Learning from Feedback:

Evidence from New Ventures

5/18/2017

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Abstract

Despite the importance of learning to theories of firm entry and exit, there is evidence that entrepreneurs are overconfident, failing to update their beliefs in response to new information. I assess the effect of negative feedback on early stage startups using application and judging data from 96 new venture competitions, some of which privately inform ventures of their relative rank. I use a difference-in-differences design as well as two matching estimators to compare lower and higher ranked losers, across competitions in which they did and did not observe their standing. Receiving negative feedback increases venture abandonment by about 26 percent. Feedback also seems to increase the efficiency of serial entrepreneurship. Responsiveness varies by venture technology type, stage and risk, as well as founder education and other dimensions. This heterogeneity has implications for theories of innovation and firm dynamics.

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For especially useful comments, I thank Manuel Adelino, Augustin Landier, Josh Lerner, David Robinson, Annette Vissing-Jørgensen, and seminar participants at the NBER Entrepreneurship Working Group. I am also grateful to the Kauffman Foundation, which funded this project. Finally, I thank Adam Rentschler of Valid Evaluation, and all the others who provided the data, including Lea Lueck, Allison Ernst, and Catherine Cronin. Lucy Gong, Sreyoshi Mukherjee, and Jack Reiss provided excellent research assistance.

"The distance is so great between our present psychological knowledge of the learning and choice processes and the kinds of knowledge needed for economic and administrative theory that a marking stone placed halfway between might help travellers from both directions to keep to their courses."
-- Herbert Simon, *The Quarterly Journal of Economics*, 1955

1 Introduction

Models of firm dynamics and occupational choice often rely on learning assumptions (e.g. Jovanovic 1982, Hopenhayn 1992, Ericson & Pakes 1995, Aghion & Howitt 2006). In these and other models, entrepreneurs enter an industry and incumbent managers exit in response to new information about the net present value of the enterprise.[1] In parallel, a recent strand of the entrepreneurship literature emphasizes the importance of experimentation and adaptability (e.g. Manso 2016, Kerr et al. 2014).[2]

Yet it is unclear that belief sensitivity to new information is an important feature of high-growth entrepreneurship. Even among founders of venture capital-backed startups, the returns to entrepreneurship seem low (Moskowitz & Vissing-Jørgensen 2002).[3] Cognitive biases, particularly overconfidence, may explain the low returns. This behavioral view implies that entrepreneurs enter irrationally and fail to update their priors in light of new information.[4] One example of how such evidence is incorporated into theory is the contracting model in Landier & Thesmar (2009), in which entrepreneurs receive nothing if the venture fails because they assign this event zero probability.[5] A second example is Bergemann & Hege (2005)'s R&D investment model. They note that "entrepreneurs express

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

[2] Also see Dillon & Stanton (2016), McGrath (1999), and Stern (2006).

[3] Also see Hamilton (2000), Hall & Woodward (2010), and Hurst & Pugsley (2015).

[4] For example, Cooper et al. (1988), Camerer & Lovallo (1999), Arabsheibani et al. (2000), and Koellinger, Minniti & Schade (2007) find evidence of entrepreneur overconfidence. Astebro, Jeffrey & Adomdza (2007) find that inventors fail to respond to negative feedback. Non-pecuniary benefits may also play a role (Hurst & Pugsley 2011, Hvide & Møen 2010 and Giannetti & Simonov 2009). See Astebro et al. (2014) for a review.

[5] Landier and Thesmar (2009) assume a form of Bayesian updating in which the entrepreneur ignores negative feedback at an interim stage.

a strong preference for continuation regardless of present-value considerations."

This paper offers new empirical evidence of a particular type of managerial learning: the effect of negative feedback on entrepreneurial outcomes. Feedback might not be useful if it does not contain new, actionable information, or if founders are unresponsive for behavioral reasons. I show that nascent entrepreneurs on average are quite responsive to new and credible - but low stakes - information about their project quality.

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

For both types of competitions (with and without feedback), I observe all ranking and scoring information. The ranks and scores are otherwise confidential to the competition organizers, and is not revealed to judges or other ventures.[6] Information about ventures and their founders comes from LinkedIn, AngelList, CB Insights, and other sources. Most founders are first-time entrepreneurs seeking external finance in order to grow quickly. I show that the ventures and founders in my data are roughly representative of the U.S. population of early stage startups and entrepreneurs.

I assess the causal effect of negative feedback among losers in a round (e.g. "the semifinals") with a difference-in-differences specification. 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. 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.

[6] There may additionally be unobserved informal, oral feedback, but in general its level does not vary systematically across the two sets competitions compared in this paper.

There are two empirical concerns. First, to be a credible signal, ranks must be relevant to venture outcomes. In a regression discontinuity design, I show that conditional on win status, both percentile rank and z-score robustly predict measures of success like subsequent external financing, employment, and undergoing an acquisition or IPO. Second, losers in the two types of competitions may have distributional differences around the median. I use visual and statistical evidence to demonstrate that the distributions of observable characteristics are not systematically different around the loser median. I also use exact and propensity-score matching estimators to ensure that such distributional differences do not explain the main effect.

If founders' beliefs are very sensitive to new information about their project quality, negative feedback in the competitions should reduce the likelihood of venture continuation. Alternatively, founders may be overconfident; given the low stakes of the competitions, I expect negative feedback to have little effect on overconfident founders. I find that receiving negative feedback reduces the probability of survival by about about eight percentage points, or 23% of the mean.[7] This effect persists using the matching estimators, with polynomials in z-score, as well as within a single program that employed feedback in one year but not others.

Among founders that abandon their ventures, I examine the time to abandonment and predictors of serial entrepreneurship.[8] Founders likely have more valuable outside options, such as those with a top ten MBA or PhdD, abandon much quicker conditional on abandoning. Combined with the two findings that scores robustly predict commercial outcomes even in the non-feedback competitions, and ventures are very responsive to feedback when they get it, I conclude that low-stakes feedback seems useful, helping the nascent entrepreneur determine whether a project's expected value exceeds his outside options. In a social

[7]The proxy for venture survival I use is an indicator for the venture having at least one employee besides the founder on LinkedIn as of August, 2016.

[8]Time to abandonment is calculated using new job start dates on LinkedIn, and serial entrepreneurship is defined as founding or being the CEO of a subsequent venture (after substantial effort to account for venture name changes).

welfare sense, this suggests that at least some entrepreneurs would benefit from more information about their ventures' quality. My primary specification find that feedback increases abandonment by about 12%; to make this specific, it means that had the 1,453 unique below-median losers in the no-feedback competitions in my data received feedback, an additional 174 would have been abandoned - or should have been abandoned, to the degree that the feedback scenario must be weakly more efficient.

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

My results confirm the emphasis on uncertainty in all three approaches, and align most closely with models of active learning before entry. For example, I find greater responsiveness to feedback among firms that are not yet incorporated, are software-based, are student led, and that have not yet received outside financing.[9] This suggests that firm boundaries are in part defined by initial learning about project success probabilities, and also that more experimentation is feasible when it has a lower cost. Together with the absence of heterogeneity in founder or venture age, the results imply that ventures with market-based information about their own high type are more likely to dismiss a low ranking as noise.

Heterogeneity analyses reveal that ventures with more information about their own high type are less responsive. They are also less responsive when there is more variance in judge scores, but not when this variance is predicted using

[9]This result is not explained by correlation between software and other characteristics, like expected non-pecuniary benefits. I find no effect of founder or venture age.

a leave-one-out leniency measure as a naive instrument. This hints that entrepreneurs with riskier, radical technologies are less responsive to feedback than those with incremental ideas. It may be possible to micro-found technological discontinuities in the small fraction of entrepreneurs that enter without regard to signals about future cash flows, and occasionally transform the industry. The mass of entrants could remain rational and responsive to new information.

Criteria scores and judge types illuminate how negative feedback affects outcomes. As in Bernstein, Korteweg & Laws (2015), the strongest predictor of success is the team (management quality) rank. I find that negative feedback affects continuation along the financials, business model, market, and team dimensions, but not the product/technology or presentation dimensions. Founders may have better private knowledge than judges about their product quality and scope for improvement in presentation skills, while judges have valuable insight into business viability.

My results are in general consistent with a rational rather than a behavioral view of entrepreneurial behavior. As in Vereshchagina & Hopenhayn (2009), bad news leads the entrepreneur to exit to a more valuable outside option. In their model, the presence of the outside option helps to resolve the "private equity premium" puzzle. I show that type revelation is an important part of the entry process.

This paper focuses on new ventures close to their moment of founding. Despite the importance of startups to economic growth, a lack of data has left these firms relatively understudied in their earliest phases (Haltiwanger, Jarmin & Miranda 2013, Guzman & Stern 2016). Further, much of the venture capital literature assumes the entrepreneur knows his type, and focuses on information asymmetry, governance, and occupational choice (e.g. Hochberg, Ljungqvist & Lu 2007, Bernstein, Giroud & Townsend 2015, Ewens & Rhodes-Kropf 2015). I extend and depart from this literature by focusing on the entrepreneur's own uncertainty about his type.

Manso (2011) models innovation as learning through a series of experiments. The optimal contract to encourage exploration rewards long term success but tolerates early failure, and it reduces the cost of experimentation by providing timely feedback about performance. Competitions provide nascent entrepreneurs with failure-tolerant, timely feedback. While they reward top performers, they do not penalize especially poor performance, as ranks are private.[10] The regression discontinuity design that demonstrates the predictive power of rank tangentially shows that winning increases subsequent external financing, employment, and the chances of an IPO or acquisition, even in preliminary rounds.[11] This supports Manso's theory, in a similar vein as Lerner & Wulf (2007), Azoulay et al. (2011), and Tian & Wang (2014).

Beyond the work cited thus far, this paper contributes to the literatures on the relationship of executive characteristics and corporate decisions (Bertrand & Schoar 2003, Graham, Harvey & Puri 2013); financial constraints facing startups and the evaluation of policies to alleviate them (Howell 2017, Ozmel, Robinson & Stuart 2013, Schmalz, Sraer & Thesmar 2015); peer effects in entrepreneurship (Nanda & Sørensen 2010, Lerner & Malmendier 2013, Guiso et al. 2015), and predicting startup success (Scott, Shu & Lubynsky 2015).[12]

The paper proceeds as follows. Section 2 describes the competitions. It summarizes the data and discusses its representativeness. Section 3 explains the empirical approach, and Section 4 contains the results. Section 5 concludes.

2 The new venture competition context

This paper contributes to the entrepreneurship literature by introducing a new source of data. New venture "pitch" competitions have proliferated in the past

[10]Overall ranks are never publicized. They are aggregated from judge ranks. Judges do not know others' ranks, though it is possible that they converse after the competition.

[11]This paper also relates to recent work on the role of feedback in other settings, such as Gross (2016) and Ganglmair et al. (2016).

[12]The first topic also includes Lazear (2005), Gabaix & Landier (2008), Gompers, Kovner, Lerner & Scharfstein (2010), Shane et al. (2010), Kaplan, Klebanov & Sorensen (2012), and Lindquist, Sol & Van Praag (2015). Also relevant is recent work on new startup resources (e.g. Winston Smith et al. 2013, Yu 2014, Hallen et al. 2014, and Eesley & Wu 2016).

decade and are often publicly funded.[13] Data from these competitions permits us to observe new ventures and their founders at an earlier stage, with greater granularity, and in a larger sample than extant studies. Survey or population data sources such as the Survey of Consumer Finances, the National Longitudinal Survey of Youth (NLSY79), or the Business Dynamics Statistics contain relatively few high-growth entrepreneurs, as Levine & Rubinstein (2013) point out. For example, the NLSY79 has roughly 5,400 workers, only about 10% of which were ever self-employed. Further, in these databases it is difficult to distinguish high-growth, young firms from local small businesses (e.g. restaurants, plumbers, and self-employed accountants).

In contrast, the participants in new venture competitions are effectively never subsistence businesses or sole proprietorships. They are deciding whether to found a startup or have very recently founded a startup. By definition, a startup is a temporary entity that aims to grow quickly, and usually to transform an industry. While most will fail, the few that do succeed will be very valuable.

Data description

New venture competitions, which sometimes describe themselves as competitive accelerators or business plan competitions, are an intermediary between entrepreneurs and investors; they act as certifiers, conveners, and sometimes educators. They are sponsored by universities, corporations, foundations, governments at the federal, state, and city level, angel investor groups, and others. In a competition, new ventures present their technologies and business models to a panel of judges.

New venture competitions are an important part of the startup ecosystem, particularly for first-time founders. CB Insights (the most comprehensive early stage financing database for recently founded startups) contains about 16,000 ventures that got their first early stage financing between 2009 and 2016, of

[13] 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.

which 14.5% won a new venture competition or competitive accelerator. Bernstein, Korteweg & Laws (2015) note that on the AngelList platform, 57% of their experimental sample and 30% of all startups listed on AngelList have been through an accelerator or incubator. There is no formal count of new venture competitions, but one startup resource provider listed 4,623 competitive events as of September, 2016.[14] Another listed 382 business plan competitions with $48 million in prize money in 2015 alone.[15]

The sample of 96 competitions in this paper is not accidental; they are similar enough to be evaluated together, but offer variation in the feedback they give to participants, in location, and in venture characteristics.[16] All the competitions have the following features:

1. They include a pitch event, where the company presents its business plan;
2. They involve formal judging, in which volunteer judges score the company and these scores are recorded;
3. Specific participants are publicly announced as winners, but no loser ranks are made public;
4. The sponsoring organization does not take equity in the participating or winning ventures.[17]
5. The sponsoring organization explicitly seeks to enable winners to access subsequent external finance.

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[14] See https://www.f6s.com/.

[15] See http://www.bizplancompetitions.com.

[16] 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.

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

The data are summarized in Table 1, and the individual competitions are listed in Appendix Table A1. Competition dates range from 1999 to 2016. As Table 1 Panel 1 describes, there are 214 rounds (where a round might be the semifinals in a given competition).[18] On average there are 44 ventures in a preliminary round, and 18 ventures in a final round. The average number of winners is 4.5, and the average award amount conditional on receiving a cash prize is $66,000.

I observe overall firm ranks in a round, as well as the judge-specific ranks from which overall ranks are derived. Some competitions calculate the judge-specific rank as an average of judge-dimension specific ranks. The main dimensions (or "criteria") are Team, Financials, Business Model, Market Attractiveness, Technology/Product, and Presentation. Different competitions use different score ranges, and the number of of ventures varies across rounds. For much of the analysis, I convert raw scores to percentile ranks; primarily deciles. I also use z-scores. (See Appendix Table A2 for statistics on the various levels of scores.)

I chose competitions for analysis that yielded an important econometric advantage: because only some competitions inform ventures of their ranks after the round, it is possible for the econometrician to observe more than the agents under study. Thirty-five of the programs provide structured feedback through software from Valid Evaluation, a private company. These competitions inform ventures of their overall and dimension ranking relative to other ventures in their round overall.

In none of the competitions do ventures learn judge-specific scores, nor do judges ever observe each other's scores. In the non-feedback competitions, ventures learn only that they won or lost, not their rank or score. Although each competition is unique, there are no systematic differences in services provided (e.g. mentoring, training) across the two competition types. In no case did a competition with feedback advertise itself as providing the relative ranks or especially copious feedback in advance, so there is no reason to believe that ventures

[18] A few competitions divide preliminary rounds into panels. For example, the roughly 40 startups participating in the first round of each year's Rice Business Plan Competition are divided into about seven panels of around six startups and 25 judges each.

with greater informational needs would have selected into these competitions. In Section 3, I test for differences across competitions with and without structured feedback.[19]

The 4,328 unique ventures in the data are described in Table 1 Panel 2, and are categorized by sector and technology type in Table 2 (and by state in Appendix Table A3). There are 558 ventures that participate in multiple competitions. In the main analysis, I consider only a venture's first competition. The average age of the ventures is 1.9 years.[20] 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. These yielded 752, 638, 1,528, and 1,933 unique company matches, respectively.[21] The probability of subsequent financing is 0.24, relative to 0.16 before the competition-round. I focus on subsequent angel investment and initial venture capital (VC Series A rounds) as a success metric because it is a good indicator of commercial potential for high-growth startups, about which data are otherwise sparse. I proxy for continuation with whether a venture has at least two employees as of August, 2016 (mean is 0.34). The probability that a venture has an active website as of September, 2016 is 0.63. Note that in the analysis, competition fixed effects will control for date, obviating truncation concerns. Three percent of ventures were acquired or went public.

Founders are described in Table 1 Panel 3, using data from the competitions and LinkedIn profiles. Of the 3,643 team leaders (listed either as CEO or team leader on the competition application), 2,554 matched to a LinkedIn profile that contains data on experience, education, or both. The average founder had 4.4 jobs prior to the round, in 2.7 locations. Forty-eight percent of founders have

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

[20] 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.

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

an MBA, a little more than half of which are from top 10 programs (based on U.S. News & World Report rankings in Appendix Table A4). Twenty-seven percent of founders graduated from a top 20 college. I also divide the college majors into groupings; the largest is engineering, with 484 founders.[22] Genders were assigned to founder names using a publicly-available algorithm. Unclear cases, such as East Asian names, coded by hand. Twenty-one percent of founders are women, and 72% are men (the remaining 7% had both ambiguous names and no clear LinkedIn match).

Among founders that abandoned their ventures, 39% describe themselves as the founder or senior executive of a subsequent venture. I use this as an indicator for subsequent serial entrepreneurship. Almost no founders had a prior venture. Effort was made to ensure that this is not spuriously engendered by name changes; about 17.6% of ventures in the competitions, or around 765 companies, were found to change their names. To the degree that my sample is representative of early stage startup founders in general (see below), and there is not too much noise from name changes, this suggests that there is a meaningful share of founders inclined to pursue entrepreneurship as a career, rather than pursue a one-off idea. On average, within the pool of abandoned ventures, serial entrepreneurship is correlated with quality as measured by judge scores. The correlations between serial entrepreneurship and decile rank (z-score) are -.14 (.21).

The founder "time to fail" metric is constructed as follows. First, I take the set of ventures that did not survive, measured as not having at least one person other than the founder identify as an employee on LinkedIn. Within this group, I find the number of days between the competition's end date and the first subsequent new job start date. The average (median) is 313 (148) days. For some specifications, I use a binary "abandoned fast" variable, which is 1 if the abandonment time is above the median.

Judges participate in order to source deals, clients, or job opportuni-

[22] The data are not always clean; one founder identified his/her major as "Persuasion - The Science and Art of Effective Influence."

ties. They also sometimes describe judging as a way to "give back" to the entrepreneurial ecosystem. There are 2,514 unique judges, whom I have parsed by profession where I have the judge name, job title, and company. I consider nine occupations, listed in Table 2. The largest group is venture capital investors, with 676 judges. There is concern that any impact of the competitions on venture financing might be contaminated by the judges themselves investing. Careful comparison of funded ventures' investors and judges revealed 95 instances in which a judge's firm invested in the venture, and 3 instances in which the judge personally invested, relative to more than 51,000 judge-venture pairs.

Representativeness of the data

There is little empirical analysis of startups prior to their first external funding event or of new venture competitions, so it is difficult to assess the representativeness of the sample. Appendix Table A5 compares the distribution of ventures in my data to overall U.S. VC investment, based on the National Venture Capital Association's (NVCA) 2016 yearbook. The share of software startups in my data, 37%, is very close to the national average for both deals and dollars of 40%. However, in part because of data from the Cleantech Open, a national non-profit competition focused on clean energy startups, the data skews somewhat towards clean energy. With the exception of Arizona, which is oversampled in my data due to the presence in my data of the large Arizona Innovation Challenge, the top twenty states in my data almost entirely overlap with the top twenty states for VC investment.

The competitions take place in 17 U.S. states. The VC industry is concentrated in California, New York, and Massachusetts; in 2015, these states accounted for 77% of total U.S. VC investment, and 80% of VC deals.[23] Ventures in these states - 35% of the sample - have access to richer networks of investors, advisers, and other resources. Relative to the NVCA data, my data under-samples

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

California and over-samples Massachusetts. Many successful startups that raise VC move to Silicon Valley, so this is perhaps to be expected from earlier stage firms. Nonetheless, the location and sector statistics suggest the sample may be biased towards more marginal ventures than the universe of startups at hazard of VC investment.

The probability of an IPO or acquisition in my sample, 3%, is roughly similar to the 5% found in Ewens & Townsend (2017)'s sample of AngelList startups. The average number of team members in my data 3. 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 the sample is representative of startup founders in terms of age depends on the comparison set. The founders in my data are older than the average Y-Combinator founder, who is just 26.[24] Wadhwa et al. (2009), on the other hand, find that the average age of entrepreneurs of successful, high-growth startups is 40. Analysis of startups valued at $1 billion or above between 2003 and 2013 by Cowboy Ventures found the average entrepreneur age at company founding was 34 (Lee 2013).

Which venture and founder characteristics predict success?

The data permit descriptive statistics that are, to my knowledge, new to the world on the relationship between early stage entrepreneur and venture characteristics and subsequent success. Appendix Table A6 panel A contains estimates of projecting success proxies on vectors of characteristics. The dependent variables are subsequent angel/VC investment, and having at least 10 employees as of August, 2016.[25] Columns 2 and 4 exclude venture and founder age, which are not available for many ventures.

The associations differ across the two outcome metrics, sometimes dramatically. For example, attending a top 10 college is associated with a 5-6 pp increase

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

[25] Too few ventures have thus far exited through IPO or acquisition for this to be a useful variable.

in the probability of angel/VC, but is only noisily associated with having at least 10 employees. In contrast, 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. 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.

When considered independently, founders from top 10 colleges are much more likely to succeed. Figure 7 contains coefficients from regressions of each outcome on the two indicators for elite education and competition fixed effects. Having a top 10 college degree is strongly associated with success, recalling a similar relationship between college selectivity and success for CEOs of VC-backed companies in Kaplan et al. (2012). However, having a top 10 MBA degree has no association with success.

Appendix Table A6 panel B shows the association between 17 venture sectors and success. The base sector is "Air/water/waste/agriculture". Software and education ventures are relatively more likely to succeed for both outcomes, while social enterprise and biotech ventures are not. Media and entertainment ventures are far more likely to raise Angel/VC, but are not measurably more likely to reach 10 employees. A similar exercise using college majors does not find robust differences in success rates across majors, though majoring in either entrepreneurship or political science/international affairs is weakly associated with success.

3 Analytical approach to learning

Managerial learning is challenging to study rigorously because it is abstract and often subjective. New venture competition data are well-suited to this task. The new, external information that entrepreneurs receive is feedback from judges about the quality of their ventures. This feedback is codified in scores or ranks. I observe these scores, but the entrepreneurs observe only mappings of the scores;

in some cases, they learn their overall rank in the round, and in other cases, they learn only that they won or lost.

Signal informativeness

If the judges cannot predict success, rational founders have nothing to "learn" from their feedback. That is, if ranks are to be useful in studying learning, they must be relevant to firm outcomes. In a regression discontinuity design, I show that judge scores are informative about venture outcomes, and that winning a competition is useful. Since winning is useful, founders should be expected to try to improve across rounds within a competition, and across competitions when they compete in more than one.

I establish that the competitions generate valuable, informative signals by estimating variants of Equation 1.

$$Y_i^{Post} = \alpha + \beta_1 WonRound_{i,j} + f\left(Rank/Z - score_{i,j}\right) + \beta_2 AwardAmt + \gamma' \mathbf{f.e.}_{j'/k} + \delta' \mathbf{X}_i + \varepsilon_{i,j}$$
$$\mathbf{X}_i = [prior\ fin,\ judge\ invested,\ sector\ f.e.,\ venture\ age,\ team\ size] \tag{1}$$

Here, i indicates a venture, and j a competition-round-panel (e.g. the MIT Clean Energy Prize Semifinals). The dependent variable Y_i^{Post} is a measure of venture success, such as whether it had 10 or more employees by August, 2016, or whether it raised angel/VC series A investment after the round. $WonRound_{i,j}$ is an indicator for whether the venture was a winner in the round. In the baseline empirical analysis, I include competition-round-panel or judge fixed effects.[26] The former absorb the date and location. Among the controls, I include an indicator for whether the judge or judge's company ever invested in the venture, and an indicator for whether the company previously raised external financing, and the number of team members. I cluster standard errors by competition-round-panel or by judge.

The coefficient on percentile rank or z-score measures signal informative-

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

ness. I primarily use decile ranks, either within losers and winners or in the round overall. For example, the variable "decile rank in round among losers" divides the losers in a round into ten groups, where the group with the best ranks is in 1, and the worst in 10. Some specifications use judge decile ranks, which is the venture's decile rank among ventures the judge scored. A negative coefficient on decile rank indicates that judge ranks are positively predictive of the success metric. 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.[27]

A tangential benefit of this design is that it contributes to the program evaluation literature, providing to my knowledge the first multiple-program causal assessment of the benefit of winning a round or a competition. To do this, I estimate versions of Equation 1 that provide a closer approximation to a regression discontinuity design; I limit the sample to ventures immediately around the cutoff, and use polynomials in rank or score.

The primary empirical concern is that judges may sort firms on unobservables around the cutoff. This is unlikely. Although the number of awards is generally known ex-ante, judges score independently. Also, they typically only score a subset of the participating ventures. Thus they cannot sort the firms around the cutoff. The judges' scores are averaged to form the overall score, which determines which firms move forward and win. Sometimes the judges discuss which among the teams they observed to send forward, but this occurs after they have independently entered scores electronically or on score-sheets. Judges individually do not rank or score candidates; they provide numeric scores or ranks and do not know what the "high score" in a competition will be. Thus there is little means for sorting to happen ex-ante.

One limitation of this study from a policy perspective is that the evaluation is limited to participating firms. Accelerators may have region- or sector-wide effects beyond the companies that participate. For example, the mere presence

[27] 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.

of a business plan competition at a university might make students more likely to become entrepreneurs. Fehder & Hochberg (2014) address this issue by comparing regions with and without accelerators. They find that the presence of an accelerator in a region increases financing events for non-accelerated firms. Unfortunately, this is beyond the scope of the present study.

Estimating responsiveness to feedback

The ideal experiment to assess responsiveness is to randomly allocate feedback across ventures within rounds. I approximate this by comparing competitions where ventures receive structured feedback - they learn their rank relative to other participating ventures - with competitions where ventures learn only that they won or lost. In the latter competitions, feedback is much less precise; it is informal and disconnected from peer performance. I ask whether ventures that receive especially negative feedback are more likely to be abandoned. This empirical approach provides the first causal relationship between feedback to entrepreneurs or managers and firm real outcomes.

The empirical design is a difference-in-differences model within the population of losers. The first difference is between above- and below-median losers in a given competition. The second difference is across structured feedback and non-structured feedback competitions. That is, I estimate among losers the combined effect on the entrepreneur of receiving a below-median score, and knowing that he received a low score:

$$\begin{aligned} Y_i^{Post} &= \alpha + \beta_1 \left(\mathbf{1} \mid LowRank_{i,j}\right)\left(\mathbf{1} \mid StructuredFeedback_j\right) + \beta_2 \left(\mathbf{1} \mid LowRank_{i,j}\right) \\ &\quad + \beta_3 \left(\mathbf{1} \mid StructuredFeedback_j\right) + \gamma' \mathbf{f.e.}_{j'/k} + \delta' \mathbf{X}_i + \varepsilon_{i,j} \qquad (2) \\ &\quad \text{if } i \in Losers_j \end{aligned}$$

The coefficient of interest in Equation 2 is β_1. I similarly estimate whether there is a symmetric effect for especially positive feedback among winners. I am able to study heterogeneity by adding a venture characteristic as a third interaction, controlling for the three individual effects and the three two-way interactions.

A concern with this approach is that the structured feedback and non-structured feedback competitions may be different, for example attracting different types of ventures. Note, however, that the control group is the above-median losers within a round in both types of competitions. Therefore, average differences across the types of competitions are differenced out. I also construct

Evidence of Distributional Similarity by Structured Feedback Status

While average differences across the two competition types (structured feedback and non-structured feedback) is not a concern, distributional differences among losers could bias the results. That is, it could be problematic if the distribution of losing ventures around the median is systematically different along dimensions that are not captured by the controls. The average difference in quality between low and high ranked losers could be different across the two types of competitions. Figures 5-6 and Tables 3-4 provide evidence that among losers at the time of the competition, the distributions of observable characteristics seem similar.

Visual evidence is in Figures 5 and 6, which contain spikes representing the fraction of ventures within narrow z-score bandwidths for various observables at the time of the competition. I sum the total number of incorporated companies in all structured feedback competitions. Then, again for only structured feedback competitions, I sum within a 0.1 z-score bandwidth the number of incorporated companies. Finally, I divide the second sum by the first. Thus, if Inc_i is an indicator for a company being incorporated, the bar height for a z-score 0.1 band of z in structured feedback competitions is: $\frac{\sum_{z,SF} Inc_i}{\sum_{SF} Inc_i}$. Note that these distributions are across all rounds in the structured feedback and non-structured feedback categories; as Table 1 Panel 1 shows, the number of ventures varies dramatically across rounds. Most of the distribution weight of the variables, therefore, falls near the means, giving a roughly bell-shaped pattern to all the variables.

Figure 5 shows venture-level characteristics, including company incorporation, prior financing, whether the company's product is IT or software based, whether the company is in a hub state, and whether the company describes itself as social impact-oriented or in a clean technology sector. Figure 6 shows founder-level characteristics, including whether the founder is a student at the time of the round, ever received an MBA, attended a top-20 college, and is of above median age (in years). The distributions are clearly not exactly the same and for some variables data is sparser across z-score bins. For example, because the HBS New Venture Competition is large and does not have structured feedback, there are overall more founders from elite schools in this group. The important takeaway from the figures is that in no case does the distribution of losers (left tail) appear meaningfully lopsided, providing visual evidence that the characteristics are similarly distributed in the two types of competitions.

I test more formally for distributional differences around the median among losers in Table 3. The goal is to assess whether above and below median losers are systematically different across rounds with and without structured feedback status. If the gap between losers is larger in one type of competition (for example, the bottom tail was relatively lower quality), this would suggest possible bias in estimates of Equation 2. I calculate the variable's mean above and below the median among losers in each round (e.g. whether a company is incorporated at the time of the round or not). Then I subtract the below median mean from the above median mean. Finally, I conduct a t-test across rounds with and without structured feedback.

Among the nine observables at the time of the round considered in Table 3, the only significant difference is found in the probability that the venture is located in a hub state. In the non-structured feedback competitions, above median losers are slightly more likely than below median losers to be in a hub state, while there is essentially no difference in structured feedback competitions. If anything, this should bias against my result that negative feedback leads to a higher probability of failure, since ventures in hub states are unconditionally much more likely to succeed than those outside of hub states (Appendix Table A6).

I compare overall competition and round characteristics in Table 4, also using t-tests. The types of competitions are broadly similar: importantly the number of ventures, winners, and judges are not statistically different across the two groups. However, the award amount is much higher in the structured feedback competitions. Given the distributional tests above, however, there is no obvious reason that this should give rise to a systematic difference between below and above median losers.

A concern is that founders with more uncertainty about their project quality may select into competitions with more feedback. Although competitions did not publicize their feedback program (use of Valid Evaluation software), in theory this could have been public knowledge. One way to test for such selection is to examine ventures in multiple competitions, where performance in the first competition permits a proxy for uncertainty. If founders with high information needs tend to select into competitions with feedback, then it should be the case that when they compete in a second competitions, these founders are more likely to participate in a structured feedback competition.

I proxy for high information needs with having a low average or highly dispersed score in the first competition. Appendix Table A14 presents summary statistics for the sample used in the test, and t-tests for whether the proxies for uncertainty, measured in the first round of the first competition, are associated with a propensity to participate in a second competition that has structured feedback. I find no association, suggesting that in the overall sample, it is unlikely that founder selection into competition type is affected by information needs.

Matching

To address any remaining concerns about systematic distributional differences between participants that receive feedback and those that do not, I build two matching estimators. These estimators try to solve the problem of "missing" potential outcomes by matching subjects in a treatment group to their closest counterparts in the the untreated group. Here, the treated group comprises losers with below-median ranks who do receive feedback, and the untreated group comprises losers with below-median ranks who do not receive feedback. Matching estimators use the outcome (here, survival) for the "closest" untreated participant as the missing potential outcome for the treated participant, and take the average difference between observed and predicted outcomes to arrive at an average treatment effect. Here, this compares survival for these matched groups to the respective above-median matched groups.

The first matching method is exact matching, and the second is propensity score matching on many binary covariates. Note that if the matching is done correctly, both methods should yield roughly similar results. When matching is exact, there is no conditional bias in the estimated treatment effect (Abadie & Imbens 2006). The samples of above-and below-median losers were matched exactly on thirteen sectors, competition year, student status, and company incorporation status. I conduct out-of-sample covariate balance tests in Appendix Table A7. Panel 1 contains these variables not used in the matching process after matching, and Panel 2 shows their balance before matching. The match dramatically reduces the differences. For example, the differences in the probability that the founder has an MBA is reduced from 27 percentage points (pp) to 3 pp, and the difference in venture age is reduced from 1.2 years to 0.4 years.

The propensity-score match process first estimates a probability of treatment using a logit model. It then identifies, for each treated participant, the untreated participant whose scores (probabilities of treatment) are closest. I try to eliminate bias in several ways. First, I match without replacement. This means that once an untreated participant is matched to a treated participant, it cannot be considered as a match for subsequent treated participants. Since each untreated subject appears no more than once, variance estimation is uncomplicated by duplicates (Hill & Reiter 2006). Second, I match only on binary covariates. 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. Requiring common support means that participants are excluded if their propensity scores fall outside the range that overlaps across the treatment and control groups. Appendix Table A8 shows the covariate balance after (Panel 1) and before (Panel 2) matching. The process brings the samples almost entirely in line, with no p-values below 0.5 and no differences greater than 1 pp.

4 Results

4.1 Signal informativeness and the benefit of winning

The learning metrics require judge ranks to be meaningful signals about startup quality. If ventures seek to improve in the judges' estimation, then improvement in rankings reflect venture learning regardless of whether the ventures observes their scores. It is crucial that rank predicts subsequent success independently of win status. Figures 1-2 demonstrate visually the effects of winning and the predictive power of rank on either side of the cutoff for subsequent financing.

Estimates of Equation 1 in Tables 5 and 6 show that the competitions generate valuable signals. Across all the success proxies, the coefficients on rank (or z-score in column 5) are negative and highly significant, even within judge (e.g., Table 5 column 3).[28] For most outcomes, rank is predictive on both sides of the award cutoff (among losers and winners of a round).[29] For example, Table 6 column 1 implies that being ranked one decile higher increases the probability a venture subsequently raises angel/VC investment by about a little less than one percentage point (pp) among winers, and a bit more than one pp among losers.

[28] 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.

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

Further, Appendix Table A9 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.

Table 5 indicates that winning a round increases a venture's probability of subsequent external private finance by 7-12 pp, depending on the specification, relative to a mean of 24%. Column 4 limits the sample to the quintiles around the cutoff for advancement in a preliminary round, and finds a 9.8 pp effect of winning. Appendix Table A10 separates rounds into final and preliminary. Within preliminary rounds, there is an independent effect of winning a preliminary round, at 4-8 pp. The effect of winning the overall competition is much larger for this group, at 21 pp. Within final rounds, the effect is about 12 pp. Many specifications include the cash award amount. While winning is useful independently of the award, an extra $10,000 in cash prize increases the probability of financing by about 1 pp. This appears small in economic magnitude relative to the overall effect of winning and the predictive power of rank.[30]

Table 6 shows the effect of winning for other outcomes. Winning increases the probability of subsequent angel or series A VC investment by 11-15 pp, relative to a mean of 15% (columns 1-2). It increases the probability the venture has at least 10 employees in 2016 by 7-12 pp, relative to a mean of 20% (columns 5-6). Winning increases the likelihood the venture experienced a successful exit by 2 pp, relative to a mean of 3% (columns 7-8). I do not find a consistent effect of winning on serial entrepreneurship, among founders that abandon their ventures.

I find that winning is much more useful when the venture and the competition are in the same state (Appendix Table A11). This suggests that local network effects are important for competitions to be a useful learning intervention. The competitions draw ventures from diverse geographic locations, but judges

[30] 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.

are mostly local. While winning is useful to ventures across locations (Appendix Table A12), it is much more useful when the venture and competition are in the same, non-VC hub state. Through this channel, competitions may promote local entrepreneurship, which Glaeser, Kerr & Kerr (2015) and Gennaioli et al. (2013) show is correlated with local economic growth.

It is possible that the positive effect of winning actually reflects a negative effect of losing. Perhaps it is costly in time and travel expense for the venture to compete, or perhaps losing generates a negative signal about venture quality. This would require substantial irrationality on the ventures' part. If the downside of losing - which is much more likely given that only a small share of competitors win - were much larger than the upside of winning, there should be little demand for competitions. Instead, the programs are typically oversubscribed. For example, the Rice Business Plan Competition receives between 400 and 500 applications for 40 places in its annual competition. The results in Table 4 indicate that winning a preliminary round is useful even when the venture ultimately loses, and that among losers, a higher rank is predictive of success. Thus competitions may well be useful for a majority of participants.

4.2 Responsiveness to feedback

This section shows that entrepreneurs who receive especially negative feedback about their ventures are more likely to be abandon them. This finding both demonstrates that ranks contain meaningful information for the entrepreneurs, and also offers some of the first empirical evidence of managerial learning in the sense of a real outcome response to new information. Equation 2 is estimated in Table 7. The coefficient of interest gives the effect of having a below median rank among losers in a round where the venture is informed of its rank, relative to having a below median rank among losers in a round where the venture is *not* informed of its rank, after controlling for the two individual effects of below median rank and receiving structured feedback. The control group is above-median losers in both types of competitions.

The dependent variable, survival, is one if the venture reported at least one employee beside the founder on LinkedIn in August, 2016. The main specification in column 1 finds that negative feedback reduces the likelihood of continuation by 8.8 pp, relative to a mean of 34%. This translates to a 12% increase in the probability of failure. Note that all rounds are included, so ventures that make it to a final round have multiple "chances" to receive especially negative feedback. Column 2 uses only data from preliminary rounds, and finds a larger effect of 12 pp, significant at the 1% level. Two tests account for potential non-linearities. First, in column 6 I control for the venture z-score first and second moment in z-score. Second, in column 7 I show the results from a logit specification.[31]

The results of the matching exercises (described in Section 3) are in columns 8-10. The result with exact matching is almost precisely the same as the full sample result, at 7.6 pp, significant at the 1% level. The effect falls somewhat in the propensity-score matching. With controls (replicating the baseline specification), it is 5.6 pp, significant at the 5% level, and without controls it is just 4 pp, and significant only at the 1% level. Despite the decline, the general robustness of the effect to these approaches provides strong evidence against distributional differences across the two types of competitions driving the main effect.

I also test for robustness within a single program in my data, the Cleantech Open (CTO). CTO gave structured feedback in 2011 but in no other year. As the competition did not otherwise change in 2011, there is no reason that the distribution of quality among losers should have been different in 2011.[32] Within the CTO sample, Appendix Table A15 shows that negative feedback reduces the probability of survival by 11-13 pp, very similar to the main specification. This is true using only the years 2010-12, as well as all years for which I have CTO

[31]This effect is weakly symmetrical for winners. Appendix Table A13 examines whether receiving particularly positive feedback makes winners of a round more likely to continue. The sample is smaller, as most rounds have far fewer winners than losers. With judge fixed effects, there is a strong positive effect on continuation of extremely good feedback. However, this effect disappears when I use the standard sample of one venture-round observation.

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

data (2008-14). It also holds using both OLS and logit.

The effect of negative feedback on abandonment persists within important subsamples. Three models in Appendix Table A16 show that the effect persists within the population of founders with MBAs, among ventures from VC hub states, and among student-led ventures. In sum, these results provide strong evidence that on average, the entrepreneurs in this setting are not so overconfident as to give zero probability to the failure state; interim signals do matter. Recall that the RD design showed that feedback is informative about commercial outcomes. Therefore, receiving feedback must be weakly more efficient than not receiving it. This allows a simple back-of-the-envelope welfare calculation within my setting. My primary specification find that feedback increases abandonment by about 12%, so had the 1,453 unique below-median losers in the no-feedback competitions in my data received feedback, an additional 174 would, or "should", have been abandoned.

Yet there is also substantial heterogeneity in responsiveness. Table 8 adds a venture characteristic as a third interaction; for brevity, panels 2 and 3 do not report control coefficients. The heterogeneity results seem to relate closely to our expectations of a venture's informational needs. Some of the characteristics discussed below are correlated with each other; a full correlation table is in Appendix Table A17.

Venture Stage

The option to abandon should be most valuable at an earlier stage. I examine three proxies for firm stage: receiving prior external financing, being incorporated, and age in years. Table 8 panel 1 column 1 shows that ventures with previous private financing are much less responsive to the negative signal; they are 18 pp more likely to continue after receiving especially negative feedback. Panel 2 shows that incorporated ventures are similarly much less responsive. These results indicate that firms with a lower cost of experimentation seem to do more of it.

I also find that students and younger founders are more responsive (Table 8 panel 2 column 4, and panel 3 column 6); the option to abandon the idea is perhaps most valuable for this group. Students may place greater weight on judges' advice because they have little personal experience and thus a less precise private prior. Yet I find no effect of prior jobs or founding a prior venture (Table 8 panel 2 columns 5-6).

These results could reflect earlier stage ventures having less private information about their own potential, leading them to update more when they receive negative feedback. Yet the lack of a result for venture age suggests that if anything judge feedback is less informative for the youngest ventures. Unreported tests find that when Equation 1 is estimated separately for below- and above-median age ventures (median age is about 9 months), the predictive power of rank on having at least two employees almost doubles for the older ventures. In sum, these results are consistent with the option to abandon becoming less valuable as the venture reaches the milestones of incorporation and initial funding.

Software-based, unincorporated, and student-led ventures may be more responsive to feedback because they have less precise knowledge of their type, and thus optimally update less. Alternatively, they may be more responsive because they have already committed more resources to the venture, and have greater sunk cost. Since ventures with prior financing are much more responsive, but there is no effect of venture or founder age, it seems that the first hypothesis is more likely: Ventures with market-based information about their own high type are more likely to dismiss a low ranking as noise.

Venture Technology

Technology type is related to a firm's cost of pivoting and to external financing availability. The cost of experimentation (or pivoting) should be lower for IT/software ventures than for hardware startups. Table 8 Panel 1 column 2 shows that IT/software startups are more responsive; they are 11 pp more likely to fail after receiving especially negative feedback than hardware startups. This does not seem to relate to non-pecuniary motivations among hardware founders, as column 3 finds no effect for social impact/clean technology ventures. A lower cost of launching a startup appears to make abandonment a more attractive option.

This supports the argument in Kerr et al. (2014) and Ewens et al. (2015) that the cost of resolving initial uncertainty about whether a new technology or business model will work helps determine which projects are funded and thus the direction of innovation in the economy. They attribute the dramatic increase in web- and software-based startups in the 2000s in part to the dramatic fall in computing power and storage costs. One implication is that new ventures with high initial capital intensity have become relatively higher cost experiments. In particular, the option value of entrepreneurship is lower for a hardware technology. Resolving uncertainty about their viability will require more time and more money than software ventures.

Venture Risk

One measure of venture risk is uncertainty among judges.[33] 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.[34] The triple interaction has a positive effect (Table 8 panel 2 column 3); when judges are uncertain, founders are less sensitive to their overall rank. This is suggestive evidence that more confident founders choose riskier business models, consistent with the findings among CEOs in prior work, including Hirshleifer et al. (2012) and Graham et al. (2013).

An alternative explanation is that the higher standard deviation reflects a less precise signal, and interactions with judges during the competition inform founders that there was no consensus among judges.[35] In this case, the finding

[33] Appendix Table A18 suggests that judge uncertainty - after controlling for rank and winning - predicts angel/VC series A financing, consistent with these types of investors targeting risky ventures.

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

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

suggests that updating reflects signal precision, consistent with experimental evidence in Poinas et al. (2012) and survey evidence in Ben-David et al. (2013). I find that overall rank continues to predict subsequent external financing within the sample of high standard deviation startups. As with the elite college founders, this suggests that rational founders ought not to ignore it.

To further tease apart these hypotheses, I instrument for standard deviation with judge leniency measures. 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. If this instrumented standard deviation leads to less responsiveness, it suggests that the alternative explanation, in which ventures do not update when the signal is less precise, is the primary channel.

I use a version of the leave-one-out judge leniency from Dobbie & Song (2015) and Chang (2013). Specifically, I measure the judge propensity to give highest scores, leaving out venture i. Let S_{ij} be an indicator for the highest score a venture received across judges, where this score came from judge j.[36] Let n_j be the count of ventures that the judge scored. The leave-one-out leniency measure is then $L_{ij} = \frac{1}{n_j - 1}\left(\sum_{k=1}^{j} S_k - S_i\right)$. I then take the standard deviation of the leniency score across judges that scored the venture. When this variation in leniency ($V_{i,\sigma}^{high}$) is high, the venture receives an especially noisy signal that is independent of the venture characteristics.[37] As robustness tests, I create an additional measure that focuses on extremes. I take the standard deviation of L_{ij} considering only the four most extreme judges that scored a venture (the most lenient, least lenient, harshest, and least harsh). I call this measure $V_{i,\sigma}^{ext}$. These measures are summarized in Table 2 Panel 3.

Appendix Table 19 columns 1-4 shows that the variation in leniency instrument ($V_{i,\sigma}^{high}$) predicts the standard deviation of judge scores quite well. The f-statistic of a first stage regression testing for the excluded instrument being

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

[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.

statistically significant from zero is 31 in the primary specification, suggesting a reasonably strong instrument. The f-statistic for the first stage using $V_{i,\sigma}^{ext}$ is 16. I then replace the standard deviation measure from Table 8 (predicted in columns 1-4) with these variation measures. This is a naive instrumentation approach; given the small sample and need for many instruments in the interacted regression, a two-stage-least-squares approach here is infeasible. (I would need a separate instrument for each interacted variable, which is unavailable.) In the naive instrumented regressions in columns 5-6, there is no effect of the triple interaction between having a low rank, receiving feedback, and having judges with high expected variation in leniency.

Thus ventures are not more responsive when they are randomly assigned greater noise (a high standard deviation in scores). This suggests that the effect in the main regression in Table 8 panel 2 column 3, using the raw standard deviation, reflects riskier ventures being less responsive. This finding builds on Hvide & Panos (2014), who show that among Norwegians, entrepreneurs tend to be more risk tolerant in their personal investment behavior, and that greater risk tolerance is associated with poorer performance.

Elite-School Founders

Founders with elite college degrees are less responsive to feedback; for example, founders with top 10 college degrees are 23 pp less likely to fail in response to negative feedback (Table 2 panel 3 columns 4 and 5). Figures 7 and 8 suggest this may reflect rational behavior. The graphs show coefficients from regressing outcome measures on an indicator for elite status, with competition-round fixed effects. Founders with top college degrees are significantly more likely to succeed on average (Figure 7), and within the sample of losers, they are much more likely to raise angel/VC but not more likely to reach at least 10 employees (Figure 8).

That elite-school founders do not learn is potentially consistent with overconfidence. Conversely, it may be that winning is not useful and/or ranks are not informative for this subsample. To test this possibility, I estimate Equation

1 within subsamples of elite-school founders in Appendix Table 20. Winning and rank are roughly as predictive of success for elite-school founders as for the whole sample. Thus it does not seem that the competitions and the information they generate are less useful for these founders.

It is perhaps not surprising to find evidence of overconfidence among elite school founders. Landier & Thesmar (2009) find that entrepreneur overconfidence increases with the entrepreneur's outside option. Elite school founders likely have better outside options. At the same time, they may have personal wealth that reduces the cost of failure.

Theory suggests that in certain leadership contexts, failing to learn may be optimal. Bernardo & Welch (2001) and Goel & Thakor (2008) theorize that the few entrepreneurs or CEOs who do succeed benefit from their overconfidence. Bolton, Brunnermeier & Veldkamp (2013) theorize that good leaders make an initial assessment of their environment, and then persist in their strategy regardless of new information. Related empirical work by Kaplan et al. (2012) finds that better performing CEOs are characterized by less openness to criticism and feedback. These points apply best in my context to elite college graduates; as Figures 2 and 3 show, they are unconditionally more likely to succeed than their counterparts.

An alternative explanation is that there are differences across groups in unobservable non-pecuniary benefits to entrepreneurship. Elite school graduates may have especially high outside options that make entrepreneurship unprofitable for most. The ones that select into entrepreneurship are those with especially large non-pecuniary benefits. This is plausible, but unlikely, as I do not find a strong reduction in responsiveness among social impact and clean energy ventures.

This analysis is suggestive rather than conclusive. It raises interesting questions, however, about how learning and overconfidence interact with innovation. New entrants with radical technologies may be less responsive to feedback, while those with more incremental ideas are more adaptable. Theoretical models of industry dynamics could micro-found technological discontinuities in the small fraction of entrepreneurs that enter without regard to signals about net discounted expected cash flows, while the mass of entrants remain rational and responsive to new information. The former group are potentially transformative, and their overconfidence is crucial to coordinating others, as emphasized in Rajan (2012), Bolton et al. (2013), and the VC quote above.

Criteria-Specific Responsiveness

The overall ranks and scores I have used thus far are, in most competitions, aggregated dimension (criteria) scores. The most commonly used criteria across competitions are team (management quality), financials, business model potential (i.e. strategy to make profit), technology or product quality, market attractiveness or size, and presentation quality. I now use these criteria scores to explore the type of learning that occurs across rounds. For all outcomes other than IPO/acquisition, I find in Table 9 that a higher team rank is the strongest predictor of subsequent success. This is consistent with Bernstein, Korteweg & Laws (2015) and Gompers et al. (2016), who find that early stage investors most value information about startup teams. Related work find a positive correlation between good managerial practices and productivity in large firms (Bloom et al. 2012, Bloom et al. 2016, Guiso et al. 2015).

Presentation ranks predict financing but no other outcome (Table 9 columns 1-2). A better technology or product rank predicts IPO/acquisition and survival (Table 9 columns 7-8). The financials rank, which reflect a venture's recent and planned fundraising, as well as near-term cost management, is especially important to having a large number of employees. For a small sample, the data include scores for two additional dimensions, which confirm the value of specificity. Having IP protection and a solid legal footing predicts survival, and traction or having validated the technology predicts subsequent financing (see Appendix Table A21).

I then examine whether the overall responsiveness to negative feedback is especially relevant for certain dimensions. For each dimension, I create a new variable, "Below loser median rank in dimension D". If D is presentation quality, this variable is an indicator for having a below-median presentation quality rank among losers. Controlling for the venture's overall rank in the round, I then interact the below median variable with whether the venture was informed that it was below median (Feedback). The results, in Table 10, reveal that negative feedback impacts continuation most along the financials, business model, market, and team dimensions. There is no effect for presentation or product/technology.

A natural explanation is that while the founders have better private knowledge about the quality of their product or technology than judges do, the judges have valuable insight into the viability of the business. The ability to sell one's business to potential investors, employees, and other stakeholders is often considered by practitioners to be a crucial skill for an entrepreneur. Presentation scores may not affect survival because there is more scope for improvement (or perceived scope for improvement) along this dimension.

Time to Abandonment

I examine predictors of serial entrepreneurship and time to abandon in Table 11. As expected, venture stage negatively predicts founding a subsequent venture. More jobs before the competition and an MBA positively predict founding a new venture. Having a social impact or clean tech venture makes a founder more likely to pursue a subsequent venture and reduces the time to abandon by around 74 days, relative to a mean of 313. This is consistent with these founders having larger non-pecuniary benefits from entrepreneurship.

While having a top 10 MBA does not have a substantial effect on founding a new venture, both having a top 10 MBA associated with dramatically lower the times to abandonment (138 days). This is consistent with these founders having more valuable outside options. Older founders and founders with PhDs have longer times to abandon. They may have lower outside options.

I do not find that greater responsiveness to negative feedback, measured by the fully controlled triple interaction between fast abandonment, structured feedback, and below median rank, predicts founding or being the CEO of a new venture. The coefficient on this triple interaction is small and insignificant effect in (Appendix Table A22 column 1). Instead, receiving feedback in general affects serial entrepreneurship. In subsequent columns, individual effects and interactions between feedback and fast abandonment indicate that while abandoning fast is strongly associated with serial entrepreneurship, there is an additional large positive effect of receiving feedback and abandoning fast. The individual effect of feedback is near zero. This result suggests that feedback may make serial entrepreneurship more efficient.[38]

Judge type

Finally, I explore whether founders are especially responsive to certain judge types. First, it may be the case that "noisy" learning (as in Jovanovic & Lach (1989)) occurs when ventures are assigned especially lenient or harsh judges. I do not find effects of judge leniency scores on either outcomes in general, on time to abandonment, or on responsiveness. That is, when ventures have especially harsh judges, I have no evidence that this leads to inefficient learning.

Second, entrepreneurs may view judges from certain professions as providing especially valuable feedback. I consider the three most common judge types in Table 12 (investor, corporate executive, and entrepreneur/founder), and I interact the effect of negative feedback with an indicator for whether the any of the venture's judges fell in one of these categories. In columns 1-3, the dependent variable is continuation. Negative feedback has a much larger impact on abandonment when the venture has a judge that is an entrepreneur or founder (column 3). The effect of being a below-median loser interacted with feedback disappears for this column entirely. A somewhat similar, albeit weaker, result appears when the venture is exposed to a corporate executive judge (column 2).

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

In contrast, there is no special effect of negative feedback from exposure to VC judges, though relatively few ventures in the sample that had no VC judges.

In columns 4-6, the dependent variable is changed to founding a subsequent venture, and in columns 7-9, it is changed to the number of days between the competition and venture abandonment, conditional on abandoning the venture. Both yield stark results: having an entrepreneur or founder judge makes a venture founder much more likely to respond to negative feedback by founding a new, different venture, and dramatically reduces the time to abandonment - by 388 days, significant at the 1% level. In contrast, there are near zero and insignificant results for both dependent variables for VC and corporate executive entrepreneurs. The rankings from entrepreneur judges are not more independently predictive than those of investors (Appendix Table A23). Therefore, the channel appears to be either through founder beliefs about the value of entrepreneur feedback, or through informal and post-competition feedback that I do not observe.

5 Conclusion

It is possible that entrepreneurs are simultaneously endowed with a technology, business model, and beliefs about future profits. Their expectations may be static, changing only when the firm fails, as in in Acemoglu et al. (2013). Such static information is consistent with explanations for entrepreneurial entry based on overconfidence, over-optimism, or high non-pecuniary benefits. Yet in many theoretical models of firm dynamics and occupational choice, entry and exit occur when entrepreneurs or firm managers rationally learn from new information. If such a learning process is occurring, we know little about it. This paper offers the first evidence of entrepreneurs learning about their type from feedback.

Feedback, in my setting, makes the process of entrepreneurial experimentation more efficient. The data in this paper permit only a hint at whether feedback allows entrepreneurs to fail fast, and whether it permits entrepreneurs to pursue more radical ideas. Further research in these areas is promising.

A final comment relates to the program evaluation aspect to this paper, which is tangential to the main research agenda but important in its own right. Many new venture competitions are publicly funded, both in the U.S. and abroad. Governments view these programs as a means to foster high-growth entrepreneurship either in a specific region or in a sector perceived to have high social benefits. The White House "Startup America" initiative, launched in 2011, champions the public sponsorship of acceleration and competition programs.[39]

Despite the recent burgeoning of support, it is not obvious that competitions are useful. Since participation in a competition is typically public information, losing might generate a negative signal. Participating could also lead to a loss of intellectual property (IP). Further, competitions may not be useful if judges are uninformed or inadequately incentivized. Finally, if the skills required to win are different from those needed for commercial success, competitions could distract from productive activities.

I find that winning causally increases a venture's probability of success. While a larger cash award is associated with greater success, winning is useful even in preliminary rounds with no prize. Further, learning is useful independently of winning. Thus competitions should consider focusing on their convening power - that is, enabling useful social interactions - and on providing structured feedback, rather than on awarding large prizes.

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

Table 1: Summary Statistics

Panel 1: Competitions

	N	Mean	Median	S.d.	Min	Max
# competitions	96					
# competition-rounds	214					
# competition-round-panels	543					
# competitions with structured feedback (venture learns rank relative to other participants)	35					
# rounds per competition	96	1.9	2	.69	1	3
# ventures in preliminary rounds	120	44	36	41	4	275
# ventures in final rounds	94	18	12	20	4	152
# winners in final rounds	94	4.5	5	3.6	1	25
Award amount$\|$ Award>0 (thousand nominal $)	317	66	25	85	750	275
Days between rounds within competition	88	23	17	31	0	127
# judges in round-panel	543	17	9	23	1	178

*Panel 2: Ventures**

	N	Mean	Median	S.d.	Min	Max
# unique ventures	4,328					
Venture age at first competition (years)	2073	1.9	0.77	3	0	20
Probability incorporated at round	4328	0.44	0	0.5	0	1
Prob. in hub state (CA, NY, MA)	4,328	.35	0	.48	0	1
Prob. has ≥ 2 employees as of 8/2016	4328	0.34	0	0.47	0	1
Prob. has ≥ 3 employees as of 8/2016	4328	0.3	0	0.46	0	1
Prob. has ≥ 10 employees as of 8/2016	4328	0.2	0	0.4	0	1
Prob. raised external private investment before round	7099	0.16	0	0.36	0	1
Probability external private investment after round	7099	0.24	0	0.43	0	1
Prob. angel/VC series A investment before round	7099	0.09	0	0.29	0	1
Prob. angel/VC series A investment after round	7099	0.15	0	0.36	0	1
Probability operating as of 9/2016†	4328	0.63	1	0.48	0	1
Prob. acquired/IPOd as of 9/2016†	4328	0.03	0	0.18	0	1
Ventures in multiple competitions (stats on # of competitions if # >1)	558	2.52	2	0.98	2	9
Days between competitions among ventures in >1	978	302	215	289	1	2562
# founders/team members at first competition	2305	3.1	3	1.6	1	8
Percent of venture owned by presenting team	420	74.79	85.5	28.91	0	100
Possesses formal IP rights at round	1091	0.48	0	0.5	0	1

Panel 3: Founders (Venture Leader - One Per Venture)‡						
	N	Mean	Median	S.d.	Min	Max
# founders	3228					
# founders matched to LinkedIn profile	2554					
Age (years) at event (college graduation year-22)	1702	32.8	29	10.2	17	75
Female±	3,228	0.21	0	0.41	0	1
Male	3,228	0.72	0	0.45	0	1
Number of total jobs	2554	6.63	6	3.93	0	50
Number of jobs before round	2547	4.41	4	2.66	0	10
Number of locations worked in	2554	2.71	2	2.27	0	29
Days to abandon venture if abandoned**	1190	313	148	420	1	4810
Prob. is student at round	2554	0.2	0	0.4	0	1
Prob. graduated from top 20 college	2554	0.27	0	0.44	0	1
Prob. graduated from top 10 college	2554	0.18	0	0.39	0	1
Prob. degree from Harvard, Stanford, MIT	2554	0.1	0	0.3	0	1
Prob. has MBA	2554	0.48	0	0.5	0	1
Prob. has MBA from top 10 business school	2554	0.33	0	0.47	0	1
Prob. has Master's degree	2554	0.17	0	0.37	0	1
Prob. has PhD	2554	0.13	0	0.34	0	1
Prob founder or CEO of subsequent venture after round, if abandoned venture	1190	0.39	0	0.49	0	1

Note: This table contains summary statistics about the competitions (panel 1), ventures (panel 2), and founders/team leaders (panel 3) used in analysis. *Post-competition data from matching to CB Insights (752 unique company matches), Crunchbase (638), AngelList (1,528), and LinkedIn (1,933). †Active website. ‡From LinkedIn profiles. Not all competitions retained founder data, so the number of venture leaders is less than the number of ventures. ±Gender coding by algorithm and manually; sexes do not sum to one because some names are both ambiguous and had no clear LinkedIn match. **This is the number of days between the competition's end date and the first subsequent new job start date, among ventures that did not survive, measured as not having at least one person other than the founder identify as an employee on LinkedIn. See Appendix Table 4 for university rankings.

Table 2: Venture Sectors & Judge Professions

Panel 1: Venture Sectors

	# unique ventures
Hardware	245
Software	1,404
Air/water/waste/agriculture	146
Biotech	182
Clean tech/renewable energy	712
Defense/security	64
Education	37
Energy (fossil)	61
Fintech/financial	53
Food/beverage	88
Health (ex biotech)	270
IT/software/web	1,404
Manuf./materials/electronics	323
Media/ads/entertainment	57
Real estate	61
Retail/apparel/consumer goods	139
Social enterprise	42
Transportation	136

Panel 2: Judge Professions

	# unique judges
All	2514
Venture Capital Investor	676
Angel Investor	51
Professor/Scientist	44
Business Development/Sales	83
Corporate Executive	498
Founder/Entrepreneur	240
Lawyer/Consultant/Accountant	369
Non-Profit/Foundation/Government	164
Other	193
Investment in judged ventures	
# judge-venture pairs in which judge personally invested in venture	3
# judge-venture pairs in which judge's firm invested in venture	95
# judge-venture pairs in which judge's firm did not invest in venture	51,093

Panel 3: Judge Uncertainty and Leniency Measures

	N	Mean	Median	S.d.	Min	Max
Judge uncertainty (std dev of within-panel judge decile ranks of a venture)	5997	1.88	1.02	1.97	0	6.36
Judge dimension uncertainty (std dev of within-panel judge decile dimension ranks of a venture)	4961	1.37	0.85	1.29	0	5.66
Venture leave-one-out leniency score	3788	0.33	0.25	0.32	0	2
Venture leave-one-out harshness score	3779	0.33	0.29	0.28	0	2
$V_{i,\sigma}^{high}$ (venture leave-one-out leniency variation based on propensity to give highest score)	3770	0.21	0.19	0.13	0	0.96
$V_{i,\sigma}^{ext}$ (venture leave-one-out leniency variation based on four most extreme judges)	3788	0.31	0.29	0.13	0	1.15

Note: This table lists the number of ventures by technology type, the number of judges by profession, and the leniency measures (see Section 4.2 for details).

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

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

Note: This table compares the difference between above- and below-median losers across structured 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 structured feedback.

Table 4: Competition Characteristics by Structured Feedback Status

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

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

Table 5: Effect of Rank and Winning on Subsequent External Financing

Dependent variable: Financing after round*

		Logit		Quintiles around cutoff; prelim rounds‡		
	(1)	(2)	(3)	(4)	(5)	(6)
Won Round	.077**	.8***	.16***	.098***	.13***	.098***
	(.037)	(.14)	(.015)	(.026)	(.023)	(.026)
Decile rank winners†	-.0062	-.071***				
	(.0056)	(.021)				
Decile rank losers	-.014***	-.13***				
	(.0032)	(.017)				
Within-judge decile rank			-.0061***			
			(.0014)			
Z-score among winners					.027	
					(.019)	
Z-score among losers					.041***	
					(.01)	
Z-score^2 winners					.019	
					(.014)	
Z-score^2 losers					.000056	
					(.0073)	
Within-judge z-score						.027***
						(.0063)
Award Amount ($, 10,000s)	.0093***		.011***	.013**	.0089***	.0056*
	(.003)		(.0023)	(.0057)	(.0029)	(.0029)
Venture controls††	Y	N	Y	N	Y	Y
Competition-round-panel f.e.	Y	Y	N	Y	Y	N
Judge f.e.	N	N	Y	N	N	Y
Year f.e.	N	N	Y	N	N	Y
N	3367	5500	23785	1945	3529	13285
R^2	.4	.12	.43	.23	.41	.4

Note: This table contains OLS regression estimates of the effect of winning, rank, and award (cash prize) on whether the venture raised private investment after the competition, using variants of:

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

OLS used except in 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 6: Effect of Rank and Winning on Additional Outcomes

Dependent variable:	Angel/VC series A investment		Survival*		$\geq$ 10 employees as of 8/2016		Acquired/IPO	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Won Round	.11***	.15***	.065**	.14***	.071***	.12***	.019*	.023***
	(.022)	(.02)	(.027)	(.034)	(.027)	(.038)	(.011)	(.0084)
Decile rank winners	-.009**		-.0066		-.0048		-.0029*	
	(.0039)		(.0043)		(.0043)		(.0017)	
Decile rank losers	-.011***		-.023***		-.017***		-.0011	
	(.0019)		(.0028)		(.0023)		(.001)	
Within-judge decile rank		-.0058***		-.01***		-.0087***		-.00047
		(.00057)		(.0036)		(.0032)		(.00057)
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	6046	47065	6046	47066	6046	47065	6046	47065
R^2	.15	.11	.17	.12	.14	.083	.083	.047

Note: This table contains OLS regression estimates of the effect of winning and rank on indicators for various outcomes, using variants of:

$$Y_i^{Post} = \alpha + \beta_1 WonRound_{i,j} + f\left(DecileRank_{i,j}\right) + \gamma' \mathbf{f.e.}_{j'/k} + \varepsilon_{i,j}$$

* This measure for venture continuation is 1 if the venture had at least one employee besides founder on LinkedIn as of 8/2016. Errors clustered by competition-round or judge, depending on f.e. A smaller rank is better (1 is best decile, 10 is worst decile). Note that competition f.e. control for a specific date. All rounds included. *** indicates p-value$<$.01.

Table 7: Effect of Negative Feedback on Venture Continuation

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

		Prelim rounds only		Prelim rounds only		Z-scores	Logit	Exact matching$^{\pm}$	Propensity score matching**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Low rank·Feedback	-.088**	-.12***	-.084***	-.1***	-.079***	-.088**	-.32**	-.076***	-.056**	-.04*
	(.036)	(.044)	(.02)	(.023)	(.026)	(.036)	(.16)	(.027)	(.022)	(.023)
Low rank	-.062***	-.051**	-.051***	-.043***	-.026	-.064***	-.3***			
	(.021)	(.023)	(.014)	(.016)	(.022)	(.021)	(.099)			
Z-score						.04				
						(.029)				
Z-score^2						-.013*				
						(.0067)				
Feedback	.071*	.11**	.17*	.17	-.031	.075*	.25			
	(.04)	(.045)	(.092)	(.15)	(.14)	(.039)	(.17)			
Venture controls†	Y	Y	Y	Y	Y+‡	Y	Y	N	Y	N
Year f.e.	Y	Y	N	N	N	N	Y	N	Y	N
Judge f.e.	N	N	Y	Y	Y	N	N	N	N	N
N	3765	2689	26484	17388	14938	3765	3765	2484	3357	3357
R^2	.083	.083	.18	.14	.29	.085	.065	-	.095	.064

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

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

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

Table 8: Heterogeneity in Effect of Negative Feedback

Panel 1

Dependent variable: Survival*

Characteristic C_i:	Financing before round	Tech type IT/software	Social/ clean tech	VC hub state†	Same state	Founder female
	(1)	(2)	(3)	(4)	(5)	(6)
Low rank·Feedback· C_i	.15*	-.1*	.072	-.11	.063	-.1
	(.087)	(.062)	(.088)	(.14)	(.064)	(.096)
Low rank·Feedback	-.1**	-.015	-.1**	-.11**	-.12**	-.093**
	(.041)	(.043)	(.042)	(.042)	(.054)	(.039)
Feedback· C_i	-.19***	-.00096	-.089	.15*	.0048	.12
	(.067)	(.058)	(.08)	(.085)	(.057)	(.073)
Low rank· C_i	-.033	-.0035	.028	-.058	-.053	.071
	(.069)	(.038)	(.047)	(.04)	(.04)	(.045)
Low rank	-.047**	-.038*	-.051**	-.027	-.031	-.079***
	(.022)	(.023)	(.023)	(.031)	(.03)	(.025)
Feedback	.11**	.057	.1**	.084*	.067	.059
	(.042)	(.038)	(.041)	(.045)	(.053)	(.045)
C_i	.37***	.09**	-.098**	.049	.019	-.11***
	(.054)	(.037)	(.042)	(.037)	(.038)	(.039)
Year f.e.	Y	Y	Y	Y	Y	Y
N	3765	4136	4136	3765	3765	3048
R^2	.13	.12	.077	.085	.084	.1

Panel 2 (control coefficients not reported)

Dependent variable: Survival*

	(1)	(2)	(3)	(4)	(5)	(6)
Characteristic C_i:	Venture incorp. at round	Venture age > median	Venture has high judge rank s.d.‡	Founder age above median	Founder # previous jobs > median	Founded prior venture
Low rank·Feedback· C_i	.13***	-.053	.064*	.34**	.034	.067
	(.031)	(.092)	(.036)	(.14)	(.09)	(.073)
N	4136	2224	4136	1778	4136	4136
R^2	.067	.046	.046	.048	.047	.056

Panel 3 (control coefficients not reported)

Dependent variable: Survival*

	(1)	(2)	(3)	(4)	(5)	(6)
Characteristic C_i:	Founder has MBA	Founder has top 10 MBA	Founder top 20 college	Founder top 10 college	Founder Harvard/ Stanford/ MIT	Founder is student
Low rank·Feedback· C_i	-.068	-.18	.11	.23*	.31*	-.43***
	(.11)	(.19)	(.11)	(.12)	(.16)	(.12)
N	4136	4136	4136	4136	4136	4136
R^2	.045	.046	.046	.047	.045	.051

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

Table 9: Effect of Dimension Rank on Venture Outcomes

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

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

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

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

Table 10: Effect of Negative Dimension-Specific Feedback on Venture Continuation

Sample restricted to losers of round

Dependent variable: Survival*

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

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

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

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

Table 11: Unconditional association between characteristics and venture abandonment/founding new venture

Sample:	All			Only founders that abandoned original venture		
Dependent variable:	Founder/CEO of subsequent venture†			# days to abandon		
	(1)	(2)	(3)	(4)	(5)	(6)
Venture incorp. at round	-.12***	-.035	-.046**	-37	21	12
	(.023)	(.023)	(.018)	(25)	(25)	(20)
Financing before round	-.1***	-.081***	-.094***	-43	-8.9	-8.8
	(.018)	(.017)	(.017)	(31)	(32)	(30)
Venture tech type IT/software	-.01	-.01	.017	14	16	1.3
	(.021)	(.02)	(.02)	(30)	(30)	(27)
Venture social/ clean tech	.057**	.07***	.045**	-85***	-74***	-59**
	(.023)	(.023)	(.022)	(25)	(26)	(24)
Venture in VC hub state	-.061***	-.065***	-.018	-46	-24	-32
	(.022)	(.021)	(.018)	(36)	(37)	(24)
Founder student at round	.03	.031	-.028	-39	-51	-113***
	(.047)	(.047)	(.022)	(52)	(54)	(33)
Founder age > median at round	.015	.013	-.025	43*	40*	60***
	(.017)	(.017)	(.016)	(22)	(22)	(21)
Founder # jobs before round	.011***	.012***	.011***	-.74	.36	.39
	(.0019)	(.0018)	(.0018)	(3.3)	(3.3)	(3.1)
Founder top 10 college	-.027	-.024	-.037*	9.1	3.1	1.1
	(.021)	(.02)	(.019)	(21)	(21)	(20)
Founder top 10 MBA	.034	.033	.11***	-126***	-138***	-176***
	(.028)	(.027)	(.025)	(38)	(43)	(38)
Founder has MBA	.063***	.043**	.051**	89**	70*	55
	(.02)	(.022)	(.021)	(36)	(37)	(35)
Founder has PhD	-.018	.0067	-.00097	86**	106***	88**
	(.02)	(.02)	(.018)	(37)	(38)	(35)
Competition f.e.	Y	N	N	Y	N	N
Competition-year f.e.	N	Y	N	N	Y	N
Year f.e.	N	N	Y	N	N	Y
N	3133	3133	3133	1495	1495	1495
R^2	.13	.23	.17	.086	.16	.11

Note: This panel contains the unconditional association of characteristics and outcomes, using the OLS regression: $Y_i^{Post} = \alpha + \beta' \mathbf{C}_i + \varepsilon_{i,j}$ where $\mathbf{C}$ is a vector of characteristics. †Effort was made to identify venture name changes to ensure that the "new" venture is not simply a name change; 18% of ventures in the sample changed their names. Standard errors clustered by competition-round. *** indicates p-value<.01.

Table 12: Effect of Judge Type on Responsiveness to Negative Feedback

Sample (among losers of round):	All						Only founders that abandoned original venture		
Dependent Variable:	Survival*			Founded/ CEO of subsequent venture†			# days to abandon		
Judge type (J):	VC	Corp. Exec	Founder/ Entrepr.	VC	Corp. Exec	Founder/ Entrepr.	VC	Corp. Exec	Founder/ Entrepr.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Low rank·Feedback·Judge type J	.015	-.1*	-.12**	.018	.016	.058*	193	18	-388***
	(.057)	(.052)	(.049)	(.036)	(.032)	(.032)	(119)	(150)	(87)
Low rank·Feedback	-.084*	-.029	-.035	-.027	-.027	-.036*	-58	69	105
	(.05)	(.038)	(.033)	(.032)	(.021)	(.019)	(101)	(69)	(66)
Feedback· Judge type J	.08	.044	.1**	-.033	-.0038	-.085***	-102	26	440***
	(.05)	(.04)	(.046)	(.024)	(.024)	(.025)	(118)	(125)	(91)
Low rank· Judge type J	.0051	.08**	.056	-.034	-.026	-.033	-37	-81*	-55
	(.037)	(.034)	(.035)	(.028)	(.026)	(.027)	(49)	(48)	(55)
Judge type J	-.037	-.076***	-.07**	.037*	.011	.058***	-27	11	-4.4
	(.027)	(.028)	(.029)	(.02)	(.019)	(.021)	(46)	(42)	(49)
Low rank among losers	-.11***	-.15***	-.13***	.046*	.038**	.036**	41	52	37
	(.028)	(.022)	(.019)	(.024)	(.018)	(.016)	(42)	(33)	(33)
Feedback	.0002	.034	.022	-.027	-.048**	-.019	85	8.5	-34
	(.05)	(.04)	(.035)	(.023)	(.019)	(.019)	(98)	(62)	(70)
Venture controls†	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year f.e.	Y	Y	Y	Y	Y	Y	Y	Y	Y
N	5100	5100	5100	5100	5100	5100	1065	1065	1065
R^2	.11	.11	.11	.13	.13	.13	.13	.13	.14

Note: This table shows estimates of the effect of negative feedback on venture continuation by judge type. Specifically, I interact the effect of negative feedback (Below median rank among losers·Structured feedback) with an indicator for whether one of the judge's in the round/panel had a certain type of job. Regressions are variants of Equation 2. * This measure for venture continuation is 1 if the venture had at least one employee besides founder on LinkedIn as of 8/2016. Errors clustered by competition-round or judge, depending on fixed effects. Structured feedback varies by event, so competition-round f.e. are not used. †Includes sector indicator variables, whether the company is incorporated, and whether the founder is a student. ††The attractiveness and size of the market. Note this reduces the sample as these variables are not available for all ventures. *** indicates p-value<.01.

Figure 1: Probability venture raised external finance after round (rank 1 is best)

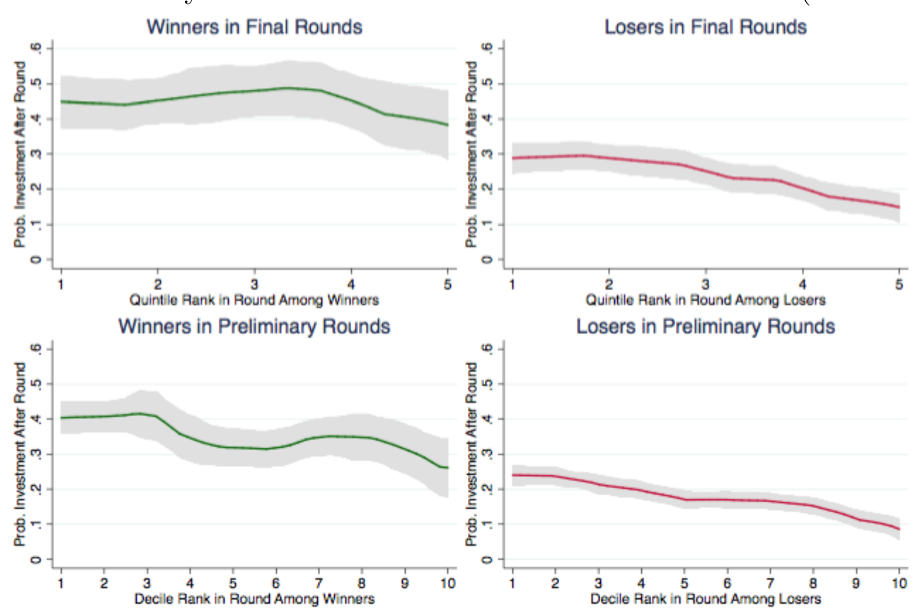
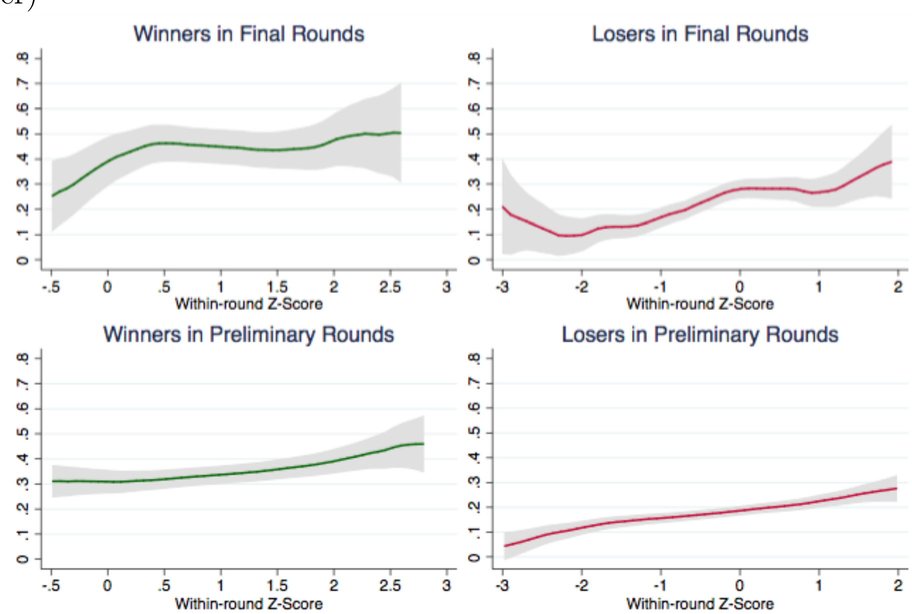


Figure 2: Probability venture raised external finance after round (higher z-score is better)



Note: The above figures show the probability of subsequent financing by venture percentile rank (top) and z-score (bottom) within a round. Local polynomial with Epanechnikov kernel using Stata's optimal bandwidth; 95% confidence intervals shown.

Figure 3: Probability venture survived (prelim. rounds, rank 1 is best)

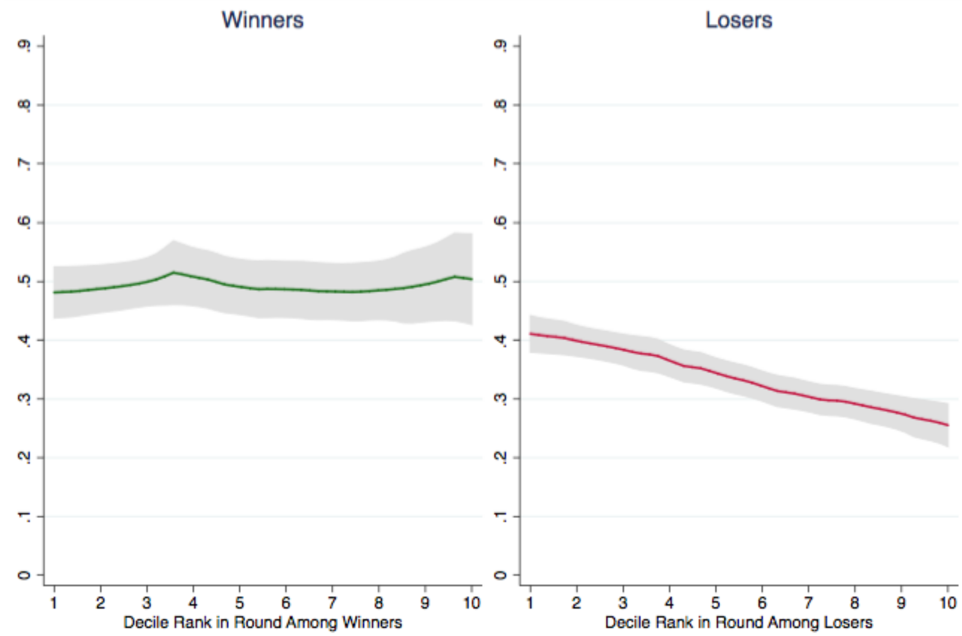
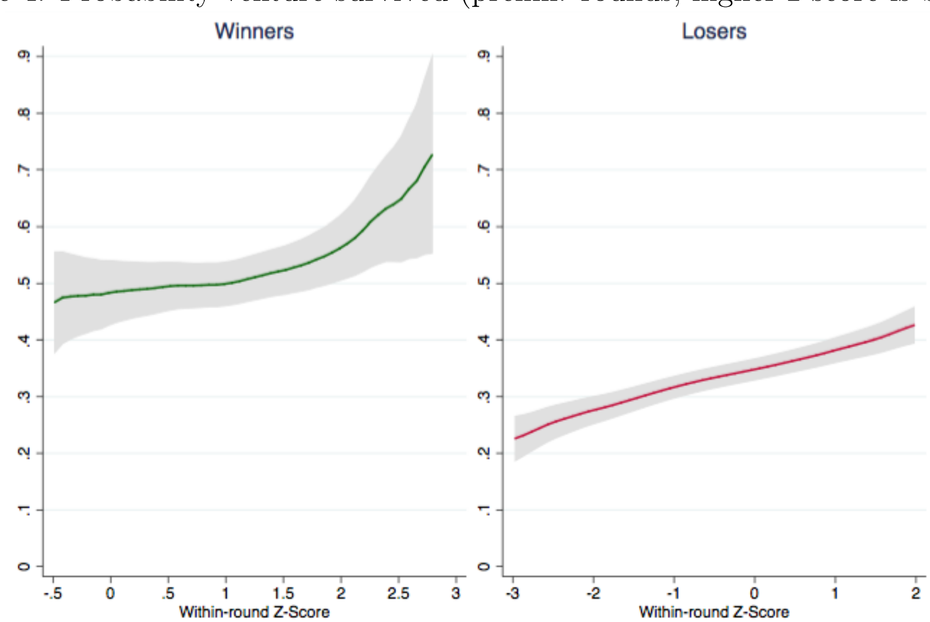


Figure 4: Probability venture survived (prelim. rounds, higher z-score is better)



Note: These figures show the probability that the venture survived (had at least one employee besides founder on LinkedIn as of 8/2016) by venture percentile rank (top) and z-score (bottom) within a round. Only preliminary rounds included. Local polynomial with Epanechnikov kernel using Stata's optimal bandwidth; 95% confidence intervals shown.

Figure 5: Distributions of Pre-Round Venture Characteristics

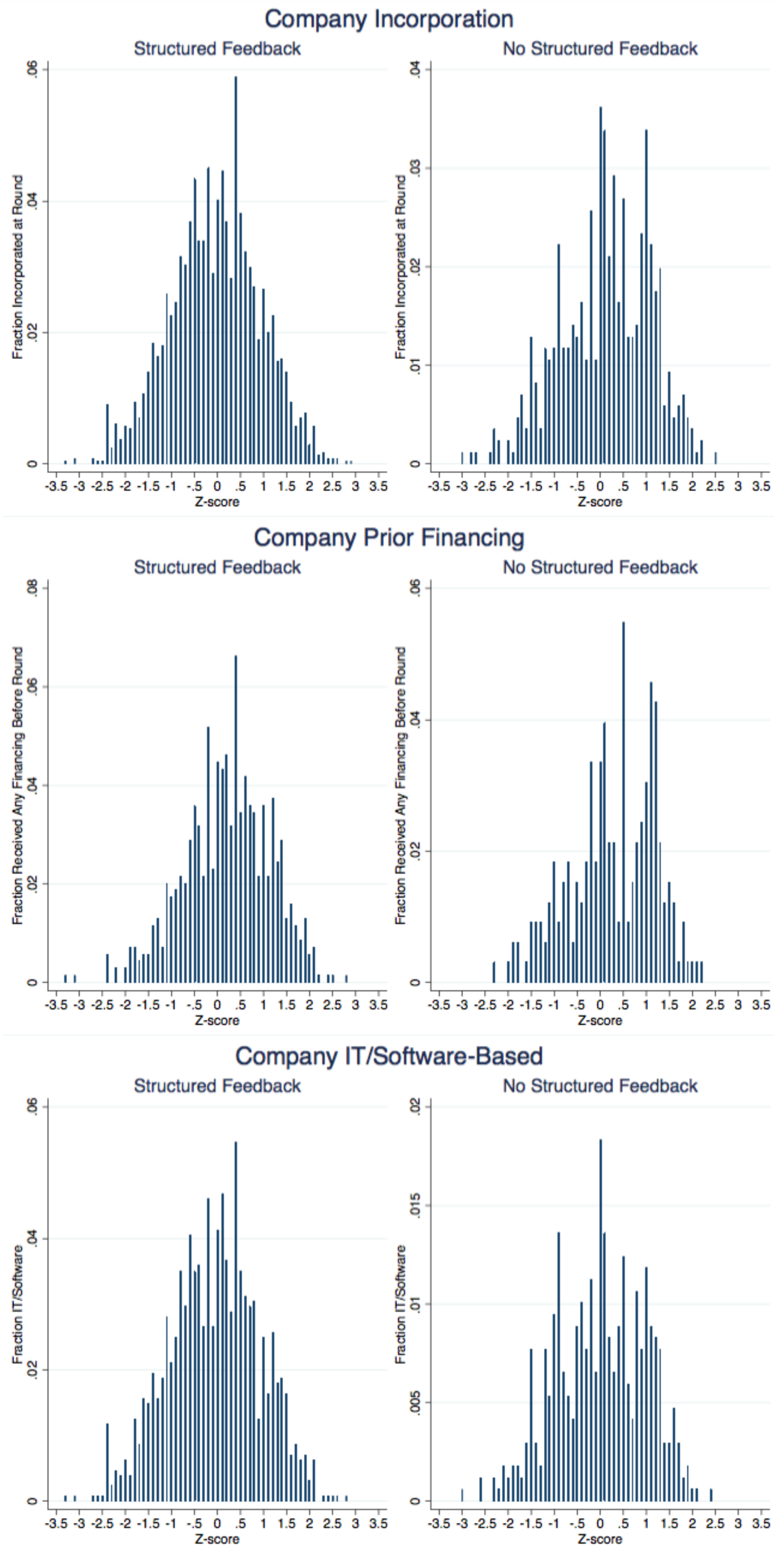
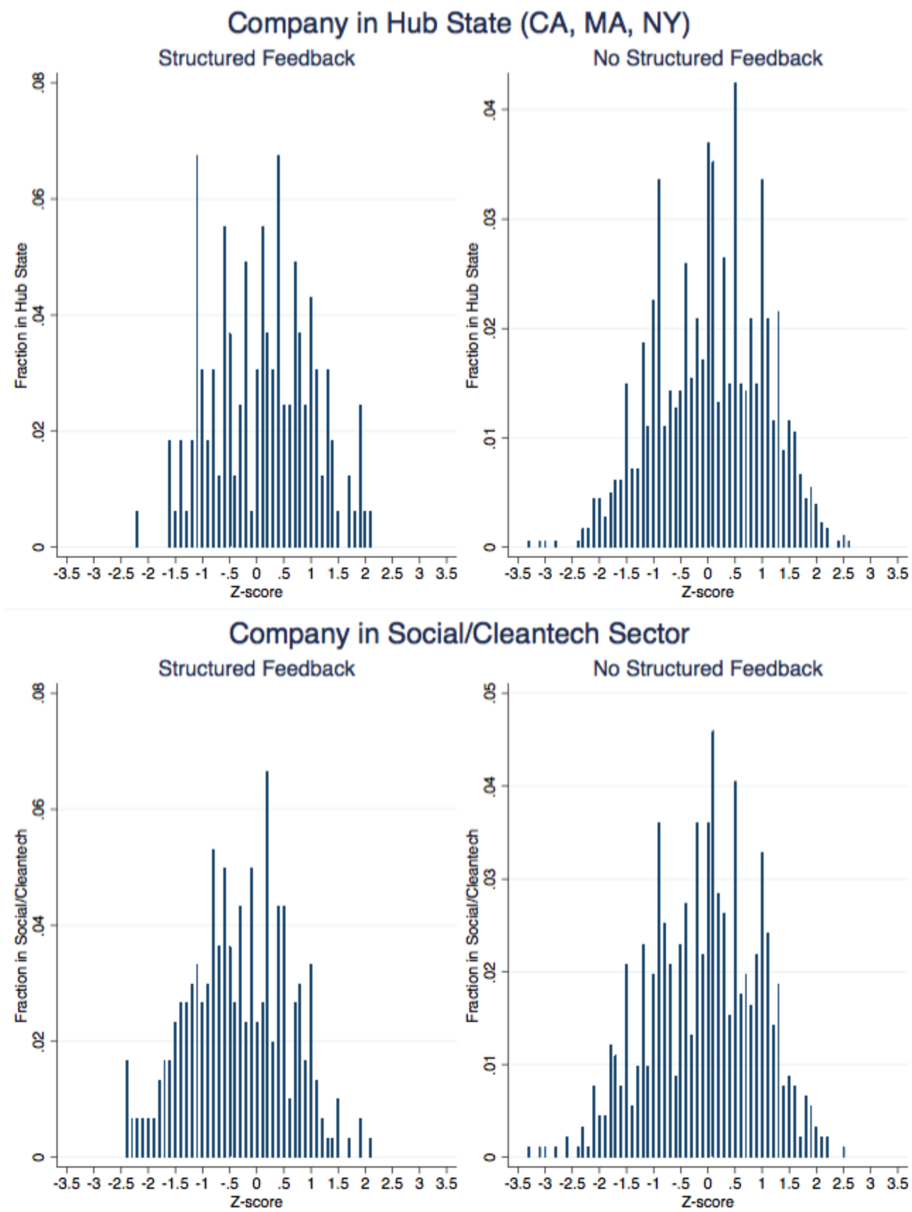


Figure 5 (continued)



Note: This figure shows spikes representing the fraction of all firms within 0.1 z-score bandwidths. For example, for variable X_i, the bar height for a z-score band of z in structured feedback competitions is: $\frac{\sum_{z,SF} Inc_i}{\sum_{SF} Inc_i}$.

Figure 6: Distributions of Pre-Round Founder Characteristics

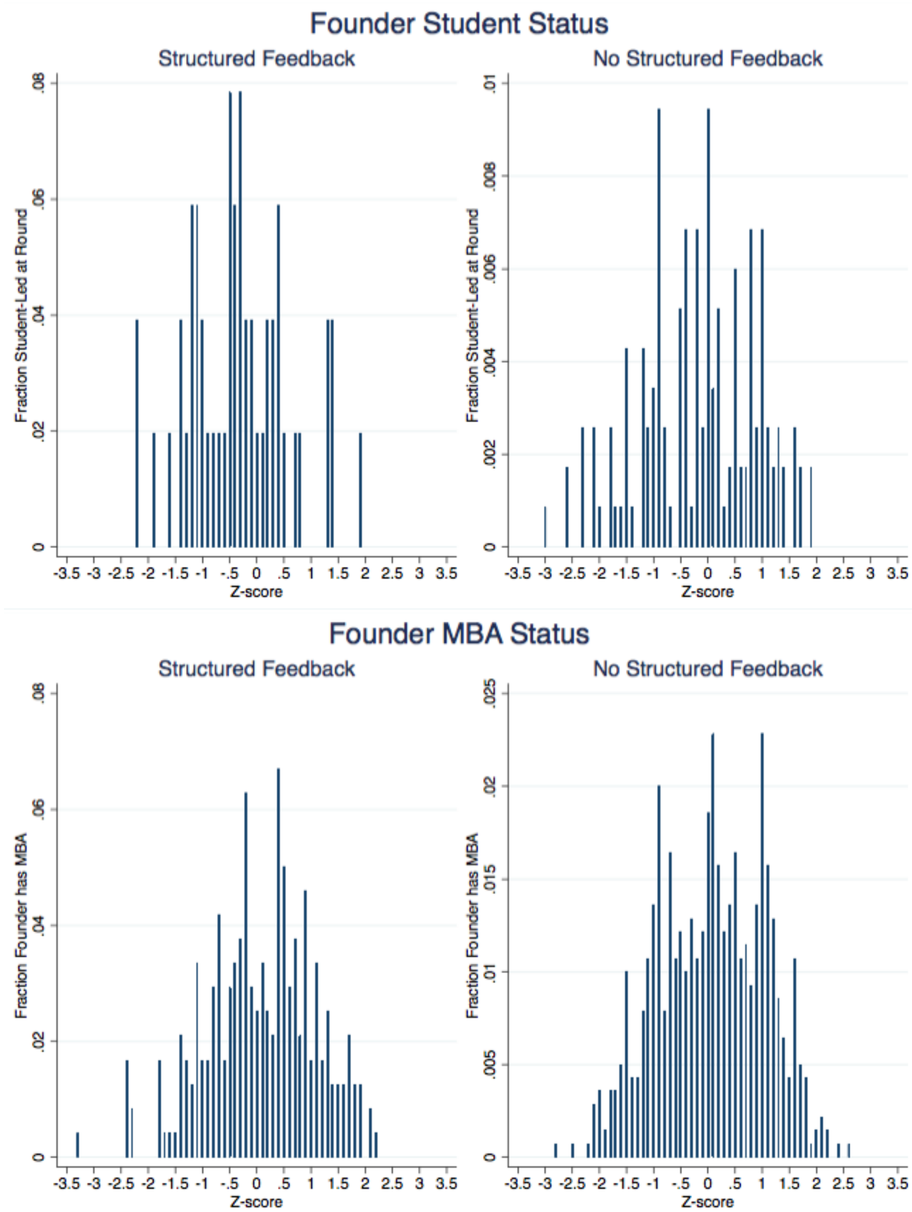
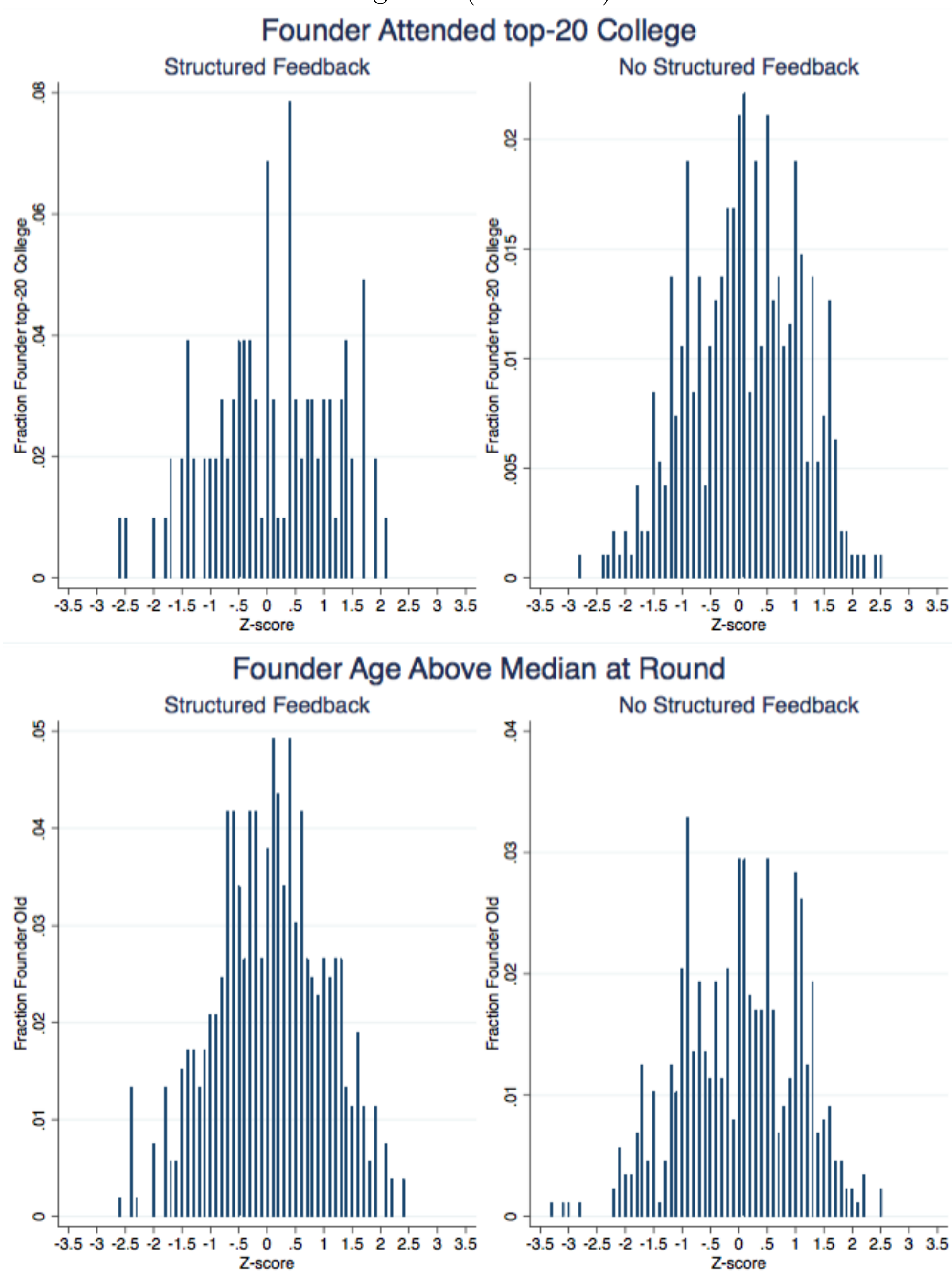


Figure 6 (continued)



Note: This figure shows spikes representing the fraction of all firms within 0.1 z-score bandwidths. For example, for variable X_i, the bar height for a z-score band of z in structured feedback competitions is: $\frac{\sum_{z,SF} Inc_i}{\sum_{SF} Inc_i}$.

Figure 7: Elite Founders and Success; Coefficients from Regressing Outcome on Elite Status within Competition

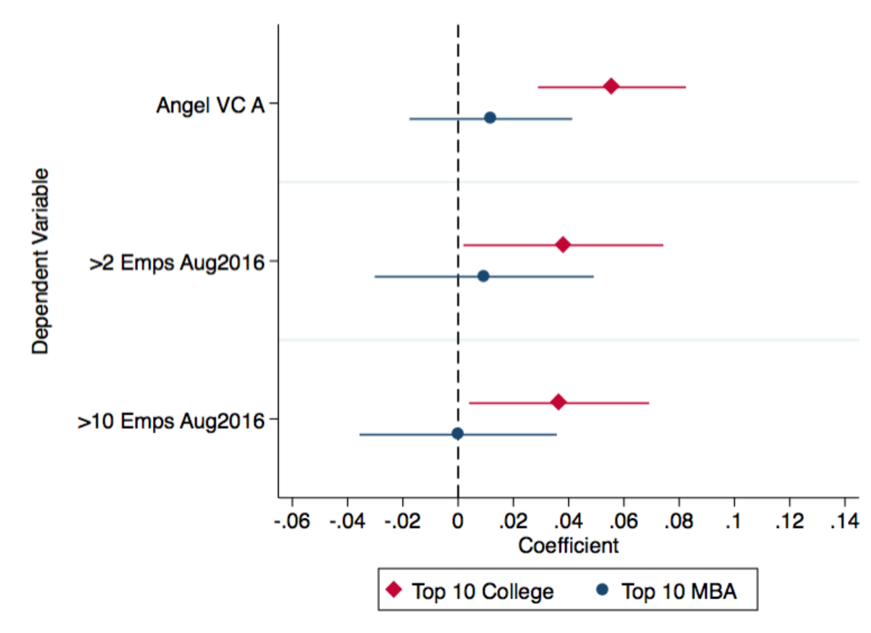
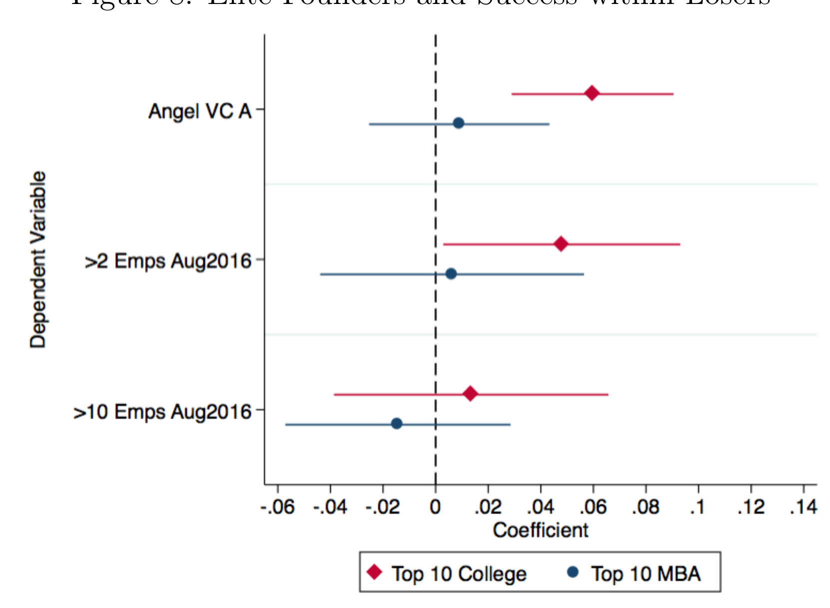


Figure 8: Elite Founders and Success within Losers



Note: These figures show coefficients and 95% confidence intervals from six regressions, each of which takes the form:$Y_i^{Post} = \alpha + \beta_1 \left(\mathbf{1} \mid C_i\right) + \gamma' \left(\mathbf{1} \mid CompRound_j\right) + \varepsilon_{i,j}$ where C_i is either an indicator for having a top 10 college degree or an indicator for having a degree from a top 10 MBA program. The bottom graph limits the sample to losers.

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