Learning from Feedback:

Evidence from New Ventures

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Abstract

This paper studies how entrepreneurs learn about the quality of their new venture (learning in the sense of type revelation). I assess the effect of negative feedback on early stage startups using application and judging data from 96 new venture competitions, some of which privately inform ventures of their relative rank. I use a difference-in-differences design and two matching estimators to compare lower and higher ranked losers, across competitions in which they did and did not observe their standing. Receiving negative feedback increases venture abandonment by about 12 percent. Cross-sectional variation is consistent with Bayesian updating, and with founders treating venture continuation as a real option.

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1 Introduction

Models of firm dynamics and occupational choice often rely on learning assumptions (e.g. Jovanovic 1982, Hopenhayn 1992, Ericson & Pakes 1995, Aghion & Howitt 2006). In these and other models, entrepreneurs enter an industry and incumbent managers exit in response to new information about the net present value of the enterprise.[1]

While learning in the sense of improvement is fairly straightforward, learning in the sense of type revelation is not, particularly in the context of high-growth entrepreneurship. Empirical evidence points to entrepreneurs suffering from cognitive biases, particularly overconfidence, that lead them to enter irrationally and fail to update their priors in light of new information.[2] This behavioral view of entrepreneurship is motivated in part by evidence of low returns, even among founders of venture capital-backed startups (Moskowitz & Vissing-Jørgensen 2002).[3] There is also much anecdotal evidence. For example, according to one venture capitalist,

> "Genetic or not, there are certain classic characteristics of the entrepreneur. The most important of these are certain kind a visionary optimism; tremendous confidence in oneself that can inspire confidence in others" (Bussgang 2011).

In turn, theory has incorporated the behavioral view. Consider three examples. Bernardo & Welch (2001) model entrepreneurs as "overconfident individuals who act on their own information and who irrationally ignore the actions of other individuals." In Landier & Thesmar (2009), entrepreneurs ignore negative

[1] Also see Lucas (1978), Jovanovic & Lach (1989), Aghion et al. (1991), Cagetti & De Nardi (2006), Vereshchagina & Hopenhayn (2009), and Poschke (2013).

[2] For example, Astebro, Jeffrey & Adomdza (2007) find that inventors fail to respond to negative feedback. Other evidence on overconfidence includes Cooper et al. (1988), Camerer & Lovallo (1999), Arabsheibani et al. (2000), Koellinger, Minniti & Schade (2007), and Kogan (2009). See Astebro et al. (2014) for a review.

[3] Also see Hamilton (2000), Hall & Woodward (2010), and Hurst & Pugsley (2015). Non-pecuniary benefits may also play a role (Hurst & Pugsley 2011, Hvide & Møen 2010 and Giannetti & Simonov 2009).

feedback at an interim stage, and assign the event of venture failure zero probability. In their R&D investment model, Bergemann & Hege (2005) note that "entrepreneurs express a strong preference for continuation regardless of present-value considerations."

Managerial learning is both non-obvious and important, yet it is challenging to measure. New venture competitions, in which founders present their businesses to a panel of expert judges, are well suited to the task. I use novel data on 4,328 new ventures participating in 96 competitions in 17 states between 1999 and 2015. In 61 of the competitions, ventures are informed only that they won or lost, and otherwise do not learn where they stand relative to their peers. In 35 of the competitions, ventures are privately informed of their rank in the round. Competitions are otherwise broadly similar.

For all competitions, I observe complete scoring information, including judge-specific scores that are never revealed to ventures. I link the ventures and founders to external data on financing events and career history. Founders are mostly first-time entrepreneurs, and essentially all seek external finance to grow quickly. There are no local or subsistence businesses, such as restaurants, that contaminate efforts to study high-growth entrepreneurship (Levine & Rubinstein 2016). I show that the ventures and founders in my data are roughly representative of the U.S. startup population.

I assess the causal effect of negative feedback among losers in a round with a difference-in-differences specification. The first difference is within round (e.g. semifinals), comparing below-median and above-median losers. The second difference is across rounds, comparing ventures that were informed of their rank with those that were not. That is, I estimate the effect of a very low rank with knowledge of that rank, relative to a very low rank without such knowledge. Judges themselves cannot learn from the feedback. They observe only their own scoring and winner identities.

There are two empirical concerns. First, to be credible signals, the scores must be relevant to outcomes. In a regression discontinuity design, I show that conditional on win status, both percentile rank and z-score robustly predict measures of success like subsequent external financing, employment, and acquisition or IPO. Second, the distribution of losers around the median in the two types of competitions may be systematically different. I demonstrate that this is not the case for observable characteristics. I also estimate the main effect with exact and propensity-score matching.

Low-stakes negative feedback should have little effect on founders who are extremely overconfident (either in the sense of over-precision or optimism). Alternatively, negative feedback may increase venture abandonment if founders' beliefs are sensitive to new information. I find that receiving negative feedback reduces the probability of survival by about eight percentage points, equivalent to a 12% increase in abandonment (the mean is 66%).[4]

This effect persists in the matching estimators, with polynomials in z-score, and within a single program that employed feedback in one year but not others. Using a leave-one-out judge leniency measure, I find no evidence of "inefficient learning." In sum, we may conclude that at least some entrepreneurs would benefit from more information about their venture quality. My result implies that among the 1,603 unique below-median losers in the no-feedback competitions, an additional 192 should have been abandoned, beyond the 1,186 that were in fact abandoned.

Are founders maximally responsive? While there is no benchmark for maximum updating in this context, the psychology and economics literatures find that being female is the characteristic most strongly associated with reduced overconfidence (e.g. Barber & Odean 2001, Niederle & Vesterlund 2007). Within women-led ventures, which comprise 21% of the sample, negative feedback increases abandonment by about 24% of their mean. Overconfidence likely plays a role in the 100% difference between women and men.

Rational people update via Bayes' theorem, while overconfident or optimistic people perceive their priors as too precise or too high (Ben-David, Graham

[4] I use an indicator for whether the venture had at least one employee on LinkedIn besides the founder as of August, 2016 as a proxy for venture continuation.

& Harvey 2013). Behavioral biases should thus concentrate the effect of negative feedback in the lowest ranked founders. Instead, the effect is broadly linear within losers, and persists, albeit weakly, among winners. Further cross sectional evidence is also consistent with Bayesian updating. Founders dismiss imprecise signals; for example, they are less responsive when there are fewer judges. Founders also update less when they have more information about their own type; for example, they are less responsive to feedback on criteria about which they likely have more private information. This evidence, together with high average sensitivity - even among men - hints that overconfidence is not the most salient feature of behavior.

Lower responsiveness among male founders (who dominate the sample and its successful outcomes) may also reflect varying real option values embedded in the ventures. A real option's value increases in its uncertainty and in its asset specificity, or irreversibility of investment (Dixit & Pindyck 1994).[5] A founder receiving negative feedback can delay abandonment, maintaining the venture's real option value.

One measure of uncertainty is the standard deviation of judge scores. I find that when judges are uncertain about a venture, the founder is less responsive to negative feedback. On the asset specificity side, I expect that abandonment is more costly when more investment has occurred tying firm value to the entrepreneur's human capital. Ventures that are not yet incorporated, are software- rather than hardware-based, and that have not yet received external private financing are more responsive. Commitment to sunk costs and greater private information seem unlikely to fully explain these results, as there is no effect of venture or founder age.

Thus variation in responsiveness appears consistent with a real options approach to entrepreneurship. In a model of the choice between salaried employment and entrepreneurship, Manso (2016) argues that the option to return to

[5] For example, consider a firm deciding whether to drill an oil well or wait. The value of delay increases in oil price volatility and in the firm's private, non-transferable information about the land's geology (e.g. Kellogg 2014).

wage work reconciles low returns during unsuccessful entrepreneurship spells. Kerr, Nanda & Rhodes-Kropf (2014) also suggest that creating real options through experimentation is a defining feature of entrepreneurship.[6] In a more general setting, Grenadier & Malenko (2010) combine a real options framework with Bayesian updating so that firms can learn about their own type.

Entrepreneurs with riskier technologies seem less responsive to feedback than those with incremental ideas. It may be that technological discontinuities stem from a small fraction of entrepreneurs who enter without regard to signals about future cash flows, while most startup founders rationally respond to new information. Relatedly, public firms have been shown to seek overconfident CEOs (Malmendier & Tate 2005, Hirshleifer, Low & Teoh 2012).

This paper is relevant to policy and theory. Governments employ two broad tools to encourage entrepreneurship: money, through grants and tax credits, and informational resources. I provide the first evidence, to my knowledge, that information alone can improve efficiency in type revelation. From a theoretical perspective, my results are more consistent with rational, dynamic views of entrepreneurial behavior than with purely behavioral or static views. My results fit especially well with the model in Vereshchagina & Hopenhayn (2009), where bad news leads the entrepreneur to exit to a more valuable outside option.

Despite the importance of startups to economic growth, they are challenging to study in their earliest phases (Haltiwanger, Jarmin & Miranda 2013, Guzman & Stern 2016). Further, much of the venture capital literature assumes the entrepreneur knows his type, and focuses on information asymmetry and governance (e.g. Hochberg, Ljungqvist & Lu 2007, Bernstein, Giroud & Townsend 2015, Ewens & Rhodes-Kropf 2015). I extend and depart from this literature by examining type revelation close to the moment of venture founding.

Other related literature includes the connection between executive characteristics and corporate decisions (Bertrand & Schoar 2003, Graham, Harvey & Puri 2013, Kaplan et al. 2012); peer effects in entrepreneurship (Nanda &

[6] Also see Dillon & Stanton (2016), McGrath (1999), Hayward et al. (2006), and Stern (2006).

Sørensen 2010, Lerner & Malmendier 2013, Guiso et al. 2015); predicting entrepreneurial success (Gompers et al. 2010, Scott, Shu & Lubynsky 2015, Lindquist et al. 2015); barriers to high-growth entrepreneurship (Hombert et al. 2016, Howell 2017); and the effect of feedback outside firm settings (Gross 2016, Ganglmair et al. 2016).

The paper proceeds as follows. The data is described in Section 2, and the empirical approach is proposed in Section 3. The main results are in Section 4. In Section 5, I present the regression discontinuity design. Section 6 concludes.

2 The new venture competition context

New venture competitions, sometimes called business plan or "pitch" competitions, have proliferated in the past decade and are often publicly funded.[7] In a competition, new ventures present their technologies and business models to a panel of judges. Sponsored by a range of institutions, including universities, foundations, governments, and corporations, competitions aim to provide convening, certification, education, and financing functions.

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% won a new venture competition or competitive accelerator. 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. In contrast to many data sources used to study entrepreneurship, such as the Survey of Consumer Finances, subsistence self-employment, including businesses such as restaurants and landscaping, do not appear in new venture competition data.

[7]Two examples of such public support in my data are the Arizona Innovation Challenge, which awards $3 million annually, and the the U.S. Department of Energy's National Clean Energy Business Plan Competition, with $2.5 million in allocated funding.

Data description

This paper uses data from 96 competitions between 1999 and 2016. Competitions consist of rounds (e.g. semifinals), and sometimes panels within round. The number of ventures in a preliminary (final) round averages 44 (18). The mean award amount is $66,000. The data are summarized in Table 1, and the individual competitions are listed in Appendix Table A1.

All the competitions have the following features[8]: 1) They include a pitch event, where the company presents its business plan; 2) Volunteer judges formally score or rank participants, and these scores determine which ventures win; 3) Specific participants are publicly announced as winners, but no loser ranks are made public; 4) The sponsoring organization does not take equity in the participating or winning ventures.[9]5) The sponsoring organization explicitly seeks to enable winners to access subsequent external finance.

Competitions were selected to systematically provide different amounts of feedback in systematic ways. Thirty-five of the programs provide feedback through software from Valid Evaluation, a private company. These competitions inform ventures of their overall and dimension ranking relative to other ventures in their round. In the remaining 65 no-feedback competitions, ventures learn only that they won or lost. While there is some informal, verbal feedback via Q&A and social events, any information that founders receive in the no-feedback competitions is much noisier and more disconnected from peer performance.

I observe complete scoring data in all cases. Thus a key empirical advantage is that the econometrician to observe more than the agents under study. In

[8]The data were obtained individually from program administrators and from Valid Evaluation. In most cases, the author signed an NDA committing not to share or publish venture/judge/founder identifying information.

[9]Some accelerators take a small equity stake in their companies, including some of the most well-known programs, like Y-Combinator and Techstars. These programs have become an additional source of seed investment, and the networking and mentorship resources they provide are not unlike those traditionally provided by conventional investors. While interesting, these programs are not the focus of this study. They should instead be evaluated alongside their counterpart investors, angel and early stage VC. By design, none of the programs examined here take equity investments in participating firms. Since the primary outcome that I examine is fundraising, it would be challenging to evaluate such programs in the same analysis.

addition to overall scores and ranks, I observe individual judges and their scores. There are 47,066 judge-venture pairs (where a judge scored a given venture in a given round). In most competitions, judges score 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. Average judge ranks form an overall rank, which determines round winners.

I convert raw scores and ranks to percentile ranks. I primarily use decile ranks, either within losers and winners or in the round overall. For example, the variable "decile rank in round among losers" divides the losers in a round into ten groups, where the group with the best ranks is in 1, and the worst in 10. Some specifications use judge decile ranks, which is the venture's decile rank among ventures the judge scored. 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.[10]I also use z-scores for the subset that begin with raw scores (Appendix Table A2 contains statistics on scores and ranks).

Ventures never learn judge-specific scores or ranks. Judges themselves cannot learn from the feedback, as they observe only their own scoring and identities of round winners, and never overall ranks of losers.[11] My understanding is that judges and outside investors do not closely monitor competition participants to identify losers. Only winning participants are typically listed on a program website.

Although each competition is unique, there are no systematic differences in services provided (e.g. mentoring, networking, training) across the two competition types. [12] In no case did a competition with feedback advertise itself

[10]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. I cannot, therefore, use the raw rank or score data provided.

[11]While judges could in theory report their scores to each other, this would be quite an undertaking, as 17 judges that score a given venture on average (at the panel-round level).

[12]In all competitions, pitches are five to fifteen minutes (typically increasing by round), with an additional five to fifteen minutes of Q&A, and between one and two hours of dedicated networking (e.g., post-competition reception).

as providing relative ranks or more feedback in general, so there is no reason to believe that ventures with greater informational needs would have selected into these competitions (see Section 3 for tests).[13] This also implies that there is no reason to believe judges would put greater effort into scoring during the feedback competitions.

The 4,328 unique ventures in the data are described in Table 1 panel 2, and are categorized by sector and technology type in Table 2 (and by state in Appendix Table A3). There are 558 ventures that participate in multiple competitions. The average age of the ventures is 1.9 years.[14] Forty-four percent of the ventures were incorporated at the round date as a C- or S-corp.

I matched ventures to investment events and employment using CB Insights, Crunchbase, AngelList, and LinkedIn.[15] Venture survival, for example, is a binary indicator for the venture having at least one employee besides the founder as of August 2016. Founders are described in Table 1 panel 3, using data from the competitions and LinkedIn profiles. Twenty-one percent of founders are women, and 72% are men (the remaining 7% had both ambiguous names and no clear LinkedIn match).[16] Ventures and judges are assigned to 16 sectors (Table 2 panel 1). For ventures, sector assignations come from competition data, and each venture is assigned only one sector. For judges, sectors are drawn from LinkedIn profile or firm webpage, and judges may have expertise in multiple sectors. There are 8,139 instances in which a judge with expertise in a given sector scored a venture in the same sector.

Judges participate to source deals, clients, job opportunities, or because they view it as warm-glow inducing volunteer work. There are 2,514 unique judges, described in Table 2 panel 2, of whom 27% are VCs, 20% are corporate

[13] In all competitions, judges verbally ask questions and usually give some type of informal feedback. I do not observe this, but have no reason to believe that it varies systematically across the two types of competitions.

[14] 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.

[15] In researching the ventures, 765 name changes were identified. Ventures were matched to private investment on both original and changed names.

[16] Genders were assigned to founder names using a publicly-available algorithm. Unclear cases, such as East Asian names, were coded by hand.

executives, and 16% are angel investors. There is concern that the judges themselves investing 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, relative to about 47,000 judge-venture pairs.

Representativeness of the data

There is little empirical analysis either of startups prior to their first external funding event or of new venture competitions, so it is difficult to assess the representativeness of the sample. Appendix Table A5 compares the distribution of ventures in my data to overall U.S. VC investment, based on the National Venture Capital Association's (NVCA) 2016 yearbook. The share of software startups in my data, 37%, is very close to the national average (40%) for both deals and dollars. In part because of data from the Cleantech Open, a national non-profit competition focused on clean energy startups, my data is skewed towards clean energy.

The competitions take place in 17 U.S. states. With the exception of Arizona, which is oversampled in my data due to the presence of the large Arizona Innovation Challenge, the top twenty states in my data almost entirely overlap with the top twenty states for VC investment. The VC industry is concentrated in California, New York, and Massachusetts. In 2015, these states accounted for 77% of total U.S. VC investment, and 80% of VC deals, but are only 35% of my sample.[17] Relative to the NVCA data, my data has fewer ventures from California and more from Massachusetts. This may be expected from such early stage firms, as startups often move to Silicon Valley to raise VC.

The probability of an IPO or acquisition in my sample, 3%, is roughly similar to the 5% found in Ewens & Townsend (2017)'s sample of AngelList startups. There are on average three members of each venture team. This is

[17] VC investment totaled $34, $6.3, and $5.8 billion in these three states, respectively, relative to a national total of about $60 billion. The fourth state had only $1.2 billion. They had 2,748 deals, relative to a national total of 3,448 (source: PWC MoneyTree 2016 report).

similar to Bernstein, Korteweg & Laws (2015), 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. Whether this is representative of startup founders depends on the reference group. The average Y-Combinator founder is just 26, but Wadhwa et al. (2009) find that the average age of successful, high-growth startup founders is 40.[18] The average entrepreneur age at company founding among startups with at least a $1 billion valuation between 2003 and 2013 was 34 (Lee 2013). In sum, the data in my sample appear roughly representative of U.S. early stage startups and their founders.

Venture and founder characteristics that predict success

Beyond representativeness, I examine the associations between venture characteristics and success to ensure they are consistent with common intuition. These regressions use subsequent angel/VC investment and having at least 10 employees as of August, 2016 as measures of success (results in Appendix Table A6 panel A). 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 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, recalling a similar relationship between college selectivity and success for CEOs of VC-backed companies in Kaplan et al. (2012).

Ventures that identify their sectors as social impact or clean technology are much less likely to raise angel/VC, but are only slightly less likely to reach at least 10 employees. Associations between 17 sectors and success are in Appendix Table A6 panel B. 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.[19]

[18] See https://techcrunch.com/2010/07/30/ron-conway-paul-graham/

[19] 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.

3 Empirical design to measure the effect of feedback

I am interested in the effect of new, credible information on founder type revelation. The signal that entrepreneurs receive is feedback from judges about the quality of their ventures, codified as a rank relative to other ventures in the round.

Signal informativeness

If the signal is not relevant to firm outcomes, rational founders have nothing to "learn." In a regression discontinuity design, I show that judge scores are informative about venture outcomes, even among losers in no-feedback competitions. The empirical design and results are described in Section 5.

Estimating responsiveness to feedback

The ideal experiment to assess responsiveness is to randomly allocate feedback across ventures within rounds. I approximate this 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. I ask whether ventures that receive especially negative feedback are more likely to be abandoned.

The empirical design is a difference-in-differences model within the population of losers. The first difference is between above- and below-median losers in a given competition. The second difference is across feedback and no-feedback competitions. That is, I estimate the combined effect on the entrepreneur of receiving a below-median score, and knowing that he received a low score:

$$\begin{aligned} Y_i^{Post} &= \alpha + \beta_1 \left(\mathbf{1} \mid LowRank_{i,j}\right)\left(\mathbf{1} \mid StructuredFeedback_j\right) + \beta_2 \left(\mathbf{1} \mid LowRank_{i,j}\right) \\ &\quad + \beta_3 \left(\mathbf{1} \mid StructuredFeedback_j\right) + \gamma' \mathbf{f.e.}_{j'/k} + \delta' \mathbf{X}_i + \varepsilon_{i,j} \qquad (1) \\ &\quad \text{if } i \in Losers_j \end{aligned}$$

Here, i indexes ventures, and j competition-round-panels (e.g. the MIT Clean Energy Prize Semifinals). The coefficient of interest is β_1. I similarly estimate whether there is a symmetric effect for especially positive feedback among winners. I study heterogeneity by adding a venture characteristic as a third interaction, controlling for the three individual effects and the three two-way interactions. The sample is restricted to losers of a given round, and where a venture participated in multiple competitions, only the first instance is included.

A concern with this approach is that the feedback and no-feedback competitions may be different, for example attracting different types of ventures. Note, however, that the control group is the above-median losers within a round in both types of competitions. Therefore, average differences across the types of competitions are differenced out.

Evidence of distributional similarity by feedback status

While I am not concerned about average differences across the two competition types, distributional differences among losers could bias the results. That is, the distribution of losing ventures around the median may systematically different.

I first present visual evidence that the distributions of observable characteristics are similar. Appendix Figures 3 and 4 contain spikes representing the fraction of ventures within narrow z-score bandwidths for observables across all rounds in feedback and no-feedback competitions.[20] Appendix Figure 3 shows venture characteristics, including company incorporation, prior financing, technology type, whether the company is in a VC hub state, and whether the company is social impact-oriented or clean technology. Figure 4 shows founder characteristics, including whether the founder is a student at the time of the round, ever received an MBA, attended a top-20 college, and is of above median age (in years). The distributions are not the same; for example, because the HBS New Venture

[20]For example, I sum the total number of incorporated companies in feedback competitions. Then, again for only feedback competitions, I sum within a 0.1 z-score bandwidth the number of incorporated companies. I divide the second sum by the first. Thus, if Inc_i is an indicator for a company being incorporated, the bar height for 0.1 z-score band z in feedback competitions is: $\frac{\sum_{z,SF} Inc_i}{\sum_{SF} Inc_i}$.

Competition is large and does not have feedback, there are more founders from elite schools in this group. However, in no case does the distribution of losers (left tail) appear meaningfully lopsided.

I test for distributional differences around the median among losers in Table 3. I calculate each variable's mean above and below the median among losers in each round. Then I subtract the below median mean from the above median mean. Finally, I conduct a t-test across rounds with and without feedback. Among the nine observables at the time of the round considered in Table 3, the only significant difference is in the probability that the venture is located in a VC hub state. In the no-feedback competitions, above median losers are 4 percentage points (pp) more likely than below median losers to be in a hub state, while this difference is -1 pp for feedback competitions. If anything, this should bias against my result that negative feedback leads to a higher probability of failure, since ventures in hub states are unconditionally more likely to succeed (Appendix Table A6).

I compare overall competition and round characteristics in Table 4, also using t-tests. The types of competitions are broadly similar: the number of ventures, winners, and judges are not statistically different across the two groups. The award amount is much higher in the feedback competitions, but this should not engender difference between below and above median losers.

A concern is that founders with more uncertainty about their project quality may select into competitions with more feedback. Although competitions did not advertise their intended feedback, in theory this could have been public knowledge. One way to test for such selection is to examine ventures in multiple competitions. Suppose that founders with high information needs tend to select into competitions with feedback. Among founders that compete in a second competition, high information need founders will disproportionately participate in feedback competitions. I use having a low average score or a highly dispersed score in the first competition as proxies for information need. Appendix Table A7 presents summary statistics for the sample used in the test, and t-tests for whether information need, measured in the first round of the first competition, is associated with participation in a second competition with feedback. I find no association, suggesting that in the overall sample, it is unlikely that founder selection into competition type is affected by information needs.

Matching

To address any remaining concerns about systematic distributional differences between participants that receive feedback and those that do not, I build two matching estimators. These estimators try to solve the problem of "missing" potential outcomes by matching subjects in a treatment group to their closest counterparts in the the untreated group.

The treated group comprises losers with below-median ranks who do receive feedback, and the untreated group comprises losers with below-median ranks who do not receive feedback. Matching estimators use the outcome for the "closest" untreated participant as the missing potential outcome for the treated participant. The difference between observed and predicted outcomes is the average treatment effect. I compare survival (the outcome) for these matched groups to the above-median matched group.

The first matching method is exact matching, which is preferable as there is no conditional bias in the estimated treatment effect (Abadie & Imbens 2006). The samples of above-and below-median losers were matched exactly on thirteen sectors, competition year, student status, and company incorporation status. I conduct out-of-sample covariate balance tests in Appendix Table A8 (that is, variables not used in matching). Panel 1 shows the balance after matching, and Panel 2 before matching. The match dramatically reduces the differences. For example, the difference in MBA incidence falls from 27 percentage points (pp) to 3 pp, and the difference in venture age falls from 1.2 years to 0.4 years.

The second method is propensity-score matching, which first estimates a probability of treatment using a logit model. It then identifies, for each treated participant, the untreated participant whose scores (probabilities of treatment) are closest. 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 (Hill & Reiter 2006). Second, I match only on binary covariates; I use the covariates from the exact match plus several others, such as prior external financing.[21] Third, I omit matches without common support, which reduces the matched sample by 408 ventures.[22] Appendix Table A9 shows the covariate balance after (panel 1) and before (panel 2) matching. The process brings the samples almost entirely in line, with no p-values below 0.5 and no differences greater than 1 pp.

4 Results: Responsiveness to feedback

4.1 Average responsiveness

This section shows that entrepreneurs who receive especially negative feedback about their ventures are more likely to abandon them. Equation 1 is estimated in Table 5. The dependent variable is survival to August 2016. The coefficient of interest is on the effect of having a below median rank among losers in a round where the venture is informed of its rank, relative to having a below median rank among losers in a round where the venture is *not* informed of its rank, after controlling for the two individual effects of below median rank and receiving feedback. The control group is above-median losers in both types of competitions.

The main specification in Table 5 column 1 finds that negative feedback reduces the likelihood of continuation by 8.8 pp, relative to a mean of 34%. This translates to a 12% increase in the probability of failure. Two tests account for potential non-linearities. First, in column 4 I control for the venture z-score first

[21] Abadie & Imbens 2006 note that the matching estimator's bias increases in the number of continuous covariates used to match.

[22] Requiring common support means that participants are excluded if their propensity scores fall outside the range that overlaps across the treatment and control groups.

and second moment in z-score. Second, in column 5 I show the results from a logit specification.

The results of the matching exercises are in columns 6-8. The result with exact matching is almost precisely the same as the full sample result, at 7.6 pp, significant at the 1% level. The effect falls somewhat in the propensity-score matching. With controls (replicating the baseline specification), it is 5.6 pp, significant at the 5% level, and without controls it is just 4 pp, and significant only at the 1% level. Despite the decline, the general robustness of the effect to these approaches indicates that distributional differences across the two types of competitions are unlikely to drive the main effect.

Note that in Table 5, all rounds are included, so ventures that make it to a final round have multiple "chances" to receive especially negative feedback. Table 6 columns 1-2 use only data from preliminary rounds, and find larger effects of about 12 pp, significant at the 1% level. The effect also persists within important subsamples. Three models in Appendix Table A10 show that the effect persists within the population of founders with MBAs, among ventures from VC hub states, and among student-led ventures.

I also test for robustness within a single program in my data, the Cleantech Open (CTO). CTO gave feedback in 2011 but in no other year. As the competition did not otherwise change in 2011, there is no reason that the distribution of quality among losers should have been different in 2011.[23] Within the CTO sample, Appendix Table A11 shows that negative feedback reduces the probability of survival by 11-13 pp, very similar to the main specification. This is true using only the years 2010-12, as well as all years for which I have CTO data (2008-14).

It may be the case that "noisy" or inefficient learning (as in Jovanovic & Lach 1989) occurs when ventures are assigned especially lenient or harsh judges. I use a version of the leave-one-out judge leniency from Dobbie & Song (2015) and

[23] Cleantech Open did not advertise its use of Valid Evaluation software, so it would have been almost impossible for applicants to be aware of whether or not they would be informed of their rank after each round.

Chang (2013). Specifically, I measure the judge propensity to give ventures their highest scores, leaving out venture i. Let S_{ij} be an indicator for the highest score a venture received across judges, where this score came from judge j.[24] Let n_j be the count of ventures that the judge scored. The leave-one-out leniency measure is then $L_{ij} = \frac{1}{n_j - 1}\left(\sum_{k=1}^{j} S_k - S_i\right)$, summarized in Table 2 panel 3. In unreported tests, these leniency measures do not predict outcomes or responsiveness, though the leniency score strongly predicts a given judge's score. This suggests, together with the evidence in Section 5 that rank predicts outcomes, that the average responsiveness is likely efficient.

More generally, since feedback is private and costless to ventures, receiving it must be weakly more efficient. While my data do not permit a welfare calculation, the main result implies that had the 1,603 unique below-median losers in the no-feedback competitions received feedback, an additional 192 should have been abandoned, beyond the 1,186 that were abandoned.[25]

4.2 Are founders overconfident?

The large effect of subtle, low-stakes feedback rules out extreme overconfidence or optimism. Overconfidence often takes the form of miscalibration, or over-precision, in which a person believes his own information is more accurate (has lower variance) than it is in reality (Daniel et al. 1998). Optimism, or the above average effect, occurs when people erroneously believe they are better (higher mean) than a reference group. This bias is more global, while over-confidence is more task-specific (Astebro et al. 2007).

Contrary to the suggestions of some existing empirical and theoretical work in entrepreneurship, startup founders are not characterized by blind persistence in the face of negative information about their project's quality. This by no means, however, rules out some degree of overconfidence or optimism.

[24] Two competitions only use ranks, and do not have scores. I omit them from this analysis, so the sample is somewhat smaller.

[25] Based on the primary specification, where the coefficient of 8.8 pp translates into feedback increasing abandonment by about 12%.

The remainder of Section 4 searches for the presence of these biases using heterogeneity in the main effect. In Table 7, I add a binary venture characteristic as a third interaction to Equation 1; for brevity, panel 2 does not report control coefficients. Some of the characteristics are correlated with each other; a full correlation table is in Appendix Table A13.

Benchmarking overconfidence with gender

Being a woman is the characteristic that is most robustly associated with reduced overconfidence and optimism (e.g. Barber & Odean 2001, Beyer & Bowden 1997). Roberts & Nolen-Hoeksema (1989) show in the laboratory that women are specifically more responsive to negative feedback than men.

The gender difference in overconfidence is most prominent in masculine tasks (Lundeberg et al. 1994). For example, Niederle & Vesterlund (2007) conduct an experiment where men and women choose between piece rate and tournament compensation. Women choose competition 35% of the time, compared to 73% for men. They conclude this disparity is

> "largely explained by gender differences in overconfidence, while risk and feedback aversion seem to play a negligible role...We find that men are substantially more overconfident about their relative performance than women and that the beliefs on relative performance help predict entry decisions."

Motivated by this evidence, I expect women, who comprise 21% of founders, to provide the best proxy for maximum responsiveness. I therefore partition the sample on gender. Table 7 panel 1 column 5 finds that within the sample of women, negative feedback reduces the probability of survival by 18 pp, an increase of 69% relative to the mean (which is only 26%). This translates to a 24% increase in abandonment. Column 6 finds that the effect is 7 pp among men, close to the effect in the full sample, and a roughly 11% increase in abandonment. Thus the overall population of new venture founders is remarkably sensitive to a subtle form of feedback, but is far from maximally responsive.

4.3 Are founders Bayesian updaters?

The results thus far suggest that while entrepreneurs respond to low-stakes feedback, learning may co-exist with behavioral biases. This section explores whether learning in my context is consistent with Bayes' rule, which dictates how rational agents update their beliefs (see e.g. Pastor & Veronesi 2009 for examples of Bayesian learning behavior in financial markets). Given a prior belief and a new signal, both of which are normally distributed, the posterior belief of the Bayesian updater is a precision-weighted average of the two.[26]

If founders are Bayesian updaters, neither overconfidence nor optimism can be too extreme. They imply, respectively, that the variance (mean) of the prior is too low (high). Conversely, behavior inconsistent with Bayes' rule would suggest that overconfidence and optimism are salient features of the founders in my setting.

Linearity in the effect

If founders are optimists, I expect that only very negative feedback would elicit responsiveness. Founders informed that they are closer to the cutoff for winning the round may update their prior downward, but since their starting prior is too high, they will not update "enough." Conversely, if founders are Bayesians without excessively optimistic priors, I expect the effect to be broadly linear.

I test this in two ways. First, I ask if the effect seems to vary across the distribution of losers. The last three columns of Table 6 use alternative definitions of "low rank." In column 3 (4), "low rank" is one if the venture is in the bottom three (seven) deciles among losers, and zero if in the top seven (three) deciles.

[26] A Bayesian updater solves the following problem regarding beliefs about a parameter θ, such as the true quality of his venture. His prior belief is $\theta \sim \boldsymbol{\mathcal{N}}\left(\theta_0, \sigma_0^2\right)$, and he receives independent signals $s_t = \theta + \varepsilon_t$, where $\varepsilon_t \sim \boldsymbol{\mathcal{N}}\left(0, \sigma_s^2\right)$. A more uncertain prior means that σ_0^2 is higher, while a noisier signal means that σ_s^2 is higher. After receiving T signals, his posterior is $\theta \sim \boldsymbol{\mathcal{N}}\left(\theta_T, \sigma_T^2\right)$, where $\theta_T = \theta_0 \frac{\frac{1}{\sigma_0^2}}{\frac{1}{\sigma_0^2}+\frac{T}{\sigma_s^2}} + \bar{s}\frac{\frac{T}{\sigma_s^2}}{\frac{1}{\sigma_0^2}+\frac{T}{\sigma_s^2}}$, and $\sigma_T^2 = \frac{1}{\frac{1}{\sigma_0^2}+\frac{T}{\sigma_s^2}}$. Thus, more signals (higher T) increases the weight on the average signal, as do more precise signals (lower σ_s^2) and noisier priors (higher σ_0^2).

In column 5, I omit the lowest ranking losers and define “low rank” as one if the venture is in deciles 5-8, and zero if in deciles 1-4. The effect is similar to the main specification across the three columns; if anything, it is slightly larger towards the higher end of the loser distribution.

Second, the effect of feedback is weakly symmetrical for winners (Appendix Table A12). The sample is smaller, as most rounds have far fewer winners than losers. With judge fixed effects, there is a strong positive effect on continuation of extremely good feedback, but the effect is insignificant in the standard sample of one venture-round observation. In general, however, these tests suggest that founders are not excessively optimistic, and further that any biases do not vary dramatically across the quality spectrum.

Signal precision

While most people exhibit some miscalibration (De Bondt & Thaler 1995), if it is a defining feature of the founder, I do not expect founders to behave in ways consistent with Bayesian updating. Bayesian updaters should dismiss imprecise signals and update less when they have more information about their own type.

One obvious measure of signal precision is the number of judges. While founders are not informed of judge-specific scores, they can observe the number of judges in the competition. I find that founders are much less responsive when there are fewer judges. This is, in fact, the strongest heterogeneity result in terms of magnitude and significance. The effect of negative feedback on continuation is 29 pp greater when the number of judges is above median (Table 7 panel 2 column 5).

Signal precision: Judge expertise

A natural source of variation in signal precision is judge expertise: if a Bayesian founder receives negative feedback from judges that are highly informed about his chances of success, he should be more responsive. Unfortunately, exploiting variation in judge expertise is challenging. First, the number of judges and their composition vary across competitions and rounds. For example, in a final round of the Rice Business Plan competition, there are 133 judges, relatively few of whom are elite venture capitalists, while in a first-round panel of the HBS New Venture Competition, there are only five to six judges, many of whom are elite venture capitalists. Second, ventures are not typically given a list of judges before the competition, so it may be hard for them to infer skill or industry experience.

Nonetheless, I use judge expertise by sector (based on LinkedIn profiles and firm webpages) and judge occupations (based on competition data, AngelList, and LinkedIn profiles) to test whether having an especially large fraction of a certain type of judge is associated with more responsiveness. The results are in columns 15-21 of Table 7 panel 2. The binary characteristic C_i is constructed as follows one if the share of judges in a certain category (say, being a VC) who scored a venture is higher than the median share for all competition-round-panels.

I find no variation in responsiveness when an especially large fraction of judges are VCs, elite VCs, or angel investors (columns 15-17). I also find no greater responsiveness when a large share of judges has expertise in the venture's sector. This measure may be noisy because judges are assigned to multiple, crudely defined sectors. These results could be consistent with irrational updating, or they may also reflect the challenges described above.

In contrast, I find that ventures are much more responsive when the fraction of judges who are corporate executives is above-median (column 18). Directionally, I also find more responsiveness to founder/entrepreneur judges (p-value of 0.12). It seems plausible that founders are more able to identify these judges as having relevant business acumen. First, they lead companies whose names are usually immediately associated with a specific function or industry. Which asset managers have selection skill may be more challenging to ascertain (indeed, it is difficult even with large amounts of data, as Sørensen (2007) points out).

Signal precision: Criteria-specific responsiveness

A further test for signal precision comes from criteria, or dimension scores.[27] The short pitch duration and judge backgrounds imply that judges tend to have more expertise in business viability (e.g. the extent to which there is demand for the venture's proposed product) than technology viability.[28] I expect that Bayesian founders will be more responsive to negative feedback on criteria where the judges likely have expertise than on criteria where the founder likely has more private information.

First, the unconditional association between dimension ranks and outcomes, controlling for win status, is in Table 8. For all outcomes other than IPO/acquisition, a higher team rank is the strongest predictor of subsequent success. This is consistent with Bernstein, Korteweg & Laws (2015) and Gompers et al. (2016), who find that early stage investors most value information about startup teams. Related work find a positive correlation between good managerial practices and productivity in large firms (Bloom et al. 2016). Except for business model, the other dimension ranks have some predictive power, notably financials.

I next examine whether the overall responsiveness to negative feedback is especially relevant for certain dimensions. The variable "low rank" is now an indicator for being a below-median loser within a specific dimension. The results, in Table 9, reveal that negative feedback impacts continuation most along the financials, business model, market, and team dimensions. There is no effect for product/technology.[29] Founders likely have better private knowledge about the quality of their product or technology than judges do, making them more likely to dismiss low ranks in this dimension.

[27]The overall ranks and scores I have used thus far are, in most competitions, aggregated dimension (criteria) scores.

[28]Table 2 shows that they are mostly investors, corporate executives, consultants, and lawyers

[29]There is also no effect for presentation. Presentation scores may not affect survival because there is more scope for improvement (or perceived scope for improvement) along this dimension.

4.4 Do founders treat the venture as a real option?

The previous sections showed that while average responsiveness is lower than the theoretical maximum, founders seem to behave consistently with Bayesian updating. While surely present, overconfidence and optimism do not seem especially salient. An alternative explanation for reduced responsiveness is that founders treat their ventures as real options. A real option's value increases in its uncertainty and in its asset specificity, or irreversibility of investment (Dixit & Pindyck 1994).[30] The data do not permit affirmatively establishing whether or not founders treat their ventures as real options, but I conduct cross-sectional tests for whether less responsive founders likely have higher option values from continuing.

Venture risk

In a real options framework, the value of delaying abandonment should increase with venture risk. One measure of venture risk is uncertainty among judges.[31]I interact the effect of negative feedback with an indicator for whether the standard deviation of judge ranks within a competition-round-panel is above median.[32] The triple interaction has a positive effect (Table 7 panel 2 column 4); when judges are uncertain, founders are less sensitive to their overall rank.

There are two alternatives, however. First, more overconfident founders may choose riskier business models, as has been found among CEOs in Hirshleifer et al. (2012) and Graham et al. (2013). Second, this finding could be another case of founders updating less when the signal is less precise. Founders do not observe the standard deviation, as they are informed only of their overall rank. However, they might learn from verbal interactions with judges that they lacked

[30] For example, consider a firm deciding whether to drill an oil well or wait. The value of delay increases in oil price volatility and in the firm's private, non-transferable information about the land's geology (e.g. Kellogg 2014).

[31] Appendix Table A14 suggests that judge uncertainty - after controlling for rank and winning - predicts angel/VC series A financing, consistent with these types of investors targeting risky ventures.

[32] Ventures are unaware of this uncertainty; they receive only their aggregated rank in the feedback competitions.

consensus.[33]

To test the second possibility, I instrument for standard deviation using the judge leniency measure described above. 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. I consider two measures. First, $V_{i,\sigma}^{high}$ is the standard deviation of the leave-one-out leniency measure (L_{ij}). Second, $V_{i,\sigma}^{ext}$ is the standard deviation of L_{ij} 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 Table 2 panel 3. When variation in leniency is high, the venture receives an especially noisy signal that is independent of the venture characteristics.[34]

Appendix Table 15 columns 1-4 shows that the variation in leniency instrument ($V_{i,\sigma}^{high}$) predicts the standard deviation of judge scores quite well. The F-statistic of a first stage regression testing for the excluded instrument being statistically significant from zero is 31 in the primary specification, suggesting a reasonably strong instrument. The F-statistic for the first stage using $V_{i,\sigma}^{ext}$ is 16. In a naive instrumentation approach, I replace the standard deviation with the leave-one-out variation measures.[35] Columns 5-6 show no effect of the triple interaction between having a low rank, receiving feedback, and having judges with high expected variation in leniency. Thus reduced responsiveness when judges are more uncertain seems to reflect venture risk, consistent with a real options framework.

Venture stage and technology

On the asset specificity side, I expect that abandonment is more costly when more investment that ties the firm value to the entrepreneur's human capital has

[33] A lack of consensus in judge ranks could manifest during the competition through questions and verbal feedback.

[34] This measure assumes that judges are randomly assigned to ventures; based on discussions with competition organizers, I believe that this generally to be the case.

[35] Given the small sample and need for many instruments in the interacted regression, a two-stage-least-squares approach here is infeasible, as I would need a separate instrument for each interacted variable, which is unavailable.

occurred. I expect ventures that have already received investment, those with hardware rather than software technologies, and those that are already incorporated at the time of the competition have greater irreversibility of investment and thus value of continuing. Indeed, I find that indicators for these three characteristics are associated with less responsiveness (Table 7). Ventures with prior external financing are 15 pp more likely to continue after receiving especially negative feedback than those without prior financing, relative to a mean of 24% (panel 1 column 1). This effect is 11 pp relative to a mean of 44%, and more precisely estimated, for incorporated relative to unincorporated ventures (panel 2 column 1). This also suggests that firm boundaries are in part defined by initial learning about project success probabilities.

The cost of launching and then experimenting with an IT/software venture is lower than with a hardware venture, which requires more investment and, in particular, more irreversible investment (Kerr et al. 2014). Table 7 panel 1 column 2 shows that IT/software startups are more responsive; they are 10 pp more likely to fail after receiving especially negative feedback than hardware startups. This does not seem to relate to non-pecuniary motivations among hardware founders, as column 3 finds no effect for social impact/clean technology ventures. Exercising an abandonment option is more appealing when launching a startup requires little irreversible investment, helping to explain why software ventures are much easier to fund.

Heterogeneity in prior financing, incorporation, and technology type characteristics, however, could reflect two other mechanisms. First, they may proxy for venture stage, so if founders suffer from a sunk cost bias we might expect the same heterogeneity. Second, they may proxy for greater founder private information. Older ventures and non-student founders will have spent more time on their venture, and thus have larger sunk costs, but they will not necessarily have generated more specific assets. Table 7 panel 2 shows that ventures with above median age (0.8 years), student founders, and older founders are no more or less responsive.

Elite-school founders

I find that founders with elite college degrees are less responsive to feedback; for example, founders with top 10 college degrees are 23 pp less likely to fail in response to negative feedback (Table 2 panel 3 columns 4 and 5). This source of heterogeneity could reflect a variety of mechanisms, only some of which I am able to directly test. They include variation in venture risk, non-pecuniary benefits, outside options, personal wealth, private information, and overconfidence. I consider each of these in turn.

First, elite school founders may select riskier businesses. However, there is no association between elite school founders and judge uncertainty (the correlation between a founder having a top ten college degree and the standard deviation of the judges' scores of is -0.01). This is suggestive evidence that elite school founders are not simply choosing riskier businesses, where the option value of continuing is higher.

Second, there may be different non-pecuniary benefits to entrepreneurship. Since elite school graduates likely have especially high outside options, it may be that only those with especially large non-pecuniary benefits select into entrepreneurship. This is plausible, but unlikely, as I do not find a strong reduction in responsiveness among social impact and clean energy ventures (Table 7 panel 1 column 3).

Third, I expect that elite founders have higher outside options and more personal or family wealth. Unfortunately, these point in opposite directions. A higher outside option should make a rational founder more likely to abandon when he receives negative feedback. However, the extent to which the venture resembles a call option will increase with the personal and family wealth of the founder. More personal wealth will both make it less costly to continue with the venture (which likely is not providing current cash flow) and also reduce downside risk in the event the venture ultimately fails. The present data, therefore, do not permit me to use these hypotheses to test whether less responsiveness among elite school founders is consistent with a real option framework.

Fourth, elite school founders may have better private information, and they may rationally respond less. Appendix Figures 5 and 6 show coefficients from regressing outcome measures on an indicator for elite status, with competition-round fixed effects. Founders with top college degrees are unconditionally more likely to succeed, so it seems potentially rational that elite school founders are less responsive.

Despite their higher average chances of success, I cannot rule out that elite founders are simply more overconfident. In certain leadership contexts, failing to learn may be optimal. For example, Bernardo & Welch (2001) and Goel & Thakor (2008) theorize that the few entrepreneurs or CEOs who do succeed benefit from their overconfidence. Bolton, Brunnermeier & Veldkamp (2013) theorize that good leaders make an initial assessment of their environment, and then persist in their strategy regardless of new information. Related empirical work by Kaplan et al. (2012) finds that better performing CEOs are characterized by less openness to criticism and feedback. These points apply best in my context to elite college graduates.

This analysis is suggestive rather than conclusive. It raises interesting questions, however, about how learning and overconfidence interact with innovation. New entrants with greater ambitions (e.g., elite school founders) or more radical technologies (e.g., higher judge uncertainty) are less responsive to feedback, while those with more incremental ideas are more adaptable. Theoretical models of industry dynamics could micro-found technological discontinuities in the small fraction of entrepreneurs that enter without regard to signals about net discounted expected cash flows, while the mass of entrants remain rational and responsive to new information. The former group are potentially transformative, and their overconfidence is crucial to coordinating others.

Time to Abandonment & Serial Entrepreneurship

Many founders appear inclined to pursue entrepreneurship as a career, rather than pursue a one-off idea. Among founders that abandoned their ventures, 39%

describe themselves as the founder or senior executive of a subsequent venture. On average, within the pool of abandoned ventures, serial entrepreneurship is correlated with quality as measured by judge scores. The correlations between serial entrepreneurship and decile rank (z-score) are -.14 (.21).

I examine predictors of serial entrepreneurship and time to abandon in Appendix Table 16. Having a top 10 MBA is associated with dramatically faster time to abandonment (138 days). This is consistent with these founders having more valuable outside options. Older founders and founders with PhDs have longer times to abandon. They may have lower outside options, or may be more overconfident. Relatedly, Ben-David et al. (2013) find that more education in the form of an MBA or PhD as well as age are associated with greater miscalibration among CFOs.

I measure time-to-fail as the number of days between the competition's end date and the first subsequent new job start date, among founders of abandoned ventures. I ask whether greater responsiveness in terms of quickly abandoning after negative feedback associated with more serial entrepreneurship in Appendix Table 17. The "abandoned fast" variable is 1 if the abandonment time is above the median (148 days). While fast abandonment is strongly associated with founding a new venture, I find no effect of the triple interaction between being a below median loser, feedback, and abandoning fast (column 1).

However, I find a large positive effect of receiving feedback and abandoning fast on serial entrepreneurship. Subsequent columns in Appendix Table 17 show individual effects and interactions between feedback and fast abandonment. Feedback itself has no effect, but when founders learn their relative standing *and* abandon quickly, they are 21 pp more likely to start a new venture (about half the mean rate). This seems to reflect learning from feedback among higher types. Column 4 omits the bottom three deciles among losers, and finds an even stronger effect of feedback and fast abandonment, at 25 pp (significant at the 1% level). This is tentative evidence that among responsive founders, feedback may make serial entrepreneurship more efficient.[36]

5 Signal informativeness

This section establishes that the competitions generate valuable, informative signals. I estimate variants of Equation 2, where the dependent variable Y_i^{Post} is a measure of venture success, such as whether it had 10 or more employees by August, 2016, or whether it raised angel/VC series A investment after the round.

$$Y_i^{Post} = \alpha + \beta_1 WonRound_{i,j} + f\left(Rank/Z - score_{i,j}\right) + \beta_2 AwardAmt + \gamma' \mathbf{f}.\mathbf{e}._{j'/k} + \delta' \mathbf{X}_i + \varepsilon_{i,j}$$
$$\mathbf{X}_i = [prior\ fin,\ judge\ invested,\ sector\ f.e.,\ venture\ age,\ team\ size] \tag{2}$$

$WonRound_{i,j}$ is an indicator for whether the venture was a winner in the round. In the baseline empirical analysis, I include competition-round-panel or judge fixed effects.[37] The former absorb the date and location. Among the controls, I include an indicator for whether the judge or judge's company ever invested in the venture, and an indicator for whether the company previously raised external financing, and the number of team members. I cluster standard errors by competition-round-panel or by judge. The coefficient on percentile rank or z-score measures signal informativeness.

The primary empirical concern is that judges may sort firms on unobservables around the cutoff. This is unlikely. Although the number of awards is generally known ex-ante, judges score independently. Also, they typically only score a subset of the participating ventures.

[36] I also find no relation between being below median along a certain dimension and fast abandonment. That is, when fast abandonment or days to abandonment is the dependent variable (within the population of abandoners), no dimension seems to especially influence the time to abandon.

[37] Where a competition does not divide its preliminary rounds into panels, this is a fixed effect at the round level.

5.1 Results

Rank and z-score robustly predict measures of success, indicating that the competitions generate valuable signals This is shown visually in Figure 1, which limits the sample to losers in no-feedback competitions. Estimates of Equation 2 are in Table 10, where the dependent variable is external financing. Appendix Table 18 uses other proxies for success as dependent variables. Coefficients on rank (or z-score in Table 10 columns 6-7) are negative and highly significant, even within judge (Table 10 column 4).[38] Note that a negative coefficient on decile rank indicates that judge ranks are positively predictive of the success metric, while the opposite is true for z-scores. For most outcomes, rank is predictive on both sides of the award cutoff (among losers and winners of a round).[39]

Further, Appendix Table 19 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 the indicators are statistically significant at the 5% or 1% level.

A tangential benefit of this exercise is that it provides, to my knowledge, the first large sample, multiple-program causal assessment of the benefit of winning a competition. Many new venture competitions are publicly funded, both in the U.S. and abroad. 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. For example, the White House "Startup America" initiative, launched in 2011, champions the public sponsorship of acceleration and competition programs.[40]

Table 10 indicates that winning a round increases a venture's probability

[38]Note that models with judge fixed effects have larger samples because an observation is a judge-venture-round, rather than a venture-round. Also note that models with venture/founder controls have smaller sample sizes because they are not available for all ventures.

[39]A logit specifications in Table 5 column 2 confirms the strong predictive power of rank. I rely on OLS models in the remaining analysis. Not only does OLS have a simpler interpretation, but logit drops groups without positive outcomes, leading to overestimation when there are many fixed effects.

[40]https://www.whitehouse.gov/startup-america-fact-sheet

of subsequent external private finance by 7-12 pp, relative to a mean of 24%. The effect is 10 pp, significant at the 1% level, when the sample is limited to the quintiles around the cutoff for advancement within a preliminary round (Table 10 column 4).[41] Appendix Table 18 shows that the effect of winning and rank persist for other outcomes. For example, winning increases the chances of at least 10 employees in 2016 by 7-12 pp, relative to a mean of 20% (columns 5-6), and increases the chances of a successful exit by 2 pp, relative to a mean of 3% (columns 7-8). Among founders that abandon their ventures, I do not find an effect of winning on serial entrepreneurship. While winning is useful independently of the award, an extra $10,000 in cash prize increases the probability of financing by about 1 pp. This appears small in economic magnitude relative to the overall effect of winning and the predictive power of rank.[42]

My results suggest that competitions should consider focusing on their convening power and on providing feedback, rather than on awarding large prizes.[43] Competitions provide nascent entrepreneurs with failure-tolerant, timely feedback. While they reward top performers, they do not penalize especially poor performance, as ranks are private. They therefore appear a good implementation of Manso (2011)'s optimal contract to encourage exploration. He models innovation as learning through a series of experiments. The optimal contract tolerates early failure and reduces the cost of experimentation by providing timely

[41]It is possible that the positive effect of winning actually reflects a negative effect of losing. Perhaps it is costly in time and travel expense for the venture to compete, or perhaps losing generates a negative signal about venture quality. This would require substantial irrationality on the ventures' part. If the downside of losing - which is much more likely given that only a small share of competitors win - were much larger than the upside of winning, there should be little demand for competitions. Instead, the programs are typically oversubscribed. For example, the Rice Business Plan Competition receives between 400 and 500 applications for 40 places in its annual competition. However, I find that winning a preliminary round is useful even when the venture ultimately loses, and that among losers, a higher rank is predictive of success. Thus competitions may well be useful for a majority of participants.

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

[43]One limitation of this study from a policy perspective is that the evaluation is limited to participating firms. Fehder & Hochberg (2014) examine regional effects of similar programs by comparing regions with and without accelerators.

feedback about performance. This paper joins recent work, including Lerner & Wulf (2007), Azoulay et al. (2011), and Tian & Wang (2014), in finding empirical support for Manso's theory.

6 Conclusion

There are models of firm entry in which entrepreneurs' expectations are static, changing only when the firm fails (e.g. Acemoglu et al. 2013). Imperviousness to new information is consistent with explanations for entrepreneurial entry based on cognitive biases or non-pecuniary benefits. Entrepreneur sensitivity to new information is immaterial to some theoretical agendas, but it is important to models of firm dynamics and occupational choice, in which entry and exit occur when entrepreneurs or firm managers rationally learn about their type from new information. If such a learning process is occurring, we know little about it. This paper offers evidence of how entrepreneurs learn about their own type.

One way to conceptualize type revelation is passive Bayesian updating, in which a firm learns about its randomly drawn type through normal business operation, as in Jovanovic (1982) or Pástor et al. (2009). A second approach, associated with Ericson & Pakes (1995), is an active model of exploration in which a firm pays an entry cost without knowing its type. Through investment to improve productivity, the firm's state changes in a way that is informative about future profitability. Syverson (2004) typifies a third approach, in which potential entrants pay an initial cost to learn their type and then decide whether to produce or exit.

My results confirm the emphasis on uncertainty in all three approaches, and align most closely with models of active learning before entry. Firm boundaries are in part defined by initial learning about project success probabilities. Founders behave consistently with Bayesian updating, and when they do not respond to negative feedback by abandoning, the continuation decision seems roughly consistent with a real options approach to the venture. In a social welfare sense, this suggests that at least some entrepreneurs would benefit from more information about their ventures' quality. These frameworks coexist with overconfidence, and further research is needed to more fully investigate how innovation, cognitive biases, and learning in the sense type revelation interact.

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Table 1: Summary Statistics

Panel 1: Competitions

	N	Mean	Median	S.d.	Min	Max
# competitions	96					
# competition-rounds	214					
# competition-round-panels	543					
# competitions with feedback (venture learns rank relative to other participants)	35					
# rounds per competition	96	1.9	2	.69	1	3
# ventures in preliminary rounds	120	44	36	41	4	275
# ventures in final rounds	94	18	12	20	4	152
# winners in final rounds	94	4.5	5	3.6	1	25
Award amount$\vert$ Award$>$ 0 (thousand nominal $)	317	66	25	85	750	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					
Venture age at first competition (years)	2073	1.9	0.77	3	0	20
Probability incorporated at round	4328	0.44	0	0.5	0	1
Prob. in hub state (CA, NY, MA)	4,328	.35	0	.48	0	1
Prob. has $\geq$ 2 employees as of 8/2016	4328	0.34	0	0.47	0	1
if founder female	645	0.26	0	0.44	0	1
if founder male	3684	0.36	0	0.48	0	1
Prob. has $\geq$ 3 employees as of 8/2016	4328	0.3	0	0.46	0	1
Prob. has $\geq$ 10 employees as of 8/2016	4328	0.2	0	0.4	0	1
Prob. raised external private investment before round	7099	0.16	0	0.36	0	1
Probability external private investment after round	7099	0.24	0	0.43	0	1
Prob. angel/VC series A investment before round	7099	0.09	0	0.29	0	1
Prob. angel/VC series A investment after round	7099	0.15	0	0.36	0	1
Probability operating as of 9/2016†	4328	0.63	1	0.48	0	1
Prob. acquired/IPOd as of 9/2016†	4328	0.03	0	0.18	0	1
Ventures in multiple competitions (stats on # of competitions if # > 1)	558	2.52	2	0.98	2	9
Days between competitions among ventures in >1	978	302	215	289	1	2562
# founders/team members at first competition	2305	3.1	3	1.6	1	8
Percent of venture owned by presenting team	420	74.79	85.5	28.91	0	100
Possesses formal IP rights at round	1091	0.48	0	0.5	0	1

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	0.21	0	0.41	0	1
Male	3,228	0.72	0	0.45	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
Prob. is student at round	2554	0.2	0	0.4	0	1
Prob. graduated from top 20 college	2554	0.27	0	0.44	0	1
Prob. graduated from top 10 college	2554	0.18	0	0.39	0	1
Prob. degree from Harvard, Stanford, MIT	2554	0.1	0	0.3	0	1
Prob. has MBA	2554	0.48	0	0.5	0	1
Prob. has MBA from top 10 business school	2554	0.33	0	0.47	0	1
Prob. has Master's degree	2554	0.17	0	0.37	0	1
Prob. has PhD	2554	0.13	0	0.34	0	1
Prob 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. *Post-competition data from matching to CB Insights (752 unique company matches), Crunchbase (638), AngelList (1,528), and LinkedIn (1,933). †Active website. ‡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, measured as not having at least one person other than the founder identify as an employee on LinkedIn. See Appendix Table 4 for university rankings.

Table 2: Sector and Judge Data

Panel 1: Sectors

	# unique ventures	
Hardware	245	
Software	1,404	
	Sectors[‡]	
	Ventures	Judges
Air/water/waste/agriculture	146	31
Biotech	182	64
Clean tech/renewable energy	712	273
Defense/security	64	66
Education	37	118
Energy (fossil)	61	373
Fintech/financial	53	522
Food/beverage	88	24
Health (ex biotech)	270	291
IT/software/web	1,404	586
Manuf./materials/electronics	323	96
Media/ads/entertainment	57	157
Real estate	61	82
Retail/consumer goods	139	159
Social enterprise	42	42
Transportation	136	51

Panel 2: Judge Professions

	# unique judges
All	2,514
Venture Capital Investor	676
Elite VC[†] (by IRR/Multiple)	21
Angel Investor*	397
Mean (med) AngelList investments	12.8 (8)
Professor/Scientist	44
Business Development/Sales	83
Corporate Executive	498
Founder/Entrepreneur	240
Lawyer/Consultant/Accountant	369
Non-Profit/Foundation/Government	164
Other	193
# judge-venture pairs in which judge personally invested in venture	3
# judge-venture pairs in which judge's firm invested in venture	95
Total # judge-venture score pairs	47,066
# judge-venture pairs in same sector	8,139

Panel 3: Judge Uncertainty and Leniency Measures

	N	Mean	Median	S.d.	Min	Max
Judge uncertainty (std dev of within-panel judge decile ranks of a venture)	5997	1.88	1.02	1.97	0	6.36
Venture leave-one-out leniency score	3788	0.33	0.25	0.32	0	2
Venture leave-one-out harshness score	3779	0.33	0.29	0.28	0	2
$V_{i,\sigma}^{high}$ (venture leave-one-out leniency variation based on propensity to give highest score)	3770	0.21	0.19	0.13	0	0.96
$V_{i,\sigma}^{ext}$ (venture leave-one-out leniency variation based on four most extreme judges)	3788	0.31	0.29	0.13	0	1.15

Note: This table lists the number of ventures by technology type, the number of judges by profession, and the leniency measures (see Section 4.2 for details). [†]Preqin top 20 VC firm by either IRR or Multiple, as of 2016. *Identifies as angel investor in competition data, or has AngelList profile and at least one investment (160 judges). [‡]Venture sectors from competition data; each venture assigned to one sector. Judge sectors based on LinkedIn profile or firm webpage; judges may have expertise in multiple sectors.

Table 3: Test for distributional differences around median among losers, by round feedback status

	Feedback			No Feedback				
	N	Mean	S.d.	N	Mean	S.d.	Difference	2-tailed p-value
Venture characteristics								
Incorporated	127	0.03	0.24	48	0.06	0.20	-0.04	0.35
Financing before round	127	0.05	0.25	48	0.11	0.31	-0.06	0.21
IT/Software-based	127	-0.02	0.24	48	0.00	0.29	-0.02	0.68
Hub state (CA/MA/NY)	127	-0.01	0.17	48	0.04	0.17	-0.06	0.05
Social impact/cleantech	127	-0.02	0.28	48	-0.06	0.24	0.03	0.46
Founder characteristics								
Student at round	127	-0.03	0.14	48	0.00	0.09	-0.03	0.23
Has MBA	127	0.05	0.36	48	0.10	0.37	-0.04	0.51
Attended top 20 college	127	0.03	0.31	48	0.01	0.19	0.02	0.66
Age above median	99	0.05	0.37	26	0.08	0.25	-0.03	0.68

Note: This table compares the difference between above- and below-median losers across feedback status. Specifically, for each round the below- and above-median means are calculated. Then the below median mean is subtracted from the above median mean. Finally, a t-test is conducted across rounds with and without feedback.

Table 4: Competition Characteristics by Feedback Status

	No feedback			Feedback				
	N	Mean	S.d.	N	Mean	S.d.	Difference	2-tailed p-value
# ventures in round	77	31.81	21.07	53	40.53	46.08	-8.72	0.15
# winners	77	8.38	7.08	53	11.14	11.46	-2.76	0.09
# judges on panel	233	18.51	26.53	55	17.62	14.05	0.89	0.81
Award amount	94	42181	40650	55	183400	89941	-141219	0.00

Note: This table compares the difference between competition rounds by whether they have feedback or not.

Table 5: Effect of Negative Feedback on Venture Continuation

Sample restricted to losers of round, all rounds included
Dependent variable: Survival*

				Z-scores	Logit	Exact matching±	Propensity score matching**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Low rank·Feedback	-.088**	-.084***	-.079***	-.088**	-.32**	-.076***	-.056**	-.04*
	(.036)	(.02)	(.026)	(.036)	(.16)	(.027)	(.022)	(.023)
Low rank	-.062***	-.051***	-.026	-.064***	-.3***			
	(.021)	(.014)	(.022)	(.021)	(.099)			
Z-score				.04				
				(.029)				
Z-score^2				-.013*				
				(.0067)				
Feedback	.071*	.17*	-.031	.075*	.25			
	(.04)	(.092)	(.14)	(.039)	(.17)			
Venture controls†	Y	Y	Y+‡	Y	Y	N	Y	N
Year f.e.	Y	N	N	N	Y	N	Y	N
Judge f.e.	N	Y	Y	N	N	N	N	N
N	3765	26484	14938	3765	3765	2484	3357	3357
R^2	.083	.18	.29	.085	.065	-	.095	.064

Note: This table shows estimates of the effect of negative feedback. That is, the effect of a below-median rank among losers when losers learn their ranks, relative to competitions where they do not learn their ranks. Models are variants of:

$$Y_i^{Post} = \alpha + \beta_1 \left(\mathbf{1} \mid LowRank_{i,j}\right)\left(\mathbf{1} \mid StructuredFeedback_j\right) + \beta_2 \left(\mathbf{1} \mid LowRank_{i,j}\right) + \beta_3 \left(\mathbf{1} \mid StructuredFeedback_j\right) + \gamma' \mathbf{f.e.}_{j'/k} + \delta' \mathbf{X}_i + \varepsilon_{i,j} \text{ if } i \in Losers_j$$

"Low rank" is 1 if the venture's rank is below median among losers. Errors clustered by competition-round or judge, depending on fixed effects. Feedback varies by event, so competition-round f.e. are not used. * Survival is 1 if the venture had ≥ 1 employee besides founder on LinkedIn as of 8/2016. ±Causal effect via exact matching between "treated" group (low-ranked losers who received feedback) and control group (low ranked losers who did not receive feedback) on sector (there are 16 sectors), year, student status and company incorporation statuses. **Causal effect via propensity score (logit prediction of treatment) matching of treated and control groups. †Includes sector indicator variables, student status and company incorporation statuses. ‡Also includes company age and whether the company received investment before the round. *** indicates p-value<.01.

Table 6: Effect of Negative Feedback on Venture Continuation

Sample restricted to losers of round

Dependent variable: Survival*

	Preliminary rounds only		Low rank among losers definition:		
			Bottom 3 deciles	Bottom 7 deciles	Deciles 5-8 (9-10 omitted)
	(1)	(2)	(3)	(4)	(5)
Low rank·Feedback	-.12***	-.1***	-.06**	-.099**	-.082*
	(.044)	(.023)	(.029)	(.04)	(.046)
Low rank	-.051**	-.043***	-.065***	-.047**	-.024
	(.023)	(.016)	(.019)	(.022)	(.025)
Feedback	.11**	.17	.035	.078*	.08*
	(.045)	(.15)	(.028)	(.043)	(.043)
Venture controls†	Y	Y	Y	Y	Y
Year f.e.	Y	N	Y	Y	Y
Judge f.e.	N	Y	N	N	N
N	2689	17388	3765	3765	2381
R^2	.083	.14	.082	.082	.099

Note: This table shows estimates of the effect of negative feedback as in Table 6, but with alternative samples. Columns 1-2 use only preliminary rounds (no final rounds), and columns 3-4 redefine "low rank" as being either in the bottom 70 percentiles among losers or the bottom 30 percentiles (instead of below median). * Survival is 1 if the venture had ≥ 1 employee besides founder on LinkedIn as of 8/2016. †Includes sector indicator variables, student status and company incorporation statuses. *** indicates p-value$<$.01.

Table 7: Heterogeneity in Effect of Negative Feedback

Panel 1

Dependent variable: Survival*

Characteristic C_i:	Financing before round	Tech type IT/software	Social/ clean tech	Founder female	Sample: Women	Men
	(1)	(2)	(3)	(4)	(5)	(6)
Low rank·Feedback· C_i	.15*	-.1*	.072	-.1		
	(.087)	(.062)	(.088)	(.096)		
Low rank·Feedback	-.1**	-.015	-.1**	-.093**	-.18*	-.07*
	(.041)	(.043)	(.042)	(.039)	(.092)	(.04)
Feedback· C_i	-.19***	-.00096	-.089	.12		
	(.067)	(.058)	(.08)	(.073)		
Low rank· C_i	-.033	-.0035	.028	.071		
	(.069)	(.038)	(.047)	(.045)		
Low rank	-.047**	-.038*	-.051**	-.079***	-.0056	-.079***
	(.022)	(.023)	(.023)	(.025)	(.039)	(.025)
Feedback	.11**	.057	.1**	.059	.2**	.14***
	(.042)	(.038)	(.041)	(.045)	(.086)	(.04)
C_i	.37***	.09**	-.098**	-.11***		
	(.054)	(.037)	(.042)	(.039)		
Year f.e.	Y	Y	Y	Y	Y	Y
Sector f.e.	Y	N	N	Y	Y	Y
N	3765	4136	4136	3048	577	3188
R^2	.13	.12	.077	.1	.17	.054

Panel 2 (control coefficients not reported)

Dependent variable: Survival*							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
C_i:	Incorp. at round	Venture age > median	Founder age >med	Judge rank s.d. > med‡	# judges > median	From VC hub state†	From same state as comp.
Low rank·Feedback· C_i	.11***	.026	-.11	.12***	-.29***	-.018	.063
	(.033)	(.067)	(.089)	(.044)	(.11)	(.11)	(.064)
N	3765	2119	1594	3765	3765	3765	3765
R^2	.084	.082	.1	.086	.088	.085	.084
	(8)	(9)	(10)	(11)	(12)	(13)	(14)
C_i:	Founder is student	Founder top 20 college	Founder top 10 college	Founder Harvard/ Stanford/MIT	Founder has MBA	Founder top 10 MBA	Founder # previous jobs > median
Low rank·Feedback· C_i	-.02	.15	.24**	.31**	.11*	.055	-.058
	(.089)	(.11)	(.11)	(.14)	(.066)	(.12)	(.054)
N	3765	3765	3765	3765	3765	3765	3765
R^2	.083	.086	.087	.085	.085	.086	.087
	(15)	(16)	(17)	(18)	(19)	(20)	(21)
C_i: >med share of judges are±...	VC	Elite VCs	Angel investor	Corporate executive	Founder/ Entrepren.	Lawyer/ Cons./ Acct	Expert in venture sector
Low rank·Feedback· C_i	.088	.35	.06	-.14**	-.11	-.099	.038
	(.071)	(.25)	(.071)	(.061)	(.074)	(.064)	(.056)
N	3765	3765	3765	3765	3765	3765	3765
R^2	.085	.085	.085	.086	.084	.084	.083
Year f.e.	Y	Y	Y	Y	Y	Y	Y
Sector f.e.	Y	Y	Y	Y	Y	Y	Y

Note: This table shows estimates of the effect of negative feedback on venture continuation; specifically, the effect of a below-median rank among losers when losers learn their ranks, ("feedback"), relative to competitions where they do not learn their ranks. "Low rank" is one if the venture's rank is below median among losers, and 0 if it is above median among losers. Regressions are OLS variants of Equation 2, with an additional interaction. * This measure for venture continuation is 1 if the venture had at least one employee besides founder on LinkedIn as of 8/2016. Errors clustered by competition-round. Feedback varies by event, so competition-round f.e. are not used. †Venture state is California, New York, or Massachusetts. ‡Standard deviation of judge ranks for the venture is above median, among ventures in round. ±The fraction of judges in a given occupation/sector who scored the venture is above median, relative to that fraction for all ventures. *** indicates p-value<.01.

Table 8: Effect of Dimension Rank on Venture Outcomes

Dependent variable:	Financing after round*		≥ 3 employees as of 8/2016		≥ 10 employees as of 8/2016		Acquired/IPO	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Percentile rank in round:†								
Team	-.021***	-.023***	-.014***	-.021***	-.0091	-.017***	.00069	-.0012
	(.0057)	(.0053)	(.0051)	(.0052)	(.0063)	(.0049)	(.0026)	(.0024)
Financials	-.014**	-.0079	-.03***	-.027***	-.036***	-.026***	.0034	.0023
	(.0067)	(.005)	(.0094)	(.0058)	(.0083)	(.0057)	(.0031)	(.0027)
Business Model	.0032	.002	.0091	.012	.0024	.0035	.0046	-.0059
	(.016)	(.011)	(.016)	(.012)	(.014)	(.011)	(.0074)	(.0074)
Market††	.01	-.0091	.002	-.022*	.0075	-.011	-.00047	.0039
	(.015)	(.011)	(.015)	(.012)	(.013)	(.011)	(.0072)	(.0074)
Tech./Product	.0098	.0031	-.0043	-.0093*	-.0015	-.0081	-.0062**	-.0056**
	(.0078)	(.0054)	(.0075)	(.0055)	(.0069)	(.0054)	(.0024)	(.0024)
Presentation	-.015**	-.0098**	-.0023	-.0041	.0074	.008	-.0032	-.0013
	(.0059)	(.0043)	(.0083)	(.0048)	(.0071)	(.0052)	(.0024)	(.0022)
Won Round	.14***	.2***	.12***	.21***	.1***	.17***	.011	.023***
	(.024)	(.013)	(.035)	(.014)	(.032)	(.015)	(.013)	(.0068)
Judge/judge co invested	.47***	.56***						
	(.11)	(.027)						
Competition-round- panel f.e.	Y	N	Y	N	Y	N	Y	N
Judge f.e.	N	Y	N	Y	N	Y	N	Y
N	1926	8794	1926	8794	1926	8794	1926	7043
R^2	.15	.14	.16	.15	.13	.12	.065	.066

Note: This table contains OLS regression estimates of the effect of dimension-specific ranks on indicators for various outcomes, using variants of:

$$Y_i^{Post} = \alpha + \beta_1 Won\ Round_{i,j} + \delta' DimDecileRank_{i,j}/JudgeDimQuintileRank_{i,j,k} + \gamma' f.e._{j/k} + \varepsilon_{i,j,/k}$$

All rounds are included. Note that dimension scores are generally averaged to produce the overall ranks used in other tables. Errors clustered by competition-round or judge, depending on f.e. †Decile rank in round or quintile rank within judge. A smaller rank is better (1 is best decile, 10 is worst decile). *All private external investment after round. Note that competition f.e. control for a specific date. ††The attractiveness and size of the market. *** indicates p-value<.01.

Table 9: Effect of Negative Dimension-Specific Feedback on Venture Continuation

Sample restricted to losers of round

Dependent variable: Survival*

Criteria (dimension=D):	Presentation	Team	Product/ tech	Market††	Financials	Business model
	(1)	(2)	(3)	(4)	(5)	(6)
Low rank in D·Feedback	.0036	-.09**	-.052	-.089**	-.11***	-.097**
	(.062)	(.038)	(.033)	(.04)	(.038)	(.04)
Low rank in D	-.0096	.01	-.026	.087**	-.0013	.097**
	(.059)	(.037)	(.029)	(.04)	(.032)	(.04)
Feedback	.17**	.058	.04	.07*	.071	.072*
	(.071)	(.038)	(.034)	(.042)	(.053)	(.042)
Decile rank	-.034***	-.019***	-.017***	-.031***	-.016***	-.032***
	(.0059)	(.0046)	(.0045)	(.0048)	(.0054)	(.0049)
Venture controls†	Y	Y	Y	Y	Y	Y
N	2147	3147	3126	2538	2240	2538
R^2	.084	.089	.085	.089	.096	.09

Note: This table shows estimates of the effect of negative feedback; specifically, the effect of a below-median rank among losers when losers learn their ranks, ("Feedback"), relative to competitions where they do not learn their ranks. Regressions are variants of:

$$\begin{aligned} Y_i^{Post} &= \alpha + \beta_1 \left(\mathbf{1} \mid LowRank_{i,j}\right) \left(\mathbf{1} \mid StructuredFeedback_j\right) + \beta_2 \left(\mathbf{1} \mid LowRank_{i,j}\right) \\ &+ \beta_3 \left(\mathbf{1} \mid StructuredFeedback_j\right) + \gamma' \mathbf{f.e.}_{j'/k} + \delta' \mathbf{X}_i + \varepsilon_{i,j} \text{ if } i \in Losers_j \end{aligned}$$

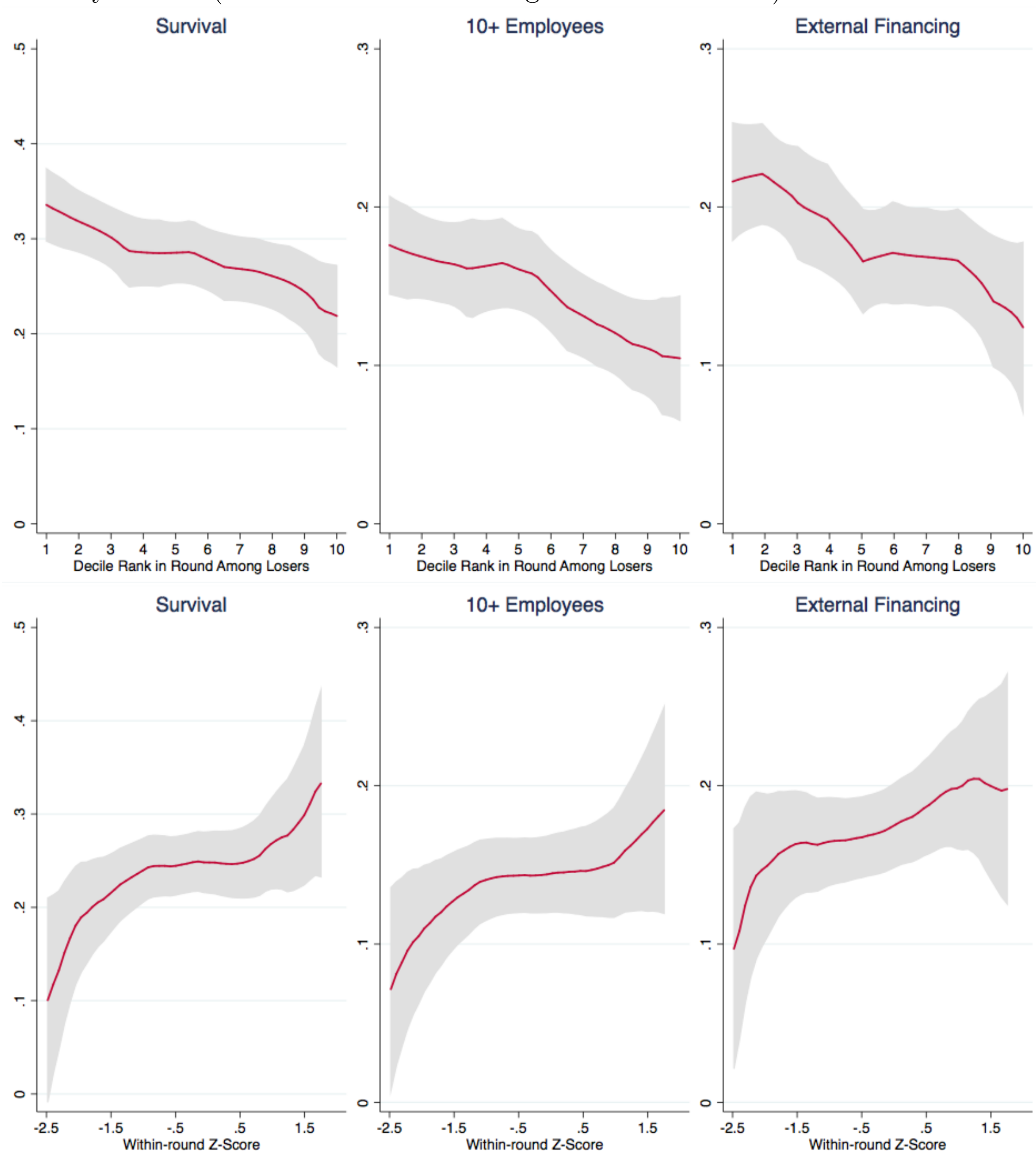
"Low rank" is one if the venture's rank is below median among losers, and 0 if it is above median among losers. * This measure for venture continuation is 1 if the venture had at least one employee besides founder on LinkedIn as of 8/2016. Errors clustered by competition-round or judge, depending on fixed effects. Feedback varies by event, so competition-round f.e. are not used. †Includes sector indicator variables, whether the company is incorporated, and whether the founder is a student. ††The attractiveness and size of the market. *** indicates p-value<.01.

Table 10: Effect of Rank and Winning on Subsequent External Financing

Dependent variable: Financing after round*		Venture controls	Logit	Judge f.e.	Quintiles around cutoff; prelim rounds‡	Z-scores		No feedback only	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Won Round	.13***	.077**	.8***	.16***	.098***	.13***	.098***	.13***	.15***
	(.026)	(.037)	(.14)	(.015)	(.026)	(.023)	(.026)	(.034)	(.02)
Decile rank winners†	-.012***	-.0062	-.071***					-.0091	
	(.0044)	(.0056)	(.021)					(.0061)	
Decile rank losers	-.018***	-.014***	-.13***					-.011***	
	(.0025)	(.0032)	(.017)					(.0033)	
Within-judge decile rank				-.0061***					
				(.0014)					
Z-score among winners						.027			.0064
						(.019)			(.023)
Z-score among losers						.041***			.031***
						(.01)			(.011)
Z-score^2 winners						.019			.013
						(.014)			(.016)
Z-score^2 losers						.000056			.0097
						(.0073)			(.0084)
Within-judge z-score							.027***		
							(.0063)		
Award Amount ($,	.0085***	.0093***		.011***	.013**	.0089***	.0056*	.011**	.012**
10,000s)	(.0024)	(.003)		(.0023)	(.0057)	(.0029)	(.0029)	(.0055)	(.0055)
Venture controls††	N	Y	N	Y	N	Y	Y	N	N
Comp.-round- panel f.e.	Y	Y	Y	N	Y	Y	N	Y	Y
Judge f.e.	N	N	N	Y	N	N	Y	N	N
Year f.e.	N	N	N	Y	N	N	Y	N	N
N	6046	3367	5500	23785	1945	3529	13285	3429	3980
R^2	.16	.4	.12	.43	.23	.41	.4	.2	.19

Note: This table contains OLS regression estimates of the effect of winning, rank, and award (cash prize) on whether the venture raised private investment after the competition, using variants of:
$Y_i^{Post} = \alpha + \beta_1 WonRound_{i,j} + f\left(DecileRank_{i,j}\right) + \beta_2 AwardAmt + \gamma' \mathbf{f.e.}_{j'/k} + \delta' \mathbf{X}_i + \varepsilon_{i,j}$. OLS used except column 2. Errors clustered by competition-round or judge, depending on f.e. A smaller rank is better (1 is best decile, 10 is worst decile). *All private external investment after round. ‡Includes only the two quintiles around the cutoff for winning a preliminary round (no final rounds included). †Decile rank in round among winners. ††Includes whether the company received investment before the round, whether any of the venture's judges or those judges' firms ever invested in the venture, sector indicator variables, company age, and whether the founder is a student. Note that competition f.e. control for a specific date. *** indicates p-value<.01.

Figure 1: Outcomes in no-feedback competitions, within sample of losers in preliminary rounds (lower decile rank and higher z-scores better)



Note: These figures show the probability of three binary outcomes: survival (venture had at least one employee besides founder on LinkedIn as of 8/2016), 10+ employees (venture had at least 10 employees besides founder on LinkedIn as of 8/2016), and external financing (venture raised seed or series A investment after the round). The x-axis is the venture percentile rank among losers (top) and the venture's z-score (bottom). Only losers in preliminary rounds included, but z-scores calculated relative to all ventures in the round. Local polynomial with Epanechnikov kernel using Stata's optimal bandwidth; 95% confidence intervals shown.