Identifying and Teaching High-Growth Entrepreneurship: Experimental Evidence from Academies for University Students in Uganda

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Abstract

We evaluate the effects of skill formation and selection on entrepreneurial success. To study skill formation of nascent entrepreneurs among Ugandan university students, we randomly accept applications to a business training program fostering an entrepreneurial mindset. We measure labor market outcomes, business creation and success, and cognitive and non-cognitive skills as key outcomes up to three years after program participation. Our preliminary findings 9 months after the training show higher business creation for training participants. To better understand individual motivation for entrepreneurship, we experimentally vary marketing messages to all interested students prior to their application decision, emphasizing either entrepreneurial profit or entrepreneurial freedom. Emphasizing non-pecuniary benefits of entrepreneurship leads to more applications, as well as heterogeneous selection. Lastly, we describe endogenous self-selection through non-experimental comparisons among applicants and eligible students from the same population who were aware of the entrepreneurship training program but did not express interest. The results are consistent with training being a substitute for other sources of entrepreneurial knowledge.

Keywords: Entrepreneurship training, firm productivity, selection, field experiment
JEL codes: O12, D22, L26, D91, C93
Study pre-registration: American Economic Association RCT Registry #4502.

Timeline

The ongoing project consists of three waves of entrepreneurship training academies. A *wave* consists of pre-intervention data collection, intervention, an implementation check (one to two months after the intervention), a midline survey (nine months later) and two endline surveys. Endline Survey I takes place 18 months after the intervention and Endline Survey II 30 months after the intervention. Currently, we have implemented two waves. This allows us to report (pre-registered) results regarding short-term outcomes of the training, endogenous self-selection into the training and individual motivations of students through experimentally varied marketing messages.

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1. Introduction

Entrepreneurship is key for economic development (Schumpeter, 1911). While most individuals in low-income countries are self-employed (e.g., 78.1 percent of the working population in Uganda was self-employed in 2019), these are mainly small-scale businesses that are only remotely related to the Schumpeterian entrepreneurship that drives economic growth (Hsieh and Olken, 2014; Porta and Shleifer, 2008). They typically lack capital and entrepreneurial ability, preventing them from reaping the full benefits of high-return investment opportunities (Beaman, Magruder, and Robinson, 2014; Bruhn, Karlan, and Schoar, 2018; De Mel, McKenzie, and Woodruff, 2012). While relieving credit constraints shows some improvement in terms of business profits, it does not result in sustained business growth (Banerjee et al., 2015). Interventions aimed at improving business practices and managerial capital have not been shown to result in transformational increases in profits or employment (McKenzie and Woodruff, 2014; McKenzie, 2020). More promising approaches focus on the role of the psychology of entrepreneurship. Campos et al. (2017) show that training programs focusing on soft skill concepts, such as *personal initiative* and the *entrepreneurial mindset*, outperform programs teaching accounting, finance and marketing skills.[1]

Most business training studies target existing businesses—with the notable exception of Klinger and Schündeln (2011), Blattman, Fiala, and Martinez (2014), and Premand et al. (2016)—but neglect the importance of selection into entrepreneurship. Levine and Rubinstein (2017) and Levine and Rubinstein (2018) provide evidence that successful entrepreneurs in the USA are positively selected on human capital. Moreover, evidence from high-income countries shows that cognitive and non-cognitive traits predict entrepreneurial success (Andersen et al., 2014; Koudstaal, Sloof, and Van Praag, 2016; Levine and Rubinstein, 2017). Yet little is known on whether traits are shaped by entrepreneurial activity, or whether people select into entrepreneurship based on these traits. This distinction is important for policy. If relevant non-cognitive traits are malleable, this would favour programs aimed at developing an entrepreneurial mindset. If they are not, interventions designed to identify high-potential entrepreneurs would be more promising.

We seek to disentangle the extent entrepreneurial success can be attributed to skill formation and to selection. First, we causally identify the effects of a business training program, which develops an entrepreneurial mindset, on business creation and business performance. In our field experiment, training is randomly offered to university students in Uganda who had expressed interest in entrepreneurship, a suitable sample positively

[1] Entrepreneurial mindset is one's ability to spot and benefit from opportunities that are encountered in daily life. Personal initiative captures one's desire to proactively tackle problems (Frese et al., 2007).

selected on human capital (about 5 percent of Ugandan students pursue tertiary education). Second, we study how selection into the entrepreneurship training program varies by motives and personality traits. Using panel-data drawn from the same population, we document how students interested in entrepreneurship differ from those that are not with respect to socio-economic, cognitive and non-cognitive factors, as well as labor market outcomes, including self-employment. Third, we causally identify what motives draw students to entrepreneurship training.

We partner with a Ugandan organization, StartHub Africa, that provides extra-curricular entrepreneurship training academies at local leading universities. We track three semesters of training academies (henceforth "waves") conducted at eight to ten universities with a combined enrollment of around 2,000 students in our study sample.[2] Each wave consists of a marketing campaign, an application phase, and an entrepreneurship training academy. A wave begins with an untargeted marketing campaign to raise general awareness of the program. Then, to be eligible for the program, students must attend an information session that consists of short presentations that summarize the training program. This is also where the application forms are distributed.

Our experimental design relies on two sources of exogenous variation. First, we randomly vary the motivational message for becoming an entrepreneur that is marketed in the information session video presentations: financial gains or creative freedom. This allows us to causally identify the motivations of applicants. Second, among those who applied, we randomly offer admission to the program to identify the effect of being offered admission on business creation, survival and performance. We complement these analyses by documenting patterns of entrepreneurial self-selection by comparing applicants to those who were aware of the training program but did not express interest along several repeated measures of socio-economic indicators, personality traits and preferences.[3] The data collection effort includes surveys at different points in the self-selection and application process, as well as surveys administered both before and after the entrepreneurship training academies (Figure 1).

This study relates to four strands of literature. First, we contribute to the literature on entrepreneurship and business training in low-income countries by studying a unique sample of highly-educated, high-potential individuals (see Levine and Rubinstein, 2017, 2018) who aspire to be entrepreneurs. Despite extensive research on business training interventions, there is a paucity of evidence on the effects of training on high-skilled youths. Interventions in low-income countries typically provide middle-aged, incumbent

[2]Two waves have been conducted to date. We plan to include one more wave. We will discuss the feasibility of this extension and base our power calculations both on the status quo and the planned implementation.
[3]We elicit data on the Big-5 personality traits, grit, personal initiative and aspirations. Further, we gather measurements of time and risk preference as well as individuals' degree of loss aversion.

micro-entrepreneurs with education on business skills and managerial capital, which have not been found to result in sustained increases in revenue, profits or employment (Hsieh and Olken, 2014; Bruhn, Karlan, and Schoar, 2018; Bruhn and Zia, 2013; McKenzie and Woodruff, 2014; McKenzie, 2017; Rigol, Hussam, and Roth, 2018). This population, however, may lack the necessary skills for becoming successful entrepreneurs (Levine and Rubinstein, 2018; Bjorvatn and Tungodden, 2010; Hurst and Pugsley, 2011; Carlson and Rink, 2019) or may be unwilling or unable to change the way they run their businesses (Burmeister and Schade, 2007). With respect to our target population, the most closely related study are Chioda et al. (2021) and Premand et al. (2016). Chioda et al. (2021) implement a mini-MBA program in Ugandan high-schools, whose participants are, on average, about three years younger than ours. Training participants are more likely to own a business and achieve significantly higher profits 3.5 years after the training. Premand et al. (2016) analyze the inception of an official entrepreneurship track at universities in Tunisia. They document modest increases of one to four percent in self-employment rates but no effect on overall employment.[4] Our setting differs from theirs in that we study an extra-curricular program that is more likely to only attract the genuine subpopulation of those interested in pursuing entrepreneurship.

Second, we contribute to the literature on the entrepreneurial mindset. The entrepreneurship training program we study is based on a curriculum that aims to foster an entrepreneurial mindset and personal initiative. Campos et al. (2017) show that this type of training results in larger increases of profits than a traditional business training program. Ubfal et al. (2019) find transient, short-term effects of this type of training on micro-entrepreneurs in Jamaica. We complement this burgeoning literature by offering further evidence on the merits of non-traditional training programs and enhance it by focusing on nascent entrepreneurs who have been found to benefit from traditional training programs (see Klinger and Schündeln, 2011).

Third, we contribute to the literature on selection into entrepreneurship and predictors of entrepreneurial success. Levine and Rubinstein (2017) show that successful entrepreneurs select along both cognitive and non-cognitive dimensions. Evidence from high-income countries suggests that cognitive and non-cognitive traits are important predictors of entrepreneurial success (Andersen et al., 2014; Koudstaal, Sloof, and Van Praag, 2016; Levine and Rubinstein, 2017). For example, entrepreneurs are generally more risk-tolerant (Bouchouicha and Vieider, 2019) and display more overconfidence (Åstebro, Jeffrey, and Adomdza, 2007; Herz, Schunk, and Zehnder, 2014). Evidence is scarce on

[4]This speaks to substitution from wage employment to self-employment, and does not imply overall employment effects. Alaref, Brodmann, and Premand (2020) present results from a medium term follow-up and show that any effects were short lived: four years after the program, there are no differences in self-employment and wage employment rates between the treatment and control groups.

whether non-cognitive traits are shaped by entrepreneurial activity or whether people select into entrepreneurship based on these traits. On one hand, an established view suggests that preferences are relatively stable (Schildberg-Hörisch, 2018). On the other hand, there is recent evidence that personality traits, such as grit, may be malleable —- at least among young adolescents (Alan, Boneva, and Ertac, 2019). We extend this literature by documenting personality traits, preferences, and beliefs before individuals select into entrepreneurship, how these differ by interest in entrepreneurship, and by identifying how entrepreneurship training affects these characteristics.

Fourth, we speak to the motivations of becoming an entrepreneur, and whether selection patterns differ by motivation. A sparse literature using observational data from the USA stresses that non-pecuniary benefits, such as being one's own boss or having flexible working hours, play a first-order role for business creation decisions and that these independence-oriented workers are willing to forgo higher earnings from wage-employment (Hamilton, 2000; Hurst and Pugsley, 2011, 2015). Guzman, Oh, and Sen (2020) and Ganguli, Huysentruyt, and Le Coq (2018) confirm the importance of motives and differential responses to monetary and non-pecuniary motives resulting in selection patterns into entrepreneurship competitions in randomized field experiments in the USA and the UK, respectively.[5] We complement this recent literature by identifying the differential selection decisions made by high-skilled youth in a low-income country using random variation in the salience of different motives for entrepreneurship.

2. Research design

2.1. **Background.** StartHub Africa (SHA) conducts the academy at local universities during the academic semester. There is one academy per university which has a target class size of 40 students that spans nine weeks with one three-hour session each week. The academy covers all stages of training for nascent entrepreneurs: developing a business idea, creating a prototype, and implementing the idea. In the curriculum developed by SHA, management skills, such as cost accounting, and basic principles of finance and marketing are included, but emphasis is placed on developing participants' personal initiative to foster their entrepreneurial mindset. In this respect the training program is similar to the program studied by Campos et al. (2017). Lecturers are encouraged to create an interactive atmosphere, and the standardized materials SHA provides to the instructors require active input from the participants. Finally, the curriculum contains a number of practical exercises outside of the classroom. For instance, students are taught

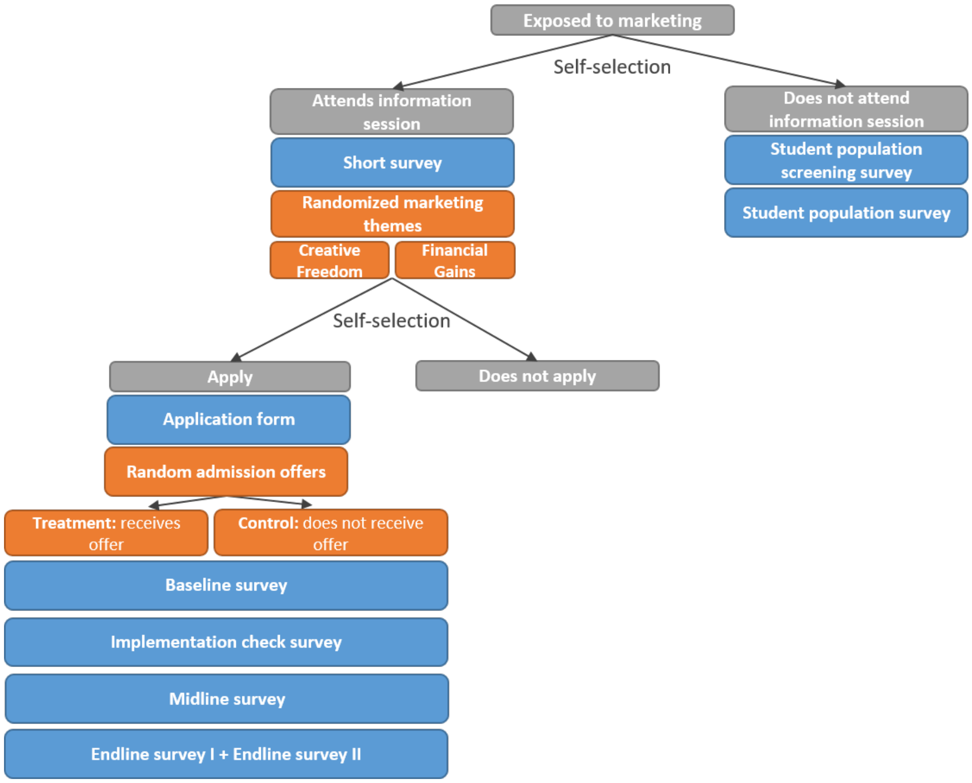
[5] Ashraf et al. (2020) vary the salience of career incentives in a recruitment drive for public health workers in Zambia, and also show that the salience of motives affects selection patterns, and later, performance on the job.

basic principles of market research, then brainstorm product ideas and spend the rest of the session venturing out on campus to assess people's reaction to their product ideas. The training is taught by university lecturers or respected entrepreneurs from the local community that have been extensively trained and are continuously supported by SHA.

The academy is preceded by a marketing and application phase which spans the first three weeks of the semester. During the marketing phase, SHA creates awareness of the program using posters and flyers across campus, and in short pitches in classrooms and at campus events. Students are informed that attending an information session is a prerequisite for applying. Six to twelve of these 30-minute sessions are held per day over two or three days in a central location at each university. The information sessions provide detailed information on the academy's content, the expectations of the participants, in particular the time commitment necessary to complete the academy, success stories from previous participants, and the possibility to ask questions to SHA staff. To harmonize the information sessions as much as possible, the same SHA staff hold the information sessions throughout each day. Moreover, the presentations are video-based and contain the same structure: motivation for the academy, details, deliverables and requirements of the academy, and success stories from alumni. After the information session, students could pick up an application form in person, fill it out (in 10 to 15 minutes) and return it either to the team conducting information session, or to a well-know place on campus indicated on the application form. Application forms were only available to participants of the information sessions.

2.2. **Experimental design.** We exploit two sources of exogenous, experimental variation. First, in the *entrepreneurship training experiment*, admission to the academy is randomly offered to a subset of applicants. We use this variation to estimate the causal effect of being offered admission to the academy on entrepreneurial activity and economic outcomes. We also investigate effects on cognitive and non-cognitive skills. Second, to understand in more detail the characteristics and motivations of these young entrepreneurs, we add a second layer of exogenous variation: In the *selection experiment*, we randomly vary whether marketing for the academy emphasizes financial independence or creative freedom as motivation for becoming an entrepreneur. This variation allows us to identify how motivation impacts the application decision and to study heterogeneous effects based on individual characteristics. Figure 1 presents the experimental design. Finally, using a sample drawn from the same population, we document endogenous self-selection by comparing eligible students who did not express interest in the academy to applicants. We also investigate how key outcomes from the *entrepreneurship training experiment* evolve differentially over time between students who did not express interest to those who applied but did not receive training.

Figure 1. Experimental design and data collection



Notes. Different phases of the experimental design and self-selection decisions is marked in grey, exogenous experimental variation is marked in orange, and data collection is marked in blue.

We first discuss the *selection experiment* and the complementary observational examination of self-selection, and then the *entrepreneurship training experiment* because this follows the chronological journey of a student from hearing about the training to submitting an application and possibly being offered admission. The sample selection procedure will be detailed in Section 3.

Understanding selection and motives. The first layer of experimental variation is induced by randomly exposing clusters of students to different marketing messages during the information sessions. In order to apply to the entrepreneurship training program, students ought to attend information sessions where application forms can be obtained. We randomly vary the content of those information sessions by emphasizing either that entrepreneurship offers the possibility of achieving *financial independence*, or that entrepreneurship offers the *freedom to be creative*. Information sessions take approximately 15-20 minutes and the content is presented by a member of our partner organization. Support staff ascertained that no student listened to two information sessions by either staying in the room for the next session or entering early during an ongoing session. In each session, a presenter went through 12 presentation slides and two videos.

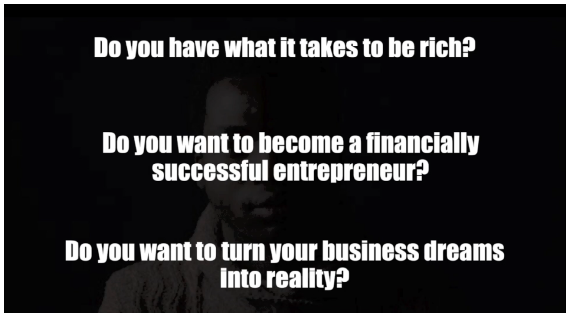
In the *selection experiment*, the focus of the presentations is randomly varied by emphasizing i) that entrepreneurship offers the possibility of achieving financial independence, and ii) that entrepreneurship offers the freedom to be creative. Two videos constituted the main source of variation in the presentation. This guaranteed that students across sessions are exposed to the identical content. The first video differed in both visual and audio content, the second video only differed in audio content. Videos were embedded in the presentations to reduce technological complexity. In addition, the respective motives are varied in four of the twelve overall slides reiterating the benefits of becoming an entrepreneur and in the corresponding voice-over of these slides. Everything else is kept constant. This exogenous variation allows us to cleanly identify how the pool of applicants differs across these two messages.

In Figure 2 we show examples of different content across the two treatments. In panels (a) and (b), we show a still frame of the first video's first slide. Two of three statements differ, and the voice over emphasized the differences between the two treatments. Note that *not* entire presentation was kept in this black and white layout. In panels (c) and (d) we show the first frame of the second video. Again, the voice over emphasized the differences.

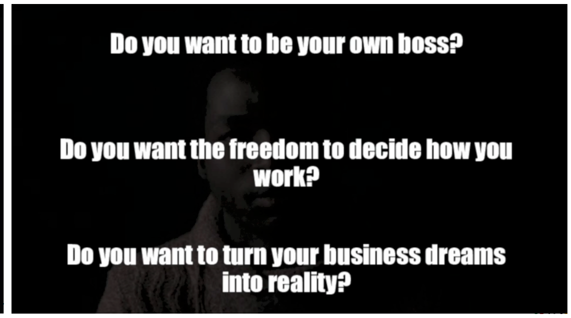
To analyze selection into the academy, we compare students who are interested in entrepreneurship, indicated by applying to the academy, with those who are not interested in entrepreneurship indicated by being aware of the entrepreneurship academy and not attending an information session. We refer to this latter group as the *non-interested subpopulation*. In other words, conditional on having been exposed to the marketing phase, we investigate what drives certain individuals to opt-in to the academy.

Entrepreneurship training experiment. The *entrepreneurship training experiment* allows for causally estimating the effect of the academy on individuals' self-employment probability, as well as on labor market outcomes and personality traits. Having participated in an information session, students decide whether to apply to the academy. A random sample, stratified by year and field of study, is then drawn from the set of all applications and offered admission to the training program — the treatment group. The remainder is placed into the control group.

2.3. **Hypotheses.** Grounded in the results of previous work, there are several hypotheses we seek to test. The first set of hypotheses concerns the effects of entrepreneurship training on economic and business outcomes and inputs. First, as shown by Klinger and Schündeln (2011) for a traditional entrepreneurship training program, we hypothesize



(a) First video, profit



(b) First video, creative freedom

(c) Second video, profit

(d) Second video, creative freedom

Figure 2. Example for treatment variation in information sessions

that participating in the entrepreneurship academy fosters business creation. We further hypothesize that participation in the academy will improve business performance, captured by indicators such as monthly sales and profits, measures of capital and labor input, and measures of general economic self-sufficiency. One particular dimension we are interested in is labor input, and whether treated subjects create jobs through the businesses they create. The hypotheses are summarized in Table 1, Family 1.1. Positive findings for these hypotheses would provide evidence for entrepreneurial activity being teachable.

Second, we seek to identify channels through which the entrepreneurship training effects the primary outcomes of business creation and performance. Campos et al. (2017) find that a personal initiative training program can deliver lasting improvements for small business owners and they identified several channels: application of successful business practices, increased personal initiative, increased capital and labor inputs, substantial innovative activity (e.g., in the form of new products originating from own ideas) and product differentiation. We therefore hypothesize that participation in the academy leads to implementing more successful business practices, improved financial professionalization, marketing activities, product and process innovation, and better access to business networks. The hypotheses are summarized in Table 1, Family 1.2, Hypotheses 1 to 6. Finding effects along these dimensions would lend evidence to the most effective channels through which entrepreneurship training impacts the economic outcomes listed in Family 1.1.

Moreover, as laid out before, there is evidence that entrepreneurs are positively selected on cognitive and non-cognitive traits. Little is known, however, about whether non-cognitive traits may be shaped beyond adolescence. We therefore test hypotheses that investigate whether participating in the academy shapes non-cognitive traits. These hypotheses are summarized in Table 1, Family 1.2, Hypotheses 7 and 8. These hypotheses allow us to test whether — and to what extend — non-cognitive traits are malleable through participation in entrepreneurship training.

The second set of hypotheses concerns selection into entrepreneurship. First, individuals may have different motives for desiring to be an entrepreneur. Guzman, Oh, and Sen (2020) study entrepreneurs and find that women and individuals located in more altruistic cultures are motivated more by social-impact messages than money, whereas men and those in less altruistic cultures are motivated more by money than potential social-impact. Ganguli, Huysentruyt, and Le Coq (2018) document a crowd-out between extrinsic, cash-based and intrinsic, social motives for social entrepreneurs. While extrinsic motivational messages affect effort in applications for a start-up grant, it reduces the pool of applicants at the same time. Further, business success was less likely: social entrepreneurs motivated by extrinsic messages worked fewer hours per week, created fewer employment opportunities, and profited less from their venture. We therefore test which marketing message attracts more applicants: whether monetary motives or the promise of independent work better draws young, highly-educated individuals to entrepreneurship. We also investigate the types of individuals that are drawn to the different marketing messages. We consider measures of average cognitive ability, over-confidence and entrepreneurial self-assessment. These hypotheses are summarized in Table 1, Family 2.1, Hypotheses 1 to 4. These hypotheses test whether stressing different motivations for becoming an entrepreneur lead to differential application patterns, both in terms of the quantity of applications and the attributes of the applicants themselves. Finding differences between the two messages would also speak to how different motivations to undertake entrepreneurship training are correlated with certain individual characteristics, and how such motivations shape the composition of applicants.

Further, we document selection into entrepreneurship (as proxied by selection into the academy) by comparing those that applied to the academy to those that were exposed to the marketing campaign but did not apply for the program (*non-interested subpopulation*). The outcomes of interest are listed under Hypothesis Families 2.2.1 and 2.2.2, and mirror those in Hypothesis Families 1.1 and 1.2 from the primary outcomes of the *entrepreneurship training experiment*. Comparing baseline characteristics and outcomes between the two groups allows us to identify the dimensions on which individuals select into entrepreneurship. Those and additional measures are investigated at endline to document how the non-interested subpopulation evolves over time compared to those that applied to the training and were not admitted (control group).

The outcome variables and their measurement are detailed in Section 3.2, while the empirical analysis is detailed in Section 4. Our results will inform to what extent teaching entrepreneurial skills and selection are important aspects for entrepreneurship. This is interesting from an academic perspective as it addresses fundamental questions on skill formation and its potential repercussions for entrepreneurship. It is also of utmost importance for policy: If entrepreneurial skills can indeed be formed, we offer an evaluation of a cost-effective, relatively easy to implement, and scalable intervention for high-potential, well-educated individuals. We can also document whether the nascent entrepreneurs originate from high-skilled individuals that would otherwise be unemployed or whether they are substituting away from formal-employment. If selection is found as relatively more important for entrepreneurial success, our study would inform policy makers that identifying high-potential entrepreneurs is of first-order importance (see McKenzie (2017) and Rigol, Hussam, and Roth (2018) who seek to identify high-potential entrepreneurs, and Shane (2009) who warns about dragging people into risky, non-growth entrepreneurship). Our results would also offer some guidance on the motives that attract these entrepreneurs-to-be.

2.4. **Time frame.** The proposed project consists of three waves of entrepreneurship training academies. Each wave consists of the implementation of the entrepreneurship academies, the experimental variation introduced in both the *entrepreneurship training experiment* and the *selection experiment*, and the data collection before and after the intervention. As detailed below, there will be a baseline survey, an implementation check survey (one to two months after the intervention), a midline survey (nine months later) and two endline surveys. The Endline Survey I takes place 18 months after the intervention, the Endline Survey II 30 months after the intervention of the last wave.

The first wave started in September 2019, and the second wave started in January 2020. The third wave is scheduled for September 2021. Table 2 sets out the detailed time line for all steps in all waves. The implementation of Wave I and Wave II is already in progress, midline survey data collection (Wave II) scheduled soon. Later data collection and the implementation of Wave III is planned.

Table 1. Overview of hypothesis families.

Family	H #	Hypotheses title	Index	Data collection				Sample	Exogenous variation
				Baseline	Midline	Endline	Short		
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
1. Entrepreneurship training									
1.1. Economic outcomes (primary)	1	Business creation	✓	✓	✓	✓		Applicants	Random assignment to training (*entrepreneurship training experiment*)
	2	Business success	✓		✓	✓			
	3	Capital and labor input	✓		✓	✓			
	4	Economic self-sufficiency	✓		✓	✓			
1.2. Business and personal input (secondary)	1	Business practices	✓		✓	✓		Applicants	Random assignment to training (*entrepreneurship training experiment)*
	2	Financial professionalization	✓		✓	✓			
	3	Marketing	✓			✓			
	4	Innovation	✓			✓			
	5	Networks	✓	✓	✓	✓			
	6	Entrepreneurial mindset	✓	✓	✓	✓			
	7	Owner's non-cognitive traits	✓	✓	✓	✓			
	8	Preferences	✓	✓	✓	✓			
2. Selection									
2.1 Selection into entrepreneurship (primary)	1	Submitted application					✓	Attendees at Info Sessions	Random assignment to marketing messages (*selection experiment*)
	2	Cognitive ability					✓		
	3	Over-confidence	✓				✓		
	4	Entrepreneurial self-assessment	✓				✓		
2.2.1 Economic outcomes (non-experimental)	1	Business creation	✓	✓		✓		General population (exposed to marketing campaign)	
	2	Business success	✓			✓			
	3	Capital and labor input	✓			✓			
	4	Economic self-sufficiency	✓			✓			
2.2.2 Business and personal input (non-experimental)	1	Business practices	✓			✓		General population (exposed to marketing campaign)	
	2	Financial professionalization	✓			✓			
	3	Marketing	✓			✓			
	4	Innovation	✓			✓			
	5	Networks	✓	✓	✓	✓			
	6	Entrepreneurial mindset	✓	✓	✓	✓			
	7	Owner's non-cognitive traits	✓	✓	✓	✓			
	8	Preferences	✓	✓	✓	✓			

Due to the current Covid-19 crisis, we were not be able to implement Wave III as planned in September 2020, but had to postpone it to the spring semester 2022. In the worst possible case, we may not be able to implement Wave III at all. The statistical power calculations in the appendix account for a worst-case scenario with only the two already implemented waves and a base-case scenario with all three planned waves. Originally, we planned to conduct both endline surveys as an in-person survey and all other surveys via telephone. Due to the ongoing Covid-19 crisis, we have to conduct the Endline survey I via telephone as well and hope to conduct the Endline survey II in-person.

Table 2. Timeline

	Stage/Instrument	Sample	Status	Date
Piloting		3 academies, 380 applicants	Completed	March-May 2018
Wave I		10 academies		
	Marketing / information sessions / short surveys	$n = 1019$	Completed	Aug.- Sept. 2019
	Application data / Baseline survey	$n_{app} = 713$, $n_{baseline} = 672$	Completed	Aug.- Sept. 2019
	Enterpreneurship academy	$n = 414$	Completed	Aug. 2019 - Jan. 2020
	Implementation check survey	$n = 625$	Completed	Jan. - Feb. 2020
	Midline survey	$n = 607$	Completed	Nov. - Dec. 2020
	Endline survey I	$n = 604$	Completed	Sept. - Oct. 2021
	Endline survey II		Planned	Jan. - Feb. 2023
Wave II		8 academies		
	Marketing / information sessions / short surveys	$n = 760$	Completed	Feb. - March 2020
	Application data / Baseline survey	$n_{app} = 584$, $n_{baseline} = 562$	Completed	Feb. - March 2020
	Enterpreneurship academy	$n = 313$	Completed	Feb. - July 2020
	Student screening survey	$n = 926$	Completed	Feb. - March 2020
	Student population survey I	$n = 489$	Completed	May- June 2020
	Implementation check survey	$n = 509$	Completed	July - Aug. 2020
	Midline survey	$n = 515$	Completed	June. - July. 2021
	Endline survey I		Planned	Mar.- Apr. 2022
	Endline survey II & Student population survey II		Planned	Jan. - Feb. 2023
Wave III		9 academies		
	Marketing / information sessions/ short surveys		Planned	Aug.- Sept. 2020
	Application data		Planned	Aug.- Sept. 2021
	Enterpreneurship academy		Planned	Aug. 2021 - Jan. 2022
	Student screening survey		Planned	Sep. - Oct. 2021
	Student population survey I		Planned	Oct. - Dec. 2021
	Implementation check survey		Planned	Jan. - Feb. 2022
	Midline survey		Planned	July - Aug. 2022
	Endline survey I&II		Planned	Jan. - Feb. 2023 & Jan. - Feb. 2024
	Student population survey II		Planned	Jan. - Feb. 2024

2.5. **Treatment assignment and statistical power.**

Selection experiment. Each information session presenter was provided with a randomly drawn marketing theme — financial independence or creative freedom — for the first session of the day. This was randomly chosen by the research team using a fair coin. The themes for the remaining sessions were then alternated by the presenter.

Entrepreneurship training experiment. The randomization procedure offered admission to the training program to individuals with complete applications. Within *each* training cohort (i.e., university-semester), the target was to offer admission to 40 students, an optimal classroom size determined by SHA.[6] We targeted a control group of equal size; however, the group sizes were constrained by the number of applications received. In Appendix C, we detail the randomization algorithm. Most importantly, it stratified along two dimension. First, we grouped students according to how many years they had studied their current degree. Second, we ascertained that the share of business students (students who study business, management, finance, marketing or related fields) is balanced between treatment and control *within* each year of study.

For the power calculations, we perform simulation. This allows us to incorporate myriad factors such as attrition, non-compliance, varying treatment and control group sizes. The simulations indicate that the design is sufficiently powerful (76 percent) to detect an effect of 20 percent (or 0.2 of a standard deviation) even in the worst-case scenario, in which we cannot implement the planned Wave III at all. This effect size seems to be a typically observed change (McKenzie and Woodruff, 2014). In the base-case scenario, our design would be well-powered to detect an effect size of 20 percent (89 percent power). If the effect size is actually only 15 percent of our standardized variable, the statistical power of our design reduces to 66 percent. Overall, our design is well-powered for the base-case scenario with three waves. The conservative, worst-case scenario still yields better power than previous studies despite being below the generally accepted appropriate target of 80 percent power (McKenzie and Woodruff, 2014).

3. Data

3.1. **Data collection and processing.** Measuring treatment effects at two levels and describing selection into entrepreneurship requires a multitude of surveys. Figure 1 details our data collection efforts, and to which subpopulation surveys are administered. We make all survey instruments available through attachments to the pre-registration in the AEA registry #4502.[7]

Selection into entrepreneurship. The highest level of self-section occurs when individuals select into being interested in entrepreneurship training and attend an information session (see top of the pyramid in Figure 1). From this subpopulation, we collect the following data during the information session: pen and paper based *short surveys* eliciting contact

[6]SHA allowed for deviations from the optimal size within a range of between 30 to 45 students. In case of excess (insufficient) interest, the classes were larger (smaller).

[7]To ascertain data integrity and safety, and to ensure survey respondents' privacy, we collect, manage and store data in the following way: First, the interview data is collected by experienced local enumerators. Prior to each data collection effort, PIs personally conduct extensive multi-day workshops with the enumerators. Data is collected using Kobo toolbox, and its Android-based mobile device app. Data is stored on secure drives provided by the University of Munich digital infrastructure. When data is collected using pen and paper, data is digitized also using Kobo toolbox in a timely manner and physical records are safely kept at the University of Munich to ensure privacy thereafter.

details, field of study, measures of cognitive ability using four Raven matrices, student's assessment of how many of these they believed they completed correctly and their assessment of their own entrepreneurial potential on a scale from one through ten.[8] To reach the non-interested subpopulation we track those classes where the training academy was advertised using short pitches. We classify all students of such a class as having been exposed to marketing. We return to the same classrooms a few weeks later and distribute *student population screening surveys*. These surveys mimic short surveys conducted during information sessions and also elicit students' awareness of entrepreneurship training programs. This allows us to identify students who were aware of the academy based on whether they have heard about our training program or about any entrepreneurship training program at their university.[9] The pool of students who are aware of a training program but did not apply constitutes the sampling frame for the *student population survey*. We then randomly sample 80 students per university, and survey them at two points in time. First, we conduct a phone survey mimicking the *baseline survey* conducted with academy applicants, which allows us to describe predictors of selection into entrepreneurship (*Student Population Survey I*). Second, we repeat this in *Student Population Survey II* to analyze how the subpopulation of non-interested students evolved over time relative to those who expressed in training but were not admitted—the control group. There is no experimental variation at either stage of this comparison.

Selection experiment. Attending information sessions is a necessary requirement for students to be able to apply to the training program since the exogenous variation of the marketing messages in the *selection experiment* is implemented in the information sessions. At the end of an information session, interested students can pick up a paper-based application form. Thus, application form data is only available for the subset of those interested in the training who actually submit a (complete) application form. Application forms contain contact details, demographic information, questions about motivations for and experience with entrepreneurship. We also include questions on students' expected future wage income, as well as expected earnings from entrepreneurship. With the experimental variation of the marketing messages we identify how selection into applying for entrepreneurship training varies with the stressed motives.

Entrepreneurship training experiment. To causally identify the effect of being offered entrepreneurship training, admission to the training program is offered on a random basis

[8] A short and standardized illustration on how Raven matrices work in general and how students ought to indicate their answers on the short surveys was provided.

[9] Most universities do not offer alternative entrepreneurship training programs. Thus, it is reasonable to assume that students who were aware of a general academy were aware of *our* academy despite being unable to exactly recall the name of the program.

among those who apply. We gather pre-treatment data by conducting a *baseline survey* prior to individuals being informed about their admissions decisions. After the entrepreneurship training academy, we conduct an *implementation check survey* (around one to two months after the academy ends) and a *midline survey* (around six month later) with the treatment and control groups. Finally, we carry out two *endline surveys*: Endline Survey I will be conducted 12 months after each cohort is finished with their training; Endline Survey II surveys the entire sample around two years after the last round of academies. This survey will be conducted simultaneously for all cohorts and allows us to look at how medium to long-term effects evolve.

While the baseline, implementation check and midline survey are conducted over the phone, the endline surveys will be conducted in person. As detailed below, the surveys elicit information on socio-economic characteristics and main outcome variables, such as prior and ongoing wage and self-employment, preferences measures (risk and time preferences, degree of loss aversion), and non-cognitive traits (Big-5, grit, aspirations and personal initiative). Financial compensation for participation in the endline surveys helps to minimize attrition.

3.2. **Key outcomes.** We use the collected data to construct outcome measures for our five families of hypotheses, as laid out in Section 2.3. To test hypotheses we follow the approach by Kling, Liebman, and Katz (2007) and aggregate variables into indices to test each main hypothesis (see Table 1) when possible. This reduces the number of tests conducted within each family. For instance, rather than testing for effects across ten business practices, we define an index using adherence to those ten practices and only conduct one hypothesis test. This hypothesis test in turn is part of a family of hypothesis tests. While we focus on indices of outcome measures here to address multiple hypothesis testing, we will also look at individual outcome variables during the analysis. We will clearly mark which results are accounting for multiple hypothesis testing and which are not.

Testing primary Hypothesis Families 1.1 and 2.1 will allow us to draw general conclusions about the entrepreneurship training experiment. Testing hypotheses within Hypothesis Family 1.2 is informative about the mechanisms through which the training program works. Hypotheses 2.2.1 and 2.2.2 set out to analyze dimensions which correlate with entrepreneurial aspirations and success by comparing applicants to the non-interested subpopulation.[10]

To create a summary index from several continuous variables we calculate the unweighted average of those variables' z-scores. Z-scores are constructed using the control

[10]We can compare the non-interested subpopulation to the full set of applicants using baseline data (pre-intervention). Using endline data, we compare the non-interested subpopulation to the control group (post-intervention).

group mean and dividing by the control group standard deviation. Thus, each component of the index has a mean of zero and a standard deviation of one for the control group. To create an index of a set of binary variables we calculate their mean; that is, the fraction of "successes" across all component variables. If required, variables that are used to construct an index are reversed so that meaning is consistent.[11] In Appendix A we describe which variables are used to construct the indices in Table 1. The pre-analysis plan details the construction of the specific indices.

Hypothesis Family 1.1 consists of four indices: i) business creation (extensive margin), ii) business success (revenue, profits), iii) labor (employees) and capital (assets, inventory) input, and iv), an index of economic self-sufficiency which aggregates earnings from self-employment, wage employment and other sources.

Hypotheses Family 1.2 consists of six primary indices: i) business practices (we draw on an abbreviated version of the 22-item questionnaire used in McKenzie and Woodruff (2016), and retain ten elements of the original questionnaire (see Appendix A), ii) financial professionalization (contains among others, knowledge and usage of financing instruments, indicators of business registration and licensing), iii) marketing practices, iv) capacity to innovate, v) business networks, and vi) development of an "entrepreneurial mindeset" (a composite index constructed from measures of personal initiative, aspirations and entrepreneurial future and self-efficacy (Frese et al., 2007; Campos et al., 2017; Bernard and Taffesse, 2014; Streicher et al., 2019)). For the last two hypothesis families, non-cogntive traits, such as the Big-5 personality traits or grit (Rammstedt and John, 2007; Duckworth and Quinn, 2009), and time and risk preferences, as well as one's degree of loss aversion (Falk et al., 2018; Fehr and Goette, 2007), we create indices where there is a natural grouping (e.g., risk and subjective risk preferences), and investigate sub-indices in other cases (e.g., Big-5 indices).

Hypotheses Family 2.1 is the essence of the selection study and consists of four hypotheses: i) the relative effectiveness of the two randomly chosen marketing messages in terms of attracting applications, ii) whether applicants differ in their cognitive ability (proxied by performance on Raven matrices), iii) whether applicants exhibit differences in over-confidence, and iv) whether applicants self-assess their entrepreneurial potential differently. We construct a measure of over-confidence by comparing individuals' observed and subjective (self-reported) performance on the Raven matrices (Åstebro et al., 2014; Moore and Healy, 2008).

There are two families of hypotheses which we use to study correlates of entrepreneurial aspirations and success in the wider population, Families 2.1.1 and 2.1.2. They mirror the

[11] For example, all variables used to create the "Innovation" index are arranged so that a larger number indicates more innovative.

hypotheses from Families 1.1 and 1.2 and therefore mimic the baseline and endline. These two families of hypotheses describe patterns through which students select into being interested in entrepreneurship training. We non-experimentally study baseline and endline differences between students who were interested in entrepreneurship training, and students who were not. First, the baseline comparison sheds light on how the subpopulation that applied to the training program differs from the general student population at large. Second, by comparing those that did not express interest (*non-interested subpopulation*) to interested students who were not offered admission to the training (control group) at endline, we can observe how those groups evolved over time.

3.3. **Randomization balance.** At this point, implementation of the intervention of the first two waves is completed. We have data available for all participants of the information session where we implemented the selection experiment using randomly chosen marketing messages across both waves. Additionally, we have collected baseline data and short-term follow-up data from applicants and non-applicants (see Figure 1).

In Table 3, we conduct balance checks using the baseline data and compare those individuals who were offered admission (treatment) to those who were not (control) in the *entrepreneurship training experiment*. Columns 1 and 3 report the *unconditional* means for the treatment and control group. In column 5, we regress the respective variable on a treatment indicator, controlling for training cohort fixed-effects, and report the estimated treatment effect (*regression-adjusted difference*). Using heteroskedasticity robust standard errors, we then conduct a two-sided t-test of whether the treatment effect is equal to zero, and report the p-value in column 6. Overall, Table 3 suggests that randomization was successful; from 49 tests we conduct, only one difference is statistically significant at the five percent level. Specifically, treatment subjects report higher time preference scores which we attribute to random sampling variation.

For the *selection experiment*, we only have the short-surveys of participants at the information sessions as baseline data. The elicited characteristics (gender, field and year of study) were balanced across both randomly assigned marketing themes (see also Table 6).

4. Analysis

We first discuss the empirical specifications for our *selection study* and the complementary observation of self-selection. Then we present the specifications for the *entrepreneurship training experiment* and the short-term results nine months after the intervention. In all specifications, we use OLS if the outcome measure is continuous. We report results from both logit and OLS regressions for binary outcomes, with the logit specification being our preferred. Details on data processing, corrections for multiple-hypothesis testing

Table 3. Balance in entrepreneurship training sample

	Treatment		Control		Reg. Adj.		
	Mean (1)	St. dev. (2)	Mean (3)	St. dev. (4)	Diff. (5)	p-value (6)	N (7)
General							
Profit marketing theme (d)	0.44	0.50	0.41	0.41	0.01	0.83	1215
Male student (d)	0.52	0.50	0.58	0.58	−0.02	0.48	1215
Employment							
Working for a wage during the semester (d)	0.10	0.30	0.07	0.07	0.02	0.21	1214
Employer is company (d)	0.04	0.20	0.03	0.03	0.01	0.33	1214
Compensation per month in UGX (ths)	39.37	184.51	35.40	35.40	6.35	0.65	1214
Hours per week working	3.71	13.88	2.42	2.42	1.01	0.19	1214
Business							
Ever owned a business (d)	0.28	0.45	0.25	0.25	0.01	0.73	1214
Currently owning a business (d)	0.22	0.41	0.19	0.19	0.01	0.62	1214
Founder/Co-founder of business (d)	0.26	0.44	0.24	0.24	0.00	0.98	1214
Number of partners in business [*]	0.38	1.82	0.29	0.29	0.10	0.39	547
Business officially registered (d) [*]	0.02	0.14	0.02	0.02	0.00	0.71	546
Business has local trading license (d) [*]	0.03	0.16	0.05	0.05	−0.02	0.13	536
Length of existence of business	0.57	1.74	0.53	0.53	0.04	0.74	1214
Length of work at business in months	0.56	1.72	0.48	0.48	0.07	0.53	1214
Number of full-time employees	0.71	11.25	0.40	0.40	0.48	0.43	1214
Number of part-time employees	0.20	1.20	0.20	0.20	−0.01	0.93	1214
Hours per week working at business	7.71	18.58	6.12	6.12	0.19	0.85	1209
Profit per month at business in UGX (ths)	174.32	689.43	147.19	147.19	31.29	0.43	1214
Number of additional businesses owned [*]	0.06	0.27	0.04	0.04	0.01	0.52	547
Networks							
Personal contacts for business advice	0.70	0.46	0.73	0.73	−0.01	0.71	1210
Number of contacts in family and friends	2.74	3.30	2.86	2.86	−0.02	0.91	1206
Number of contacts outside family and friends	0.80	1.83	1.03	1.03	−0.23	0.12	1210
Contacts can help discussing business ideas (d)	0.67	0.47	0.72	0.72	−0.02	0.51	1202
Contacts helped discussing business ideas in the past (d)	0.40	0.49	0.43	0.43	−0.02	0.42	1202
Contacts can help collecting payments (d)	0.38	0.49	0.41	0.41	−0.02	0.48	1142
Contacts helped collecting payments in the past (d)	0.12	0.32	0.13	0.13	−0.01	0.55	1142
Contacts can help with sharing tools, inputs, employees (d)	0.37	0.48	0.39	0.39	−0.03	0.37	1126
Contacts helped with sharing tools, inputs, employees in the past (d)	0.12	0.33	0.13	0.13	−0.00	0.82	1126
Contacts can help with purchasing inputs, stocks (d)	0.36	0.48	0.38	0.38	−0.02	0.51	1129
Contacts helped with purchasing inputs, stocks in the past (d)	0.13	0.33	0.12	0.12	0.00	0.84	1129
Funding							
Ever took loan to fund business idea (d)	0.08	0.28	0.08	0.08	0.00	0.97	1213
Number of known funding initiatives (out of 7)	1.38	1.16	1.41	1.41	0.01	0.88	1172
Non-Cognitive							
Big-5: extraversion	0.88	1.44	0.84	0.84	0.13	0.13	1210
Big-5: agreeableness	1.56	1.27	1.44	1.44	0.03	0.75	1212
Big-5: conscientiousness	1.99	1.22	1.90	1.90	0.00	0.95	1211
Big-5: neuroticism	−1.15	1.35	−1.13	−1.13	−0.07	0.40	1212
Big-5: openness	7.55	1.27	7.50	7.50	0.04	0.56	1213
Grit score (1-5)	3.57	0.44	3.58	3.58	−0.03	0.32	1203
Personal initiative score (1-5)	4.02	0.41	4.01	4.01	−0.00	0.99	1208
Stress score (0-16)	6.20	2.25	6.02	6.02	0.11	0.42	1200
Preferences							
Risk preference: scale (1-5)	4.07	0.79	4.06	4.06	−0.03	0.54	1212
Risk preference: final number (1-32)	15.53	11.79	16.09	16.09	0.46	0.52	1213
Loss aversion: Final number (0-6)	4.68	2.02	4.57	4.57	0.16	0.20	1213
Time preference: scale (1-5)	4.03	0.90	3.96	3.96	0.06	0.28	1212
Time preference: final number (1-32)	11.58	12.36	9.79	9.79	1.42	0.05	1213
Entrepreneurial Self-Assessment							
Confidence in ability to start own company (1-5)	4.24	0.66	4.24	4.24	−0.03	0.46	1213
Confidence in ability to pursue self-employed career (1-5)	4.30	0.59	4.22	4.22	0.05	0.16	1213
Confidence in ability to manage challenges of an entrepreneur (1-5)	4.17	0.61	4.17	4.17	−0.03	0.39	1213
Confidence in ability to work in own business one year from now (1-5)	3.90	0.90	3.86	3.86	−0.00	0.96	1200

Notes. Columns 1 and 3 report the unconditional mean, columns 2 and 4 the standard deviation for the treatment, who was randomly offered admission to the training program, and control group, respectively. Column 5 reports the regression adjusted mean $\hat{\beta_1}$ estimated using $y_{i,u} = \beta_0 + \beta_1 treat_{i,u} + \alpha_u + \varepsilon_{i,u}$ where α_u is training-cohort fixed effect. Column 6 displays the p-value from a two-sided t-test of $\mathtt{H}_0 : \beta_1 = 0$ using heteroskedasticity-robust standard errors. The last column shows the number of non-missing observations. (d) denotes an indicator variable. Variables marked with a [*] are those that were only measured in the second wave.

and adjustments for reporting errors are presented in Appendix D.1. Please note that the results in this draft do not yet include any correction for multiple hypotheses testing.

4.1. **Selection into entrepreneurship.** We describe and analyze selection at two steps before being (randomly) offered admission to the training program (see Figure 1). While we implemented the *selection experiment* in both cohorts, data collection on the non-interested subpopulation was only after the successful implementation of the first cohort. Hence, the results on non-experimental selection into the treatment in Section 4.1.1 only refer to the second cohort.

4.1.1. *Non-experimentally describing selection.* On the highest level, we document selection into entrepreneurship by comparing those who were informed about the training program but did not attend an information session (non-interested subpopulation), to those who applied to the training program using baseline data. In the future, we will additionally document trends in how the non-interested subpopulation evolves over time relative to the subpopulation that expressed interest in the training. We do so by comparing them to those who applied but were not admitted—the control group—using Endline I ($r = 2$) data. Both comparisons are based on estimating the following specification.

$$\mathtt{y}_{i,u,r} = \beta_0 + \beta_1 \mathtt{applied}_{i,u} + \alpha_u + \varepsilon_{i,u,r}. \tag{1}$$

$\mathtt{applied}_{i,u}$ is an indicator equal to one if an individual applied to the training, and zero otherwise. There is no experimental variation at this stage and therefore $\widehat{\beta_1}$ does not measure a causal effect, but is merely informative of a correlation. We calculate heteroskedasticty-robust Eicker-Huber-White standard errors. For the baseline, we currently show results for the individual index components. In the future, we will additionally calculate indices as for the *entrepreneurship training experiment* and examine them at the baseline ($r = 0$), and again at the Endline I ($r = 2$). We present the results for the individual components at baseline in Table 4.

Both groups are equally well off in terms of financial outcomes. Non-interested students and applicants have similar rates of business start-up, make comparable monthly profits, and run businesses of an analogous size with a similar degree of formalization (see Table 4, 1.1.2-1.2.2). Also their total monthly earnings through self-employment and wage employment are closely aligned (Table 4, 1.1.4). Yet, applicants and non-interest students differ on non-cognitive traits and soft skills.

Selection is consistent with the training being a substitute for other sources of entrepreneurial knowledge. First, students in the control group have fewer contacts to whom they could turn for business advice compared to the non-interested population

(Table 4, 1.2.5). Non-interested students are more likely to state that they have an information source they can contact for business advice (81.5 vs. 70.2 percent; $p = 0.07$, reg-adjusted t-test) and report to have more information sources (5.9 vs. 4.0 contacts; $p = 0.01$, reg-adjusted t-test). Second, interested students have a lower confidence to manage entrepreneurial challenges (4.34 vs. 4.20 on a five-point Likert scale; $p = 0.01$, reg-adjusted t-test). Both observations are consistent with the training being a substitute for other knowledge sources. At the same time, interested students have a higher confidence in their ability to work in their own business in one year (3.64 vs. 3.91 on a five-point Likert scale; $p < 0.01$, reg-adjusted t-test). Students thus actually expect to benefit from the training when applying.

We also document some differences along demographics, traits and economic preferences. Applicants are more likely to be male than the non-interested population ($p = 0.01$). For the Big-5, we observe that non-interested students score lower on conscientiousness ($p < 0.01$) and openness ($p = 0.09$), while they score higher on neuroticism ($p = 0.05$). The control group also seems to display less grit than the non-interested students ($p = 0.01$). For economic preferences, we observe the control group to be more loss averse ($p = 0.04$) and marginally more patient ($p = 0.09$).

4.1.2. *Selection experiment.* In the *selection experiment*, we study selection into applying for the training conditional on attendance at an information session. Random assignment to marketing messages during information sessions provides us with orthogonal variation which we exploit to study selection into applying for the training program along two salient motivations.[12] Specifically, we use the following specification to analyze the differential effect of exposure to a specific marketing messages on a student's propensity to apply (Hypothesis 1 of Hypothesis Family 2.1):

$$\texttt{applied}_{i,u} = \beta_0 + \beta_1 \texttt{treat\_profit}_{i,u} + \alpha_u + W'_{i,u}\delta + \varepsilon_{i,u}. \tag{2}$$

Here, `applied` is an indicator equal to one if an individual submits an application for the training program, and zero otherwise; `treat_profit` is an indicator equal to one if an individual participated in an information session randomly emphasizing *financial independence*, and equal to zero if the theme was *creative freedom*. The vector $W_{i,u}$ is included to increase the precision of estimates and -depending on the specification- contains an individual's gender, indicators for years in the current degree, academy fixed effects

[12]As all students participating in a given information session are exposed either to the *creative freedom* or the *financial independence* marketing message, the standard errors should be clustered at the session level (Abadie et al., 2020); the level at which treatment varies. However, due to administrative issues, for some individuals we are unable to observe the exact session an individual attended and cannot cluster at this appropriate level. We attempt to overcome this by conservatively clustering at the training cohort level which is the next highest level.

Table 4. Baseline comparison: General population and (control group) applicants

		Non-Interest.		Control		Reg Adjust	
		Mean (1)	St. Dev. (2)	Mean (3)	St. Dev. (4)	Diff (5)	p-value (6)
General	Male student (d)	0.43	0.50	0.54	0.50	-0.12	0.01
1.1.1 Creation	Currently owning a business (d)	0.15	0.35	0.17	0.38	-0.02	0.60
	Hours per week working at business	6.95	17.68	5.72	15.39	0.97	0.55
1.1.2. Success	Profit per month at business in UGX	110.75	480.94	161.16	691.20	-49.60	0.44
1.1.3. Inputs	Number of full-time employees	0.14	0.54	0.27	1.44	-0.09	0.18
	Number of part-time employees	2.28	46.18	0.23	1.08	-0.02	0.85
	Number of partners in business	0.20	0.82	0.28	0.84	-0.07	0.34
1.1.4. Self-sufficiency	Earnings from self-employment per month	110.75	480.94	161.16	691.20	-49.60	0.44
	Earnings from wage employment per month	30.69	135.56	39.37	341.59	-24.23	0.42
1.2.2. Formalization	Ever took a loan to fund business idea (d)	0.05	0.22	0.06	0.24	0.00	0.89
	Known funding initiatives (out of 7)	1.35	1.24	1.24	1.03	0.16	0.14
	Business officially registered (d)	0.01	0.12	0.02	0.13	0.00	0.76
	Business has local trading license (d)	0.02	0.15	0.05	0.21	-0.02	0.37
1.2.5. Networks	Personal contacts for business advice	0.82	0.39	0.70	0.46	0.08	0.07
	Number of contacts in family and friends	4.80	4.54	3.18	3.52	1.29	0.00
	Number of contacts outside family and friends	5.39	65.43	0.86	2.69	4.16	0.15
1.2.6. Mindset	Personal initiative score (1-5)	4.09	0.36	4.01	0.40	0.05	0.12
	Confidence to start own company (1-5)	4.30	0.61	4.22	0.61	0.07	0.21
	Conf. to pursue self-employed career (1-5)	4.30	0.60	4.24	0.62	0.06	0.26
	Conf. to manage entrepreneurial challenges	4.34	0.58	4.20	0.51	0.14	0.01
	Conf. to work in own business in one year	3.64	1.12	3.91	0.83	-0.28	0.00
1.2.7 Non-Cogn. traits	Big-5: extraversion	0.86	1.37	0.70	1.31	0.19	0.14
	Big-5: agreeableness	1.72	1.37	1.38	1.35	0.20	0.1
	Big-5: conscientiousness	2.47	1.13	1.93	1.19	0.55	0.00
	Big-5: neuroticism	-1.51	1.24	-1.15	1.43	-0.27	0.05
	Big-5: openness	7.86	1.09	7.50	1.23	0.19	0.09
	Grit Score (1-5)	3.76	0.45	3.63	0.40	0.10	0.01
1.2.8 Preferences	Risk preference: scale (1-5)	4.04	0.80	4.04	0.70	0.02	0.81
	Risk preference: final number (1-32)	17.24	11.81	16.57	12.18	0.84	0.46
	Loss aversion: final number (0-6)	3.93	2.07	4.53	2.12	-0.42	0.04
	Time preference: scale (1-5)	3.97	0.76	3.85	0.89	0.07	0.40
	Time preference: final number (1-32)	7.65	11.00	9.40	11.94	-1.93	0.09

Notes. Columns 1 and 3 report the unconditional mean, columns 2 and 4 the standard deviation for the non-interested students and the applicants in the control group, respectively. Column 5 reports the regression adjusted mean $\hat{\beta}_1$ estimated using $y_{i,u} = \beta_0 + \beta_1 treat_{i,u} + \alpha_u + \varepsilon_{i,u}$ where α_u a university fixed effect. Column 6 displays the p-value from a two-sided t-test of $\mathtt{H}_0 : \beta_1 = 0$ using heteroskedasticity-robust standard errors. The table shows results for the second cohort (implemented in spring 2020). The number of observations for the control group is 246, for the non-interested population 468.

as well as survey controls. The latter contain elicited information in the information session, namely the number of correctly solved Raven's Matrices, individuals' beliefs about their performance, as well as their entrepreneurial self-assessment and the version of the student screening survey.

Table 5 consistently shows a higher appeal of the *creative freedom* theme. We estimate Equation (4) with four different specifications, Column 2 being our pre-registered specification. In this specification, we estimate the propensity to apply to the training to be around 5.3 percentage points lower under the profit theme. The application rate under the *creative freedom* theme is around 76 percent. Effect sizes are similar when removing individual's gender as well as study year fixed effects in Column (1). The same holds true when adding adding university fixed effects (Column 3) and additional survey controls (Column 4). We corroborate the results from the logit regression with a binary OLS regression in Table A.1.

Table 5. Marketing theme and propensity to apply

	1[Student applied to the training]			
	(1)	(2)	(3)	(4)
Profit theme = 1	-.053**	-.053**	-.060***	-.054***
	[-.105,-.000]	[-.105,-.002]	[-.096,-.023]	[-.091,-.017]
Male = 1		-.007	-.001	-.002
		[-.073,.059]	[-.061,.059]	[-.060,.055]
Freedom app. rate	.76	.76	.74	.74
Study year FE	No	Yes	Yes	Yes
Academy FE	No	No	Yes	Yes
Survey controls	No	No	No	Yes
Pseudo R-sq	.0031	.005	.065	.071
# of participants	1367	1367	1271	1271

Notes. *** p <0.01, **p <0.05, * p <0.1.
This tables shows marginal effects at the mean from logit regressions where the dependent variable is an indicator for whether someone did submit an application (which is conditional on having been at an info session). 95% confidence intervals based on clustered standard errors are at the academy level in square brackets. Profit theme is an indicator equal to one if the student was exposed to the *financial independence* theme in the information session.

Hypotheses 2 through 4 of Family 2.1.1 capture the idea that selection patterns may differ relative to the underlying motivation for entrepreneurship. Denote a dimension of hypothesized heterogeneity in selection (cognitive ability, over-confidence, and entrepreneurial self-assessment, see Hypotheses 2 through 4 of Family 2.1.1 in Table 1) with Z_i. We then estimate the following specification to test for different selection patterns. Conclusions about differential selection are based on assessing whether the estimated coefficients of our heterogeneity analyses are statistically significantly different from zero ($\mathtt{H}_0 : \gamma = 0$).

$$(3)\quad \mathtt{applied}_{i,t} = \beta_0 + \beta_1 \mathtt{treat\_profit}_{i,u} + \beta_2 Z_{i,t} + \gamma Z_{i,t} * \mathtt{treat\_profit}_{i,u} + \alpha_u + W_{i,u}\delta + \varepsilon_{i,t}.$$

To analyze differential selection, we rely on the data from the student screening survey that every participant of the information session filled out. This survey was administered before students decided whether to apply or not, but after having been exposed to one of the two marketing themes. Therefore, students' responses may have been affected by the treatment. This does not seem to be the case for the number of correctly solved Raven's Matrices, nor for the fraction of overconfident students (see Table 6). However, there is a sizable and significant difference for entrepreneurial self-assessment. Students judge their entrepreneurial ability to be higher after being exposed to the *creative freedom* theme.

Table 6. Balance: Info Sessions

	Creative Freedom		Profit			
	Mean	SD	Mean	SD	Difference	p-value
Num. of correct Raven's	1.85	0.99	1.75	0.94	0.056	0.23
Overconfidence = 1	0.68	0.47	0.69	0.46	0.001	0.98
Entrepreneurial self-assessment	7.64	3.21	6.97	3.50	0.49	0.004
Business student = 1	0.55	0.50	0.58	0.49	-0.013	0.60
Male = 1	0.57	0.50	0.54	0.50	0.032	0.19

Notes. *** p <0.01, ***p* <0.05, * p <0.1.
This tables shows summary statistics for the dimensions in the heterogeneity analysis of Table 7. p-values are based on two-sided t-tests. The number of correctly solved Raven's matrices is between zero and four. Overconfidence is an indicator that equals one when the student estimates his or her own performance to be better than it objectively is. Entrepreneurial self-assessment refers to the self-reported ability to be a successful entrepreneur on a scale from 1 to 10. Business student is an indicator that equals one if the student stidues business, management, finance, marketing, or a related degree.

Table 7 reports evidence for differential application patterns in terms of the information session participants' attributes. Our pre-registered specifications in Columns 1-3 control for the participants' gender as well as study year fixed effects. Column 1 provides evidence that when entrepreneurship is framed in terms of creativity, more cognitively able students select in. Our proxy for cognitive ability, the number of correctly solved Raven's Matrices, is positively correlated with the decision to apply in the *creative freedom* theme. In the profit theme, it is not. Closely linked, we also document differences based on students' accuracy in predicting the number of Raven's Matrices solved. Overconfident students (i.e., students who judge their performance better than it objectively is) are around 18.2 percentage points more likely to select into applying in the profit theme. For entrepreneurial self-assessment, we find a small but significant point estimate, suggesting very weak differential selection across treatments -keeping in mind that entrepreneurial self-efficacy was directly affected by the marketing theme (see Table 6). The results on differential selection for cognitive ability and overconfidence hold true in the OLS specification and when including academy fixed effects (see Tables A.2 and A.3).

In an exploratory analysis, we assess whether marketing messages appeal differently to male and female students (see Table 7, Column 4). Emphasizing the freedom to choose how to work may be particularly attractive to women. On the one hand, *creative freedom* may highlight the possibility to balance work and family responsibilities. On the other hand, females may be more able to afford to pursue these motives because they are not the main breadwinners of the household. The positive appeal of the *creative freedom* theme seems to be somewhat, although not significantly, higher for female students. We proceed to analyze whether applicants display differential characteristics depending on the marketing theme they were exposed to. To analyze differences within the group of applicants, we rely on information from the application forms.

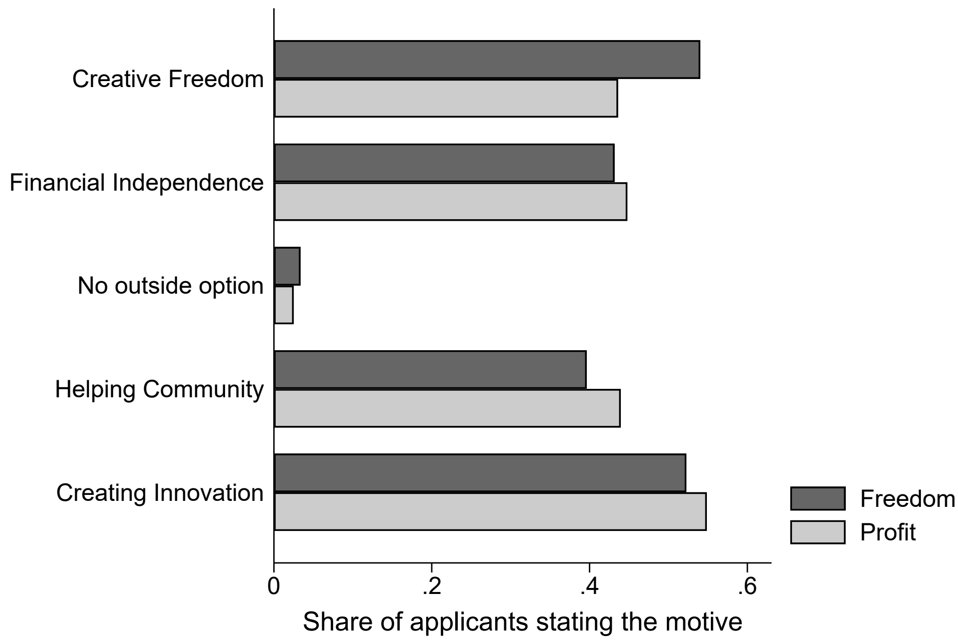
Table 7. Marketing themes and heterogeneity in the propensity to apply

	1[Student applied to the training]			
	(1)	(2)	(3)	(4)
Profit theme X Num. of correct Raven's	-.069***			
	[-.113,-.026]			
Num. of correct Raven's	.055***			
	[.025,.085]			
Profit theme X Overconfidence		.182***		
		[.091,.272]		
Overconfidence = 1		-.097***		
		[-.154,-.040]		
Profit theme X Entrepreneurial self-assessment			.003**	
			[.001,.005]	
Entrepreneurial self-assessment			.002	
			[-.008,.011]	
Profit theme X Male				.058
				[-.044,.160]
Male = 1				-.035
				[-.123,.052]
Profit theme = 1	.070*	-.179***	-.046*	-.071
	[-.009,.149]	[-.232,-.125]	[-.096,.004]	[-.163,.022]
Freedom mkt app rate	.76	.76	.76	.76
Academy FE	No	No	No	No
Gender	Yes	Yes	Yes	Yes
Study year FE	Yes	Yes	Yes	Yes
Pseudo R-sq	.012	.013	.0098	.0054
# of participants	1366	1366	1366	1366

Notes. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.
This tables shows marginal effects at the mean from logit regressions where the dependent variable is an indicator for whether someone did submit an application (which is conditional on having been at an info session). 95% confidence intervals based on clustered standard errors are at the academy level in square brackets. The number of correctly solved Raven's matrices is between zero and four. Overconfidence is an indicator that equals one when the student estimates his or her own performance to be better than it objectively is. Entrepreneurial self-assessment refers to the self-reported ability to be a successful entrepreneur on a scale from 1 to 10.

Applicants who were exposed to the *creative freedom* theme are more likely to cite this freedom as a primary reason for becoming an entrepreneur in their application (compared to applicants who were exposed to the *financial independence* theme). The application form (filled out after having been exposed to the different themes) asked students to indicate their two main reasons for wanting to become an entrepreneur. The answer choices related to 1) creative freedom, 2) financial independence, 3) lack of employment opportunities, 4) helping the community, and 5) creating innovation. Figure 3 displays the shares of applicants who stated the respective motive. Applicants who saw the *creative freedom* theme are significantly more likely to state *creative freedom* as a motive (53.8 percent vs. 44.52 percent, $p = 0.004$; two-sided proportion test).[13] At the same time, applicants who were exposed to the freedom theme are no less likely to state financial reasons as a motive (44.2 percent vs. 44.1 percent; $p = 0.952$; two-sided proportion test). For the other motives, differences are not significant.

Figure 3. Applicants' stated motives for entrepreneurship



Notes. The graph shows the share of applicants stating the respective motive for becoming an entrepreneur. Each applicant could state up to two motives. The graph is split between applicants which were exposed to the *creative freedom* theme, and applicants who saw the profit theme.

At the same time, profit expectations through entrepreneurship are higher for applicants who have seen the profit theme. Winsorizing expected profits at the 99^{th} percentile to reduce the weight from extreme outliers, applicants who have seen the profit theme expect around 42 percent higher monthly profits when being an entrepreneur. This compares to a value of approximately USD 1177 per month for applicants who have seen the creative profit theme. Table A.4 in Appendix B shows that this difference is significant in different specifications both with and without controls and with and without academy fixed effects. The results on stated motives and profit expectations are both consistent with

[13]Non-reported logit regressions with the binary outcome of whether the applicant states independence as a motive and standard errors clustered at the academy level confirms this finding. The result is robust to (e.g.) including gender, study year fixed effects as well as academy fixed effects.

the hypothesis that students apply at different rates based on their underlying beliefs about the returns to entrepreneurship, and that the treatment changes applicants' beliefs.[14]

The data on short-term outcomes of the *entrepreneurship training experiment* allow us to understand how differential selection by marketing themes is related to training success. On the one hand, highly confident students with elevated profit expectations may perceive the returns from education to be higher, be more motivated and the training in turn be more successful. On the other hand, the actual returns to education for these students who applied after seeing the *financial independence* theme may be lower due to negative selection on cognitive ability. In addition, having overly optimistic profit expectations and being confronted with the entrepreneurial reality through the training may lead to frustration and lower training success (McKenzie, Mohpal, and Yang, 2021). We address this empirical question in Section 4.2.

4.2. **Entrepreneurship training experiment.** In the *entrepreneurship training experiment*, we identify the Intention-to-Treat (ITT) effect of being offered admission to the entrepreneurship training. We separately estimate the coefficient of interest $\beta_{1,r}$ for medium-term ($r = 2$, Endline I) and long-term effects ($r = 3$, Endline II) according to Equation (4):

$$y_{i,u,r} = \beta_{0,r} + \beta_{1,r}\texttt{treat}_{i,u} + \alpha_u + \texttt{strata}_{i,u} + \varepsilon_{i,u,r} \tag{4}$$

where $y_{i,u,r}$ is outcome (measured by an index) for individual i, training cohort $u \in \{1, 2, 3\}$, and survey round r. The indicator variable $\texttt{treat}_{i,u}$ is equal to one if individual (applicant) i in training cohort u was randomly offered admission, and zero otherwise. Since randomization of admission offers was stratified by field of study and year of study, we include an indicator variable for every combination of the two variables.[15] Since the probability of being assigned to treatment differs across training cohorts, and is a function of the number of applicants, we include a training cohort fixed effect α_u.

Equation (4) is our preferred specification, and results from it will be reported first in the analysis. Put differently, estimates of β_1 from Equation (4) will be used to address the questions and hypotheses posed earlier. The following specifications are intended to provide more precise estimates in order to help us better gauge the magnitude of the estimated effects.

To improve the precision of $\widehat{\beta_1}$ we run a second set of specifications which includes a set of pre-treatment predictors. We follow the recommendation in Duflo et al. (2020) and

[14] In both treatments, beliefs about the returns to entrepreneurship are very optimistic. The average non-admitted applicant to the training who ran a business nine months after the training earned about USD 237 per month.

[15] This results in five indicators included in the regressions, with one reference category omitted. These randomization cells refer to every combination of field of study (business and non-business) and year of study (first, second, and third).

use a variable selection approach. The double post-lasso estimation proposed by Belloni, Chernozhukov, and Hansen (2014) selects a low-dimensional set of predictors which are then included in the estimation. The method uses two separate Lasso regressions; one model to predict treatment assignment, another model to predict the outcome, and each model returns a set of variables to be included. Denote the union of this (as of now unknown) set of covariates by $X_{i,u,r=0}$. We further include the baseline value of the dependent variable $y_{i,u,r=0}$ whenever available.

$$y_{i,u,r} = \beta_0 + \beta_{1,r}\texttt{treat}_{i,u} + \beta_2 y_{i,u,r=0} + X'_{i,u,r=0}\gamma + \texttt{strata}_{i,u} + \alpha_u + \varepsilon_{i,u,r} \tag{5}$$

McKenzie (2012) discusses the benefits of a design that uses several post-treatment surveys to obtain more precise treatment effect estimates. Variables central to the analysis, such as profits and revenues, are likely to exhibit little auto-correlation. In this setting, statistical power in ANCOVA specifications is increased by pooling post-treatment observations. Section 3 describes that we conduct one midline follow up in addition to two endline surveys, resulting in three ($r \in \{1, 2, 3\}$) post-treatment surveys. Pooling those rounds, we estimate

$$y_{i,u,r} = \delta_r + \beta_1\texttt{treat}_{i,u} + \beta_2 y_{i,u,r=0} + \alpha_u + \varepsilon_{i,u,r} \tag{6}$$

where δ_r is a survey round fixed effect, and $r = 0$ indexes the baseline.

Effect heterogeneity. We are interested in analyzing heterogeneity in the ITT-effects along four independent, preregistered dimensions. First, we explore whether effects differ by an individual's field of study. Students in a business-related degree may have a higher ex ante likelihood of starting (successful) businesses due to higher entrepreneurial intentions or a different skill set (e.g., Solesvik, 2013; Bae et al., 2014). Second, we test whether effects differ by an individual's year in their degree. Students closer to graduation are more likely to move into (self-)employment in the near future. Third, we assess whether effects are different for students who report having sufficient financial means at baseline. Individuals who already possess the required funds may stand to benefit in a more immediate way (McKenzie and Woodruff, 2014). Fourth, we analyze differential effects by gender (Shinnar, Hsu, and Powell, 2014). Additional exploratory heterogeneity analyses (e.g., along self-reported motives and randomly assigned marketing themes, economic preferences or personality traits) will be clearly indicated as such.

Inference. Inference about the estimates in Equations (4) and (5) will be based on conventional heteroskedastic-robust Eicker-Huber-White standard errors. In case of Equation (6) standard errors will be clustered at the individual level since we use up to three observations per individual and randomization Randomization of admission offers occurs at the individual level.

Analysis of short-term effects. The data collection for our main specifications (Equations (4)-(6)) is currently ongoing. At the moment, we can report short-term results from our follow-up survey nine months after the training (Table 8).

The training led to significantly higher business activities nine months after the training. The index *business creation* captures both whether applicants are running business (extensive margin) and their input in terms of hours worked into the business (intensive margin). Admitted participant are 7 percentage points more likely to own a business after 9 months, corresponding to 20.7 percent increase evaluated at the control group mean of 33.8 percent. The average training participant contributes about 2.7 hours more to a business of his or her own than do non-admitted applicants ($mean_{control}$ = 13.88 hours). In Table A.5, we show that these results are robust to winsorization and inverse hyperbolic sine transformation (IHTS) of the working hours. The short-term results suggest that training participants are using more capital and labor in their businesses without this translating into substantially higher current sales and profits. Our main specification indicates some positive results on overall monthly earnings when adding up profits from business activities, wage employment, and other sources of income. While the point estimate in Table 8 corresponds to a substantial increase in earnings of 27.2 percent compared to the control group mean of about 180 USD, these results are not robust to winsorization and IHTS transformation (Table A.5). We display the results for the individual components of our primary outcome indices in Table A.6.

Our secondary outcome variables suggest that the training was successful in instilling certain aspects of an entrepreneurial mindset (see Table 9). Admitted students report themselves to be more competitive (Buser, Niederle, and Oosterbeek, 2021), show higher patience (Falk et al., 2018) compared to non-admitted applicants, and report themselves to be more proactive (Frese et al., 2007; Campos et al., 2017) nine months after the training.[16] In addition, training participants are more successful in establishing networks. Looking at the individual components of the index for networks (Table 9, Column 4), we find that admitted students have more business contacts outside their family network to ask for advice (by about 9.9 percent) and are more likely to ask these contacts for advice (by about 7.2 percent). Hence, the training does act as a substitute for a lack of networks as suggested by the non-experimental selection patterns into the training (see Section 4.1.1).

[16]While we do not observe overall positive effects on the "Entrepreneurial Mindset" index, the training shows significant effects on the "personal initiative" component of the index.

Table 8. ITT Effects of Entrepreneurship Training (+ 9 months): Primary outcome indices

Outcomes	Business Creation	Business Success	Capital & Labor Input	Economic Self-sufficiency
	(1)	(2)	(3)	(4)
Admission to Training = 1	0.13**	0.04	0.14*	0.10*
	(0.06)	(0.06)	(0.07)	(0.06)
Business student = 1	0.02	0.09	0.06	0.06
	(0.06)	(0.06)	(0.07)	(0.07)
2^{nd} year student = 1	0.02	-0.12	-0.12	-0.07
	(0.07)	(0.07)	(0.09)	(0.07)
3^{rd}+ year student = 1	-0.13*	-0.11	-0.09	0.11
	(0.07)	(0.07)	(0.08)	(0.07)
2^{nd} Cohort, spring 2020 = 1	-0.00	0.06	-0.12	0.06
	(0.06)	(0.06)	(0.07)	(0.06)
Strata	1,093	1,086	1,093	1,095
Cohort	1,093	1,086	1,093	1,095
Observations	1,093	1,086	1,093	1,095
R-squared	0.01	0.01	0.01	0.01

Notes. OLS regression *** p <0.01, **p <0.05, * p <0.1. Robust standard errors in parentheses. Admission to the training is an indicator equal to one if the student was randomly admitted to the training, conditional on applying. Business student is an indicator that equals one if the student stidues business, management, finance, marketing, or a related degree.

Table 9. TT Effects of Entrepreneurship Training (+ 9 months): Secondary outcome indices

Outcomes	Business Practices	Financial Profess.	Non-cogn. Traits	Entrepr. Mindset	Risk Preferences	Time Preferences	Loss Aversion	Competitive	Networks	Innovation	Marketing
	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Admission to Training = 1	0.08*	0.01	-0.03	0.04	0.07	0.08*	-0.01	0.21***	0.08*	0.07	0.04
	(0.04)	(0.03)	(0.05)	(0.04)	(0.05)	(0.05)	(0.06)	(0.06)	(0.04)	(0.05)	(0.06)
Business student = 1	-0.03	-0.02	-0.06	-0.03	-0.05	0.06	-0.01	-0.10	-0.01	-0.03	-0.08
	(0.04)	(0.03)	(0.05)	(0.04)	(0.05)	(0.05)	(0.06)	(0.06)	(0.04)	(0.05)	(0.06)
2^{nd} year student = 1	-0.01	-0.00	-0.10*	-0.02	-0.01	-0.01	0.01	0.01	-0.01	0.10	0.10
	(0.05)	(0.03)	(0.06)	(0.05)	(0.06)	(0.06)	(0.07)	(0.07)	(0.05)	(0.07)	(0.07)
3^{rd}+ year student = 1	-0.02	-0.02	-0.10	0.05	-0.02	0.04	-0.01	0.20***	-0.07	-0.09	-0.01
	(0.05)	(0.03)	(0.06)	(0.04)	(0.06)	(0.06)	(0.07)	(0.07)	(0.05)	(0.06)	(0.07)
2^{nd} Cohort, spring 2020 = 1	0.02	-0.02	0.03	-0.08**	-0.07	-0.04	-0.02	-0.05	-0.05	0.01	0.07
	(0.04)	(0.03)	(0.05)	(0.04)	(0.05)	(0.05)	(0.06)	(0.06)	(0.04)	(0.05)	(0.06)
Observations	1,093	1,156	1,095	1,095	1,095	1,095	1,095	1,092	1,095	1,093	1,156
R-squared	0.00	0.00	0.01	0.01	0.01	0.01	0.00	0.02	0.01	0.01	0.01

Notes. OLS regression *** p <0.01, **p <0.05, * p <0.1. Robust standard errors in parentheses. The outcome variables are constructed as outlined in Section A of the Appendix. We refrain from constructing one index for economic preferences and show indices for risk preferences (risk preferences and subjective risk prefereces), time preferences (time preferences and subjective time preferences), and individual results for our measures of loss aversion competitiveness. Admission to the training is an indicator equal to one if the student was randomly admitted to the training, conditional on applying. Business student is an indicator that equals one if the student stidues business, management, finance, marketing, or a related degree.

Our results also give first insights into how differential selection and different expectations about the returns to entrepreneurship influence the training success. The positive effect on business creation seems to be mainly driven by participants of the profit sessions. The likelihood to run an own business nine months after the training is 13.5 percentage points higher for admitted applicants who saw the *financial independence* theme than the non-admitted applicants watching the same theme (29.3 percent in control vs. 42.8 percent in treatment). For participants of the *creative freedom* sessions, start-up rates are very similar, regardless of whether they received the training or not (36.7 percent in control vs. 38.9 percent in treatment). At the same time, lower cognitive ability and a higher rate of over-confidence did not lead to worse outcomes in terms of success or income for admitted participants exposed to *financial independence* compared to those training participants who saw the *creative freedom* theme.

Table 10. ITT Effects, Interaction with Marketing Treatment: Primary outcomes (9 months after training)

Outcomes	Business Creation	Business Success	Capital & Labor Input	Economic Self-sufficiency
	(1)	(2)	(3)	(4)
Profit theme = 1	-0.10	0.09	-0.04	0.06
	(0.09)	(0.10)	(0.05)	(0.07)
Admission to Training = 1	0.04	0.03	0.22**	0.10*
	(0.08)	(0.06)	(0.10)	(0.06)
Admission X Profit theme	0.21*	0.02	-0.18	0.01
	(0.12)	(0.14)	(0.12)	(0.12)
Business student =1	0.02	0.09*	0.06	0.05
	(0.06)	(0.06)	(0.07)	(0.06)
2^{nd} year student = 1	0.03	-0.11	-0.12	-0.07
	(0.07)	(0.08)	(0.08)	(0.07)
3^{rd}+ year student = 1	-0.14**	-0.11	-0.09	0.11
	(0.07)	(0.07)	(0.09)	(0.07)
2^{nd} Cohort, spring 2020 = 1	0.00	0.07	-0.14*	0.07
	(0.06)	(0.07)	(0.07)	(0.06)
Observations	1,086	1,079	1,086	1,088
R-squared	0.01	0.01	0.01	0.01

Notes. OLS regression *** p <0.01, **p <0.05, * p <0.1. Robust standard errors in parentheses. Profit theme is an indicator equal to one if the student was exposed to the *financial independence* theme in the information session. Admission to the training is an indicator equal to one if the student was randomly admitted to the training, conditional on applying. Business student is an indicator that equals one if the student stidues business, management, finance, marketing, or a related degree.

Reporting Errors and Attrition. We provide evidence that the results are not driven by differences in reporting quality between admitted and non-admitted students. In business training interventions, whose overall effectiveness is judged in part by financial outcomes and adherence to "good" management practices, reporting errors may not be independent of treatment assignment. Individuals who have gone through the training program may be better at accurately judging profits and sales. We construct a measure of sales minus profits which should equal costs. If the difference is negative, it likely signals a reporting error. The treatment does not predict the incidence of a reporting error. The point estimate in Table A.7 is close to zero and non-significant (p = 0.856).

Treatment-specific attrition is minor. Table A.8 does not indicate significant difference in the completion rate of surveys. Interviews that were not completed are either the result of our not being able to reach respondents (e.g., due to changing phone numbers) or their refusal to participate. Reaching training participants may be less difficult as there exist social media groups within training classes and trainers may retain additional information. Our data does not indicate that this is a major concern. At the same time, we observe a small difference is the refusal rate, admitted treatment participants being slightly less likely to refuse participation (significant at the 10% level). In the future, we may construct treatment effect bounds to account for treatment-specific attrition (Lee, 2009; Behaghel et al., 2015).

5. Discussion and Preliminary Conclusions

Successful entrepreneurship is key for economic development. Still, fostering the creation of high-growth businesses has been challenging (e.g. McKenzie and Woodruff, 2014) and policies that encourage entrepreneurship have been criticized for dragging people into (risky) non-growth self-employment (e.g Shane, 2009). Our population consists of high skilled youth with capabilities to innovate (Levine and Rubinstein, 2018), given the option to participate in a promising training program targeted at spurring high-growth entrepreneurship. Negative effects of entrepreneurship education are supposed to be minimal due to limited options for high-skilled youth on the Uganda labor market. Entrepreneurship education aimed at fostering a proactive mindset can even have an impact on other careers as an employee.

In this ongoing project, we seek to causally identify the impact of a training with the aim of teaching an entrepreneurial mindset, to experimentally study motivations of potential high-growth entrepreneurs, and to document endogenous self-selection into the training. The project consists of three waves, two of which we have already implemented. For these, we are currently collecting longer-term follow-up data to answer our first question. With the data we already collected, we can speak to short-term effects of the training and provide evidence for the last two questions. We give first insights into differences between interested and non-interested students and analyze selection patterns through a randomized marketing intervention.

The endogenous selection patterns are consistent with the training being a substitute for other sources of entrepreneurial knowledge and networks. Non-interested students report to have more information sources they can contact for business advice and also feel more confident with regards to entrepreneurial challenges. At the same time, applicants seem to expect benefits from the training, being more confident about their future ability as entrepreneurs. In addition, we document differences between non-interested and interested students with respect to non-cognitive traits and economic preferences that are often associated with entrepreneurial success.

Through our randomized marketing intervention, we provide evidence on the importance of non-pecuniary motives for entrepreneurship. The propensity to apply to the training is higher when non-pecuniary motives for entrepreneurship are highlighted compared to monetary ones. Students apply at a significantly higher rate when we emphasize the *creative freedom* of entrepreneurship, compared to emphasizing the opportunity to achieve *financial independence*. This complements recent experimental findings on the importance of non-pecuniary motives of entrepreneurs (Guzman, Oh, and Sen, 2020; Ganguli, Huysentruyt, and Le Coq, 2018). We extend these findings by studying selection decisions in a sample of high-skilled prospective entrepreneurs in a low-income country.

In addition to the extensive margin effect of the marketing intervention, we document differential selection. We find heterogeneity in cognitive ability and students' overconfidence. When entrepreneurship is framed in terms of *creative freedom*, more cognitively able students select in. This is not the case in the profit theme. In contrast, the profit theme seems to attract overconfident students. This is also mirrored in the profit expectations of applicants. Applicants who were exposed to the *financial independence* theme have substantially higher profit expectations than applicants who saw the *creative freedom* theme. Our follow-up data allow us to shed light on the implications of these differential selection patterns for the causal effect of the training.

Nine months after the training, we observe substantially higher business creation for admitted students. Training participants are around 7 percentage points more likely to run their own business and devote 2.7 weekly hours more to it. While they also invest more into capital and labor input, this does not (yet) translate into substantially higher profits and sales. In addition, admitted students report being more competitive, more patient, and more proactive - all aspects of an entrepreneurial mindset. They also expand their business network. The training seems to give the admitted applicants (directly or indirectly) access to a network that they previously lacked compared to their fellow students who did not show interest in the training.

The positive effect on business creation is driven by applicants who were exposed to the *financial independence* theme at the information sessions. Comparing admitted and non-admitted students who saw the *financial independence* theme, admitted students are around 46.1 percent (13.5 percentage points higher rate than $mean_{control}$ = 29.3 percent) more likely to own a business nine months after the training [compared to 6 percent (2.2 percentage points higher than $mean_{control}$ = 36.7 percent) higher business creation of admitted students in the group of those who saw the *creative freedom* theme]. While we observe negative selection into the training on cognitive ability and positive selection on overconfidence in the *financial independence* theme, our results do not suggest that training is less or more effective for these students in terms of business success or income.

Through our ongoing data collection, we will be able to see the persistence of these effects, study dynamics and analyze the long-term impact of entrepreneurship education.

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Additional information

Ethics approval.

- We have obtained Internal Review Board (IRB) approval via the Ludwig Maximilian University of Munich. The project is identified under # 2018-07, and approval was issues on March 14^{th}, 2019.
- We have also obtained approval for conducting this research project by the Research Ethics Committee (MUREC) in Uganda on July, 5^{th}, 2019 (# REC REF 0204-19) and registered the project at the Uganda National Commission of Science and Technology (UNCST) where it is identified under #SS 5075.

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Appendix

A. Construction of outcome indices

Table 1 provides an overview of the hypotheses. In the following, we detail which variables are used to construct those indices. Note that we spell out winsorization and transformation in Section D.1, the creation of indexes based on z-scores in Section 3.2.

1. Entrepreneurship training study.

1.1. Economic outcomes (four hypotheses)

1 *Business creation*
- Business exists (yes/no)
- Average hours contributed by the hour per week

2 *Business success*
- Monthly profits
- Monthly sales

3 *Capital and labor input*
- Value of physical assets
- Value of inventory
- Capital investment over past 3 months
- Number of full-time employees
- Number of part-time employees
- Number of partners in business

4 *Economic self-sufficiency*
- Earnings from self-employment (monthly profits)
- Earnings from wage employment
- Earnings from other sources

1.2 Business and personal input (eight hypotheses)

1 *Business practices*
- Share of business practices employed

2 *Financial professionalization*
- Taken out a loan (yes/no)
- Size of loan
- Business registration
- Local trade licenses
- Knowledge about funding initiatives
- Actual funding from initiatives
- Received equity investment

- Banking account
- Emergency borrowing
- Business banking account
- Hours of consulting services

3 *Marketing*

- Number of marketing channels used

4 *Innovation*

- Introduction of a new product (yes/no)
- Number of new products
- Main new product is a new product line (yes/no)
- Product improvement (yes/no)
- Product new to neighborhood (yes/no)
- Origin of idea (own idea vs. inspired vs. purchased/others idea)
- Process improvement (yes/no)
- Introduced a new method for pricing (yes/no)
- Website with functioning URL (yes/no)

5 *Networks*

- Number of contacts in friends and family
- Number of contacts in "other"
- Scope of potential advice
- Scope of advice used
- Number of business partners

6 *Entrepreneurial mindset*

- Personal initiative
- Aspirations
- Entrepreneurial self-efficacy (general and task-specific separately)
- Entrepreneurial future

7 *Owner's non-cognitive traits*

- Big-5
- Grit

8 *Preferences*

- Risk preferences
- Subjective risk preferences
- Loss aversion
- Time preferences
- Subjective time preferences
- Competitiveness

2. *Selection study.*

   2.1 Selection into entrepreneurship among those with interest (four hypotheses)

      1 *Submitted application*

      2 *Cognitive ability*

      - Number of correcly solved Raven's matrices

      3 *Over-confidence*

      - Over-estimation
      - Over-placement

      4 *Entrepreneurial self-assessment*

      - Believes about becoming a successful entrepreneur,
      - Subjective rank of entrepreneurial ability,

   2.2.1 Economic outcomes (non-experimental) [*identical to 1.1*]

   2.2.2 Business and personal input (non-experimental) [*identical to 1.2*]

B. Additional Analyses.

Table A.1. Marketing theme and propensity to apply (OLS)

	1[Student applied to the training]			
	(1)	(2)	(3)	(4)
Profit theme	-.052*	-.053*	-.056**	-.051**
	[-.111,.006]	[-.110,.004]	[-.099,-.013]	[-.093,-.009]
Male		-.007	-.004	-.006
		[-.080,.065]	[-.066,.058]	[-.067,.054]
Freedom app. rate	.76	.76	.76	.76
Study year FE	No	Yes	Yes	Yes
Academy FE	No	No	Yes	Yes
Survey controls	No	No	No	Yes
Pseudo R-sq	.0035	.0058	.095	.1
# of participants	1366	1366	1366	1366

Notes. *** p <0.01, **p <0.05, * p <0.1.
This tables shows effects from OLS regressions where the dependent variable is an indicator for whether someone did submit an application (which is conditional on having been at an info session). 95% confidence intervals based on clustered standard errors are at the academy level in square brackets. Profit theme is an indicator equal to one if the student was exposed to the *financial independence* theme in the information session.

Table A.2. Marketing themes and heterogeneity in the propensity to apply (OLS)

	1[Student applied to the training]			
	(1)	(2)	(3)	(4)
Profit theme X Num. of correct Raven's	-.067**			
	[-.115,-.019]			
Num. of correct Raven's	.052***			
	[.020,.084]			
Profit theme X Overconfidence		.180***		
		[.077,.282]		
Overconfidence		-.087***		
		[-.143,-.030]		
Profit theme X Entrepreneurial self-assessment			.003**	
			[.000,.005]	
Entrepreneurial self-assessment			.002	
			[-.009,.013]	
Profit theme X business student				.033
				[-.087,.153]
Business student				-.005
				[-.075,.066]
Profit theme	.068	-.176***	-.046*	-.072
	[-.024,.161]	[-.237,-.116]	[-.102,.009]	[-.177,.033]
Freedom mkt app rate	.76	.76	.76	.76
Academy FE	No	No	No	No
Gender	Yes	Yes	Yes	Yes
Study year FE	Yes	Yes	Yes	Yes
Pseudo R-sq	.014	.015	.011	.0063
# of participants	1366	1366	1366	1366

Notes. *** p <0.01, ** p <0.05, * p <0.1.
This tables shows effects from OLS regressions where the dependent variable is an indicator for whether someone did submit an application (which is conditional on having been at an info session). 95% confidence intervals based on clustered standard errors are at the academy level in square brackets. The number of correctly solved Raven's matrices is between zero and four. Overconfidence is an indicator that equals one when the student estimates his or her own performance to be better than it objectively is. Entrepreneurial self-assessment refers to the self-reported ability to be a successful entrepreneur on a scale from 1 to 10.

Table A.3. Marketing themes and heterogeneity in the propensity to apply (w/ academy FE)

	1[Student applied to the training]			
	(1)	(2)	(3)	(4)
Profit theme X Num. of correct Raven's	-.056*** [-.095,-.017]			
Num. of correct Raven's	.032** [.006,.057]			
Profit theme X Overconfidence		.176*** [.098,.254]		
Overconfidence		-.090*** [-.154,-.026]		
Profit theme X Entrepreneurial self-assessment			.001 [-.002,.004]	
Entrepreneurial self-assessment			.007 [-.003,.016]	
Profit theme X business student				.007 [-.088,.101]
Business student				.023 [-.049,.096]
Profit theme	.038 [-.029,.105]	-.180*** [-.227,-.133]	-.054*** [-.090,-.018]	-.063 [-.140,.015]
Freedom mkt app rate	.76	.76	.76	.76
Academy FE	Yes	Yes	Yes	Yes
Gender	Yes	Yes	Yes	Yes
Study year FE	Yes	Yes	Yes	Yes
Pseudo R-sq	.069	.073	.069	.066
# of participants	1270	1270	1270	1270

Notes. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.
This tables shows marginal effects at the mean from logit regressions where the dependent variable is an indicator for whether someone did submit an application (which is conditional on having been at an info session). 95% confidence intervals based on clustered standard errors are at the academy level in square brackets. The number of correctly solved Raven's matrices is between zero and four. Overconfidence is an indicator that equals one when the student estimates his or her own performance to be better than it objectively is. Entrepreneurial self-assessment refers to the self-reported ability to be a successful entrepreneur on a scale from 1 to 10.

Table A.4. Marketing themes and expected profits

	Expected monthly profits (in UGX/1000)		
	(1)	(2)	(3)
Profit theme = 1	1,756.5***	1,754.3***	1,594.2**
	[544.9,2,968.0]	[568.2,2,940.4]	[420.0,2,768.3]
Freedom profit expectations	4,215.88	4,215.88	4,215.88
Gender	No	Yes	Yes
Study year FE	No	Yes	Yes
Academy FE	No	No	Yes
# of participants	984	984	984

Notes. *** p <0.01, ** p <0.05, * p <0.1.
This tables shows results from OLS regressions where the dependent variable is the expectation of monthly profits in UGX/1000. 95% confidence intervals based on clustered standard errors are at the academy level in square brackets.

Table A.5. ITT Effects of Entrepreneurship Training (+ 9 months): Robustness of primary outcome indices

Outcomes	Business Creation		Business Success		Capital&Labor Input		Economic Self-sufficiency	
	Winsorized (1)	IHTS (2)	Winsorized (3)	IHTS (4)	Winsorized (5)	IHTS (6)	Winsorized (7)	IHTS (8)
Admission to Training = 1	0.13**	0.15**	0.05	0.15**	0.10**	0.10**	0.07	0.05
	(0.06)	(0.06)	(0.06)	(0.06)	(0.04)	(0.04)	(0.05)	(0.04)
Business student = 1	0.03	0.02	0.04	0.03	0.00	0.01	0.00	-0.01
	(0.06)	(0.06)	(0.06)	(0.06)	(0.04)	(0.04)	(0.05)	(0.04)
2^{nd} year student = 1	0.02	-0.03	-0.09	-0.08	-0.05	-0.04	-0.11**	-0.13**
	(0.07)	(0.08)	(0.07)	(0.08)	(0.05)	(0.05)	(0.06)	(0.05)
3^{rd}+ year student = 1	-0.14*	-0.17**	-0.05	-0.18**	-0.04	-0.04	0.08	-0.08
	(0.07)	(0.07)	(0.07)	(0.07)	(0.05)	(0.05)	(0.05)	(0.05)
2^{nd} Cohort, spring 2020 = 1	-0.01	-0.02	0.01	-0.02	-0.03	-0.03	0.06	0.05
	(0.06)	(0.06)	(0.06)	(0.06)	(0.04)	(0.04)	(0.05)	(0.04)
Observations	1,093	1,093	1,086	1,086	1,093	1,093	1,095	1,095
R-squared	0.01	0.01	0.00	0.01	0.01	0.01	0.01	0.01

Notes. OLS regression *** p <0.01, ** p <0.05, * p <0.1. Standard errors in parentheses. Winsorizaion occurs at the 99^{th} percentile and -should the variables lack a natural lower bound, at the 1^{st} percentil. Admission to the training is an indicator equal to one if the student was randomly admitted to the training, conditional on applying. Business student is an indicator that equals one if the student stidues business, management, finance, marketing, or a related degree.

Table A.6. ITT Effects of Entrepreneurship Training (+ 9 months): Components of primary outcome indices

Outcomes	Running Business	Hours p. Week	Profit	Sales	Assets (total)	Inventory	Assets (recent)	Employees (Fulltime)	Employees (Parttime)	Partners	Wage Income	Other Income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Admission to Training = 1	0.07**	2.73*	4,869	32,407	239,066	496,498	26,745	0.12*	0.00	0.05	59,371*	111,535
	(0.03)	(1.52)	(97,807)	(371,654)	(287,213)	(477,820)	(32,625)	(0.06)	(0.13)	(0.07)	(34,919)	(99,551)
Business student = 1, yes	0.00	1.08	70,998	568,432	45,702	470,361	-12,498	0.14**	-0.08	-0.08	26,689	-53,200
	(0.03)	(1.51)	(97,412)	(370,317)	(285,982)	(475,772)	(32,485)	(0.06)	(0.13)	(0.07)	(35,406)	(103,231)
2^{nd} year student = 1	-0.03	2.50	-141,481	-523,747	-569,206*	-356,275	-9,929	-0.11	-0.33**	-0.05	-15,004	73,457
	(0.04)	(1.82)	(117,232)	(445,920)	(344,529)	(573,175)	(39,135)	(0.08)	(0.16)	(0.08)	(42,161)	(119,003)
3^{rd}+ year student = 1	-0.09**	-2.38	-68,356	-294,364	-806,185**	-439,310	15,133	0.02	-0.27*	-0.09	176,644***	162,044
	(0.03)	(1.77)	(114,246)	(433,900)	(334,989)	(557,303)	(38,052)	(0.07)	(0.15)	(0.08)	(41,557)	(119,007)
2^{nd} Cohort, spring 2020 = 1	-0.02	-0.55	-246,165**	-601,778	-761,675***	-482,544	-48,772	-0.08	-0.07	-0.04	-316,783***	77,377
	(0.03)	(1.50)	(96,601)	(367,485)	(283,681)	(471,945)	(32,224)	(0.06)	(0.13)	(0.07)	(39,738)	(104,206)
Control group mean	0.338	13.88	284,103	924,425	596,000	248,816	45,550	0.182	0.263	0.236	155,623	198,560
Observations	1,093	1,090	1,085	1,074	1,093	1,093	1,093	1,058	1,056	1,093	666	300
R-squared	0.01	0.01	0.01	0.01	0.01	0.00	0.00	0.01	0.01	0.00	0.11	0.01

Notes. OLS regression *** p <0.01, **p <0.05, * p <0.1. Standard errors in parentheses. Admission to the training is an indicator equal to one if the student was randomly admitted to the training, conditional on applying. Business student is an indicator that equals one if the student stidues business, management, finance, marketing, or a related degree.

Table A.7. Testing whether treatment predicts the incidence of a reporting error

Outcome	Reporting Error (1)
Admission to Training = 1	0.00
	(0.01)
Business student	Yes
Cohort	Yes
Study year indicators	Yes
Observations	1,073
R-squared	0.00

Notes. OLS regression *** p <0.01, **p <0.05, * p <0.1. Standard errors in parentheses. Reporting error is an indicator equal to one if monthly sales minus profits is negative.

Table A.8. Testing whether non-response is treatment-specific

Outcome	(1) Completed Survey	(2) Refused Survey
Admission to Training = 1	0.03	-0.01*
	(0.02)	(0.01)
Business student	Yes	Yes
Cohort	Yes	Yes
Study year indicators	Yes	Yes
Observations	1,251	1,251
R-squared	0.01	0.01

Notes. OLS regression *** p <0.01, **p <0.05, * p <0.1. Standard errors in parentheses. Completed is an indicator equal to one if a respondend was reached and the survey fully conducted. Refused in an indicator equal to one if a respondent was reached and he or she refused to participate in the phone survey.

C. Details on treatment assignment and statistical power.

The treatment and control group sizes were a function of the number of applicants. Specifically, if there were over 120 applications, we picked 45 students at random and offered admission, assigned 75 to the control group and omitted the remaining students from the study.[17] We anticipated low demand in some training cohorts and chose to over-sample the control group when possible; in case of low demand, having a sufficiently sized treatment group took priority. When we received between 85 and 120 applications, 45 students were randomized into the treatment group, and the rest was assigned to the control group. In case of 80 to 85 applications, we assigned 40 students to control and offered treatment to the remaining ones. Finally, if there were less than 80 applications we offered treatment to $n_T = min[\mathtt{n}_{\mathtt{applications}}, 40]$, and assigned $\mathtt{n}_{\mathtt{applications}} - n_T$ to control.[18]

Having chosen the experimental group sizes, we implemented the following randomization algorithm which stratifies along two dimensions. First, we grouped students according to how many years they had studied their current degree. This is top coded at three years as this is the modal number of years students require to complete a Bachelor degree.[19] The rationale for this is that students who are close to graduation are more likely to move into (self-) employment in the near future. Second, the algorithm ascertains that the share of business students (students who study business, management, finance, marketing or related fields) is balanced between treatment and control *within* each year of study. Students' prior knowledge about business and entrepreneurship concepts may interact with the training content and business students' responses to the training program would systematically differ vis-à-vis non-business students.

We form six cells based on the program of study: business-related (two dimensions: yes or no), and years into the program (three dimensions: one, two or three years). We first use both cells for third-year students, and within each assign an equal number to either treatment or control. This ensures that all applications from third-year students are used.[20] We then applied the same procedure to second-year students. If not all applications from second-year students were necessary to complete target group sizes, we chose a subset at

[17]This is done due to capacity and resource constraints. In practice, it is rare to receive over 120 applications for an academy.

[18]Note that the second term can be zero if less than 40 applications are received.

[19]Most applicants are Bachelor students ($\approx$ 87 percent) and those that are not are almost exclusively enrolled in "certificate" and "diploma" programs, which can either be a preparatory or supplementary degree. These usually take two years and can precede or follow a Bachelor degree.

[20]In theory, it would be possible to receive applications from third-year students in excess of the experimental group sizes. In such cases, we would have randomly picked the respective number. In practice this was never the cases.

random. Finally, if group sizes were still not exhausted, we included (a random subset of) first-year students.[21] The exact same procedure will be used in Wave III.

Our power calculations show both the worst-case scenario, in which we cannot implement the planned third wave at all, and the base-case scenario, in which we proceed with our project as planned or with some delays. To benchmark the statistical power of detecting effects of the training program on business success, we are conservative and present minimum detectable effect sizes based on the actual training cohort sizes from the first two waves of training conducted in the fall of 2019 and the spring of 2020 as the worst-case scenario. We further provide power calculations for various scenarios of attrition and non-compliance given the realized sample size.

During the first two waves we worked with 18 cohorts, meaning 18 university-by-semester blocks. There are 727 and 497 students in the treatment and control groups respectively. This corresponds to an average treatment group size and control group size of 40.4 and 27.6, respectively, and 68 students per cohort in total.

To incorporate myriad factors such as attrition, non-compliance, varying treatment and control group sizes into the power calculations, we perform simulations. We specify a data generating process and set the magnitude of our treatment effect to be equal to a pre-specified percentage of the standard deviation of a generic outcome; this can be interpreted as an effect size in percentage terms. This maps well into our strategy to deal with concerns from testing multiple hypotheses which rests on constructing normalized indices of our outcome variables with a mean of zero and a standard deviation of one.[22]

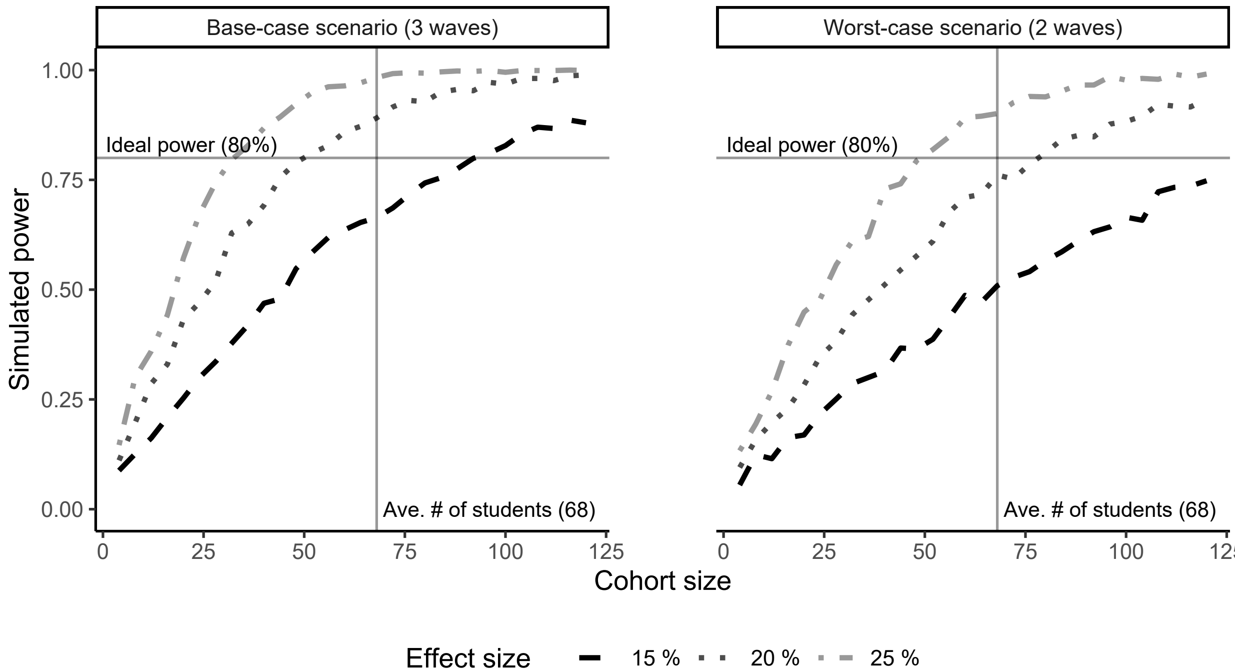
For the simulations, we estimate the primary specification (see Equation (4)) in a simulated sample and conduct a two-sided t-test of the null hypothesis of a zero effect of the treatment using standard errors that are robust to heteroskedasticity for inference. For the simulated sample, we set the number of cohorts, rates for attrition, non-compliance, and percent of sample treated as specified in the next paragraph. Then we vary the sample size per cohort starting from four, going until 122 in steps of four.[23] We draw 1,000 simulation samples per sample size considered. Across all simulated samples, we calculate the share of rejected null hypotheses at $\alpha = 0.05$ which is the measure of simulated power.

[21] As an example, suppose there are 80 third year applicants; 56 in business-related degrees, 24 in non-business related degrees. The procedure allocates 28 of the business students to *each* of treatment and control; similarly, 12 of the non-business students would be in *each* of treatment and control. Overall, there would be 40 students in treatment and 40 in control, but the shares of business and non-business students would be equal across the groups.

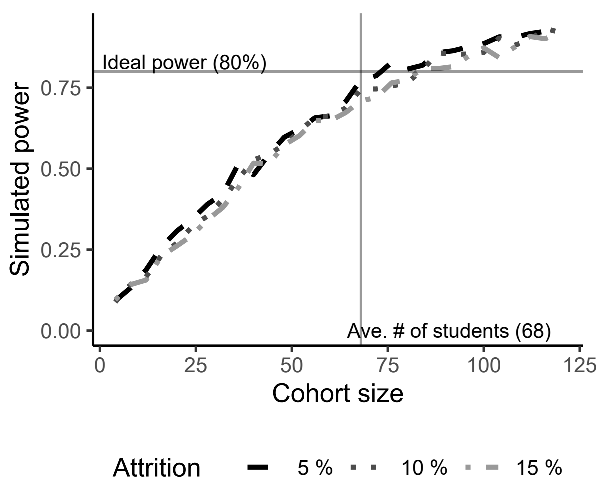
[22] In Section 2.3, we detail the procedure. In short, combining several measures into one index measure reduces the number of hypotheses to be tested. Rather than testing one hypothesis per variable, general conclusions are drawn by testing a hypothesis regarding the index.

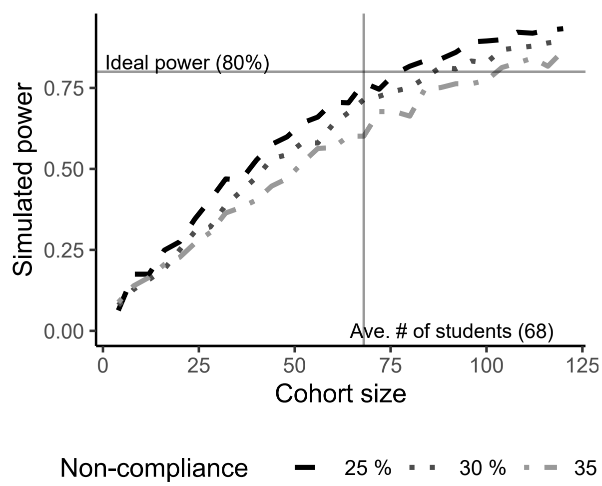
[23] The lower sample sizes are not realistic, though they help to visualize the trend in power with respect to cohort size.

Figure C.1. Statistical power simulations



(a) Effect size in base (3 waves) - and worst-case scenario (2 waves)





(b) Worst-case scenario + attrition

(c) Worst-case scenario + non-compliance

Notes. The simulations in Panel (a) have the following specifications: attrition rate is five percent, non-compliance is 25 percent, within-cohort correlation is 10 percent and the treatment probability is 59 percent. Statistical power to detect an effect of 15 percent, 20 percent or 25 percent for different average cohort sizes is presented. Cohort size is the sum of treatment and control group individuals. The right hand panel reports the worst-case scenario (two waves) while the left hand panel illustrates calculations for the base-case scenario (three waves). The worst-case simulations vary the attrition rate in Panel (b) and the non-compliance rate in Panel (c) for an effect size of 20 percent.

The simulation results are shown in Figure C.1. Panel (a) presents the base-case scenario based on three waves of academies (left panel) and the worst-case scenario based on the two waves of academies that have been implemented already. We set the following parameters for our benchmark simulations: attrition rate of 5 percent, corresponding to twice the actually observed attrition in the implementation check of the first wave in fall 2019; a non-compliance rate of 25 percent as calculated based on the attendance data for the first wave in fall 2019; and a correlation within training cohorts of 10 percent, corresponding to a generously upward rounded measure from pilot data. The right half of panel (a) indicates that the design is sufficiently powerful (76 percent) to detect an effect of 20 percent (or 0.2 of a standard deviation) even in the worst-case scenario which seems to be a typically observed change (McKenzie and Woodruff, 2014).[24] In the base-case scenario in the left half of panel (a), our design would be well-powered to detect an effect size of 20 percent (89 percent power). If the effect size is actually only 15 percent of our standardized variable, the statistical power of our design reduces to 66 percent.

In panel (b) and (c) of Figure C.1, we take the worst-case scenario and calculate the power to detect a 20 percent effect considering even more severe scenarios of attrition and non-compliance, holding the other parameters constant.[25] Panel (b) reports that attrition rates of 10 percent and 15 percent would only have a marginal effect on the power of the design. Panel (c) shows that non-compliance rates of 30 percent and 35 percent decrease statistical power to detect an effect of 20 percent to 71 percent and 60 percent, respectively.

[24] Our study is not only well-powered to detect typical effect sizes, it also improves on existing studies. McKenzie and Woodruff (2014) notes that in most studies the power to detect an increase of 25 or even 50 percent in profits or revenues is well below generally accepted levels of power of above 80 percent.

[25] In results not reported, we can also demonstrate that a correlation of 0.15 within training cohorts has only a negligible effect on the minimum detectable effect.

D. Details on data processing & analysis.

D.1. **Data processing.** First, to establish that our results, especially those involving monetary outcomes, are not driven by extreme observations, we will report results with and without winsorizing outcomes at the 99^{th} percentile. Should a variable lack a natural lower bound (i.e., revenues are bound at zero, while profits are unbounded), we also winsorize at 1^{st} percentile.

Second, distributions of variables such as revenue and profits are likely be skewed to the right. We apply the inverse hyperbolic sine transformation to this data which is defined as $f(x) = log(x + \sqrt{x^2 + 1})$ (Burbidge, Magee, and Robb, 1988).

Third, in order to limit noise caused by variables with minimal variation, questions for which 95 percent of observations have the same value within the relevant sample will be omitted from the analysis and will not be included in any indicators or hypothesis tests.

Fourth, whenever a survey's skip logic was triggered by a "yes" or "no" answer, we code the subsequent questions in the logical fashion.

Section 3.2 describes how we construct indices to reduce the number of hypotheses tests. Note that the index value is missing if there is one or more missing values in the component variables (e.g., if a person answers "don't know" to one of the questions). We address this problem by providing two estimates in addition to the estimate based on the actually observed number of non-missing cases. First, we impute missing values using the mean value for the entire population, and then generate the index. For robustness, we also provide benchmarks for imputing minimum and maximum values for the entire population. Second, we implement an Inverse Probability Weighting (IPW) estimator in which each non-missing index value is weighted by the inverse probability of having data observed (Seaman and White, 2013). We model the incidence of observing an index value using a logit model with complete baseline characteristics (sex, employment status, self-employment; see Table 3), and use the predicted probability.

Fifth, in order to compare monetary values across time, we adjust values using Consumer Price Index data published by the Uganda Bureau of Statistics.

D.2. **Multiple hypotheses testing.** We construct several indices within each family of outcomes as detailed in Section 3.2 and Appendix A. We employ two approaches to control the FWER, that is, controlling the probability of a false positive within each family. First, we implement the approach used by Aker et al. (2016) who use a traditional Bonferroni-type adjustment but account for correlations across variables used to test hypotheses.[26] Their method nests the classic Bonferroni adjustment when outcomes are uncorrelated.

[26] In our cases, we employ the correlation between index measures within each family.

Second, we also employ the method outlined by Barsbai et al. (2020) who develop a regression-adjusted version of List, Shaikh, and Xu (2019). This is a bootstrap-based stepwise procedure designed to control the FWER in settings with multiple hypotheses.

Thus, for each hypothesis across our five families we obtain two p-values which control the FWER, on top of standard p-values. The p-values that correct for multiple hypothesis testing are of interest for researchers with no priors on the specific hypotheses we test. Our preferred procedure is the one by Barsbai et al. (2020) and our main conclusions will be based on being able to reject null hypotheses using those p-values. We report p-values using the procedure by Aker et al. (2016) for comprehensiveness.