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Digital vs. in-person financial education: What works best for Generation Z? ☆

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ABSTRACT

Nowadays, financial literacy is one of the most important skills that can be acquired by a tech-savvy Generation Z student. In order to understand what format of financial education works best for Generation Z, we set up an experiment that involved implementing a financial education program called "Futuro Sicuro" with a sample of 650 High School students in Italy. The program allowed us to gather data from two treatments at the class level, namely 1) a traditional financial education simplified program with the presence of a financial advisor, and 2) a digital financial education program using web-based applications based on learning-by-playing concepts. The two treatments were associated with different costs but showed similar effects: three weeks after their conclusion, we find that both courses did increase actual financial knowledge and the results also aligned with participants' realistic assessments of their own financial skills. A follow-up study also reveals the persistence of these effects three months later for the traditional course.

1. Introduction

With the advent of globalization and the development of technology, individuals have greater access to financial products and services but the financial decisions that individuals need to make are increasingly complex. To avoid financial mistakes, individuals need to be financially literate. In fact, higher levels of financial literacy are correlated with planning for retirement (Fornero and Monticone, 2011; Lusardi et al., 2017) , lower indebtedness and financial fragility (Lusardi et al., 2011;

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Lusardi and Mitchell, 2011; Van Rooij et al., 2012; Alessie et al., 2011; Klapper et al., 2012; Lusardi and Tufano, 2009, 2015; Clark et al., 2020) and lower overdrawing (Hasler et al., 2018; Lusardi et al., 2018). Despite institutions and policy makers recognizing the need to improve financial literacy levels worldwide, the best way to achieve it is far from clear. How to improve financial literacy is an even more important question in light of the Covid-19 pandemic, which has impacted both financial stability and traditional learning environments.

In this paper, we contribute to the literature by presenting the results of a randomized control trial (RCT) called "Futuro Sicuro" financial education program, conducted in a large school in the south of Italy and involving high school students of 16–18 years. In particular, this is the first experiment that randomizes delivery methods of two compulsory courses on financial education given to students. The first delivery method consists of four in-person lessons by a teacher and financial advisor using slides and the role-of-thumb approach to summarize the content of each lesson following Drexler et al. (2014). The second delivery method consists of a highly digital course on the "Kahoot" platform following with the same number of lessons which gave the students the possibility to answer short quizzes on an interactive multimedia whiteboard accessed via their smartphones. Students in the digital course were able to verify and improve their knowledge by watching short videos freely available on the official website of the Museum of Savings (Museo del Risparmio).[1] The experiment tracked students' financial literacy outcomes before and after the courses together to a control group which was not exposed to any course. Both courses were delivered during school hours, allowing us to reach students enrolled in both courses without dispersion, discrimination for the online setting or attrition.

Our experiment provides the first impact evaluation on courses' delivery methods—digital vs. in-person—and at least two key findings. First, our results indicate a positive effect of financial education on financial literacy. After any course, the treated students displayed improved levels of financial literacy and greater financial literacy than the control group. In particular, on average, the probability to become financially literate for students in the traditional course increases by 28 percentage point (p.p.) after three weeks and by 34 p.p. after three months, despite the difference in these effects is not statistically significant.[2] By contrast, the digital course shows a positive and weakly statistically significant marginal effect only in the short-run. The digital course increases the probability to become financially literate by 13 p.p. after three weeks, losing statistical significance three months later. Second, following the course, self-assessed financial literacy fell, which suggests better alignment with actual financial literacy. In fact, the probability to self-assess medium or high financial knowledge level increases three weeks (50 p.p.) and three months (46 p.p.) after the treatment, on average, among students who attended the traditional course. However, female students are less confident than their male peers. In particular, females are 18 p.p. less likely to self-assess medium or high financial knowledge. This result is interesting in the context of overconfidence and investment choice. Overall, our findings suggest that the digital course may be a good option to improve financial literacy in the short run but the more expensive traditional course might be more effective over a longer time frame.

We also compute the cost-effectiveness ratio for the traditional treatment which would be equal to 0.02 (0.28/11.04) if the financial advisors were paid for their time. In other words, each additional euro spent on the traditional course increases by 2% the probability that students get all the Big Three[3] questions right. To have the same increase in probability to become financially literate, for the digital course the amount of euros invested would be equal to 6.5 per student. In our program, neither the traditional course nor the digital one had required additional implementation costs. We stress that a traditional course is more expensive. Hence its feasibility depends on the available budget.

For financial education programs, the results are mixed and potentially driven by heterogeneity in the timing and quality of intervention (Brown et al., 2016; Cole et al., 2016). The finding shows that financial education program attendees increase on average both their financial knowledge and financial behavior (indebtedness or long-term savings). Results are irrespective of student age and similar to the improvement rate in other domains (Cheung and Slavin, 2016). Kaiser and Menkhoff (2019) also find that the intensity of programs affects outcomes and declining marginal returns of education. Kaiser et al. (2020) find economically meaningful treatment effects both on knowledge and behavior. Their results are comparable to other domains and are at least three times as large as the average effect reported in Fernandes et al. (2014).

From a traditional delivery perspective, other studies focused on a similar target. Bruhn et al. (2016) implement an RCT in Brazil among 25,000 students enrolled in the last year of High School. They find mixed results, positive impacts on saving (12.5%) and budgeting (21%) but negative effects on borrowing (2.9% in the short run and 3.7% in the long run) and purchasing (1.4%). They interpret this "perverse" effect as a "multitasking problem" and the lack of a clear direction in purchasing behavior in their course books. Nevertheless, they highlight an additional positive result, which translates into parents' spillover effect. Improved savings are also found by Frisancho (2021) in a traditional financial education program called "Fi-

[1] The Museum of Savings is not a traditional museum; it is a place in which technology, interactivity and games are used for educational purposes. In particular, "edutainment" and gamification are used to spread financial literacy. Videos are available at the following link http://www.museodelrisparmio.it/edupop-la-nuova-educazione-finanziaria/ (last visited on November 26, 2021).

[2] Running a t-test on the difference between the share of correct responses after three months versus after three weeks, we find no statistical significance (Wald test, $p = 0.4542$). Hence, we cannot reject the null that the two percentages are equal.

[3] To measure financial literacy, Lusardi and Mitchell (2007) created three simple and basic questions to capture the fundamentals of personal finance. These questions are known worldwide as the Big Three and investigate how people deal with inflation, compound interest, and risk diversification concepts necessary for financial decisions. These questions revealed that knowledge is poor throughout the world (with only 2.1 percent of countries qualifying as top performers), particularly among the young, women and the elderly (OECD, 2014). The Big Three questions are mainly used to assess financial literacy among adults. Due to higher comparability with several national and international surveys and our target group's age, we follow Lusardi and Mitchell's (2007) approach.

nanzas en mi Colegio" (Frisancho, 2021), conducted on a large scale using counterfactual techniques and a comprehensive evaluation strategy. It involves 300 schools in Peru and the main results are increased knowledge (0.14 SD), awareness (0.11 SD), and savings behavior both for students and teachers (0.09 SD). In addition, similarly to our results, they find that their intervention is cost-effective. In fact, for a sample of 31,000 students in 150 schools, they spend 6.6 USD per student and the cost-effectiveness ratio is 0.021.

Frisancho (2021) finds a positive impact of short training sessions on financial attitudes such as interest in financial matters and saving propensity. In the north of Italy, Brugiavini et al. (2018), focusing on 579 university students, also find that a short one-day course increased more self-assessed financial literacy compared to the actual increase in knowledge (0.046 for "Inflation" after the course, 0.176 for "Compound interest" and no effect for the "Diversification" outcome. However, the number of correct answers after the course increased by 0.229). By contrast, Becchetti et al. (2013) and Cole et al. (2016) find no statistically significant effect of the treatment on financial literacy, arguing that increasing math hours in school positively impacted both asset accumulation and credit management.

Considering different delivery methods, our digital course is unique in its delivery. This differs from previous work, contributing to the innovative literature on financial education (see Hinojosa et al., 2009; Escueta et al., 2020; Batty et al., 2020). Hinojosa et al. (2009) include student participation in a Stock Market Game simulating real financial markets involving a large sample of schools competing at the class and state level and lasting 10 to 15 weeks of the school year. Due to the particular design of the game and the improvement in math knowledge, these authors find a positive impact on students in all grades (4th to 12th) in relation to concepts of saving and investing. However, our short financial education program is not designed to improve math knowledge but to improve experiential learning and financial literacy (see Table A1). An experiential financial education program is used by Batty et al. (2020) in a primary school. They propose an RCT with five in-class well-targeted financial education lessons integrated into other curricula (math, social studies, languages, arts) in primary schools. However, their lessons are shorter than the Futuro Sicuro program design (45 min each) and focus only on savings, financial decision-making, and money management. Instructors in this program are mainly teachers who attended three-hour training sessions. Batty et al. (2020) support implementing a well-targeted brief experiential financial education program which is a cost-effective strategy to teach financial literacy.

To sum up, our paper aims to investigate how to improve Gen Z's financial literacy through the "Futuro Sicuro" financial education program, comparing two courses similar in terms of their content, duration, and goals but very different in the method of dissemination and instructor ability.

This paper contributes to the literature in three ways. First, it investigates the impact of digital vs. in-person delivery of financial education on confidence and knowledge. Second, it examines one of the most financially vulnerable groups: Generation Z (i.e., young adults between 16 and 18 years).[4] Third, it represents the first randomized controlled trial (RCT) targeting Generation Z in Italy,[5] which is the only country with a statistically significant financial literacy gender gap among 15-year-old students ((OECD, 2017).

The main limitations of the paper are lack of any financial behaviors and small sample size compared to other financial education programs in the literature. However, a sensitivity power calculation in the Appendix reveals enough power to support the reliability of our results on knowledge and confidence.

The paper is organized as follows. Section 2 describes our field experiment, including its context, our research design, and our methodology. Section 3 provides a description of our data. Section 4 explains the estimation strategy and discusses the main results (both for financial literacy and self-assessed financial literacy with a deep analysis on the do-not-know option) and provides insights for policy about cost-effectiveness analysis. Section 5 concludes.

2. The field experiment

Having reviewed all the initiatives of banking and non-banking parties (Franceschi et al., 2017; Kaiser and Menkhoff, 2017; Kaiser and Menkhoff, 2019; Frisancho, 2019), we develop a program in line with what has already been done for extrinsic parameters (such as mean duration and general content) but innovative in methods. We conduct clustered-randomization at the classroom level in the last two grades (the fourth and fifth grades corresponding to US twelfth and thirteenth grades respectively) of a scientific high school in Reggio Calabria. We chose a large school, which enabled us to include two very similar treated groups and a large control group. This school has 19 fifth grade classes and 15 fourth grade classes, allowing us to enlarge the sample as much as possible, including students with the same family and school-based characteristics. The average class size is 19 students. The choice is similar to other Scientific High Schools in similar cities in Northern and Central Italy through the website www.eduscopio.it[6]. In addition, we compare administrative data

[4] According to several international evidence, Generation Z is a financially vulnerable group due to their low level of financial literacy and lower financial exposure as well. Gen Zers are digital natives who seek out information via social networks and the Internet, often trusting friends more than the experts. Moreover, Generation Z is on the cusp of making financial decisions that can critically affect their future. Just-in-time financial education is not enough. They need to be prepared well in advance, so it is important to understand how to effectively increase Gen Z's financial knowledge and confidence.

[5] The South of Italy setting makes it even more noteworthy due to the higher vulnerability of people leaving in the Southern regions.

[6] The school comparison is also based on the following data of the chosen school: an ability index (69), which aggregate the normalized average grades on a scale from 0 to 100 (24) and school credits (68), finally we also compare school taking into account the percentage of graduate students (80). All data refer to the field experiment period, 2018.

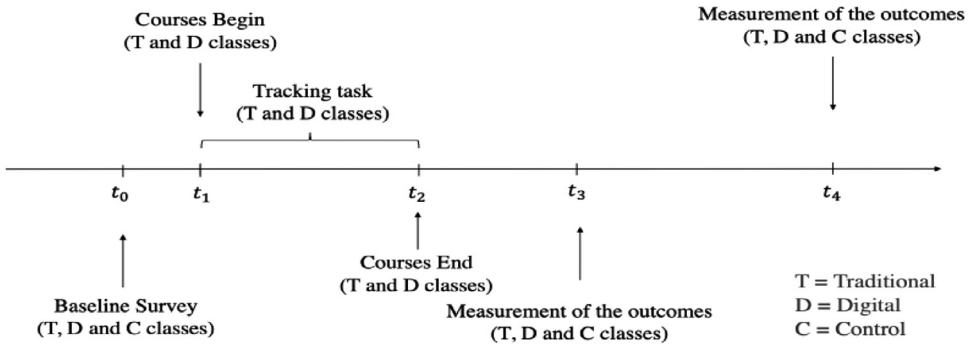

Fig. 1. Study Timeline.

about teachers and students' characteristics (school size, gender balance, and student-teacher ratio, see Appendix Table A2). We find very similar values not only in relation to the percentage of regular graduate students and average student knowledge (grades) but also in school size, gender balance of the student body, and teachers' characteristics. We compare these data with two scientific high schools in Verona and Pescara.[7] We can assume that the chosen school's characteristics are in line with the average Italian teachers' characteristics from OECD report (2020b), supporting the external validity of our results. Due to the impossibility of stopping people from interacting in the school, our design suffers from potential contamination effects. However, any contamination effect should increase the knowledge of the control treatment, thus biasing the results of the control upwards. Hence our treatment effects should be considered as a lower bound of the potential effect of the course.

High school students in Italy are a relevant target group for financial literacy courses for several reasons. First, the age range (between 16 and 18) is of interest for examining the impact of long-term early-impact targeted policy measures. In Italy, just 36.1% of 15-year-olds talk about financial topics compared to an average of 51.5% in OECD countries (OECD, 2019, 2020a). In addition, teenagers who receive financial education from their families are better at managing their wealth in adulthood (Bucciol and Zarri, 2019; Bucciol et al., 2018; Bucciol and Veronesi, 2014; Fornero et al., 2019; Montanaro and Romagnoli, 2016; Sundarasen et al., 2016; Otto and Webley, 2016; Bernheim et al., 2001). In addition, high school students are not yet economically independent, have lower financial exposure than university students, and are highly motivated to attain financial independence as soon as possible. Their intrinsic motivation encourages meaningful engagement with a financial education program.

The experiment involves 6 treated and 28 untreated classes, chosen randomly. Treatment is exposure to the financial course, either traditional or digital (henceforth, T and D). We randomly assign different treatments (T and D) to 4th and 5th grade classes (12th and 13th level of the US system, respectively). From our balancing test between groups (see Table 1), we can safely assume that randomization works well. We describe the study timeline in Fig. 1. Our data collection and experimental study is facilitated by the fact that students in our sample are enrolled in a school that randomly assigns them to different classes and the school stores personal details and marks for each student and each subject.

Before the start of the course (t_0 in Fig. 1), we measure the level of financial literacy with a questionnaire for students in T, D, and the untreated classes (henceforth, C). Students are required to answer questions about their personal characteristics and family background in the first section, questions about their financial habits (such as saving habits and use of prepaid cards) and risk aversion in the second section,[8] and questions that measure their financial literacy (inflation, interest and diversification) in the final section. We administer the baseline questionnaire the week before the start of the courses. Due to the number of classes, it is not possible to administer the questionnaire to all classes at the same time, but we survey all the classes before assigning the treatments.

One week after the survey (which takes place in t_0, see Fig. 1), courses T and D start (t_1), with one two-hour lesson each week for one month (until t_2). Attendance is compulsory for both courses because they are in school hours. We ask students in T, D, and C (untreated classes) to fill in the same questionnaire again three weeks after the end of the course (t_3). We repeat the procedure (i.e., asking students to fill in the questionnaire) three months later (t_4) for all classes.

Following Drexler et al. (2014), we support the idea that simple explanations help the learning process. In our setting, students in the traditional course have a two-hour financial education lesson with a professional financial advisor. The financial expert uses slides to explain the main aim of the lesson, then presents a case study with two potential scenarios, one to follow and one to avoid. On the left of the slide, a green box with thumbs up indicates the best choice in the

[7] We have randomly chosen some cities similar to Reggio Calabria from the North and the South of Italy which Scientific High schools were available on Eduscopio. The first two schools which were able to provide the information required have been included in this comparison.

[8] Question 1: "You win the lottery and an amount corresponding to your family's annual income is paid to you over a year. How much would you give up to receive the remaining amount immediately?a) I would give up 20% b) I would give up 10% c) I would give up 5% d) I would give up only 2%"Question 2: "From the following options, which would you choose?a) a certain gain of 5% b) flip a coin to gain 10% if it comes down heads, 0 if it comes down tails".

Table 1
Balancing test – personal characteristics and financial literacy.

	(1) $T+D$	(2) T	(3) D	(4) C	(5) Diff1	(6) Diff2	(7) Diff3	(8) Diff4
VARIABLES	Classes	Classes	Classes	Classes	(2)-(4)	(3)-(4)	(2)-(3)	(1)-(4)
Female	0.524	0.459	0.587	0.472	−0.013	0.116*	−0.128	0.052
	(0.501)	(0.502)	(0.496)	(0.500)	(0.067)	(0.066)	(0.089)	(0.050)
Age	16.59	16.58	16.60	16.82	−0.238***	−0.217***	−0.019	−0.227***
	(0.555)	(0.561)	(0.555)	(0.529)	(0.076)	(0.073)	(0.1)	(0.055)
Wealthier_students	0.629	0.661	0.6	0.601	0.060	−0.001	0.061	0.027
	(0.485)	(0.477)	(0.494)	(0.490)	(0.066)	(0.065)	(0.087)	(0.049)
Foreign	1	1	1	0.982	0.017***	0.017***	0	0.017***
	(0)	(0)	(0)	(0.130)	(0.005)	(0.006)	(0)	(0.005)
Foreign_parents	0.968	0.967	0.969	0.94	0.026	0.028	−0.003	0.027
	(0.176)	(0.181)	(0.174)	(0.237)	(0.025)	(0.024)	(0.031)	(0.018)
Only_child	0.166	0.147	0.184	0.131	0.017	0.053	−0.037	0.035
	(0.374)	(0.357)	(0.391)	(0.337)	(0.047)	(0.050)	(0.050)	(0.036)
City_center	0.861	0.88	0.844	0.889	−0.007	−0.045	0.037	−0.027
	(0.346)	(0.326)	(0.366)	(0.314)	(0.044)	(0.047)	(0.062)	(0.034)
Repeating_students	0.008	0.016	0	0.018	−0.001	−0.018***	0.016	−0.009
	(0.090)	(0.128)	(0)	(0.133)	(0.017)	(0.006)	(0.016)	(0.010)
Perspective_Eco_Stud	0.15	0.193	0.111	0.116	0.077	−0.005	0.081	0.033
	(0.358)	(0.398)	(0.316)	(0.320)	(0.054)	(0.042)	(0.066)	(0.035)
Father's_degree	0.369	0.339	0.4	0.321	0.018	0.078	−0.061	0.048
	(0.484)	(0.477)	(0.494)	(0.467)	(0.065)	(0.067)	(0.089)	(0.048)
Mother's_degree	0.418	0.439	0.4	0.357	0.082	0.043	0.038	0.061
	(0.495)	(0.501)	(0.493)	(0.480)	(0.069)	(0.064)	(0.090)	(0.049)
Self-employed_father	0.411	0.517	0.312	0.343	0.173**	−0.032	0.205	0.068
	(0.494)	(0.504)	(0.467)	(0.475)	(0.069)	(0.063)	(0.089)	(0.049)
Self-employed_mother	0.11	0.069	0.15	0.2	−0.131***	−0.05	−0.081	−0.089***
	(0.314)	(0.255)	(0.360)	(0.4)	(0.038)	(0.050)	(0.057)	(0.034)
FinLit_baseline	0.304	0.229	0.375	0.313	−0.084	0.061	−0.145*	−0.009
	(0.461)	(0.424)	(0.487)	(0.464)	(0.057)	(0.064)	(0.081)	(0.046)
Observations	126	61	65	524	585	589	126	650

Note: Table 1 reports personal characteristics for the whole treated groups (column 1), and each treated group separately (T Classes in column 2 and D Classes in column 3), as well as for the control group (C Classes in column 4), that students' personal characteristics are quite balanced. In particular, mean and standard deviations are reported in brackets. The "Diff" columns report the output of a *t*-test on the equality of the mean in different groups. Numbers with *** indicate significance at the 1 percent level, ** indicates significance at the 5 percent level, and * indicates significance at the 10 percent level. Although the differences in age between both groups of treated and the control group are statistically significant, the size of this difference amounts to just a few months, so there is no reason to believe that this particular difference would distort our results in any way. The same can be said of the difference in repeating the school year or the difference in the number of foreign students between T and C groups. However, the statistically significant differences that emerge from the comparison between the traditionally treated and the control group regarding parents' self-employment are controlled for in the analyses. The difference may have resulted from the large difference in sample size between the treated and control groups.

particular setting. On the right, a red box with thumbs down gives details of a case not to be followed. This trick allows the financial advisor to introduce story-telling based on student experience and to increase student awareness in relation to diverse scenarios. The aim of the traditional course is to increase financial literacy in a standardized, replicable manner with in-person teacher interaction.

In our setting, the first lesson of the traditional course is a motivational lecture about the importance of increasing human capital to increase awareness and involvement in the learning process as a whole, not only for the course. The task is explained (using a balance sheet) introducing the importance of planning in wealth management as well as in everyday life. We use slides to keep things simple and to allow the financial advisor to introduce examples focused on investments (i.e., planning, diversification and so on). Price and inflation are the main topics of the second lesson. The third lesson is about savings and payment instruments, taking into account inflation and refreshing the concept of simple interest calculations. After an overview of financial markets, the fourth and last lessons are a brief introduction to mortgages, insurance, and retirement. The traditional lessons are based on the same freely available material published by the Museum of Savings.

The digital financial education program has the same aim but without the live interactions with the instructors and increased social peer interaction in a computer-assisted learning environment (see Escueta et al., 2020). Although the digital course was not designed for emergencies such as Covid-19, its scalability is well suited to respond to situations where in-person teaching is impossible, such as during the pandemic. To the best of our knowledge, this is the first short course in financial literacy that uses this particular digital methodology and that covers all the major wealth management topics and the only study focused on the effect of learning-by-playing concepts applied to financial education for High School students.

Several scholars argue that by playing, children retain knowledge (Dau, 1999; Levin, 1996; Samuelsson et al., 2008). Despite the popularity of learn-by-playing theory, the definition of play differs across scholars in the literature. Nevertheless, some criteria are common and can be used to define what playing means: intrinsic motivation, attention to the process rather than products, familiar object, "as if" activity, free from public roles, and active engagement (Takhvar, 1988).

Levin (1996) claims that during play, children become aware of what they know and take control of what is happening. In addition, we follow the principles enshrined in the Swedish preschool curriculum ((Ministry of Education and Sciences in Sweden, 1998) since the online environment is joyful, gives students a feeling of belonging and mixes communication, play and learning as a whole during the activities. Finally, Johansson and Pramling Samuelsson, 2006 argue that in such an environment, children exchange their ideas learning from each other through discussion, argument and exploration.

Our digital program includes videos showing concrete experience, simulations and digital quizzes based on "hands-on" type pedagogy. Since the aim of the digital course is to learn by entertainment, we show a video on human capital and explained details of the course and the task structure. This differs from previous work, contributing to the innovative literature on financial education (see Hinojosa et al., 2009; Escueta et al., 2020; Batty et al., 2020). In particular, our digital course lets students play using their smartphone as a pushbutton transforming the lesson into a class competition, increasing their active engagement (Takhvar, 1988). Competition is a crucial element in a learn-by-playing environment. This course involves an interactive lesson approach via the Kahoot platform, which is familiar to the students. We then administer the first short quiz (10 questions) about financial planning entertaining the students into an "as if" activity. The quiz is projected onto an interactive multimedia whiteboard. Using computers or smartphones as pushbuttons, students join by choosing a nickname and entering a unique code (randomly generated automatically every time a quiz starts). It increases competition and intrinsic motivation, showing the countdown to the start and nicknames put in order of merit on the screen. Moreover, the sound, efficient educational and instructional design of the quizzes motivates and engages students to focus on what they already know about financial topics from their personal experience with their family and in social life (focusing on process and not on products). Short videos on financial planning, freely available on the official website of the Museum of Savings, are projected after each quiz. A Q&A section follows at the end of each lesson. Our results for the digital course are less strong than for the traditional course but are still positive and useful, sufficiently so to be taken into consideration in the increasingly online-learning environment.

Additionally, we require each treatment group to track their expenses for the duration of the course.[9] We hand out a pre-printed sheet to the T treatment course. We ask the digital course students to download a free app called 70.20.10 (available for both iOS and Android devices until October 2019) to keep track of their expenses. For both, we set the deadline to complete the task at the end of the course. This task allows us to evaluate the degree of student participation. Furthermore, it enables us to understand which tracking method is more effective for Generation Z. Finally, it tells us whether these methods succeeded in changing savings habits, interest and consequently knowledge. In order to increase participation, we provide a prize consisting of a handmade credit card holder (valued at around 10 EUR) to those who are able to achieve the course's requirements.

Teaching-to-the-test is one of the most frequent doubts that commonly arise about education impact evaluation's results. We argue that our design does not suffer from this kind of limitation for several reasons. First of all, the digital content is standard and even if it includes contents needed to correctly answers the Big Three, it does not stress them more than other topics covered in the videos. Second, we ensure that neither financial advisors nor the school staff nor the students knew they were being evaluated and how, neither the frequency of questionnaire administration. Moreover, financial advisors had never seen the questionnaire and, more importantly, the evaluation happened in their absence. Besides, volunteers received no incentive. They are intrinsically motivated to spread their knowledge. Third, we ensure that school staff and teachers had very limited contact with instructors and researchers. The main information provided only directly to the headteacher of the school is that we would like to spread financial literacy among their students completing a study by the University of Messina. We ask only the headteacher permission to survey another time the whole school the same day as the test administration, twice. They were not aware of the design of our field experiment, but they learned it by the direct experience. Finally, the test includes several questions and no one knew which one would be taken under consideration to measure their knowledge. For these reasons we are confident that they had no key information to support teaching-to-the-test behavior.

Some teachers would be interested in including our courses in the school mandatory program but they are also aware that they are time-constrained and subject to the ministerial program. Moreover, they know that they have neither the expertise nor the time to include autonomously additional lessons or applications during their school hours. Furthermore, we do not provide them with the material used for the lessons. Hence, we are confident that teachers' effort does not change after the course.

The design of the field experiment allows us to test two main hypotheses. The first is the effectiveness of the financial courses. The second is whether any change occurs in Self-assessed Financial Literacy with potential implication for a gender effect.

3. Data description

The sample comprises a total of 650 students, 126 treated (61 in T treatment, 65 in D treatment) and 524 untreated. Before starting the courses, we collected data through a paper-based questionnaire following Lusardi and Mitchell (2014) ap-

[9] These extensions are then part of a bundled treatment. Given that we used different technologies, we create a challenge. It could be that the recording strategies and the teaching method itself influenced willingness to record. As we cannot disentangle these two effects, retrospectively, we decide not to analyze these data.

proach – Simplicity, Relevance, Brevity and Capacity to Differentiate. The questionnaire is given to treated students (in the T and D classes) and to the students in the untreated classes (C).

The questionnaire is divided into three sections. The first is related to family background and personal characteristics (gender, age, nationality, number of brothers/sisters, University preference, Italian, math and English grades, income, parents' education, parents' job, distance from home to the city center, students repeating the year). Part of these variables are used as control variables in our analyses. The second section comprises items about behavior and attitudes (receiving pocket money, savings attitudes, risk attitudes, saving instruments and intertemporal choice preferences). However, these data are self-reported and show low variability. We decide not to use them. The main outcomes of interest are in Section 3, where financial knowledge is investigated. We focus our attention on the three questions commonly used to measure financial literacy (previously cited as the Big Three).

The same financial literacy questions are asked (but arranged in a different order, as suggested by Lusardi and Mitchell, 2017; and Van Rooij et al., 2011) three weeks and three months afterwards, for all classes. We collect three observations for each student for each variable in Sections 2 and 3 of the questionnaires. In addition, new questions are aimed at understanding if the treatments have any positive externalities in their everyday life. After discussing practical examples from daily life with the help of financial experts or through a learning-by-playing approach, we also investigate any spillover effect of the courses on families. Finally, by comparing answers from the control group and the treated groups in the second and the third wave, we ascertain the effects of learning.

We create the dummy variable "FinLit Big Three" as an indicator of students' financial knowledge for each period (before, after three weeks and three months later). It takes value 1 if students answer all three questions correctly, 0 otherwise. Another outcome of interest concerns perceived financial literacy. To investigate changes in student awareness of financial literacy we run the same analysis using the dummy "Self-assessed FinLit" as dependent variable with value 1 if students self-assessed their level of financial knowledge as medium or high, 0 otherwise. Finally, we investigate the "do-not-know" option as a driver of female confidence, changing the dependent variable by using the dummy "Do-not-know option" with value 1 if at least in one out of three questions students choose the "Do-not-know" option, 0 otherwise.

Section 4 sets out the analyses and results. Additional details are available in the Appendix.

3.1. Descriptive statistics

As mentioned before, there are two treated groups and one control group. Students in the treated groups receive either training D (digital course) or T (traditional course). Students in the control group C do not receive any training but took part in the survey. The experiment is conducted at the class level. In other words, in some classes all the students are treated and in others none are treated.

The identifying assumption in our experiment is that students are assigned to treatment D or T (or to the control group) randomly. We stratify by grade and then do a simple randomization of the 34 classes in each treatment arm and the control. Since randomization is done at the class level with a few classes, this assumption is equivalent to the random assignment of students into different classes. Conversations with the principal of the school and the teachers reveal that students are indeed allocated to different classes randomly so no classes should have a disproportionate number of high-ability students. To support our assumption, we conduct a balancing test showing for the three groups (T, D or Control) the average characteristics of all observables at the student level. Unfortunately, budget and time constraints do not allow us to extend the treatment to other classes of the treated group, balancing the control group size. Coherently with this, we find balanced observable characteristics of students both in the treated and untreated classes.

Table 1 shows that students' personal characteristics are quite balanced. We also collect data about abilities in subjects related to this field and control for math ability. Table 1 bears out the assumption that financial knowledge among students is balanced before the course. Columns 1 to 4 reports mean and standard deviation for each treated (or as a whole) and control group. Columns 5 to 8 report the output of a *t*-test on the equality of the mean across different groups. Some differences emerge. Although the differences in age between both groups of treated and control are statistically significant, the size of this difference amounts to just a few months, so there is no reason to believe that this particular difference would distort our results in any way. The same can be said of the difference in repeating the school year or the difference in the number of foreign students. However, the statistically significant differences that emerge from the comparison between the T and C groups regarding parents' self-employment are controlled for in the analyses. The difference may have resulted from the large difference in sample size between T, D and C groups. The D group seems to be slightly more financial literate and with higher presence of female students compared to the T group, but any difference emerged in this case compared to the C group. Section 4 presents several analyses and results based on the above data.

4. Results

We use a three-step analysis in exploring the real financial knowledge data that we collected, as shown in Sections 4.2, 4.3 and 4.4. The first step is the main analysis of the Big Three questions three weeks and three months after the course. To this end, Section 4.1 details results from a diff-in-diff probit regression in which we control for several observable characteristics. We then extend the diff-in-diff probit model to analyze the effects of the treatment on self-assessed financial liter-

acy, including consideration of "do-not-know" responses of female students. The section concludes with a cost-effectiveness analysis.

4.1. Estimation strategy

Considering some statistical differences between the treatment and control groups, as shown in Table 1, we cannot exclude any differential impact of the courses for those students at that stage. Moreover, we cannot exclude students' classroom changes in the years prior to the program's implementation. This is important because if many students wanted to self-select in the classroom X, then, higher abilities may result in some classes. For these reasons, and because of our particular experimental design, a diff-in-diff analysis is the preferred option. With this approach we can relax the assumption that students in T, D, and C classes are randomly allocated. In particular, while the diff-in-diff design can account for differences in fixed characteristics, the key hypothesis is the absence of different trends for the treated and untreated students affecting their level of financial literacy.

We estimate the following model (1). The treatment dummies, D and T, are equal to 1 if a student $_{i}$ is in class $_{j}$ treated with D or T, and 0 otherwise, and y_{ij} is the outcome of interest ("FinLit Big Three", "Self-assessed FinLit," and "Do-not-know Option"). We estimate the variation of the model as follows:

$$y_{ij} = \alpha + \beta_1 D_{ij} + \beta_2 P1_{ij} + \beta_3 P2_{ij} + \beta_4 D_{ij} P1_{ij} + \beta_5 D_{ij} P2_{ij} + \beta_6 T_{ij} + \beta_7 T_{ij} P1_{ij} + \beta_8 T_{ij} P2_{ij} + \beta_9 C_{ij} P1_{ij} + \beta_{10} C_{ij} P2_{ij} + \beta_{11} X_{ij} + \varepsilon_{ij} \quad (1)$$

where the dummy variables P1 and P2 have value 0 before the treatment, 1 three weeks and three months after the treatment. β_4, β_5, β_7 and β_8 are the coefficients of interest, namely the interactions between each treatment dummy and the dummy period. They reveal whether the expected mean change in outcome from before to after was different in the two groups both three weeks and three months later. X is a vector of individual controls (gender, age, prospective economics student, high math skill, wealthier student, parent degree, parent self-employment, distance from home to the city center, student repeating the year). We include clustered standard errors at the class level.

All the three dichotomic dependent variables we have chosen to investigate (FinLit Big Three, Self-assessed FinLit, and the Do-not-know Option) lead to the natural choice of a probit model. Another advantage of this model is the easy interpretation of results. We prefer to present results as the increasing probability of both courses to make easier comparisons among groups.

4.2. Financial literacy

Table 2 indicates that both the traditional and the digital courses increase knowledge (FinLit Big Three) three weeks afterwards.

In particular, the effect of the traditional course is sizeable and economically meaningful. There is a positive correlation between following one of the financial education programs and correctly answering financial literacy questions compared to the control group. On average, students who attended the traditional course are 28 p.p. more likely to become financially literate after three weeks, and 34 p.p. after three months. We test the difference between the traditional course marginal effect three weeks later (0.28) and three months later (0.34) showing more stable results[10] than the digital course. On average, the digital course increases the probability to get all the Big Three questions right, by 13 p.p. only in the short-run (after three weeks), losing statistical significance three months later.

Previous work on financial education (see Kaiser and Menkhoff, 2019) recognize the critical role of the instructors' ability. Having an instructor or teacher who is trained in teaching personal finance affects the impact of the course. To the best of our knowledge, this is the first Italian RCT in which the instructor is a professional financial advisor. Their level of expertise is assumed to be higher than a classroom teacher trained to teach a financial education course. This financial professional could improve motivation and desire for learning, potentially explaining higher and more persistent results for the traditional compared to the digital course (Kaiser and Menkhoff, 2019). In line with PISA results (OECD, 2014; OECD, 2019; 2020a), we find a positive correlation between mathematical ability and overall financial knowledge and a negative correlation between being female and overall financial knowledge. We also control for high financial literacy before the intervention, which is positive and highly statistically significant (0.41).

Noteworthy is that the digital program is promising despite its effect being statistically significant only in the short run. Due to the cheaper and more scalable design of digital programs, we need more evidence involving different digital designs since it seems worthwhile to delivery methods that could increase effectiveness. The most recent meta-analysis by Kaiser et al. (2020) suggests that intensity of programs equates to positive effects. Based on this evidence, increasing the intensity of the digital program may make results more persistent. In fact, increasing the digital course's hours may increase its effects on knowledge, making it equivalent to the effects of a traditional course. Outcomes may also relate to the quality of the material in a digital course, though this would not be the case in our study, as both courses covered the same material.

[10] Running a t-test on the difference between the share of correct responses after three months versus after three weeks, we find no statistical significance (Wald test, $p = 0.4542$). Hence, we cannot reject the null that the two percentages are equal.

Table 2
Probit DID – financial literacy and sensitivity analysis.

	(1) FinLit	(2) FinLit
VARIABLES	Big Three	2/3 Big Three
D_P1	0.134*	0.257***
	(0.081)	(0.083)
D_P2	0.094	0.216*
	(0.140)	(0.111)
T_P1	0.278***	0.395***
	(0.067)	(0.103)
T_P2	0.343***	0.296**
	(0.051)	(0.135)
C_P1	0.027	0.047
	(0.039)	(0.046)
C_P2	0.040	0.030
	(0.045)	(0.035)
D	0.058	−0.018
	(0.042)	(0.032)
T	−0.044*	0.015
	(0.025)	(0.051)
Female	−0.040	−0.098***
	(0.025)	(0.024)
Age	−0.051**	−0.005
	(0.024)	(0.028)
Wealthier_students	−0.002	−0.027
	(0.025)	(0.035)
Repeating_students	−0.011	0.141
	(0.146)	(0.130)
Perspective_Economics_students	−0.037	−0.029
	(0.046)	(0.059)
Self_employed_father	0.015	0.043
	(0.029)	(0.044)
High_math_skill	0.040	0.098***
	(0.027)	(0.037)
FinLit_baseline	0.415***	0.342***
	(0.015)	(0.035)
Other Control	YES	YES
Observations	1160	1060
Pseudo-R^2	0.264	0.178

Note: Table 2 reports average marginal effects from a probit diff-in-diff estimation. The dependent variable FinLit Big Three (column 1) is a dummy which takes value 1 if students answer all Big Three questions correctly, 0 otherwise. Column (2) shows a different specification as sensitivity analysis. FinLit 2/3 Big Three is a dummy which takes value 1 if students answer at least 2 out of 3 questions correctly, 0 otherwise. Clustered standard errors at the class level are reported in parentheses. Numbers with *** indicate significance at the 1 percent level, ** indicates significance at the 5 percent level, and * indicates significance at the 10 percent level. Other Control not statistically significant: Only_child, Foreign, Foreign_parents, City_center, Father's_degree, Mother's_degree, Self-employed_father, Self-employed_mother. We compute a post hoc power analysis. We find sufficient power (>0.80) for all groups except for the direct comparison between the traditional group and the digital one (=0.72).

We compute a sensitivity analysis that repeats the main analysis (column 1) using different dependent variable specifications. Column 2 shows estimation results in which the dependent variable is a dummy variable "FinLit 2/3 Big Three," which takes the value of 1 if students answer at least two out of three questions correctly, and 0 otherwise. Our benchmark is the dummy with value 1 if 3 out of 3 questions are answered correctly.

Potential contamination effects are not a concern since the probability of students in the control group answering two questions correctly three weeks after the course is not statistically significant. The probability of answering all three financial literacy questions correctly increases only for the treated groups three weeks after the course (by 13 p.p. for the digital group, even if weakly statistically significant; 28 p.p. for the traditional group, on average).

Our results are in line with previous works (Brugiavini et al., 2018; Frisancho, 2021). To fully generalize our results, we compare students' financial literacy level before the intervention with a comparable and representative sample from the Survey of Household Income and Wealth (SHIW) compiled by the Bank of Italy. Since financial literacy is higher for adults, we created a dummy for young household heads (people born after 1982). We consider the recent 2016 SHIW wave, the first in which all three variables (inflation, interest and diversification) are collected together. We can confirm that our results

can be generalized since the financial literacy level of students before the course (0.31) is, on average, similar to the financial literacy level of young adults in Italy in 2016 (0.32).

Moreover, our results are robust to a sensitivity power analysis (see Appendix Figs. A1–A3). We compute the effect size *d*, including the mean of each group's correct answers to the Big Three following the course and their pooled standard deviations. Based on the effect size values (0.59 for the traditional course and 0.44 for the digital one), setting a significance level of 0.05 and the size of each treated group and the control group, respectively, we perform a post hoc power analysis finding power higher than 0.90 for both the traditional and the digital courses.

Determining whether treatment have differential impacts based on pre-determined characteristics has important policy implications. To this end, in one variant of the model (1), we split the sample and studied the differential effects of the treatments on different groups. Previous evidence has shown a gender gap in financial literacy, with men displaying higher literacy levels than women, a positive correlation between math abilities and financial literacy, and a positive correlation between wealth and financial literacy. Recent research indicates that people whose fathers are self-employed have higher financial literacy levels. Accordingly, in Table 3, we run a heterogeneity analysis for gender, math ability (math grades equal to/higher or lower than 7), student's age (above or below the median value 17), family income (above or below 30,000 euros per year), and father's employment status (self-employed or not).

What emerges is that, on average, the probability to become financially literate among female students (Table 3, Column 1) increases by 19 p.p. three weeks after the digital course (statistically significant at the 5% level) and by 21 p.p. and 49 p.p. three weeks and three months (respectively) after the traditional course.

Male students (Table 3, Column 2) seem to gain more from the traditional course than from the digital course, with a higher probability to get all the Big Three questions correctly by 35 p.p. and 20 p.p. in the short and long run, respectively, for those in the traditional course.

For math-skilled students (Table 3, Column 3), only the traditional course has a (highly) statistically significant effect on financial literacy, with the effect seen both in the short run (24 p.p.) and the long run (26 p.p.). However, students with lower math skills (Table 3, Column 4) show an increase in the probability to become financially literate following both the traditional course (42 p.p. in the short run and 68 p.p. in the long run) and the digital course (19 p.p. in the long run).

For students younger than 17 years-old (Table 3, Column 5), the digital course has a statistical effect on financial literacy in the long run (35 p.p.) and the traditional course has an effect in the long run (41 p.p.). Students aged 17 and older (Table 3, Column 6) increase their financial literacy only through the traditional course, with an increase of 37 p.p. in the short run and 32 p.p. in the long run.

For wealthier students (Table 3, Column 7), the traditional course has a highly statistically significant impact: 24 p.p. in the short run and 32 p.p. in the long run. The digital course weakly increases their financial literacy, increasing the probability of correctly answering all of the Big Three questions by 0.18 p.p., on average. Lower-income students (Table 3, Column 8) benefit only from attending the traditional course with an effect of 37 p.p. and 35 p.p. in the short run and long run, respectively.

Students whose father is self-employed (Table 3, Column 9) benefit from both the digital (27 p.p. in the short and in the long run) and traditional (24 p.p. in the short run and 31 p.p. in the long run) courses. Students whose father is not self-employed (Table 3, Column 10) benefit only from the traditional course: 32 p.p. and 37 p.p. in the long run.

To summarize, Table 3 shows that both courses have some positive effects on financial literacy. The digital course improved the financial literacy of female and wealthier students in the short run and low math skilled as well as younger students in the long run. The traditional course seems to be the most effective for all groups, both in the short run and in the long run. Students with fathers who are self-employed benefitted more from the digital course than did students whose father is not self-employed.

4.3. Self-assessed financial literacy and gender gap

Mainstream literature and new evidence find that an excess of self-assessed financial literacy not followed by high level of financial knowledge leads to worst financial decisions. This is true in particular among the most vulnerable groups such as young or the elderly attending short financial education courses (up to 4 h following Bucciol et al., 2021; Brugiavini et al., 2018). These results suggest that the main issue in promoting financial education is not only improving financial literacy but also financial awareness, aligning them as much as possible to make conscious financial choices. Becoming aware of the own level of financial knowledge is crucial to do a sound and autonomous wealth-management. This issue is made even more critical by the existence of a gender gap in financial literacy. The most recent Programe for International Student Assessment (PISA) survey promoted by the OECD confirms that, since 2012, Italian 15-years-old students still performed worse than their peers among 65 OECD countries, even more in the South of Italy (Italy is thirteenth out of 20 countries with a mean of 476 vs. 505 for the OECD). In addition, Italy is the only country which shows a statistically significant gender gap at this early stage of life. Results from Bottazzi and Lusardi (2020), Bucciol and Piovesan (2011), ISTAT (2011) and Bucher-Koenen et al. (2021) explain part of the gender gap in financial literacy as lack of confidence. They suggest the need to start at school learning about money, giving up old prejudices affecting self-esteem, thereby empowering female students as well as males.

In our sample, several findings emerge from the descriptive analysis. Among them, an interesting aspect is a misalignment between self-assessed and real financial knowledge. In contrast with the adult population (Di Salvatore et al., 2018),

Table 3
Probit DID – heterogeneity analysis.

	(1) Female FinLit	(2) Male FinLit	(3) High_Math_skill FinLit	(4) Low_Math_skill FinLit	(5) <17 yrs FinLit	(6) >=17 yrs FinLit	(7) High_Income FinLit	(8) Low_Income FinLit	(9) Self_employed FinLit	(10) No_Self_employed FinLit
VARIABLES	Big Three	Big Three	Big Three	Big Three	Big Three	Big Three	Big Three	Big Three	Big Three	Big Three
D_P1	0.195**	−0.006	0.175	0.083	0.204	0.066	0.189*	0.053	0.268***	0.076
	(0.086)	(0.098)	(0.149)	(0.072)	(0.190)	(0.068)	(0.100)	(0.128)	(0.080)	(0.084)
D_P2	0.135	0.001	0.024	0.192**	0.353**	−0.044	0.066	0.145	0.269*	0.011
	(0.130)	(0.134)	(0.209)	(0.091)	(0.179)	(0.138)	(0.143)	(0.152)	(0.148)	(0.131)
T_P1	0.211**	0.350***	0.245***	0.422***	0.136	0.368***	0.243***	0.366***	0.238***	0.320***
	(0.107)	(0.064)	(0.077)	(0.125)	(0.153)	(0.047)	(0.060)	(0.061)	(0.087)	(0.061)
T_P2	0.493***	0.198***	0.265***	0.684***	0.407***	0.321***	0.329***	0.347***	0.315***	0.377***
	(0.064)	(0.055)	(0.083)	(0.078)	(0.091)	(0.077)	(0.060)	(0.075)	(0.093)	(0.053)
C_P1	0.080	−0.021	0.023	0.025	0.122**	−0.002	0.022	0.043	0.016	0.031
	(0.052)	(0.052)	(0.051)	(0.057)	(0.054)	(0.041)	(0.042)	(0.062)	(0.062)	(0.040)
C_P2	0.067	0.016	0.007	0.073	0.033	0.039	0.055	0.018	0.053	0.032
	(0.044)	(0.057)	(0.062)	(0.053)	(0.078)	(0.043)	(0.056)	(0.063)	(0.062)	(0.048)
D	0.047	0.095	0.066	0.047	−0.005	0.095***	0.039	0.070	−0.009	0.090***
	(0.051)	(0.093)	(0.070)	(0.072)	(0.077)	(0.027)	(0.045)	(0.063)	(0.074)	(0.033)
T	−0.068	−0.042	0.002	−0.184**	−0.023	−0.052**	−0.031	−0.096*	−0.015	−0.081**
	(0.049)	(0.049)	(0.041)	(0.075)	(0.088)	(0.021)	(0.034)	(0.054)	(0.033)	(0.040)
Other Control	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	545 0.282	612 0.269	658 0.242	499 0.311	2920.228	868 0.299	712 0.251	433 0.331	390 0.325	767 0.261
Pseudo-R^2										

Note: Table 3 reports average marginal effects from a probit diff-in-diff estimation. The dependent variable FinLit Big Three is a dummy which takes value 1 if students answer all Big Three questions correctly, 0 otherwise. Clustered standard errors at the class level are reported in parentheses. Numbers with *** indicate significance at the 1 percent level, ** indicates significance at the 5 percent level, and * indicates significance at the 10 percent level. Other Control variables: Female, Age, Wealthier_students, Repeating_students, Only_child, Foreign, Foreign_parents, City_center, Father's_degree, Mother's_degree, Self-employed_father, Self-employed_mother, FinLit_baseline.

Generation Z is not overconfident about its level of financial literacy. In fact, the first survey conducted before the course reveals that more than 75% of students interviewed (from a sample of 642 students answering the question) assessed themselves as having a low financial literacy level, 23% claim that they know enough about financial concepts and only 1% of students consider themselves very financially literate. Those who followed one of the two courses showed an increased self-assessed financial literacy level. Moreover, in comparison to the digital course, the results suggest that the traditional course not only increases financial knowledge but also awareness of the real financial literacy level. To show this, we create the dummy variable Self-assessed FinLit. It takes value 0 if students consider their level very low, and 1 if they claim to have a good or very good level of financial literacy.

We can conclude that following one of the two simplified financial education programs, there is an improvement and greater alignment of real and perceived financial literacy. In fact, even if both real and perceived financial literacy are still higher in comparison to the period before the course, when real financial literacy decreases over the course of time then, so also does the perception of financial knowledge.

These results also show that perceived financial literacy is linked to real financial knowledge as a whole learning process and not only to confidence in a single right answer. Hence, the control group increased its share of right answers but not its perceived level of real financial knowledge.

Thus, regardless of the course the students attend, they are able to increase not only their knowledge but also the perception of their own knowledge of economics and finance. Indeed, over 34% of students in the digital course and 50% of students in the traditional course no longer declared a low level but a medium perceived level of financial literacy. The absence of a high perceived level of knowledge is in itself a good result. Following Brugiavini et al. (2018) and Frisancho (2021), we analyze whether self-assessed knowledge corresponds to real knowledge before and after the courses and if a gender effect is present.

This particular design allows us to estimate a diff-in-diff model running a probit estimation on self-assessed financial knowledge. The self-assessed financial knowledge dummy takes value 0 if students consider their financial knowledge very poor and 1 if they claim to have a good or a very good level of financial literacy.

The main result, shown in Table 4, Column 1, is that the probability to self-assess medium or high financial knowledge level increases three weeks (by 50 p.p.) and three months (by 46 p.p.) after the treatment only in students who attended the traditional course. We also see a positive and robust effect (14 p.p.) that emerges between self-assessed financial literacy and the desire to choose an Economics major in the near future. However, female students are less confident than their male peers. In particular, females are 18 p.p. less likely to self-assess a good or high level of financial knowledge. The fact that financially educated members of Generation Z are more aware of their financial knowledge supports the idea that they will make better financial decisions in the future. The lower confidence of female students is in line with the most recent literature on the financial gender gap (GFLEC, 2020). In our experiment, the difference in self-assessed financial literacy level shrinks after the course.

Self-assessed financial knowledge is higher than real knowledge on average. Taking part in this type of financial education program improves both their knowledge and their awareness of their real financial literacy level. The interaction terms (Table 4, Column 2) are negative for both courses but (weakly) statistically significant only for the digital course after three weeks. The probability of self-assess a medium or high level of financial literacy fell by 21 p.p. after being financially literate attending the digital course.

There is empirical evidence of a gender gap (Bottazzi and Lusardi, 2020; Lusardi and Mitchell, 2014; Almenberg and Dreber, 2015). This suggests that while women are less financially literate, they also recognize their limits in this field by more often choosing “do not know” as a response (47% of women against only 26% of men in the US).

To investigate potential gender differences, we conduct both through descriptive analysis and quantitative estimation. From summary statistics it emerges that less than 34% of financially literate students are female. Of these, 78.68% are underconfident (female students self-assessing low financial literacy levels even if they answered all Big Three correctly). Moreover, underconfidence in financial literacy can be a driver of future university choice. 95.83% of underconfident females declare a preference for a non-Economics major regarding the future choice of university. Focusing on the illiterate female sample who self-assessed a low financial literacy level, 92.63% exclude an Economics major in their futures.

From a quantitative perspective, in Table 5 we explore students’ confidence further using the “Do-not-know option” dummy variable as the dependent variable and repeated the analysis. In the full sample, attending one of the two courses reduces the likelihood of answering “do not know” to at least one of the Big Three questions (by 47 p.p. in the digital course and 60 p.p. in the traditional course, three weeks after, by 30 p.p. and still 60 p.p. respectively three months afterwards). The differential course effect is higher and statistically significant for the traditional course compared to the digital course.

Due to the lack of sufficient observations to investigate the existence of differential effects for female students, we have to restrict the model conducting the diff-in-diff estimation comparing only two periods: before the intervention and three weeks after the intervention including both treated groups and control groups. These results reveal that female students who attended a financial education program after three weeks are less likely to choose the “do-not-know” option. In particular, the traditional course decreases the probability of female students being underconfident more than the digital course. Since the difference is highly statistically significant, attending a traditional course increases women’s confidence more than a digital course (since the share of “do-not-know” responses decreases more for female students in the traditional than in the digital course). In addition, the higher the math ability, the lower the probability of choosing the do not know option by 8 p.p..

Table 4
Probit DID – self-assessed financial literacy and knowledge.

	(1)	(2)
VARIABLES	Self_assessed FinLit	Self_assessed FinLit (with interaction)
D_P1_FinLit		−0.212*
		(0.116)
D_P2_FinLit		0.045
		(0.116)
T_P1_FinLit		−0.135
		(0.187)
T_P2_FinLit		0.083
		(0.091)
C_P1_post		0.070
		(0.061)
D_FinLit		−0.032
		(0.067)
T_FinLit		−0.105
		(0.130)
D_P1	0.152	0.243*
	(0.102)	(0.137)
D_P2	0.064	0.035
	(0.076)	(0.088)
T_P1	0.506***	0.563***
	(0.054)	(0.093)
T_P2	0.462***	0.382***
	(0.057)	(0.112)
C_P1	0.023	−0.01
	(0.033)	(0.043)
C_P2	0.003	0.02
	(0.029)	(0.040)
D	0.101	0.112
	(0.068)	(0.078)
T	−0.186	−0.146
	(0.122)	(0.099)
P2_FinLit		−0.052
		(0.054)
FinLit_baseline	0.100**	0.138**
	(0.046)	(0.057)
Female	−0.184***	−0.180***
	(0.039)	(0.039)
Age	−0.040	−0.027
	(0.044)	(0.044)
Wealthier_students	0.048	0.048
	(0.032)	(0.031)
Repeating_students	0.316**	0.307**
	(0.143)	(0.142)
Perspective_Economics_students	0.137**	0.149***
	(0.056)	(0.053)
Self_employed_father	0.051*	0.044
	(0.029)	(0.028)
High_math_skill	0.015	0.010
Other Control	(0.036) YES	(0.037) YES
Observations Pseudo-R^2	1168 0.141	1166 0.148

Note: Table 4 reports average marginal effects from a probit diff-in-diff estimation. The dependent variable Self-Assessed FinLit is a dummy which takes value 1 if students self-assessed a medium or high level of financial knowledge, 0 otherwise. We run the same model twice, first in simple form (column 1) and then adding an interaction term (column 2) between treatment dummies and the financial literacy dummy FinLit Big Three. The interaction terms evaluate the additional marginal effects of being treated and financially literate on self-assessing higher level of financial literacy. Clustered standard errors at the class level are reported in parentheses. Numbers with *** indicate significance at the 1 percent level, ** indicates significance at the 5 percent level, and * indicates significance at the 10 percent level. Other Control not statistically significant: Only_Child, Foreign, Foreign_Parents, City_center, Father's_Degree, Mother's_Degree, Self-employed_mother.

Table 5
Probit DID – "Do-not-know" option.

	(1)
VARIABLES	Do-not-know Option
D_P1	−0.468***
	(0.134)
D_P2	−0.299***
	(0.062)
T_P1	−0.597***
	(0.121)
T_P2	−0.597***
	(0.119)
C_P1	−0.057*
	(0.034)
C_P2	−0.056
	(0.039)
D	−0.083*
	(0.049)
T	0.084*
	(0.044)
Female	0.074***
	(0.023)
Age	0.056**
	(0.024)
Wealthier_students	−0.045
	(0.037)
Repeating_students	−0.251**
	(0.125)
Perspective_Economics_students	0.007
	(0.048)
Self_employed_father	−0.039
	(0.031)
High_math_skill	−0.065**
	(0.030)
FinLit_baseline	−0.408***
	(0.029)
Other Control	YES
Observations	1175
Pseudo-R^2	0.268

Note: Table 5 average reports marginal effects from a probit diff-in-diff estimation. The dependent variable Do-not-know Option is a dummy which takes value 1 if students choose the "do-not-know" option at least once out of the Big Three, 0 otherwise. Clustered standard errors at the class level are reported in parentheses. Numbers with *** indicate significance at the 1 percent level, ** indicates significance at the 5 percent level, and * indicates significance at the 10 percent level. Other Control not statistically significant: Only_child, Foreign, Foreign_parents, City_center, Father's_degree, Mother's_degree, Self-employed_mother.

Confidence is strictly linked with moral values and self-esteem. Bucciol and Piovesan (2011) find a gender gap among children in a field experiment about cheating, concluding that girls are more prone to moral suasion than boys. This result may explain the higher "do not know" response rate. A recent contribution to the literature explaining the reason behind the gender gap finds that stereotypes and underconfidence affect financial literacy (Bottazzi and Lusardi, 2020; GFLEC, 2020; Bucher-Koenen et al., 2021). Based on a stereotype index (ISTAT, 2011), it is believed that men manage the family budget, make economic decisions or take a degree and are less suitable than women for household chores.

4.4. Cost-effectiveness analysis

Taking part in one of the two types of course based on the same freely available material published by the Museum of Savings, has positive effects on the financial education at negligible costs. Both courses increase the probability of students correctly responding to the Big Three questions three weeks after. While both the traditional and digital courses are effective in the short run, the traditional course has long-run effectiveness. The traditional course, however, is more expensive to deliver than the digital course. Cost-effectiveness is one of the main considerations for policy, and considering the low scores among Italian students on the PISA financial literacy assessment and the increasing efforts in Italy to increase financial literacy, information on comparative cost-effectiveness of these methods can be useful.

Table 6
Probit DID – "Do-not-know" option for female students.

	(1)
VARIABLES	Do-not-know Option
D_P1_Female	1.030***
	(0.245)
T_P1_Female	1.069***
	(0.161)
D_P1	−1.443***
	(0.131)
T_P1	−1.708***
	(0.088)
D_ Female	−0.013
	(0.128)
T_ Female	0.054
	(0.074)
P1_ Female	−0.139**
	(0.058)
P1	0.029
	(0.048)
D	−0.094
	(0.093)
T	0.149*
	(0.083)
Female	0.116**
	(0.045)
Age	0.022
	(0.034)
Wealthier_students	−0.032
	(0.036)
Repeating_students	−0.406***
	(0.130)
Perspective_Economics_students	−0.001
	(0.056)
Self_employed_father	−0.018
	(0.041)
High_math_skill	−0.083***
	(0.030)
FinLit_baseline	−0.381***
	(0.029)
Other Control	YES
Observations	797
Pseudo-R^2	0.251

Note: Table 6 reports average marginal effects from a probit diff-in-diff estimation. The dependent variable Do-not-know Option is a dummy which takes value 1 if students choose at least once the "do-not-know" option out of the Big Three questions, 0 otherwise. The interaction term between treatment dummies and "Female" evaluate the additional marginal effects on the "do-not-know" option choice for female attendees. Clustered standard errors at the class level are reported in parentheses. Numbers with *** indicate significance at the 1 percent level, ** indicates significance at the 5 percent level, and * indicates significance at the 10 percent level. Other Control not statistically significant: Only_child, Foreign, Foreign_parents, City_center, Father's_degree, Mother's_degree, Self-employed_mother.

If we assume that each school provides students with a Wi-Fi connection, implementing the Futuro Sicuro program in one school (126 students) takes a large effort but is free of charge. The traditional course would be more expensive if financial advisors were not volunteers. Based on their standard hourly fee (29 euro/hour) and the average class size, our eight-hour financial education course would cost about EUR 11.04 per student. However, if we had to create the content, the costs would be higher than for the digital course. The convenience persists because of their scalability and standardization (the material costs can be spread over all classes in the future, unlike a financial advisor's live performance).

Our findings suggest that the digital course is a good option to improve financial literacy in the short run but the most expensive traditional course might be more suitable if the effects are to persist over a longer time frame. We can compute the cost-effectiveness ratio only for the traditional treatment which would be equal to 0.02 (0.28/11.04) if the financial advisors were regularly payed. This means that one additional euro increases by 2% the probability to answer Big Three questions correctly. To have the same increase in probability to become financially literate for the digital course the amount

of euros invested would be equal to 6.5 per student. In our program, neither the traditional course nor the digital one requires additional costs to be implemented. We stress that a traditional course is more expensive. Hence its feasibility depends on the available budget.

5. Conclusions

Today, the need to improve financial literacy is ubiquitous. As the OECD-PISA report states in its title (OECD, 2014), financial literacy is a required skill for the 21st century. Following OECD-INFE guidelines, we set up a field experiment implementing a financial education program called "Futuro Sicuro: Sapere per sapersi difendere" with a sample of 650 high school students in Italy. This paper identifies alternative ways to teach financial concepts, making the learning process more straightforward and enjoyable at negligible costs. The school environment makes it possible to take advantage of the period during which people tend to be particularly receptive (Heckman, 2006). This study addresses challenges pushed by the Pandemic using modern technological facilities, scalable and useful for others too, helping to reduce social distance through the online environment.

We randomly provide two treatments at the class level. One is a traditional financial education program conducted by a financial advisor, and the other is an entertaining digital financial education program that uses web-based applications based on learn-by-playing concepts. The study, carried out with six classes, suggests that both courses have some positive and statistically significant effects. More research is required before we can make conclusive statements about which financial education program is better able to spread financial literacy. The two treatments have different costs but positive effects. In particular, three weeks after their conclusion, we find that, on average, the likelihood of the traditionally treated group answering the Big Three questions correctly increases by 28 p.p. and by 13 p.p. for the digital group compared to the control group. Moreover, results were aligned with participants' realistic assessments of their financial skills. A follow-up study reveals that only the traditional course seems to confirm the effect in the long run. The heterogeneity analysis suggests a powerful effect on female students. Increasing female education and confidence shows also a future positive indirect impact on students' financial knowledge since students whose mother has a degree benefit more from financial education programs. Finally, having a self-employed father strengthens the financial literacy courses' effect on students' knowledge.

It appears that the "Futuro Sicuro" setting causes significant improvements and can both simplify training and rely more on digital power. Findings indicate the opportunity to obtain higher results with lower stress, increasing self-assessed financial knowledge. Comparing the costs of the traditional and digital courses, the latter is cheaper. However, although the traditional course is much more expensive due to the presence of an expert such as a financial advisor in the classroom, its effect seems to endure longer than for the digital course. Focusing on the control group, merely filling in questionnaires more than twice may trigger a mechanism that excites student curiosity about the topic but has no effect on their self-assessed financial literacy. This could be a compelling starting point for public policy.

Moreover, talking about daily problems with experts in the field may increase student curiosity as well as their ability to meet wealth management requirements. In line with OECD-PISA reports (OECD, 2014, OECD, 2019, OECD, 2020a, OECD, 2020b; OECD, 2017), a gender gap emerges. Female students perform worse than male students. The average difference could be due to non-cognitive skills and the declared higher comfort of males with technology.

However, several steps need to be taken before reaching a definitive conclusion. Our study has at least four limitations which need to be addressed for future research. First of all, behavioral aspects must be analyzed to understand if the courses can produce lasting changes in financial attitudes, preferences, or behavior. A better format for future research needs to include other financial outcomes that do not suffer from self-reported results with the aim to follow students in their adult life. Moreover, it would be better to follow students also in the long run (more than three months). Finally, the courses could be carried out more broadly to increase financial literacy in Generation Z in a particular setting which prevents contamination effects. The replication of the study in other areas could help to identify specific regional patterns as well as in increasing external validity (Tables A1 and A2).

Table A1
Math abilities – treatment effect.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	$T+D$ Classes	T Classes	D Classes	C Classes	Diff1 (2)-(4)	Diff2 (3)-(4)	Diff3 (2)-(3)	Diff4 (1)-(4)
High_math_skill	0.584	0.633	0.538	0.517	0.115*	0.021	0.094	0.066
	(0.494)	(0.485)	(0.502)	(0.500)	(0.066)	(0.065)	(0.088)	(0.049)
High_math_skill_P2	0.754 (0.432)	0.724 (0.450)	0.781 (0.416)	0.700 (0.458)	0.023 (0.062)	0.081 (0.055)	−0.057 (0.078)	0.053 (0.044)
Observations	126	61	65	524	585	589	126	650

Note: Table A1 reports high math ability (whose math grade is equal or higher than 7) for the whole treated groups (column 1), and each treated group separately (T Classes in column 2 and D Classes in column 3), as well as for the control group (C Classes in column 4). In particular, mean and standard deviations are reported in brackets. The "Diff" columns report the output of a t-test on the equality of the mean in different groups. Numbers with *** indicate significance at the 1 percent level, ** indicates significance at the 5 percent level, and * indicates significance at the 10 percent level.

Table A2
External validity – characteristics of the school chosen.

High Schools	Teachers		4th grade		5th grade		All grades		Students-Teacher ratio	Region of the school
	M	F	M	F	M	F	M	F		
Targeted School	23%	77%	50%	50%	53,30%	46.68%	54.30%	45.60%	10	Calabria
School Benchmark 1	29%	71%	55%	45%	53,39%	45,61%	55.60%	44.40%	13	Abruzzo
School Benchmark 2	24%	76%	45%	45%	62,23%	37,77%	57%	43%	13	Veneto
All Scientific HS	21%	79%	n/a	n/a	n/a	n/a	51%	49%	12	Italy

Note: Table A2 reports administrative data about teachers and students' characteristics: school size, gender balance and teacher-student ratio. We compare these data with two Scientific High Schools of Verona and Pescara. Moreover, the average teacher's age in each school is about 54 years-old. We can safely assume that the school chosen's characteristics are in line with the Italian average teachers' characteristics from OECD report (2020), "Education at a Glance 2020: OECD Indicators", OECD Publishing, Paris, available at the following link https://doi.org/10.1787/69096873-en. All data refer to the field experiment period, 2018.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jebo.2021.12.001.

Appendix

Sensitivity power analysis

To help in planning more powerful designs in the future, and more importantly, to determine the strength of our reliably detected effects, we conduct a sensitivity power analysis.

Figs. A1 and A2 evaluate risk and advantages related to sample size choice, for each treated group, D Classes and T Classes, separately.

In both of them, Y-axis reports the effect size *d* which identifies the lower bound of the set of sample effect sizes that would be statistically significant (with power 0.80) considering the total sample size N.

Fig. A1 reports that the digital course's lower bound of the set of sample effect sizes is between 1.5 and 0.3 considering the total sample size of 585.

Fig. A2 reports that the Traditional course's lower bound of the set of sample effect sizes is between 1.5 and 0.4, considering the total sample size of 589.

Considering the total sample of treated students (126) and the control group (524), we can safely argue that we have enough power to rely on our results based on a total sample of 650 students.

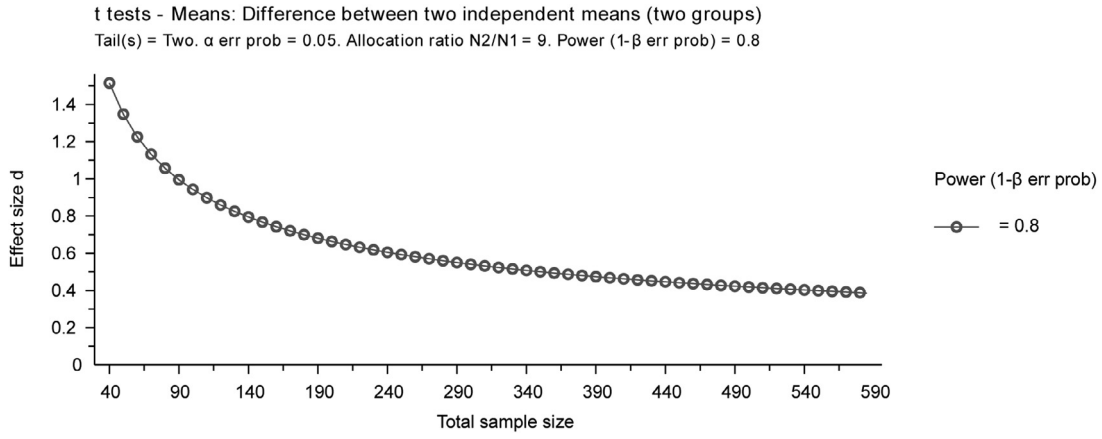

Fig. A1. Digital course - D Classes vs. Control Group (Total sample size 585).

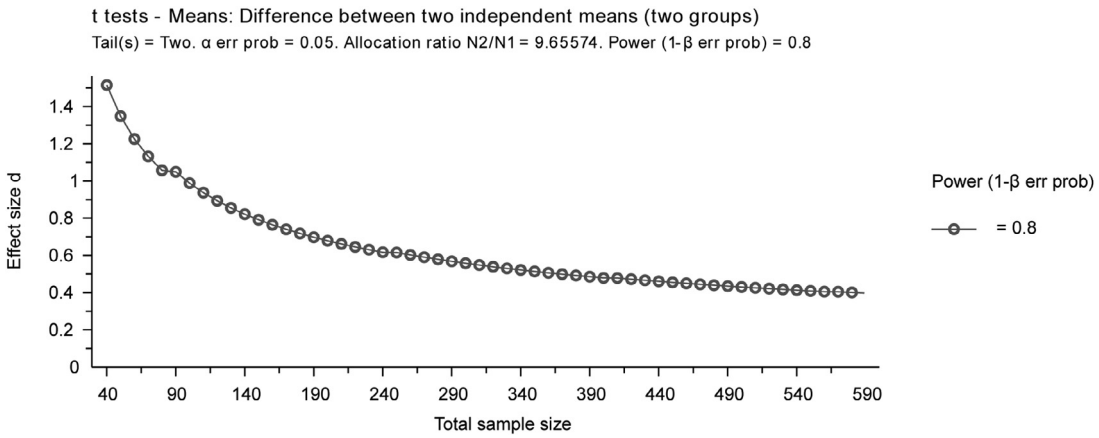

Fig. A2. Traditional couse - T Classes vs. Control Group (Total sample size 589).

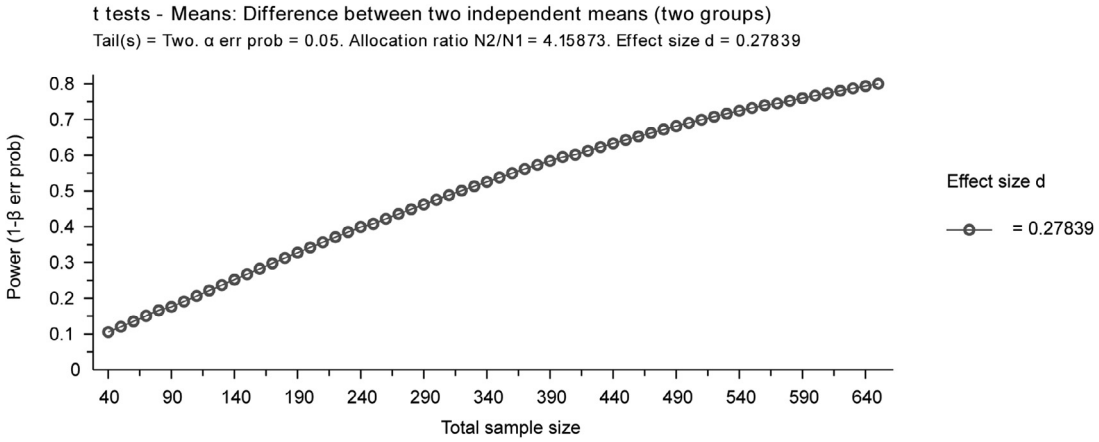

Fig. A3. Total sample size sensitivity power analysis.

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