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The effects of parental involvement in homework: two randomised controlled trials in financial education

Joana Elisa Maldonado[1] · **Kristof De Witte**[1,2] · **Koen Declercq**[3]

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Abstract

This paper provides causal evidence on the effects of parental involvement on student outcomes in a financial education course based on two randomised controlled trials with a total of 2779 students from grade 8 and 9 in Flanders. Using an experimental design with three treatment groups, the impact of parental involvement in homework is distinguished from the standalone impact of the classroom intervention and homework itself. Intention-to-treat analysis reveals that access to the intervention effectively improves students' financial literacy in the two dimensions of knowledge and behaviour. The classroom intervention combined with a homework assigned to be completed with the parents increases financial literacy by 0.38 standard deviations. On average, the added value of prompting parental involvement in homework is not statistically significant. Yet, stimulating parental involvement has significant positive effects on behaviour for disadvantaged students.

Keywords Financial literacy · Parental involvement · Randomised controlled trial · Education

JEL classification C93 · I21 · G53 · A21

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✉ Joana Elisa Maldonado
joanaelisa.maldonado@kuleuven.be

1 Leuven Economics of Education Research, Faculty of Economics and Business, KU Leuven, Naamsestraat 69, 3000 Leuven, Belgium

2 UNU-MERIT, Maastricht University, Maastricht, the Netherlands

3 Center for Research in Economics, UCLouvain - Saint-Louis Bruxelles, Brussels , Belgium

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1 Introduction

Parents are a child's gateway to education as direct and indirect parental influences are key determinants of student outcomes. Empirical and theoretical evidence, dating back to the Coleman report and early research about the determinants of student achievement, indicates that parental background and effort could be more relevant inputs to the education production function than school resources (Coleman 1966; Hanushek 1986). Increasing school resources might even crowd out parental efforts (Datar and Mason 2008; Das et al. 2013; Houtenville and Conway 2008). Hence, both students and society could benefit if parental involvement in education is stimulated. However, parental involvement has implicit costs for parents as it requires significant time investments. Therefore, evidence on the benefits of parental involvement measures is needed.

This paper contributes to the existing literature by providing causal evidence on the effects of parental involvement in the context of financial education. The study is based on two large-scale randomised controlled trials with a total of 2779 students from grade 8 and 9. We evaluate the effects of prompting parental involvement in a homework assignment in a financial education intervention. Comparing three different treatment groups, we explore whether there is a dynamic complementarity between home and school inputs.

As there is evidence of a positive association between financial literacy and wealth holdings (Van Rooij et al. 2011), financial literacy has become indispensable in today's increasingly complex market economies. However, many studies reveal that a large share of the population lacks basic financial literacy (e.g. Lusardi and Mitchell 2011). Observational studies indicate a link between parents and their children's level of financial literacy and behaviour.[1] Parental influence is therefore expected to matter considerably in financial education.

While earlier meta-analyses reported ambiguous effects of financial education (Fernandes et al. 2014; Miller et al. 2015), more recent meta-analyses including a broader set of experimental studies clearly show statistically and economically significant effects of financial education on financial knowledge and behaviour (Kaiser and Menkhoff 2017, 2020; Kaiser et al. 2020). Most randomised controlled trials in secondary schools record positive effects of financial education courses on financial literacy in developed economies (Becchetti et al. 2013; Bover et al. 2018; Compen et al. 2021; Iterbeke et al. 2020a, b; Lührmann et al. 2018). However, only a small number of studies considered the role of parents in financial education. A few studies assessed the effects of financial education in school on family communication about financial topics (Batty et al. 2020; Bover et al. 2018; Frisancho 2020). Bruhn et al. (2016) and Sherraden et al. (2011) evaluated financial education interventions with parental involvement measures, but did not distinguish the effects of the classroom intervention from the parental involvement components. Maldonado and De Witte (2020) demonstrate that the provision of subject-specific information to parents does not improve the outcomes of a financial education intervention.

[1] See, for example, Dohmen et al. (2012) and Webley and Nyhus (2006).

In correlational studies, a reverse causal relationship between student outcomes and parental involvement is a serious concern as parents are likely to increase their efforts when their child is experiencing difficulties. Causal evidence on parental involvement is limited to a small number of heterogeneous interventions with mixed outcomes.[2] While the stimulation of teacher–parent communication has been assessed in a number of field experiments (for example, Avvisati et al. 2014; Bergman 2021; Haelermans and Ghysels 2016; Islam 2019; Kraft and Rogers 2015), there is a lack of evidence on student-centred measures. In financial education, homework has been suggested as a means to increase parental participation (Hanson and Olson 2018; Van Campenhout 2015).

The main contribution of this paper is to identify the causal effect of stimulating parental involvement in homework based on a randomised intervention in a large and representative sample. Next to assessing the effects of a financial education programme, we specifically address the value added of prompting parental involvement. In addition, we consider the heterogeneous effects with respect to students' socio-economic background, providing new evidence regarding the question on whether parental involvement in education neglects students with a disadvantaged family background.

Intention-to-treat analysis confirms that access to the financial education intervention significantly improves students' financial literacy. Assignment of the classroom intervention together with homework prompting parental involvement increases financial literacy by 0.38 standard deviations. However, the added value of assigning homework with parental involvement compared to homework without parental involvement is not significant. Our analysis accounts for selective attrition during the intervention and the results hold true when accounting for multiple hypothesis testing and non-compliance with the homework assignment. Finally, assigning parental involvement has significant positive effects in the behavioural dimension for students with a low socio-economic status and students with low levels of family communication at baseline.

These results have relevant implications for policy debates about the utilisation of parental resources for improving student outcomes. Parental involvement interventions, such as the one tested in this study, are scalable and cost-effective measures which are simple to develop and implement. While stimulating parental involvement does not seem to systematically increase learning effects in financial education, it could, contrary to common beliefs, benefit underprivileged students.

The paper is structured as follows. Section 2 explains the intervention and Sect. 3 introduces the experimental design and sample. Section 4 describes the methodology, followed by a section presenting the results. Section 6 concludes with a discussion.

[2] In contrary to interventions at school, there are a large number of experimental studies on parental involvement in early childhood (for example, Fryer et al. 2015; Doss et al. 2019; Mayer et al. 2019). Another strand of literature considers parental schooling choices rather than involvement in the content of schooling (Bursztyn and Coffman 2012; Dizon-Ross 2019).

2 Intervention

In this study, two randomised controlled trials were conducted with 14- to 16-year-old high school students in Flanders, Belgium. The intervention was designed to measure the effects of stimulating parental involvement in financial education. To this end, a homework assignment was used which was created by senior teachers in the project team. The assignment consisted of a handout with instructions and a link to a short video on payment methods, followed by an online quiz with six questions about payment methods, designed for a duration of 10–15 min. The assignment encouraged the students to involve their parents in the homework in two ways. First, clear instructions in the handout and in the online quiz asked students to involve a parent. Second, several questions in the quiz were formulated to prompt the students to engage a parent. These questions could not easily be answered by the student and required the help of their parent. For instance, some questions enquired about parents' habits regarding payment methods.[3] Upon completion, the correct solutions from the quiz were displayed.

To distinguish the effects of stimulating parental involvement from the effects of the homework, a homework assignment without stimulation of parental involvement was used as comparison. This homework assignment was identical to the homework with parental involvement, except for the elements prompting parental involvement. The questions triggering parental involvement were replaced with questions that could be answered by the students. Students were given the same information in both versions of the homework. Online Appendix C presents the questionnaires of both homework assignments.

Teachers assigned this homework to the students as preparation for a financial education course. The classroom intervention consisted of a computer game about payments methods with a duration of four class periods.[4] The game was developed by senior teachers in the project team and included contents which were not a part of the regular curriculum. The course covered topics such as different means of payment, safety of payments, digital payments and the calculation of discounts. The learning objectives were to increase knowledge about payment methods, awareness of associated risks and the ability to search for independent information. The course was hence in accordance with the OECD core competencies for financial literacy (OECD 2015), which promote the broad concept of financial capability as opposed to financial literacy in terms of mere knowledge.[5]

[3] It should be noted that we could not observe whether parents were eventually involved. Instead, we measure the effect of prompting parental involvement, which can be assumed to represent a lower bound estimate of the true effect of parents getting involved.

[4] Participating teachers could use a paper version of the game in the case of limited ICT infrastructure. In the first wave, this was only used in 5.86% of the classes. In the second wave of the experiment, this was not measured.

[5] The teaching material was in line with the evidence showing the benefits of interactive learning in financial education (Amagir et al. 2018; Batty et al. 2015, 2020).

The role of the teacher was reduced to a minimum to guarantee a standardised implementation in all participating schools. Teachers received clear instructions explaining the game as a deliberately autonomous task for the students.[6] Students completed the financial education game in groups of two and the game led students independently through the different steps. The groups were randomly assigned with a jigsaw game at the start of the class. Each team of students received a booklet with all necessary explanations. A small prize was given to the winning team to encourage students to perform well.

There are four potential mechanisms through which the stimulation of parental involvement in the homework could increase the learning outcomes of the intervention. First, learning effects of parental involvement can result from direct instruction. Parents are likely familiar with the topics in this intervention, since transactions in their everyday lives make them familiar with payment methods. Therefore, parents could offer explanations to the students. Second, knowing their child well, parents could adapt the explanations based on their child's character, needs and interests. Third, the homework could trigger parents to raise the topic again or repeat explanations after completing the assignment. Finally, parent–child communication is positively related with student achievement (Houtenville and Conway 2008), arguably because parents can communicate the importance of a topic to their child (McNeal 2001; Harackiewicz et al. 2012). The discussion between the parent and the student could increase learning outcomes through motivational factors, reinforcing students' interest (Dumont et al. 2012; Heddy and Sinatra 2017).

3 Experimental design and sample selection

The intervention was implemented in two randomised controlled trials in grade 8 and 9, i.e. the second and third year of secondary education in Flanders. Schools were recruited in an open call via the Belgian government's financial literacy agency (Wikifin). During the registration period, the teaching material was tested in two pilot schools. The intervention was implemented in two waves to increase external validity by replicating the same intervention with a different group of students within a short time frame. In the first wave, classes in grade 8 participated between February and mid-April 2018, followed by a second wave with classes in grade 9 between mid-April and June 2018 (see Fig. 1). 116 schools registered to participate, among which 66 in wave 1 and 50 in wave 2.

All treatment groups were assigned to a four-hour financial education class (see Fig. 2).[7] The first treatment group only received the classroom intervention (no homework

[6] To reduce the uncertainty about the implementation of the intervention, a team of three trained PhD students in education economics visited 18 classes in 16 different schools. Observed classes were filmed unless prohibited by school regulations. The behaviour of students and teachers was evaluated based on a standardised rating scheme. The observations confirmed that the standardised intervention was correctly implemented by the teachers. Teachers were compliant that is only involved with the class to provide technical support or suggest the use of the provided material.

[7] Teachers could choose to teach the four class periods separately or in one block. 80% of students received the classes in four blocks of one hour each, 13% in two blocks of two hours and 7% in one block of four hours.

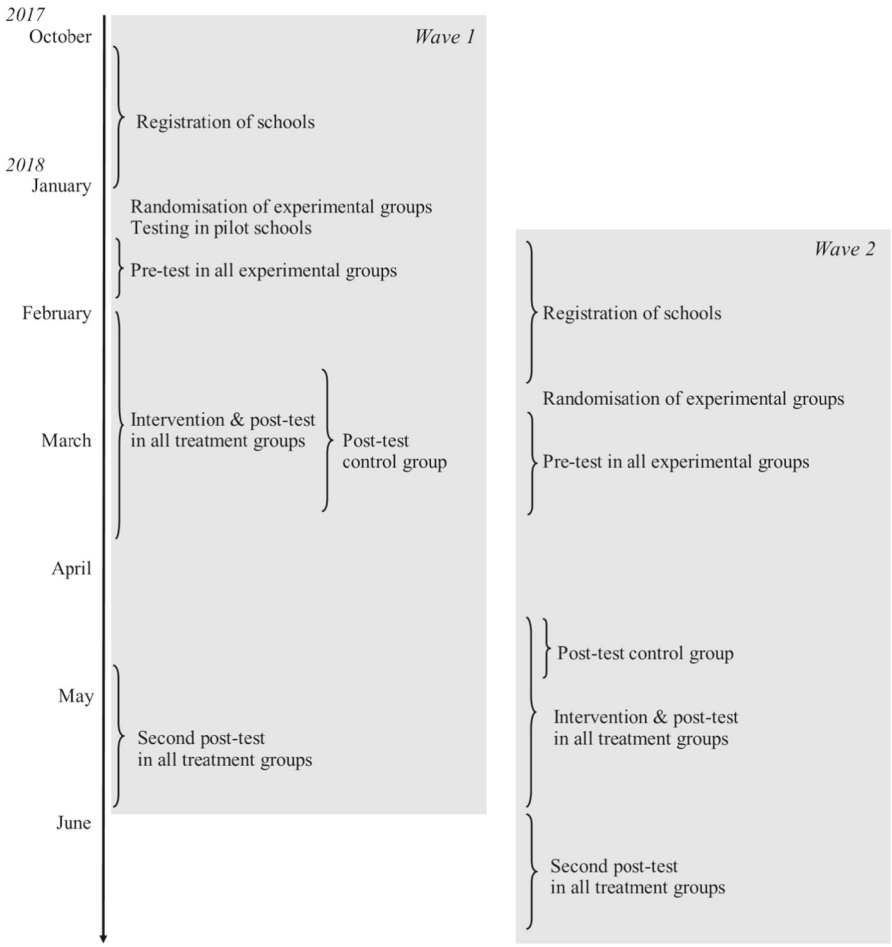

Fig. 1 Time line

group). The second treatment group received in addition a preparatory homework assignment (homework alone group). In the third treatment group, this homework additionally prompted parental involvement (homework with parents group).[8] Randomisation was done at school level in order to avoid contamination at class or teacher level. Registered schools were randomised in four experimental groups that is one control group and three treatment groups.

Before the intervention, students in all experimental groups completed a self-administered digital pretest under the supervision of the teacher. At the end of the last class, students in the treatment groups completed a post-test. The control group

[8] Compliance with the homework was high in both waves and for both types of homework. On average, 70% of the students completed their homework in both treatment groups. Online Appendix A provides more details about compliance with the homework assignment. Using the time stamp of the homework assignment, it was verified that all the students completed the task before the post-test.

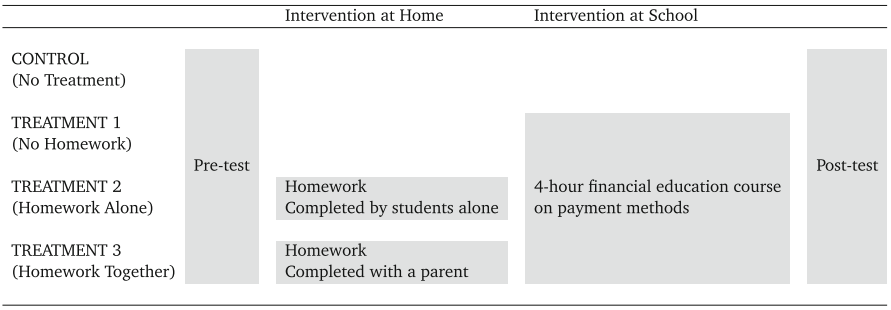

Fig. 2 Overview of the experimental groups

received the post-test six weeks after the pretest. On average, the post-test was completed 6.5 weeks after the pretest. Two weeks after the end of the intervention period, schools in the treatment groups were sent a second post-test, which was, on average, completed seven weeks after the first post-test. After the experiment, the teaching material was sent to the control group as a reward for participation in the tests.

The pretest consisted of 38 items (see Online Appendix C). Financial literacy was assessed on nine multiple choice questions about the course material, comprising the two dimensions of knowledge and behaviour. Six of the nine financial literacy items measured financial knowledge, such as about different means of payment, identifying neutral information on financial issues and calculating discounts. The other three items captured behavioural aspects of financial literacy through decisions in fictional scenarios on payment safety, online fraud and advertising.[9] Due to the age of the students, it was not possible to test decisions involving real stakes. All questions included the option to indicate not knowing the answer. The remainder of the test items measured attitudes and background characteristics, such as the frequency of family communication about financial topics, gender, age, school grades and family background. Teachers were asked not to provide correct answers or feedback to the students after the pretest.

The post-test comprised an equivalent, slightly modified question set with 36 items. Similarly, the second post-test was a modified version of the first post-test. Teachers were asked not to teach to the tests and students had no incentive to study for the tests, since the results were not available to teachers and students were informed that the tests would not influence their grades. Teachers in all experimental groups and parents in the treatment groups with homework were asked to fill in a survey to collect background characteristics.

Online Appendix A gives more information about the sample selection and attrition in the two waves of the experiment. Figure A1 in Online Appendix shows that attrition occurred before the pretest, during the intervention and between the first and second post-test. Minor imbalances between treatment groups in student characteristics at

[9] As the questions capture different concepts covered in the course, factor analysis is by design inconclusive. The corrected item–total (point-biserial) correlation, however, demonstrates a good discrimination with values larger than 0.2 (Walstad and Rebeck 2017) for 4 of the 9 items of the financial literacy score, 2 of the 6 items of the knowledge score and 2 of the 3 items of the behaviour score.

baseline are due to the dropout of schools after randomisation, before the completion of the pretest. The sample of students who filled in the pretest amounted to 3,902 students, of which 3,165 students in wave 1 and 742 students in wave 2. Attrition also occurred between the pretest and the post-test. However, the differences in baseline measure of students across the experimental groups appear minor and no clear pattern can be identified. The final sample of students who completed both tests consists of 2,228 observations for wave 1, covering 40 schools with 164 classes. The final sample for wave 2 comprises 551 students from 24 schools, within total 58 classes. Online Appendix A also demonstrates that our sample of students is representative for the general student population of grades 8 and 9 in Flanders and that the financial literacy level of parents in our sample is comparable to the OECD assessments of adults in Belgium and other OECD countries.

Table 1 presents the baseline characteristics and test scores of the composite dataset of both waves, comprising 2,779 students from 65 schools. On the pretest, the mean financial literacy score was 3 out of 9 points. On the subset of six knowledge questions, students scored on average 2 of 6 points, while on the subset of three behaviour items, an average score of 1 of 3 points was obtained. There are no significant differences in pretest scores across experimental groups.

Next, we observe a significantly higher average homework quiz score in the 'homework together with parents' group, indicating that parents likely got involved in this group. Finally, students in the control group obtained an average post-test financial literacy score of 3 out of 9 points, similar to the pretest. All treatment groups had significantly higher mean post-test scores than the control group. Average scores on the second post-test are lower than on the first post-test, except for students who completed the homework with their parents.

4 Methodology

To identify the causal parameter of interest, we employ the following intention-to-treat (ITT) OLS estimation:

$$y^1_{i,j,k} = \alpha_0 + \alpha_1 y^0_{i,j,k} + \sum_{k=0}^{3} \beta_k treatment_k + \delta \sum X_i + \varepsilon_{i,j},$$

where $y^1_{i,j,k}$ represents the standardised post-test score of student i, in school j, in treatment k, $y^0_{i,j,k}$ denotes the standardised pretest score. $treatment_k$ stands for the experimental group each school was assigned to: control ($k = 0$), classroom intervention with no homework ($k = 1$), classroom intervention with homework alone ($k = 2$) or classroom intervention with homework with parents ($k = 3$). X_i is representative of a set of covariates of student characteristics at baseline. To conduct inference, the standard errors are clustered at the level of randomisation that is the school level j, to account for arbitrary correlations in the error terms between students within schools (Abadie et al. 2017; Cameron and Miller 2015). The equation is estimated for three outcome variables that is financial literacy as a composite indicator as well as its two

Table 1 Combined sample: summary statistics

	Control		No HW		HW alone		HW together	
Number of schools	18		16		18		13	
Share of private schools	0.667		0.813		0.833		0.846	
Number of classes	57		54		65		46	
Number of students	850		561		722		646	
Share by track								
Academic	0.872		0.907		0.713		0.839	
Technical	0.041		0.070		0.202		0.101	
Vocational	0.087		0.023		0.085		0.060	
Student characteristics								
Age	14.28	(0.557)	14.38	(0.613)	14.35	(0.582)	14.39	(0.732)
Female	0.552		0.590		0.558		0.544	
Dutch language grade	3.518	(0.982)	3.381	(1.027)	3.478	(1.005)	3.335	(1.045)
Mathematics grade	3.062	(1.291)	3.050	(1.264)	3.173	(1.295)	3.313	(1.200)
Socio-economic status (/4)	2.937	(1.013)	3.094*	(1.021)	2.971	(0.995)	3.062	(0.993)
Dutch	0.840		0.790		0.832		0.800	
Attitudes								
Self-assessment	3.500	(0.947)	3.449	(0.905)	3.435	(0.974)	3.377**	(1.001)
Importance of financial literacy	4.181	(0.743)	4.212	(0.714)	4.213	(0.702)	4.262	(0.723)
Frequency of saving	3.821	(1.161)	3.861	(1.138)	3.756	(1.171)	3.802	(1.157)
Importance of saving	4.210	(0.967)	4.200	(0.933)	4.198	(0.984)	4.251	(0.896)
Family communication	3.158	(1.133)	3.103	(1.143)	3.197	(1.139)	3.133	(1.211)

Table 1 continued

	Control		No HW		HW alone		HW together	
Pretest								
Financial literacy score (/9)	2.993	(1.490)	3.289	(1.578)	2.971	(1.554)	3.132	(1.586)
Knowledge score (/6)	1.836	(1.094)	2.075	(1.170)	1.884	(1.170)	2.003	(1.173)
Behaviour score (/3)	1.157	(0.817)	1.214	(0.789)	1.087	(0.793)	1.129	(0.838)
F-test for joint orthogonality			0.690		0.412		0.051	
Homework								
Homework score (/4)					2.547	(0.872)	2.927***	(0.879)
Post-test								
Financial literacy score (/9)	2.957	(1.639)	3.515**	(1.780)	3.501**	(1.768)	3.662**	(1.701)
Knowledge score (/6)	1.936	(1.199)	2.339**	(1.250)	2.314**	(1.237)	2.364**	(1.248)
Behaviour score (/3)	1.021	(0.892)	1.176*	(0.941)	1.187	(0.980)	1.298***	(0.930)
Second post-test								
Financial literacy score (/9)			3.255	(1.542)	2.908	(1.585)	3.713	(1.673)
Knowledge score (/6)			1.982	(1.147)	1.842	(1.201)	2.419	(1.303)
Behaviour score (/3)			1.273	(0.732)	1.066	(0.772)	1.294	(0.679)

Standard deviations in parentheses. Significance levels report differences to the control group from an OLS regression with standard errors clustered at school level, * $p<0.1$, ** $p<0.05$, *** $p<0.01$. Private (catholic) schools are publicly funded, but privately run. In grade 8, students have chosen a preparatory track, which we assigned to the three track levels. Dutch language and math grades refer to the past school term and are self-reported in five categories: <50%, 50–60%, 60–70%, 70–80%, >80%. Grading is not standardised and levels of examination differ by school and study tracks. We approximate socio-economic status by number of travels abroad in the past year (0, 1, 2, >2). Dutch (binary) indicates whether a student speaks Dutch at home. A five-point Likert scale of agreement measures: frequency of savings, opinion on importance of savings, regular family communication about financial topics, self-assessment of knowledge about financial issues, opinion on importance of financial literacy. The financial literacy score is a scale of nine points, with the subsets knowledge and behaviour of six and three points, respectively. For the F-test for joint orthogonality, all baseline characteristics at student level are jointly compared to the control group and the p-value is shown in the table. The statistics on the homework score refer to the sample of 946 students that completed the homework assignment. The statistics on the second post-test refer to the sample of 268 students that completed the second post-test. The control group did not receive the second post-test

dimensions of knowledge and behaviour, from the first and the second post-test. We compare the effects of the different treatment variants by testing the pairwise equality of the coefficients β_k with an F-test. The coefficient β_k identifies the effect of access to the treatment. While this represents a conservative estimate of the policy impact, it is useful, since non-compliance is likely to differ across jurisdictions if financial education were introduced. As we test multiple outcomes for multiple treatment groups, we present adjusted p-values based on the methodology suggested by List et al. (2019).

Since the attrition that occurred in all treatment groups between the pretest and the post-test could potentially bias the ITT estimates, we estimate a weighted regression, following the approach outlined by Wooldridge (2002) and illustrated by Ding and Lehrer (2010). We first compute the probability of remaining in the sample between the pretest and the post-test based on a probit estimation with all observable characteristics and then use the inverse probability to reweight the sample. This method ensures that the characteristics of the final sample are comparable to those of the initially selected sample at the pretest.

5 Results

5.1 Intention-to-treat effect

Table 2 presents the intention-to-treat estimates with inverse probability weights accounting for attrition, for financial literacy in the composite sample as well as in the two waves of the experiment separately. For each sample, the first regression only controls for the pretest score and the second regression additionally controls for all observed baseline characteristics.[10] The results reveal that access to the treatment significantly improves students' financial literacy, as students assigned to a treatment group perform significantly better than the students in the control group in the combined sample and wave 1. Lower levels of statistical significance in wave 2 might be due to the limited number of observations in this wave. Since the estimates are similar in the regressions with and without control variables, the correlations between the covariates and the treatment indicators appear to be weak. The following discussion is limited to the specifications with control variables.

The second column of Table 2 indicates that students assigned to the classroom intervention without homework score 0.25 standard deviations higher than the control group students. Students assigned to homework in addition to the classes improve 0.34 standard deviations compared to control and students assigned to homework with parental involvement improve 0.38 standard deviations. In wave 1, financial literacy improves most for students assigned to 'homework alone', whereas in wave 2, the assignment of 'homework together with parents' yields the most substantial

[10] This includes the track, age, gender, socio-economic status, Dutch language grade, mathematics grade, home language, family communication at baseline, attitudes towards the importance of saving and financial literacy, and self-assessment of financial knowledge.

Table 2 Intention-to-treat estimates: financial literacy

	Combined sample		Wave 1		Wave 2	
No HW	0.250**	0.249**	0.216*	0.219*	0.236*	0.141
	(0.096)	(0.099)	(0.109)	(0.119)	(0.134)	(0.138)
HW alone	0.329***	0.336***	0.319**	0.345***	0.317*	0.192
	(0.107)	(0.102)	(0.122)	(0.126)	(0.159)	(0.206)
HW together	0.382***	0.381***	0.384***	0.317**	0.278	0.344*
	(0.110)	(0.113)	(0.125)	(0.140)	(0.217)	(0.194)
Pretest score	0.366***	0.298***	0.354***	0.287***	0.344***	0.250***
	(0.020)	(0.021)	(0.023)	(0.023)	(0.052)	(0.045)
Controls	No	Yes	No	Yes	No	Yes
No HW vs. HW alone	0.431	0.277	0.390	0.191	0.571	0.763
No HW vs. HW with parents	0.200	0.157	0.171	0.402	0.837	0.239
HW alone vs. HW with parents	0.643	0.639	0.632	0.808	0.863	0.321
R-squared	0.161	0.205	0.149	0.197	0.126	0.205
N	2779	2779	2228	2228	551	551

*$p< 0.10$, **$p< 0.05$, ***$p< 0.01$. Intention-to-treat by OLS regression with inverse probability weights accounting for the probability to drop out of the sample as predicted by all baseline characteristics. Reference category: control group. Robust standard errors clustered at school level in parentheses. Bottom panel shows p-values of F-test for equality of coefficients. Control variables included are the track, age, gender, socio-economic status, Dutch language grade, mathematics grade, home language, family communication at baseline, attitudes towards the importance of saving and financial literacy, and self-assessment of financial knowledge.

Table 3 Intention-to-treat estimates: knowledge

	Combined sample		Wave 1		Wave 2	
No HW	0.267***	0.259**	0.233*	0.199	0.279*	0.202
	(0.100)	(0.106)	(0.116)	(0.125)	(0.138)	(0.130)
HW alone	0.313***	0.314***	0.276**	0.282**	0.433***	0.342*
	(0.105)	(0.099)	(0.120)	(0.122)	(0.123)	(0.175)
HW together	0.333***	0.318***	0.272**	0.178	0.414	0.488**
	(0.116)	(0.116)	(0.119)	(0.127)	(0.265)	(0.201)
Pretest score	0.262***	0.206***	0.243***	0.192***	0.244***	0.173***
	(0.020)	(0.021)	(0.020)	(0.023)	(0.060)	(0.051)
Controls	No	Yes	No	Yes	No	Yes
No HW vs. HW alone	0.633	0.518	0.709	0.439	0.125	0.393
No HW vs. HW with parents	0.543	0.553	0.735	0.860	0.585	0.149
HW alone vs. HW with parents	0.861	0.963	0.971	0.345	0.938	0.344
R-squared	0.092	0.124	0.075	0.108	0.084	0.164
N	2779	2779	2228	2228	551	551

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Intention-to-treat by OLS regression with inverse probability weights accounting for the probability to drop out of the sample as predicted by all baseline characteristics. Reference category: control group. Robust standard errors clustered at school level in parentheses. Bottom panel shows p-values of F-test for equality of coefficients. Control variables included are the track, age, gender, socio-economic status, Dutch language grade, mathematics grade, home language, family communication at baseline, attitudes towards the importance of saving and financial literacy, and self-assessment of financial knowledge

effect. The two waves of the experiment appear to be consistent, as the equality of the coefficients across waves cannot be rejected.[11]

The F-tests in the second panel of Table 2 establish that the differences between the indicators for each treatment group are statistically not significant. This implies that, on average, the assignment of homework and parental involvement do not significantly improve the learning outcomes of the financial education class.

Table 3 displays the ITT estimates for the knowledge score, which is composed from the subset of questions measuring financial knowledge in the financial literacy scale. In the composite sample, knowledge scores in all treatment groups are significantly higher than those in the control group. The knowledge scores of students assigned to treatment improve by 0.26 standard deviations without homework, 0.31 standard deviations with added homework and 0.32 standard deviations when parental involvement is prompted in addition. The differences between these coefficients are not statistically significant. Again, the coefficients of the indicators of each treatment group are consistent across the samples of the two waves.[12]

Table 4 shows that the results are different in the behavioural dimension of financial literacy, which is measured by the subset of questions in the financial literacy scale that refer to behavioural concepts. In the composite sample, assignment of the classroom intervention improves the behaviour score by 0.12 standard deviations, with the addition of a homework assignment by 0.21 standard deviations and with additional stimulation of parental involvement by 0.28 standard deviations. In wave 1, assigning homework, both with and without prompting parental involvement, has significantly larger effects on behaviour than the classroom intervention without homework. In wave 2, none of the interventions has a statistically significant effect. Although this might be due to the smaller sample size, the coefficients are very small and negative. While the coefficients of the classroom intervention and homework without parental involvement are consistent across waves, equality is rejected at the 5% level for the treatment with parental involvement.[13] This heterogeneous finding could be due to the age difference, since students in wave 1 are in grade 8 and students in wave 2 are in grade 9. In the composite sample, where we control for students' age, the homework assignment with stimulation of parental involvement provides significant value added to the classroom intervention. Yet, the difference compared to assigning homework without parental involvement is statistically not significant.

The results for all three outcome variables are comparable to estimates from an unweighted OLS regression, confirming that we do not observe any behavioural differences in the initial behavioural relationships as recorded at baseline (see Table B2 in Online Appendix).

[11] Hausman Chi-squared test, treatment 1: $\chi^2(1, N = 2,779) = 0.05, p = 0.826$), treatment 2: $\chi^2(1, N = 2,779) = 0.14, p = 0.709$, treatment 3: $\chi^2(1, N = 2,779) = 0.12, p = 0.727$ based on seemingly unrelated regression using the unweighted OLS estimation.

[12] Hausman Chi-squared test, treatment 1: $\chi^2(1, N = 2,779) = 0.04, p = 0.847$), treatment 2: $\chi^2(1, N = 2,779) = 0.48, p = 0.489$, treatment 3: $\chi^2(1, N = 2,779) = 2.64, p = 0.104$ based on seemingly unrelated regression using the unweighted OLS estimation.

[13] Hausman Chi-squared test, treatment 1: $\chi^2(1, N = 2,779) = 0.68, p = 0.409$), treatment 2: $\chi^2(1, N = 2,779) = 2.67, p = 0.103$, treatment 3: $\chi^2(1, N = 2,779) = 4.06, p = 0.044$ based on seemingly unrelated regression using the unweighted OLS estimation.

Table 4 Intention-to-treat estimates: behaviour

	Combined Sample		Wave 1		Wave 2	
No HW	0.110*	0.118*	0.091	0.141*	0.066	−0.009
	(0.061)	(0.066)	(0.064)	(0.075)	(0.129)	(0.128)
HW alone	0.194**	0.207**	0.224**	0.263***	0.010	−0.100
	(0.085)	(0.085)	(0.091)	(0.089)	(0.208)	(0.192)
HW together	0.265***	0.284***	0.347***	0.347***	−0.037	−0.014
	(0.083)	(0.088)	(0.091)	(0.111)	(0.109)	(0.147)
Pretest score	0.331***	0.279***	0.331***	0.276***	0.313***	0.232***
	(0.018)	(0.018)	(0.022)	(0.021)	(0.039)	(0.038)
Controls	No	Yes	No	Yes	No	Yes
No HW vs. HW alone	0.322	0.201	0.162	0.065	0.783	0.546
No HW vs. HW with parents	0.059	0.027	0.008	0.027	0.333	0.963
HW alone vs. HW with parents	0.484	0.377	0.276	0.405	0.808	0.535
R-squared	0.121	0.157	0.128	0.162	0.093	0.172
N	2779	2779	2228	2228	551	551

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Intention-to-treat by OLS regression with inverse probability weights accounting for the probability to drop out of the sample as predicted by all baseline characteristics. Reference category: control group. Robust standard errors clustered at school level in parentheses. Bottom panel shows p-values of F-test for equality of coefficients. Control variables included are the track, age, gender, socio-economic status, Dutch language grade, mathematics grade, home language, family communication at baseline, attitudes towards the importance of saving and financial literacy, and self-assessment of financial knowledge

As there are multiple outcomes and treatment groups, we are testing multiple null hypotheses simultaneously. We therefore present adjusted *p*-values based on the methodology proposed by List et al. (2019). As done by Lee and Shaikh (2014), we apply a differences-in-differences approach to account for initial differences in the outcome variable. Table 5 reveals that including the three outcome variables and accounting for pairwise testing of all experimental groups, leads overall to similar *p*-values as before adjustment. Only four *p*-values are not statistically significant at the 10% level anymore after adjustment. On financial literacy, all comparisons between experimental groups remain significant at the 10% level after adjustment. On knowledge and behaviour, scores of students in treatment group 1 do not differ significantly after adjustment from scores of students in the control group and in treatment group 2. It has to be noted that the unadjusted *p*-values differ slightly from the ITT results above and from the unweighted OLS estimation, as the methodology does not allow for the inclusion of covariates and the clustering of standard errors.[14]

The observed effect sizes are in line with previous interventions in financial education. Meta-analyses report an average effect on financial knowledge of 0.26 standard deviations in the general population (Kaiser and Menkhoff 2017), 0.2 standard deviations in randomised controlled trials (Kaiser et al. 2020) and 0.19 standard deviations in randomised controlled trials in school, with larger effect sizes in developed economies, immediate measurements and interventions of higher intensity (Kaiser and Menkhoff 2020). The average effect on financial knowledge of 0.3 standard deviations in this study is hence in accordance with previous studies, given the randomised design, the setting in a developed economy, four hours of instruction and an immediate post-test. With an average effect of 0.2 standard deviations, the effect we observe on financial behaviour is lower than the effect found for financial knowledge. This difference in effect sizes between financial knowledge and behaviour is smaller than in previous randomised controlled trials, for which an average effect of 0.08–0.1 standard deviations is reported on financial behaviour (Kaiser and Menkhoff 2020, 2017; Kaiser et al. 2020). This is likely due to the fact that we measure simulated behaviour rather than actual behaviour. In combination with a homework with parents, Bruhn et al. (2016) find an effect of 0.25 standard deviations of a financial education course at school. Given that in our study, the outcomes were measured more immediately, the effect of 0.32 standard deviations of the classroom intervention with parental involvement on financial knowledge seems comparable.

5.2 Heterogeneous effects

The previous research has found higher parental income to be associated with higher perceived parental influence on financial behaviour (Jorgensen and Savla 2010). Also, better educated parents might have a better sense of efficacy to help their child (Avvisati

[14] Table B1 in Online Appendix demonstrates that all significant results of the main regressions remain statistically significant at 10% level when correcting for multiple hypothesis testing following the methodology proposed by Anderson (2008) which includes clustered standard errors and covariates. Lehrer et al. (2021) suggest an alternative approach for statistical inference using the block bootstrap, which could be considered in future work to apply multiple testing procedures when standard errors are clustered.

Table 5 Multiple hypothesis testing

	Financial Literacy		Knowledge		Behaviour	
	Unadj. *p*-value	Adjusted *p*-value	Unadj. *p*-value	Adjusted *p*-value	Unadj. *p*-value	Adjusted *p*-value
Control vs. no HW	0.012**	0.078*	0.047*	0.216	0.071*	0.263
Control vs. HW alone	0.000***	0.000***	0.000***	0.000***	0.000***	0.000***
Control vs. HW together	0.000***	0.000***	0.001***	0.006**	0.000***	0.000***
No HW vs. HW alone	0.003***	0.020**	0.047*	0.233	0.020**	0.120
No HW vs. HW together	0.006***	0.039**	0.305	0.577	0.001***	0.004***
HW alone vs. HW together	0.938	0.938	0.339	0.471	0.267	0.610

*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$. Adjustment of *p*-values for multiple hypothesis testing based on the methodology proposed by List et al. (2019), using first differences of the outcome variables. We account for using multiple outcomes variables as well as pairwise comparisons between multiple treatment groups. The *p*-values indicate the significance of the difference between experimental groups in the first differences of the means of the outcome variables

et al. 2010). We therefore estimate heterogeneous effects by socio-economic status (SES).

SES is proxied by the number of travels abroad in the past year, since this could be self-reported by students.[15] We split the sample in two groups of similar size that is students who travelled at most once (low SES) and students who travelled at least twice (high SES).

The first three columns of Table 6 present the results from a regression that includes interaction effects of the treatment indicators with the indicator of low SES. This specification assumes that having a lower socio-economic status only has a differential impact on treatment, while the coefficients for all other variables are constrained to be constant across the two subgroups. On financial literacy, none of the interaction effects are statistically significant. On knowledge, the interaction term is negative and statistically significant for the classroom intervention group without homework, suggesting that assignment of this treatment benefits students with a higher socio-economic status. Regarding behaviour, the interaction term with the parental treatment is positive and statistically significant at the 10% level, indicating that prompting parental involvement benefits students with a lower socio-economic status in the behavioural dimension.

These results contrast the survey-based findings that the positive behavioural effects of parental involvement can only be reaped for students from families with a higher socio-economic status (McNeal 2001). Instead, our results match the hypothesis that parents can help by motivating their children to learn, independent of their own background, for example, through discussions (Heddy and Sinatra 2017; Harackiewicz et al. 2012). Therefore, parental involvement measures could be particularly useful for families with low parental involvement in education.

This hypothesis is confirmed when considering the heterogeneous effects by family communication about the course topics that is an approximation of parental involvement, at baseline.[16] The last three columns of Table 6 display the results of a regression with interaction terms of the treatment indicators with the indicator of low family communication. On financial literacy, there are no statistically significant differences in the estimate for parental involvement between the two subgroups. On the knowledge dimension, the estimate for the parental involvement intervention is significantly larger for students with regular family communication at baseline. On the behaviour outcome, however, assigning parental involvement yields significantly larger improvements for students with low family communication at baseline.

[15] Online Appendix A demonstrates the validity of this indicator based on correlations with indicators of socio-economic status in official school data as well as Flemish budget data. The pretest included a question about the educational level of the mother, but 39% of students indicated not knowing this.

[16] Family communication at baseline is measured on a 5-point Likert scale ('At home, I regularly speak about financial topics, such as savings, costs and payment methods': completely disagree–disagree–no opinion–agree–completely agree). The sample is split into two groups of similar size, with a group of students with low levels of family communication (1–2) and a groups of students with high levels of family communication (4–5).

Table 6 Heterogeneous effects: family communication and socio-economic status

	SES			Family Communication		
	Financial Literacy	Knowledge	Behaviour	Financial Literacy	Knowledge	Behaviour
No HW	0.283***	0.316***	0.106*	0.240**	0.259*	0.100
	(0.099)	(0.111)	(0.063)	(0.115)	(0.137)	(0.079)
HW alone	0.365***	0.333***	0.234**	0.438***	0.409***	0.269***
	(0.113)	(0.111)	(0.098)	(0.120)	(0.127)	(0.095)
HW together	0.376***	0.354***	0.228**	0.406***	0.409***	0.209*
	(0.116)	(0.130)	(0.094)	(0.139)	(0.146)	(0.113)
Low SES	0.097	0.122	0.017			
	(0.078)	(0.078)	(0.086)			
No HW x Low SES	−0.108	−0.180*	0.038			
	(0.082)	(0.093)	(0.093)			
HW alone x Low SES	−0.081	−0.057	−0.075			
	(0.082)	(0.078)	(0.096)			
HW together x Low SES	0.028	−0.107	0.194*			
	(0.132)	(0.138)	(0.116)			
Low Family				−0.029	−0.047	0.009
Communication				(0.107)	(0.130)	(0.131)
No HW x Low				0.006	−0.027	0.047

Table 6 continued

	SES			Family Communication		
	Financial Literacy	Knowledge	Behaviour	Financial Literacy	Knowledge	Behaviour
Family Communication				(0.122)	(0.123)	(0.104)
HW alone x Low				−0.210**	−0.223**	−0.094
Family Communication				(0.087)	(0.110)	(0.092)
HW together x Low				−0.074	−0.253**	0.198**
Family Communication				(0.092)	(0.107)	(0.085)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.206	0.125	0.159	0.206	0.127	0.159
N	2779	2779	2779	2193	2193	2193

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ Intention-to-treat by OLS regression with inverse probability weights accounting for the probability to drop out of the sample as predicted by all baseline characteristics. Reference category: control group. Robust standard errors clustered at school level in parentheses. The regressions with the interaction with family communication exclude students who indicated 'no opinion'

5.3 Persistence over time

To test whether the positive treatment effect persists several weeks after the first post-test at the end of the class, we evaluate a second post-test, taken by students in the treatment groups on average seven weeks after the end of the intervention period.[17] In both waves combined, only 268 students completed the second post-test which was given as a take-home quiz. Attrition occurred as many teachers did not assign the test to their students and students had lower incentives to complete the quiz because the course had ended. Therefore, we only evaluate the test in the composite sample.

Table 7 shows that the effects of the intervention on the financial literacy score persist, but are smaller than the immediate results. This is in line with previous experiments on financial education in school which report a decay of effect sizes over time (Kaiser and Menkhoff 2020). On financial literacy and knowledge, the estimates for the intervention prompting parental involvement are larger than those for the treatments without parental involvement. On financial literacy, the effects equal 0.15 standard deviations for the classroom intervention, 0.12 standard deviations when homework was assigned in addition and 0.22 standard deviations when parental involvement was stimulated. Partly due to the small sample size, statistical significance is low. Only the coefficient of the parental involvement treatment remains significant at the 10% level. The same holds true for the effects on knowledge, where none of the coefficients of treatment are statistically significant. For behaviour, the classroom intervention has a significant effect of 0.3 standard deviations, while adding stimulation of parental involvement yields 0.13 standard deviations. However, the estimates of the different experimental groups might differ in precision due to unbalanced compliance rates.

5.4 Robustness

As an alternative approach to account for the potential imbalances between experimental groups resulting from attrition, we apply matching.[18] Intention-to-treat regressions on the matched samples yields similar results as the main ITT regressions with inverse probability weights (see Table B3 in Online Appendix).

Based on an instrumental variables (IV) approach, we establish that the effects found in the ITT regressions in Sect. 5.1 hold true when accounting for non-compliance with the homework assignment. Compliance with the homework differs from attrition, since students who did not complete the homework proceeded to completing the post-test and did not leave the sample. Compliance was on average 70% among students in the final sample.[19] The IV estimates identify the average treatment effect on the treated and accordingly scale up the ITT effects from Sect. 5.1 (see Table B4 in Online

[17] In the regression analysis, we control for the exact time lag between the first and the second post-test. For the control group, the scores of the first post-test are used, assuming that no further changes would have occurred without treatment. Indeed, the change from the pretest to the first post-test is negligible for the control group (see Table 1).

[18] Based on a set of student characteristics at baseline, we match students of each treatment group to students in the control group using exact matching on coarsened variables, following the Coarsened Exact Matching approach proposed by Blackwell et al. (2009).

[19] Compliance with the homework assignment is discussed more in detail in Online Appendix A.

Table 7 Intention-to-treat estimates of the second post-test scores

	Financial Literacy	Knowledge	Behaviour
No HW	0.152	−0.002	0.297**
	(0.163)	(0.152)	(0.142)
HW alone	0.121	0.107	0.081
	(0.152)	(0.180)	(0.113)
HW together	0.218*	0.205	0.128*
	(0.127)	(0.151)	(0.070)
Pretest score	0.256***	0.176***	0.243***
	(0.023)	(0.026)	(0.026)
Time lag	−0.005*	−0.007**	−0.001
	(0.003)	(0.003)	(0.003)
Controls	Yes	Yes	Yes
No HW vs. HW alone	0.859	0.573	0.176
No HW vs. HW with parents	0.702	0.265	0.216
HW alone vs. HW with parents	0.552	0.651	0.678
R-squared	0.178	0.096	0.166
N	1118	1118	1118

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$ Intention-to-treat by OLS regression with inverse probability weights accounting for the probability to drop out of the sample as predicted by all baseline characteristics. Reference category: control group. Robust standard errors clustered at school level in parentheses. Bottom panel shows p-values of F-test for equality of coefficients. Control variables included are the pretest score, track, age, gender, socio-economic status, Dutch language grade, mathematics grade, home language, family communication at baseline, attitudes towards the importance of saving and financial literacy, self-assessment of financial knowledge, and the time lag between the post-test and the second post-test

Appendix). The value added of stimulating parental involvement remains statistically non-significant.

Since privately run schools represent with 85% of schools in the sample a slightly higher share than the 75% in Flanders, we estimate the ITT effects for the subset of privately run schools. Privately run schools might be attractive for students with higher abilities and a higher socio-economic status. Table B5 in Online Appendix reveals that the results are similar to the main results from Sect. 5.1, with smaller coefficients. This suggests that the larger effect sizes in the main result are driven by the 15% of publicly run schools.

A robustness test further confirms the minimal role played by the teachers in the standardised intervention. Interaction effects of teacher characteristics with the treatment dummies are jointly insignificant ($F(9, 57) = 1.72$, $p = 0.104$).[20]

[20] The characteristics include years of teaching experience, experience with financial education, and financial knowledge, behaviour and attitudes.

6 Discussion and conclusion

This paper provides evidence on the intention-to-treat effects of stimulating parental involvement in financial education based on two large-scale randomised controlled trials with 2,779 students from grade 8 and 9 in Flanders. The results indicate that access to the financial education intervention effectively improved students' financial knowledge and behaviour. Providing access to a classroom intervention with a homework prompting parental involvement increased financial literacy scores by 0.38 standard deviations. This effect was statistically not different from the effects of assigning homework without stimulating parental involvement. As these results reflect the effect of access to the treatment, they represent a conservative estimate of the true effects of treatment.

Against the popular belief that homework and parental involvement would only benefit students of a higher socio-economic status, we find that access to an intervention that stimulates parental involvement matters the most for disadvantaged students. In the behavioural dimension, the intention-to-treat effect of prompting parental involvement in homework was higher for students of a low socio-economic status and students with low baseline levels of family communication.

The results show that the dynamic complementarity between home and school inputs appears to be stronger in the behavioural dimension than on knowledge. Possibly, parents can be a link between the classroom and everyday life of children (Heddy and Sinatra 2017).

Our findings have important policy implications. The results confirm that providing financial education at school can be effective, paving the way for the integration of financial education in compulsory education. Schools can employ parental involvement measures to improve behavioural outcomes, in particular of disadvantaged students. Next to financial education, this could be applied to other topics with implications on students' daily life, such as health, nutrition or e-safety (such as Sylvia et al. 2013; Vanderhoven et al. 2014).

This study confirms the promising potential of stimulating parental involvement in education. Additional research is needed on parental costs and benefits of parental involvement measures, since it is possible that parents also learn when involved in their children's education. Moreover, future research should explore whether the effects of prompting parental involvement differ in settings of longer duration or intensity.

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