Learning from Feedback:

Evidence from New Ventures

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Abstract

This paper studies how early stage entrepreneurs learn about the quality of their ventures. I assess the effect of negative feedback on abandonment using application and judging data from 87 new venture competitions, 34 of which privately informed founders of their relative rank. I use a difference-in-differences design and matching estimators to compare lower and higher ranked losers, across competitions in which they did and did not observe their standing. Receiving negative feedback increased venture abandonment by about 13 percent. The effect occurs quickly, doubles among women founders, and increases with signal precision. It decreases with venture maturity and riskiness.

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1. Introduction

Learning plays a pivotal role in many models of firm dynamics, including Jovanovic (1982), Aghion, Bolton, Harris & Jullien (1991), and Ericson & Pakes (1995).[1] New information determines entry and exit decisions in these models, implying that entrepreneurs should be sensitive to external signals about their project quality. Yet empirical work has thus far found little evidence of this, and instead has yielded a strong paradigm that entrepreneurs do not learn about their own probability of success. This behavioral view emphasizes the role of cognitive biases such as over-precision and optimism in entrepreneurial decision-making.[2] It also stems from evidence of low returns, even among founders of venture capital-backed startups (Moskowitz & Vissing-Jørgensen 2002).[3]

Motivated by this evidence, some theorists model entrepreneurs as overconfident individuals who fail to update their priors in light of new information (Bernardo & Welch 2001, Bergemann & Hege 2005, Landier & Thesmar 2009). More broadly, financial contracting theory focuses on information asymmetry, and typically assumes that the entrepreneur knows his type or has static beliefs about it (Aghion & Bolton 1992, Admati & Pfleiderer 1994, Clementi & Hopenhayn 2006, Sørensen 2007).[4] Empirical research in entrepreneurial finance often makes similar assumptions (Megginson & Weiss 1991, Hsu 2004, Gompers, Kovner, Lerner & Scharfstein 2010).

To my knowledge, there is little evidence about firm or entrepreneur type revelation.[5] New venture competitions, in which early stage founders present their businesses to a panel of expert judges, are well suited to the challenging task of studying learning. I use novel data on 4,328 new ventures participating in 87 competitions in 17 states between 2007 and 2015 to explore the sensitivity of founder beliefs to new information.

I exploit variation in feedback provision to isolate its effect. In 53 of the competitions,

[1] Also see Berk, Green & Naik (2004), Pástor, Taylor & Veronesi (2009), and Poschke (2013).

[2] See Astebro, Jeffrey & Adomdza (2007), Cooper et al. (1988), Camerer & Lovallo (1999), Arabsheibani et al. (2000), Koellinger et al. (2007), Kogan (2009), and Bloom et al. (2014).

[3] Also see Hamilton (2000) and Hall & Woodward (2010). There is related evidence of non-pecuniary benefits from entrepreneurship (Hvide & Møen 2010, Hurst & Pugsley 2011).

[4] Also see Hellmann (1998), Cagetti & De Nardi (2006), and Ewens et al. (2013).

[5] In related work, Wagner (2017) studies feedback in the Startup Chile accelerator program, and finds that in general it led to better outcomes for startups. This paper is different in that it focuses on how negative feedback affects startup continuation (learning as type revelation), and addresses heterogeneity in founder and startup characteristics.

ventures are informed only that they won or lost, and otherwise do not learn where they stand relative to their peers. In 34 of the competitions, ventures are privately informed of their rank in the round. The competitions are otherwise similar, and in the feedback competitions neither ventures nor judges were informed that structured feedback would be provided. Founders never observe individual judge scores or ranks, but I observe overall ranks and individual judges' scores and ranks in all cases.

I link the ventures to employment, financing, and survival outcomes. I also link the founders to education and career histories. Founders are mostly first-time entrepreneurs, and essentially all seek external finance to grow quickly. They are roughly representative of the U.S. startup population, with no local subsistence businesses – such as restaurants or landscapers – that often contaminate efforts to study high-growth entrepreneurship (Levine & Rubinstein 2016).

Despite the importance of startups to economic growth, they are challenging to study in their earliest phases (Haltiwanger, Jarmin & Miranda 2013, Guzman & Stern 2016). This is the first paper to study new venture competitions, which have become a common venue for entrepreneurs to test their ideas and attract attention.[6] I shed light on these competitions and their founders. For example, founder job experience or having a software venture are associated with success, while having an MBA or a hardware venture are not.

To be informative, signals must be relevant to outcomes. In a regression discontinuity design, I show that percentile rank and z-score robustly predict measures of success, such as subsequent external financing, employment, and acquisition or IPO. This exercise has the tangential benefit of finding that winning a competition is causally useful, to my knowledge the first such result.

I identify the effect of negative feedback on venture continuation using a difference-in-differences specification among losing ventures. The first difference is within round, 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.[7]

[6] Recent work studies related programs, including Hallen et al. (2014), Fehder & Hochberg (2014), Scott, Shu & Lubynsky (2016), Fehder (2016), and Gonzalez-Uribe & Leatherbee (2016).

[7] Judges cannot learn from the feedback. They observe only their own scoring and winner identities.

This low-stakes negative feedback should not affect extremely overconfident founders. Instead, I find that receiving negative feedback significantly reduces the probability of venture continuation. Specifically, it reduces the chances a venture has at least one employee besides the founder as of August, 2016 by about eight percentage points, equivalent to a 13% increase in abandonment (the mean is 66%). The effect occurs quickly, mostly in the first six months. It does not reflect an effect of feedback on subsequent financing. The data reject models in which entrepreneurs have static types, or equivalently models in which entrepreneurs are so overconfident that they ignore new information.

Ventures that are not yet incorporated, which comprise 56% of the sample, and those without prior external private financing (84%), are more responsive. In some models, including Cornelli & Yosha (2003), Schmidt (2003), and Grenadier & Malenko (2010), firms update their beliefs after initial investment and business operation. My results support these models but also show that type revelation can occur before entry, at *de minimis* cost.[8] In my data, founders learn from experts who are able to forecast with some accuracy how the market will react.

I also find that software-based ventures are more responsive than hardware-based ventures. Women, who lead 21% of the ventures, are more than twice as responsive as men. Founders dismiss imprecise signals; they are less responsive when there are fewer judges. The signal is an average of judge ranks, but I also observe secret individual judge scores. I find that when judges are uncertain about a venture, measured as a high score standard deviation, the founder is less responsive to negative feedback. This seems to reflect venture risk, as a leave-one-out leniency variation measure predicts the standard deviation but not lower responsiveness. Finally, I examine founders that abandon the venture but launch a second one. These serial entrepreneurs are not more or less responsive.

The central empirical concern is whether the main effect may reflect systematically different distributions among losers in the two types of competitions (differences in levels are absorbed). To address this concern, I use three tests and five robustness exercises. First, I demonstrate visually and statistically that the distributions of observables across the two types of competitions are similar ex-ante. Second, I show that rank reflects measures of ex-ante quality equally in both types of competitions. Third, I exploit ventures in multiple

[8] This is akin to potential entrants paying to learn their type in Syverson (2004), if the payment were very low. Participating in a competition usually costs little more than the founder's time.

competitions to test for selection into feedback.

The first robustness test shows that the results persist in exact and propensity score matching estimators. The second measures the effect of feedback as the difference between ordinal and nominal scores, within the feedback competitions. The intuition is that two ventures in different competitions may have the same rank but different distances in score to the next highest rank. After accounting for the venture's quality in the eyes of the judges, I continue to find a strong effect of feedback. The third finds a similar result within a single competition that gave feedback in one year but not others. The fourth interacts feedback with competition characteristics likely associated with participant diversity, signal quality, and venture survival, as well as venture characteristics associated with ex-ante quality. These interactions do not affect the main finding. Finally, the results are robust to including polynomials in z-score and to estimation within relevant subsamples, such as student founders.

The main contribution of this paper is to reject the hypothesis that entrepreneurs' beliefs about their own types are static. The heterogeneity results, however, suggest that three mechanisms are at work. The first is that overconfidence varies across entrepreneurs. It is not surprising that women are more responsive, as being male is the characteristic most associated with overconfidence (e.g. Niederle & Vesterlund 2007). Entrepreneurs also may become more confident as the venture matures. A second explanation is that some ventures have higher real option values from delaying abandonment, as in Manso (2016).[9] An option's value increases in its uncertainty and in its asset specificity, or irreversibility of investment (Dixit & Pindyck 1994). The results can be interpreted to be consistent with both of these predictions.

Third, founders behave consistently with Bayesian updating, though I cannot reject other models. Over-precision and optimism biases should 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. The information seems to induce near-winners to continue as much or more than it encourages the poorest performers to exit. 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.[10]

[9] Also see Dillon & Stanton (2016), McGrath (1999), Hayward et al. (2006), Kerr, Nanda & Rhodes-Kropf (2014), and Stern (2006).

[10] Motivated by this evidence, the Appendix uses a Bayesian framework to formally model and calibrate the sensitivity to new information.

Significant social costs from this private feedback seem unlikely, as there are no highly successful outcomes among low-ranked uninformed ventures. While I cannot estimate long run returns to abandonment, I do not find that feedback reduces the likelihood of subsequent non-entrepreneurial leadership positions among founders who abandon their ventures.

Recent related work outside of firm settings has found that individuals can learn about their ability through performance (Seru, Shumway & Stoffman 2010, Hochberg, Ljungqvist & Vissing-Jørgensen 2013, Ganglmair, Simcoe & Tarantino 2016, Gross 2017, Xu 2017). A useful avenue for future research is whether entrepreneurs learn differently than other groups of people. My setting demonstrates learning from precise and unambiguous feedback. The nature of feedback likely affects its efficacy. For example, Odean (1999) suggests that noisy market signals explain why investors fail to learn how suboptimal their trading strategies are, and Hanna, Mullainathan & Schwartzstein (2014) show that limited attention prevents farmers from learning when there are many signal dimensions. Understanding which types of signals yield learning is a second promising avenue for future research.

Further related work studies the connection between executive characteristics and corporate decisions (Bertrand & Schoar 2003, Graham, Harvey & Puri 2013, Kaplan, Klebanov & Sorensen 2012); peer effects in entrepreneurship (Nanda & Sørensen 2010, Lerner & Malmendier 2013, Guiso, Pistaferri & Schivardi 2015); barriers to high-growth entrepreneurship (Hombert, Schoar, Sraer & Thesmar 2016, Howell 2017); and learning through knowledge spillovers (Belenzon & Schankerman 2013, Aghion & Jaravel 2015).

The paper proceeds as follows. I describe the data in Section 2. I show that rank predicts success in Section 3, and present the empirical approach in Section 4. The results are in Section 5. Section 6 offers several interpretations for the heterogeneity results.

2. Empirical context

This section first introduces new venture competitions and describes the ones I study. Then, in Section 2.2, I present summary statistics. I relate the startups and founders in my data to the U.S. startup ecosystem in Section 2.3.

2.1. *New venture competitions*

New venture competitions, sometimes called business plan or "pitch" competitions, have proliferated in the past decade. In a competition, new venture founders present their technologies and business models to a panel of judges. 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. Further, unlike many data sources commonly used to study entrepreneurship, such as the Survey of Consumer Finances or the Panel Study of Income Dynamics, local subsistence businesses do not appear.

This paper uses data from 87 competitions between 2007 and 2016.[11] Competitions consist of rounds (e.g. semifinals), and sometimes panels within round. The number of ventures in a preliminary (final) round averages 45 (19). There are 558 ventures that participate in multiple competitions. The mean award amount is $73,000. The data are summarized in Table 1, and the individual competitions are listed in Online Appendix (hereafter "A") Table A1.

All the competitions have the following features: (1) They include a pitch event, where the company presents its business plan; (2) Volunteer judges formally and privately rank participants, and ranks determine which ventures win; (3) Ranks and scores are secret, except when a feedback competition informs a venture of its rank; (4) The sponsoring organization does not take equity in any participating ventures; (5) The sponsoring organization explicitly seeks to enable winners to access subsequent external finance.

I select competitions for analysis that are otherwise similar but provided systematically different feedback. Competition organizers generally do not treat explicit feedback as a program goal. Instead, they are concerned with facilitating networking and identifying the

[11] 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.

"best" ventures as winners. However, 34 of the programs I study provide feedback through Valid Evaluation, a private company. Ventures received an email after the round that revealed their rank in the round overall and along specific dimensions (such as "Team" or "Technology"). These emails reflect Valid Evaluation's interest in formal feedback. My interviews with competition organizers indicated that they do not share this interest, and in fact sometimes discontinued use of Valid Evaluation in part because it seemed more concerned with feedback than with other elements the organizers valued more, such as the simplicity of the user interface. The remaining 53 no-feedback competitions used different software, and ventures received no feedback or unstructured feedback, such as anonymous comments.

Although each competition is unique, there are no systematic differences in the way judges scored or in the services provided (such as mentoring, networking, or training) across the two competition types. In no case did a competition with feedback advertise itself as providing relative ranks or more feedback in general, so ventures with greater informational needs could not have selected into these competitions (see Section 4 for a test). Judges were not informed that structured feedback would be provided, so there is no reason to believe judges would put greater effort into scoring in the feedback competitions.

Participants can always observe the number of judges. Judges score independently. In all competitions, pitches are five to 15 minutes (typically increasing by round), with an additional five to 15 minutes of questions from the judges, and between one and two hours of dedicated networking (e.g., post-competition reception). There is, therefore, some degree of informal, verbal feedback. To the best of my knowledge, it does not vary systematically across the two types of competitions. The competitions are usually open to the public, but typically there are few people besides the judges in the room, except in the final round.

The key econometric advantage is that I observe overall ranks in all cases, whereas the no-feedback competition participants do not observe their ranks. Further, I observe individual judges' scores and ranks, while in no case do founders observe individual judge scores or ranks. In most competitions, judges score or rank based on six dimensions (or "criteria"): Team, Financials, Business Model, Market Attractiveness, Technology/Product, and Presentation. These dimension scores or ranks are aggregated into a judge-specific venture score or rank. When scores are used, they are ordered to produce ranks. Judge ranks are then averaged to create an overall rank, which determines round winners.

Ventures never learn judge-specific scores or ranks. Judges cannot learn from the feedback, as they observe only their own scoring and identities of round winners, and never overall ranks of losers.[12] Only winning participants are typically listed on a program website, and my understanding is that judges and outside investors do not closely monitor competitions to identify losers. The feedback analysis focuses on losers, which comprise 75% of the data.

I use three transformations of the rank and score data. First, I use decile ranks calculated within losers and winners separately. That is, I divide losers in a round into ten groups, with the best ranks in group 1, and the worst in group 10. Second, I use judge decile ranks, calculated within ventures that the judge scored. Third, I use z-scores for the subset that begin with raw scores. The z-score indicates how far, in terms of standard deviations, a given absolute score falls relative to the sample mean. A higher z-score is better.[13]

2.2. *Data description*

The 4,328 unique ventures in the data are described in Table 1 panel 2. 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 is a binary indicator for the venture having at least one employee besides the founder as of August 2016. Among ventures that are abandoned, I defined time to abandonment as the number of days between the competition and the founder's next job start date.

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 ambiguous names and no clear LinkedIn match).[16] Elite degree status is

[12] 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).

[13] 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.

[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. For LinkedIn, I only use public profile data as a non-logged-in user, based on Google searches for person and school or firm.

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

tabulated using Table A2. Ventures and judges are assigned to 16 sectors (Table A3). They are sorted by state in Table A4. For ventures, sector assignations come from competition data, and each venture is assigned only one sector. Judge sectors are drawn from LinkedIn profiles or firm webpages, and judges may have expertise in multiple sectors.

Judges participate to source deals, clients, job opportunities, or as volunteer work. There are 2,514 unique judges, described in Table A3, of whom 27% are VCs, 20% are corporate 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.

2.3. *Data representativeness*

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. 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 close to the national average (40%) in deals and dollars. In part because VC investment in clean energy has declined dramatically in recent years (Saha & Muro 2017), as well as the presence of the Cleantech Open in my sample, the data is skewed towards clean energy. The competitions take place in 17 U.S. states. With the exception of Arizona, the top twenty states for venture location in my data almost entirely overlap with the top twenty states for VC investment. 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 comparable to the 5% found in Ewens & Townsend (2017)'s sample of AngelList startups. Each venture team averages three members. This is 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

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were coded by hand.

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.[17] 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 first-time, early stage startups and their founders in the U.S.

3. What predicts success?

This section first shows that the venture and founder characteristics that predict success are consistent with intuition (Section 3.1). Second, it demonstrates that the signal is informative (Section 3.2).

3.1. *Characteristics*

Associations between venture characteristics and success are consistent with common knowledge about high-growth startups. I regress two measures of success, subsequent angel/VC investment and having at least 10 employees as of August, 2016, on venture and founder characteristics. The results are in Table A6 panel 1. 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 sector and success are in Table A6 panel 2. Software and education ventures are more likely to succeed, while social enterprise and biotech ventures are less so. Media and entertainment ventures are far more likely to raise Angel/VC.[18]

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

[18] 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.

3.2. *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 ranks and z-scores are informative about subsequent venture outcomes, even among losers in no-feedback competitions.

I estimate variants of Equation 1, where the dependent variable Y_i^{Post} is a binary measure of venture success.

$$Y_i^{Post} = \alpha + \beta_1 WonRound_{i,j} + f\left(Rank/Zscore_{i,j}\right) + \beta_2 AwardAmt + \gamma' \mathbf{f.e.}_{j/k} + \delta' \mathbf{X}_i + \varepsilon_{i,j} \tag{1}$$

$WonRound_{i,j}$ indicates that the venture was a winner in the round. I include competition-round-panel or judge fixed effects.[19] The former absorb the date and location. Controls $\mathbf{X_i}$ include an indicator for whether the judge or judge's company ever invested in the venture, 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 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 and typically only score a subset of the participating ventures.

Among losers, rank and z-score robustly predict success, indicating that the competitions generate valuable signals. Estimates of Equation 1 are in Table 2, where the dependent variable is external financing.[20] For example, a one decile improvement in rank in column 1 of Table 2 increases the probability of subsequent external financing by 1.8 percentage points (pp), relative to a mean of 24%. The relationship persists with judge fixed effects (column 4), within the no-feedback competitions (columns 8-9), and for other outcomes, such as having at least 10 employees and exit through acquisition or IPO (Table A7).

Table A8 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.

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

[20] A logit specification in Table 3 column 3 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.

3.3. *Effect of winning*

A tangential benefit of this analysis is that it provides, to my knowledge, the first multi-competition program evaluation. Many new venture competitions are publicly funded, both in the U.S. and abroad.[21] 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.[22]

I find that winning is useful in a causal sense, using variants of Equation 1 as a regression discontinuity design. Appendix Figures A1 and A2 show success outcomes by percentile rank around the cutoff. Since there are fewer winners than losers, the data are noisier on the winning side. There appears to be a jump at the cutoff for having at least 10 employees. For external finance, the visual evidence is less compelling, because the jump seems to occur at the second decile away from the cutoff.

In Table 2 column 5, I limit the sample to quintiles around the cutoff, and find that winning increases a venture's chances of subsequent external finance by 10 pp. Broadly, Tables 2 and A7 find that winning increases a venture's chances of subsequent external finance by 7-12 pp (mean of 24%), of having at least 10 employees in 2016 by 7-12 pp (mean of 20%), and of experiencing an acquisition or IPO by 2 pp (mean of 3%). The effect remains highly significant within judge (Table 2 column 4).[23]

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 effect seems small in economic magnitude relative to the overall effect of winning and the predictive power of rank.[24] Among founders that abandon their ventures, I do not find an effect of winning on serial entrepreneurship.

My results indicate that competitions are useful not only because the winners ben-

[21] 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.

[22] For example, the White House "Startup America" initiative, launched in 2011, champions the public sponsorship of acceleration and competition programs. See https://www.whitehouse.gov/startup-america-fact-sheet

[23] 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.

[24] 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.

efit from certification and prize money, but also because they convene experts and entrepreneurs.[25] In Manso (2011)'s optimal contract, feedback should be timely and tolerant of failure. New venture competitions with feedback implement this guidance: While they reward top performers, they do not penalize especially poor performance.

4. Responsiveness to feedback: Estimation strategy

This section first proposes the main design for estimating the effect of feedback on venture continuation (Section 4.1). It then describes the challenge to causal identification and strategies to address it (Section 4.2).

4.1. Analytical approach

Having established that rank is an informative signal, the ideal experiment would 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 founders that receive especially negative feedback are more likely to abandon their ventures.

The empirical design is a difference-in-differences model among losers. Where a venture participated in multiple competitions, only the first instance is included. The first difference is between above- and below-median losers in a given competition ($Low\ Rank_{i,j}$). The second difference is across feedback and no-feedback competitions ($Feedback_j$).

$$\begin{aligned} Y_i^{Post} = \alpha + \beta_1 Low\ Rank_{i,j} \cdot Feedback_j + \beta_2 Low\ Rank_{i,j} \\ + \beta_3 Feedback_j + \gamma' \mathbf{f.e.}_{j'/k} + \delta' \mathbf{X}_i + \varepsilon_{i,j} \text{ if } i \in Losers_j \end{aligned} \tag{2}$$

Here, i indexes ventures, and j indexes competition-round-panels The dependent variable is continuation, measured as having at least one employee besides the founder as of August, 2016.

[25] 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.

4.2. *Identification challenge*

In Equation 2, above-median losers comprise the control group. Therefore, average differences across the types of competitions are differenced out. The primary concern with this approach is that the distribution of losers around the median may be systematically different in the two types of competitions, even though applicants did not know whether the competition would inform them of their rank in the round.

An especially problematic situation is if the mapping from quality to rank is systematically different. There are two main ways that this could lead to bias. First, suppose that ranks in the feedback competitions better correlate to true quality than ranks in the no-feedback competitions. Then feedback might be inherently correlated with continuation, leading low-ranked losers in the feedback competitions to abandon their ventures at higher rates than high-ranked losers, relative to the same difference in the no-feedback competitions, without any effect of information.

Second, suppose that in all competitions, true quality type maps monotonically to rank, but the feedback competitions have diverse participants while the no-feedback competitions have participants with similar quality. Again, we might observe more abandonment in response to a lower rank in the feedback competitions; participants with rationally higher priors in the no-feedback competitions will be more likely to continue with their ventures.

To address this concern, I use three tests and five robustness exercises. The three tests are: (1) Test for ex-ante differences in the distributions of observables across the two types of competitions; (2) Test whether rank reflects measures of ex-ante quality equally in both types of competitions; (3) Exploit ventures in multiple competitions to test for selection into feedback.

The five robustness exercises are: (1) Use matching estimators in lieu of the difference-in-differences strategy in which participants are matched on characteristics likely to predict survival; (2) Measure the effect of feedback as the difference between ordinal and nominal scores; (3) Interact feedback with competition characteristics likely associated with participant diversity, signal quality, and venture survival, as well as venture characteristics associated with ex-ante quality; (4) Estimate the effect of feedback within a single competition that gave feedback in one year but not others; (5) Include polynomials in z-score, and ensure that the results persist within relevant subsamples.

The following subsections describe the three tests. I explain the five robustness tests together with their results in Section 5.

4.2.1. *Tests for ex-ante distributional differences*

Two types of visual evidence and a formal test find that the distributions of observable characteristics are similar across the two types of competitions. While the levels of observables are not always similar, the demeaned distributions are never measurably different.

First, I show the probability of three characteristics that I expect to predict survival as a function of decile rank in Figure 1: whether the founder attended a top 10 college, whether the venture was incorporated at the time of the round, and whether the venture received external financing before the round. All limit the sample to losers. There are no obvious differences around the medians between feedback and no-feedback competitions. However, there are level differences. For example, ventures are more likely to be incorporated in the feedback competitions. This is largely due to the difference between the Arizona Innovation Challenge, a large feedback competition that caters to more advanced ventures, and the HBS New Venture challenge, a large no-feedback competition whose participants are typically teams of students deciding whether to enter entrepreneurship. I match on incorporation below, in case it makes rank a more informative signal of quality.

Second, I present histograms of the distributions, and find no obvious differences in skewness or kurtosis across the two types of competitions.[26] Appendix Figures A3 and A4 contain spikes representing the fraction of ventures within narrow z-score bandwidths for observables in feedback and no-feedback competitions.[27] Appendix Figure A3 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. Appendix Figure A4 shows founder characteristics, including whether the founder is a student at the time of the round, ever received an MBA, attended a top-20

[26]Greater skewness means that the data are more concentrated on one side of the distribution, and greater kurtosis (or peakedness) means that the data are more concentrated around the middle, as opposed to being more spread out (fatter-tailed).

[27]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: $\frac{\sum_{z,SF} Inc_i}{\sum_{SF} Inc_i}$.

college, and is of above median age (in years). The distributions are not the same, but 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, and subtract the below median mean from the above median mean. Then I conduct a t-test across rounds with and without feedback. Among the nine observables at the time of the round considered in Table 4, 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 pp more likely than below median losers to be in a hub state, while this difference is -1 pp for feedback competitions. Any bias should act against my main result, since ventures in hub states are unconditionally more likely to succeed (Table A6). Note a Kolmogorov-Smirnov test for equality of distribution functions is not appropriate here, as it tests for stochastic dominance rather than differences in shape.

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

4.2.2. *Rank reflects quality consistently*

I next test whether rank reflects measures of quality observable at the time of the competition. In Table A9, I regress whether the founder attended a top 10 college, whether the venture was incorporated at the time of the round, and whether the venture received external financing before the round on *Low Rank*, within the sample of losers.

The sample is restricted to the no-feedback competitions in columns 1, 3, and 5. These regressions find strong, negative, and statistically significant coefficients on *Low Rank*. I include all competitions and interact *Low Rank* with *Feedback* in columns 2, 4, and 6. The coefficients on the interaction term are uniformly zero. These regressions are within round, so the independent effect of feedback is absorbed. This exercise demonstrates that the mapping between observable quality and rank is not different across the two types of competitions.

4.2.3. *Selection into feedback*

There may be concern that founders with more uncertainty about their project quality select into feedback competitions, even though competitions did not advertise this feedback explicitly. I test for such selection using ventures that participated in multiple competitions: Among founders that compete in a second competition, I expect high information need founders to disproportionately sort into feedback competitions.

To proxy for information need, I use a low average score or a highly dispersed score in the first competition. Table A10 panel 1 contains summary statistics for the sample used in the test. Panel 2 shows 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. None are significant. It is therefore unlikely that founder selection into competition type is affected by information needs.

5. **Responsiveness to feedback: Results**

Section 5.1 delivers the main findings, including the timing of abandonment and effects on other outcomes. Section 5.2 contains five robustness tests. Section 5.3 explores whether learning appears efficient. Section 5.4 describes heterogeneity in the main finding.

5.1. *Main results*

This section shows that entrepreneurs who receive especially negative feedback about their ventures are more likely to abandon them. The raw effect is in Figure 2. Rank and score are far more predictive of continuation in the feedback competitions. The higher average probability in feedback competitions reflects two things. First, feedback induces highly ranked losers to continue. Second, ventures are more mature on average in the feedback competitions.

Equation 2 is estimated in Table 5. The main specification in panel 1 column 1 finds that negative feedback reduces the likelihood of continuation by 8.6 pp, relative to a mean of 34%. This translates to a 13% increase in the probability of failure.[28] Summing the three

[28] The coefficient on *Low Rank* $\cdot$ *Feedback* (-.086) is relative to above median losers in no-feedback competitions. The coefficient on *Low Rank* is -.062, implying that in no-feedback competitions low-ranked losers

coefficients gives a mean effect of $Low\ Rank \cdot Feedback$ of -.084. The effect is roughly linear. Table 5 panel 2 columns 3-5 use alternative definitions of "low rank." For example, 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. The effect is slightly larger at the higher end of the loser distribution. This suggests that feedback induces near-winners to "stick with it" as much as or more than it encourages the poorest performers to exit. Also, the effect is weakly symmetrical among round winners that did not ultimately win the overall competition. The results, in Table A11, are much noisier than among losers. This may reflect the smaller sample.

I next examine the timeliness of abandonment. I measure time-to-fail as the number of days between the competition's end date and the founder's first subsequent new job start date, among founders of abandoned ventures. This permits using an indicator for abandoning within a certain time frame as a dependent variable. When the dependent variable is abandoning within 6 months, the coefficient on the interaction between feedback and low rank is 8 pp, relative to a mean of 51%; this increases to 8.7 (8.9) pp within 1 (2) year(s), relative to means of 57% (64%) (Table 5 panel 2 columns 6-8). The sample mean is 66%, so the effect is almost entirely concentrated in the first two years. Founders abandoning bad ideas quickly drive long term responsiveness.

An alternative to abandonment is that informed highly ranked losers are better able to raise financing than their uninformed counterparts. Perhaps they inform prospective funders of their relatively high ranking. However, Table A12 shows that negative feedback has no effect on subsequent external financing or angel/VC Series A investment specifically. The large effect of subtle, low-stakes feedback on survival shows that entrepreneurs can learn about their types, rejecting the hypothesis that entrepreneurs are characterized by extreme overconfidence.

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are 6.2 pp less likely to continue than high ranked losers. The coefficient on feedback is 0.066, as there is a higher probability of survival in feedback competitions.

5.2. *Robustness tests*

5.2.1. *Matching estimators*

I also use exact and propensity score matching estimators. These adjust for "missing" potential outcomes by matching subjects in a treatment group to their closest counterparts in the untreated group. The difference between observed and predicted outcomes is the average treatment effect. I compare continuation for these matched groups to the above-median matched group.

The first 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 13 sectors, competition year, student status, and company incorporation status. I conduct balance tests of variables not used in matching in Table A13. 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 the probability of treatment using a logit model. It then identifies, for each treated participant, the untreated participant with the closest probability of treatment.[29] Table A14 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.

Results using the matched samples are in Table 5 panel 1 columns 6 and 7. Exact matching yields nearly the full sample result, at 7.6 pp, significant at the 1% level. The effect falls somewhat in the propensity-score matching, to 5.6 pp, significant at the 5% level.

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

5.2.2. *Exploiting nominal scores*

In all but two of the competitions, the conference organizers arrive at ordinal scores (ranks) by ordering nominal scores. These nominal scores are never revealed to ventures. I exploit them to better approximate the random allocation of feedback. To illustrate the approach, consider a pair of ventures with ranks five and six, and a second pair in a different round that also has ranks five and six. Now suppose that the first pair had very similar scores, while the second pair had more distant scores. As perceived by the judges, the quality difference of the second pair is larger than that of the first pair. If all four ventures are informed of their rank, their feedback is the same but their quality is different. The venture ranked sixth in the second pair got randomly higher feedback relative to its true quality.

If scores measure latent quality, then residual variation in rank reflects noise in transforming nominal scores to forced ranks. I estimate both by including score in Equation 2, and using Equation 3.

$$\begin{aligned} Y_i^{Post} &= \alpha + \beta_1 Rank_{i,j} + \beta_2 Score_{i,j} + \gamma' \mathbf{f.e.}_{j'/k} + \delta' \mathbf{X}_i + \varepsilon_{i,j} \\ &\text{if } i \in Feedback_j \end{aligned} \tag{3}$$

The results are in Table 5 panel 1 columns 8-10. Column 8 confirms that score strongly predicts survival. Column 9 replicates the main specification with a control for score. The effect of $Low\ Rank \cdot Feedback$ strengthens somewhat, to 9.5 pp. Column 10 estimates Equation 3, and finds that increasing a venture's rank by one decile reduces the probability of abandonment by 1.3 pp. This is strong evidence that ex-ante quality distributional differences do not explain the main result.

5.2.3. *Interacting feedback with competition and ex-ante quality characteristics*

There is a risk that the distribution of participants is correlated with feedback. Feedback could be more informative or impactful in competitions with feedback if ventures in those competitions have inherently more precise signals. For example, the organizers who chose to inform participants of their ranks may have been savvier.

I add interactions between feedback and competition characteristics likely associated with signal quality, venture survival, and participant diversity to Equation 2. Competition signal quality proxies are whether the competition is at a university, the number of ventures, the number of judges, and the location.[30] For likelihood of venture survival, I use the share of founders that attended a top 10 college, the share of incorporated ventures, and the share of ventures that previously received external financing. Competition diversity might affect the slope in rank. I proxy for it with the number of venture sectors (out of a total possible 16 sectors), the share of ventures that are software-based, and the share of ventures that are clean energy based. The results from interacting feedback with competition characteristics (always controlling for the characteristic itself) are in Table A15 panels 1-3. The effect of $Low\ Rank \cdot Feedback$ persists, and even grows somewhat larger (about 9 pp).

I conduct a similar exercise at the venture level. I interact feedback with whether the venture was incorporated at the time of the round, whether it had previous external financing, whether the founder attended a top 10 college, whether the founder has a PhD from a top 20 university, and whether the founder is a student at the time of the competition. Panel 4 shows interactions with venture characteristics associated with ex-ante quality. The effect of feedback is somewhat attenuated, to 6.7 pp, significant at the 10% level. As expected, being incorporated, receiving prior financing, and attending a top 10 college are all strong predictors of survival. In sum, the general robustness to these approaches indicates that distributional differences across the two types of competitions are unlikely to drive the main effect.

5.2.4. *Effect of feedback within a single competition*

A single program in my data, the Cleantech Open (CTO), gave feedback in 2011 but in no other year. As the CTO did not otherwise change in 2011, there is no reason that the distribution of quality among losers was different in 2011. Although the sample is much smaller, comparing the effect of having a low rank in 2011 relative to other years provides a useful robustness test.

The results are in Table A16. I limit the sample to 2010-12, and also estimate the effect using all years for which I have CTO data (2008-14). Negative feedback reduces the probability of survival by 11-13 pp in 2011 relative to the surrounding years. This is quite similar to the main specification.

[30] For location, I use indicators for the nine U.S. Census divisions.

5.2.5. *Functional form and subsamples*

Two tests account for potential non-linearities. In Table 5 in panel 1 column 4, I control for the first and second moment in z-score. In column 5, I use a logit specification. The main effect is robust to both approaches, though significance declines to the 5% level.

A final set of tests ensures that the results are robust to subsamples. Table 5 panel 2 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 the population of founders with MBAs, among ventures from VC hub states, and among student-led ventures (Table A17).

5.3. *Is learning efficient?*

Entrepreneurs face uncertainty about the quality of their ventures. I cannot assess the welfare impacts of feedback, but it is plausible that private, costless, informative signals at an early stage might enable poor quality startups to fail faster, making innovation more efficient. The main result implies that had the 1,603 unique below-median losers in the no-feedback competitions received feedback, an additional 137 would have been abandoned, beyond the 1,186 that were abandoned.[31] While I cannot affirmatively test whether learning is efficient, I can examine three obvious ways that learning might *not* be efficient.

First, inducing abandonment could be socially costly if a few highly successful outcomes are foregone. Among below-median firms in the feedback competitions, 2.1% were acquired, compared to 3.2% of firms in the no-feedback competitions. All appear to be minor acquisitions, as valuation data is in no case available. There were no IPOs in either group. Thus if there is a cost in right-tail outcomes, it seems small.

Second, learning may be privately inefficient if abandoning after negative feedback leads to poorer long run labor market performance. Lacking earnings data, I create an indicator for whether the latest job title of founders who abandoned their ventures implies a leadership role.[32] Founders have a revealed taste for leadership, so leadership in other domains is a reasonable proxy for non-entrepreneurial success. In unreported regressions, I find no evidence that receiving feedback in general, or negative feedback in particular, has

[31] Based on the primary specification, in which feedback increases abandonment by about 13% of the mean, which is 66% $(0.13 \cdot (0.66 \cdot 1603)$.

[32] Indicator for the title containing any of the following words: CEO, CFO, CTO, Chief, Managing Director, Manager, Senior, President, Partner, Director.

any relation to subsequent non-entrepreneurial leadership among founders that abandoned their ventures. Therefore, while I cannot argue that feedback leads abandoners to do better, I can posit that it does not cost them leadership positions.

Third, even if learning is on average efficient, there may be many cases in which ventures are randomly assigned especially lenient or harsh judges, leading to inaccurate signals. I look for such "noisy" learning using a version of the leave-one-out judge leniency in Dobbie & Song (2015). Let S_{ij} be an indicator for the highest score a venture received across judges. Let j denote a judge, and let n_j be the count of ventures that the judge scored. The leave-one-out leniency measure at the venture-judge pair level is then $L_{ij} = \frac{1}{n_j-1}\left(\sum_{k=1}^{j} S_k - S_i\right)$. For a venture i, it is the number of times one of its judges gave a high score to other ventures, divided by the number of other ventures the judge scored. L_{ij} is summarized in Table A3 panel 3. In Table A18, I show that leniency predicts scores (columns 1-2), but that there is no effect of leniency on responsiveness (column 4). Lenient judges do not have enough of an effect on a venture's overall rank to affect the abandonment decision.

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

5.4. *Heterogeneity*

This section assesses heterogeneity by adding a binary characteristic as a third interaction in Equation 2. The results are in Table 6.[33] I describe how the main effect varies with founder and venture characteristics in Sections 5.4.1 and 5.4.2, respectively. Section 5.4.3 examines variation in the feedback itself.

5.4.1. *Founder characteristics*

I first partition the sample on gender. Women comprise 21% of founders. Table 6 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. This translates to a 24% increase

[33]For brevity, panel 2 does not report control coefficients. Some of the characteristics are correlated with each other; a full correlation table is in Table A19.

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. Despite this large magnitude difference, the triple interaction (column 7) is not statistically significant.

I find that founders with elite college degrees are less responsive to feedback (Table 6 panel 2 columns 13-16). Conversely, having an MBA makes founders somewhat more responsive (Table 6 panel 2 columns 17-18). I find no heterogeneity along a number of other founder dimensions, including having other degrees (e.g., PhD), and being a student at the time of the competition. The founder's age and whether he founded a prior venture also have no relationship to responsiveness.

Serial entrepreneurs might tend to be especially persistent in the face of negative feedback, or they might be especially willing to rapidly abandon ventures when they get bad news. Serial entrepreneurship is common in my sample; among founders that abandoned their ventures, 39% founded or were a senior executive of a subsequent venture. Within the pool of abandoned ventures, serial entrepreneurship is weakly 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 Table A20. Having a top 10 MBA is associated with dramatically faster time to abandonment (138 days). Older founders and founders with PhDs have longer times to abandon. I ask whether greater responsiveness, measured as quickly abandoning after negative feedback associated with more serial entrepreneurship in Table A21. The "abandoned fast" variable is 1 if the abandonment time is below the median (148 days). While fast abandonment is correlated 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).

5.4.2. Venture characteristics

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% (Table 6 panel 1 columns 1-2). Similarly, unincorporated ventures are 11 pp more responsive, relative to a mean of 44% (Table 6 panel 2 columns 1-2). Ventures with above median age (0.8 years) are no more or less responsive (Table 6 panel 2).

Software-based ventures are also somewhat more responsive than hardware-based ventures (Table 6 Panel 1 columns 3-4). 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.

One measure of venture risk is disagreement among judges.[34] 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.[35] The triple interaction has a positive effect (Table 6 panel 2 columns 7-8); when judges disagree, founders are less sensitive to their overall rank. Recall that founders do not observe individual judge ranks, but they do know how many judges there are. When there are more judges, the standard deviation is measured with greater accuracy, but it does not get smaller in expectation.

There are two alternative stories. First, more overconfident founders may choose riskier business models, as has been found among CEOs in Hirshleifer, Low & Teoh (2012) and Graham, Harvey & Puri (2013). Second, this finding could reflect signal precision if founders learn from verbal interactions with judges that they lacked consensus.[36] To test the second possibility, I instrument for standard deviation using the judge leniency measure described above (L_{ij}). 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 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 A3 panel 3. When variation in leniency is high, the venture randomly receives a particularly noisy signal.[37]

Table A23 shows that variation in leniency predicts the standard deviation of judge scores quite well. The F-statistics in first-stage regressions range from 14 to 31. In a naive instrumentation approach, I replace the standard deviation with the leave-one-out variation measures.[38] Columns 5-6 show no effect of the triple interaction between having a low rank,

[34] Table A22 suggests that judge disagreement (after controlling for rank and winning) predicts angel/VC series A financing, consistent with these types of investors targeting risky ventures.

[35] Ventures are unaware of judge agreement; they receive only their aggregated rank in the feedback competitions.

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

[37] 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.

[38] Given the small sample and need for many instruments in the interacted regression, a two-stage-least-

receiving feedback, and having judges with high expected variation in leniency. This is evidence that the result in Table 6 reflects venture risk, not signal precision.

5.4.3. *Nature of feedback*

One 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 6 panel 2 columns 9-10).[39]

A second source of variation in signal precision is judge expertise, though this is challenging to measure. 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. I use judge sector (based on LinkedIn profiles and firm webpages) and occupation (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 21-30 of Table 6 panel 2. The binary characteristic C_i is one if the share of a venture's judges in a certain category (say, VCs) 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.

In contrast, I find that ventures are much more responsive when they face an above median number of corporate executive judges (column 18). Directionally, I also find more responsiveness to founder/entrepreneur judges (column 25, p-value of 0.12). Founders may assign business acumen to these judges if they perceive them as role models, or if they associate the judge's company name with activity relevant to their venture. Ascertaining selection skill among investors may be more challenging. Indeed, it is difficult even with

squares approach here is infeasible, as I would need a separate instrument for each interacted variable, which is unavailable.

[39]Precision might also be higher when there are more ventures in a round, but I do not find that responsiveness varies significantly with the number of participants.

large amounts of data, as Sørensen (2007) points out.

I next explore whether responsiveness varies at the criterion, or dimension score level. The unconditional association between dimension ranks and outcomes, controlling for win status, is in Table 7. For all outcomes other than IPO/acquisition, a higher team rank is the strongest predictor of subsequent success. Relatedly, Bernstein, Korteweg & Laws (2015) and Gompers, Gornall, Kaplan & Strebulaev (2016) find that early stage investors care most about information regarding founder team quality. More generally, Bloom et al. (2013) find a positive correlation between good managerial practices and productivity in large firms.

I find that founders are more responsive to negative feedback along certain dimensions. The variable *Low Rank* is now an indicator for being a below-median loser within a specific dimension. The results, in Table 8, reveal that negative feedback impacts continuation most along the financials, business model, market, and team dimensions. There is no effect for product/technology or presentation.

6. Mechanism exploration

The main contribution of this paper is to reject the hypothesis that entrepreneurs' beliefs about their own types are static. The heterogeneity results, however, suggest that three mechanisms are at work. The first is that overconfidence varies across entrepreneurs; this is discussed in Section 6.1. The second is that some ventures may have higher real option value from delaying abandonment (Section 6.2). Finally, a natural framework for interpreting responsiveness to feedback is Bayesian updating. In Section 6.3, I examine whether founders behave consistently with Bayesian updating, though the evidence does not permit rejecting other models.

6.1. *Overconfidence*

Three of the heterogeneity results may pertain to overconfidence. First, being male is the characteristic most robustly associated with overconfidence, in the sense of both excessive optimism and an excessively precise prior (e.g. Barber & Odean 2001, Beyer & Bowden 1997). Roberts & Nolen-Hoeksema (1989) show that women are more responsive to negative feedback than men. Women also have less confidence in their entrepreneurial abilities (Kirkwood 2009, Koellinger et al. 2008). Niederle & Vesterlund (2007) demonstrate that men are more interested in competing than women. In light of this literature, my finding that women are more responsive is consistent with less confidence in this group.

Second, unincorporated ventures are less responsive. This could imply that confidence increases as the founder grows more attached to his venture, and thus that learning about type is most important before firm boundaries form. Third, founders that graduated from an elite college may be more confident, though they have higher average chances of success. In certain leadership contexts, failing to learn may be optimal, as in Bernardo & Welch (2001) and Goel & Thakor (2008). 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 have less openness to criticism and feedback. These points apply best in my context to elite college graduates.[40]

6.2. *The venture as a real option*

Varying responsiveness could also reflect founders treating 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).[41] I found that riskier ventures were less responsive, consistent with the first prediction. Hardware ventures have more investment irreversibility, so a real options framework may help explain why they are less responsive.

Ventures that have already incorporated or prior external financing are also less responsive and likely have more sunk costs, which implies greater investment irreversibility. However, these characteristics should be associated with lower uncertainty and more private information. I found that older ventures and non-student founders are not more or less responsive than their counterparts. These groups may have more information and less

[40]The result on signal precision, in which ventures are more responsive when there are more judges, can also be interpreted through the lens of the self-attribution bias. This predicts that noisier signals generate non-linearity in responsiveness. People with this bias interpret bad outcomes as signals of bad luck, and good outcomes as signals of skill, as Gervais & Odean (2001) find among stock traders. If the self-attribution bias is present, I expect that noise will not affect responsiveness among winners Table A11 column 5 repeats the signal analysis from Table 6, but for positive feedback. It finds a negative and insignificant coefficient on the triple interaction, consistent with the self-attribution bias. However, this regression is quite noisy, and the effect on positive feedback in general is also insignificant.

[41] 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).

uncertainty, but may not have generated more specific assets. Future research might test the hypothesis that the formal milestones of incorporation and external investment indicate irreversible assets, while age primarily reduces uncertainty.

Founders with top college degrees are less responsive. Venture resemblance to a call option should 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, as in Vereshchagina & Hopenhayn (2009). If elite school founders have more personal wealth, they may behave more consistently with a real options approach and be less responsive. This interpretation offers an empirical counterpart to the model in Grenadier & Malenko (2010), which combines a real options framework with Bayesian updating so that firms can learn about their own type.

6.3. *Bayesian updating*

Bayes' rule dictates how rational agents update their beliefs.[42] Bayesian updaters should dismiss imprecise signals, as the founders in my data do. However, dismissing imprecise signals is consistent with many models of learning and with cognitive biases. I also expect Bayesians to be less impacted by feedback as they learn about their quality. Consistent with this, ventures that have received external financing are less responsive.

Bayesians should update less when they have more information about their own type. 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. The short pitch duration and judge backgrounds suggest that information asymmetry will tilt in the judges' favor more on business viability (e.g. market demand) than on technology viability. I found no effect of negative feedback for product/technology scores.[43] 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.

Non-linearity in the effect could be consistent with cognitive biases, because rank

[42] Given a prior belief and a new signal, the posterior belief of the Bayesian updater is a precision-weighted average of the two. More signals increase the weight on the average signal, as do more precise signals and noisier priors.

[43] 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.

predicts success in a linear way. Excessively elevated or precise priors should prevent founders from updating downward enough when they receive a middling rank among losers. Instead, the effect is roughly linear, and persists, if weakly, among winners. In sum, my findings reject extreme miscalibration or optimism, and are consistent with founders being Bayesians, though again this does not rule out other models.

Appendix Section 1 presents a simple model of how a Bayesian updater responds to feedback. I assume the founder interprets his rank as the result of a series of Bernouilli trials, where the number of signals is the number of judges. This permits using a Beta distribution as the conjugate prior, and hewing closely to the information structure and main results from the preceding sections. I calibrate the model to show how feedback affects a founder's success probability distribution. Appendix Figure A5 shows the results of the calibration exercise; and Appendix Figure A6 depicts how more judges affect the posterior by improving signal precision.

7. Conclusion

This paper shows that entrepreneurs are quite responsive on average, rejecting the hypothesis that they have static types. Arrow (1962) closes by noting that

> "It has been assumed here that learning takes place only as a by-product of ordinary production. In fact, society has created institutions, education and research, whose purpose it is to enable learning to take place more rapidly. A fuller model would take account of these as additional variables."

This paper shows that new venture competitions can enable firms to learn before they produce. Under conditions in which it is not socially costly to deter low quality startups, my data suggest that giving entrepreneurs private, expert feedback may improve resource allocation and the efficiency of innovation.

The substantial heterogeneity raises questions about how learning and overconfidence interact with innovation. I find that risky ventures and those with elite degree founders are less responsive to negative feedback. This hints that even as most entrants are rational and responsive to new information, a small subset may have ambitious, radical ideas and also may be imperviousness to negative feedback. Ventures in this subset may be the ones with the potential to transform industries, and the overconfidence of their founders may be crucial to coordinating other stakeholders. Theoretical models of industry dynamics could micro-found technological discontinuities in the small fraction of entrepreneurs that enter without regard to signals about expected cash flows. A promising avenue for future research is whether innovative, risky new firms tend to have founders who ignore negative feedback.

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Table 1: Summary Statistics

Panel 1: Competitions

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

Panel 2: Ventures[*]

	N	Mean	Median	S.d.	Min	Max
# unique ventures	4,328					
# unique ventures in feedback competitions	1,614					
Venture age at first competition (years)	2073	1.9	0.77	3	0	20
Incorporated at round	4328	0.44	0	0.5	0	1
In hub state (CA, NY, MA)	4,328	.35	0	.48	0	1
Survival (Has ≥ 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
Abandoned within 6 months[†]	3228	0.51	1	0.5	0	1
Abandoned within 1 year	3228	0.57	1	0.5	0	1
Abandoned within 2 years	3228	0.64	1	0.48	0	1
Has ≥ 3 employees as of 8/2016	4328	0.3	0	0.46	0	1
Has ≥ 10 employees as of 8/2016	4328	0.2	0	0.4	0	1
Raised external private investment before round	7099	0.16	0	0.36	0	1
External private investment after round	7099	0.24	0	0.43	0	1
Angel/VC series A investment before round	7099	0.09	0	0.29	0	1
Angel/VC series A investment after round	7099	0.15	0	0.36	0	1
Acquired/IPOd as of 9/2016	4328	0.03	0	0.18	0	1
Ventures in multiple competitions (#$\|>1$)	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

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
Is student at round	2554	0.2	0	0.4	0	1
Graduated from top 20 college	2554	0.27	0	0.44	0	1
Graduated from top 10 college	2554	0.18	0	0.39	0	1
Degree from Harvard, Stanford, MIT	2554	0.1	0	0.3	0	1
Has MBA	2554	0.48	0	0.5	0	1
Has MBA from top 10 business school	2554	0.33	0	0.47	0	1
Has Master's degree	2554	0.17	0	0.37	0	1
Has PhD	2554	0.13	0	0.34	0	1
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). †1 if the number of days between the competition's end date and the first subsequent new job start date for the founder is less than 180, among ventures that did not survive and where the founder was matched to a LinkedIn profile. ‡From LinkedIn profiles. Not all competitions retained founder data, so the number of venture leaders is less than the number of ventures. ±Gender coding by algorithm and manually; sexes do not sum to one because some names are both ambiguous and had no clear LinkedIn match. **This is the number of days between the competition's end date and the first subsequent new job start date, among ventures that did not survive.

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

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 winners						.027			.0064
						(.019)			(.023)
Z-score 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 external financing after the competition. 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.

Table 3: Round-level test for distributional differences around median among losers

	Feedback			No Feedback				
	N	Mean	S.d.	N	Mean	S.d.	Difference	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	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

Panel 1

Dependent variable: Survival*

				Z-scores	Logit	Exact matching±	Propensity score matching**	Nominal score		Nominal score: Feedback only
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Low rank·Feedback	-.086**	-.084***	-.079***	-.086**	-.32**	-.076***	-.056**		-.093**	
	(.036)	(.02)	(.026)	(.036)	(.16)	(.027)	(.022)		(.04)	
Low rank	-.062***	-.051***	-.026	-.065***	-.31**				-.047*	
	(.021)	(.014)	(.022)	(.021)	(.16)				(.026)	
Z-score				.04						
				(.029)						
Z-score^2				-.013**						
				(.0067)						
Feedback	.066*	.17*	-.03	.07*	.23				.082	
	(.04)	(.092)	(.14)	(.039)	(.17)				(.05)	
Nominal score								.0052**	.0027	.073***
								(.0024)	(.0022)	(.02)
Decile rank										-.014*
										(.0073)
Venture controls†	Y	Y	Y+‡	Y	Y	-	Y	Y	Y	Y
Year f.e.	Y	N	N	N	Y	-	Y	Y	Y	Y
Judge f.e.	N	Y	Y	N	N	N	N	N	N	N
N	3751	26443	14915	3751	3751	2484	3357	3305	2974	2028
R^2	.082	.18	.29	.084	0.065	-	.095	.071	.086	.085

Note: This table shows estimates of the effect of negative feedback: a below-median rank among losers when losers learn their ranks, relative to competitions where they do not learn their ranks. "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. Sample restricted to losers of round. All rounds included post-2006, except in column 11, where only feedback competitions are included. * 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.

Panel 2

Sample restricted to losers of round

Dependent variable:	Survival*					Abandoned within...		
						6 months	1 year	2 years
	Preliminary rounds only		Low rank among losers defined as: Bottom 3 deciles	Bottom 7 deciles	Deciles 5-8 (9-10 omitted)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Low rank·Feedback	-.12***	-.1***	-.062**	-.097**	-.079*	.079*	.085**	.087**
	(.044)	(.023)	(.029)	(.04)	(.046)	(.041)	(.041)	(.039)
Low rank	-.051**	-.043***	-.065***	-.048**	-.025	.056***	.06***	.058***
	(.023)	(.016)	(.019)	(.022)	(.025)	(.021)	(.022)	(.022)
Feedback	.11**	.17	.032	.073*	.075*	-.0074	-.031	-.056
	(.045)	(.15)	(.028)	(.043)	(.043)	(.042)	(.042)	(.04)
Venture controls†	Y	Y	Y	Y	Y	Y	Y	Y
Year f.e.	Y	N	Y	Y	Y	Y	Y	Y
Judge f.e.	N	Y	N	N	N	N	N	N
N	2689	17388	3751	3751	2372	3751	3751	3751
R^2	.083	.14	.081	.081	.097	.061	.06	.073

Note: This table shows estimates of the effect of negative feedback as in Table 5, but with alternative samples. *Survival is 1 if the venture had ≥ 1 employee besides founder on LinkedIn as of 8/2016. Columns 1-2 use only preliminary rounds (no final rounds). Columns 3-5 redefine "low rank" as being either in the bottom 30%, 70%, or between the 50th and 80th percentile. The bottom two deciles are omitted in column 5. The dependent variable in columns 6-8 is 1 if the venture was abandoned within the given time frame after the competition (see Table 1 for definition). †Includes sector indicator variables, student status and company incorporation statuses. *** indicates p-value<.01.

Table 6: Heterogeneity in Effect of Negative Feedback

Sample restricted to losers of round

Panel 1

Dependent variable: Survival*

Characteristic C_i:	Financing before round		Tech type IT/software		Social/ clean tech		Founder female	Sample: Women	Sample: Men
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Low rank·Feedback· C_i	.15*	.15*	-.1*	-.097	.072	.085	-.1		
	(.087)	(.088)	(.062)	(.063)	(.088)	(.095)	(.096)		
Low rank·Feedback	-.1**	-.1**	-.015	-.016	-.1**	-.1**	-.093**	-.18*	-.07*
	(.041)	(.042)	(.043)	(.043)	(.042)	(.042)	(.039)	(.092)	(.04)
Feedback· C_i	-.19***	-.19***	-.00096	-.016	-.089	-.08	.12		
	(.067)	(.066)	(.058)	(.057)	(.08)	(.087)	(.073)		
Low rank· C_i	-.033	-.052	-.0035	-.0021	.028	.036	.071		
	(.069)	(.069)	(.038)	(.039)	(.047)	(.047)	(.045)		
Low rank	-.047**	-.031	-.038*	-.04*	-.051**	-.052**	-.079***	-.0056	-.079***
	(.022)	(.02)	(.023)	(.023)	(.023)	(.023)	(.025)	(.039)	(.025)
Feedback	.11**		.057		.1**		.059	.2**	.14***
	(.042)		(.038)		(.041)		(.045)	(.086)	(.04)
C_i	.37***	.39***	.09**	.11***	-.098**	-.13***	-.11***		
	(.054)	(.053)	(.037)	(.038)	(.042)	(.042)	(.039)		
Year f.e.	Y	Y	Y	Y	Y	Y	Y	Y	Y
Sector f.e.	Y	N	N	N	N	N	Y	Y	Y
Competition f.e.	N	Y	N	Y	N	Y	N	N	N
N	3765	4136	4136	4136	4136	4136	3048	577	3188
R^2	.13	.13	.12	.14	.077	.092	.1	.17	.054

Panel 2 (control coefficients not reported)

Dependent variable: Survival*										
C_i:	Incorp. at round		Venture age > median		Founder age > median		Judge rank s.d. > median†		# judges > median	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Low rank·Feedback· C_i	.11***	.13*	.026	.034	-.11	-.094	.12***	.1**	-.29***	-.31***
	(.033)	(.069)	(.067)	(.064)	(.089)	(.093)	(.044)	(.046)	(.11)	(.1)
N	3765	4136	2119	2224	1594	1778	3765	4136	3765	4136
R^2	.084	.086	.082	.1	.1	.1	.086	.088	.088	.087
C_i:	Founder is student		Founder top 10 college		Founder Harvard/ Stanford/MIT		Founder has MBA		Founder had prior venture	
	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
Low rank·Feedback· C_i	-.02	-.087	.24**	.19	.31**	.26*	.11*	.076	.063	.066
	(.089)	(.094)	(.11)	(.12)	(.14)	(.16)	(.066)	(.063)	(.073)	(.076)
N	3765	4136	3765	4136	3765	4136	3765	4136	4136	4136
R^2	.083	.086	.087	.088	.085	.086	.085	.086	.077	.091
C_i: >med share of judges are‡:	VC		Corporate executive		Founder/ Entrepren.		Lawyer/ Cons./ Acct		Expert in venture sector	
	(21)	(22)	(23)	(24)	(25)	(26)	(27)	(28)	(29)	(30)
Low rank·Feedback· C_i	.088	.083	-.14**	-.14**	-.11	-.061	-.099	-.11	.038	.064
	(.071)	(.069)	(.061)	(.061)	(.074)	(.077)	(.064)	(.065)	(.056)	(.057)
N	3765	4136	3765	4136	3765	4136	3765	4136	3765	4136
R^2	.085	.088	.086	.088	.084	.086	.084	.086	.083	.087
Year f.e.	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Sector f.e.	Y	N	Y	N	Y	N	Y	N	Y	N
Competition f.e.	N	Y	N	Y	N	Y	N	Y	N	Y

Note: This table shows estimates of how the effect of negative feedback on venture survival varies by characteristics C_i. * 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. †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 7: 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. 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 8: Effect of Negative Dimension Feedback on Venture Continuation

Sample restricted to losers of round

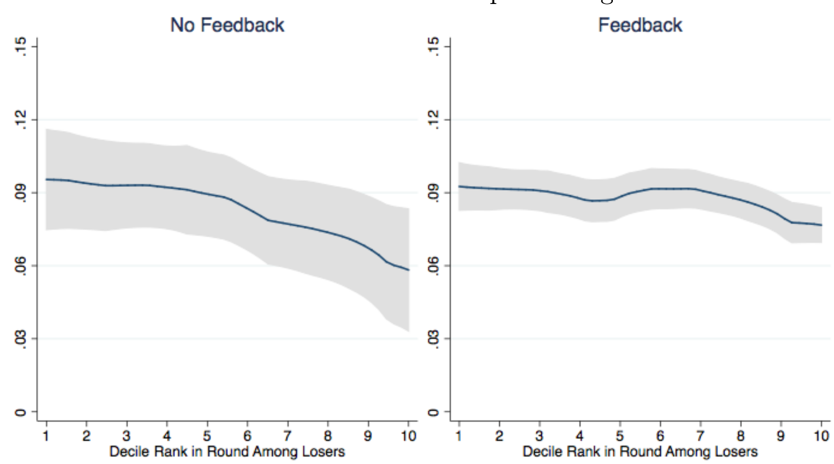
Dependent variable: Survival

Criteria (dimension=D):	Presentation	Team	Product/ tech	Market††	Financials	Bus 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)
Overall 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

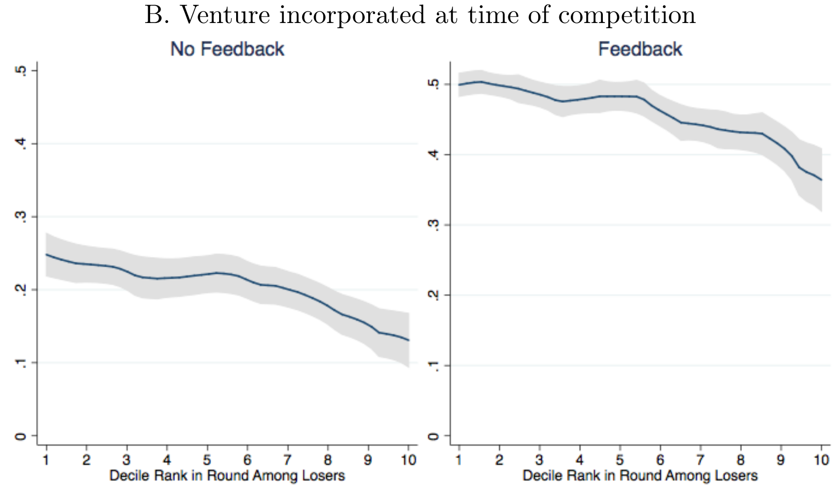
Note: This table shows estimates of the effect of negative feedback within dimensions. Errors clustered by competition-round. †Includes sector dummies, whether venture incorporated, and whether founder is student. ††Market attractiveness and size. *** indicates p-value<.01.

Figure 1: Ex-ante characteristics among losers (decile 1 is best)

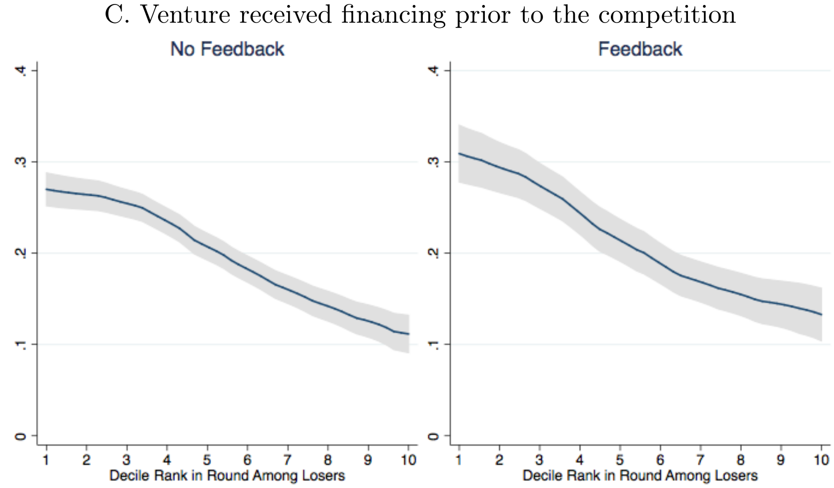
A. Founder attended top 10 college



B. Venture incorporated at time of competition

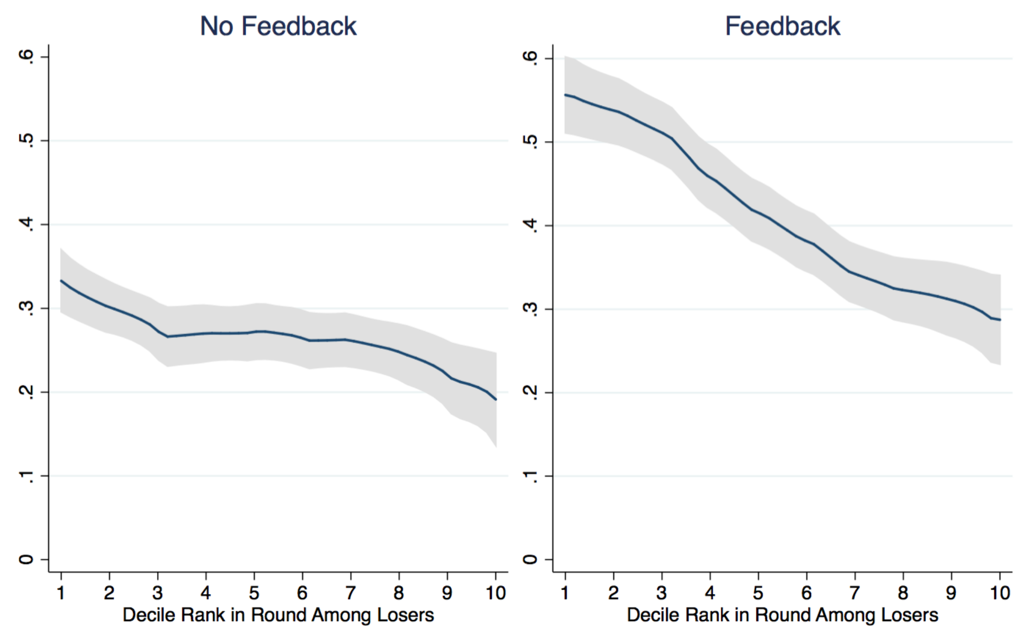


C. Venture received financing prior to the competition



Note: These figures show a characteristic's probability by venture decile rank among losers in the round. Only losers in preliminary rounds included. Local polynomial with Epanechnikov kernel using Stata's optimal bandwidth; 95% confidence intervals shown.

Figure 2: Survival probability by decile rank among losers (decile 1 is best)



Note: This figure shows the probability of survival (venture had ≥1 employee besides founder as of 8/2016) among losers in prelim rounds. The x-axis is the venture percentile rank among losers in the round. Local polynomial with Epanechnikov kernel; 95% CI shown.