Information Frictions and Employee Sorting Between Startups*

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August 2025

Abstract

Would workers apply to better firms if they were more informed about firm quality? Collaborating with 26 science-based startups, we create a custom job board and invite business school alumni to apply. The job board randomizes across applicants to show coarse expert ratings of all startups' science and/or business model quality. Making ratings visible strongly reallocates applications toward higher-rated firms. This reallocation holds restricting to high-quality workers. Treatments operate in part by shifting worker beliefs about firms' right-tail outcomes. Despite these benefits, workers make post-treatment bets indicating highly overoptimistic beliefs about startup success, suggesting a problem of broader informational deficits.

Keywords: Hiring, job applications, startups, overconfidence
JEL Classifications: M50, M51

*We are grateful to David Autor, Shai Bernstein, Nick Bloom, Christian Dustmann, Claudine Gartenberg, Matt Gentzkow, Andrea Prat, Jonah Rockoff, Kathryn Shaw, Chris Stanton, Chris Walters, and especially Michèle Belot and Alan Benson for detailed comments. We also thank numerous seminar and conference participants for their comments. We thank the anonymous science-based entrepreneurship program (SEP) for its collaboration and generous assistance in conducting the RCTs. The study and a pre-analysis plan were registered with the AEA RCT registry under ID AEARCTR-0004242. Our pre-analysis plan is viewable on the AEA RCT registry. Two of the authors have performed paid work for SEP on topics unrelated to hiring and the workforce. Financial support from SSHRC and the Strategic Innovation Fund is gratefully acknowledged. We are deeply grateful to the Creative Destruction Lab for its support.

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1 Introduction

Hiring is a central issue in economics and especially in personnel economics (Ichniowski *et al.*, 1997; Staiger & Rockoff, 2010; Oyer & Schaefer, 2011; Bloom & Van Reenen, 2011). As surveyed by Benson & Shaw (2024), a growing body of work analyzes how firms select among workers, increasingly via randomized controlled trials (RCTs). Less is known about how workers choose among firms. Just as firms may have imperfect information about the skill of a potential employee, workers may have difficulty identifying "good jobs." Particularly for startup firms, who play a key role in overall job creation (Haltiwanger *et al.*, 2013), it may be hard for workers to separate firms with promising futures from lemons.

Consider a worker choosing where to apply. With established firms, she might compare pay packages, consult employer review websites, and talk to contacts who have worked there. In contrast, small startups often pay similar low base salaries (Sorenson *et al.*, 2021), are unlikely to have online reviews, and have few past or present employees. This assessment process for applicants is even harder for *science-based* startups, such as those in machine learning or quantum computing. In these fields, the technology can be hard to evaluate, especially for non-experts, and market demand can be uncertain. Workers with imperfect ability to evaluate firms may therefore apply to ones with limited potential. This harms workers and potentially economic efficiency more broadly if promising startups have trouble hiring good workers, a problem which is believed substantial in the qualitative management literature on startups (Wasserman, 2013; Harnish, 2014). While investors often conduct "deep diligence" to address information deficits before investing in startups, e.g., by paying outside medical school or computer science professors to evaluate the firms (Gompers *et al.*, 2020), potential employees presumably lack bargaining power to require such information.

How severe is this imperfect information to the functioning of labor markets, especially for startups? If credible information about firm quality was made available to workers, would they change who they apply to? Would they apply to *ex ante* better firms? We address these questions using two RCTs. In the *primary RCT*, we recruit 26 actively-hiring, early-stage startups from a world-leading science-based entrepreneurship program (SEP) with over $20 billion in equity from the first ten cohorts of startups (described more in Section 2). After these 26 startups provide job ads, we build a custom job board accessible to nearly 20,000 business school alumni. Besides startups' own ads, some applicants randomly receive coarse ratings of each firm's science quality from leading scientists and/or ratings about the firms' business model quality from experienced incubator staff. As in other job settings, applicants are free to use firm websites, press coverage, and so on to investigate firms they may apply to. In total, 1,877 applications are made by 250 job-seekers.

Science quality and business model quality can be viewed as determinants of the long-run productivity of science-based firms. Science quality refers to the quality of the firm's core technology or product, such as whether it is novel, does what it is supposed to do, and is well-understood by the founding team. Business model quality refers to whether the firm has a good plan for generating profits from the core technology or product.[1]

Applicants are unaware that information is randomized. From their perspective, the job board looks similar to other recruitment websites. Treated workers are exposed to expert ratings for all firms, allowing us to examine the impact on workers of *market-level* shifts in the precision of information (e.g., how would workers respond if the government or a jobs platform provided expert ratings on all startups in addition to startups' own job ads). The *secondary RCT* examines similar choices to the primary RCT, but in a highly controlled environment. In it, 191 MBA students examine several real startups and answer incentive-compatible belief questions (explained further below), just as in the primary RCT, but they state their hypothetical interest in working at the startups after graduation instead of making actual job applications.

It is not *ex ante* obvious how workers would react to expert ratings of science or business quality. They will not react if they already have precise information about these dimensions of firm quality.They will also not react much if they don't care much about these features. For instance, they may focus on other job characteristics like salary, industry, and city. Expert ratings predict firm success, both in past data and for firms in the RCT.

Our paper's main finding is that expert ratings substantially affect what firms applicants apply to. Both science and business quality ratings matter and both matter to a broadly similar degree. Relative to not providing information, providing positive information on science quality increases the probability a worker applies to a firm by 12%, while negative information decreases application probability by 24%. Likewise, a positive signal about business model quality raises application probability by 29%, and negative information decreases applications by 12%. Put another way, startups with above average business and science quality receive 11% more applications than those below average on each dimension when workers receive no additional signals. However, in the treatment where workers receive both quality signals, that gap increases to 80%.

To better understand these effects on worker preferences, we examine how treatments affect worker beliefs. Expert ratings substantially affect workers' perceptions of science and business quality of firms. They also affect workers' beliefs about whether firms will succeed.

[1]Business and science quality are not the same. Firms can be good at one but not the other. Consider Theranos, a health startup once valued at over $8 billion. There was huge demand for Theranos' product as marketed by its CEO, but underlying science was poor (Carreyrou, 2018). In our data, startups' science and business quality scores are uncorrelated, as detailed in Section 2.1.

To ensure that workers provide thoughtful answers regarding firm success, we incentivized worker beliefs using a betting game, developed in experimental economics, where participants could win up to $250 CAD (≈ $200 USD). The betting game incentivizes participants to provide honest beliefs about the probability observable events will occur. Even under significant incentives, workers are overoptimistic about firm success, and they dramatically overestimate the chance that firms will have a successful exit (i.e., a high-value acquisition or IPO) within a year. Despite this, workers update beliefs in response to expert ratings, particularly beliefs about whether firms will raise venture capital. These results suggest that beliefs about firm quality and success are a mechanism for our paper's main finding.

Turning to treatment effect heterogeneity, we find limited heterogeneity for most dimensions of worker and firm quality. There is no significant heterogeneity by worker startup experience or STEM background, though men respond more than women to science quality ratings. A critical dimension of heterogeneity in theory is worker quality, which we measure by having a startup-focused HR expert rate resumes. If anything, higher-quality workers respond more strongly to our intervention than lower-quality workers. Importantly, there is no evidence that our overall treatment effects are driven by low-quality workers, suggesting that interventions like ours would not simply drive low-quality workers to high-quality firms.

Returning to whether results were obvious, we consider this both via an economist survey and via the revealed preference of startups in the RCT. Following DellaVigna & Pope (2018), we ask economist experts in related fields to predict the main results of the primary RCT in terms of job applications. Economists correctly predict the qualitative finding that expert ratings affect applications, but substantially underpredict the quantitative magnitude. While we find that high-rated firms get 80% more applications than low-rated firms when both ratings are shown, the median economist prediction is a difference of only 25%, and 86% of economists underpredict the magnitude. In terms of revealed preference, our RCT was structured where startups wrote their own job ads without any restrictions on our end. Despite this, only 4 of our 26 startups mentioned any hard business or science quality signal, a percentage in line with what we find in AngelList ads more broadly.

Our paper contributes to four literatures. First, it contributes to work in personnel economics, organizational economics, and labor economics on worker/firm matching, where a growing body of research uses natural experiments or RCTs to understand how workers choose among jobs or firms. To our knowledge, our paper is the first RCT in this literature to study how workers choose between startups, as well as the first about the role of imperfect information about firm quality in affecting how workers choose among firms.[2] We provide

[2]Other recent studies using natural experiments or RCTs to understand choice among jobs or firms include Ashraf *et al.* (2020), Flory *et al.* (2015), Hedegaard & Tyran (2018), and Wiswall & Zafar (2018).

the first evidence that even highly-educated workers have limited ability to identify high-quality startups. Broadly speaking, our results also speak to understanding how persistent differences across firms in performance (Ichniowski *et al.*, 1997; Bloom & Van Reenen, 2007; Bloom *et al.*, 2013, 2019, 2022) are appreciated by workers. Our results are broadly consistent with Bernstein *et al.* (2022), who show that job interest in startups rises when a prominent venture capitalist invests.

Our results provide the first evidence that workers would apply to substantially different firms if they had more precise knowledge about the science or business model quality of firms. Of particular relation to our paper is Belot *et al.* (2018, 2022a), who also conduct RCTs where information about aspects of jobs is randomly provided to jobseekers. Belot *et al.* (2018, 2022a) show that providing generally available labor market data to unemployed jobseekers, such as skill requirements for different jobs, has sizable effects on their job applications. Our results indicate that informational deficits exist in labor markets even for highly skilled and advantaged workers.[3]

Second, our results relate to work in personnel economics on startups. A central question is why startups and other firms often pay workers with equity instead of salary, common answers being taxes, credit constraints, or aligning worker and firm beliefs (Oyer, 2004; Oyer & Schaefer, 2005). Bergman & Jenter (2007) propose an alternative: workers overestimate the probability of positive events occurring to workers. To our knowledge, our paper presents the first direct evidence, obtained using incentivized experimental methods, that workers overestimate the probability of a successful startup exit.[4] Thus, our work suggests the possibility that firms may find it cheaper in expected value to pay workers in equity instead of salary. Moreover, since workers respond strongly to expert ratings, workers may face challenges in accurately evaluating expected returns from equity-based compensation in the absence of expert ratings. Our results bear on discussion by legal and business scholars on potential regulation of worker pay at startups (Aran & Murciano-Goroff, 2023).

Unlike us, other papers in this literature generally focus on established firms or all firms, and analyze other characteristics like whether a firm is family-friendly or lets scientists publish. Benson *et al.* (2020) create new firms on mTurk and show that randomly endowed better reputations increase job fill rates. Working with an agency staffing for thousands of firms, Bapna *et al.* (2021) show that rejection email content substantially affects whether workers apply to firms in the future. Appendix B.1 discusses more on related work outside of economics.

[3] Belot *et al.* (2018, 2022a) study information frictions about what types of jobs are available or would be a fit, while we study information frictions over firm quality. Dustmann *et al.* (2022) show that a non-information policy, higher minimum wages, reallocates workers to better firms. Jäger *et al.* (2024) show workers have biased beliefs about their outside option in the broad German labor market. Many papers use RCTs and surveys to examine information frictions outside of job decisions (e.g., Angelucci & Prat, 2024).

[4] There is much work on CEO overconfidence and the overconfidence of entrepreneurs (Hall & Woodward, 2010; Moskowitz & Vissing-Jørgensen, 2002; Puri & Robinson, 2013). Our focus is overconfidence of lower-level workers.

Third, our paper contributes to work in entrepreneurship on resource acquisition. Work examines why good startups have trouble acquiring resources like capital and partnerships (Lerner, 1995; Hsu & Ziedonis, 2013). Our work helps address why good startups have difficulty getting workers, and suggests good startups may have trouble hiring because workers don't know which startups are most promising.

Fourth, our paper contributes to work in behavioral labor economics. It has been shown that workers exhibit "behavioral" tendencies in the context of job search, including present bias (Belot *et al.*, 2021), overconfidence (Spinnewijn, 2015), and rejection aversion (Bapna *et al.*, 2021). Our paper shows that workers also exhibit overoptimism, believing that firms will be more successful than they actually are.e

2 Context, RCT Design, & Theoretical Framework

Context. SEP is a non-profit, selective, nine-month program operating across several high-profile business schools in seven countries (as of 2023). The core mission is to provide mentorship to early-stage, science-based startups. SEP is akin to a forum that convenes angel and institutional investors and technical experts five times a year to offer structured mentorship.[5] At the time of our RCTs, about 900 ventures had gone through SEP, including 389 in the 2018-19 cohort, indicating SEP's rapid growth.

SEP describes its program as suitable for *seed-stage* startups, meaning ventures expecting to raise capital during or after the program. Ventures in other stages of their financing life-cycle may still be admitted if they are expected to benefit from SEP. However, ventures believed not to be scalable are not usually admitted. As SEP progresses through each of its five meetings, a subset of startups gets cut from SEP. The average graduation rate over the first seven cohorts of SEP is roughly 40%.

To support startups with distinct technological trajectories, SEP is offered through specialized streams such as machine learning, quantum machine learning, space, health, and energy. Streams use staff and mentors with corresponding experience or expertise. For example, the space stream's mentors include three astronauts, leaders of public and private space exploration entities, and investors in the launch and propulsion sectors.

There are three features of SEP that make it an ideal setting for our research questions. First, SEP is one of the largest and most esteemed programs of its kind in the world. SEP's stature means it accesses world-class science and business expert raters, and also that a sizable number of startups want to participate in the RCT.

[5]SEP's mentorship structure aims to help startups design and prioritize short-term measurable objectives. For brevity, we avoid describing aspects of SEP not relevant to our study.

Second, SEP startups are the type of startups who frequently engage in hiring. SEP is not for student companies or projects. Rather, all firms in SEP are existing startups, many of which include world-leading scientists on their founding team, who are thought to possess a chance of becoming a large, venture-backed business.[6]

Third, ours is a natural setting for analyzing uncertainty over business and science quality, as SEP firms tackle frontier science and business problems. This is a key feature of SEP given our research questions, though we fully acknowledge that our findings may not necessarily apply to all young firms (e.g., firms dealing with simple consumer problems).

2.1 Primary RCT: Job Board

Origins of the RCT and firm recruitment. Hiring is a natural part of growth for SEP firms, and one that firms often struggle with anecdotally, even when they are high-quality. In 2019, SEP decided to launch a pilot job board, which we helped design and implement. A key factor for SEP to engage in the RCT was a belief that its strongest firms were having a hard time hiring, perhaps because it was hard for workers to identify their quality. Past research argues that startups may face challenges competing with established firms for talent (Manchester *et al.*, 2023), and this is also true in our setting. However, SEP was especially concerned that job applicants face particular challenges in distinguishing between startups and this helped motivate SEP's desire to conduct the RCT. Like a social planner, SEP's mandate is to create economic value. Given most startups fail, SEP is much more interested in helping its strongest firms succeed rather than improving outcomes for its weaker firms.

SEP emailed the 183 firms who were part of the 2018-2019 cohort of firms that participated in the program at SEP's headquarter location, and asked if they wished to participate. Of the 183 firms, 26 firms (or 14%) chose to participate. As analyzed later in Section 3, participating firms are broadly representative of SEP firms. Firms were told truthfully that experimentation may occur, but were not informed specifically about the nature of the RCT. Like most startups, SEP firms tend to pay relatively low wages, but offer equity to new hires. To participate, each startup wrote a short ad about their firm.

In creating the job board, a key goal was to make it similar to existing job boards for startup firms (e.g., AngelList careers, or Y Combinator's workatastartup.com). Specifically, SEP wanted its job board to be easy to use, visually appealing, and feature the type of information that would appear on AngelList. Firms were assigned positions on the board

[6]To fix ideas, a reader might think of typical SEP founders as two computer science professors with an advancement in image selection for machine learning by autonomous cars, or of a doctor and scientist with a new application of machine learning to healthcare. This is distinct from student startups, where time commitments are limited and who are less likely to grow and hire people.

alphabetically from left-to-right and top-to-bottom based on the founders' first name.[7] Figure 1 shows the type of information workers saw when they clicked on a firm logo.

Worker recruitment. We recruited applicants for the RCT by partnering with two prominent North American business schools. Each agreed to email their alumni list customized and trackable links. The alumni emailed were graduates from MBA, specialized masters, and undergraduate business programs.[8]

RCT procedure. Potential applicants arrived at the job board via a code hidden in the website URL which triggered a randomized change in the visibility of two aspects of information about each firm, for four total arms. When a potential applicant viewed details of a firm, they would see, in addition to a link to the firm's website and a self-written description of the firm, one of four information treatments: control, information about business model quality, information about science quality, or both. Treatment was assigned by applicant, so each applicant saw expert ratings for all firms or for none. Applicants were unaware that other applicants may see different information or that they would be part of an RCT.

Since workers accessed the job board via a custom link, we can match the randomization each received to their name and ensure that the treatment stays constant in case they visit the board multiple times. Randomization in the emailed links was stratified by gender, graduation year, and current city, where known. The job board is a standard website viewable in any browser, so there was no restriction on workers' ability to search for further information about any firm, or even attempt to contact firms before applying.

At any point after investigating these firms, over a roughly four-week period, workers could upload their resume to the centralized job board application system. Applicants were told that a small number of resumes would be highlighted in emails to each firm. The stated reason was that, since these firms are small, SEP did not want to overburden founders with hundreds of resumes. For the same stated reason, applicants were only permitted to apply to up to ten startups each, and were asked to rank their relative interest in each firm.

Applicants were told that the likelihood of having their application sent to a firm was higher for firms they ranked higher, and further, that the names would be chosen using an incentive-compatible mechanism: random serial dictatorship (RSD) (Abdulkadiroglu & Sonmez, 1998). Under RSD, workers are first placed in a random order, and firms are given a fixed number of "slots". Since workers can rank up to ten startups, there are up to ten rounds. In each round, the algorithm goes through workers in order, matching workers

[7] It wasn't possible logistically to randomize the visual position of firms across workers. This isn't an important concern because our results use *within-firm* differences in whether applicants see expert ratings.

[8] From the 1st business school, we contact 5,681 undergraduate alum (graduated from 2008-2019) and 7,894 graduate alum (graduated from 1946-2018). From the 2nd b-school, we contact 3,701 undergrad alum (graduated from 2009-2015) and 2,083 grad alum (from 2009-2019).

with their highest-ranked firm with remaining slots. This is analogous to the draft in the National Basketball Association where draft order is random and the number of slots (i.e., teams per player) is one. Although we left details of RSD in the experiment to a linked text, we explained that an award-winning algorithm ensured that it was in the best interest of candidates to truthfully rank firms in order of interest:[9]

> "To avoid inundating these start-ups with an excessive number of resumes, we have agreed to forward a limited number of resumes to each startup. **It is in your interest to state your true preference ranking!** Specifically, the probability your information is sent to a given venture is strictly higher the higher you rank a venture. An algorithm by leading economists ensures that there is no benefit to manipulating your true preference about which ventures you would like to meet." [see Appendix D for screenshots of how this appeared to subjects]

Ten application cap. The limit of ten applications imposes a "cost" on applicants via the opportunity cost of not signaling interest in other firms. This cost is meant to mimic the actual cost of applying for jobs in a less centralized job search environment.[10] As described in Section 3 below, we investigate four pre-registered outcomes using these rankings: the probability of applying at all, the probability of listing a firm in one's top N ranks (we use top rank and top three rank), and the absolute rank of the firm (with unranked firms treated as having been ranked $N + 1$). As seen below, results are similar across all four outcomes, including top rank and top three rank, which are not affected by the ten application constraint, indicating that the constraint does not drive our findings. As discussed in Section 3 ("Worker selection into the RCT"), there is no evidence that workers found the ten application cap to be strange or that it limited who wished to participate. As seen in Table A2, 45% of applicants hit the cap, and the rate is similar across treatment arms (p-val of equality across four arms equals 0.17).

Worker beliefs. After submitting their ranked list of firms they wished to apply to, workers were asked to voluntarily evaluate three randomly selected ventures from the set of

[9] Past work shows that RSD often yields incentive-compatible choices (Chen & Sönmez, 2002; Artemov *et al.*, 2017). We expect our high-skill subjects have an easier time understanding it than most. RSD is incentive-compatible for interest in *getting an interview*, not in getting a job, as workers may worry that better firms get better applicants, and thus the chance of getting a job conditional on an interview is lower. We view this as a feature and not a bug of our RCT, as this issue would occur in a full-scale policy rollout where all workers see ratings. RSD is used around the world in many job markets (Fadlon *et al.*, 2024).

[10] On many job boards, applicants are unrestricted in the number of firms they can apply to, but each application costs time due to different application formats. However, some job boards and job matching processes restrict the number of applications, e.g., the National Resident Matching Program for US physician residents imposes extra fees for applying to more than 20 programs and forbids applying to more than 300. Our anecdotal understanding is that firms and workers in the RCT saw the 10-firm constraint as very natural.

firms they had just considered applying to. This request was made only after the ranked true applications were sent, to limit Hawthorne effects. For each job seeker, the three firms in question remained the same for all belief questions. We asked candidates to estimate each firm's science quality, business quality, probability of raising capital at a valuation of $1 million or more within a year, probability of having an IPO or being acquired for at least $50 million within one year, and their interest in working for the company. Before answering the two questions on probabilities (i.e., the IPO and capital raise questions), subjects were given a standard "explanation of probabilities" as developed in the work of Charles Manski, and as used in many subsequent papers and large-scale surveys (see the review by Bruine de Bruin *et al.* (2023)). The explanation gives examples of probabilities, and is designed to be simple and avoid leading subjects in any way (see Appendix D for the exact wording).

The probability questions were incentivized with a risk-invariant quadratic scoring rule (McKelvey & Page, 1990), under which applicants can win up to $250 CAD (≈ $200 USD) on the basis of the accuracy of their predictions. Following past applied work using this scoring rule (Hoffman, 2016), we explain that the incentive system makes it optimal to state one's true beliefs, and we provide math formulas separately for people to look at if interested. [11] Workers' perceptions of startups' science and business quality cannot be incentivized because they are subjective.

Science score. To grade the quality of each startup's underlying science, we use detailed scientific evaluations that SEP conducts to assist intake into each cohort. These evaluations are otherwise not made public. The assessment is performed by a group of distinguished university scientists and research scientists from Canada's National Research Council. The assessments are based on a 30-minute interview with the firm by a scientist with expertise in the core technology (e.g., a machine learning startup is evaluated by a scientist with expertise in machine learning), as well as detailed written materials provided by the firm before the interview. The scientists are paid to do the assessments as part of their salary from the National Research Council, and thus, take the assessments quite seriously.[12]

When onboarding scientists to conduct assessments, SEP tells scientists to focus on the viability of the core technology and the technical ability of the founders to execute, regardless of what they think about the market potential or business viability of the technology. In the RCT, ventures on the job board are divided into above- and below-median for science quality.

[11] If a subject guesses correctly and states confidence level c, they get a lottery with a $2c - c^2$ probability of winning $250 CAD and a $(1-c)^2$ probability of receiving zero. If they guess incorrectly, they get a $1 - c^2$ probability of winning $250 CAD and a c^2 probability of receiving zero. Under these incentives, it is optimal to report one's true confidence. Appendix B.2 further discusses past work supporting the reliability of eliciting beliefs this way.

[12] National Research Council is the main research institute in Canada's federal government. SEP and National Research Council have an agreement for assessments to count toward one's salary.

A binary version of the rating was used to make the ratings easy to interpret for jobseekers, and coarse ratings are common for many ratings (e.g., pass/fail for health inspections).[13]

Business model score. The business quality score is performed by full-time staff from the SEP who specialize in evaluating startups.[14] Like the science score, the business model score is based on an in-person interview and extensive supporting documents. The score is normally assigned when firms apply to SEP. However, since the job board occurred a year after the business model was first evaluated, and firms often update their model, SEP staff re-evaluated the business model score. They did so based on their interactions and conversations with the firms about the business model. SEP staff evaluate business quality along three margins: size of the market being targeted, quality of the business model, and ability of the team to execute on this opportunity. These three margins are considered critical by venture capitalists and are a standard way of measuring a startup's potential (Gompers *et al.*, 2020). We average these three scores to create the overall business model score.

The evaluators were unaware which aspect of their evaluation was being used, or the purpose of the re-evaluation. However, like the scientists, we believe that the business experts took the scores very seriously, as evaluating startup business models is a central part of their job at SEP, and they have career incentives to produce thorough and accurate ratings. Once scores were collected, ventures on the job board were divided into above- and below-median business model scores, dichotomized to be easy to interpret for jobseekers.

Critically, the business model score does not include any evaluation of the firm's underlying science. The evaluators are not PhD scientists and are told to focus solely on evaluating firm business models. Business model scores are uncorrelated with science scores for our 26 firms, both in continuous form (correlation coefficient of $\rho = 0.06$, which is indistinguishable from zero with $p = 0.75$) and when they are dichotomized ($\rho = -0.23$, $p = 0.27$).

Expert score predictiveness. To what extent are expert ratings predictive of actual firm success? We consider this question both in terms of historical data from SEP and for the 26 startups in our primary RCT, and find evidence of predictive power in both.

While the firms in the RCT are both relatively small in number and recently founded, we exploit the fact that science ratings have been conducted on SEP firms for several years. Table 1 examines the correlation between expert science ratings and two outcomes, namely,

[13]The number of scores above/below median is not perfectly even because the underlying score is discrete (1-5 for science, 1-10 for business). For ethical reasons of protecting startups, SEP required that information be presented to subjects as "Science Quality was Rated as Above Average: Yes" or "Science Quality was Rated as Above Average: No." This phrasing leaves some ambiguity about the score and is likely to be a more policy-relevant way of presenting scores in other contexts.

[14]These staff are analogous to portfolio managers at venture capital firms who narrow the list of possible investments for partners. In fact, SEP staff often go work for venture capital firms as portfolio managers.

(1) whether the firm graduated from the SEP program and (2) whether the firm raised money after the SEP program. As seen in Panel B, a 1σ increase in science rating predicts that a firm will have a 9.4 percentage point ("pp") higher chance of raising money after the SEP, off an overall mean of 25%. Likewise, a 1σ increase in science rating predicts a 9.2pp higher likelihood of graduating from the SEP program, off a mean of 41%.

Turning to the firms in our RCT, of our 26 ventures, by August 2022 (i.e., three years after the start of the RCT), 17 were still in business ("survival"), 8 had publicly raised an additional $4 million USD or more, and a further 3 had hired at least 10 employees ("raised or hired"). Both business and science ratings are predictive of these outcomes: an above-average business model rating increases the probability of survival from 50% to 79%, while an above-average science rating increases it from 64% to 67%. More starkly, an above-average business model rating increases the probability of "raised or hired" from 42% to 50%, and an above-average science rating increases "raised or hired" from 29% to 58%.

Remarks on the design. Note the most important aspects of this information treatment. First, the business model and science evaluations were performed by experts in the respective domains, using information that goes well beyond what would generally be found on a company website. Second, above-average and below-average in the information treatments were relative to the selection of companies on the job board. These ventures, just by virtue of having taken part in the SEP, are already well in the upper tail of quality of all tech-based startups. Third, workers are shown truthful information at all times, where we vary the amount of information shown to the worker. That is, the information treatment is a coarse signal (a binary above/below average rating) where we either show the true binary score or not. This is a contrasting approach to an audit study where participants receive the same amount of information but certain information elements are randomly varied.

Finally, the information treatment here is analogous to treatments that policymakers, incubators, or job search websites could pursue. For instance, a logo denoting firms in an incubator that were thought to have the best underlying science, or a promising business model, could be added to the incubator's employment website. Job websites, or startups themselves, could explicitly highlight competitive markers of quality such as participation in a top incubator, investments from prominent venture capitalists, or the scientific renown of the founding team. In the discussion of our results, we give evidence about the extent to which this currently happens.

Timing & implementation. The job board began in May 2019. Emails were sent in 3 batches (batch 1 = MBA alum of 1st business school, batch 2 = undergrad alum of 1st b-school, batch 3 = MBA and undergrad alum of 2nd b-school), as detailed in Appendix E. For each batch, the board was active for about one month.

After the application period ended, startups were emailed a link with secure access to the resumes of applicants who used the job board. They also received the name of ten candidates who showed particular interest in the firm, as measured by the RSD mechanism (Appendix B.4 gives further details on RSD implementation). Four months after the job board closed, we followed up with ventures about interviews or hires made on the basis of these applications. We follow startups through August 2022 to track outcomes.

In total, 250 workers applied to at least one firm, and 1,877 total applications were submitted (i.e., the firms were ranked by an applicant). Most workers applied to 5-10 firms. Time stamp data indicate candidates took the process seriously. As seen in panel (a) of Figure 1, jobseekers land on a webpage where they can browse different firms, as well as enter their contact information and job application rankings. After jobseekers first click on the data entry part of the webpage (which is presumably after they have started browsing the firms on the job board), the median time spent on the job board and answering the beliefs questions was 22 minutes (25th percentile = 12 mins, 75th percentile = 54 mins).

2.2 Secondary RCT: MBA Student Experiment

In March 2018, we conducted a secondary RCT with MBA students applying for competitive entry to a course associated with the SEP. This allows us to perform similar analyses to the primary job board RCT, but under highly controlled conditions that minimize inattention. As part of admissions to the SEP MBA course, candidates evaluated 3 randomly chosen firms among firms that had recently participated in the SEP. To do so, they received corporate information about 3 firms from a set of 20, including descriptions of the firm's product, founding team, and business strategy, plus technical briefing documents. For each firm, MBA students filled out a quantitative evaluation and answered some qualitative questions on suitability for SEP.

This evaluation was performed in a controlled classroom environment. Students had 40 minutes to evaluate each firm, and most students took most of the full 2 hours. Students were also told that their responses, particularly the qualitative questions not part of our study, would determine whether they were accepted into the SEP MBA course. Thus, students took it seriously. As in the primary RCT, students were given firm documents that were randomized to include no additional information, a binary expert evaluation of the firm's science, a binary expert evaluation of their business model, or both, and within student, all firms are treated the same (e.g., one sees a business expert rating for all firms). The source of these evaluations was identical to the primary RCT, although the firms were not identical.[15]

[15] In Appendix F, we provide examples of a firm dossier and expert ratings shown to MBA students.

Instead of providing a ranked ordering of firms using incentive-compatible RSD, students were asked how interested they would be in working in the firm after graduation on a 1-5 scale.[16] We also elicited the same beliefs about firm outcomes and the perceived quality of science and business as in the primary RCT, and using the same procedure (i.e., using incentives for beliefs about firm outcomes). This allows us to analyze worker beliefs using pooled data from both RCTs.

2.3 Theoretical Framework

In Appendix C, we provide a simple equilibrium model of costly job application where workers observe the quality of firms imperfectly when deciding whether to apply. For some firms, workers are unable to separate productive from less productive ones. Reducing this imperfect information via expert ratings increases applications to above-average firms and decreases applications to below-average firms. This prediction holds even when jobseekers are forward-looking about competing with one another for jobs at better firms. Although our actual RCT was designed and powered to study applications instead of hiring outcomes, we show in the model that expert ratings lead better firms to attract higher-quality hires.

Ultimately, whether expert ratings affect job applications and beliefs about firm success and quality is an empirical question. Also, it is not clear which dimensions of startups (science or business quality) matter. We turn to these next.

3 Empirical Strategy and Randomization

Outcomes. Via the RSD mechanism, our RCT uses applicant rankings to determine which job applications are highlighted to firms. Thus, we measure worker interest across firms using these rankings. In our pre-analysis plan (available at www.socialscienceregistry.org/trials/4242), we specified that we would consistently analyze four functional forms:

1. Whether a job candidate ranked a firm at all. We refer to this generically as whether someone applied to a firm.

2. Whether a firm was a candidate's top choice.

3. Whether a firm was in a candidate's top 3 choices.

[16] Job applications are the key object of our study. Because expressed interest in the secondary RCT cannot be incentivized, we restrict our main results on application behavior to the primary RCT. Results using the non-incentivized job applications from the secondary RCT yield similar conclusions.

4. The normalized rank of a firm. Specifically, a top ranked firm gets a score of 10, the second rank firm a score of 9, ..., the 10th rank firm a score of 1, and unranked firms a score of 0. We then normalize this score.

Job applications are a central object of interest in personnel economics and provide the cleanest expression of jobseeker preferences. We discuss the subsequent outcomes of interviews and hiring in Section 4.2.

Empirical strategy. Our pre-registered regressions are as follows:

$$\begin{aligned} y_{nf} &= \alpha_0 + \alpha_1 \text{GotBizInfo}_n + \alpha_2 \text{GotBizInfo}_n \times \text{GoodBizFirm}_f + \boldsymbol{X}_{nf}\boldsymbol{\alpha} + \varepsilon_{nf} \\ y_{nf} &= \beta_0 + \beta_1 \text{GotScienceInfo}_n + \beta_2 \text{GotScienceInfo}_n \times \text{GoodScienceFirm}_f + \boldsymbol{X}_{nf}\boldsymbol{\beta} + \xi_{nf} \end{aligned}$$

Here, n denotes workers and f denotes a firm that a worker evaluates. Thus, an observation is a worker-firm. The outcome, y_{nf}, will be one of the above outcomes. The regressor GotBizInfo_n measures whether subject n is randomly assigned to receive information about firm business quality. Likewise, GotScienceInfo_n measures whether a subject randomly gets information about science quality. The variable GoodBizFirm_f indicates whether firm f is rated positively or not in terms of business quality, while GoodScienceFirm_f indicates whether a firm is rated positively in science.

The controls $\boldsymbol{X}_{nf}$ include firm fixed effects to control for underlying firm quality, as well as any strata dummies for RCTs when we do a stratified randomization. We cluster standard errors by worker, as our treatments are randomized across workers.

Our pre-analysis plan (PAP) also states that worker beliefs and perceptions of firm quality will be secondary outcomes. These are analyzed in the same way as our results on worker application rankings. Finally, the PAP specifies that we will run heterogeneity analyses based on whether workers have a STEM degree or not.[17]

Randomization check. Table 2 shows that the four treatment groups of the Primary RCT (Panel A) and Secondary RCT (Panel B) are balanced on observables.

In Panel A, characteristics are only from workers who apply to at least one firm, for which we can observe their resume. A strong majority of applicants are currently employed, reflecting that this is a high-skill group. Workers have 10 years of experience on average and nearly half have an undergraduate STEM degree. The number of workers who apply differs slightly by treatment group. This reflects that we sent emails to equal numbers of business school alumni across the treatment groups, but the actual number who apply can still vary. There are no statistically significant differences across treatment arms in gender,

[17]The PAP also says we will try to analyze heterogeneity based on firm characteristics related to technological sophistication. In practice, most firms are highly sophisticated technologically. It is not obvious how to compare the sophistication of, e.g., an AI fintech company to a quantum drug discovery platform.

applicant location, graduation year, startup work history, STEM background, or years of work experience. The Secondary RCT's randomization was not stratified, but participants are balanced on race and gender, as seen in Panel B.

Worker selection into the RCT. Participation in the RCT is defined as submitting an application to at least one firm. Based on rich tracking data, we obtain aggregate numbers about the number of alumni who complete various intermediate steps before participating in the RCT, such as opening the email or clicking through to the job board. However, reflecting university privacy rules, we lack personal IDs from the tracking data, so we cannot analyze participation conditional on intermediate steps.

From our initial sample of 19,359 alumni emailed, 37% (or 7,083) open the invitation email, reflecting possible spam filters or that alumni may not regularly check old emails on file with the advancement office.[18] Of those who open the email, 8% (or 587) of workers click on the job board link. The 8% rate reflects that many alumni are highly established in the business world, and are not actively seeking employment or are not interested in working at a startup. Finally, 43% (or 250) of alumni apply to at least one firm conditional on viewing the job board, suggesting that our job board is well-received and intuitive to workers. Thus, among the 587 people who click on the job board, the participation rate is 43%. Starting from the initial 19,359 alumni, the overall participation rate is 1.3%.

Column 1 of Table 3 shows a linear probability model where overall participation in the primary RCT is regressed on covariates we can observe from our partner business school alumni lists. It is run on the overall sample of 19,359 alumni. For classes graduating in 1980 or before, the participation rate is 0.46%. Participation increases with year of graduation, and is highest for the class of 2019, where the rate is 4.3%. Men are 0.9pp more likely (i.e., twice as likely) to participate than women. People in the city where SEP is headquartered are more likely to participate. These patterns are intuitive, e.g., recent alum are less likely to have well-established careers than older alum. The treatment subjects are assigned does not predict participation, which is unsurprising given that subjects receive identical emails and experiences across treatments until subjects access the website.

Still, could the treatments affect whether subjects apply to at least one firm conditional on arriving at the job board website? A challenge is we don't observe which alumni click on the job board link. Nevertheless, we address the possibility of selection into applying to at least one firm using a statistical analysis in Appendix B.3. Assuming that the treatment doesn't affect participation prior to arrival at the website (and thus that the number of

[18]The average email address was seven years old. At one business school, 46% of emails had university domain names, which seem less likely to be regularly checked than personal emails. This share with university domains is unknown for the other business school. Alumni email lists often have outdated emails. See Appendix E.1 for details.

candidates in each treatment arm is similar upon arrival to the RCT website), we observe no evidence that our treatments affect whether workers apply to at least one firm. Intuitively, the number of RCT participants is broadly similar across treatment arms, and this would be unlikely to occur if the treatments substantially affected the probability of applying to at least one firm.

A different possibility is that the treatments would affect the number of applications conditional on participating. Table 3 also examines this. As seen in column 2, none of the three treatments has a statistically significant effect on the number of applications among people who apply to at least one firm.

Another concern is that the treatment effect on RCT participants would differ from that on a non-experimental job board. Do participation patterns indicate something unusual about the target sample or the RCT job board? MBA alumni are a natural population for analyzing hiring at science-based startups, as such startups seek high-skill workers. Most selection occurs based on opening the email and clicking on the link for the job board. Conditional on clicking the link in the email for the job board, participation is high. In email comments we received and anecdotal discussions with potential participants, the consensus was that the job board—including the ten application cap and the RSD mechanism—was very natural. While most job boards don't have a ten application cap, alumni were given a clear reason for the cap (as mentioned above, to avoid overburdening founders with lots of resumes). Applicants found this reason to be sensible.

Firm selection into the RCT. As noted above, 14% of contacted firms agreed to participate. This rate reflects that many startups are not looking to hire at any one time. Startups did not select into the RCT unless they were actively considering job candidates. Appendix Table A1 compares means of firm characteristics of RCT firms and non-participating firms (columns 1-5), whereas column 6 shows a linear probability model of participation in the RCT on firm characteristics. Observable characteristics are generally weak predictors of whether a startup participates in the RCT, suggesting that participating startups are broadly representative of SEP firms.[19]

[19] While not available for all non-participating firms, average science quality is similar between participating and non-participating firms. Business quality is unavailable for non-participating firms.

4 Results

4.1 Impacts on Job applications

Table 4 shows that expert ratings create large shifts in applications toward better-quality firms. For brevity, we focus on the results in column 3 of Table 4, the specification that simultaneously analyzes both business and science rating treatments. We also provide a visual summary of our results in Figure A2.

Starting in Panel A of Table 4, informing applicants that a firm has below-average science decreases the chance that a candidate applies by 7pp or 24% on a base application probability of 28.6%. For firms with above-average science, showing this information increases the chance a given worker applies by 4pp, though this increase is only marginally statistically significant ($p = 0.08$). Candidates are 10pp more likely to apply to a firm that received a positive science rating compared to a negative science rating. Showing that a firm has an above-average business model led to an 8.3pp, or 29%, increase in the probability a worker applies, and showing negative business-model information lowers application likelihood by 3.4pp, or 12%.

In Panel B, negative science information decreases the chance an applicant considers a startup their top choice by 1.2pp (or 32% on a base rate of 3.8%), whereas receiving positive information increases it by roughly an equal amount. We see roughly symmetric results with respect to business model information. Likewise, in Panel C, getting negative science information lowers the chance a startup is ranked in the top 3 by a given applicant by 4.2pp, or 38%, with nearly symmetric effects from viewing positive information. Again, business model information also has roughly symmetric effects in the positive and negative directions, though to a somewhat smaller degree. In Panel D, receiving negative science information decreases the normalized rank of a given firm by 0.17σ, while positive science information increases normalized rank by 0.12σ. Negative business information decreases normalized rank by 0.11σ, but positive business information increases normalized rank by 0.16σ.

In sum, applications respond strongly to expert ratings, both business and science ones. Effects tend to be roughly symmetric for negative and positive information.

While Table 4 analyzes the decision to apply to particular firms, exploiting randomized within-firm variation, it is also illustrative to look at the distribution of firms applied to across treatment arms. This is done in Table 5. As seen in column 1, when no expert ratings are shown (i.e., in the control group), firms evaluated to have both above-average science and an above-average business model receive 11% more applications on average than firms graded below-average on each metric (i.e., 21.5% in row 4 vs. 19.3% in row 1). However, as seen in column 4, when workers observe both science and business ratings, startups rated above-

average on both metrics receive 80% more applications than those graded below-average.

Multiple hypothesis testing. To account for multiple hypothesis testing related to our four outcomes, Table A8 shows family-wise error rate adjusted p-values based on Westfall & Young (1993). These findings support our main results in Table 4, and show that our conclusions are not driven by multiple testing.

Magnitudes. How large is the magnitude of our effects on job applications? Belot *et al.* (2018, 2022b) experimentally vary wages of posted jobs in a UK job board and find an elasticity of application-like behavior to posted wage of approximately 0.7. An RCT modifying offered wages in the Mexican Civil Service found an arc-elasticity of approximately 0.8: a 33% increase in offered wages led to 26% more applications (Dal Bo *et al.* (2013)). At those elasticities, our estimated treatment effects from communicating even coarse information that a startup has above-median business model or science quality are able to generate as many additional applications as a 15 to 44% increase in offered wage.

Complements or substitutes? The main analyses in Table 4 are pre-registered and focus on the average effect of providing science and business ratings. One also wonders whether effects are complementary or not. In Table A9, following Muralidharan *et al.* (2023), we test for complementarity by including an interaction of dummies for receiving business and science ratings, plus this interaction multiplied by whether a firm is good. For clarity, we focus solely on firms that are good in both science and business model, or that are bad in both. Across all four outcomes, we see consistent evidence that they are substitutes. This may occur if workers update favorably about business quality based on positive science ratings, and visa versa, as shown below in Table 6 and Section 4.3. Thus, providing both sets of ratings provides less than double the average impact of providing one set of ratings.

Unincentivized job interest. Panel A of Table A3 shows the impact of expert ratings on unincentivized job interest, pooling the primary and secondary RCTs. Results are in the expected direction, but weaker than for actual job applications.

4.2 Impacts on Interviews and Hiring

As seen in our RCT registration, we designed our study to be well-powered for examining the impact of our treatments on job applications as they are central for understanding jobseeker preferences. We fully understood that with only 26 participating startups, we would be under-powered to examine later outcomes like interviews and hiring as outcomes.[20]

[20]Early-stage startups also tend to hire following financing rounds, and in our follow-up interviews, a number of firms in the primary RCT mentioned that they were still planning to hire once their next tranche of financing was secured. Recall, however, that our primary job board RCT concluded just several months

Nonetheless, we conducted a follow-up interview where 19 of 26 startups responded concerning their post-participation interviews and hiring. Of these 19 firms, 4 had interviewed 13 applicants in our sample, and extended 1 formal offer. Five firms had made offers to an early employee, generally either through the founders' network or a technical hire outside the scope of our experiment. These numbers imply call-back rates that are fairly low, but are not atypical at all for high-skill firms like the ones we study.[21]

4.3 Beliefs

Beliefs descriptives. Before showing impacts of information on beliefs, we first summarize these incentivized beliefs, particularly as they relate to the probability of firm success. Figure 2 summarizes beliefs both on the probability of raising money at a $1m valuation within a year and on the probability of having a successful exit, defined as IPO or acquisition valued at over $50m within a year. There is a lot of heterogeneity in beliefs. There is also bunching at round numbers, consistent with most research using subjective belief data.

Our most striking finding is that respondents dramatically overestimate the probability of a successful exit within one year. While the true probability is essentially zero (i.e., less than 1% of SEP firms have ever had a successful acquisition within a year, and none have had an IPO, figures consistent with seed-stage high-tech startups more broadly), the median answer is 25%. For raising money within a year, the median answer was 52%, compared to the true probability in the data is 23%.

We are not aware of any direct prior evidence that workers substantially overestimate the chance of startup success. These results are particularly noteworthy because (1) the beliefs are strongly incentivized (i.e., people are "putting their money where their mouth is") and (2) the sample includes many experienced workers for whom overestimation is perhaps more surprising than in less sophisticated workers.

Appendix Figure A3 shows beliefs by worker and firm characteristics. Figure A3a shows that female applicants are particularly likely to overestimate the probability of a raise or IPO, and that the overestimation occurs even among high-quality workers.[22] Panel A of Appendix Table A7 shows the OLS estimates of these results with additional worker characteristics. Interestingly, former startup employees are significantly less optimistic than others about the chance of startup success. A likely explanation is that prior experience calibrates the expectations of startup exit.[23]

before the beginning of the COVID-19 pandemic.

[21] For example, as of 2019, Google accepted about 0.2% of candidates. https://cnb.cx/3H29TbB.

[22] While research often finds that men are more overconfident than women, there are also many situations where men and women appear equally overconfident (e.g., Huffman *et al.*, 2022).

[23] We also highlight that we did not ask workers for their beliefs about the firms they applied to, but rather

Overconfidence about positive startup outcomes occurs widely across many types of workers and appears highly robust. One concern is that results could be driven solely by the beliefs of unsophisticated applicants. However, as noted above, overoptimism is highly prevalent among workers rated as high-quality and among workers who have STEM degrees. A second concern is that since the true probability of an IPO or acquisition within a year for a seed-stage venture is essentially zero, any elicitation or rounding error would lead to overestimation. However, the finding of overprediction is highly robust to excluding beliefs in multiples of 5 above zero, indicating that overprediction is not driven by rounding errors. Finally, beliefs about firm success are correlated with application decisions (Appendix Table A14), suggesting that beliefs questions are answered seriously.

Impacts of expert ratings on beliefs. Table 6 shows that expert ratings also substantially affect worker beliefs, particularly about perceptions of the business and science quality of firms, but also about firms' perceived chances of raising money and (more tentatively) having a successful exit.

Panel A shows results for normalized perceived science quality. Receiving negative science information lowers perceived science quality by roughly 0.3σ, whereas positive information increases it by roughly 0.15σ. Interestingly, business model ratings also cause individuals to change their beliefs about science quality, but not to the same degree; this is consistent with a prior that assumes correlation between the quality of different aspects of a startup. Panel B shows results for normalized perceived business quality. We see the same qualitative pattern as in Panel A, with relatively strong effects of business information on perceived business quality, and weaker (and here insignificant) effects on perceived science quality but in the expected direction.

Panel C shows that the treatments had substantial effects on workers' beliefs that firms will raise venture capital. Negative science information decreases the perceived chance of a firm raising money by 4pp, very similar to the impact of negative business information. We also see statistically significant effects of positive business information. Panel D shows that positive business information significantly increases the perceived chance of firms having a major exit. The other coefficients tend to be noisy, likely reflecting the strong heterogeneity in beliefs across workers.

Figure A2 shows these results graphically, plotting impacts of business and science information on perceived firm quality and chances of positive longer-term outcomes.

Results presented separately by RCT. Appendix Tables A3, A4, and A5 present results on unincentivized job interest and beliefs separately by the primary and secondary

about a fixed, pre-chosen subset of firms on the job board.

RCTs. As expected, results on each sample are noisier relative to the full samples. In general, results on beliefs and unincentivized job interest are weaker in the primary RCT relative to the secondary RCT. We believe this reflects (1) that subjects in the primary RCT were asked about 3 randomly selected firms, including firms they were not interested in, whereas in the seconary RCT, subjects only considered 3 firms in total, and (2) that in the primary RCT, the questions on beliefs and unincentivized job interest were asked after subjects had completed their job applications.[24]

4.4 Treatment Effect Heterogeneity

This section considers heterogeneity analyses on the extent to which applications respond to expert ratings. We examine heterogeneity according to worker and firm characteristics. We find limited heterogeneity with respect to most characteristics. However, men respond more to science expert ratings than women, and this finding is robust to multiple hypothesis testing correction. Importantly, we find no evidence that low-quality workers—as evaluated by an HR expert focused on startup hiring—are driving our main results.

We address heterogeneity using two methods. First, we examine simple interaction effects in OLS regressions. To maximize statistical power, instead of looking separately at heterogeneity between good and bad ratings, we examine heterogeneity according to overall worker responsiveness to expert ratings. We define the variable BizInfoShock_n (SciInfoShock_n), which is -1 if negative business (science) expert rating is shown, 1 if positive business (science) expert rating is shown, and zero if no information is shown. For each worker characteristic C_n, we estimate a model:

$$\begin{aligned} y_{nf} = {} & \alpha_0 + \alpha_1 \text{BizInfoShock}_n + \alpha_2 \text{SciInfoShock}_n + \alpha_3 \text{C}_n \\ & + \alpha_4 (\text{BizInfoShock}_n \times \text{C}_n) + \alpha_5 (\text{SciInfoShock}_n \times \text{C}_n) + \boldsymbol{X}_{nf}\boldsymbol{\alpha} + \varepsilon_{nf}. \end{aligned}$$

For firms, the estimation equation is the same except C_n are firm characteristics.

Second, we apply the sorted effects method of Chernozhukov *et al.* (2018), which uses machine learning to characterize the observations most and least affected by our treatments, and addresses issues of multiple hypothesis testing. Both methods yield the same conclusions.

Measuring worker quality. Worker quality is a key variable in models of sorting (Eeckhout & Kircher, 2011; Bandiera *et al.*, 2015), but is not easily observed. To measure it, we contracted with an independent human resources consultant who specializes in startup hiring, having over 15 years of experience, and asked her to rate workers in terms of their

[24] In the primary RCT, subjects' beliefs and unincentivized job interest also seem more affected by business rating relative to science ratings.

quality on a scale from 1-10. Specifically, she was asked to evaluate worker resumes in terms of their suitability for a management job at a high-tech startup.

Heterogeneity by worker characteristics. Panel (a) of Figure 3 examines heterogeneity by worker quality. There is no evidence that lower quality workers respond more to expert ratings; in fact, across all 8 specifications, higher quality workers show greater responsiveness. However, the difference is not statistically significant. Our finding is important because if the marginal worker who switches their application to better firms were low-quality, then providing expert ratings could cause adverse selection, and hence misallocation of human capital. Appendix Table A10 shows the same estimates in tabular form.

Our estimates are precise enough to usually rule out that higher quality workers respond less than lower quality workers by even relatively small margins in response to business ratings. For the key outcome of applying, with 95% confidence, we rule out that the treatment effect for higher quality workers is more than 18% below that for lower quality workers.[25] Likewise, for the outcomes of top 3 choice and normalized rank, we rule out treatment effect differences favoring lower quality workers of more than 15% and 14%, respectively.

Panels (b) and (d) of Figure 3 show no difference in responsiveness by whether workers have a STEM degree or by current employment status. Regarding having a STEM degree, on one hand, one might imagine that workers with STEM degrees would have better knowledge about which startups have good science, so that expert ratings are less needed. On the other hand, it is possible that STEM workers intrinsically value good science more than non-STEM workers as an amenity (Stern, 2004), so the two effects could offset each other.

Panel (c) of Figure 3 shows that men respond more to science ratings than women, which is robust to adjusting p-values for testing multiple dimensions of worker heterogeneity, as seen in Table A11. There are various explanations for this consistent with prior work on job search, including that women may be more sensitive to commuting and relocation costs (Le Barbanchon *et al.*, 2021). For example, a woman might be interested in applying to a highly-rated firm, but be constrained to do so because the job is far away, whereas a man may be less constrained in relocating (Benson, 2014).[26]

[25] Lower quality workers have a treatment effect coefficient of 0.05. We rule out that higher quality workers have a treatment effect coefficient of less than 0.04, based on the confidence interval on the interaction term.

[26] More broadly, it could be that women value non-pecuniary aspects of jobs more than men, and that expert ratings primarily provide information about the pecuniary return to working at a firm instead of the non-pecuniary return. Separately, one may wonder how our results can be squared with the finding that women are more risk-averse than men (Bertrand, 2011). All the jobs at the firm in our sample are high-risk (e.g., in terms of possibly losing one's job) even conditional on receiving positive expert ratings, so one would not necessarily expect women to respond more to ratings even if they are more risk-averse. Finally, that men respond more to information is not driven by having more statistical power for men (e.g., there being more male than female subjects), as this would affect our ability to detect significant interaction terms. As seen in panel (c) of Figure 3, all eight point estimates of effects for men are larger than those for women.

As seen in Appendix Table A12, machine-learning based sorted effects models yield the same qualitative conclusions as those in Figure 3.

Firm characteristics. Figure A4 shows limited evidence of heterogeneity with respect to firm characteristics. There is some weak evidence for a broad picture where workers rely more on signals where there is less information from the firm on that dimension. Using machine learning sorted effects, Table A12 Panel B shows similar findings to Figure A4.

Panel (a) of Figure A4 examines heterogeneity based on whether founders have prior business development experience, such as serving as an executive at another firm. One might worry less about business model quality for a founder with such experience compared to, say, a computer science professor who has never worked in the private sector (Shaw & Sørensen, 2019). Indeed, for two outcomes, we see that workers respond more to expert ratings on business quality when founders do not have prior business development experience.

Panel (b) examines heterogeneity by whether the founder has a PhD, which likely serves as a signal of science quality (and not business quality). There is weak evidence that workers respond more to science quality when the founders do not have a PhD, consistent with expert ratings serving as a substitute signal.

Panels (c) and (d) examine heterogeneity based on whether a firm has positive revenue (many science-based startups take time to make revenue) or has external financing. We do not find consistent evidence that effects depend on these factors.

5 Discussion

5.1 Threats to Validity

Hawthorne Effects. A common concern in RCTs is whether results are driven by experimental demand effects, also called Hawthorne Effects. Hawthorne Effects are believed to be most likely when there is an experimental manipulation that subjects are aware of, and where subjects wish to please or influence the researcher (Levitt & List, 2011). However, participants in our study did not know that there was experimental manipulation of what they observe. As noted earlier, workers did not know they were in an RCT, and we received no indication that expert ratings seemed artificial.[27] Although workers were told their data may be used for research, it is unlikely that solely knowing this would drive our results: job

[27] Likewise, firms were only told that the SEP was testing aspects of the job board and hence that there could be experiments performed on its structure both for research and internal-to-SEP purposes. Moreover, the RCT treats workers and there are no firm decisions to analyze, so Hawthorne Effects with respect to firms are not an important concern.

applications are a relatively high-stakes decision, and hence workers would need to prioritize pleasing researchers in some way over choosing which jobs they were most interested in.

Salience effects. A separate concern is whether simply making any information salient could drive our results (Li & Camerer, 2022). A key feature of our RCT design is that our treatment has equal salience for every firm. Each applicant sees each job marked in a similar fashion (e.g., "Business Model Was Rated Above Average: Yes" and "Business Model Was Rated Above Average: No"), so it is not the case that some jobs receive greater visual cues than others. In addition, workers behave in ways that suggest response to the particular type of information we provide and not simply salience effects. For example, providing business ratings affects perceived business quality, but has limited effect on perceived science quality.

A related but different salience concern is whether the treatment could serve as a cheap marker for low-effort jobseekers to make decisions. Perhaps jobseekers are searching for startup jobs across multiple platforms, and the expert ratings provide a quick way of making decisions on the SEP job platform for low-effort jobseekers. We believe that this is unlikely to drive our results given that jobseekers spent substantial time completing the RCTs, both primary (where people spent a median of 22 minutes after clicking on the part of the job board website where they enter information) and secondary RCTs. All our main results are robust to excluding jobseekers who took less than 10 minutes after clicking on the data entry portion during the primary RCT.

Lack of comprehension for the RSD and quadratic scoring rule. Our RCT uses random serial dictatorship (RSD) to allocate applications, and uses a quadratic scoring rule to elicit beliefs. What if workers don't understand these mechanisms or are inattentive? As discussed in footnote 9 and Appendix B.2, past research indicates that these mechanisms are generally effective and reliable. Following past research, we explained the mechanisms in a simple and intuitive manner, emphasizing that people would do best for themselves if they reported their true preferences and beliefs. To the extent that the RSD created measurement error in job application rankings, this would increase the size of our standard errors. This is not a concern for our overall effects, where we find clear statistical significance, though we acknowledge that this may make it harder to detect heterogeneity. Measurement error in the quadratic scoring rule would not affect our main results; it would also not bias our results on beliefs in Table 6, and will instead contribute to larger standard errors.

External validity. Our study focuses on high-skilled workers applying to science-based startups. Science-based startups are a growing sector, with AI startups getting almost half of US startup funding in 2024 (N.Y. Times, 2024), and high-skilled workers are natural to study for this. Thus, we believe that our sample is well-suited for our research question,

policy-relevant, and highly independently interesting, and that science-based startups are substantially understudied by labor economists. That said, it is not clear whether our results on substantial information frictions would hold for more established firms or for young firms in "conventional" industries (e.g., restaurants, construction), and this could be explored in future research. Within our sample, effects on job applications are generally similar across various dimensions of worker background and experience (Figure 3), as well as across firm characteristics (Figure A4), suggesting that our effects may also hold among broader populations of workers and firms. While we acknowledge that our results are specific to high-skill workers, we speculate our results may provide a lower bound for a broader population of workers if lower-skill workers are less sophisticated in their ability to evaluate firms.

5.2 Are the Results Obvious? Economist Expert Predictions

In order to evaluate the predictability of our findings, we asked economist experts to predict our findings, following DellaVigna & Pope (2018). In April 2023, we sent the survey to 270 economists randomly selected from those who attended the 2022 NBER Summer Institute in Personnel Economics, Entrepreneurship, or Labor Studies, from which we received 86 responses, i.e., a response rate of 32%. The first question asks whether the respondent was already familiar with the main findings of our study, to which 7 respondents said Yes who were immediately screened out and not asked further questions, leaving 79 responses. Of this set, 30% are full professors, 21% are associate, and 26% are assistant. The remainder are PhD students/postdocs (15%) and industry economists (8%). We also distributed the survey on the Social Science Prediction Platform, where it got 14 additional responses, of which 4 were screened out, leaving us with 89 total responses. Of the 89 respondents, 16 skip almost all questions (e.g., answer demographics only), so there are 70-73 responses for most questions. Among the 73 focal respondents, the median response time is 503 seconds (p25 =328s, p75= 872s), suggesting that respondents took the survey seriously.

The survey focused exclusively on our primary RCT. After the screener question, the survey described our setting and the expert rating treatments, including showing a screenshot of what job seekers saw. Appendix G provides exact survey questions and other details.

Table 7 summarizes the results of the economist survey. Column 2 of Table 7 lists actual RCT results, whereas Column 3 summarizes economist predictions.

Underprediction. As seen in rows 1-4 of Table 7, economists severely underestimate the quantitative magnitude of our effects. In the absence of expert ratings, economists are relatively accurate in forecasting proportional number of applications received by good firms (i.e., firms rated above-average in terms of both science and business) compared to bad firms

(rated below-average in terms of both science and business), as seen in row 1. However, as seen in row 4, when both ratings are shown, good firms get 80% more applications than bad firms, while the median expert prediction is 25% and the mean is only 36%. 86% of economists underpredict the magnitude of the treatment effect, and 60% of economists underpredict the treatment effect by at least half. Substantial underprediction is also observed when economists are asked about providing only science ratings or business ratings.

Other aspects of effects. Economists also fail to predict many aspects of the effects. Our RCT finds comparable effect sizes between science and business ratings, as well as between positive and negative information.[28] However, for each finding, fewer than 15% of economists predict correctly, while most economists (59% and 72%, respectively) incorrectly believe that business rating and negative information would yield a larger impact.

Recall that our RCT does not find heterogeneity in effects based on having a STEM degree or the level of worker quality (as measured by an HR expert). However, for each of these findings, less than 30% of economists predict correctly. The most popular economist response is to predict stronger responses for STEM degree and high-quality workers.
While we find larger effects for men than women, only 14% of economists predict this.

Perhaps many economists fail to predict various aspects of our effects because some of our heterogeneity estimates are somewhat imprecise? This seems very unlikely to be a complete explanation. As also observed in Englmaier *et al.* (2024), expert beliefs are quite dispersed. Consider heterogeneity by whether jobseekers have a STEM degree. Expert predictions are roughly evenly split over three options (smaller for workers with a STEM degree, no difference, larger for workers with a STEM degree). If roughly 1/3 of experts each predict one of three options, then 2/3 of experts will be incorrect in their predictions.

5.3 Firm Provision of Quality Signals

In self-written ads, 19 of 26 SEP firms describe technical details, 23 describe their commercial product, and 8 mention their current or planned business model. However, only 4 of 26 give *any* credible quality signal.[29] One explanation, which seems possible to us, is founders

[28] Rows 2-3 of Table 7 show that science and business ratings increase the ratio of applications to good firms to applications to bad firms by 32% and 73%, respectively. How can this be squared with the fact that row 5 of Table 7 shows that science or business ratings had similar effects (based on Table 4)? This reflects that the row 5-7 findings are based on overall effects using all the data, whereas rows 1-4 focus solely on firms that have both good science and good business or that have bad science and bad business.

[29] We include any mention of founder education, whether the firm is a spinout, whether the firm participated in incubators, possession of IP, named buyers/partners, existing sales abroad, a named investor, prominent advisor or government grant, a prize or contest victory, named previous experience by founders in a startup or high-level corporate position, any award or prize given to the founders for related work, any media mention, unnamed investors with previous exits, or specific existing sales traction. One firm mentioned their product

do not realize that without credible signals, their firm is hard for applicants to evaluate. Alternatively, even top startups may not have outside credible signals to cite. This isn't the case for the 26 firms in our primary RCT: we verified in SEP internal documents that all 26 had outside signals as defined above which could've been shown in a job ad.

A third explanation is our sample is unusual. To investigate this more broadly, we examine the content of ads on AngelList's hiring board. We scrape the universe of job ads for full-time positions from companies with 1-10 employees who posted a job over two weeks (n=1017).[30] From an ad's full text, including company descriptions, we hand code whether it describes the company's product, business model, technical details about the product, and credible outside signals of quality, using the same coding as we used for SEP job ads (for details, see Appendix E.4). Appendix Table A13 summarizes this AngelList data. Only 23% of ads contain even one such signal, though 93% describe the product being sold and 25% even give a technical description of the company's product.

Ultimately, our RCT cannot answer definitively why many startups do not seem to provide quality signals, and there are many other alternative explanations. These include (1) the possibility that the value of high-quality applicants is low for some firms; (2) that using quality signals could yield lower-quality candidates; and (3) that signals aren't credible when firms provide them; and (4) that it is costly for startups to construct and communicate these signals. Further work is needed to understand the firm choice problem. Our RCT is designed to understand the behavior of job applicants.

5.4 Translating Treatment Effects on Quality of Firm Applied To into Effects on Worker Earnings

We estimate here how much a worker's expected lifetime earnings would change if they work for the average *post-treatment* firm they apply to instead of the average *pre-treatment* firm. We estimate that the expected benefit is \$800-\$2,800 USD per applicant. Broader considerations of welfare are considered in Section 6.

Intuitively, more precise information about firm quality leads workers to apply to better firms, better firms are more likely to have a liquidity event, and liquidity events lead to payoffs for early hires via their equity. In math, the benefit to a treated worker is $\mathbb{E}[\Delta_q] \times \frac{\partial R}{\partial q} \times \mathbb{E}[LS]$. Here, $\mathbb{E}[\Delta_q]$ is the expected change in the expert-estimated quality of firms applications are made to, $\frac{\partial R}{\partial q}$ the marginal increase in the present value of firm revenue R as quality increases,

is based on research published in a top scientific journal. A second discussed a unique FAA certification. A third discussed their link to an academic lab and their partnership with two major international firms. A fourth discussed the educational background of the founder.

[30] These are all ads posted between 10/30/2020 and 11/13/2020.

and $\mathbb{E}[LS]$ the expected labor share of revenue accruing to an early hire.

Both science and business rating treatments shift roughly 9.5% of applications from below- to above-median firms.[31] Assuming below-median firms have in expectation 25th-percentile expert-evaluated quality, and above-median ones 75th-percentile quality, each treatment shifts expert-evaluated quality of the average application by $1.35 \times .095 = .128\sigma$. A $.128\sigma$ increase in science quality, as shown in Table 1, predicts a 1.2pp increase in the chance a firm raises a venture round after SEP. A similar calculation using just the (noisier) outcomes of firms in the RCT as discussed in Section 2.1 suggests that a $.128\sigma$ increase in science or business quality raises the chance the firm raises money or hires 10+ employees within 3 years of the RCT by .8pp to 2.8pp. Therefore, for each of our information types, $\mathbb{E}[\Delta_q] \times \frac{\partial R}{\partial q}$ is between .8 and 2.8.

As for $\mathbb{E}[LS]$, the increase in pay from working at a better firm, Kerr *et al.* (2013) estimate being funded increases the chance of a positive successful exit by roughly 10pp. Thus, each of our treatments leads the average application to be made to a firm with a .08pp to .28pp higher chance of a successful exit. There is not good data on the payoff to early workers of a successful exit, but conditional on being venture-backed, one rough calculation suggests the equity share of an early employee has an expected value of just under \$1m.[32] Thus, each treatment has an expected value per applicant of \$800-\$2,800 USD, solely in terms of working at a firm that is more likely to have an IPO or be acquired.[33]

6 Conclusion

Science-based startups like OpenAI (the creator of ChatGPT) garner enormous interest from the public, investors, and jobseekers, but jobseekers may struggle in identifying which of these firms are best to apply to. We find that workers make substantially different job application decisions when randomly given coarse expert opinions on the quality of science-based startups, shifting applications to better firms. Workers react to both positive and negative expert ratings, and to both information on science and business model quality. Changes in worker beliefs about startup success appear to be one mechanism for these

[31] Science ratings raise the chance of an above-median application by 12% and reduce the chance of below-median by 24%. The share of above-median applications post-treatment is thus $\frac{1.12}{1.12+.76} = .596$. Likewise, the post-treatment share of above-median applications for business quality information is .594.

[32] See https://80000hours.org/2015/10/startup-salaries-and-equity-compensation.

[33] This calculation assesses the value to workers of working at the average post-treatment firm applied to compared to the average pre-treatment firm. This is a clear, policy-relevant way of assessing how treatments affect choice quality, but differs from the treatments' average earnings benefit (e.g., since treatments shift applications to better firms, workers may also face more competition). Besides more pay, working at a better firm may also provide benefits in terms of skill development, career progression, or job stability. Accounting for such benefits would increase the value of the treatments.

results, though these beliefs also reveal considerable overestimation about the likelihood of exit events. While it may seem surprising that startups don't provide more quality signals on their own, economists significantly underpredict the magnitude of our treatment effects.

Organizational implications and rollout. Turning to implications of the study for SEP itself, the organization seemed quite pleased with the results of the RCT. SEP's top executive described the results on the impact of expert ratings as "compelling" and others voiced similar opinions. Unfortunately, the timing of the RCT was not good for a quick and well-resourced implementation. The main RCT occurred in late 2019, with preliminary results presented to top SEP staff at the end of January 2020. Once Covid hit in early 2020, SEP shifted its focus to re-organizing the program to operate virtually.

In response to our RCT results documenting the difficulty high-quality startups have in hiring early employees, in late 2021, SEP rolled out a non-experimental pilot job board for graduating SEP firms looking to hire business school alumni. Interestingly, expert ratings were not initially used on the job board, presumably reflecting that SEP, which is a non-profit, faced financial, staffing, and attention constraints during the pandemic. The pilot job board without expert ratings operated only for one year,[34] but an SEP executive indicated that they are likely to consider it again in the future, and expert ratings are a possibility for the future. In addition to the non-experimental rollout, as of Spring 2023, SEP started providing coaching regarding the importance of quality signals in startup hiring. Startups are encouraged to provide quality signals in job ads when available, even if it feels like bragging. That aspects of the RCT have scaled in a world-leading entrepreneurship program supports the external validity of the findings.

Policy implications. Our results are relevant for policy discussion. When a venture capitalist or government wants to invest in a startup, they generally conduct deep diligence on the firm, including obtaining expert opinions. Our results indicate that workers would make quite different job application decisions in the presence of similar information. Expert ratings cause higher-ranked firms to get more applications.

Are such changes in application behavior beneficial for social welfare? To the extent that applications to higher-ranked firms have the greatest social benefit, the treatment would seem to improve social welfare. This could be the case if startups are generally labor-constrained; given that most startups fail and assuming the social returns to startup quality is convex, it may be natural for a social planner to focus on helping the highest-potential startups succeed. However, it is not clear that higher-ranked firms have the greatest benefit from more applications. It could be that a social planner would prefer more applications

[34] It is possible that the lack of expert ratings limited the success of the non-experimental pilot job board that was rolled out. Limited staffing due to Covid may also play a role, according to SEP leadership.

to go to "marginal" startups as opposed to startups that are likely to succeed regardless. Further research, both theoretical and empirical, is needed to understand the social welfare consequences of interventions like ours.

On a narrower scope, one might imagine that treatments like ours would be beneficial for worker welfare. A challenge is that workers exhibit highly inaccurate and overoptimistic beliefs about the chance that startups will have a liquidity event. Though it seems useful for workers to redirect applications from lower-ranked to higher-ranked startups, one could worry that some workers would be better-suited not to work for startups at all.

Open question. While we study imperfect information of workers applying to high growth startups, and the extent to which even coarse information about firm quality changes job search, it is an open question how widespread this inefficiency is in the broader labor market. There are many aspects of a job workers imperfectly observe, even at large firms: how quick are promotions, do bosses train new workers, does the firm have a bright future. Workers can of course rely on testimony from friends or commenters on sites like GlassDoor, or infer prospects from salary offers. Nonetheless, just as how a large literature now shows inefficient job matching due to firm uncertainty about worker quality, the reverse (i.e., worker uncertainty about firm quality) also seems important, and is currently not well understood.

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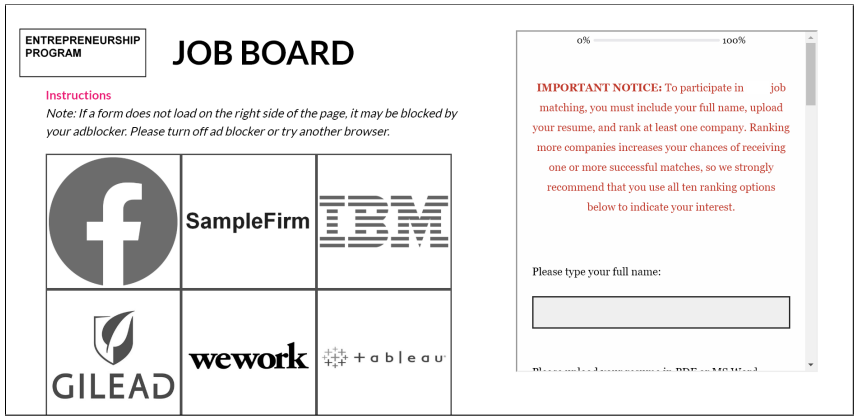
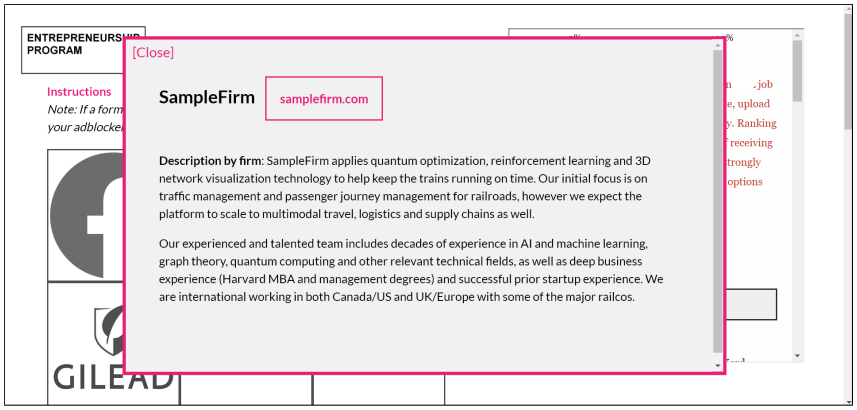
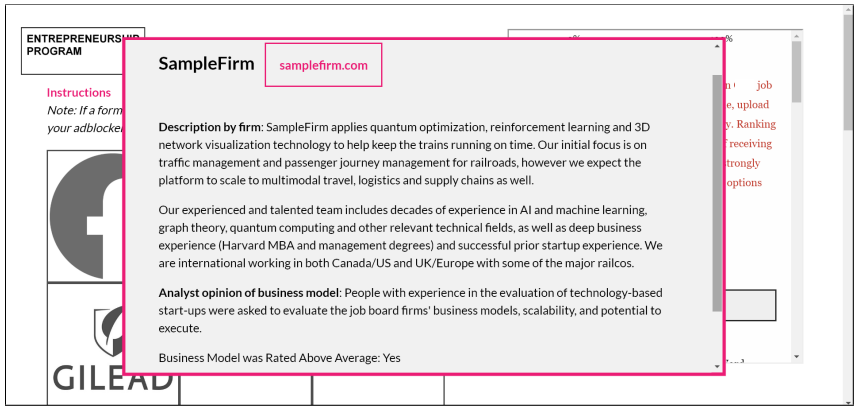
Figure 1: Screenshots from the RCT Job Board



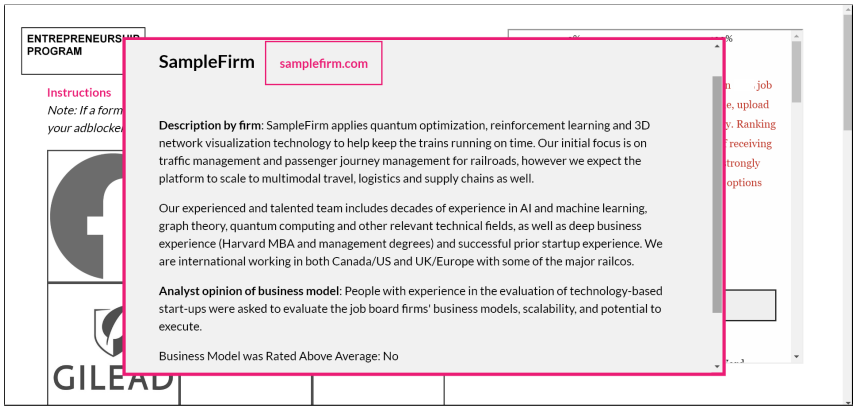
(a) Job board



(b) Control



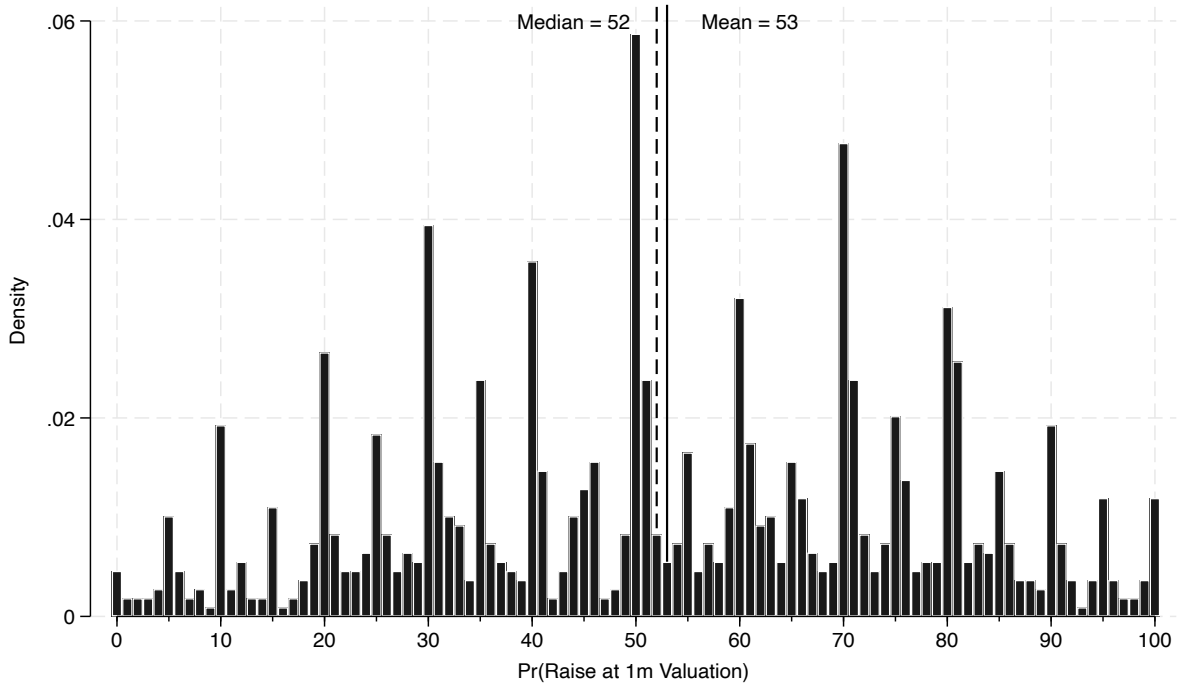
(c) Business rating only, positive



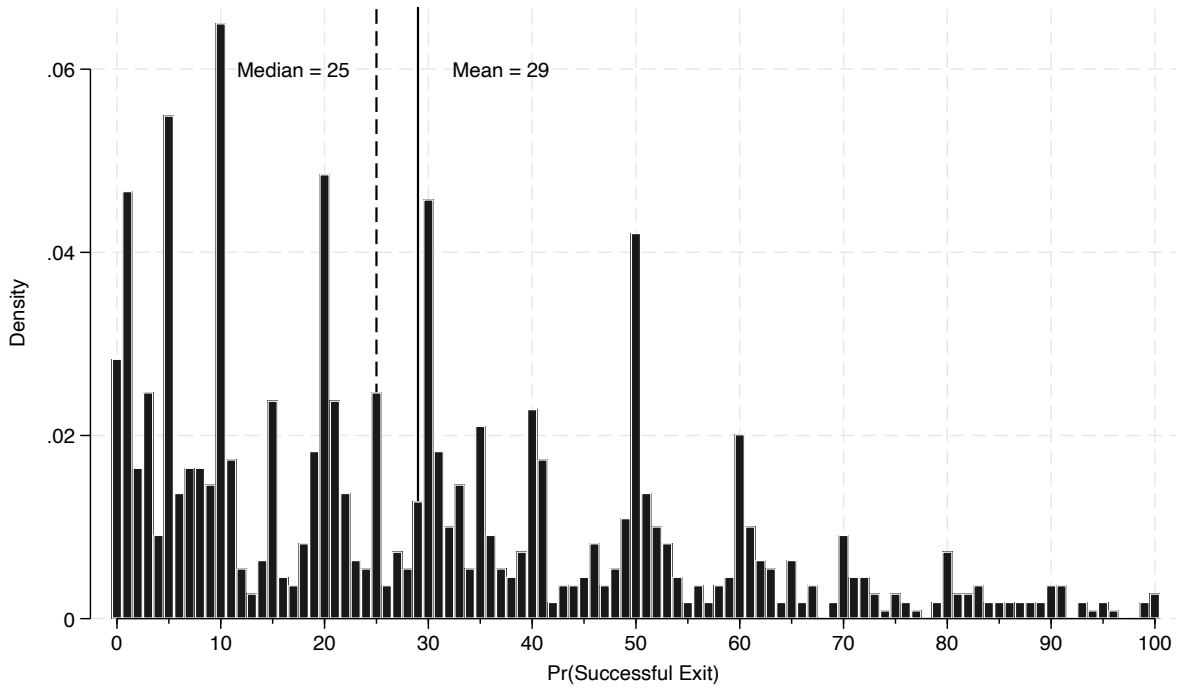
(d) Business rating only, negative

Notes: This figure presents screenshots from the RCT job board. Identifying information about the SEP has been redacted. The description of SampleFirm is based on a mixture of actual firm-written ads. To preserve anonymity of the job board, the logos of actual firms have been replaced with the logos of well-known startups. All of the actual startups in the RCT were between seed stage and Series A.

Figure 2: Histograms of Worker Beliefs



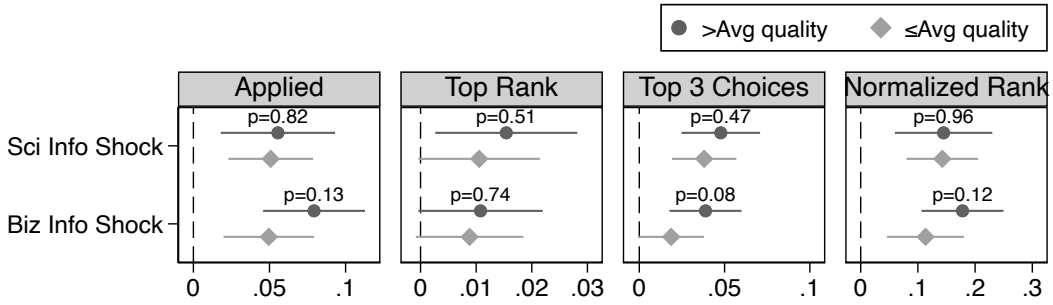
(a) Raise at $1m Valuation in 1 Year



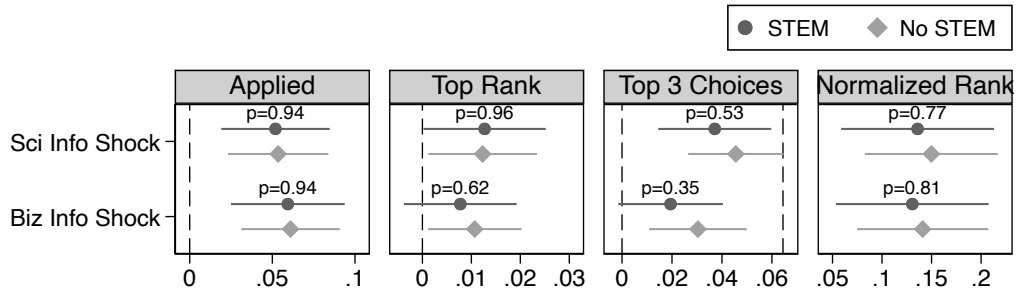
(b) Major Exit (IPO or $50m Acquisition) in 1 Year

Notes: This figure shows histograms of worker beliefs about firm success elicited using a risk-invariant quadratic scoring rule. Data are pooled beliefs from the Primary and Secondary RCTs.

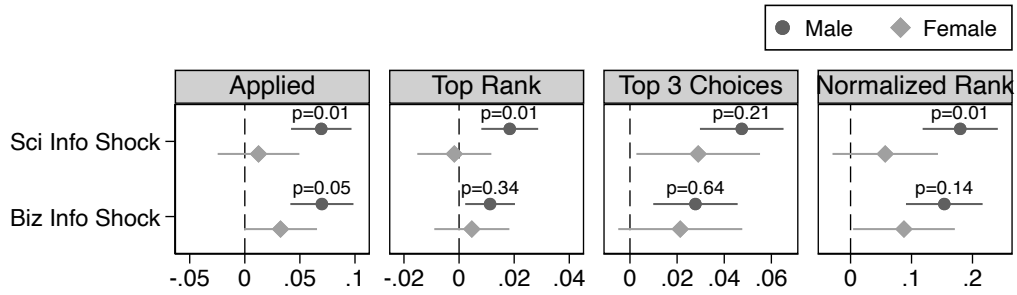
Figure 3: Treatment Effect Heterogeneity by Worker Characteristics



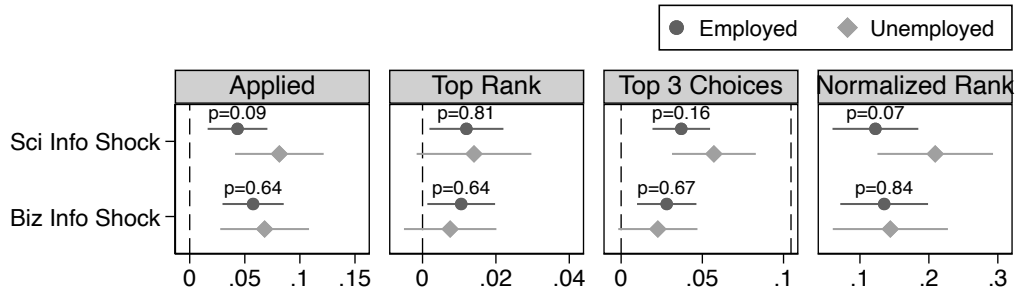
(a) By worker quality



(b) By whether the worker has a STEM undergraduate degree



(c) By gender



(d) By current employment status

Notes: This figure shows heterogeneity in worker response to information shocks in the Primary RCT. Estimates are from regressing application outcomes on science and business treatments and their interactions with worker characteristics. Regressions include venture and strata fixed effects. The lines shown are 95% confidence intervals. The p-values correspond to the null hypothesis that there is no heterogeneity in treatment effects based on different worker characteristics.

Table 1: The Correlation Between Expert Science Ratings and Firm Outcomes

	(1)	(2)
Panel A: Dep. Var. = Graduated from SEP		
Science quality	0.102*	0.092*
	(0.055)	(0.050)
R^2	0.03	0.10
Observations	106	106
Mean of DV		0.41
Panel B: Dep.Var. = Raised After SEP		
Science quality	0.087*	0.094**
	(0.048)	(0.045)
R^2	0.03	0.04
Observations	106	106
Mean of DV		0.25

Notes: This table shows results from regressing start-up success outcomes four years after participating in SEP on expert science scores given at the time the startup applied to SEP. Data are the full cohort of 130 startups in 2017-18, from which 24 startups were dropped due to missing science scores. Robust standard errors in parentheses. Column 1 has no controls while column 2 controls for the number of founders, an indicator for having a PhD founder, and technology fixed effects. Statistical significance is denoted by *(10%), **(5%), or ***(1%).

Table 2: Balance Table

	No Info	Science Info	Business Info	Science & Business Info	p-value
Panel A: Job Board RCT					
Male	0.77	0.72	0.79	0.69	0.51
City is SEP HQ	0.56	0.47	0.52	0.47	0.70
Graduation year	2012	2012	2013	2012	0.69
Startup founder	0.24	0.23	0.12	0.12	0.14
Startup employee	0.27	0.28	0.19	0.28	0.60
Employed	0.80	0.81	0.69	0.70	0.24
Yrs of exp	9.95	10.69	8.64	10.36	0.55
STEM	0.38	0.49	0.39	0.36	0.49
Worker Quality (1-10)	5.20	5.66	4.90	5.00	0.35
Num. Workers	66	53	67	64	
Panel B: MBA Student RCT					
Male	0.74	0.54	0.57	0.67	0.18
White	0.26	0.29	0.24	0.21	0.82
Hisp./Latino	0.09	0.08	0.06	0.06	0.94
Asian	0.30	0.33	0.49	0.35	0.25
Num. Workers	46	48	49	48	

Main notes: This table compares applicant characteristics across treatment groups for the Primary RCT (Panel A) and Secondary RCT (Panel B). "Science info" and "Business Info" in column headers refer to the subjects that received science and business scores. "No info" is the control group. The p-value is from a test of equality of means across the four treatment arms.

Panel A: In the Primary RCT, randomization was stratified on gender, city, and year of graduation at the time potential applicants were contacted. Other variables were not observable prior to application. Worker Quality is based on a startup-focused HR expert's evaluation of resume quality. Of 259 resumes submitted in the job board, 9 were ineligible and were removed from all analysis. Ineligible candidates were forwarded the link to the job board from eligible candidates. The remaining 19,109 individuals contacted did not apply to any firm.

Panel B: In the Secondary RCT, randomization was not stratified.

Table 3: Primary RCT: Predicting Selection into the RCT and Predicting the Number of Applications Submitted

	(1) Submitted App	(2) Number of Apps
Male	1.030***	0.591
	(0.163)	(0.411)
City is SEP HQ	0.758***	−0.209
	(0.183)	(0.375)
Graduation Year, Base Level = 1980		
1985	0.402	−2.879**
	(0.455)	(1.383)
1995	0.392	−3.546***
	(0.409)	(1.014)
2005	1.110***	−2.552***
	(0.404)	(0.566)
2013	1.152***	−2.373***
	(0.352)	(0.422)
2018	0.867**	−1.277**
	(0.360)	(0.494)
2019	4.560***	−0.992*
	(0.771)	(0.530)
Treatment Group, Base Level = No Info		
Business + Science info	−0.037	0.030
	(0.233)	(0.528)
Business info	0.025	0.599
	(0.235)	(0.463)
Science info	−0.268	−0.759
	(0.223)	(0.550)
R^2	0.01	0.10
Observations	19,359	250

Notes: Using the Primary RCT, this table examines overall selection into the RCT (column 1) and the relationship between treatment assignment and number of job applications submitted conditional on participating in the RCT (column 2). Column 1 shows a linear probability model, where RCT participation—defined as applying to at least one firm on the job board—is regressed on subject characteristics. Coefficients are multiplied by 100 for readability. An observation is an alumnus who is emailed. Column 2 shows a linear model, where the number of applications submitted is regressed on subject characteristics. Robust standard errors in parentheses. More details on the selection process are provided in Section 3 of the main text. Statistical significance is denoted by *(10%), **(5%), or ***(1%), and we denote significance this way throughout the tables.

Table 4: The Effect of Expert Ratings on Job Applications

	(1)	(2)	(3)
Panel A: Dep. Var. = Applied			
Science info X Good science	0.102***		0.103***
	(0.025)		(0.025)
Science info	−0.067***		−0.068***
	(0.018)		(0.017)
Business info X Good business		0.116***	0.117***
		(0.026)	(0.026)
Business info		−0.034*	−0.034*
		(0.019)	(0.019)
F(Sci + Sci X GoodSci = 0)	0.076		0.078
F(Bus + Bus X GoodBus = 0)		0.000	0.000
Mean of DV			0.286
Panel B: Dep. Var. = Top Rank			
Science info X Good science	0.025***		0.025***
	(0.009)		(0.009)
Science info	−0.011***		−0.012***
	(0.004)		(0.004)
Business info X Good business		0.019**	0.019**
		(0.009)	(0.009)
Business info		−0.010**	−0.010**
		(0.005)	(0.005)
F(Sci + Sci X GoodSci = 0)	0.009		0.009
F(Bus + Bus X GoodBus = 0)		0.032	0.029
Mean of DV			0.038
Panel C: Dep. Var. = Top 3 Choices			
Science info X Good science	0.083***		0.084***
	(0.016)		(0.016)
Science info	−0.042***		−0.042***
	(0.008)		(0.008)
Business info X Good business		0.051***	0.052***
		(0.017)	(0.017)
Business info		−0.025***	−0.025***
		(0.009)	(0.009)
F(Sci + Sci X GoodSci = 0)	0.000		0.000
F(Bus + Bus X GoodBus = 0)		0.001	0.001
Mean of DV			0.111
Panel D: Dep. Var. = Normalized Rank			
Science info X Good science	0.282***		0.285***
	(0.057)		(0.057)
Science info	−0.163***		−0.166***
	(0.032)		(0.032)
Business info X Good business		0.267***	0.270***
		(0.059)	(0.058)
Business info		−0.106***	−0.106***
		(0.036)	(0.036)
F(Sci + Sci X GoodSci = 0)	0.001		0.001
F(Bus + Bus X GoodBus = 0)		0.000	0.000
Observations	6,500	6,500	6,500

Notes: This table shows the effect of information on employee ranking in the Primary RCT. Dependent variable for each panel shown at the beginning of the panel. Regressions include startup fixed effects and randomization strata based on gender, whether the worker lives in the same city as SEP headquarters, and 3 bins for year of graduation. Standard errors clustered by worker in parentheses.

Table 5: Share of Job Applications to Firms Under Different Treatments: Showing Expert Ratings Shifts Applications to Firms with Better Ratings

	(1) No Info	(2) Science Info	(3) Business Info	(4) Science & Business Info	p-value
Bad Sci Bad Biz	0.193	0.118	0.145	0.142	0.004
Good Sci Bad Biz	0.211	0.361	0.171	0.228	0.000
Bad Sci Good Biz	0.381	0.329	0.435	0.374	0.005
Good Sci Good Biz	0.215	0.191	0.250	0.256	0.019
Overall p-value					0.000

Notes: This table shows the share of applications to firms under different treatments in the Primary RCT. For example, the second row shows that in the control group (column 1), 21% of applications are made to startups with above-average science quality and below-average business quality, while in the science ratings treatment (column 2), this share increases to 36%. Each observation in the dataset corresponds to a job application (unlike Table 4, where the unit is a worker-firm possible application). p-values in the last column are from regressions of the firm type indicator (e.g., "Bad Sci Bad Biz") on treatment dummies, testing the joint significance of the treatments. The "Overall p-value" in the last row reports the joint significance of the treatment variables from a seemingly unrelated regression (SUR) model estimated using three of the four outcome variables. One outcome is excluded from the SUR because the four outcomes are mutually exclusive and exhaustive and sum to one, making it impossible to estimate all four equations jointly. All p-values account for clustering of standard errors by worker.

Table 6: The Effect of Expert Ratings on Worker Beliefs

	(1)	(2)	(3)
Panel A: Dep. Var. = Perc. Sci Quality			
Science info X Good science	0.444***		0.441***
	(0.115)		(0.115)
Science info	−0.269***		−0.268***
	(0.091)		(0.091)
Business info X Good business		0.264**	0.262**
		(0.111)	(0.112)
Business info		−0.210**	−0.207**
		(0.086)	(0.085)
F(Sci + Sci X GoodSci = 0)	0.035		0.038
F(Bus + Bus X GoodBus = 0)		0.528	0.526
Observations	1,094	1,094	1,094
Panel B: Dep. Var. = Perc. Biz Quality			
Science info X Good science	0.129		0.131
	(0.115)		(0.115)
Science info	−0.075		−0.079
	(0.091)		(0.090)
Business info X Good business		0.451***	0.451***
		(0.115)	(0.116)
Business info		−0.273***	−0.273***
		(0.087)	(0.087)
F(Sci + Sci X GoodSci = 0)	0.513		0.528
F(Bus + Bus X GoodBus = 0)		0.037	0.038
Observations	1,095	1,095	1,095
Panel C: Dep. Var. = Pr(Raise at 1m Valuation)			
Science info X Good science	6.084**		6.130**
	(2.820)		(2.838)
Science info	−4.241*		−4.325*
	(2.259)		(2.264)
Business info X Good business		9.343***	9.388***
		(2.797)	(2.804)
Business info		−4.517**	−4.537**
		(2.138)	(2.139)
F(Sci + Sci X GoodSci = 0)	0.389		0.400
F(Bus + Bus X GoodBus = 0)		0.033	0.032
Observations	1,090	1,090	1,090
Panel D: Dep. Var. = Pr(Successful Exit)			
Science info X Good science	1.664		1.691
	(2.793)		(2.800)
Science info	−0.618		−0.713
	(2.311)		(2.322)
Business info X Good business		6.200**	6.200**
		(2.709)	(2.713)
Business info		−2.036	−2.050
		(2.267)	(2.270)
F(Sci + Sci X GoodSci = 0)	0.671		0.690
F(Bus + Bus X GoodBus = 0)		0.098	0.099
Observations	1,092	1,092	1,092

Notes: This table shows the effect of information on employee beliefs using pooled data from the Primary and Secondary RCTs. Dependent variable for each panel shown at the beginning of the panel. Standard errors clustered by worker in parentheses.

Table 7: Economist Expert Survey Results: Economists Underestimate the Impact of Ratings and Fail to Predict Other Aspects of our Treatment Effects

Prediction problem	Actual	Expert Prediction
What percent more/less apps to good sci & biz firms compared to bad sci & biz firms when:		
1. No ratings shown	11%	Mean = 15 p25=3.5%, p50=10%, p75=20%
2. Science ratings shown	62%	Mean = 23 p25=10%, p50=19%, p75=30%
3. Business rating shown	73%	Mean = 28 p25=10%, p50=20%, p75=40%
4. Both ratings shown	80%	Mean = 36 p25=12%, p50=25%, p75=50%
Was the effect larger for:		
5. Science or business rating	Similar effect	Larger for sci 22%, Larger for biz 59% **Similar Effect 15%**, No effect 4%
6. Positive or negative rating	Similar effect	Larger for +ve 14%, Larger for -ve 72% **Similar effect 11%**, No effect 3%
Were science/business ratings:		
7. Complements or substitutes	Substitutes	Complements 67%, **Substitutes 27%**, No effect 6%
Did the treatment effect vary by worker:		
8. STEM degree	No difference	Smaller with STEM 30%, **No difference 29%** Larger with STEM 41%, No effect 0%
9. Quality	No difference	Smaller for high-quality 18%, **No difference 26%** Larger for high-quality 53%, No effect 3%
10. Gender	Smaller for women	**Smaller for women 14%**, Larger for women 44% No difference 42%, No effect 0%

Notes: This table summarizes expert predictions of our RCT findings by 89 economists. There are around 73 respondents per question. The first column shows the abbreviated form of the prediction questions, the second column shows our results, and the third column shows economist predictions. The percentages shown for nonnumeric questions 1 to 3 and 8 to 10 indicate the fraction of responses for each category. The exact survey questions and information shown to responders are provided in Appendix G. In terms of order in the survey, economists first made predictions related to 5-7, followed by 1-4, followed by 8-10.