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Can gamified online training make high school students more entrepreneurial? Experimental evidence from Rwanda ☆

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ABSTRACT

We study the short-run effects of a gamified online entrepreneurship training offered to high school students in Rwanda during the COVID-19 pandemic. Using a randomized controlled trial, we estimate sizeable effects of the 6-week training on entrepreneurial activity. One month after the training, participants in schools offered the training were much more likely to own a business than participants in control schools. The training induced students to participate more actively in their school's business club, to undertake more business-oriented actions, to improve their business practices, and to interact more with other youth and family members about their business ideas. We hypothesize that the training motivated treated students to sustain their business activities during the COVID-19 crisis.

1. Introduction

The school to work transition is challenging for many youth in low- and middle-income countries due to limited employment opportunities. Considerable government and non-governmental organization (NGO) investment in youth skills such as vocational training has yielded mixed results, suggesting the binding constraint is not trade skills, but insufficient wage employment demand for youth (Blattman & Ralston, 2015). Because most youth enter self-employment instead, policymaker attention has shifted more recently to youth programs focused on entrepreneurship, in an attempt to increase productivity in the sectors youth actually enter (Fox & Kaul, 2018; Fox et al., 2016). Yet despite the potential for high returns, evidence on the effectiveness of youth entrepreneurship programs is limited.

The COVID-19 pandemic introduced two new challenges for potential youth entrepreneurs. First, the pandemic generated pressures on older students to make greater economic contributions to their households. In developing countries, many students help to run small and informal businesses, but the pandemic was a major negative shock to such businesses throughout the world (Bartik et al. 2020, Crane et al. 2020, Fairlie 2020 in the US, Gourinchas et al. 2020 in Europe, and Apedo-Amah et al. 2020, Muzi et al. 2021 across continents). Young informal firms, female-owned businesses, and those in Sub-Saharan Africa were particularly affected (Aga & Maemir, 2021; Liu et al., 2021; Muzi et al., 2021). Interventions that can help businesses survive in this context are particularly relevant. Second, education disruptions from the COVID-19 pandemic forced many schools to substitute for foregone instruction with technology such as online education, with mixed success (Angrist

☆ This experiment is registered at the AEA RCT Registry, study ID AEARCTR-0007249 (Pugatch et al. 2021). The study was approved for human subjects research ethics by the Institutional Review Board of PUC-Chile, study number 200527001, and by the Rwanda National Ethics Committee, study No. 654/RNEC/2020. We thank the Wavumbuzi project team, especially Chris Campbell, Geoffrey Forbes, Daniel Hampton, David Jenkins, Aline Kabanda, Roselyn Marandu-Kareithi, and Phumlani Nkontwana; and the Kigali office of Innovations for Poverty Action (IPA), particularly Cassien Havugimana, Baba Ali Mwango, Jean Aime Nsabimana, Jean De Dieu Ntivuguruzwa, and Phillip Okull. Katia Everke Reyes and Leopoldo Lazcano provided superb research assistance. Soha Ahmed provided excellent support in identifying relevant literature. Luis Baldomero-Quintana, Lee Crawfurd, Alex Eble, and Anne Fitzpatrick provided helpful comments. We acknowledge financial support from Allan and Gill Gray Philanthropy , which implemented the program but made no restrictions on the analysis or publication of results. Lafortune thanks funding from ANID Fondecyt Regular 1210772. The findings, interpretations, and conclusions expressed in this paper are entirely those of the authors. They do not necessarily represent the views of the World Bank or those of the Executive Directors of the World Bank or the governments they represent.

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et al., 2022; Crawfurd et al., 2021). Although online education has long been promoted as a potential substitute for on-site classes, evidence on its effectiveness remains limited and mixed (for reviews and meta-analyses, see Bernard et al., 2004; Escueta et al., 2020; Means et al., 2010; Nguyen, 2015). A key concern is the low and limited engagement of online students. However, a gamified approach could generate the engagement necessary to improve outcomes (Rodriguez-Segura, 2022). These challenges created an opportunity for an online, gamified entrepreneurship training program targeting youth.

In this paper, we study the short-run effects of a gamified online entrepreneurship training offered to high school students in Rwanda in early 2021, during the COVID-19 pandemic. The main goals of the training were to develop an entrepreneurial mindset, encourage students to aspire to be entrepreneurs, and to create or consolidate their own businesses. The training had a gamified approach and consisted of approximately 40 "bite-sized" business-related challenges, spaced over six weeks. We conducted a cluster randomized control trial (RCT) with 159 schools, with randomization at the secondary school level. Our results are based on an online endline survey conducted within one month of the end of the training.[1]

While online teaching methods can substantially reduce the cost of the training, it may also be problematic if few individuals have access to internet in low- and middle-income countries. Focusing on youth may make a difference in that setting. In our sample, half of the youth connected to the internet through a school computer or tablet. Another third used their own cellphone, while 10 percent employed a family's cellphone to connect. This suggests that while internet connection may be problematic for online training of adults, youth may face less difficulty because of school infrastructure. Finally, only 15 percent claimed to have a slow internet connection, indicating that school- or mobile-based internet is considered reliable.

We find significant effects of the program on the probability that participants were running their own business right after the training, when COVID-related restrictions in Rwanda were still in place. The program also encouraged participation in income-generating school business clubs, the gateway to entrepreneurship in many high schools of Rwanda. These effects did not happen at the expense of other economic activities: total earnings and hours worked significantly increased for participants compared to the control group.

We study possible mechanisms for these effects and find evidence that the program fostered entrepreneurial actions, adoption of improved business practices, interaction with other youths and family members to discuss business ideas, and greater preference for entrepreneurship courses at school. We do not find evidence that training generated a more entrepreneurial mindset, nor that it improved curricular business knowledge, as we do not observe improvements in soft skills related to entrepreneurial success nor on questions on business topics covered in the course.

We make several contributions to the literature on entrepreneurship. First, we contribute to the small but growing evidence suggesting that adolescents in developing countries can acquire entrepreneurial skills. Several experiments have found increased self-employment in response to programs targeting youth (e.g., Bandiera et al., 2010; Bertrand et al., 2021; Blattman & Dercon, 2018; Blattman et al., 2019, 2014, 2020). However, most of these programs did not specifically train participants in entrepreneurial skills, but instead provided start-up grants or bundled entrepreneurship training with credit or other services. We find significant (short-run) effects on self-employment of a program that is brief and online. The gamified approach of the training could have played a key role in keeping participants engaged and motivated.

Rigorous evidence specific to youth entrepreneurship training is less abundant. An experiment training entrepreneurship teachers in Rwandan secondary schools — the same context as our study — strengthened curricular implementation, but with modest increases in youth business participation and no effect on income (Blimpo & Pugatch, 2021). Similarly, entrepreneurship training for university students in Tunisia had modest short-run effects on self-employment, which faded in a four-year follow-up (Alaref et al., 2020; Premand et al., 2016). However, an intensive three-week program combining soft and hard skills and modeled in part after master's of business administration (MBA) curricula from the United States led to increases in business creation, survival, and earnings among recent secondary school graduates in Uganda (Chioda et al., 2021). Whereas the less successful programs in Rwanda (Blimpo & Pugatch, 2021) and Tunisia were embedded in traditional academic settings, the successful programs in Uganda and our study were short, intensive programs with innovative curricula. These patterns merit further consideration in future research.

Second, we contribute to the overall literature on entrepreneurship training. McKenzie and Woodruff (2013) summarized the early literature on the subject, finding few studies with statistically significant effects on business outcomes, mainly due to a lack of statistical power. Simpler interventions such as rule-of-thumb training have been argued to be more efficient in some contexts (Drexler et al., 2014). Complementary interventions such as mentoring (Brooks et al., 2018) or role models (Lafortune et al., 2018) may be needed to increase the potential of these programs. To our knowledge, gamifying the training has not been explored before.

McKenzie (2021) recently conducted a review and meta-analysis, finding that the typical training program does increase profits and sales by an average of 5 to 10 percent. These programs would pass a cost–benefit analysis if their costs were not too high. Therefore, a key question in the literature is how to reduce costs to serve many firms in a financially sustainable way. One potential solution is to offer online training, but the literature on its effectiveness is new and limited (Liguori & Winkler, 2020). Jin and Sun (2020) find improved business outcomes in response to online training for sellers on a Chinese internet marketplace, while Cusolito et al. (2021) find that differences between Balkan firms trained either online or (largely) in-person fade after two years. Davies et al. (2024) studied the effect of live training sessions over Zoom for self-employed women from Mexico and Guatemala and found short-term improvements in business outcomes, which dissipated after 6 months. Furthermore, the potential cost savings of their online training compared to in-person sessions were not substantial mainly due to the cost of trainers. The results from our study suggest that a gamified online version of business training can significantly foster entrepreneurship among secondary school students, at least in the short run, without incurring the cost of online trainers. In a related study, Moberg (2021) conducts an experiment with 580 Danish youth to estimate the effects of an online-based entrepreneurship program, which was not gamified but based on role models. He also finds strong effects on entrepreneurial intentions in the short-run.

Within the entrepreneurship training literature, our study also contributes to understanding the transmission of soft skills. A large literature has linked personality traits and entrepreneurship (de Mel et al., 2010; Hamilton et al., 2019; Lazear, 2005; Levine & Rubinstein, 2017), but drawing a causal link has been more challenging. Campos et al. (2017) shows large positive impacts of personal initiative training for entrepreneurship in Togo. Ubfal et al. (2022) and Alibhai et al. (2019) find more nuanced evidence on similar programs. The former detects positive effects only in the short-run in Jamaica, arguing for the need of complementary interventions; while the latter finds mixed effects of two programs in Ethiopia, claiming that the more effective of the

[1] Figure A1 shows the cumulative proportion of endline responses by week. Almost 90 percent of responses occurred in the week of or preceding the end of the program. The treatment group responded at a higher rate in the initial week the endline opened, but by the second week response rates were nearly identical (90 percent of the treatment endline sample, 88 percent for control). Our results should therefore be interpreted largely as coinciding with the end of the intervention, with little difference in timing between treatment and control.

two programs had much higher quality trainers. Finally, Chioda et al. (2021) shows large effects from two training programs with different combinations of soft-skill and hard-skill training in Uganda. We extend this literature to a soft skills training that is both online and gamified.[2] In our study, the program did not improve soft skills, despite being one of the goals of the training. However, we do find increased motivation and entrepreneurial actions.

Third, we contribute to the evaluation of gamification as a tool to improve learning outcomes. Gamification has been experimentally tested as an intervention in several domains, including to improve sales performance, to elicit contributions to public goods such as Wikipedia, and to promote weight loss, with mixed results (Gallus, 2017; Kurtzman et al., 2018; Mollick & Rothbard, 2014). In the education literature, gamification has been studied little relative to other learning technologies, despite its potential to sustain student engagement (Escueta et al., 2020; Rodriguez-Segura, 2022). Meta-analyses of gamification in educational settings show small but positive effects on learning (Sailer & Homner, 2020; Yildirim & Sen, 2021). Randomized experiments of gamified math curricula in India, China, and Chile improved skills among primary school students (Araya et al., 2019; Banerjee et al., 2007; Dillon et al., 2017; Lai et al., 2015, 2013; Ma et al., 2020). Experiments with university statistics students also showed positive results (Legaki et al., 2021, 2020). The success of these programs suggests gamification could be an important mechanism to realize the potential gains from computer-aided instruction. However, another RCT in the United States of a math software with a gaming component did not show positive impacts (Rutherford et al., 2014). We believe our study is the first randomized evaluation of a gamified entrepreneurship training.[3]

The rest of the paper is organized as follows. The next section presents the research design. Section 3 presents the methodology. We review balance, take-up, and attrition in Section 4. We present the results in Section 5, and conclude in the last section.

2. Research design

2.1. Program

The Wavumbuzi[4] Entrepreneurship Challenge is a six-week, online entrepreneurship training program for adolescent youth. Wavumbuzi was designed by Allan and Gill Gray Philanthropy, a charity based in South Africa, to promote youth entrepreneurship. The program has previously run in South Africa and Kenya. We evaluate the launch of Wavumbuzi in Rwanda.

In Rwanda, Wavumbuzi serves as an extracurricular supplement to the study of entrepreneurship in secondary schools. Entrepreneurship has been a required subject in Rwandan secondary schools since 2009, making the country the "site of one of the most extensive efforts to promote youth entrepreneurship in the world" (Honeyman, 2016, p. xii). A major reform in 2016 reoriented the entrepreneurship curriculum towards practical business skills and encouraged students to form revenue-generating business clubs based at their schools. Among students assigned to the control group in our sample, nearly one of three (28 percent) are already entrepreneurs and nearly nine of ten (87 percent) view entrepreneurship as a future career. Wavumbuzi therefore occurred in a context of high familiarity with entrepreneurship among its participants.

The goals of Wavumbuzi are to promote entrepreneurial mindset, intentions, and actions. Wavumbuzi consists of approximately 40 modules called "challenges", spaced over the six weeks of the program. Challenges focus on elements of business development, such as conducting market research, designing a minimum viable product, and marketing, as well as socio-emotional skills such as grit and goal-setting. The program relies heavily on gamification to promote participant engagement. Participants progress through a virtual game world with each challenge. They earn virtual badges and points, based on quality ratings by other participants, when they submit challenges. An internal rater reputation system encourages good faith in ratings of peers. Participants can view live leaderboards by school, and leaders earn weekly prizes, such as consumer electronics, clothing, or school supplies. Figures A2–A3 show examples of a challenge and the leaderboard, respectively, as seen by participants.

The program ran for six weeks in January–February 2021. Secondary school students (grades 7–12) were eligible to register. A major marketing campaign was conducted in the month prior to program launch, with advertisements in national media and program "ambassadors" deployed throughout the country. Secondary school entrepreneurship teachers were encouraged to recruit their students to register.

The program launch coincided with a spike in COVID-19 cases in Rwanda, triggering a 15-day lockdown in the capital, Kigali. Moreover, in a national survey in November 2020, seven in ten households (71 percent) reported lower income than before the pandemic, with nearly four in ten (37 percent) reporting income reductions of at least 75 percent (Egger et al., 2021). The program thus occurred during a time particularly favorable for student attention to online activities, and of heightened economic insecurity.[5]

2.2. Evaluation design

We evaluate Wavumbuzi via a cluster randomized control trial (RCT), with randomization at the school level. The sampling frame includes Rwandan secondary schools selected by the project as a collaboration between Wavumbuzi and the Rwandan Education Board (REB). All eligible students in these schools were encouraged to register online to Wavumbuzi. The evaluation sample includes schools in the sampling frame that had at least one student registered to the platform at the moment of program launch in January 2021.

Using the evaluation sample, we randomly assigned 108 schools to treatment and 51 schools to control. The proportions allocated to treatment and control were determined to balance considerations regarding statistical power, logistical capabilities of the team and the desire of Wavuzumbi to maximize the number of participants in the Rwandan launch of the program. The randomization was stratified by district and number of registered students in the school (1 student, 2–10 students or more than 10 students).[6] Schools in the sample were similar to secondary schools throughout Rwanda, though somewhat larger and more likely to be public schools (Table A1).[7]

[2] While a few prizes were offered to winning individuals and schools, we are unable to identify the impact that those could have had above and beyond the gamification of the learning process.

[3] We distinguish gamification from entrepreneurial "edutainment", such as reality television competitions among start-ups, which have shown promise to improve perceptions of entrepreneurship (Barsoum et al., 2021; Bjorvatn et al., 2020).

[4] *Wavumbuzi* is the Swahili word for entrepreneur. We hereafter refer to the program simply as Wavumbuzi.

[5] School closures were limited to Kigali during the 15-day lockdown. Schools in other areas of the country remained open for the duration of the program. Students in Kigali account for 5 percent of program registrations. The program therefore was not competing with remote instruction from schools for student attention.

[6] "Misfit" schools not initially assigned due to uneven strata sizes were assigned independently across all strata, using the `wstrata` option of the `randtreat` command in Stata (Carril, 2017).

[7] The eventual number of schools in our sample is less than the initial 159 for a few reasons. First, some schools were registered under two different

As students registered, they were allowed to complete the first week of activities, which included four short challenges, before the randomization took place. After the first week, only students in treated schools were granted full access. We allowed this initial participation among students eventually assigned to the control group for several strategic reasons. First, we expected initial program access to encourage completion of the baseline survey, higher participation in the program among those selected, and later completion of the endline survey. Second, Wavumbuzi implementers wanted to provide some access to all registered students to encourage control group participation in later iterations of the program.

Students registering for the program were encouraged to complete an online baseline survey, which closed after random assignment.[8] The online endline survey opened in the final week of the program and closed one month after the program ended. We provided incentives of about USD 3 in mobile airtime for each completed survey. In addition to these surveys, we also have administrative data on program participation. Figure A4 summarizes the program and data collection timelines.

3. Methodology

Our analysis follows a pre-analysis plan registered at the AEA RCT Registry (Pugatch et al., 2021). We label analysis outside this plan as exploratory. Our main specification is the ordinary least squares (OLS) regression:

$$y_{isg} = \alpha + \beta T_{sg} + \delta y_{0isg} + \gamma_g + \epsilon_{isg} \quad (1)$$

where i, s, and g are indexes for student, school, and strata (by district and registration size), respectively; y is the outcome; T is an indicator for being offered the training; y_0 is the baseline outcome (when available); γ is a stratum fixed effect; and ϵ is the error term. For covariates (e.g., baseline outcomes), we replaced missing values with zero and include a dummy for missing values in the regression. The coefficient of interest is β, which measures the intent to treat (ITT) effect, or the effect of the offer of the program. We cluster standard errors by school, the unit of treatment. We also report p-values for main results when using the wild cluster bootstrap, a conservative approach given the relatively large number of clusters (133 schools with at least one student observation) in our sample.[9]

In addition to the specification in Eq. (1), the pre-analysis plan describes the outcomes, baseline characteristics to check for balance, and tests for differential attrition.

4. Balance, attrition and take-up

We test for balance across treatment and control groups in two different data sets: (1) the registration data, which was a short form completed by all participants, and (2) the baseline survey, which was not required for participation in the program and completed by a selected sample of participants. At the moment of registration, all students needed to fill a short form which included data on their gender, age, grade, mother tongue, and a contact email or mobile phone number. The baseline survey was conducted after registration within the platform and included more questions regarding business activities before the program, family background, school activities, and academic performance.

Table 1
Baseline characteristics (from registration data) and balance.

	Mean (Control)	S.D.	Treatment All students	Endline
Female	0.42	0.49	0.03	−0.08
			(0.11)	(0.11)
Mobile	0.43	0.50	0.22	0.25
			(0.18)	(0.21)
Email	0.88	0.32	−0.22	−0.21*
			(0.13)	(0.12)
Age	17.93	2.23	0.07	−0.19
			(1.11)	(1.02)
English	0.24	0.43	0.30***	0.35***
			(0.08)	(0.12)
Kinyarwanda	0.76	0.43	−0.30***	−0.35***
			(0.08)	(0.12)
Senior 1	0.05	0.22	−0.02	−0.07**
			(0.04)	(0.03)
Senior 2	0.05	0.22	−0.01	−0.03
			(0.03)	(0.02)
Senior 3	0.14	0.34	0.02	0.07
			(0.06)	(0.07)
Senior 4	0.19	0.39	0.02	0.07
			(0.06)	(0.06)
Senior 5	0.37	0.48	−0.01	−0.02
			(0.06)	(0.08)
Senior 6	0.20	0.40	−0.01	−0.03
			(0.08)	(0.10)
Num. Students (Senior 6, School)	76.10	45.28	−7.09	−6.63
			(13.26)	(12.63)
Num. Students (School)	434.43	191.32	−67.92*	−73.00*
			(39.71)	(42.43)
Observations	1885	1885	1885	1102
Joint F-test statistic			2.20**	7.23***

The first and second columns report the mean and standard error of observations in the control group, respectively. The third and fourth columns report the coefficient of a regression where the outcome is the baseline characteristic and the explanatory variable is a dummy for being assigned to treatment, including strata fixed effects and standard errors clustered at the school level. The third column includes the full sample, the fourth, only individuals who answered the endline. Senior 1–6 refer to grades 7–12, respectively. Significance level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

In Table 1, we present the characteristics of our sample using the registration data.[10] We see that 42 percent of students are female, 43 percent have a mobile phone, and 88 percent provide an email address. The average age is around 18 years old. The majority reports that their mother tongue is Kinyarwanda and a smaller fraction, English. A majority of participants are in their second-to-last year of high school (Senior 5) and more than 75 percent are within 3 years of graduating. The average school in our sample has more than 400 students, but with a large variance.

The third and fourth columns of Table 1 indicate that despite random assignment, there were some imbalances by treatment status. The treatment group is less likely to provide an email address; more likely to report English as their mother tongue compared to Kinyarwanda; and less likely to be in Senior 1 (grade 9). Schools in the treatment group are also smaller. This leads the joint F-test for no differences between treatment and control groups across all outcomes to be rejected. Because of these imbalances, we control for all baseline characteristics in our estimates, as described below.

names, which we only discovered after randomization. We unified those schools and when assignment had been to two different groups, all participants from those schools were assigned to treatment. We then lost a large fraction of our initial sample of schools when we conducted the analysis on the sample that answered the endline since many schools had few participants.

[8] Some participants answered the baseline survey after random assignment was announced and those answers were excluded from the analysis.

[9] Our pre-analysis plan specified clustering by strata, following the recommendation by de Chaisemartin and Ramirez-Cuellar (2020) to cluster by strata when the number of units (schools) within strata is small. However, we find that clustering by school is, in general, more conservative. Results following the pre-analysis plan appear in the appendix.

[10] For many characteristics, we do not have answers for all participants. We report results on non-missing values. Non-response is uncorrelated with treatment.

Table 2
Attrition by survey.

	Mean (control group)	S.D. (control group)	Treatment
Baseline Survey	0.72	0.45	−0.20
			(0.15)
Endline Survey	0.52	0.50	−0.01
			(0.13)

Attrition takes value 1 for students who did not complete the baseline (endline) survey. The first two columns report the average and standard deviation of the attrition dummy for the control group. The third column reports the coefficient of a regression where attrition is regressed on a dummy for being assigned to treatment and strata fixed effects, with standard errors are clustered at the school level. Significance level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

As explained above, only a selected sample of students completed the baseline survey. Baseline completion rates differed between the treatment and control groups, as shown in Table 2. Only 28 percent of the control group answered the baseline survey, while 48 percent of the treatment group did. The difference is large and statistically significant. The correlation between treatment and participation in the baseline limits the usefulness of the baseline survey. We do not have any explanation for this correlation, as the baseline was answered before treatment assignment was revealed to participants. Appendix Table A2 presents a balance table using registration data for participants who completed the baseline survey. Most characteristics are balanced, but we do see important differences in gender. The last three rows of Table A2 use baseline data on our main outcomes and show balance across treatment arms in the subsample of students with baseline data.

Reassuringly, we also find that response rates to the endline survey were balanced between the two groups. Although attrition rates were very large (on the order of 50 percent), these response rates are typical of online surveys without interaction with participants.[11]

Finally, Table 3 provides evidence that attrition at endline was not differential by characteristics of the sample depending on treatment assignment. Attrition was less likely among those who had reported a mobile number, who were older, who spoke Kinyarwanda instead of English, and who were in higher grades. Reassuringly, however, we find only one characteristic where the attriters differ statistically between the treatment and the control group, and the difference is only significant at the 10 percent level. The joint test of significance does not reject the null of no differential attrition.

Because we face large imbalances in some variables, even when using the registration data, we depart from the pre-analysis plan and control for all available pre-determined variables in our main regressions. We report the pre-analysis plan specifications for all outcomes in the appendix, which show that most of the results are robust to including the additional controls. This suggests that the imbalance we face is not correlated with the impact of the treatment.

Table 4 shows evidence on program take-up. We show this for our full sample and for the sample that answered the endline. The treatment group was 31 percentage points more likely than the control group to complete at least one challenge. This difference increases to 55 percentage points when we focus on the sample that answered the endline survey. Treated participants completed on average 35 percent of the challenges, while the control group completed only 2 percent of the challenges. In the sample that answered the endline, the difference is even more marked, with those assigned to treatment completing 65 percentage points more of the challenges than the control group. Overall, assignment to treatment induced significant participation in the program. While not reported here, we also find that participants were significantly more likely to conduct reviews of their peers' submission (28 reviews, versus 4 for the control group), suggesting that the objective of the challenge to make participants interact with one another was fulfilled. Finally, we find that take-up of the program was the same before the treatment was randomized, suggesting that both treatment and control groups had similar behavior in terms of interaction with the platform before they were randomly allocated to each group (last two columns of Table 4).

Finally, we note that individuals who completed a challenge pre-randomization were particularly more likely to answer the baseline (40 percent more) and the endline (20 percent more). Since both surveys were conducted within the platform, this seems logical. We will thus verify if our results are robust to focusing on that subgroup in the main analysis.

[11] A phone survey conducted a few months after our endline survey tracking the Rwandan secondary school entrepreneurship alumni from (Blimpo & Pugatch, 2021) had a similar attrition rate, suggesting widespread challenges contacting this population during the pandemic.

Table 3
Characteristics of those who did not answer the endline survey, by treatment assignment.

	Treatment	Attrition	Treatment x attrition	Obs.
Female	−0.02	−0.10	0.16*	1,632
	(0.11)	(0.07)	(0.09)	
Mobile	0.24	−0.11	−0.06	1,885
	(0.18)	(0.07)	(0.08)	
Email	−0.19	−0.05	−0.07	1,885
	(0.13)	(0.05)	(0.06)	
Age	−0.13	−1.19***	0.54	1,088
	(0.99)	(0.42)	(0.40)	
English	0.34***	0.09**	−0.14	1,635
	(0.10)	(0.04)	(0.08)	
Kinyarwanda	−0.34***	−0.09**	0.13	1,635
	(0.10)	(0.04)	(0.08)	
Senior 1	−0.03	0.02	0.04	1,638
	(0.03)	(0.01)	(0.04)	
Senior 2	−0.02	0.03	0.03	1,638
	(0.02)	(0.03)	(0.04)	
Senior 3	0.03	0.02	−0.03	1,638
	(0.06)	(0.04)	(0.06)	
Senior 4	0.03	0.07	−0.02	1,638
	(0.06)	(0.05)	(0.07)	
Senior 5	−0.01	−0.05	0.02	1,638
	(0.07)	(0.04)	(0.07)	
Senior 6	−0.00	−0.08*	−0.04	1,638
	(0.08)	(0.05)	(0.08)	
Num. Students (Senior 6, School)	−8.31	4.91*	4.08	1,702
	(12.89)	(2.57)	(7.00)	
Num. Students (School)	−68.41	14.35	2.47	1,702
	(20.90)	(8.13)	(22.11)	
F-test of joint significance			1.16	

Attrition takes value 1 for students that do not have endline data. The first three columns report the coefficients in a regression of the student report characteristic on strata fixed effects and dummies for being assigned to treatment, for not answering the endline and for the interaction of both dummies, respectively. The last column includes the sample size. Senior 1-6 refer to grades 7-12, respectively. The F-test is conducted by estimating all equations in a SURE framework and testing the equality of all interaction coefficients to 0. Significance level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

5. Results

5.1. Business and economic outcomes

We now turn to the effect of treatment assignment on business outcomes, the ultimate goal of the program. First, we study how being assigned to treatment impacted business ownership. As shown in the first column of Table 5, 28 percent of the control students group reported running their own business in the endline survey. The program increases this share by 19 percentage points, a very large effect. Not controlling for the baseline value increases our impact even further to 40 percentage points. Both effects are significant using conventional p-values. The specification without the baseline outcome remains significant when using the wild cluster bootstrap.

Table 4
Impact of random assignment on program participation.

	Completed at least one challenge		Proportions of challenges completed		Prop. of challenges pre-randomization	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.309***	0.546***	0.340***	0.650***	−0.01	−0.02
	(0.056)	(0.102)	(0.071)	(0.115)	(0.03)	(0.05)
Sample	All	Endline	All	Endline	All	Endline
Mean control	0.233	0.329	0.017	0.026	0.05	0.08
N. obs.	1885	1102	1885	1102	1885	1102
N of schools	133	65	133	65	133	65
Baseline variable	No	No	No	No	No	No
Add. controls	Yes	Yes	Yes	Yes	Yes	Yes

Regressions include strata fixed effects and control for all registration variables. Standard errors clustered at the school level are reported in parentheses. Significance level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 5
Impact of assignment to challenge on business ownership, weekly sales and profits.

					Conditional on having business	
	Runs own business (1)	(2)	Business sales (USD) (3)	Business profits (USD) (4)	Business sales (USD) (5)	Business profits (USD) (6)
Treatment	0.192***	0.399***	2.533***	1.442***	−0.761	1.575*
	(0.079)	(0.102)	(0.637)	(0.208)	(2.069)	(0.901)
	[0.206]	[0.082]	[0.030]	[0.003]	[0.743]	[0.322]
Mean control	0.280		1.143	0.545	5.504	2.415
N. obs.	1102		1024	1013	319	319
N. schools	65	65	65	65	39	39
Baseline variable	Yes	No	No	No	No	No
Add. controls	Yes	Yes	Yes	Yes	Yes	Yes

Regressions include strata fixed effects and control for all registration variables. We control for the baseline value of the outcome when available; we replace missing observations with 0 and add dummies for missing observations. Standard errors clustered at the school level are reported in parentheses, wild cluster bootstrap p-values reported in brackets. Significance level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

To explore whether this effect is due to the program leading to business creation or to business survival, we use a question in the endline survey about business ownership in December 2020. We find that the program effect on business ownership is not due to business creation but rather a higher probability of participants keeping their business alive (Fig. 1). Around 5 percent of treated and control participants created a business between December 2020 and April 2021. However, while only 20 percent of the control group kept their business alive from December 2020 to April 2021, this share was more than 30 percentage points higher in the treatment group.[12] Given that our intervention took place during the COVID-19 pandemic, we see these results as consistent with higher constraints for creating new firms, and large closure rates. The challenge might have encouraged students to sustain their business in a period of crisis.[13]

The next two columns of Table 5 show that those assigned to participate in the challenge experienced significantly larger sales and profits when measured unconditionally.[14] Sales and profits in the 7 days before the endline survey increased by 2.5 USD and 1.4 USD, respectively. These are small absolute numbers, but very large increases over the means in the control group (1.1 and 0.5 USD, respectively). They are statistically significant, even when using wild clustered bootstrap. Conditional on having a business, we observe a negative and not statistically significant effect for sales and a positive and only marginally statistically significant effect for profits (with a large standard error, no longer significant under the wild cluster bootstrap). This indicates that most of the increase in business outcomes was driven by the extensive margin: many more businesses were alive in the treatment group than

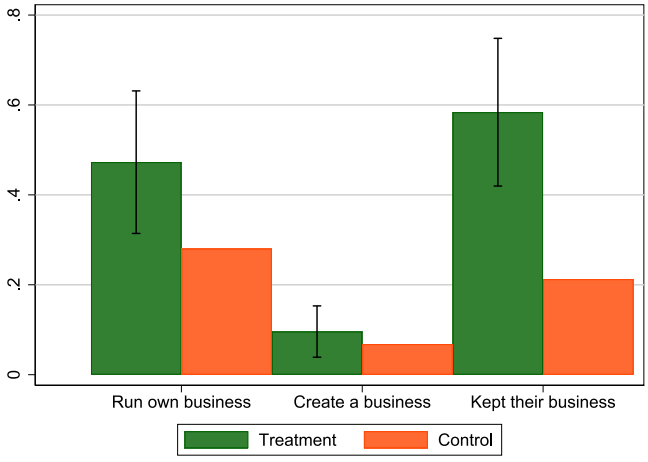

Fig. 1. Impact of assignment on business ownership.
Notes: The value reported as *Treatment* is the sum of the treatment effect and the control mean of the respective outcome measured in April 2021. The results on "Run own business" were obtained from a regression where the main outcome is a binary variable for having a business in April 2021 controlling for strata fixed effects and reported business ownership in December 2020. The results for "Create a business" and "Kept their business" were obtained from regressions that condition on not having a business in December 2020 and having a business in December 2020, respectively, controlling for strata fixed effects. Standard errors are clustered at the school level. *Control* displays the control mean. Both treatment effects on the probability of running a business and on the probability of keeping one's business are statistically significant at the 1 percent level. Wild-bootstrapped p-values are respectively 0.206, 0.634 and 0.046.

[12] The bars for "Run own business" in Fig. 1 were obtained from a regression where the main outcome is a binary variable for having a business in April 2021; whereas the bars for "Create a business" and "Kept their business" were obtained from regressions that condition on not having a business in December 2020 and having a business in December 2020, respectively. For this reason, the three categories in the figure do not add up to 100 percent.

[13] As shown in Appendix Table A3, results are very similar if we use the specification from our pre-analysis plan.

[14] The shares of missing values due to no reporting in sales are profits are not correlated with treatment.

Table 6
Impact on business club behavior.

	Business club participation		Business club revenue generation	Non agricultural business club	Agricultural business club
	(1)	(2)	(3)	(4)	(5)
Treatment	0.477***	0.532***	0.634***	0.189	0.253
	(0.112)	(0.126)	(0.117)	(0.131)	(0.152)
	[0.031]	[0.033]	[0.017]	[0.423]	[0.346]
Mean control	0.629		0.581	0.428	0.280
N. obs.	1102		1102	1102	1102
N. schools	65	65	65	65	65
Baseline variable	Yes	No	No	No	No
Add. controls	Yes	Yes	Yes	Yes	Yes

Regressions include school fixed effects and control for all registration variables. Standard errors clustered at the school level are reported in parentheses, wild clustered bootstrap p-values are reported in brackets. Significance level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

in the control group.[15] Interpreting these conditional results is complex, however, given that selection is endogenous to treatment.

Given that most high school students do not run their own business (i.e., an entrepreneurship rate of 28 percent in the control group, as shown in Table 5), we look at the effect of Wavuzumbi on alternative outcomes related to entrepreneurial activity. First, we focus on participation in school business clubs. Although business clubs are extracurricular activities, they are encouraged in the Rwandan secondary school entrepreneurship curriculum. They can be considered the entrance door to entrepreneurship.

Table 6 shows that while 63 percent of control students participated in a business club, the training increased this share by 48 percentage points.[16] This is a statistically significant result even when using wild clustered bootstrapped p-values. The effect size is even larger when we do not control for baseline variable, as shown in Column 2. Column 3 indicates that the increase in business club participation was driven by business clubs that generate some revenue. We fail to find effects on the prevalence of agricultural or non-agricultural business clubs (Columns 4–5), suggesting no effects of the program on business sector.[17]

We next explore whether this increase in entrepreneurship crowded out other economic activities. The first two columns of Table 7 show that the probability that a youth had a paid job or worked within a family firm doubled because of the program. The increases were 27 and 35 percentage points, respectively, and statistically significant using clustered standard errors and wild bootstrap procedures in the case of the first outcome. This indicates that participants increased their overall labor force participation. When we use as outcome a dummy for any economic activity (running a firm, working for a paid job or working in a family firm), we find an effect of more than 45 percentage points.

Overall, we see no evidence that the intervention led youth to substitute other economic activities to run their own business. On the contrary, the intervention encouraged participation in other economic activities.[18] This is notable given that our intervention took place during the COVID-19 pandemic and that overall economic opportunities appear to have been limited.

The higher degree of labor force participation resulted, as shown in the Columns 3 and 4 of Table 7, in very large relative increases in total earnings (including wage or business earnings) and hours worked. The effects are of 1.6 USD, and nearly six weekly working hours, respectively. While the effect in earnings seems small in absolute terms, it more than doubles the value of earnings in the control group. This is statistically significant using conventional clustered standard errors and, in the case of hours, using the wild cluster bootstrap. Conditional on working, we find a negative impact on earnings, which is not statistically significant, and a marginally significant increase in hours worked (albeit no longer significant under the wild cluster bootstrap). However, as we remarked above, we should take conditional effects with caution since the employed sample is selected, and these last two effects do not have a causal interpretation.[19] Again, these results seem to be driven by changes in the extensive margin, with treatment generating higher participation in economic activities.

This increased involvement in economic activities could have been detrimental to students' academic outcomes. While we do not have information on school grades, we asked about time spent in school activities. When using the specification from our analysis plan, we observe that participants are more likely to report devoting more hours than in the previous year to their studies (both on entrepreneurship and other classes). However, these results are not robust to adding controls.[20] Overall, we do not find strong evidence that the program crowded out school activities.

While those results suggest important gains from the program, one may worry about the high attrition rate we faced in the endline survey. As a robustness check, we contrast our results when looking at a group that had a higher participation rate in the baseline and endline, namely participants who completed at least one challenge pre-randomization. In Appendix Table A8, we present the results of all our main outcomes where the treatment assignment is included and is also interacted with a dummy for not having interacted with the platform before randomization. We also control for the main effect of having interacted with the platform prior to randomization. These results, while affected by lack of statistical power, suggest that even for the small group that participated in the platform before randomization and thus faced lower rates of attrition in the surveys, assignment to treatment led to better outcomes. We observe that for that group, participation in business clubs, weekly profits, the probability of having a paid job outside the family, weekly earnings and weekly hours are all higher when assigned to the treatment group compared to the control.[21] Overall, we interpret these results as indicative that our results do not seem to stem spuriously from non-response to our survey.

[15] Results are very similar when using the pre-analysis plan specification, except for the coefficient in the last column, as shown in Appendix Table A3.

[16] Because of the strata fixed effects and the additional controls, the mean of the treatment group does not correspond to the sum of the treatment effect and the mean of the control group. This explains why for some outcomes, the sum is negative or higher than 1 for binary outcomes.

[17] These results are robust to using the analysis plan specification as shown in Appendix Table A4.

[18] The fact that micro-entrepreneurship training can lead to higher labor force participation has been documented by Martínez A. et al. (2018) for adults.

[19] Appendix Table A5 shows that these results are robust to using our analysis plan specification instead of that adding registration data controls. Appendix Table A6 also show that they are robust to the exclusion of baseline controls.

[20] Results presented in Appendix Table A7.

[21] For some outcomes, the treatment*no participation interaction is positive. One potential explanation is that those who did not interact with the platform before randomization may have more room to improve their entrepreneurial skills.

Table 7
Impact on overall economic activity, hours and earnings in previous week.

					Conditional on employment	
	Paid job outside family (1)	Work in family firm (2)	Weekly earnings (USD) (3)	Weekly hours worked (4)	Weekly earnings (USD) (5)	Weekly hours worked (6)
Treatment	0.270***	0.347***	1.612**	5.667***	−0.775	3.114*
	(0.087)	(0.104)	(0.654)	(0.962)	(1.681)	(1.657)
	[0.032]	[0.164]	[0.221]	[0.010]	[0.742]	[0.158]
Mean control	0.212	0.314	1.389	1.539	5.725	6.048
N. obs.	1102	1102	1033	986	334	345
No. schools	65	65	65	65	38	41
Baseline variable	Yes	Yes	No	No	No	No
Add. controls	Yes	Yes	Yes	Yes	Yes	Yes

Regressions include strata fixed effects and control for all registration variables. We control for the baseline value of the outcome when available; we replace missing observations with 0 and add dummies for missing observations. Standard errors clustered at the school level are reported in parentheses and wild clustered bootstrap p-values reported in brackets. Significance level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

5.2. Heterogeneity

Given that the literature on entrepreneurship training and that of school interventions has found significant differences in outcomes by gender, we explore whether the impact of the challenge on girls was different from that on boys. The first two columns of Table 8 show that, overall, we see similar treatment effects for girls and boys. We find two outcomes where the difference in treatment effects is statistically significant using conventional p-values. We observe a larger effect for girls on running their own business and having a paid job outside of their family.[22] This suggests the program may have fostered more independence of girls in terms of economic activity. We interpret these results with caution, however, as the significance levels are only 10 percent and no longer significant when using the wild cluster bootstrap.

We next turn to heterogeneity by school type.[23] We study the differential program effects for students attending public schools compared to those attending private or government-aid schools, both of which charge fees. Public schools include about a third of all registered students; they typically enroll students from households of lower socioeconomic status. Among public school students in our sample, 46 percent have no parent or guardian with a secondary education, compared to 26 percent in non-public schools. In addition, control students from public schools are less likely to participate in a business club and to have Internet access, which could have limited their benefits from the program.[24]

Columns (3) and (4) of Table 8 show large differences in the way the program affected public school participants. With the exception of the effect on sales, profits, and earnings, all other interaction terms are positive and statistically significant using conventional p-values. Most effects persist as significant using the wild cluster bootstrap. Using conventional p-values, we see that the effect on running their own business is driven by public school students.[25] We also see much stronger treatment effects for public school students on participation in a business club, in particular one that earns revenue, and on working in a family business. Using conventional p-values, we also find stronger effects for public school students on having a job outside one's family and weekly hours worked.[26] As in the case of gender, we do not find that take-up was different for public school students, suggesting that their program effects are not driven by participating more in the program. This could indicate that the program particularly benefited students who were in a school environment where motivating entrepreneurship was limited by the school budget. However, since enrollment in public schools is correlated with socio-economic status in Rwanda, these results could also simply imply that poorer students benefited more from the intervention.

To test this, we conduct exploratory heterogeneity analysis based on the educational attainment of the student's guardian.[27] We compare students whose guardian has completed at least secondary education (65 percent of the sample) with the other students. The last two columns of Table 8 show little evidence of treatment effect heterogeneity in this dimension. Using conventional p-values, effects are statistically larger at 10 percent for participants with more educated guardians on weekly hours worked. However, we do not find statistically significant differences in the other outcomes. Overall, this warns us against interpreting the results on school type as simply measuring heterogeneity in effects by socio-economic status. The combination of both sets of results seems to suggest that the program distinctly affected participants in public schools, but not necessarily those from the lowest socio-economic background as measured by parental education. This could reflect greater effectiveness of the program where previous entrepreneurship education was most lacking.

5.3. Mechanisms

Having shown significant impact of the intervention on business and economic outcomes, in this section we explore the possible mechanisms. The program's theory of change posited that it would generate more entrepreneurial activity through three main channels: increasing the attractiveness of entrepreneurship, enhancing entrepreneurial abilities, and encouraging entrepreneurial actions with the final goal of running a business.

5.3.1. Entrepreneurial preferences

We first study whether the program changed students' preferences regarding entrepreneurship. In the top panel of Table 9, we show the impact of being assigned to the treatment on the student's favorite subject at school. We note a high preference for entrepreneurship classes,

[22] These differences are not driven by differential take-up of the program, which is not statistically different for girls and boys.

[23] The heterogeneity analyses by school type and parents' education were not specified in the pre-analysis plan. We add it as exploratory analysis based on our ex-post discussions with the Wavuzumbi team. We selected these two variables because they can be considered proxies for the socio-economic status of the students and the type of school environment without the program. These variables were also collected during registration or endline and thus available for the full sample.

[24] We do not have the school type for around 60 students, which reduces the sample size.

[25] On separate regressions we find that both business creation and survival increased in response to treatment for public school students, while only business survival increased for private school students. These results are available upon request.

[26] Results are similar when omitting baseline controls. See Table A9.

[27] This variable was not collected at registration, and we thus use the value reported at endline by students.

Table 8
Differential impact of assignment by student characteristics.

	By gender		By school type		By guardian's education	
	T	T*female	T	T*public	T	T* ≥ secondary
	(1)	(2)	(3)	(4)	(5)	(6)
Run own business	0.146	0.105*	−0.000	0.313**	0.164	0.078
	(0.095)	(0.061)	(0.065)	(0.117)	(0.102)	(0.111)
	[0.392]	[0.140]	[0.999]	[0.162]	[0.369]	[0.619]
Weekly sales (USD)	2.349***	0.386	2.048*	1.059	2.100**	1.062
	(0.702)	(0.848)	(1.075)	(1.095)	(0.826)	(0.916)
	[0.037]	[0.682]	[0.192]	[0.505]	[0.116]	[0.296]
Weekly profits (USD)	1.308***	0.299	1.750***	−0.287	1.326***	0.281
	(0.232)	(0.319)	(0.442)	(0.481)	(0.199)	(0.315)
	[0.002]	[0.490]	[0.158]	[0.709]	[0.002]	[0.480]
Part. in business club	0.493***	−0.035	0.179**	0.650***	0.512***	−0.083
	(0.124)	(0.082)	(0.074)	(0.063)	(0.125)	(0.086)
	[0.033]	[0.170]	[0.196]	[0.000]	[0.044]	[0.443]
Rev. generating bus. club	0.677***	−0.098	0.347***	0.581***	0.651***	−0.050
	(0.126)	(0.066)	(0.090)	(0.087)	(0.124)	(0.080)
	[0.012	[0.735]	[0.118]	[0.004]	[0.010]	[0.633]
Paid job outside family	0.214**	0.131*	0.174	0.259**	0.249***	0.032
	(0.101)	(0.070)	(0.117)	(0.112)	(0.092)	(0.087)
	[0.278]	[0.130]	[0.335]	[0.157]	[0.141]	[0.747]
Works in family business	0.356***	−0.025	0.053	0.489***	0.287**	0.146
	(0.117)	(0.080)	(0.133)	(0.143)	(0.126)	(0.095)
	[0.166]	[0.806]	[0.781]	[0.038]	[0.297]	[0.268]
Weekly earnings (USD)	1.629**	−0.036	0.464	1.981	1.455*	0.302
	[0.230]	[0.970]	[0.776]	[0.357]	[0.257]	[0.813]
	(0.713)	(0.697)	(1.237)	(1.349)	(0.801)	(1.019)
Weekly hours	5.592***	0.178	4.211***	2.989*	4.897***	1.759*
	(1.156)	(0.755)	(1.308)	(1.558)	(1.162)	(0.926)
	[0.032]	[0.826]	[0.116]	[0.242]	[0.044]	[0.156]

Regressions include strata fixed effects and control for all registration variables. We control for the baseline value of the outcome when available; we replace missing observations with 0 and add dummies for missing observations. Standard errors clustered at the school level are reported in parentheses and wild clustered bootstrap p-values reported in brackets. In columns (1) and (2), we interact treatment with a female dummy. In columns (3) and (4), we interact treatment with a dummy equal to one if the school is public. In columns (5) and (6), we interact treatment with a dummy equal to one if the guardian of the student has at least secondary schooling. The number of observations is 1,102 for most outcomes in the first and last set of regressions, while it is 1,045 for the middle columns, except for weekly sales (1024, 964), weekly profits (1,013 and 957), weekly earnings (1033, 977) and weekly hours (986, 931). Significance level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

with 64 percent of control group students reporting that their favorite subject at school was entrepreneurship. We see a very large program effect — of 30 percentage points — on this variable (significant with conventional inference, but not the wild cluster bootstrap). Our results suggest that participants switched from preferring history, science, and other subjects to entrepreneurship.[28]

The bottom panel of Table 9 looks at the effect on future career choices. Most students in the sample saw entrepreneurship as their future career (87 percent in the control group), and almost 90 percent anticipated it being their primary or secondary activity in five years. We find positive but not statistically significant treatment effects on these measures. Perhaps this is not surprising given that the high baseline levels implied little margin to change these outcomes.[29]

5.3.2. Entrepreneurial abilities

The program aimed to enhance entrepreneurial abilities, including cognitive skills, but focused mainly on socio-emotional skills to develop an entrepreneurial mindset.

We first look at whether the program increased knowledge of business concepts. Our endline survey included a question on opportunity costs and four questions related to topics covered in the challenge.[30] Table A12 shows a statistically significant impact on answering correctly the opportunity cost question. But this result is not robust to excluding controls.[31] We find no evidence that participants responded more accurately to topics covered in the challenge, perhaps due to familiarity with these concepts, which are also included in the regular secondary school entrepreneurship curriculum. The treatment effect is negative and becomes statistically significant when using our pre-analysis plan specification, as can be seen in Panel B of Table A12. Overall, we do not find strong evidence for changes in business knowledge.

The challenge targeted socio-emotional skills aimed at developing a successful entrepreneurial mindset, such as intellectual imagination, resilience, personal initiative, spirit of significance, and drive (Frese & Gielnik, 2014). We used questionnaires validated in other studies to measure the following soft skills: creativity, persistence, initiative, self-efficacy and locus of control (Bandura, 2006; Caliendo et al., 2016; Delavallade & Rouanet, 2020). We also included a measure of risk taking (Dohmen et al., 2011). We find no robust evidence of positive impact on any of these outcomes, as shown in Appendix Table A13. We actually find negative and significant coefficients for some of these outcomes. One hypothesis is that participants become more conscious of their shortcomings due to the challenges.

Overall, we do not find evidence that the program improved cognitive or non-cognitive abilities.

5.3.3. Entrepreneurial actions

The third channel that could have led to business creation and survival is an effect on business actions and practices. We first explore whether the program led participants to take more actions oriented at creating a business, and we then look at business practices.

[28] As shown in Appendix Table A10, we observe a similar shift away from history when using our analysis plan specification but the increase in entrepreneurship is not statistically significant.

[29] In our pre-analysis plan specification, we find similar results except that there is a marginally significant effect on entrepreneurship as a secondary activity. See Appendix Table A10. Table A11 presents results omitting baseline controls.

[30] Specifically, the questions related to the "gig economy", "making a dent in the universe", the "root cause analysis", and the "beachhead market."

[31] See Panel B of Table A12 for the results corresponding to our pre-analysis plan.

Table 9
Impact of assignment on entrepreneurial preferences.

	Treatment (1)	Mean control (2)	N.obs (3)
	Favorite topic at school		
Entrepreneurship	0.305***	0.640	1102
	(0.110)		
	[0.242]		
History	−0.101**	0.048	1102
	(0.043)		
	[0.331]		
Language	−0.022	0.048	1102
	(0.021)		
	[0.467]		
Math	0.065	0.062	1102
	(0.041)		
	[0.177]		
Science	−0.181***	0.139	1102
	(0.054)		
	[0.054]		
Other	−0.066*	0.062	1102
	(0.036)		
	[0.233]		
	Future aspirations		
Entrepreneurship as future career	0.069	0.867	1102
	(0.089)		
	[0.589]		
Entrepreneurship as primary activity in 5 years	0.051	0.898	1102
	(0.054)		
	[0.506]		
Entrepreneurship as secondary activity in 5 years	0.086	0.765	1102
	(0.108)		
	[0.504]		

Regressions include strata fixed effects and control for all registration variables. In the second panel, we control for the baseline value of the outcome when available; we replace missing observations with 0 and add dummies for missing observations. Standard errors clustered at the school level are reported in parentheses and wild clustered bootstrap p-values reported in brackets. Significance level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Fig. 2 presents program effects on four business actions: preparing a business plan, identifying a business opportunity, setting a business goal, and performing a financial analysis. These are usually considered actions required to transform a business idea into a successful business, and many of them were covered by specific challenges in the training. We see effects of the program on all these actions.

Fig. 2 next shows significant treatment effects on three recommended business practices: tracking business sales and costs, using technology and developing a market plan. These practices are positively correlated with business outcomes for businesses of different sizes around the world (McKenzie & Woodruff, 2017). All effects reported in the figure are significant using conventional p-values. Most are also significant when using the wild cluster bootstrap (p-values reported in figure notes). If we construct a standardized index including these three business practices, we find a large treatment effect of 0.5 standard deviations, which is statistically significant at the 1 percent level.

Finally, another strategy to foster entrepreneurship is to develop business networks. The program encouraged students to engage with each other, their teachers, and local entrepreneurs while they completed the challenge. Table 10 shows suggestive evidence that the training increased the likelihood of talking about business ideas with classmates (16 percentage points increase over a control mean of 49 percent), friends (16 percentage points over a control mean of 41 percent) and family members (33 percentage points over a control mean of 58 percent). Each of these effects is significant with conventional inference, though not when using the wild cluster bootstrap. We see no effects on talking with teachers or other adults, which might indicate no changes in exposure to adult entrepreneurs. Given the restrictions due to the COVID-19 pandemic and social distancing measures, it is possible that treated students found it easier to interact more with people of their age or family members than with adults and even with their teachers. While not statistically significant, the negative coefficient on interaction with teachers indicates that trained students might have substituted discussions with teachers with classmates and family interactions.[32]

6. Conclusions

We study the effects of an online gamified entrepreneurship training offered to Rwandan students enrolled in secondary schools. We find substantial short-term impacts of the training on business ownership and economic outcomes. Our estimated effect on entrepreneurship, 19 percentage points, greatly exceeds the largest effect of 6 percentage points reported in other youth entrepreneurship programs (Alaref et al., 2020; Blimpo & Pugatch, 2021; Chioda et al., 2021; Moberg, 2021). Similarly, our estimated effect on business profits is also positive and relatively large compared to the maximum of 32 percent reported in other programs (Chioda et al., 2021). Leading explanations for these differences are the moderating effect of the COVID-19 pandemic, and the short-term nature of our results.[33] Yet our results are also notable when considering the intervention coincided with significant pandemic-related disruptions in business activity.

We present evidence that these impacts operated through increased business-oriented actions, adoption of business practices, and networking. We hypothesize that this short and intensive gamified training motivated treated students to sustain their business activities during the COVID-19 pandemic. It is possible that we would have observed

[32] We obtain very similar results using the analysis plan specification, as shown in Appendix Table A14. Results are noisier but qualitatively similar when omitting baseline controls, as shown in Appendix Table A15.

[33] Our initial agreement with the implementing partner secured the possibility to run the intervention and to carry out the surveys presented in this paper. Unfortunately, we could not reach an agreement to implement a survey to study longer-term impacts with sufficient academic independence.

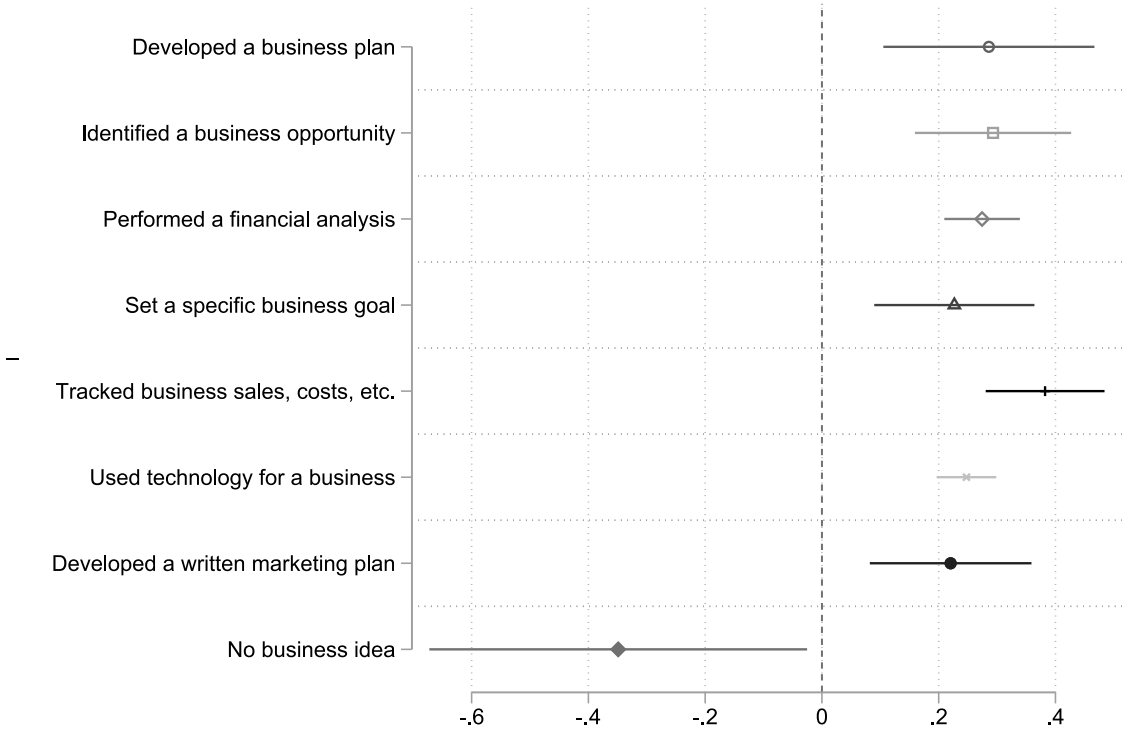

Fig. 2. Impact on business actions.
Notes: Graph shows the coefficient of a dummy for being assigned to receive the treatment in Eq. (1) where the dependent variables are dummies indicating each of the variables listed. Regressions include school fixed effects and control for all registration variables. Standard errors clustered at the school level are used to build the confidence intervals. The bootstrapped p-values corresponding to each estimate are 0.264, 0.019, 0.003, 0.006, 0.001, 0.109, 0.168, 0.304, respectively.

Table 10
Impact on business networks.

	Classmates	Friends that are not classmates	Other adults	Parent/Adult family members	Teachers
	(1)	(2)	(3)	(4)	(5)
Treatment	0.164**	0.163**	0.134	0.329***	−0.193
	(0.078)	(0.077)	(0.097)	(0.088)	(0.127)
	[0.165]	[0.172]	[0.385]	[0.066]	[0.428]
Mean control	0.490	0.408	0.445	0.581	0.731
N. obs.	1099	1099	1099	1099	1099
Baseline variable	Yes	Yes	Yes	Yes	Yes
Add. controls	Yes	Yes	Yes	Yes	Yes

Regressions include strata fixed effects and control for all registration variables. We control for the baseline value of the outcome when available; we replace missing observations with 0 and add dummies for missing observations. Standard errors clustered at the school level are reported in parentheses and wild clustered bootstrap p-values reported in brackets. Significance level: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

more business creation instead of business survival in a more amicable economic environment.

The fact that the program was unable to improve soft skills linked to successful entrepreneurship, one of its main goals, is worth highlighting. Training programs that introduce components on soft skills might be more effective at increasing overall enthusiasm and interest in entrepreneurship than programs focusing only on hard skills, even if they do not actually generate measurable changes in soft skills. This is a promising avenue for future research.

Was the program cost-effective? The implementer would not share cost data with us. However, as an online program the marginal cost of additional participants is likely low, potentially close to zero. Moreover, the main driver of higher costs in earlier entrepreneurship training programs is the individualized components, such as technical assistance, personal or group training, and on-site visits (Davies et al., 2024; Sonobe et al., 2012). For instance, in one NGO program, visits cost ten times as much as lighter interventions (Lafortune et al., 2018). The program we study excludes these individualized components, suggesting it could be cost-effective at scale, at least for the short-term results we document.

Our paper provides evidence that online training programs can motivate aspiring entrepreneurs in difficult times. Despite the potential dependence of our results on the challenging atmosphere faced by participants, this paper also brings valuable evidence indicating that online gamified training programs can be a promising way to scale up business training programs among students. Future work should investigate whether effects persist over time, to different samples (e.g., adult entrepreneurs), contexts, or more normal times.

Data availability

Data will be made available on request.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.econedurev.2024.102559.

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