about outcomes.

Equation 2 is estimated in Table 6. The main specification in Panel 1 column 1 finds that negative feedback reduces the likelihood of continuation by 8.6 pp, relative to a mean of 34 percent, significant at the .05 level.[19] This 14 percent increase in the probability of failure is economically large, especially given the subtle, low stakes nature of the feedback. Remarkably, the effect is almost exactly the same when judge fixed effects are included (column 2). Column 3 adds additional venture controls. The effect is symmetrical among

[19] The coefficient on $Low\ Rank \cdot Feedback$ (-.086) is relative to above median non-winners in no-feedback competitions. The coefficient on $Low\ Rank$ is -.062, implying that in no-feedback competitions low-ranked non-winners are 6.2 pp less likely to continue than high ranked non-winners. The coefficient on feedback is 0.066, as there is a higher probability of survival in feedback competitions. Summing the three coefficients gives a total average effect of $Low\ Rank \cdot Feedback$ of -8.4 pp.

round winners that did not win the overall competition (column 4). When a round winner has an above-median rank, it is associated with a 11 pp increase in the probability of survival, relative to when a round winner has a below-median rank. Positive feedback induces continuation, just as negative feedback induces abandonment.

Abandonment in response to negative feedback is quick. When the dependent variable is an indicator for abandoning within six months, the effect is 7.9 pp, relative to a mean of 51 percent (Table 6 panel 1 column 5). The effect increases to 8.7 and 8.9 pp within one and two years, respectively, relative to means of 57 and 64 percent (columns 6 and 7). The overall effect in column 1 therefore occurs within the first two years.

The effect is roughly linear, but somewhat larger at the higher end of the non-winner distribution, suggesting that feedback induces near-winners to persevere as much as or more than it encourages the poorest performers to exit. In Table 6 panel 2 column 1, "Low rank" is one if the venture is in the bottom three deciles among non-winners, and zero if in the top seven deciles. In column 2, "Low rank" is one for the bottom seven deciles. In column 3, "Low rank" is one for deciles 5-8, and the bottom two deciles are omitted. The effect is not driven by the bottom deciles and is strongest when "Low rank" is one for the bottom seven deciles (column 2).

It is possible that the effect on survival operates through financing. Highly ranked non-winners with feedback may be better able to raise financing than their uninformed counterparts. However, in unreported tests negative feedback has no effect on subsequent external financing. In sum, entrepreneurs participating in new venture competitions who receive negative feedback about their ventures are more likely to abandon them.

4.4. Robustness tests

4.4.1. Exploiting nominal scores

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. They can be exploited to better approximate the random allocation of feedback. To illustrate the approach, consider a pair of ventures 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 relative to its true quality.

If scores measure latent quality, then residual variation in rank reflects noise in transforming nominal scores to forced ranks. Table 6 panel 2 column 4 confirms that score strongly predicts survival. Column 5 replicates the main specification with a control for score. The effect of $Low\ rank \cdot Feedback$ strengthens somewhat, to 9.3 pp. The model of interest is in column 6, where the sample is restricted to feedback competitions, and the effect of rank is estimated after controlling for nominal score. It finds that increasing a venture's rank by one decile reduces the probability of abandonment by 1.4 pp. This is strong evidence that ex-ante quality distributional differences do not explain the main result.

4.4.2. Matching estimators

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

The first method is exact matching, which is preferable as there is no conditional bias in the estimated treatment effect (Abadie and Imbens 2006). The samples of above- and below-median non-winners 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 A8; the match dramatically reduces the differences. The exact matching result is in Appendix Table A9 column 1, 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. It then identifies, for each treated participant, the untreated participant with the closest probability of treatment.[20] Appendix Table A10 shows that the matching brings the samples almost entirely in line. The propensity-score matching estimate is in Appendix Table A9 column 2. The effect falls slightly, to 5.6 pp, significant at the .05 level.

4.4.3. Interacting feedback with competition and ex-ante quality characteristics

There is a risk 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 and competition characteristics likely associated with signal quality, venture survival, and participant diversity. Regressions that include interactions between feedback and proxies for the quality of the signal that the competition produces are in Appendix

[20] I try to eliminate bias in several ways. First, I match without replacement, so that once an untreated participant is matched, it cannot be considered as a match for subsequent treated participants. Since each subject appears no more than once, variance estimation is uncomplicated by duplicates. Second, I match only on binary covariates; I use the covariates from the exact match plus several others, such as prior external financing. Abadie and Imbens 2006 note that the matching estimator's bias increases in the number of continuous covariates used to match. Third, I omit matches without common support, which reduces the matched sample by 408 ventures.

Table A11 panel 1.[21] Interactions with competition-level characteristics associated with venture survival are in panel 2.[22] Interactions with competition diversity are in panel 3.[23] In all cases, the effect of $Low\ Rank \cdot Feedback$ persists, and even grows somewhat larger (about 9 pp). A similar exercise at the venture level is in Table A11 panel 4.[24] After controlling for venture characteristics likely associated with ex-ante quality and their interaction with feedback, the independent effect of feedback persists. Distributional differences, therefore, do not drive the effect.

4.4.4. 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 non-winners 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 A12. 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. This is quite similar to the main specification.

[21] Competition signal quality proxies are whether the competition is at a university, the number of ventures, the number of judges, and the location. Indicators for the nine U.S. Census divisions are used for location.

[22] Characteristics associated with venture survival are the share of founders with a BA from a top 10 college, the share of incorporated ventures, and the share of ventures that previously received external financing.

[23] Competition diversity might affect the slope in rank. Proxies for diversity are the number of venture sectors (out of a total possible 16 sectors), the share of ventures that are software-based, and the share of ventures that are clean energy based.

[24] Venture characteristics likely associated with ex-ante quality are whether the venture was incorporated at the time of the round, whether it had previous external financing, whether the founder graduated from a top 10 college, whether the founder has a PhD from a top 20 university, and whether the founder is a student at the time of the competition.

4.4.5. Subsamples and functional form

The last set of robustness tests consider various subsamples and functional form. First, Appendix Table A9 column 3 restricts the sample to preliminary rounds and finds a larger effect of negative feedback, at 12 pp significant at the .01 level. To ensure that higher average venture maturity in feedback competitions does not somehow explain the effect, column 4 restricts the sample to unincorporated ventures, and finds an effect of 12 pp.[25] Further subsamples are in Appendix Table A13. The effect persists within the population of founders with MBAs, among ventures from VC hub states, and among student-led ventures. Finally, Appendix Table A9 column 5 shows that the effect is robust to using a logit specification, and column 6 shows that it is robust to controlling for the first and second moment in z-score.

4.5. Is learning efficient?

Private, costless, informative signals at an early stage might enable poor quality startups to fail faster, making innovation more efficient. The main result implies that had the 1,603 unique below-median non-winners in the no-feedback competitions received feedback, an additional 137 would have been abandoned, beyond the 1,186 that were abandoned. The data do not permit a welfare assessment of feedback, nor is it apparent whether the prior beliefs of the entrepreneurs about their likelihood of success were biased or unbiased. However, it is possible to examine three ways that learning might not be efficient.

First, inducing abandonment could be socially costly if a few highly successful outcomes are foregone. 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. Thus,

[25] 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.

if there is a cost in right-tail outcomes, it seems small.

Second, learning may be privately inefficient if abandoning after negative feedback leads to poorer long run labor market performance. In the absence of earnings data, it is useful to examine a proxy for attaining a leadership role. Founders have a revealed taste for leadership, so leadership in other domains is a reasonable proxy for non-entrepreneurial success.[26] Unreported regressions find no evidence that receiving any feedback or negative feedback is related to subsequent non-entrepreneurial leadership among founders that abandoned their ventures. Feedback does not seem to cost abandoners ultimate leadership positions.

Third, even if learning is on average efficient, there may be many cases in which ventures are randomly assigned especially lenient or harsh judges, leading to inaccurate signals. A test for such "noisy" learning is based on the leave-one-out judge leniency measure in Dobbie and Song (2015). Let S_{ik} be an indicator for the highest score a venture received across judges. Let k denote a judge and n_k the count of ventures that judge k scored. The leave-one-out leniency measure for a venture-judge pair is $L_{ik} = \frac{1}{n_k - 1}\left(\sum_{k=1}^{n_k} S_k - S_i\right)$. For a venture i, this is the number of times one of its judges gave a high score to other ventures, divided by the number of other ventures the judge scored. L_{ik} is summarized in Appendix Table A3 panel 3. The results are in Appendix Table A14. Leniency predicts scores (columns 1-2), but there is no effect of leniency on responsiveness (column 5). Lenient judges do not influence a venture's overall rank enough to affect the abandonment decision.

In sum, there is no evidence of large private or social costs to feedback, suggesting that it is weakly more efficient. However, this will not be true if encouraging more entrepreneurial entry is always socially beneficial, regardless of startup quality.

[26]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.

4.6. Who learns?

To explore which entrepreneurs learn and under what circumstances, it is possible to add an interaction for a cross-sectional characteristic. A nice feature of this heterogeneity analysis is that it permits including competition fixed effects, which address any remaining concerns about systematic differences across competitions. The results are in Table 7. There are just two venture or founder characteristics that exhibit significant heterogeneity. First, ventures with prior external financing are 15 pp more likely to continue after receiving especially negative feedback than those without prior financing (column 1). Second, founders with top college degrees are less responsive (column 2).

Founders are more responsive when there are more judges (they can observe the number of judges). This is shown with the linear number of judges in Table 7 column 3, and an indicator for above median judges in column 4. In the strongest heterogeneity result, the effect of negative feedback on continuation is 29 pp greater with above median judges.

Disagreement among judges is one proxy for venture risk. Founders are less responsive when the standard deviation of judge ranks within a competition-round-panel is higher, using both linear standard deviation and an indicator for above median standard deviation (columns 5-6).[27] However, this could reflect signal precision if founders learn that judges lacked consensus from verbal interactions. When the standard deviation is instrumented for using the judge leniency measure described in Section 4.5, there is no effect.[28] This

[27]Recall that founders do not observe individual judge ranks, but they do know how many judges there are. When there are more judges, the standard deviation is measured with greater accuracy, but it does not get smaller in expectation.

[28]When a venture is assigned an especially lenient and an especially harsh judge, the standard deviation of judge ranks should be higher independently of the venture's risk. Consider two measures: $V_{i,\sigma}^{high}$ is the standard deviation of the lenience measure L_{ik}, and $V_{i,\sigma}^{ext}$ is the standard deviation of L_{ik} among only the four most extreme judges that scored a venture (the most lenient, least lenient, harshest, and least harsh). These measures are summarized in Appendix Table A3 panel 3. When variation in leniency is high, the venture randomly receives a particularly noisy signal. Appendix Table A15 shows that variation in leniency predicts the standard deviation of judge scores quite well. The F-statistics in first-stage regressions range from 14 to 31. In a naive instrumentation approach, the standard deviation is replaced with the leave-one-out variation measures. Columns 5-6 show no effect of the triple interaction between having a low rank, receiving feedback,

indicates that the effect of standard deviation likely reflects venture risk.

These heterogeneity results are consistent with a variety of interpretations. Two stand out as particularly consistent with the data. First, if founders treat their ventures as real options, they should be less responsive – delaying abandonment despite negative feedback – when the venture is more uncertain and has more asset specificity, or irreversibility of investment (Dixit and Pindyck, 1994, Manso 2016). Indeed, riskier ventures are less responsive, as are ventures with prior external financing (the latter likely have higher sunk costs and thus greater investment irreversibility).[29] Venture resemblance to a call option should also increase with the personal wealth of the founder. More personal wealth makes it less costly to continue with the venture and also reduces downside risk in the event the venture ultimately fails, as in Vereshchagina and Hopenhayn (2009). As mentioned above, founders with top college degrees are likely richer (Chetty et al. 2017).

Bayes' rule dictates how rational agents update their beliefs.[30] Three cross-sectional findings are consistent with Bayesian updating, though other models are not excluded. First, founders are more responsive when the signal is more precise, proxied with the number of judges. Second, feedback should matter less when the prior is more precise. Consistent with this, ventures that have received external financing are less responsive. Third, non-linearity in the effect could be consistent with cognitive biases, because rank predicts success in a linear way. Excessively elevated or precise priors should prevent founders from updating downward enough when they receive a middling rank among non-winners. Instead, the effect is roughly linear, and persists among winners (see Section 4.3). In sum, founders behave like Bayesians, though again other models are not ruled out.

A simple model of how a Bayesian updater responds to feedback is in Appendix Section

and having judges with high expected variation in leniency.

[29]These characteristics could also be associated with more private information, but older ventures and non-student founders are not more or less responsive than their counterparts. These groups may have more information, but have not necessarily generated more specific assets.

[30]Given a prior belief and a new signal, the posterior belief of the Bayesian updater is a precision-weighted average of the two.

2. It assumes that the founder interprets his rank as the result of a series of Bernouilli trials, where the number of signals is the number of judges. This allows the Beta distribution as the conjugate prior. Hewing closely to the information structure and main results from the preceding sections, the model is calibrated to show how feedback affects a founder's success probability distribution. Appendix Figure A4 shows the results. The interim prior is in Appendix Figure A4A. The posteriors after negative feedback (below-median non-winner) and positive feedback (above-median non-winner) are in Appendix Figure A4B and A4C. It is possible to interpret the heterogeneity results through the Bayesian calibration. As an example, Appendix Figure A5 depicts how having an above-median number judges affects the posterior by improving signal precision.

A more speculative interpretation of the finding that risky ventures and those with elite degree founders are less responsive to negative feedback is that it sheds light on the mechanism of radical innovation. Even as most entrants are rational and responsive to new information, a small subset may have ambitious, radical ideas and also may be imperviousness to negative feedback. Ventures in this subset may be the ones with the potential to transform industries, and the overconfidence of their founders may be crucial to coordinating other stakeholders. Theoretical models of industry dynamics could micro-found technological discontinuities in the small fraction of entrepreneurs that enter without regard to signals about expected cash flows. A promising avenue for future research is whether the most innovative, risky new firms tend to have founders who ignore negative feedback.

5. Conclusion

This paper shows that new venture competitions are useful to startups and explores various mechanisms for their effects. Winning and cash prizes are useful, but competitions also appear to be valuable because they facilitate entrepreneur learning in the sense of type revelation. In Manso (2011)'s optimal contract, feedback should be timely and tolerant of failure. New venture competitions with feedback implement this guidance: While they reward top performers, they do not penalize especially poor performance. 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.

The large effect of subtle, low-stakes feedback shows that entrepreneurs can learn about their types. In addition to providing one channel for competitions to be useful, this finding rejects the hypothesis that entrepreneurs are characterized by such extreme overconfidence 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.[31] 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.

In Odean (1999) and Hanna, Mullainathan and Schwartzstein (2014), people do not learn because of noisy or multi-dimensional signals. On the other hand, recent work outside of firm settings has found that individuals can learn about their ability through performance (Seru, Shumway and Stoffman 2010, Hochberg, Ljungqvist and Vissing-Jørgensen 2013). Whether entrepreneurs learn better from certain types of signals is a promising avenue for

[31] 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).

future research.

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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 $\geq$ 2 employees as of 8/2016)	4328	0.34	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 $\geq$ 3 employees as of 8/2016	4328	0.3	0	0.46	0	1
Has $\geq$ 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	0	0.42	0	1
Male	3,228	0.73	1	0.44	0	1
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	0	0.4	0	1
Graduated from top 20 college	2554	0.27	0	0.44	0	1
Graduated from top 10 college	2554	0.18	0	0.39	0	1
Graduated from Harvard, Stanford, MIT	2554	0.1	0	0.3	0	1
Has MBA	2554	0.48	0	0.5	0	1
Has MBA from top 10 business school	2554	0.33	0	0.47	0	1
Has Master's degree	2554	0.17	0	0.37	0	1
Has PhD	2554	0.13	0	0.34	0	1
Previous founder (founded different company before competition)	2554	.02	0	0.13	0	1
Founder or CEO of subsequent venture after round, if abandoned venture	1190	0.39	0	0.49	0	1

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)	(6)	(7)
Won Round	.11***	.088***	.13***	.17***	.079**	.71***	.17***
	(.018)	(.018)	(.026)	(.033)	(.036)	(.14)	(.015)
Decile rank	-.02***	-.02***					
	(.0027)	(.0027)					
Decile rank winners			-.011***		-.0059	-.069***	
			(.0044)		(.0054)	(.021)	
Decile rank non-winners			-.018***		-.013***	-.13***	
			(.0025)		(.0031)	(.017)	
Within-judge decile rank							-.006***
							(.0011)
Prize (10,000$)		.0089***	.0085***	.0055	.0085***	.036***	.011***
		(.0023)	(.0024)	(.0057)	(.0029)	(.011)	(.0034)
Venture controls	N	N	N	N	Y	N	N
Comp.-round-panel f.e.	Y	Y	Y	Y	Y	Y	N
Judge f.e.	N	N	N	N	N	N	Y
Year f.e.	N	N	N	N	N	N	Y
N	6023	6023	6023	1705	3487	5484	26663
R^2	.16	.16	.16	.33	.4	.12	.4

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. OLS used except column 5, which uses a logit model. 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 non-winners" are, respectively, the decile rank within the round's winners and non-winners. A smaller rank is better (one is best decile, 10 is worst decile). To assess the effect of winning near the cutoff, in column 4 only the bottom quintile among winners (quintile five) and the top quintile among losers (quintile one) are included. 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. Errors clustered by competition-round-panel or judge, depending on fixed effects. *** indicates p-value<.01.

Panel 2

Dependent variable: Financing after round

Sample:	Prelims	Prelims, no prize	Finals	No judge inv.	First comp.	No feedback	Z-scores
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Won Round	.14***	.15***	.089*	.13***	.13***	.13***	.15***
	(.03)	(.04)	(.05)	(.026)	(.027)	(.034)	(.019)
Decile rank winners	-.015***	-.016**	.0031	-.012***	-.012**	-.0091	
	(.0052)	(.0066)	(.0066)	(.0043)	(.0047)	(.0061)	
Decile rank non-winners	-.018***	-.017***	-.021***	-.018***	-.017***	-.011***	
	(.0032)	(.0036)	(.0044)	(.0025)	(.0026)	(.0033)	
Z-score winners							.0074
							(.024)
Z-score non-winners							.031***
							(.011)
Prize (10,000$)	.012***		.0053	.0088***	.0067*	.011**	.012**
	(.0032)		(.0034)	(.0023)	(.0039)	(.0055)	(.0055)
Comp.-round-panel f.e.	Y	Y	Y	Y	Y	Y	Y
N	4394	3404	1605	5998	4920	3422	3973
R^2	.16	.12	.17	.16	.17	.2	.19

Note: This panel shows OLS regression estimates of the effect of winning, rank, and cash prize on whether the venture raised external financing after the competition. 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). In columns 1-2, the sample is restricted to preliminary rounds, and further to ventures that won no prize in column 2. In column 3, the sample is restricted to final rounds. Column 4 omits ventures in which a judge or judge's firm invested. Column 5 restricts the sample to ventures participating in their first competition. Column 6 restricts the sample to competitions without feedback. Column 7 uses z-scores, based on nominal scores, rather than percentile ranks. Competition fixed effects control for the date. Errors clustered by competition-round-panel. *** indicates p-value<.01.

Table 3: Effect of Rank and Winning on Additional Outcomes

Sample:	All			Prelim rounds only, no prize		
Dependent variable:	Survival	10+ employees	Acquired /IPO	Survival	10+ employees	Acquired /IPO
	(1)	(2)	(3)	(4)	(5)	(6)
Won Round	.047*	.051*	.018	.087**	.1**	.037**
	(.028)	(.027)	(.012)	(.041)	(.041)	(.016)
Decile rank winners	-.006	-.0041	-.0028*	-.013**	-.013**	-.0039*
	(.0043)	(.0044)	(.0017)	(.0065)	(.0063)	(.0023)
Decile rank non-winners	-.023***	-.017***	-.0011	-.025***	-.017***	-.00022
	(.0028)	(.0023)	(.001)	(.0032)	(.0028)	(.0013)
Prize (10,000$)	.0062*	.0074***	.0002			
	(.0032)	(.0026)	(.0013)			
Comp.-round- panel f.e.	Y	Y	Y	Y	Y	Y
N	6023	6023	6023	3404	3404	3404
R^2	.17	.14	.083	.15	.12	.075

Note: This table contains OLS regression estimates of the effect of winning, rank, and cash prize on proxies for survival and growth. Survival is one if the venture had ≥ 1 employee besides the founder on LinkedIn as of 8/2016. 10+ employees is defined analogously. 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: Heterogeneity in Effect of Rank and Winning on External Finance

Dependent variable: Financing after round

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

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. 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 5: Effect of Dimension Rank on Venture Outcomes

Dependent variable:	Financing after round		10+ Employees		Acquired/IPO	
	(1)	(2)	(3)	(4)	(5)	(6)
Percentile 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 dimension-specific ranks on indicators for venture outcomes. 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 dimension scores are averaged to produce the overall ranks used in other tables. Percentile rank in round is either the decile rank in round or the quintile rank within judge, depending on fixed effects. A smaller percentile 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 6: Effect of Negative Feedback on Venture Continuation

Panel 1

Dependent variable:	Survival				Abandoned within...		
				Positive feedback	6 months	1 year	2 years
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Low rank·Feedback	-.086**	-.084***	-.079***		.079*	.085**	.087**
	(.036)	(.02)	(.026)		(.041)	(.041)	(.039)
Low rank	-.062***	-.051***	-.026		.056***	.06***	.058***
	(.021)	(.014)	(.022)		(.021)	(.022)	(.022)
Feedback	.066*	.17*	-.03	-.032	-.0074	-.031	-.056
	(.04)	(.092)	(.14)	(.068)	(.042)	(.042)	(.04)
High rank·Feedback				.11*			
				(.06)			
High rank				.029			
				(.046)			
Venture controls	Y	Y	Y‡	Y	Y	Y	Y
Year f.e.	Y	N	N	Y	Y	Y	Y
Judge f.e.	N	Y	Y	N	N	N	N
N	3751	26443	14915	1335	3751	3751	3751
R^2	.082	.18	.29	.14	.061	.06	.073

Note: This panel shows estimates of the effect of negative feedback within the sample of non-winners (having a below-median rank among non-winners when participating ventures learn their ranks, relative to competitions where they do not learn their ranks). "Low rank" is one if the venture's rank is below median among non-winners. Sample restricted to non-winners of round, except in column 4. The dependent variable in columns 1-4 is survival, which is one if the venture had ≥ 1 employee besides the founder on LinkedIn as of 8/2016. Venture controls include sector indicator variables, student and company incorporation status. ‡In column 3, venture controls also include company age and whether the company received investment before the round. Column 4 restricts the sample to winners and tests the effect of positive feedback (above median rank). The dependent variables in columns 5-7 are based on time to abandon, which uses the founder's next job start date conditional on abandonment. For example, in column 5, the dependent is one if the venture is abandoned and the founder has a new job within six months. Errors clustered by competition-round-panel or judge, depending on fixed effects. *** indicates p-value<.01.

Panel 2

Dependent variable: Survival

	Low rank among non-winners defined as:			Nominal score		
	Bottom 3 deciles	Bottom 7 deciles	Deciles 5-8 (9-10 omitted)			Feedback only
	(1)	(2)	(3)	(4)	(5)	(6)
Low rank·Feedback	-.062**	-.097**	-.079*		-.093**	
	(.029)	(.04)	(.046)		(.04)	
Low rank	-.065***	-.048**	-.025		-.047*	
	(.019)	(.022)	(.025)		(.026)	
Feedback	.032	.073*	.075*		.082	
	(.028)	(.043)	(.043)		(.05)	
Nominal score				.0052**	.0027	.073***
				(.0024)	(.0022)	(.02)
Decile rank						-.014*
						(.0073)
Venture controls	Y	Y	Y	Y	Y	Y
Year f.e.	Y	Y	Y	Y	Y	Y
N	3751	3751	2372	3305	2974	2028
R^2	.081	.081	.097	.071	.086	.085

Note: This panel shows estimates of the effect of negative feedback within the sample of non-winners. Survival is 1 if the venture had ≥ 1 employee besides the founder on LinkedIn as of 8/2016. Columns 1-3 define "Low rank" as one if the venture's rank is as noted in the column header, and zero otherwise. For example, in column 1, "Low rank" is one if the rank is in the bottom three deciles among non-winners. Columns 4-6 use nominal score, which is ordered by the competitions to produce the ordinal ranks. This is not available for all competitions. Column 6 identifies the effect of feedback as the coefficient on rank after controlling for nominal score. Venture controls include sector indicator variables, student and company incorporation status. Errors clustered by competition-round-panel. *** indicates p-value<.01.

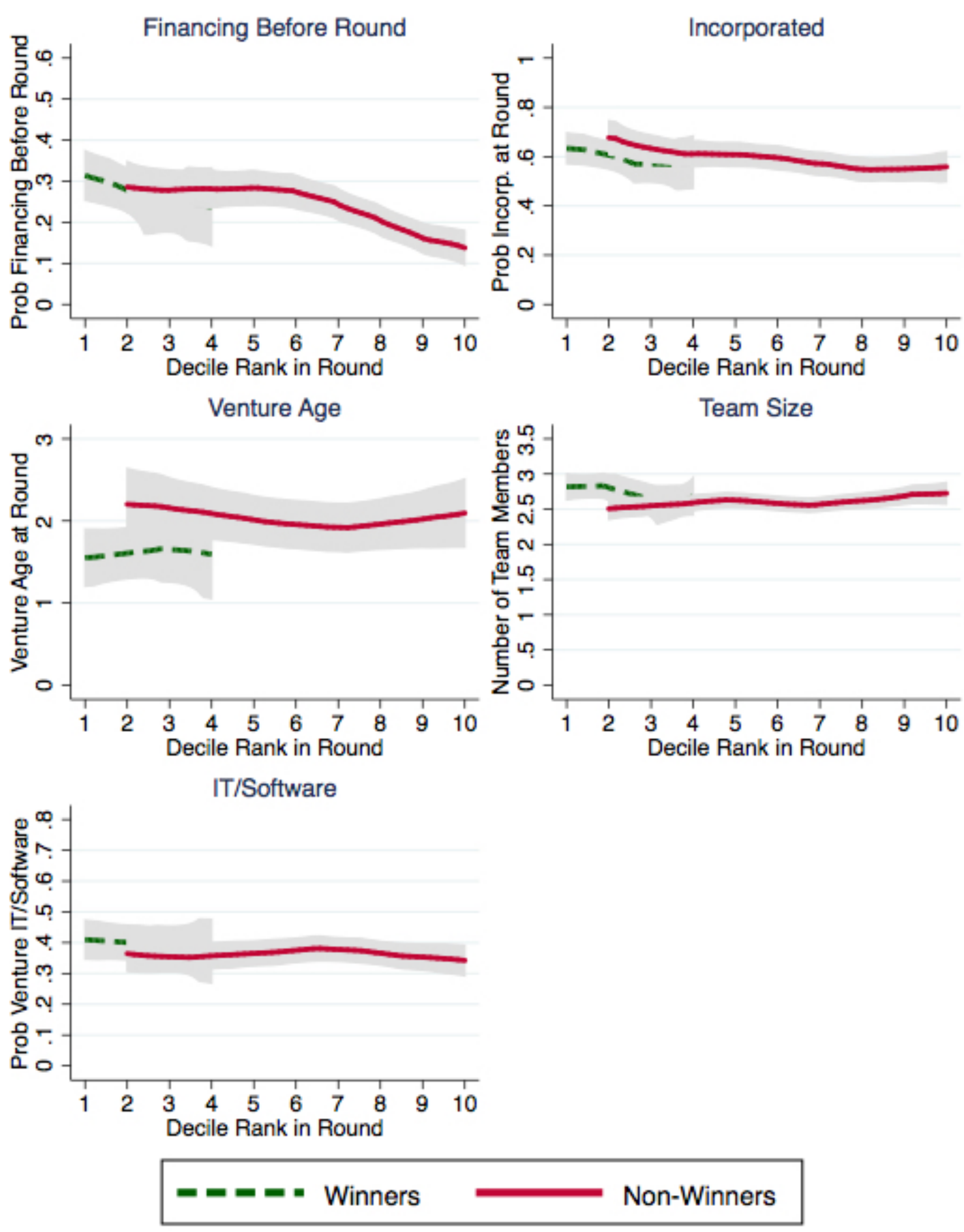
Table 7: Heterogeneity in Effect of Negative Feedback

Dependent variable: Survival

Characteristic C_i:	Financing before round	Founder Harvard/MIT /Stanford	# judges	# judges > median	Judge rank s.d.	Judge rank s.d. > median
	(1)	(2)	(3)	(4)	(5)	(6)
Low rank·Feedback· C_i	.15*	.5*	-.0038**	-.28*	.11**	.18*
	(.088)	(.28)	(.0016)	(.17)	(.046)	(.1)
Low rank·Feedback	-.1**	-.063*	.056	.22	-.15***	-.08*
	(.042)	(.037)	(.064)	(.16)	(.052)	(.043)
Feedback· C_i	-.19***	-.22	-.0021	-.15	-.066*	-.065
	(.067)	(.25)	(.0018)	(.11)	(.033)	(.092)
Low rank· C_i	-.051	-.076	.00059	.024	.011	-.034
	(.069)	(.066)	(.00075)	(.044)	(.0073)	(.068)
Low rank	-.031	-.046**	-.068**	-.074*	-.059*	-.036
	(.02)	(.023)	(.027)	(.038)	(.033)	(.033)
C_i	.39***	.056	.00082	.032	-.0086	-.036
	(.053)	(.06)	(.00092)	(.048)	(.0067)	(.076)
Controls	Y	Y	Y	Y	Y	Y
Year f.e.	Y	Y	Y	Y	Y	Y
Sector f.e.	Y	Y	Y	Y	Y	Y
Competition f.e.	Y	Y	Y	Y	Y	Y
N	3751	3751	3751	3751	3751	3751
R^2	.13	.14	.14	.14	.15	.15

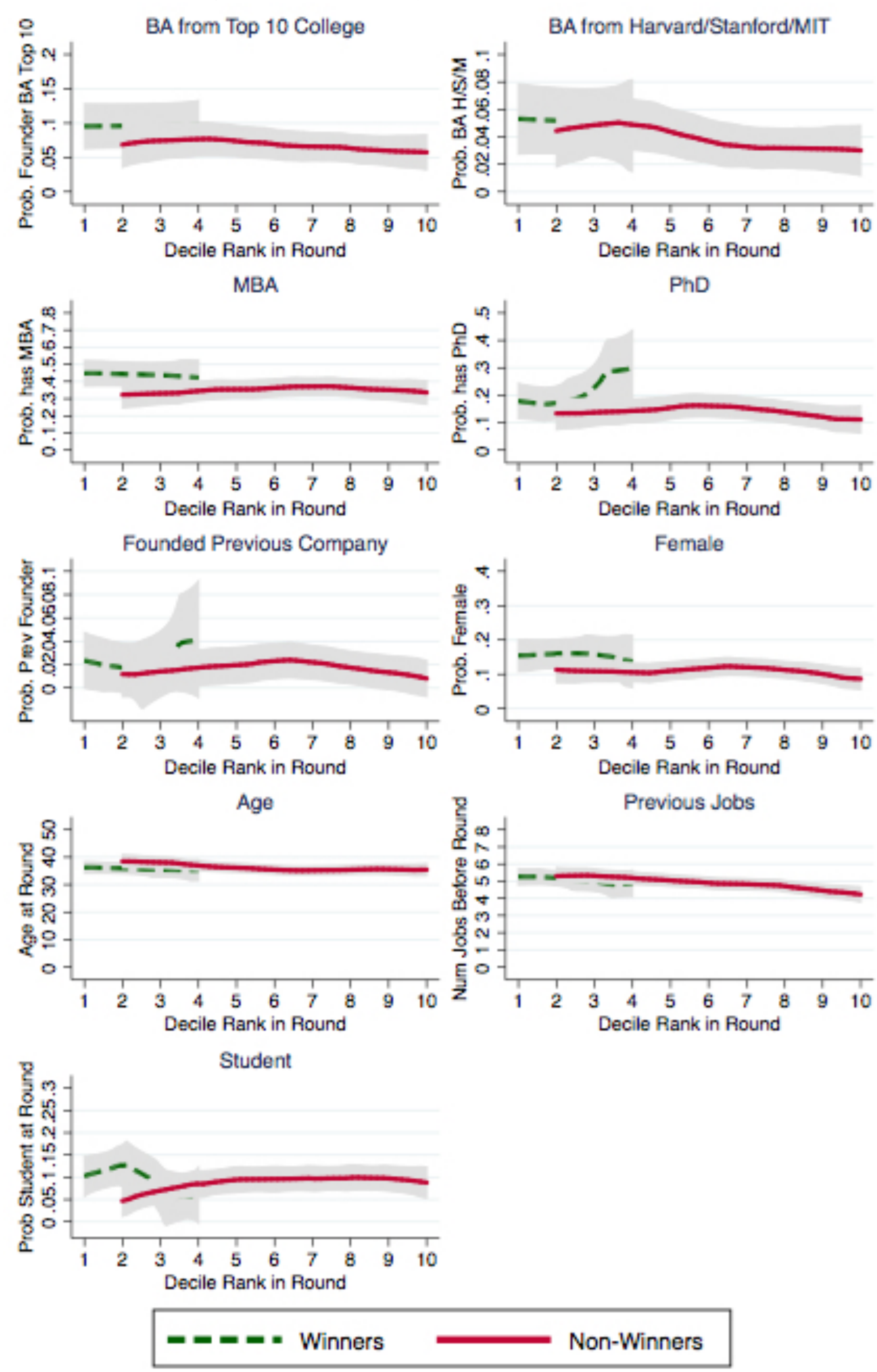
Note: This table shows estimates of how the effect of negative feedback on venture survival varies by characteristics C_i. For example, in column 6 C_i is 1 if the standard deviation of judge ranks for the venture is above median, among ventures in round. Survival is one if the venture had at least one employee besides the founder on LinkedIn as of 8/2016. Control coefficients not reported for brevity. Competition fixed effects control for the date. Errors clustered by competition-round-panel. *** indicates p-value<.01.

Figure 1: Continuity of Venture Covariates



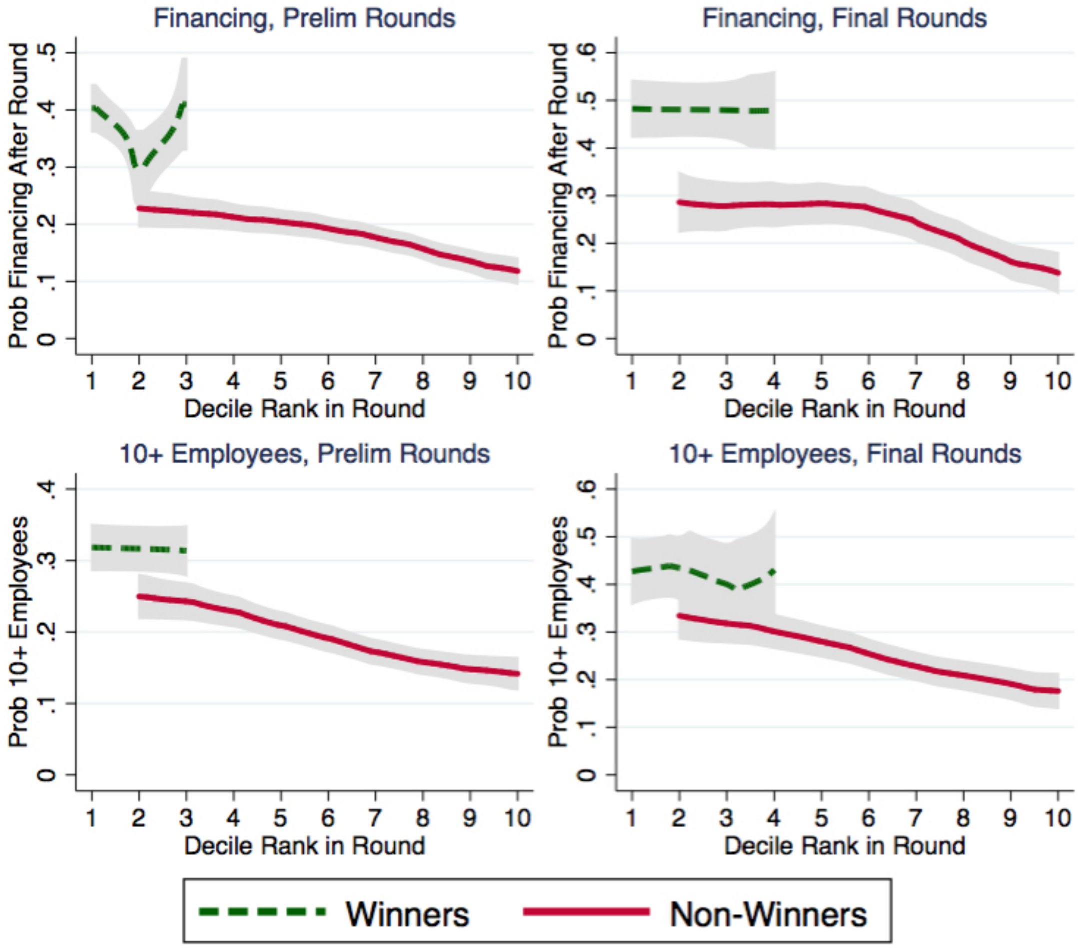
Note: This figure shows probabilities of venture-specific covariates observed at the time of the competition by percentile rank in the round (lower percentile rank is better). Final rounds are used. There are no losers in the top bin, and winners are truncated at the fifth decile. The lines overlap because 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



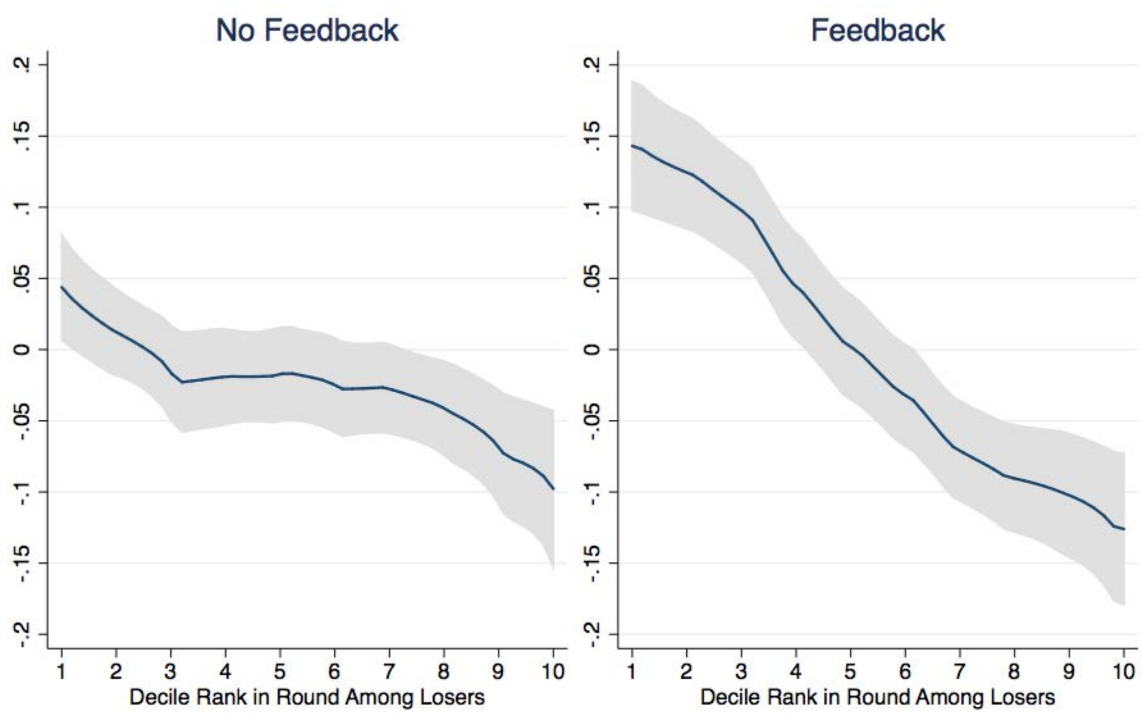
Note: This figure shows probabilities of founder-specific covariates observed at the time of the competition by percentile rank in the round (lower percentile rank is better). Final rounds are used. There are no losers in the top bin, and winners are truncated at the fifth decile. The lines overlap because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

Figure 3: Effect of winning



Note: This figure shows probabilities of any subsequent financing (top) 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: Survival probability by decile rank among non-winners



Note: This figure shows the demeaned probability of survival among non-winners in preliminary rounds, by percentile rank in the round. Local polynomial with Epanechnikov kernel. 95% CIs shown.