Entrepreneurship and Behavioral Characteristics*

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Abstract

Using incentivized experiments and unincentivized survey questions, we measure a broad set of individual characteristics in a sample consisting of practicing entrepreneurs, individuals who intend to become entrepreneurs, and a comparison group with neither experience nor intent. We then observe the entrepreneurial status of our sample in a 2018 follow-up, two years after the first wave and eleven months after the second.

At the outset, the three groups differ on one dimension in the incentivized experiment: entrepreneurs are more willing to compete. We find no significant difference on trust, trustworthiness, cooperation or depth of strategic reasoning. At follow-up, willingness to compete does not significantly predict who changed entrepreneurial status, while conditional cooperation, and to a lesser extent trust, do. The characteristics that initially distinguished entrepreneurs were not the characteristics that predicted entrepreneurship at follow-up. These results suggest that (1) differences observed at a single moment are not a reliable guide to who will become an entrepreneur, and (2) differences between entrepreneurs and non-entrepreneurs may be driven by the experience of being an entrepreneur.

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1 Introduction

Substantial public resources have been directed at programs designed to increase entry into entrepreneurship: training programs, subsidized advice, incubators, start-up grants, and tax incentives. The logic for these public investments is that entry into entrepreneurship is constrained by a shortage of preparation rather than a shortage of people well-suited to entrepreneurship, but evidence that spreading the net widely works is weak. Fairlie et al. (2015) evaluate Project GATE, a large randomized trial in which people interested in starting a business were assigned to free training or to a control group. They found essentially no lasting effect on business ownership or performance. Oosterbeek et al. (2010) found that a college entrepreneurship program *reduced* students' entrepreneurial intentions. At a macro level, Laffineur et al. (2017) found that active labor market programs aiming to increase individuals' incentive to start a business and to reduce unemployment raise necessity entrepreneurship, have no significant effect on opportunity entrepreneurship, and have a very limited effect on unemployment.

The lack of strong results from programs designed to promote entrepreneurship should not be surprising. If such programs have an effect, it is presumably concentrated among people who are on the margin. Even restricting the population to individuals who express an interest in becoming entrepreneurs, there is doubtless wide variance in traits that predict whether individuals are likely to become entrepreneurs and, conditional on entry, succeed as entrepreneurs. A minimal *average* effect is what should be expected from an untargeted program, since most resources will be spent on individuals who are not marginal. They either lack the traits associated with successful entry into entrepreneurship, or are already so likely to succeed that an additional program makes little difference. Studies like those reported in Fairlie et al., Oosterbeek et al., and Laffineur et al. do not show that assistance cannot work, but that assistance offered to everyone who expresses an interest mostly reaches people for whom it was never going to matter. The relevant policy question is therefore not whether to encourage entrepreneurship but how to target programs encouraging it. Answering this question requires knowing which characteristics predict who enters entrepreneurship, who persists as an entrepreneur, and who succeeds as an entrepreneur.

One possibility is that the characteristics that determine entrepreneurial outcomes include stable personality traits (e.g., risk attitudes, grit, competitiveness) that make some people better suited to founding and running a business than others.[1] This suggests that researchers should study whether the personality traits of entrepreneurs differ from the general population, controlling for character-

[1]This is not meant to suggest that personal traits are the *only* characteristics that matter for entrepreneurial success. It may also be that what differs between those who succeed as entrepreneurs and those who do not is the opportunity: the quality of the idea, the market entered, the timing, the capital available, none of which is a property of the person (Shane, 2003). The two types of characteristics are not mutually exclusive and both types deserve study.

istics such as age, gender, education, and general business background. Experimental economics is well suited to this task and there exists a sizable literature examining the question.[2] These studies have found fewer clear differences than one might expect. On risk the evidence is mixed, with careful studies finding no difference between entrepreneurs and comparison groups (Andersen et al., 2014; Jiang & Capra, 2018). For competition the results are more consistent: entrepreneurs, and often those who merely intend to become entrepreneurs, are usually more willing to enter tournaments and competitive payment schemes (Holm et al., 2013; Urbig et al., 2020), although this result is not universal (Elston et al., 2005). The findings on social preferences are thinner, with some evidence of greater trust (Holm et al., 2013).

Three features of this literature limit what it can establish, motivating the design reported here.

- These studies are snapshots, observing traits and entrepreneurial status at the same moment. This makes it difficult to determine causality; do differences in traits cause differences in entrepreneurial status or is there reverse causality? The results of Cooper and Saral (2013) illustrate the problem. They find that entrepreneurs are significantly less willing to join teams than similar business people who are not entrepreneurs, with the effect particularly strong for *long-term* entrepreneurs. Does a distaste for working with others push people toward founding a firm, or does running a firm teach entrepreneurs to prefer working alone? With a snapshot it is impossible to determine the direction of causality. If personal traits cause entry, measuring them makes it possible to identify likely entrepreneurs before the fact, but if entrepreneurship produces the traits, the same measurements have no predictive content for who becomes an entrepreneur.

- Most studies measure one or two characteristics, leaving any relationship vulnerable to omitted variable bias, since the measures of interest are often correlated with each other and with demographics. For example, suppose a study finds that entrepreneurs are more competitive than a control group but does not measure overconfidence. It is well known that overconfidence leads to more competitive behavior (e.g., Camerer & Lovallo, 1999; Niederle & Vesterlund, 2007), so the result that entrepreneurs are more competitive than non-entrepreneurs could reflect greater overconfidence rather than a preference for competition.

- Comparison groups are often recruited from settings where entrepreneurial interest is likely (e.g., entrepreneurship classes) which attenuates any true difference (Andersen et al., 2014;

[2] See Cooper and Saral (2021, 2023) for surveys of this literature. The question suits experimental economists because the candidate explanations — attitudes to risk, competition, cooperation, ambiguity — are precisely the preferences incentivized experiments were built to measure: a survey asks people what they would do, an experiment pays them for what they actually do.

Jiang & Capra, 2018). More generally, a comparison group defined only by *current* status conflates people with no interest in entrepreneurship and people who intend to start a business and have not yet done so.

Our study is designed to address all three of these problems. The most important feature of our experiment addresses the first issue by using a longitudinal design. We measure individual traits and then check whether these traits predict changes in entrepreneurial status in a 2018 follow-up, two years after the first wave and eleven months after the second. The timing limits the problem of reverse causality. If risk attitudes at the start date have predictive power for whether non-entrepreneurs become entrepreneurs by the end date of the study, it cannot be that the experience of being an entrepreneur caused an individual to be initially more risk loving or risk averse. That would only be possible in a universe where time machines exist!

It may strike readers with a background in economics, where preferences are typically treated as fixed and immutable, as strange to argue that traits like risk aversion or reciprocity might change with experience. Yet, there are good reasons both within and outside of economics to believe such changes might occur. It is a mainstream view in the judgment and decision making literature that preferences are constructed (Slovic, 1995), responding to the elicitation method, context, and prior experience. For economists who are familiar with models of reference dependent preferences (Kőszegi & Rabin, 2006), it is natural to believe that experience can affect reference points and by extension observed preferences. Both field and experimental evidence suggests that standard traits like risk aversion (e.g., Bursztyn et al., 2014; Cooper & Rege, 2011) and reciprocity (Frey & Meier, 2004; Gächter et al., 2013) are not stable.

Entrepreneurship is a setting with features that make it likely preferences will not be stable. Again, consider risk and reciprocity. Entrepreneurship is an inherently risky activity and for many entrepreneurs it goes badly (Hamilton, 2000; U.S. Small Business Administration, 2026, Q9). Given evidence that bad returns from risky investments decrease future willingness to take risk (Malmendier & Nagel, 2011), it is easy to imagine that the random outcome of being an entrepreneur can affect risk preferences. Reciprocity is intrinsically related to situations people face as an entrepreneur: How do you respond to poor behavior by a partner? How do you treat an employee who goes above and beyond the call of duty? It would be surprising if the everyday experiences of being an entrepreneur left social preferences untouched. If entrepreneurship affects personal traits, cross-sectional analysis is prone to treating the consequences of being an entrepreneur as though they were prior traits. Our longitudinal design should ameliorate this issue.

To address the second issue, we measure a broad battery of incentivized behaviors — trust and trustworthiness, cooperation, depth of strategic reasoning, two measures of willingness to compete,

risk and loss aversion, preference for skewed payoffs, time preferences, honesty, and the ability to read emotion in others — alongside an extensive survey battery. Vis-à-vis the third issue, our sample includes practicing entrepreneurs, people who intend to become entrepreneurs, and people with neither experience nor intent. The last group is critical for getting an accurate contrast between individuals who are most and least likely to be entrepreneurs in the future.

The striking feature of the results is the contrast between the cross-sectional and the longitudinal results. In the cross-sectional study based on the initial experiment and survey, entrepreneurs are significantly more competitive than non-entrepreneurs, as indicated by a greater willingness to enter a tournament. Other measures such as trust, trustworthiness, cooperation and depth of strategic reasoning do not differ significantly between entrepreneurs and non-entrepreneurs. The story is quite different at follow-up. Willingness to compete does not significantly predict who changed entrepreneurial status in spite of the strong difference observed at baseline. This contrasts with conditional cooperation and trust, neither of which showed a baseline difference but both of which predict movement. Higher conditional cooperation predicts movement into entrepreneurship, while more trusting subjects are less likely to move into it. The association is especially strong for conditional cooperation. This is the central contribution of our study: the measures that distinguish entrepreneurs in the cross-sectional analysis are *not* the measures that predict future entrepreneurial status.

In the next section, we describe the subject pool and study design; in Section 3 we present the results, and we conclude in Section 4.

2 Design and procedures

2.1 Subject pool

We recruited three groups: subjects with entrepreneurial experience, subjects who intended to become entrepreneurs but had no experience, and a comparison group who neither had entrepreneurial experience nor intended to become entrepreneurs. This recruitment took place across two waves.

The first wave was recruited at Emlyon Business School, France, from five programs: the European Triple Degree, the Summer Session in Entrepreneurship, the MSc in Management, the PGM general management program for practicing executives, and pre-master students taking a course on entrepreneurship research. The second wave was recruited from the standard subject pool of the GATE laboratory in Lyon, France, to enlarge the comparison group; all but one of its subjects had

no entrepreneurial experience.

The first wave ran six sessions between March and June 2016, the second two sessions in May 2017. We collected usable data from 122 subjects, 91 from the first wave and 31 from the second. They ranged in age from 18 to 54, most held at least a bachelor's degree, and roughly 20% were international students. All sessions were run in French and English. Session-level dates and counts are in the supplementary archive, a companion website holding the full methods account, every measure we collected, and the analysis code. It is available on Krista Saral's website.

Subjects were classified from their intake survey responses on past and current entrepreneurial activity and on future intent. *Entrepreneurs* reported past or current experience and future interest in continuing. *Future Entrepreneurs* reported future interest and no experience. *Non-entrepreneurs* reported neither.

Table 1. Subject characteristics by classification

	n	Age (years)	Gender (% female)	Some graduate education (%)	High income (> €25K) (%)
Non-entrepreneur	78	27.9	39.7	51.3	23.1
Future entrepreneur	16	25.1	62.5	56.2	12.5
Entrepreneur	28	30.9	28.6	82.1	35.7

Table 1 gives demographic information for the three groups. The groups differ in age, gender, education and income, so we control for these characteristics throughout. Pooling future entrepreneurs and entrepreneurs into a single class and comparing them with non-entrepreneurs, the differences in age ($p = 0.617$), gender ($p = 0.900$) and likelihood of having a high income ($p = 0.605$) are small and not statistically significant, and a probit on all three jointly cannot reject that they are zero ($p = 0.926$). Education is the exception, with the pooled group more likely to hold a higher degree ($p = 0.021$).

2.2 Study protocol

The study had four phases to keep the time commitment in any one sitting reasonable. The initial three phases were spaced over roughly a week. The fourth came in April 2018. Games requiring interaction between subjects were run in the laboratory. Individual decision tasks were run online, where subjects could participate at a time of their choosing.

The phases were, in order: (1) an unincentivized online intake survey; (2) a laboratory experiment of five incentivized games at GATE-Lab, paying €27 on average; (3) an online experiment of five individual incentivized tasks taking under 40 minutes and paying €20 on average; and (4) a 22-question follow-up survey in April 2018, paying a randomized flat fee (see 2.6 for details).

Our analysis focuses on Phase 2, the incentivized laboratory experiments. All 122 subjects completed the measures from this phase, with one exception completed by 102 (see 2.4), and the laboratory setting gave us tight control. None of the measures in Phases 1 and 3 were completed by all subjects, and many have fewer than 100 observations (see 2.3, 2.5, and the supplementary archive for details). Given the prevalence of missing observations and the lower control online, we have less confidence in the measures gathered in Phases 1 and 3.

2.3 Phase 1: Online Survey

The intake survey collected the following items: entrepreneurial history, status and intentions, demographics, education and income; risk tolerance (Dohmen et al., 2011), patience (Vischer et al., 2013), Big 5 (Gosling et al., 2003), locus of control (Levenson, 1981), grit (Duckworth et al., 2007), cognitive style (Allinson & Hayes, 1996), entrepreneurial self-efficacy (Kickul et al., 2009), entrepreneurial intentions (Kickul et al., 2009); attitudes to risk in choosing and holding a job (Cable & Judge, 1994); and whether fear of failure would deter them from starting a business.

This is a long instrument. The psychometric scales alone run to 104 items and entrepreneurial history and demographics bring the total to roughly 125. We changed how Phase 1 was administered between waves. The first wave received the battery as two separate surveys completed at different times: the first carried entrepreneurial history, demographics, self-efficacy, intentions, the job risk items and cognitive style; the second carried self-reported risk and patience, personality, locus of control and grit. After observing that many subjects failed to complete the second survey, we modified the protocol so that the second wave received a single, shorter survey. Six items were dropped: the three job-risk items, fear of failure, and the two entrepreneurial-intention items. Reflecting both the length of the survey and the issues from Phase 1, completion rates vary across items, ranging from 122 subjects for entrepreneurial status down to 86 for the job risk items (see the supplementary archive for details).

2.4 Phase 2: Laboratory Experiment

Phase 2 consisted of five tasks.[3] A brief summary of these follows:

- **Trust and Trustworthiness (Dickinson et al., 2015):** The trust game was played using the strategy method. To obtain measures of trust and trustworthiness for every subject, subjects made choices in both roles with one role selected for payment.

  Session 1 used a single-role, direct-response version of the trust game, but after we realized that this severely limited the information we gathered on trust and trustworthiness, subjects in later sessions played both roles using the strategy method. Because of the change in protocols, data from Session 1 cannot be pooled with the later sessions. Trust therefore has 102 observations while the other games from Phase 2 have 122.

- **Cooperation and Reciprocity (Fischbacher et al., 2001):** Subjects played a linear public goods game in groups of four. Each subject received an endowment of 20 points to divide between a private account and a public account. Contributing zero is payoff maximizing regardless of the other subjects' choices but social surplus is maximized by full contribution. Each subject made two types of decisions: an *unconditional* contribution (a single amount between 0 and 20), and a *conditional* contribution schedule stating how much they would contribute for each possible average contribution of the other three group members (0, 1, …, 20).
- **Depth of Strategic Reasoning**: Subjects played the 11–20 game (Arad & Rubinstein, 2012). Subjects chose an integer from 11 to 20. Each received their requested amount (1 point = €0.10), and a further 20 points for requesting exactly one point less than the other. Based on a level-k model, Arad and Rubinstein argue that lower requests indicate greater depth of strategic reasoning.
- **Competition and Overconfidence:** Subjects played the market entry game in groups of four (Camerer & Lovallo, 1999). This begins with subjects answering a 12-question quiz. After eliciting beliefs about how their performance on the quiz ranks and how many entrants they expect, subjects choose whether to enter the market. The top-ranked entrant earned €16.50 and the second-ranked entrant earned €8.50. Any excess entrants earned nothing. Subjects who did not enter earned €5. Excess entry is typical in market entry games due to overconfidence, with the difference between beliefs and a subject's actual rank providing a

[3] Tasks were always presented in the following order: trust and trustworthiness, depth of strategic reasoning, competition and overconfidence, willingness to compete, and cooperation and reciprocity.

measure of overconfidence. Unfortunately, we chose payoff parameters that made entry too attractive. Even a subject who expects everyone else to enter, and assigns only a 20% chance to each of the top two ranks, does as well entering as not. Camerer and Lovallo (1999) use payoffs that discourage low performers *if their beliefs are accurate*, making overconfidence the cause of excess entry; ours do not. Eighty-nine percent entered.

- **Willingness to Compete**: Subjects played Niederle and Vesterlund's (2007) tournament entry design. Subjects summed five two-digit numbers for five minutes under a piece rate of €0.50 per correct answer and then under a tournament paying €2.00 per correct answer to the best performer in a group of four. The subjects then chose which scheme (piece rate or tournament) to face in the third round. Choosing the tournament provides a measure of willingness to compete.

Phase 2 was conducted at GATE-Lab, lasting roughly two and a half hours. The software was programmed in z-Tree (Fischbacher, 2007). Detailed descriptions of each game, including parameters, payoffs and screenshots, can be found in the supplementary archive.

2.5 Phase 3: Online Experiment

About a week after Phase 2, subjects received an email with a link to an online experiment programmed in oTree (Chen et al., 2016). Subjects could participate at any time, and completion typically took under 40 minutes. All five tasks were individual and incentivized: time preferences (Andersen et al., 2014); risk and loss aversion (Eckel & Grossman, 2002); preference for skewness (Grossman & Eckel, 2015); honesty, using a coin-flip task (Abeler et al., 2014); and reading others' emotions, using the Reading the Mind in the Eyes test (Baron-Cohen et al., 2001; Prevost et al., 2014). Details can be found in the supplementary archive. Attrition between the laboratory and the online experiment was substantial in spite of strong incentives to complete it. The phase paid €20 on average, yet only 95 of the 122 subjects completed it.

2.6 Phase 4: follow-up survey

We re-contacted subjects in April 2018 with a 22-question follow-up survey, two years after the first wave's sessions and eleven months after the second's. It repeated a subset of the intake survey questions. Most importantly, subjects were asked about current entrepreneurial status and intentions, including whether a business had been started or closed. We also asked about firm size and turnover for those running one. The follow-up survey gathered information about demographics

and income, simple risk and patience measures, and the cognitive style items. Repeating items from the intake survey allows within-subject change to be observed. We added one new item, on preference for working in teams.

Sixty-six of the 122 subjects responded, a retention rate of 54%. The completion fee was randomized across €5, €7.50 and €10, intended as an exclusion restriction for a Heckman correction for selective response, following Cooper et al. (2021). However, because our intended instrument did not predict completion, we cannot use a Heckman correction for selection. We return to this in Section 3.2.

3 Results

We present the cross-sectional comparisons first, then the longitudinal results from the follow-up.

Table 2. Behavior by classification, raw means

Measure	Non-ent.	Future	Entrep.	n
Trust: Amount Sent (0–5)	3.27	2.36	2.85	102
Trustworthy: Average Return Ratio	0.48	0.41	0.43	102
Public Goods: Unconditional (0–20)	9.28	10.31	9.61	122
Public Goods: Conditional Cooperation	0.92	0.69	0.95	122
Depth of Strategic Reasoning: Request	17.40	16.56	17.43	122
Competition and Overconfidence: Proportion Entering Market	0.90	0.94	0.86	122
Willingness to Compete: Proportion Entering Tournament	0.46	0.56	0.79	122

3.1 Cross-Sectional Results

Table 2 gives mean outcomes for the five laboratory experiments broken down by group (non-entrepreneurs, future entrepreneurs, and entrepreneurs; see 2.1 for definitions of groups). Note that both the trust game and the public goods game produce two measures rather than one. In the trust game, the Average Return Ratio is the proportion returned, averaged over all possible amounts sent. In the public goods game, Conditional Cooperation measures how closely subjects match what others give. Using the strategy method, each subject stated what they would contribute for every possible average contribution by the other group members, and the measure is the stated contribution as a proportion of that average, averaged across the schedule. A value of one means matching the group exactly.

For the most part, initial differences between the groups look small across all measures. Two possible exceptions stand out: (1) in the public goods game, future entrepreneurs had a lower conditional cooperation ratio than the others; and (2) entrepreneurs were more willing to compete than either of the other two groups.

The regressions shown in Table 3 formally test whether there are differences between the three groups (non-entrepreneurs, future entrepreneurs, and entrepreneurs). The dependent variable is left-censored for columns (1) and (2) and left- and right-censored for columns (3) and (4), so Tobit models are used for all four columns. Possible outcomes in column (5) are the set of integers {11, 12, …, 20} so an ordered probit is used, but a Tobit model would yield similar results. The dependent variables in columns (6) and (7) are binary, so we use probit models. The base category is non-entrepreneurs, so "Future Entrepreneur" captures the difference between future entrepreneurs and non-entrepreneurs and "Entrepreneur" captures the difference between entrepreneurs and non-entrepreneurs. The difference between entrepreneurs and future entrepreneurs is reported at the bottom of Table 3; this is not a separate estimate, but it is derived from the estimates above. We also report the results of a Wald test examining whether the two entrepreneurship dummies are jointly significant. Results are reported with Age and Gender as controls. All regressions include additional controls for income and education, but these are not reported in Table 3 to save space.

Table 3. Differences in Laboratory Measures by Entrepreneurial Status

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Trust: Amount Sent	Trust: Return Ratio	Public Goods: Unconditional	Public Goods: Conditional	11–20: Request	Market Entry	Tournament Entry
Future Entrepreneur	−0.790	0.056	0.930	−0.037	−0.980	−0.004	0.162
	(0.688)	(0.116)	(2.029)	(0.165)	(0.683)	(0.067)	(0.137)
Entrepreneur	−0.609	−0.108	−0.783	−0.048	0.237	−0.037	0.320***
	(0.479)	(0.081)	(1.648)	(0.180)	(0.552)	(0.063)	(0.102)
Age	0.040**	0.009***	0.067	0.016**	−0.010	0.004	−0.005
	(0.017)	(0.003)	(0.072)	(0.007)	(0.028)	(0.003)	(0.006)
Female	−0.116	−0.178***	−1.351	−0.131	0.376	0.010	−0.324***
	(0.357)	(0.065)	(1.285)	(0.141)	(0.433)	(0.048)	(0.096)
Entrepreneur −	0.181	−0.164	−1.713	−0.011	1.217	−0.033	0.159
Future Ent.	(0.755)	(0.130)	(2.321)	(0.198)	(0.793)	(0.085)	(0.148)
Wald test, χ^2	2.467	2.187	0.553	0.093	2.877	0.419	8.076
(p-value)	(0.291)	(0.335)	(0.759)	(0.955)	(0.237)	(0.811)	(0.018)
Observations	102	102	122	122	122	122	122

Notes: Table reports marginal effects with robust standard errors in parentheses. Columns (1)–(4) are Tobit models; the marginal effect is on the expected value of the censored outcome. Column (5) is an ordered probit; the marginal effect is on the expected request. Columns (6) and (7) are probits; the marginal effect is on the probability of entry. The omitted category is non-entrepreneur.

*Columns (1) and (2) exclude session 1. Education and income controls not reported. The Wald test is a joint test that the future-entrepreneur and entrepreneur coefficients are both zero; the statistic is χ^2 with 2 degrees of freedom, with its p-value in parentheses. *** p<0.01, ** p<0.05, * p<0.1.*

The regression results are largely consistent with our impressions from the raw means. For most of the models, neither entrepreneur dummy is significant. Tournament entry, column (7), is the one notable exception where the difference between entrepreneurs and non-entrepreneurs is strongly significant. The raw data made it appear that future entrepreneurs differed on Conditional Cooperation, but the regressions with additional controls do not support this impression. Age and gender are significant in a number of the models. It is notable that women are significantly less willing to compete, again in column (7). This is consistent with Niederle and Vesterlund (2007), who find the same gender gap. The raw difference does not survive controls.[4]

Future entrepreneurs are not significantly different from either group in any of the seven models, which reflects in part their small number. Some of these individuals will eventually go on to be entrepreneurs, but many will not.

3.2 Longitudinal Results

Sixty-six subjects responded to the follow-up. Entrepreneurial status had changed for 22 of the 66 returnees. Of particular interest, 13% of returnees who had not previously been entrepreneurs became entrepreneurs and 50% of the baseline entrepreneurs exited entrepreneurship.

Table 4 reports the results from ordered probits studying the association of the experimental measures with change in entrepreneurial status. The dependent variable is entrepreneurial status in 2018 and all models include a control for entrepreneurial status at intake, so the parameters measure the effect on *changes* in entrepreneurial status. Column (1) estimates effects for all seven measures jointly. It includes additional controls for age, gender, wave, education, and income. Estimates for these controls are not included to save space. Column (1) reports parameter estimates and column (2) reports marginal effects (the effect of a one standard deviation change on the probability of being an entrepreneur in 2018).

Two baseline measures predict changes in entrepreneurial status. Subjects who were more conditionally cooperative in the public goods game were more likely to move toward entrepreneurship. This effect is strong. A one standard deviation increase is associated with a 12.6 percentage point higher probability of being an entrepreneur in 2018. Trust is negatively associated with movement

[4]The second difference is significant (p = .014) and the first barely misses (p = .107). Both come from a regression of age on the two classification dummies with robust standard errors.

toward entrepreneurship, but the association is weaker both in terms of statistical significance and the marginal effect (-7.4 percentage points). Beyond weak statistical significance, the marginal effects for the other measures are all small.[5] It is striking that competitiveness (Tournament Entry), the one laboratory measure that differed between groups at baseline, does not significantly predict change in status.

Table 4. Predicting Change in Entrepreneurial Status

	(1)	(2)	(3)	(4)
Measures are estimated …	Jointly	Jointly	Separately	Jointly
Reported	Parameters	Marginal Effects	Parameters	Parameters
Controls	All Controls	All Controls	All Controls	Baseline Status
				Education
Trust: Amount Sent	−0.285*	−0.074*	−0.136	−0.304**
	(0.156)	(0.042)	(0.118)	(0.136)
Trust: Return Ratio	0.647	0.034	0.097	0.824
	(0.796)	(0.044)	(0.654)	(0.756)
Public Goods: Unconditional	−0.024	−0.026	0.000	−0.018
	(0.026)	(0.029)	(0.024)	(0.026)
Public Goods: Conditional	0.887***	0.126***	0.717***	0.859***
	(0.226)	(0.022)	(0.174)	(0.235)
11–20: Request	−0.084	−0.036	−0.035	−0.063
	(0.057)	(0.025)	(0.059)	(0.057)
Market Entry	−0.319	−0.054	−0.116	−0.241
	(0.712)	(0.126)	(0.539)	(0.572)
Tournament Entry	−0.059	−0.009	−0.366	−0.062
(Competitive)	(0.494)	(0.078)	(0.398)	(0.403)
Controls	Full	Full	Full	Status, Education
Observations	66	66	66	66

Notes: Robust standard errors in parentheses. The dependent variable is entrepreneurial status in 2018. Columns (1), (3) and (4) report ordered probit coefficients. Column (1) enters all seven laboratory measures in a single regression. Column (2) reports, from the column (1) model, the average marginal effect of a one-standard-deviation change in each measure on the probability of being an entrepreneur in 2018 (standard deviation in the estimation sample; for the trust measures, among the 57 respondents with valid trust data); for market entry and tournament entry it is the effect of a change from 0 to 1. Column (3) reports each measure from a separate regression that includes only that measure. Column (4) repeats column (1) with reduced controls. Full controls are baseline entrepreneurial status, age, gender, wave, education and income; reduced controls

[5]Among the control variables, only education has an effect (which is strong and positive).

*are baseline entrepreneurial status and education. Control coefficients are not reported. The two trust measures are demeaned, and the 9 respondents from session 1, which ran a different version of the trust game, are assigned a value of zero. *** p<0.01, ** p<0.05, * p<0.1.*

In the introduction, we noted that a flaw in existing designs is their one-at-a-time approach, which raises the possibility of omitted variable bias. To see if this is a valid concern, we re-estimated the ordered probit using each measure on its own while keeping the other controls fixed. This is column (3). Considering the measures separately rather than jointly does affect our conclusions. Trust no longer has significant predictive power, suggesting that our concern about omitted variable bias is valid.

A second concern is that we are overfitting the data as we have a large number of covariates relative to the sample size. To address this concern, we used lasso to select covariates. The four that survive are initial entrepreneurial status, education, conditional cooperation, and trust. Column (4) reports an ordered probit that keeps all seven measures, so the estimates remain comparable with columns (1)–(3), but retains only the two controls the lasso selected. The effect on the estimates is minimal, although the Trust effect is somewhat stronger.

There are two legitimate reasons to interpret the preceding results with caution. First, the sample size is limited and we are not considering all possible measures (so omitted variable bias remains a concern). We have done some checks on whether adding our partial measure of risk tolerance affects the results, as this is a likely candidate for omitted variable bias; it does not. Second, the instrument built into our design failed, so we cannot correct for selective response. Running a probit on probability of return, we find some evidence that selection occurred. Subjects who reasoned further ahead in the 11–20 game were more likely to return, and those who chose the tournament were less likely to. Both are measures we test, so the follow-up sample is thinnest in exactly the dimensions where we report nulls. We have no way of knowing whether selection affects our results, but the concern is legitimate.

3.3 Other Measures

The study collected a great deal of data besides the laboratory games. While we focus on the lab experiments, it is worth considering the totality of the data set. Table 5 examines differences between groups on measures gathered in the Phase 1 survey and the Phase 3 online experiment. In the interest of brevity, we only report measures for which there was a significant difference between groups. Specifically, we ran regressions analogous to those reported in Table 3 for every measure and kept measures for which one of the group dummies or the Wald test was significant at the 10%

level.

Looking at which measures were kept, it is notable that three different versions of a risk measure appear. The simplest to interpret is the survey measure; entrepreneurs were more risk tolerant. Openness to experience is also significantly higher for entrepreneurs, which parallels existing findings in the entrepreneurship literature (e.g., Zhao & Seibert, 2006). Reading the Mind in the Eyes is weakly lower for entrepreneurs. This measure is usually interpreted as capturing theory of mind and has been found to be a valuable predictor of performance in group settings (Weidmann & Deming, 2021). Entrepreneurs are also weakly lower on honesty.

Table 5. Differences Between Groups, Additional Measures

Measure	Raw mean: Non-ent.	Raw mean: Future	Raw mean: Entrep.	Marginal effect: Future ent.	Marginal effect: Entrep.	Marginal effect: Entrep. − Future	Wald χ^2 (p-value)	n
Survey: Risk Tolerance	5.89	6.91	7.90	1.094**	1.754***	0.660	20.940	101
				(0.510)	(0.322)	(0.552)	(0.000)	
Avoids Risk in Job Choice	3.23	3.67	4.26	0.356	0.939*	0.583	3.548	86
				(0.568)	(0.513)	(0.743)	(0.170)	
Risk, Eckel–Grossman	3.63	4.19	3.53	0.737**	−0.272	−1.009**	6.409	95
				(0.351)	(0.326)	(0.404)	(0.041)	
Openness to Experience	5.82	6.00	6.42	0.303	0.422**	0.120	5.493	101
				(0.332)	(0.174)	(0.347)	(0.064)	
Conscientiousness	5.81	5.32	5.84	−0.492***	0.023	0.514	7.159	99
				(0.188)	(0.285)	(0.323)	(0.028)	
Reading the Mind in the Eyes	26.59	26.92	24.88	0.322	−1.862*	−2.184*	4.329	95
				(0.906)	(0.989)	(1.152)	(0.115)	
Locus of control, Internal	32.33	34.55	34.37	2.676*	1.553	−1.123	3.391	99
				(1.533)	(1.624)	(2.005)	(0.183)	
Innovate	0.65	0.58	0.56	0.046	−0.241*	−0.287*	4.086	89
				(0.147)	(0.131)	(0.163)	(0.130)	
Honesty	2.21	2.00	1.75	−0.313	−0.345*	−0.032	3.771	95
				(0.308)	(0.188)	(0.327)	(0.152)	

Raw means are unadjusted group means on each regression's estimation sample. The three marginal-effect columns report marginal effects at the covariate means, with robust standard errors in parentheses, from regressions with the same controls as Table 3 (age, gender, income and education; control estimates not reported). The omitted category is non-entrepreneur. The Wald test is a joint test that the future-entrepreneur and entrepreneur coefficients are both zero (χ^2 with 2 degrees of freedom, p-value in parentheses). *** p<0.01, ** p<0.05, * p<0.1.

It is worth noting that some variables we anticipated a priori to be more important do *not* appear in Table 5. For example, given entrepreneurs' need to deal with failure, we expected Grit to appear but it does not.

4 Conclusion

Measured at one point in time, entrepreneurs in our sample differ from everyone else on one laboratory measure: they are significantly more willing to compete. The effect is large at 32 percentage points and it remains after demographic controls. This is consistent with a growing body of work finding competitive preferences elevated among entrepreneurs, including Holm et al. (2013) and Urbig et al. (2020). We find no significant difference on trust, trustworthiness, cooperation or strategic sophistication.

At follow-up, the story changes. Willingness to compete does not significantly predict who moved into or out of entrepreneurship. What does predict movement is conditional cooperation, surviving on every specification we ran, and to a lesser extent trust, with more trusting subjects less likely to move toward entrepreneurship.

The disagreement between our cross-sectional and longitudinal results is not a failure of either. As we argued at the outset, a difference observed at one moment cannot separate characteristics that lead people into entrepreneurship from characteristics the experience of being an entrepreneur produces. A longitudinal design asks the following: given what we knew about a person in 2016, can we predict the change in their status by 2018? There is no reason the two sets should coincide, and here they do not: willingness to compete is strongly associated with being an entrepreneur but does not significantly predict, while conditional cooperation is unassociated with being an entrepreneur but does.

Two interpretations exist, and our data do not separate them. These characteristics may be partly a product of entrepreneurial experience rather than a cause of entry, as we argued above. Or willingness to compete may be a relatively fixed characteristic that does sort people into entrepreneurship, but that had already sorted most of our entrepreneurs in before we met them.

A strength of our study is that it has a sample of practicing entrepreneurs rather than just students, a comparison group of people with neither experience nor intent, a battery far wider than one or two characteristics, and the same people re-contacted at follow-up. What it does not have is size. Our number of entrepreneurs and participants planning to become entrepreneurs can only detect large differences and little else; the follow-up was unable to correct for selective response, and one of the two competition measures failed outright.

What we offer, in our view, is a map. We measured a wide battery because it was not obvious in advance which characteristics would matter, and the return on that is a short list: competition in the cross-section, conditional cooperation and trust at follow-up. The design matters as much as

the list. Our results show why. The one measure that separated entrepreneurs at the outset did not significantly predict at follow-up, while a measure showing no initial difference did. We believe this keeps open the debate over whether entrepreneurial outcomes reflect differences between people or between opportunities (Shane, 2003). That individual characteristics predict who moves, even in a sample as small as ours, suggests the debate is worth pursuing — and that longitudinal designs are better placed to settle it than the cross-sections this literature has mostly relied on.

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