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LEARNING FROM FEEDBACK: EVIDENCE FROM NEW VENTURES

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ABSTRACT

This paper explores how new venture competitions are helpful to entrepreneurs. In a regression discontinuity design using data from 87 competitions in 17 U.S. states, I show that winning is useful. While cash awards matter, winning is independently valuable in ways inconsistent with certification. Competitions instead seem to facilitate learning. I isolate learning by comparing lower and higher ranked non-winners across competitions in which they did and did not observe their standing. There is an economically large effect of negative feedback on venture abandonment. Cross-sectional variation suggests that founders treat their ventures as real options and are Bayesian updaters.

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"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."

– Arrow (1962)

1. Introduction

New venture competitions have become a ubiquitous feature of the high-growth entrepreneurship ecosystem. In these competitions, early stage startup founders present their businesses to a panel of expert judges. This paper explores whether and how new venture competitions are useful to entrepreneurs. To my knowledge, it is the first to do so with a large, administrative dataset from the developed world. It thus contributes to a nascent literature on competitions, including McKenzie (2017)'s analysis of one in Nigeria.[1]

I use novel data on 4,328 new ventures participating in 87 competitions in 17 states between 2007 and 2015. I link the ventures to employment, financing, and survival outcomes, taking care to account for name changes. The ventures 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). I also identify founders' education and career histories. I shed light on the characteristics associated with success. For example, founder job experience or having a software venture are associated with success, while having an MBA or a hardware venture are not.

Next, I employ a regression discontinuity design to assess the effect of winning and the informativeness of judge ranks. I find that winning is useful; after controlling for any cash award, winning a round increases a venture's chances of subsequent external finance by about 13 percentage points, relative to a mean of 24 percent. Winning also increases employment. There are three primary ways that competitions may be useful for startups: cash prizes,

[1] In contemporaneous related working papers, Xu (2017) and Wagner (2017) examine feedback in crowdfunding and the Startup Chile accelerator program, respectively. Other recent work studies related programs, including Hallen et al. (2014), Fehder & Hochberg (2014), Scott, Shu & Lubynsky (2016), Fehder (2016), and Gonzalez-Uribe & Leatherbee (2016).

certification, and learning. The cash prizes are valuable; an additional \$10,000 is associated with about a 1 percentage point increase in financing probability. This is somewhat higher than the effect of U.S. Department of Energy SBIR grants found in Howell (2017).

However, the effect of the cash prize is economically small relative to the independent effect of winning and the predictive power of judge scores. Percentile rank and z-score robustly predict external financing, employment, and acquisition or IPO. Ranks predict startup outcomes even when ventures do not learn their ranks and thus cannot be affected by them. Further, dimension scores are differentially predictive. Team scores are the strongest predictor of initial success, while technology/product scores are strongly predictive – and are the only predictor – of long run, high-level success (acquisition/IPO).

Score informativeness indicates that the competitions provide useful signals. Yet certification, where winning is a signal to the market about venture quality, does not seem to be the primary mechanism. Winning is more useful in preliminary rounds, where certification should be weaker, than in final rounds. Winning is also just as useful in non-selective competitions as selective competitions, and just as useful for elite college graduates as other founders. This points to learning.

I test for learning, in the sense of entrepreneur type revelation, by isolating the effect of feedback. In 53 of the competitions, ventures are informed only that they won or lost, and otherwise do not learn where they stand relative to their peers. In 34 of the competitions, ventures are privately informed of their overall and dimension ranks in the round (but never individual judge ranks). The competitions are otherwise similar, and in the feedback competitions neither ventures nor judges are informed that structured feedback would be provided.

The effect of negative feedback on venture continuation is identified with a difference-in-differences model among non-winning ventures. The first difference is within round, comparing below-median and above-median non-winners. The second difference is across rounds, comparing ventures that were informed of their rank with those that were not. 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. Receiving negative feedback significantly increases abandonment. Specifically, it reduces the chances a venture has at least one employee besides the founder as of August 2016 by about nine percentage points, equivalent to a 14 percent increase in abandonment (the mean is 66 percent). The effect occurs quickly, mostly in the first six months. It is also roughly symmetrical among winners without cash awards.

The empirical concern is whether this effect reflects systematically different distributions among non-winners in the two types of competitions (differences in levels are absorbed). To address this concern, I use three tests and five robustness exercises. The three tests show that the distributions of observables across the two types of competitions are similar ex-ante, and that entrepreneurs do not seem to select 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.

Understanding how and which entrepreneurs learn can help inform the theory of entrepreneurship. The data reject models in which entrepreneurs have static types, or equivalently models in which entrepreneurs are so overconfident that they ignore new information. This is consistent with the idea of entrepreneurship as a process of experimentation, as in Kerr, Nanda & Rhodes-Kropf (2014) and Manso (2016). While it may seem obvious that people learn, in the context of entrepreneurship it is not. Instead, there is a strong paradigm that entrepreneurs do not learn about their own probability of success (Bernardo & Welch 2001, Bergemann & Hege 2005, Landier & Thesmar 2009). This behavioral view emphasizes the role of cognitive biases such as over-precision and optimism in entrepreneurial decision-making.[2] In contrast, learning plays a pivotal role in many models of firm dynamics, including Jovanovic (1982), Aghion, Bolton, Harris & Jullien (1991), and Ericson & Pakes

[2]See Cooper et al. (1988), Camerer & Lovallo (1999), Arabsheibani et al. (2000), Astebro, Jeffrey & Adomdza (2007), Koellinger et al. (2007), Kogan (2009), and Bloom, Lemos, Sadun, Scur & VanReenen (2014). More broadly, financial contracting theory focuses on information asymmetry, and typically assumes that the entrepreneur knows his type or has static beliefs about it (e.g. Admati & Pfleiderer 1994, Clementi & Hopenhayn 2006, Sørensen 2007, Hellmann 1998, Cagetti & De Nardi 2006).

(1995). New information determines entry and exit decisions in these models, implying that entrepreneurs should be sensitive to external signals about their project quality, as they are in my data.

To speak more specifically to theory, some ventures may have higher real option values from delaying abandonment, as in Manso (2016). An option's value increases in its uncertainty and in its asset specificity. Consistent with this, when judges are uncertain about a venture, the founder is less responsive to negative feedback. Ventures that are not yet incorporated, have no prior external private financing, or are software- rather than hardware-based are more responsive. These characteristics are associated with less irreversible investment.

Founders also behave consistently with Bayesian updating. They are less responsive when there are fewer judges, suggesting that they dismiss imprecise signals. They also update less when they have more information about their own type. Over-precision and optimism biases should concentrate the effect of negative feedback in the lowest ranked founders. Instead, the effect is broadly linear. Feedback induces near-winners to continue as much or more than it encourages the poorest performers to exit. Motivated by this evidence, I use a Bayesian framework to model and calibrate sensitivity to feedback.

In Odean (1999) and Hanna, Mullainathan & Schwartzstein (2014), people do not learn because of noisy or multi-dimensional signals. On the other hand, recent work outside of firm settings has found that individuals can learn about their ability through performance (Seru, Shumway & Stoffman 2010, Hochberg, Ljungqvist & Vissing-Jørgensen 2013). Whether entrepreneurs learn better from certain types of signals is a promising avenue for future research.[3]

The paper proceeds as follows. The data are introduced in Section 2. The effect of winning and the predictive power of scores are in Section 3. The effect of feedback is in Sections 4 and 5. Section 6 uses cross-sectional evidence to explore variation in learning.

2. New venture competition data

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

[3] Also related to this paper is the literature on peer effects in entrepreneurship, including Nanda & Sørensen (2010), Lerner & Malmendier (2013), and Guiso et al. (2015).

Section 2.3.

2.1. *The competitions*

New venture competitions, sometimes called business plan or "pitch" competitions, have proliferated in the past decade. In a competition, new venture founders present their technologies and business models to a panel of judges. New venture competitions are now an important part of the startup ecosystem, particularly for first-time founders. For example, among the 16,000 ventures that the data platform CB Insights reports received their first seed or Series A financing between 2009 and 2016, 14.5 percent won a competition. Sponsored universities, foundations, governments, and corporations, among other institutions, competitions aim to serve convening, certification, education, and financing functions.

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.[4] 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 Table A1. The competitions are usually open to the public, but typically there are few people besides the judges in the room, except in the final round.

All the competitions have the following features: (1) They include a pitch event, where the company takes five to 15 minutes to present its business plan; (2) Volunteer judges formally and privately score participants, and venture ranks in the round determine which ventures win; (3) Ranks and scores are secret, except when a feedback competition informs a venture of its rank; (4) The organizer does not take equity in any participating ventures; (5) The organizer explicitly seeks to enable winners to access subsequent external finance. In most competitions, judges score or rank based on six dimensions (or "criteria"): Team,

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

Financials, Business Model, Market Attractiveness, Technology/Product, and Presentation. These dimension scores or ranks are aggregated into a judge-specific venture score or rank. When scores are used, they are ordered to produce ranks. Judge ranks are then averaged to create an overall rank, which determines round winners.

The econometrician observes all ranking and scoring information. This includes overall ranks and individual judges' scores and ranks. In no case do founders observe individual judge scores or ranks. Judges score independently and observe only their own scoring, and never overall ranks.[5] There is time for questions and usually dedicated networking (e.g., post-competition reception), providing for informal, verbal feedback. 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 non-winners. To the best of my knowledge, neither entrepreneurs nor judges perceive a penalty from losing.

I use three transformations of the rank and score data. First, I use decile ranks calculated within non-winners and winners separately. That is, I divide non-winners in a round into ten equal bins, with the best ranks in 1, and the worst in 10. Second, I use judge decile ranks, calculated among 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.[6]

2.2. *Summary statistics*

The ventures are described in Table 1 panel 2. The average age of the ventures is 1.9 years.[7] Forty-four percent of the ventures were incorporated at the round date as a C- or S-corp. Ventures are matched to investment events and employment using CB Insights, Crunchbase, AngelList, and LinkedIn.[8] In researching the ventures, 765 name changes were identified.

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

[6] The number of ventures varies across rounds, and to determine which ventures win a round, most of the competitions use ordinal ranks while a few use scores. I cannot, therefore, use the raw rank or score data provided.

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

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

Ventures were matched to private investment on both original and changed names.

Venture survival is a binary indicator for the venture having at least one employee besides the founder on LinkedIn as of August 2016. Among ventures that are abandoned, time to abandonment is the number of days between the competition and the founder's next job start date. While some startups may not appear on LinkedIn, if they are ultimately successful, they almost certainly will, because their employees will identify themselves as working at the company. That is, companies rarely remain in "stealth" mode forever. Websites are a poor survival measure because they often stay active long after a startup has failed. Founders are described in Table 1 panel 3, using data from the competitions and LinkedIn profiles. Founders are mostly first-time entrepreneurs. Twenty-one percent of founders are women, and 72 percent are men (the remaining seven percent had ambiguous names and no clear LinkedIn match).[9] Elite degree status is tabulated using the university ranking in Table A2.

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 percent are VCs, 20 percent are corporate executives, and 16 percent are angel investors. Ventures and judges are assigned to 16 sectors. Ventures sector assignations come from competition data, and each venture is assigned only one sector. Judge sectors are drawn from LinkedIn profiles or firm webpages, and judges may have expertise in multiple sectors. Ventures and competitions are sorted by state in Table A4. There is concern that the judges investing themselves might contaminate any impact of the competitions on venture financing. Careful comparison of funded ventures' investors and judges revealed 95 instances of a judge's firm invested in the venture, and three instances of the judge personally investing.

2.3. *Sample representativeness*

There is little empirical analysis of startups prior to their first external funding event, but the data are roughly representative of first-time, early stage startups and their founders in the U.S.. Table A5 compares the distribution of ventures in my data to overall U.S. VC investment. The share of software startups in my data, 37 percent, is close to the national average (40 percent) in deals and dollars. In part because VC investment in clean energy

[9] 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, were coded by hand.

has declined dramatically in recent years (Saha & Muro 2017), as well as the presence of the Cleantech Open in my sample, the data are skewed towards clean energy.

The competitions take place in 17 U.S. states. With the exception of Arizona, the top twenty states for venture location in the data almost entirely overlap with the top twenty states for VC investment, though the 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 percent, is comparable to the 5 percent found in Ewens & Townsend (2017)'s sample of AngelList startups. Each venture team averages three members. This is similar to Bernstein, Korteweg & Laws (2017), who note that on the AngelList platform, the average number of founders is 2.6. The median founder age, based on subtracting 22 from the college graduation year, is 29 years. This is roughly representative of startup founders. [10]

Associations between venture characteristics and success also accord with common knowledge. 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. Kaplan et al. (2012) find a similar relationship between college selectivity and success for CEOs of VC-backed companies. Associations between sector and success are in Table A6 panel 2. Software and education ventures are more likely to succeed, while social enterprise and biotech ventures are less so. Media and entertainment ventures are far more likely to raise Angel/VC.[11]

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

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

2.4. *Feedback*

I selected competitions for analysis that are otherwise similar but provided systematically different feedback. Interestingly, competition organizers generally do not treat explicit feedback as a program goal. Instead, they are concerned with facilitating networking and identifying the "best" ventures as winners. However, 34 of the programs I study used a third party, Valid Evaluation, to manage their judging software. Valid Evaluation believed that formal feedback might be useful, and sent each venture an email after the round containing their overall rank and dimension ranks (dimensions include "Team" and "Technology"). Ventures learned only their own ranks, and not those of other participants. Interviews with competition organizers indicated that they do not share an interest in feedback, and in fact sometimes discontinued use of Valid Evaluation in part because it seemed more concerned with feedback than with features the organizers valued more, such as the user interface.

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

3. **Effect of winning and signal informativeness**

This section first presents the empirical strategy for estimating the effect of winning (Section 3.1). The results are in Section 3.2. Section 3.3 discuss the predictive power of the scores. This analysis provides, to my knowledge, the first evaluation of the effect of winning new venture competitions in the developed world. This is relevant for policy, as many competitions are publicly funded. 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. Two examples of government-funded competitions in my data are the Arizona Innovation

Challenge, which awards \$3 million annually, and the National Clean Energy Business Plan Competition, with \$2.5 million in allocated funding.

3.1. *Empirical design*

I use a regression discontinuity design to evaluate the effect of winning. In Equation 1, the dependent variable Y_i^{Post} is a binary measure of venture success.

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

The vector $\mathbf{f.e.}$ includes competition-round-panel or judge fixed effects. The former absorb the date and location. Controls $\mathbf{X_i}$ include whether the judge or judge's company ever invested in the venture, 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 participating ventures.

3.2. *Effect of winning*

I find that winning itself and cash awards are useful to startups. Visual evidence is in Figure 1. The top two graphs use local polynomials to show the probability of subsequent external financing in preliminary and final rounds. The venture's percentile rank in the round is on the x-axis, with vigntile ranks (five percentile bins) for preliminary rounds, and decile (ten percentile bins) for final rounds. The lines overlap because the share of participants that win varies across rounds.[12] The bottom two graphs repeat this exercise for having at least ten employees. In all four cases, the winner line lies above the non-winner line, indicating a substantial raw effect of winning.

Estimates of Equation 1 are in Table 2. After controlling for any cash award won, my preferred specification finds that winning a round increases a venture's chances of subsequent external finance by 13 percentage points (pp), relative to a mean of 24 percent (Table 2 panel 1 column 1). The effect falls with venture controls, although these reduce the sample

[12] There are no losers in the top bin in either case. Winners are truncated at the sixth vigntile and fifth decile for preliminary and final rounds, respectively.

size (column 2). A logit model in column 3 finds roughly a doubling, because it drops groups without successes (panels without financing events). The effect is a bit larger at the judge-venture level with judge fixed effects (column 4). Remaining columns examine other outcomes. Winning increases a venture's chances of survival and having at least 10 employees in 2016 by about 5 pp, relative to means of 34 percent and 20 percent, respectively (columns 5-6). In the specification used here, the effect on acquisition or IPO (column 7) is not statistically significant, though it is in alternative models, such as when the separate control for cash award amount is omitted.

The cash award is also useful. An extra $10,000 increases the probability of financing by about 1 pp (Table 2 panel 1 columns 1-2). This effect seems small in economic magnitude relative to the overall effect of winning and the predictive power of rank, discussed below.[13] It is similar to the effect of U.S. Department of Energy SBIR grants found in Howell (2017). The effect of an additional $10,000 in SBIR grants on the probability of subsequent financing is 0.66 pp, or 8 percent of the sample mean, while the effect of a prize here is 1 pp, or 4 percent of the sample mean.[14]

Winning is most impactful in preliminary rounds, and when it does not involve prize money. The effect is 14 pp in preliminary rounds (Table 2 panel 2 column 1), and just 9 pp in final rounds (column 4). Within preliminary rounds, column 2 omits ventures that ultimately won any cash award are omitted. The effect increases slightly, to 15 pp. To emphasize the causality of this effect, column 3 restricts the sample to the two quintiles around the cutoff for winning in a preliminary round, and finds again an effect of 9.8 pp.[15]

A larger effect in preliminary rounds is the opposite of what we would expect if certification were the mechanism. Two further tests for certification are whether winning is more useful in selective competitions, and is less useful for founders with elite backgrounds. In Table A7, all covariates besides the panel fixed effects are interacted with an indicator for whether the competition was selective or prestigious.[16] I find no differential effect of winning

[13] 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.

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

[15] In unreported regressions, I found no difference in the effect of winning across university-hosted and non-university competitions.

[16] I include HBS' New Venture Competition, because while the competition itself is not selective, partici-

or the award (column 1). I next interact with an indicator for whether the founder graduated from a top ten college. Again, I find no differential effect of winning, but I do find that the cash award is significantly less useful for elite founders (column 2). The same is true using an indicator for whether the founder had a previous venture (column 3). The results thus far indicate that competitions are useful for reasons beyond certification or prize money, though cash awards are more useful to founders who likely have less access to resources. Competitions therefore operate differently from the SBIR grants in Howell (2017), which were found to be useful because the cash award funded prototyping.

3.3. *Signal informativeness*

A striking finding from Table 2 is the large and significant coefficients on rank. The regressions control for the decile rank among winners and among non-winners separately. Particularly within non-winners – a much larger sample – rank and z-score strongly predict success, after controlling for winning and competition fixed effects. A one decile improvement in rank increases the probability of external financing by 1.8 pp, significant at the .01 level (Table 2 panel 1 column 1). Rank is also predictive within judge (column 3). Importantly, it persists within the no-feedback competitions, where it is impossible that the judge's ranks directly affect venture outcomes (Table 2 panel 2 columns 7-8).[17]

Thus, competitions generate valuable signals, suggesting that winning – and perhaps participation more broadly – may be useful because of the opportunity to learn from the judges' expert opinions. Note that if the judges did not have predictive power, in expectation it would not be clear why ventures participate. In a rational market, there should be no positive effect of winning separate from the cash award if judges were choosing winners at random. This is the opposite of SBIR grant ranks, which were uninformative about outcomes. There are a number of differences between the SBIR grant process and new venture competitions. One is that the competition judges are expert market participants. Unreported regressions examine the predictive power of rank by judge occupation. There is little difference across investor, lawyer/consultant/accountant, and corporate executive

pating teams must include at least one HBS MBA student, and of course attending HBS is quite selective. The competition is also regarded as prestigious by local venture capitalists.

[17]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 are significant at the .01 or .05 level.

judges. Perhaps surprisingly, entrepreneur judge scores have no predictive power.

The dimension ranks that are aggregated to form overall ranks are also informative. Table 3 shows the association between dimension ranks and outcomes, controlling for win status. A higher team rank is the strongest predictor of success for all outcomes other than IPO/acquisition. For IPO/acquisition, the only dimension with predictive power is product/technology, and this is quite robust. **?** and Gompers, Gornall, Kaplan & Strebulaev (2016) find that early stage investors care most about information regarding founder team quality. Here, team matters for low-level, earlier stage success, while technology matters for high-level successes, such as IPO. This speaks to the "horse vs. jockey" debate, suggesting that the team matters initially, but the business matters in the long run. It is consistent with Kaplan, Sensoy & Strömberg (2009), who examine 50 public firms and find that business lines but not management remain stable from startup to IPO.

4. Responsiveness to feedback: Estimation strategy

Thus far, we have seen that winning a competition is useful, and much of the benefit of winning is not well-explained by either the prize money or certification. Furthermore, the judges generate informative signals in their scoring. This raises the possibility that competitions are useful because they create learning opportunities. Winning is a binary transformation of the underlying ranking information, which is not observed in the no-feedback competitions, where it is still informative about startup outcomes. Winners may push forward with their ventures because they correctly interpret winning as a positive signal. To test this possibility, it is necessary to isolate the effect of the rank signal. This section first proposes the main design for estimating the effect of feedback on venture continuation (Section 4.1). It then addresses the challenge to causal identification (Section 4.2).

4.1. *Analytical approach*

I compare competitions where ventures receive feedback – they learn their rank relative to other participating ventures – with competitions where ventures learn only that they won or lost. This feedback is relative: ventures are learning their order statistic, and thus the peer group matters. The analysis asks whether founders that receive especially negative feedback about their position relative to their peers are more likely to abandon their ventures.

The empirical design in Equation 2 is a difference-in-differences model among non-winners, which comprise 75 percent of the data. The first difference is between above- and below-median non-winners in a given competition ($Low\ Rank_{i,j}$). The second difference is across feedback and no-feedback competitions ($Feedback_j$).

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

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

4.2. *Identification challenge*

In Equation 2, above-median non-winners comprise the control group. Therefore, average differences across the types of competitions are differenced out. The concern is that the distribution of non-winners around the median may be systematically different in the two types of competitions, even though applicants did not know whether the competition would inform them of their rank in the round. The problem is if the mapping from quality to rank is systematically different. There are two main sources of bias. First, suppose that ranks in the feedback competitions better correlate to true quality than ranks in the no-feedback competitions. Then feedback might be inherently correlated with continuation without any effect of information. Second, feedback competitions could have diverse participants while the no-feedback competitions have participants with similar quality. This could also lead to more abandonment in response to a lower rank in the feedback competitions.

To address these concerns, 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 first part of the Online Appendix describes the three tests. They demonstrate that across the two types of competitions, the distributions are not meaningfully different, rank reflects observable quality at the time of the competition equally, and that there is no evidence of selection into feedback. The five robustness tests are in Section 5.

5. Responsiveness to feedback: Results

The main effect of negative feedback on abandonment is in Section 5.1. Section 5.2 contains five robustness tests. Section 5.3 explores whether learning is efficient.

5.*1.* *Main results*

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. They are also, however, predictive in the no-feedback competitions, as shown in the regression discontinuity analysis. This is important, as it demonstrates that ranks are inherently informative about outcomes. The higher average probability in feedback competitions reflects that feedback induces highly ranked non-winners to continue, and that ventures are more likely to be incorporated on average in the feedback competitions. This average difference is eliminated by Equation 2.

Equation 2 is estimated in Table 4. The main specification in panel 1 column 1 finds that negative feedback reduces the likelihood of continuation by 8.6 pp, relative to a mean of 34 percent, significant at the .05 level. This effect size is economically large, especially given the subtle, low stakes nature of the feedback. It translates to a 14 percent increase in the probability of failure.[18] Summing the three coefficients gives a total average effect of *Low Rank·Feedback* of 8.4 pp. The effect is slightly larger within preliminary rounds (column 4). To ensure that the higher average venture maturity in feedback competitions does not somehow explain the effect, column 5 restricts the sample to unincorporated ventures, and finds an effect of 12 pp. An alternative story is that highly ranked non-winners with feedback are better able to raise financing than their uninformed counterparts. Perhaps they tell prospective funders about their relatively high ranking. However, in unreported tests I find that negative feedback has no effect on subsequent external financing.[19]

The effect is roughly linear, but somewhat larger at the higher end of the non-winner distribution, suggesting that feedback induces near-winners to persevere as much as or more than it encourages the poorest performers to exit. In column 6, "low rank" is one if the venture is in the bottom three deciles among non-winners. In column 7, it is one for the bottom seven deciles. In column 8, "low rank" is defined as deciles 5-8, and the bottom two deciles are omitted. The effect is not driven by the bottom deciles, and is strongest in column 7. Supporting the hypothesis that relatively positive feedback induces continuation, the effect is symmetrical among round winners that did not ultimately win the overall competition. I show the effect of positive feedback in Table 5 panel 1 column 10. Having an above median rank but not winning is associated with a 11 pp increase in the probability of survival. However, this effect is less robust than the negative feedback effect, possibly reflecting the smaller sample.

The effect occurs quickly. When the dependent variable is an indicator for abandoning within six months, the effect is 7.9 pp, relative to a mean of 51 percent (Table 4 panel 2 column 1). In columns 2 and 3, the effect increases to 8.7 and 8.9 pp within 1 and 2 years, respectively, relative to means of 57 and 64 percent. The main effect therefore occurs within the first two years.

The large effect of subtle, low-stakes feedback shows that entrepreneurs can learn about their types. This offers a mechanism for competitions to be useful, and it also rejects

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

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

the hypothesis that entrepreneurs are characterized by extreme overconfidence. Showing that entrepreneurs learn is not as obvious as it might appear. There is in fact a strong paradigm in the literature that entrepreneurs do not learn about their own probability of success (Bernardo & Welch 2001, Bergemann & Hege 2005, Landier & Thesmar 2009). This behavioral view emphasizes the role of cognitive biases such as over-precision and optimism in entrepreneurial decision-making.[20] My results are more consistent with models of firm dynamics in which learning plays a pivotal role, including Jovanovic (1982), Aghion, Bolton, Harris & Jullien (1991), and Ericson & Pakes (1995). New information determines entry and exit decisions in these models, implying that entrepreneurs should be sensitive to external signals about their project quality.

5.*2.* *Robustness tests*

5.*2.1.* *Matching estimators*

Exact and propensity score matching estimators adjust for "missing" potential outcomes by matching subjects in a treatment group to their closest counterparts in the untreated group. The difference between observed and predicted outcomes is the average treatment effect. 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 non-winners 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 A9; the match dramatically reduces the differences. The result is in Table 4 panel 2 column 6. Exact matching yields nearly the full sample result, at 7.6 pp, significant at the .01 level.

The second method is propensity-score matching, which first estimates the probability of treatment using a logit model. It then identifies, for each treated participant, the untreated participant with the closest probability of treatment.[21] Table A10 shows that the matching

[20] 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). Financial contracting theory 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, Hellmann 1998, Cagetti & De Nardi 2006, and Ewens, Jones & Rhodes-Kropf 2013).

[21] I try to eliminate bias in several ways. First, I match without replacement, so that once an untreated

brings the samples almost entirely in line. The effect falls somewhat in the propensity-score matching, to 5.6 pp, significant at the .05 level (Table 4 panel 2 column 7).

5.2.2. *Exploiting nominal scores*

In all but two of the competitions, the conference organizers arrive at ranks by ordering nominal scores. These nominal scores are never revealed to ventures. 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. Table 4 panel 2 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 9.3 pp. The effect of interest is in column 10, which restricts the sample to feedback competitions, and estimates the effect of rank after controlling for nominal score. It finds that increasing a venture's rank by one decile reduces the probability of abandonment by 1.4 pp, significant at the .1 level. 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. I add interactions between feedback

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.

and characteristics likely associated with signal quality, venture survival, and participant diversity.

Competition signal quality proxies are whether the competition is at a university, the number of ventures, the number of judges, and the location.[22] For likelihood of venture survival, I use the share of founders that attended a top ten 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 are in Table A11 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 in Table A11 panel 4, interacting feedback with venture characteristics associated with ex-ante quality.[23] The effect of feedback persists, though it is attenuated to 6.7 pp. Thus distributional differences do not seem to drive the 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 non-winners was different in 2011. Comparing the effect of having a low rank in 2011 relative to other years provides a useful robustness test. The results are in Table A12. 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.

5.2.5. *Functional form and subsamples*

Table 4 panel 2 column 4 controls for the first and second moment in z-score. Column 5 uses a logit specification. The main effect is robust to both approaches, significant at the .05 level. A final set of tests ensures that the results are robust to subsamples. The effect

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

[23] These are 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.

persists within the population of founders with MBAs, among ventures from VC hub states, and among student-led ventures (Table A13).

5.3. *Is learning efficient?*

Private, costless, informative signals at an early stage might enable poor quality startups to fail faster, making innovation more efficient. The main result implies that had the 1,603 unique below-median non-winners in the no-feedback competitions received feedback, an additional 137 would have been abandoned, beyond the 1,186 that were abandoned. While I cannot assess the welfare impacts of feedback, I examine three ways that learning might not be efficient.

First, inducing abandonment could be socially costly if a few highly successful outcomes are foregone. Among below-median ventures in the feedback competitions, 2.1 percent were acquired, compared to 3.2 percent in the no-feedback competitions. All appear to be minor acquisitions, as valuation 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. In the absence of earnings data, I create an indicator for whether the latest job title of founders who abandoned their ventures implies a leadership role.[24] 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 any feedback or negative feedback is related to subsequent non-entrepreneurial leadership among founders that abandoned their ventures. Therefore, feedback does not seem to cost abandoners ultimate leadership positions.

Third, even if learning is on average efficient, there may be many cases in which ventures are randomly assigned especially lenient or harsh judges, leading to inaccurate signals. 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

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

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 A14, I show that leniency predicts scores (columns 1-2), but that there is no effect of leniency on responsiveness (column 5). Lenient judges do not influence a venture's overall rank enough to affect the abandonment decision. In sum, 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 if encouraging more entrepreneurial entry is always socially beneficial, regardless of startup quality.

6. Who learns?

Section 5 demonstrated that on average, entrepreneurs are quite sensitive to informative feedback, and incorporate it into their strategic decisions. This raises the questions of which entrepreneurs learn and under what circumstances. I add an interaction for a cross-sectional characteristic. A nice aspect of this heterogeneity analysis is that it permits including competition fixed effects, which address any remaining concerns about systematic differences across competitions.

I begin by showing that variation in overconfidence does not explain the results well (section 6.1). Instead, I find cross-sectional evidence consistent with two mechanisms. First, some ventures have higher real option value from delaying abandonment (Section 6.2). Second, founders behave consistently with Bayesian updating (Section 6.3).

6.1. *Overconfidence*

Being male is the characteristic most robustly associated with overconfidence, in the sense of both over-optimism and an excessively precise prior (e.g. Barber & Odean 2001, Beyer & Bowden 1997). Thus if overconfidence affects responsiveness, I expect to find a difference along gender lines. Conversely, women, who comprise 21 percent of the sample, are not more responsive. This is shown in Table 5 panel 1. In column 1, female is added as a third interaction to Equation 2. In column 2, competition fixed effects are included. Notably, the effect of low rank interacted with feedback (now identified within male founders) is almost exactly the same as in the main specification, at 8.3 pp. This is true for many of the heterogeneity analyses, confirming that differences across competition types do not explain the results.

6.2. *The venture as a real option*

If founders treat their ventures as real options, they should be less responsive – delaying abandonment despite negative feedback – when the venture is more uncertain and has more asset specificity, or irreversibility of investment (Dixit & Pindyck 1994). First, software-based ventures are more responsive than hardware-based ventures (Table 4 Panel 1 columns 3-4). This does not seem to relate to non-pecuniary motivations among hardware founders, as columns 5-6 find no effect for social impact ventures. Second, ventures with prior external financing are 15 pp more likely to continue after receiving especially negative feedback than those without prior financing (Table 5 panel 1 columns 7-8). Third, unincorporated ventures are 11 pp more responsive, relative to a mean of 44 percent (Table 5 panel 2 columns 1-2). These three types of ventures likely have higher sunk costs and thus greater investment irreversibility.[25] These results also indicate that learning about type is most important before firm boundaries form. In some models, including Cornelli & Yosha (2003) and Schmidt (2003), 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.[26]

A good proxy for risk is disagreement among judges. When the standard deviation of judge ranks within a competition-round-panel is above median, the triple interaction yields a positive effect (Table 5 panel 2 columns 7-8).[27] However, this could reflect signal precision, if founders learn from verbal interactions with judges that they lacked consensus. To tests this, I instrument for the standard deviation using the judge leniency measure described above. I find no effect using the instrument, indicating that the result likely reflects venture risk.[28]

[25]These characteristics could also be associated with more private information, but older ventures and non-student founders are not more or less responsive than their counterparts (Table 5 panel 2 columns 3-6, 11-12). These groups may have more information, but have not necessarily generated more specific assets.

[26]In unreported results, I find no variation among future serial entrepreneurs (founders that abandon this venture but found a subsequent one). I also find no variation by founder age or whether he founded a prior venture.

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

[28]When a venture is assigned an especially lenient and an especially harsh judge, the standard deviation of judge ranks should be higher independently of the venture's risk. I consider two measures: $V_{i,\sigma}^{high}$ is the standard deviation of the lenience measure L_{ij}, and $V_{i,\sigma}^{ext}$ is the standard deviation of L_{ij} among only

Venture resemblance to a call option should increase with the personal wealth of the founder. More personal wealth should make it less costly to continue with the venture and also reduce downside risk in the event the venture ultimately fails, as in Vereshchagina & Hopenhayn (2009). Founders with top college degrees are likely richer (Chetty et al. 2017). Table 5 panel 2 columns 13-16 shows that they are less responsive. In unreported tests, I find that rank is equally predictive among elite school founders as in the broader sample.

6.3. *Bayesian updating*

Bayes' rule dictates how rational agents update their beliefs.[29] Three cross-sectional findings are consistent with Bayesian updating. First, founders are more responsive when the signal is more precise, measured as having more judges. (Founders can observe the number of judges.) In the strongest heterogeneity result, the effect of negative feedback on continuation is 29 pp greater when the number of judges is above median (Table 5 panel 2 columns 9-10).[30] Second, feedback should matter less when the prior is more precise. Consistent with this, ventures that have received external financing are less responsive.

Relatedly, Bayesians should update less when they have more information about their own type. 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. That is, founders likely have better private knowledge about the quality of their product or technology than judges do. In Table 6, "low rank" is defined along a specific dimension. Negative feedback impacts continuation most along the financials, business model, market, and team dimensions. There is no effect for product/technology.[31]

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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. Table A15 shows that variation in leniency predicts the standard deviation of judge scores quite well. The F-statistics in first-stage regressions range from 14 to 31. In a naive instrumentation approach, I replace the standard deviation with the leave-one-out variation measures. Columns 5-6 show no effect of the triple interaction between having a low rank, receiving feedback, and having judges with high expected variation in leniency.

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

[30] 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.

[31] 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. I do not find substantial variation by judge occupation

Non-linearity in the effect could be consistent with cognitive biases, because rank predicts success in a linear way. Excessively elevated or precise priors should prevent founders from updating downward enough when they receive a middling rank among non-winners. Instead, the effect is roughly linear, and persists among winners (see Section 5.1). In sum, founders behave like Bayesians, though I cannot rule out other models.

A simple model of how a Bayesian updater responds to feedback is in Appendix Section 2. It assumes the founder interprets his rank as the result of a series of Bernouilli trials, where the number of signals is the number of judges. This allows the Beta distribution as the conjugate prior. Hewing closely to the information structure and main results from the preceding sections, I calibrate the model to show how feedback affects a founder's success probability distribution. Figure 3 shows the results of the calibration exercise. The interim prior is in Figure 3A. The posteriors after negative feedback (below-median non-winner) and positive feedback (above-median non-winner) are in Figure 3B and 3C. Finally, I interpret one of the heterogeneity results through the Bayesian calibration. Figure 4 depicts how having an above-median number judges affects the posterior by improving signal precision.

7. Conclusion

This paper shows how new venture competitions are useful to startups. Winning and cash awards are useful, but competitions are also valuable because they facilitate entrepreneur learning in the sense of type revelation. Founders seem to treat the venture as a real option, and they behave consistently with Bayesian updating. In Manso (2011)'s optimal contract, feedback should be timely and tolerant of failure. New venture competitions with feedback implement this guidance: While they reward top performers, they do not penalize especially poor performance. Under conditions in which it is not socially costly to deter low quality startups, the data indicate that giving entrepreneurs private, expert feedback may improve resource allocation and the efficiency of innovation.

The substantial heterogeneity raises questions about how learning interacts with innovation. 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 the most 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
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
# 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

Panel 1

Dependent variable:	Financing after round*				Survival*	10+ employees	Acquired/IPO
		Venture controls	Logit	Judge f.e.			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Won Round	.13***	.077**	.71***	.16***	.047*	.051*	.018
	(.026)	(.037)	(.14)	(.015)	(.028)	(.027)	(.012)
Decile rank winners	-.011***	-.0062	-.069***		-.006	-.0041	-.0028*
	(.0044)	(.0056)	(.021)		(.0043)	(.0044)	(.0017)
Decile rank non-winners	-.018***	-.014***	-.13***		-.023***	-.017***	-.0011
	(.0025)	(.0032)	(.017)		(.0028)	(.0023)	(.001)
Within-judge decile rank				-.0061***			
				(.0014)			
Award Amount (10,000$)	.0085***	.0093***	.036***	.011***	.0062*	.0074***	.0002
	(.0024)	(.003)	(.011)	(.0023)	(.0032)	(.0026)	(.0013)
Venture controls††	N	Y	N	Y	N	N	N
Comp.-round- panel f.e.	Y	Y	Y	N	Y	Y	Y
Judge f.e.	N	N	N	Y	N	N	N
Year f.e.	N	N	N	Y	N	N	N
N	6023	3367	5484	23785	6023	6023	6023
R^2	.16	.4	.12	.43	.17	.14	.083

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 3. 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). ††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.

Panel 2

Dependent variable: Financing after round*

	Prelim rounds only			Final rounds	Z-scores		No feedback only	
		No award	Quintiles around cutoff‡					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Won Round	.14***	.15***	.098***	.089*	.13***	.098***	.13***	.15***
	(.03)	(.04)	(.026)	(.05)	(.023)	(.026)	(.034)	(.02)
Decile rank winners	-.015***	-.016**		.0031			-.0091	
	(.0052)	(.0066)		(.0066)			(.0061)	
Decile rank non-winners	-.018***	-.017***		-.021***			-.011***	
	(.0032)	(.0036)		(.0044)			(.0033)	
Z-score winners					.027			.0064
					(.019)			(.023)
Z-score non-winners					.041***			.031***
					(.01)			(.011)
Z-score^2 winners					.019			.013
					(.014)			(.016)
Z-score^2 non-winners					.000056			.0097
					(.0073)			(.0084)
Within-judge z-score						.027***		
						(.0063)		
Award Amount (10,000 $)	.012***		.013**	.0053	.0089***	.0056*	.011**	.012**
	(.0032)		(.0057)	(.0034)	(.0029)	(.0029)	(.0055)	(.0055)
Venture controls††	N	N	N	N	Y	Y	N	N
Comp.-round- panel f.e.	Y	Y	Y	Y	Y	N	Y	Y
Judge f.e.	N	N	N	N	N	Y	N	N
Year f.e.	N	N	N	N	N	Y	N	N
N	4394	3404	1945	1605	3529	13285	3429	3980
R^2	.16	.12	.23	.17	.41	.4	.2	.19

Note: This panel is a continuation of Table 2. ‡Includes only the two quintiles around the cutoff for winning a preliminary round (no final rounds included). *** indicates p-value<.01.

Table 3: Effect of Dimension Rank on Venture Outcomes

Dependent variable:	Financing after round		3+ Employees		10+ Employees		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). Note that competition f.e. control for a specific date. ††The attractiveness and size of the market. *** indicates p-value<.01.

Table 4: Effect of Negative Feedback on Venture Continuation

Panel 1

Dependent variable: Survival*

				Prelim rounds only	No incorp. ventures	Low rank among non-winners defined as: Bottom 3 deciles	Bottom 7 deciles	Deciles 5-8 (9-10 omitted)	Positive feedback among winners
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Low rank·Feedback	-.086**	-.084***	-.079***	-.12***	-.12**	-.062**	-.097**	-.079*	
	(.036)	(.02)	(.026)	(.044)	(.058)	(.029)	(.04)	(.046)	
Low rank	-.062***	-.051***	-.026	-.051**	-.036	-.065***	-.048**	-.025	
	(.021)	(.014)	(.022)	(.023)	(.048)	(.019)	(.022)	(.025)	
Feedback	.066*	.17*	-.03	.11**	.09*	.032	.073*	.075*	-.032
	(.04)	(.092)	(.14)	(.045)	(.053)	(.028)	(.043)	(.043)	(.068)
High rank·Feedback									.11*
									(.06)
High rank									.029
									(.046)
Venture controls†	Y	Y	Y+‡	Y	Y	Y	Y	Y	Y
Year f.e.	Y	N	N	Y	Y	Y	Y	Y	Y
Judge f.e.	N	Y	Y	N	N	N	N	N	N
N	3751	26443	14915	2689	1962	3751	3751	2372	1335
R^2	.082	.18	.29	.083	.051	.081	.081	.097	.14

Note: This table shows estimates of the effect of negative feedback within the sample of non-winners (having a below-median rank among non-winners when non-winners 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 non-winners. Errors clustered by competition-round or judge, depending on fixed effects. Sample restricted to non-winners of round, except in column 10. * Survival is 1 if the venture had ≥ 1 employee besides founder on LinkedIn as of 8/2016. †Includes sector indicator variables, student and company incorporation status. ‡Also includes company age and whether the company received investment before the round. *** indicates p-value$<$.01.

Panel 2

Dependent variable:	Abandoned within...			Survival*						
	.5 year	1 year	2 years	Z-scores	Logit	Exact matching±	Propensity score matching**	Nominal score		
										Feedback only
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Low rank·Feedback	.079*	.085**	.087**	-.086**	-.32**	-.076***	-.056**		-.093**	
	(.041)	(.041)	(.039)	(.036)	(.16)	(.027)	(.022)		(.04)	
Low rank	.056***	.06***	.058***	-.065***	-.31**				-.047*	
	(.021)	(.022)	(.022)	(.021)	(.16)				(.026)	
Feedback	-.0074	-.031	-.056	.07*	.23				.082	
	(.042)	(.042)	(.04)	(.039)	(.17)				(.05)	
Z-score				.04						
				(.029)						
Z-score^2				-.013**						
				(.0067)						
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	Y	Y	N	Y	-	Y	Y	Y	Y
Judge f.e.	N	N	N	N	N	N	N	N	N	N
N	3751	3751	3751	3751	3751	2484	3357	3305	2974	2028
R^2	.061	.06	.073	.084	0.065	-	.095	.071	.086	.085

Note: This table shows estimates of the effect of negative feedback as in the previous panel, but with alternative samples. *Survival is 1 if the venture had ≥ 1 employee besides founder on LinkedIn as of 8/2016. The dependent variable in columns 1-3 is 1 if the venture was abandoned within the given time frame after the competition (see Table 1 for definition). ±Causal effect via exact matching between "treated" group (low-ranked non-winners who received feedback) and control group (low ranked non-winners who did not receive feedback) on sector (there are 16 sectors), year, student and company incorporation status. **Causal effect via propensity score (logit prediction of treatment) matching of treated and control groups. †Includes sector indicator variables, student and company incorporation status. *** indicates p-value$<$.01.

Table 5: Heterogeneity in Effect of Negative Feedback

Panel 1

Dependent variable: Survival*

Characteristic C_i:	Founder female		IT/software		Social/clean tech		Financing before round	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Low rank·Feedback· C_i	-.1	-.057	-.1*	-.097	.072	.085	.15*	.15*
	(.096)	(.094)	(.062)	(.063)	(.088)	(.095)	(.087)	(.088)
Low rank·Feedback	-.093**	-.083**	-.015	-.016	-.1**	-.1**	-.1**	-.1**
	(.039)	(.042)	(.043)	(.043)	(.042)	(.042)	(.041)	(.042)
Feedback· C_i	.12	.11	-.00096	-.016	-.089	-.08	-.19***	-.19***
	(.073)	(.072)	(.058)	(.057)	(.08)	(.087)	(.067)	(.066)
Low rank· C_i	.071	.037	-.0035	-.0021	.028	.036	-.033	-.052
	(.045)	(.042)	(.038)	(.039)	(.047)	(.047)	(.069)	(.069)
Low rank	-.079***	-.058**	-.038*	-.04*	-.051**	-.052**	-.047**	-.031
	(.025)	(.023)	(.023)	(.023)	(.023)	(.023)	(.022)	(.02)
Feedback	.059		.057		.1**		.11**	
	(.045)		(.038)		(.041)		(.042)	
C_i	-.11***	-.079**	.09**	.11***	-.098**	-.13***	.37***	.39***
	(.039)	(.037)	(.037)	(.038)	(.042)	(.042)	(.054)	(.053)
Year f.e.	Y	Y	Y	Y	Y	Y	Y	Y
Sector f.e.	Y	N	N	N	N	N	Y	N
Competition f.e.	N	Y	N	Y	N	Y	N	Y
N	3048	4121	4136	4136	4136	4136	3765	4136
R^2	.1	.084	.12	.14	.077	.092	.13	.13

Panel 2

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
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. Control coefficients not reported for brevity. *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 6: Effect of Negative Dimension Feedback on Venture Continuation

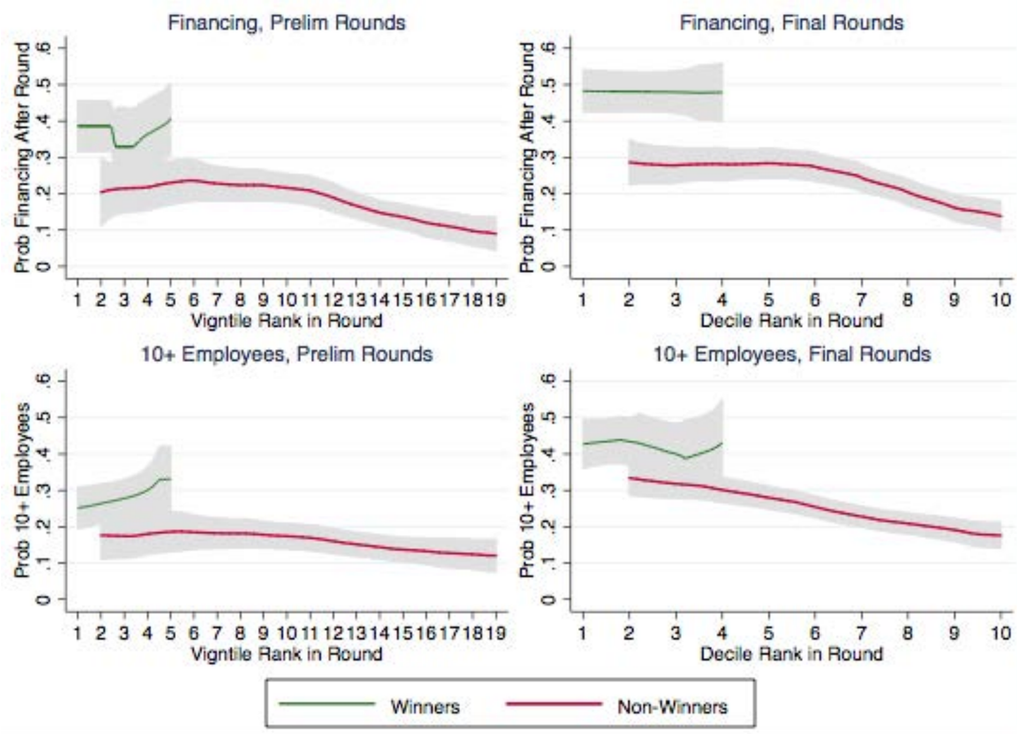
Sample restricted to non-winners of round

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

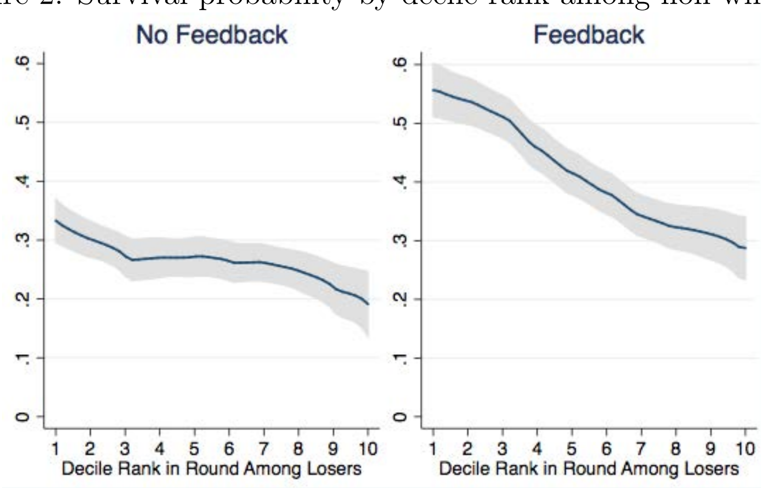
Note: This table shows estimates of the effect of negative feedback within dimensions. Errors clustered by competition-round. [†]Includes sector dummies, whether venture incorporated, and whether founder is student. [††]Market attractiveness and size. *** indicates p-value$<$.01.

Figure 1: Effect of winning (Lower percentile rank is better)



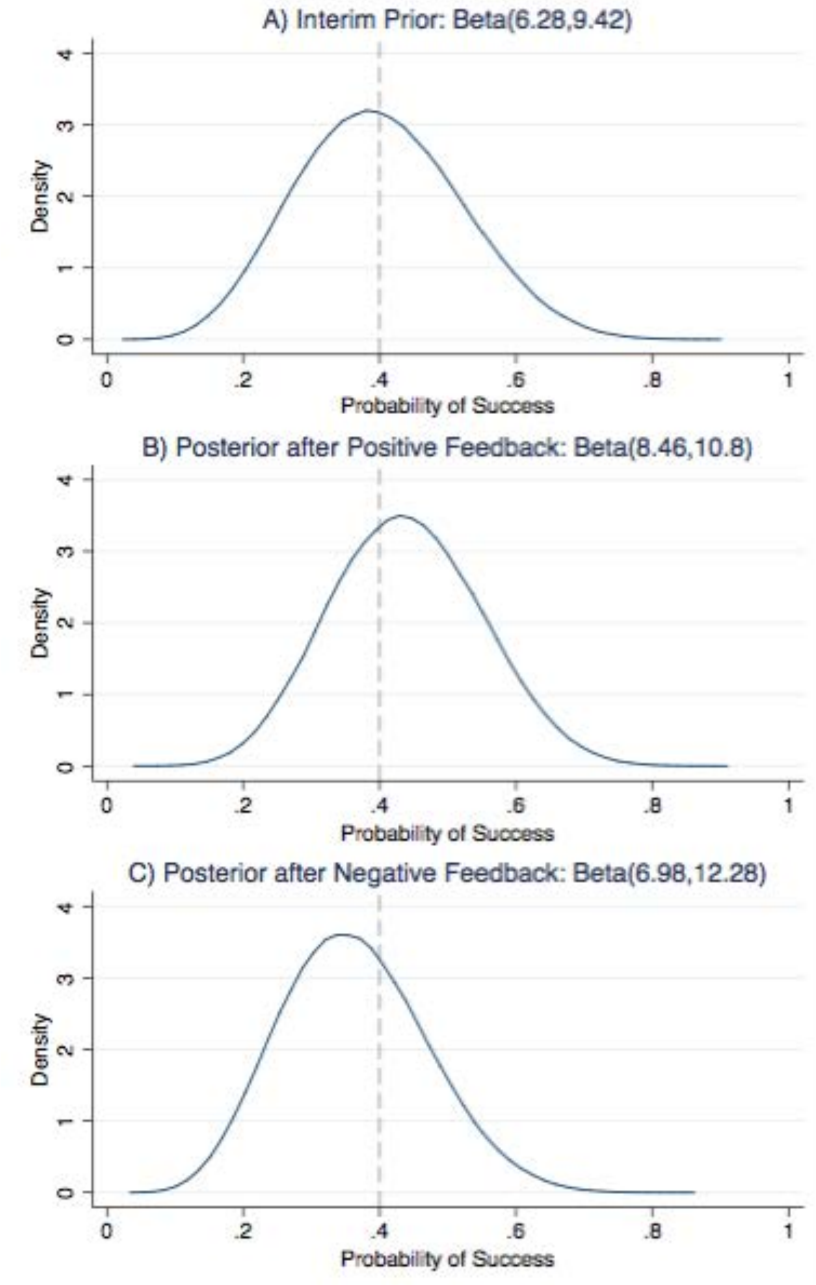
Note: This figure shows probabilities of any subsequent financing (top) and having 10+ employees (bottom) by percentile rank in the round. Local polynomial with Stata's optimal bandwidth; 95% CIs shown.

Figure 2: Survival probability by decile rank among non-winners



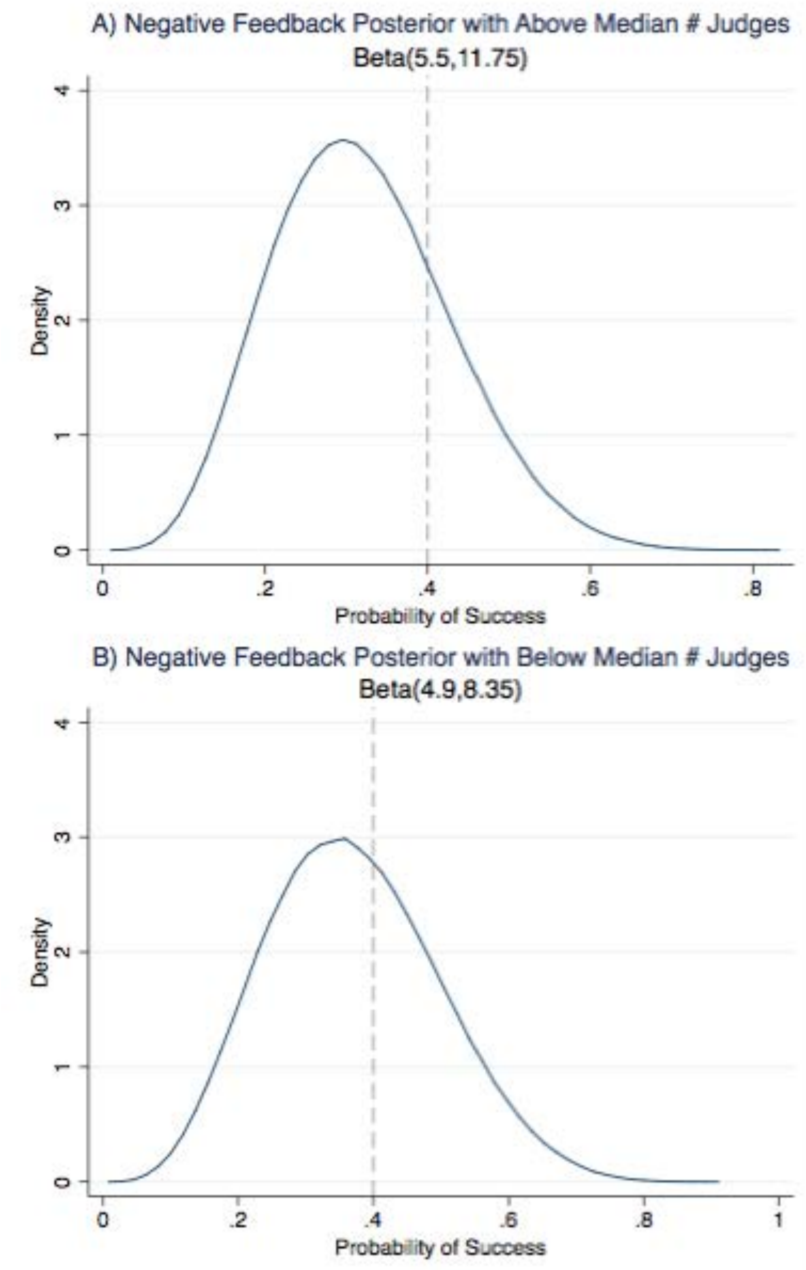
Note: This figure shows the probability of survival among non-winners in preliminary rounds, by percentile rank in the round. Local polynomial with Epanechnikov kernel; 95% CIs shown.

Figure 3: PDFs of interim prior and average posteriors after positive and negative feedback



Note: This figure is based on Equation 4 in the Online Appendix. It simulates Beta distributions using 1 million randomly generated numbers. The prior mean is the realized outcome for uninformed exactly matched losers (losers in the no-feedback competitions matched on observables to losers in the feedback competitions). The shape parameters in the bottom two figures reflect average k_i and J_i (success signals and number of judges) among above median losers (positive feedback) and below-median losers (negative feedback).

Figure 4: PDFs of interim prior and average posteriors after positive and negative feedback



Note: This figure simulates Beta distributions using 1 million randomly generated numbers. The prior mean is the realized outcome for uninformed exactly matched losers (losers in the no-feedback competitions matched on observables to losers in the feedback competitions). The shape parameters in the bottom two figures reflect average k_i and J_i (success signals and number of judges) among above median losers (positive feedback) and below-median losers (negative feedback).