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A large-scale randomized-controlled trial in elementary schools

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Abstract

We examine individual-level determinants of interest in STEM and analyze whether a digital web application for elementary-school children can increase children's interest in STEM with a specific focus on narrowing the gender gap. Coupling a randomized-controlled trial with experimental lab and survey data, we analyze the effect of the digital intervention and shed light on the mechanisms. We confirm the hypothesis that girls demonstrate a lower overall interest in STEM than boys. Moreover, girls are less competitive and exhibit less pronounced math confidence than boys at the baseline. Our treatment increases girls' interest in STEM and decreases the gender gap via an increase in STEM confidence. Our findings suggest that an easy-to-implement digital intervention has the potential to foster gender equality for young children and can potentially contribute to a reduction of gender inequalities in the labor market such as occupational sorting and the gender wage gap later in life.

JEL Classification numbers: C93, D91, I24, J16, J24
Keywords: STEM, digital intervention, gender equality, field experiment

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1 Introduction

Women are less likely than men to specialize in STEM fields, i.e., in science, technology, engineering, and mathematics, in many Western countries (Ginther and Kahn, 2004; Bertrand et al., 2010). As a consequence, women are underrepresented in STEM jobs that provide, on average, higher wages, hold relatively good chances for permanent employment, and increase the likelihood of taking up leadership positions (Joensen and Nielsen, 2016; Kahn and Ginther, 2018; ILO, 2019).[1] In the following, we call this phenomenon the gender STEM gap. We currently know surprisingly little about why women still remain underrepresented in STEM fields and which interventions might work to close the gender STEM gap.

Providing equal opportunities for women and men, i.e., closing the gender STEM gap, as one measure to narrow the gender pay gap, is a relevant societal objective in many countries. As gender differences in ability cannot explain the gender STEM gap (Hyde et al., 2008; Kahn and Ginther, 2018; Jiang, 2021), creating more interest in a specialization in STEM among girls might be a viable strategy. Moreover, global challenges such as climate change, the health crisis from the pandemic, and the fourth industrial revolution require more young talents, girls and boys, to enter STEM-oriented education tracks to meet the increasing demand for STEM professionals (Cedefop, 2015; UNESCO, 2021).[2]

In this paper, we implement a large-scale randomized-controlled trial (RCT) in schools. The treatment is a digital intervention, more specifically, a web-based application for children. The intervention addresses potential behavioral mechanisms that allegedly contribute to the STEM gap between girls and boys. We measure the overall effect of the intervention on STEM interest, and we decompose the overall effect into main behavioral mechanisms. The RCT is implemented in elementary schools in Austria, with 1,133 children as participants.

Our focus is on four potential behavioral mechanisms contributing to the gender STEM gap. First, children may be more likely to associate STEM with boys than with girls. Consequently, girls with more pronounced stereotypical beliefs concerning STEM and gender may demonstrate less interest in STEM. Second, a growth mindset, i.e., believing that you can increase your performance by practicing and using the appropriate learning techniques, may be important for establishing an interest in STEM (Kahn and Ginther, 2018; Bettinger et al., 2018; Alan et al., 2019). Third, Buser et al. (2017) show that more pronounced competitive preferences can explain the interest in STEM and, ultimately, the decision for a STEM career. Fourth, the belief in one's abilities may be relevant for building up an interest in STEM (Carlana, 2019).

[1]The higher wages in STEM professions can be explained by a relatively high demand in these jobs coupled with the specific skill set that job candidates have to acquire to work in these fields (Deming and Noray, 2018). Obviously, an intervention such as ours will not shift the equilibrium wages through a much higher general supply in STEM professionals or lead to a change of wages in the profession due to an increased share of female workforce (Levanon et al., 2009).

[2]More generally, providing equal opportunities for boys and girls to thrive and closing gender gaps in the labor market are current goals of the European Commission (see https://ec.europa.eu/info/policies/justice-and-fundamental-rights/gender-equality/gender-equality-strategy_en).

The treatment web application (treatment app) intends to increase interest in STEM *directly* by increasing knowledge and awareness about STEM professions and *indirectly* by addressing the underlying behavioral mechanisms that could interfere with the development of interest in STEM. The treatment app presents both fictitious and real STEM professionals, such as engineers and programmers, on fantasy planets. Accompanied by the professionals, the children playfully learn more about various societal challenges, such as threats from climate change and to public health, and how STEM skills can contribute to combating them. The storyline of the app comprises exercises, videos, and texts. The app also informs children about STEM-related content in general. To address the behavioral mechanisms, the app uses tutorials, exercises, and (non-monetary) rewards that teach children a growth mindset and improve their self-confidence and competitive aptitude. Moreover, the app introduces female STEM role models to overcome stereotypical beliefs.

To test the app's effect, we recruited 39 elementary schools in Vienna (an urban area) and Upper Austria (a predominantly rural area). Our target participant group is third-graders, i.e., children are around nine years old. We randomly assigned about half of the schools to the treatment and the other half to the control group. Whereas the control group used a "traditional" learning app without a STEM focus (control app), the treatment group used the treatment app *with* a STEM focus. We implemented the study as an out-of-school intervention and asked children and their parents to engage with the app each weekday for about ten minutes for four weeks. We collected data in class using incentivized decisions and survey measures before (baseline) and after the intervention.

Our RCT focuses on elementary schoolchildren's STEM interests for various reasons. First, aptitudes toward particular educational fields seem likely to develop early in life (Tai et al., 2006; DeWitt et al., 2013). That is, boys and girls start to differ in crucial determinants for educational decisions such as preferences and beliefs already at a young age (e.g., Dahlbom et al., 2011; Buser et al., 2014; Bian et al., 2017). Second, interests will guide children's skill investments, which are important inputs into their education production functions, shaping later life choices and outcomes (Heckman, 2006; List et al., 2018, 2021). Relevant education choices such as track/school choice are made as early as elementary school in Austria (at an age below or around ten years) and might be important in determining a child's preparedness to pursue a STEM career later on (Delaney and Devereux, 2019; Card and Payne, 2021). Third, interventions may be more effective at an early age when particular interests and preferences are still malleable (e.g., Heckman et al., 2010; Cappelen et al., 2020).

Our data confirm the expected baseline gender differences for confidence and competition. We find that baseline levels of math confidence and competitive preferences are significantly less pronounced among girls than boys. Moreover, we find that girls have more of a growth mindset than boys and are less prone to stereotypical thinking. Our treatment app increases girls' interest in STEM significantly, and, as a consequence, narrows the gender STEM gap between boys and girls. In a mediation analysis, we show that our measure capturing STEM confidence explains the majority of the treatment effect on girls' STEM interest. The treatment increases girls' competitiveness by seven percentage points. However, we do not find a mediating effect from competitiveness on STEM interest.

Our intervention is easy to implement and not very costly. We do not claim that the design of the intervention, including the treatment app, the duration of exposure, the age of participants, or the implementation procedure, is optimal. However, the design of the intervention, of course, took into account existing recommendations for the design of web applications for children based on previous studies. Nonetheless, we see our results as a proof of concept: addressing behavioral mechanisms explicitly in a learning app can decrease the gender STEM interest gap. More research is needed on similar interventions to understand whether specific parameters or specific types, e.g., out-of-school or in-school interventions, work perhaps even better. Importantly, there is a need to evaluate whether the effects on STEM interest are permanent or only temporary. We review issues related to this question in the discussion and the conclusion.

Our study contributes to the literature on early-childhood interventions aimed at improving later labor-market outcomes. While the bulk of the research has focused on cognitive skills, more recently, the importance of non-cognitive skills and personality traits for labor-market success has received more attention (e.g., Heckman, 2000; Currie, 2001; Almlund et al., 2011; Kautz et al., 2014; Cappelen et al., 2020; Kosse et al., 2020).[3] For example, Alan et al. (2019) and Alan and Ertac (2018a) succeed in increasing children's grit and patience in school interventions. Such intervention studies that aim to influence how preferences and traits play out are still rare. We contribute to this literature by demonstrating in the context of a large-scale intervention study how children's interest in STEM, which is strongly correlated with more favorable labor-market outcomes, can be fostered.[4]

In general, there is scant research on early-childhood interventions with a focus on fostering gender equality in educational and labor market outcomes. We know from numerous studies that women are less inclined to compete, and studies also demonstrate a link between competitiveness and actual educational and labor-market choices and outcomes (Buser et al., 2014; Flory et al., 2015; Buser et al., 2021). Alan and Ertac (2018b) and Hermes et al. (2021) demonstrate in randomized controlled trials that their grit and feedback intervention, respectively, reduce the gender gap in competitive preferences. We contribute to this literature by developing an intervention that aims at attenuating the gender STEM gap. Ultimately, such a reduction in the gender STEM gap should improve women's chances in the labor market.

There is a vast literature on potential underlying mechanisms for the gender STEM gap

[3] Many of these studies put an emphasis on children from low socio-economic status families (see for example Heckman et al., 2010; Cook et al., 2014; Heller, 2014; Oreopoulos et al., 2017; Kosse et al., 2020). Other studies with children show the effectiveness of interventions at improving later labor-market outcomes by fostering traits such as a growth mindset, patience, and competitiveness, without a focus on a specific target group (e.g., Alan and Ertac, 2018a,b; Sorrenti et al., 2020; Rege et al., 2021).

[4] In this respect, another related study is Cohodes et al. (2022) investigating the effect of several STEM summer programs on educational outcomes of underrepresented high-school students.

(for an overview see McNally, 2020). The majority of these studies focuses on adolescents and educational decisions later in life, e.g., on the influence of the gender composition in high school classes on the propensity to become a STEM professional (e.g., Brenøe and Zölitz, 2020), on the role of prior course choices on STEM degree selection (Delaney and Devereux, 2019; Card and Payne, 2021), or the effects of teachers' stereotypical bias on girls' math performance and confidence (Carlana, 2019). We add to this literature by examining individual attributes and their impact on children's interest in STEM at a young age. STEM interests may start to develop around the age of our participants. We developed a novel measure of STEM interest that is appropriate for our study, taking into account the age of our young participants and the country-specific context. The measure enables us to study the gender divergence of STEM interest at an earlier age than most previous studies. In the context of this line of research, we show that the gender difference in interest in STEM emerges at an early age and we examine the relevance of underlying behavioral mechanisms at the age at which gender differences in vocational interests may start to emerge (Ambady et al., 2001; Gottfredson, 1981).

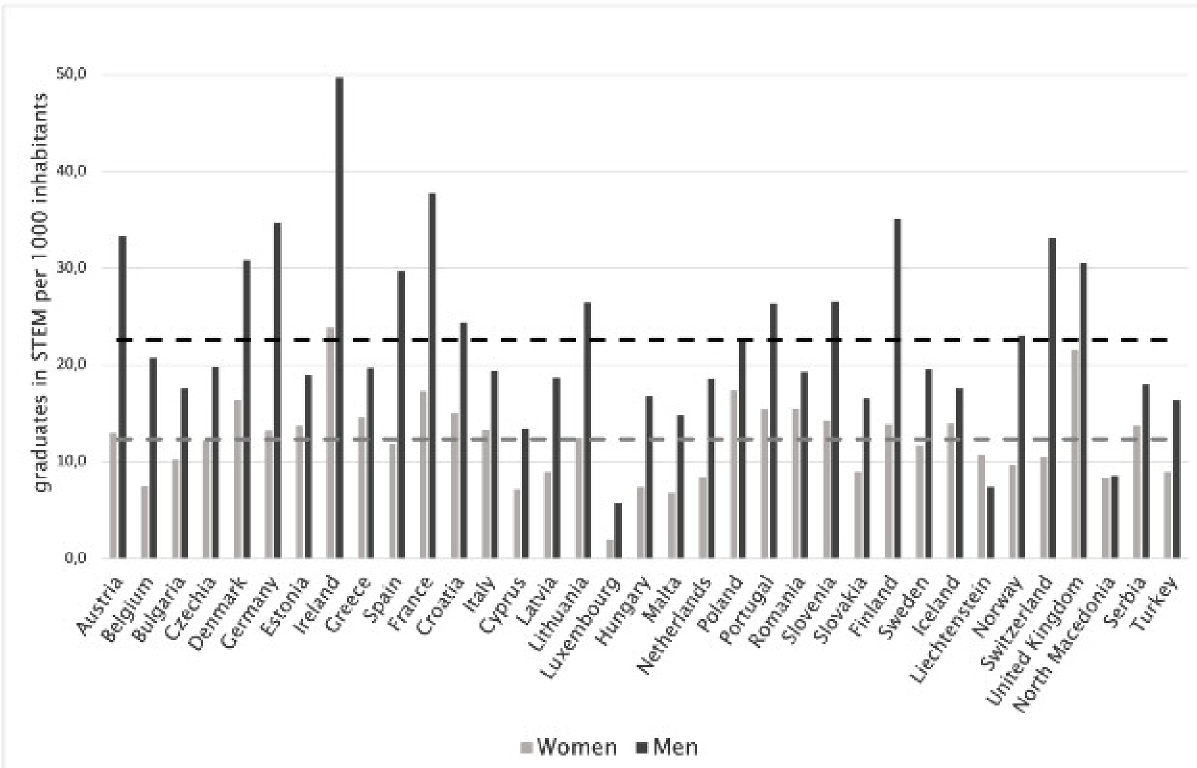
Other existing research related to our study is concerned with online education apps that often provide an easily scalable, cost-effective way of learning at an individual pace with individual feedback, holding the potential to improve educational outcomes including STEM skills (Mayo, 2009; Berkowitz et al., 2015; Escueta et al., 2020). Berkowitz et al. (2015) demonstrate that an app for first graders and their parents used out-of-school can reduce parents' math anxiety and, consequently, improve children's educational attainment in math. We add to the literature by demonstrating that an easy-to-use and not very costly app applied out-of-school can increase girls' STEM interest.

The remainder of this paper is organized as follows. In Section 2, we describe the institutional background of our research site. In Section 3, we present details of the intervention, discuss our hypotheses, explain the study design, and provide internal balancing tests. Section 4 presents results first on baseline gender differences, then general treatment effects, and eventually examines the influence of underlying behavioral mechanisms on STEM interest in more detailed analyses. In Section 5 we discuss our results, potential limitations, and policy implications. We conclude in Section 6.

2 Institutional background and country context

In Austria, the unadjusted gender pay gap was 18.9% in 2020, which is higher than the EU average of 13% (Eurostat, 2018). A large proportion of the gender pay gap can be explained by differences in payment structures of economic sectors and gender differences in occupational choice (Brown and Corcoran, 1997; Blau and Kahn, 2000, 2017). In Austria, men graduate about three times more often from STEM study programs, i.e., in science, mathematics, computing, engineering, manufacturing, and construction, than women (Eurostat, 2019). The gender difference in STEM graduates in Austria is relatively large (44%) compared to the average in European countries (30%). We illustrate the gender differences in STEM graduates across European countries in Figure 1.

Figure 1: Share of female and male STEM graduates across European Countries (source: EUROSTAT 2019; own illustration)



Generally, after four years of elementary school, children in Austria move to lower secondary school at the age of ten years. They can choose either a general secondary school (middle school) or an academic secondary school (high school), partly depending on their performance in elementary school. Lower secondary schools can have a specific specialization such as languages, music education, or STEM. The specialization is implemented either through additional subjects offered in school or through an increased number of class hours in some of the subjects. After four years in a lower secondary school, at age 14 teenagers can either choose to attend a school preparing them for vocational training, stay in the academic secondary school, or attend a college for higher vocational training.[5] While female students are over-represented in colleges with a focus on business administration or health care, the share of female students choosing to attend a school with a technical specialization is 26% and thus rather low (Statistik Austria, 2019). Focusing on the three most popular options that teenagers choose for their vocational training, women are most likely to become saleswomen, office managers, or hairstylists, while the top three vocational tracks for boys are metal technology, electrical engineering, and au-

[5]The description above simplifies the options; it focuses on the most common options.

tomobile technology (Wirtschaftskammer Oesterreich, 2020). An analogous picture regarding gender differences emerges when focusing on tertiary education.

3 Study design

We preregistered the study design and the analysis at the AEA RCT Registry. For the write-up of the paper, we make very few adaptations to the preregistration that we did not foresee at the time of preregistration, following Banerjee et al. (2020). The adaptations make the analysis more straightforward and simplify the reading of the paper. We preserve transparency beyond the preregistration by indicating the few changes (some variable definitions) explicitly and by providing an online appendix where we execute all the analyses as defined in the preregistration.[6]

3.1 Experimental groups

The core of our experiment consists of a treatment group that uses the treatment app and a control group that uses the control app. We implemented a sub-treatment informing parents of the usefulness of STEM skills for children using a cross-over design. Below, we describe our treatments in detail.

Treatment app

The treatment app is called "Robitopia" and is designed for an average usage time of ten minutes per school day over four weeks (see the Online Appendix for visualizations of the treatment app). It features an animated journey for children with a spaceship through a fantasy universe. Research has shown that children have biased beliefs about STEM professionals and their contributions to societal challenges. Such biased beliefs could constitute an impediment for the development of STEM interest, particularly in girls (Shin et al., 2019). The treatment app counteracts such biased beliefs, by letting children visit four different planets that face different societal challenges each week, relating the mastering of these challenges to STEM skills. Children use insights from STEM provided by STEM professionals to solve the weekly challenges step by step each day. In the treatment app, there is, for example, a planet with clean air, and children explore the technologies used and the ways of living that inhabitants follow to maintain air quality. Within the story, the children watch videos, read texts, play games, and meet avatars of natural scientists as well as other STEM professionals. While playing, children earn stars when they read, watch tutorials, and solve exercises that engage them playfully. Stars are also a game currency and can be used to pimp the spaceship. STEM professionals on the app are from different disciplines and education levels, ranging from engineers and programmers to technical assistants.

[6] The preregistration can be found at: https://doi.org/10.1257/rct.5014-1.1. We provide the Online Appendix on our websites: https://sites.google.com/view/kerstin-grosch/research and https://sites.google.com/view/simonehaeckl

The treatment app also addresses the aforementioned behavioral mechanisms. To foster the children's growth mindset, the app contains a tutorial about how the brain cells connect each time they learn something and work hard to solve tasks. To increase confidence and competitiveness, we let children guess their performance after they have worked on one of the app's exercises and reward them for correct guesses with the "star" game currency. Moreover, children earn badges, e.g., a math or a science badge, when they deal with content in the app. To decrease stereotypical thinking for STEM ability, the treatment app shows mixed-gender (and mixed-ethnic) teams and videos in which young female STEM professionals introduce themselves, serving as potential role models (à la Bettinger and Long, 2005; Breda et al., 2020; Porter and Serra, 2020) and counteracting existing stereotypes about STEM professionals (Miller et al., 2018).

Control app

As a control condition, we expose children to an existing learning app called "Anton" that is supported by the European Union and is publicly accessible. This app offers exercises mainly to train language skills (German) and mathematics for different school grades. For each class in our study, we created a group and ten-minute exercises to solve that would motivate the children and make control and treatment conditions comparable concerning usage time.

Sub-treatment: brochures

In a sub-treatment, we handed out information brochures for parents (see the Online Appendix for the original version in German). These brochures are designed to inform parents about the importance of STEM and to advise ways to increase children's interest in science and technology. Importantly, we only handed out brochures when the main intervention period had ended to avoid spillover effects (see the experimental schedule in Section 3.4). We present the results of the brochure intervention in Appendix D.

3.2 Hypotheses

As described above, the treatment app playfully introduces children to STEM topics and takes them on an individual journey (e.g., Mayo, 2009). The app counteracts biased beliefs about STEM fields, e.g., that STEM professionals work on abstract problems rather than contributing to finding solutions to societal challenges. Moreover, children actively engage with STEM-related content in different learning environments, i.e., tutorials, videos, and hands-on exercises. This way, the treatment app by itself may increase children's interest in STEM *directly*.

Hypothesis 1:
The treatment app increases children's interest in STEM compared to the control app.

There are fewer women than men specializing in STEM professionally (Blau and Kahn, 2000; Blau et al., 2013). Moreover, research has shown that adolescent girls are less interested in STEM than boys (e.g., Buser et al., 2017; Carlana, 2019). Furthermore, there is evidence that preferences, skills, and tastes differ already in elementary school between girls and boys (e.g., Fehr et al., 2008; Sutter et al., 2019; Brocas and Carrillo, 2021). Bringing these strands of the literature together, we expect that girls are, on average, less interested in STEM fields than boys already in elementary school. Assuming that girls exhibit less pronounced baseline preferences for STEM, there is more room for girls to increase their STEM interest than for boys. Hence, we expect that girls' STEM interest responds more strongly to the treatment app than boys'.

Hypothesis 2:
On average, girls exhibit lower levels of interest in STEM fields than boys. The treatment app leads to a higher increase in STEM interest for girls than for boys.

We identified four behavioral mechanisms potentially affecting girls' interest in STEM. First, children may hold stereotypical beliefs of boys' and girls' performance in a stereotypical male area such as math compared to a stereotypical female area such as language (German) (Reuben et al., 2014; Kollmayer et al., 2018). In general, STEM fields are associated more with men than with women (e.g., Shapiro and Williams, 2012). This stereotype forms as early as in elementary school (Cvencek et al., 2011; Miller et al., 2018). When girls do not envision themselves in STEM-related fields, they are deterred from entering by the potentially wrong perception that STEM is a "men's field" where girls do not belong (Shapiro and Williams, 2012).[7] Hence, we expect that girls with stronger stereotypical beliefs are less interested in STEM.

Second, we examine the effect of the children's way of thinking about the nature of talents, i.e., whether they are fixed or can develop in response to a dedicated effort. A "growth mindset" indicates the latter way of thinking. There is evidence that children who acquire a growth mindset are more likely to enroll in advanced mathematics courses and demonstrate higher levels of perseverance and performance in mathematics (e.g., Blackwell et al., 2007; Bettinger et al., 2018; Yeager et al., 2019; Alan et al., 2019). Mathematics is an essential aspect of most STEM subjects. Hence, we expect that having more of a growth mindset is positively associated with STEM interest.

Third, STEM subjects are often perceived as more prestigious and competitive than other fields such as the social sciences (Buser et al., 2014). In line with this view, it has been found that more pronounced competitiveness can partly explain STEM interest and, ultimately, the decision for a science focus later in school (Buser et al., 2014, 2017). In addition, there is

[7]To reduce stereotypes, children can be exposed to counter-stereotypical role-models (Olsson and Martiny, 2018). Studies empirically investigating the effect of those role models range from correlational evidence, controlling for the employment status of the mother (Eagly et al., 2000), exploiting exogenous variation in teachers' gender (e.g., Eble and Hu, 2020; Dee, 2005, 2007) to randomized control trials exposing participants randomly to certain role models (e.g., Porter and Serra, 2020; Breda et al., 2020)). The latter studies are closely related to this paper as they study the effect of short-term interventions with experimental methods.

evidence that gender differences in the willingness to compete exist (e.g., Gneezy et al., 2003; Niederle and Vesterlund, 2010) and emerge early in life (e.g., Dreber et al., 2014; Sutter and Glätzle-Rützler, 2015). Therefore, we expect that girls are, on average, less competitive than boys. Because of the lower baseline level of girls' competitiveness, the treatment app might be particularly successful in promoting competitiveness in girls.

Fourth, self-confidence in STEM-relevant skills is important for choosing a STEM track. Murphy and Weinhardt (2020) demonstrate that the academic rank in class in primary school has lasting effects, particularly on boys' confidence in their abilities, resulting in a larger number of math courses chosen at the end of secondary school. Hence, we expect that STEM confidence and STEM interest are positively related. In addition, studies have shown that girls are less confident in STEM-related subjects and their academic performance than boys (e.g., Dahlbom et al., 2011; Shi, 2018; Exley and Kessler, 2022), suggesting that the effect of the app is more pronounced for girls than for boys.

Hypothesis 3: Behavioral mechanisms for an increase in STEM interest

(a) Stereotypical thinking: Girls are more interested in STEM with a decrease in the strength of their stereotypical beliefs.

(b) Growth mindset: Children are more interested in STEM with an increase in the level of their growth mindsets.

(c) Competitiveness: Children that are more competitive exhibit more interest in STEM. Since girls are, on average, less competitive than boys, the effect of the app is more pronounced for girls than for boys.

(d) Confidence: Children are more interested in STEM with increasing STEM confidence. Since girls are, on average, less confident than boys, the effect of the app is more pronounced for girls than for boys.

The treatment app intends to address the potential behavioral mechanisms that foster interest in STEM, given the existing literature. We expect these *indirect* mechanisms to partly explain the treatment effect. The mechanism variables will not explain the entire treatment effect of the app since we also expect a *direct* effect from using the app (Hypothesis 1), i.e., there may be unobserved mediators.

Hypothesis 4:

The behavioral mechanism variables partly explain the effect of the treatment app.

3.3 Measures[8]

Outcome measures

To proxy interest in STEM for elementary school children, we have developed two outcome variables.[9] Our first measure uses the description of different jobs. We describe six different occupations, three of which are STEM jobs (programming, engineering/technical worker, mathematician), and three are non-STEM jobs (social/health worker, journalist/writer/translator, arts and humanities). The descriptions use simple language and provide children with examples of what people do in these jobs. We based our selection of STEM jobs on the classification by the European Commission (European Commission, 2015). We chose the "core STEM jobs" that excludes jobs with no, or less pronounced, occupational gender gaps. The three non-STEM jobs are occupations in which women are typically over-represented (e.g., European Institute for Gender Equality, 2017).

After each job description, children indicated how much interest they had in the job on a Likert scale from one to five. To obtain an index outcome variable for our analysis, we sum up the scores for the STEM jobs and divide this sum by the sum of scores of both STEM and non-STEM jobs. More formally, let l_i be the indicated Likert scale value and the integer i can take up numbers in the range of $[1, 6]$ for the six occupations; with $[1, 3]$ for the three STEM occupations and $[4, 6]$ for the three non-STEM occupations. The STEM interest index SII is defined as $SII = \sum_{i=1}^{3} l_i / \sum_{i=1}^{6} l_i$. In this way, we can account for differences in general interest between the children. Furthermore, a relative measure seems to be more relevant for the underlying decision to pursue a career in STEM since the decision to pursue a career is always relative to the options. We call the measure *SII (relative) STEM interest*.

Our second measure for STEM interest is a book choice. Specifically, we offered children different books to choose from at the end of the experiment. Two of the books were STEM-related ("The amazing world of technology," "The earth") and the other two were not STEM-related ("Dinosaurs," "To argue and to be friends"). Each child could choose one book as a present to be handed out at the end of the data collection. This measure is denoted *STEM book* and takes the value of 1 if a child chooses one of the STEM books and 0 otherwise.

[8] The complete instructions for children's decisions in the experiment as well as minor changes to the preregistration can be found in the Online Appendix. As a robustness check, we elicited further measures for explicit stereotypes, implicit stereotypes, and confidence. These measures are discussed in Section C in the Appendix.

[9] We refrained from using existing measures that have been employed for older children or in other countries in the educational literature to proxy science or STEM interest (e.g., DeWitt et al., 2013; Kier et al., 2014) for different reasons. First, they use terms such as "science" in their survey questions that are not understood by elementary school children in the Austrian context since there is no such school subject or related subjects at that age, yet. Second, our study uses measures that reflect the variety of STEM occupations and are not restricted to science jobs only. Third, children in elementary school have a very limited attention span and might struggle with long scales in a group data collection. Therefore, we refrained from measures with extended time requirements such as the scales mentioned above that contain more than 40 items.

Validation of our measure of STEM interest

To check whether relative STEM interest (SII) is a predictor of education choices, we validated this measure in a separate online survey with 345 high-school graduates in Austria. We collected data on (1) *STEM interest*, as elicited in the main study described above, and (2) self-reported education choices for tertiary education or training (either a study program or vocational training) at the individual level. We classify education choices (*STEM occupation*) using the classification system "ISCO-08" by the International Labor Organization.[10] In a next step, we use the ISCO-08 coding to classify occupational choices into STEM and non-STEM professions based on a study by the European Commission (2015). Following this classification, the STEM fields include life sciences, physics, mathematics and statistics, computing, engineering plus manufacturing and processing. *STEM occupation* has the value one when a respondent's occupational choice falls into one of the respective STEM categories and zero otherwise. Participants for our validation study were high-school graduates from different regions across the country. We recruited via the schools principals. The survey was conducted online using Qualtrics in spring 2021. More women (73%) than men (25%) or people that identify with neither gender (2%) responded.

In line with our findings in the sample of primary school children (see Section 4), we observe significant gender differences in relative STEM interest in the sample of high-school graduates. *STEM interest* (all (N=345): mean=0.423, sd=0.149; women (N=252): mean=0.385, sd=0.132; men (N=87): mean=0.535, sd=0.142) is significantly lower for women than for men (Mann-Whitney test, p<0.001).[11] Accordingly, fewer women than men want to specialize in STEM ($\chi^2(1)=13.003$, $p<0.001$), indicated by the variable *STEM occupation* (all (N=345): mean=0.290, sd=0.454; women (N=252): mean=0.240, sd= 0.428; men (N=87): mean=0.437, sd=0.499). Our measures of *STEM interest* and *STEM occupation* are highly significantly correlated with a correlation coefficient of 0.456 ($p<0.001$).[12] We conclude that our measure of *STEM interest* is a well-suited proxy for STEM interest and presumably a predictor of occupational choice.

Elicitation of behavioral mechanism variables

Stereotypical thinking: We cover conscious (explicit) and subconscious (implicit) stereotypical thinking. To measure explicit stereotypes, we use a set of six questions such as "Who is more talented in math?" Children can answer on a five-point scale from "Girls are more talented" (value of 0) over "Both alike" (0.5) to "Boys are more talented" (1). We use the average across

[10] https://www.ilo.org/public/english/bureau/stat/isco/isco08/index.htm

[11] A histogram of this variable by gender can be found in Figure A.2 in the Appendix.

[12] Box plots illustrating the association between the two measures can be found in Figure A.3 in the Appendix. Results from non-parametric tests (the Point-Biserial Correlation Coefficient is 0.456, with a confidence interval between 0.371 and 0.529) and a logistic regression (see Table A.1 in Appendix A) confirm the rather strong association between the two measures.

all answers as our measure of explicit stereotypes. To measure implicit gender stereotypes we use the Implicit Association Test (IAT) (Greenwald et al., 1998), developed in social psychology for adults. We modify the IAT following Cvencek et al. (2011) to measure implicit gender stereotypes in kids. In the test, children have to group words on either the right side to one category (e.g., "male") or the left side to another category (e.g., "female") of the tablet screen. The first set of words included German names of females and males, and the second set included math-related and German-related items (e.g., "plus and minus," "textbook"). One word appears at a time on the screen and needs to be grouped correctly into categories, i.e., either "male" or "female" in the first set and "German" or "Math" in the second set. In the third and fourth sets, the categories are combined. In the third set, the combined categories conform to potential gender stereotypes (German/female, Math/male). In the fourth set, the categories are counter-stereotypical (German/male, Math/female). Children are asked to answer as quickly as possible in all four sets. To prevent them from answering randomly, they receive error messages on the screen in the first two trial sets if they categorized incorrectly. To calculate the IAT, we add up the response times in set four and subtract the sum of the response times in set three.

Growth mindset: The measure of a child's growth mindset is based on Blackwell et al. (2007). Instructors read out a scenario in which a child writes an exam in a new subject in school and gets a bad grade. Four different statements provide potential reasons for the bad grade. Two of the reasons are related to a lack of innate intelligence (fixed mindset) and two to the missing dedicated effort (growth mindset). Children have to rate the provided explanations on a Likert scale from one "I do not agree at all" to five "I totally agree." We created an index of the four answers ranging from zero to one, with a higher index value indicating a more pronounced growth mindset.

Competitiveness: We measure preferences for competition with a modified version of the method by Niederle and Vesterlund (2011) similar to Sutter and Glätzle-Rützler (2015). We only implement two rounds due to time constraints. In the first round, children work on a math task under a piece-rate payment scheme for one minute. Each math task consists of four numbers that have to be added up. Children have to choose between three possible answers in a multiple-choice style. For each correct answer, they receive one point. Before we start the second round, children can decide whether they want to take part in a competition or whether they would prefer to be paid an individual piece rate for one point for each correct answer. When they choose the competition, they are anonymously matched with another child in the room. When children in the competition answer more questions correctly than the randomly-drawn competitor (they win), they receive two points per correct answer, and 0.5 points per correct answer otherwise (they lose).

STEM confidence: To measure STEM confidence, we use the same job descriptions as for

STEM interest, but here we ask children whether they would be confident working in these areas. Children can answer on a 5-point Likert scale from "No, not at all" to "Yes, very much." For the analysis, we use a relative measure of STEM confidence. More precisely, we sum up the answers for the three STEM jobs and divide it by the sum of all answers. This results in the index *Stem confidence*, ranging from zero to one. Specifically, a value of zero can be interpreted as very little relative STEM confidence and a value of one as very high relative STEM confidence.[13]

3.4 Sample, procedures, and evaluation strategy

Procedures and data collection

To test our hypotheses, we collected individual-level data using survey measures and incentivized economic decisions. We collect data before ("baseline measurement") and after ("end measurement") a four-week intervention period. The baseline measurement enables us to check for (unlikely) a priori differences in relevant variables between the control and treatment group and to test the effect of our behavioral mechanism variables on interest in STEM in general. We did not collect data on any variables related to professions or even STEM in the baseline measurements to avoid experimenter demand effects. The IAT could not be administered twice due to time constraints.

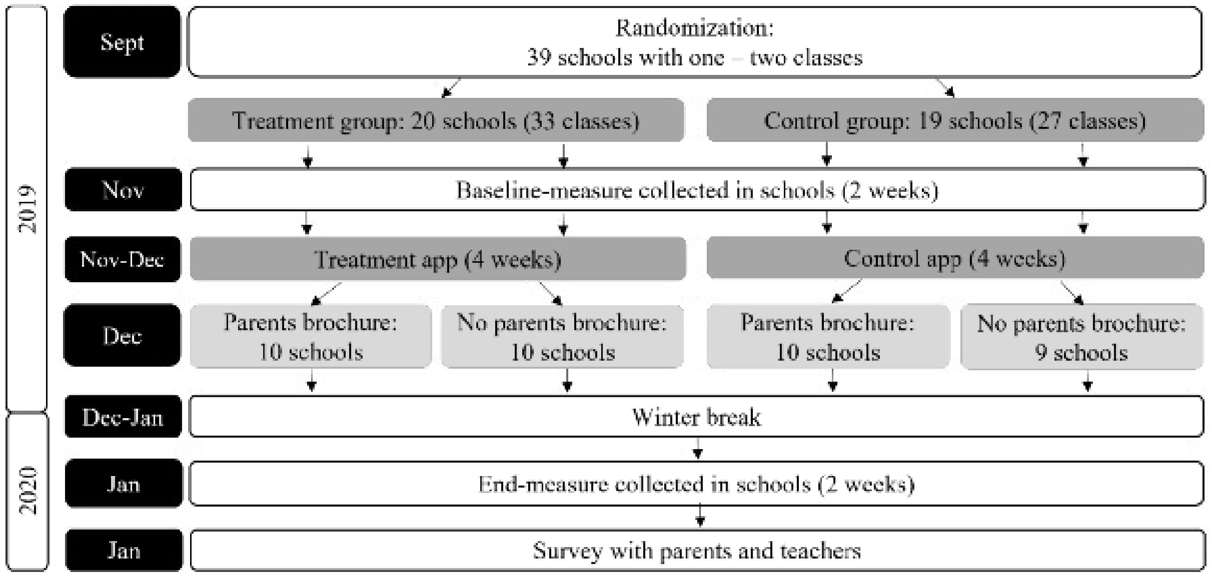
The baseline and end measurements were programmed with the software "oTree" (Chen et al., 2016). Children participated in the experiment with computer tablets. Sessions within class lasted approximately 60 minutes, and children were paid in the form of gifts with an average value of €1.50. We were given clearance by the ethics committee at the Institute for Advanced Studies.

In Figure 2, we visualize the time frame of the experiment and its phases. First, we invited school principals and teachers (the randomization process is described below). After making appointments with each class, we went for the baseline data collection in November 2019. For all 39 schools, initial data collection took two weeks. The intervention period started on Monday after the first data collection, i.e., we had one group of schools starting one week earlier ("first cohort") than the other group of schools ("second cohort"). The intervention period for the second group ended just before the Christmas break. On the last day of the intervention, teachers handed out the brochures for the parents. In January, we came back to the schools for the final data collection. This was about four weeks after the end of the intervention period. A cooling-off period of several weeks renders potential positive effects of the intervention stronger. Such positive effects would also be an indication of a lasting effect of the intervention.

For the baseline and end data collection, we sent two research assistants with a mobile lab to classes. We used dividers between the children to guarantee private decisions (see Appendix

[13]We focus on STEM confidence in the following, because we think that it is the more relevant variable in potentially determining STEM interest than math confidence that we also elicited.

Figure 2: Experimental schedule and treatment randomization



A, Figure A.1 for a picture of a session). One of the research assistants read the instructions for each task out loud. For some tasks, there were control questions on the tablet before the actual exercise to ensure that the children understood what they needed to do. In the baseline and end data collections, we use survey and lab-in-the-field experimental methods alike to elicit the mechanism variables. We informed the children at the very beginning of the session that they could earn points in several tasks/games. At the end, they would receive gifts depending on the relative number of points they collected on the experimental tasks within a class. The upper third within the class received bags with three gifts, the middle third two gifts, and the lower third, one gift. The bags contained small toys, stickers, and stationery such as pens. We showed a selection of gifts to the children in advance to make the scope of the incentives transparent. For our purpose, the coarse incentive scheme is sufficient and allows for easy implementation, given the restricted time of around one hour per class that was available for variable measurement.

The intervention, i.e., the app, was introduced to the children at the end of the baseline data collection in class. When we were present at a treatment school, we briefly described the treatment app "Robitopia," and, similarly, when at a control school the control app "Anton." To describe the apps, the same wording was used to make sure that children were equally excited to use the app in both conditions. After the introduction of the app, we handed out the gift bags (depending on the performance in the baseline data collection) and letters for the parents. In the letters, we explained the apps, how often they should be used, and how the children could log in using an individual code. The individual code was used to match the log-in data from the apps with the individual-level data from the baseline and end measurements in schools while preserving anonymity. To increase the take-up rate of the intervention as well as the usage time, we sent weekly reminders to the teachers. Moreover, we initiated a competition between classes by offering a price of €100 to the three classes with the most activity time on the apps.

Recruitment and randomization

We aimed at recruiting 40 elementary schools, assigned randomly to the treatment and the control app. We were interested in examining treatment effects in different geographical areas to be able to deduce more tailored policy implications if the intervention was to be scaled up. Therefore, we recruited schools in urban and rural areas. The two urban areas are Vienna, the capital of Austria, and Linz, a large city in Upper Austria. The rural area is called "Muehlviertel" and is a region north of Linz. To select schools, we used a list with all the public schools in Vienna, Linz, and the Muehlviertel, provided by the Federal Ministry of Education. The public education administration offices supported the recruitment by providing a recommendation letter in Vienna and by making phone calls. However, participation was voluntary for the schools, i.e., we could not and did not want to force schools to take part in our study.

We formed school pairs based on geographical proximity and randomly assigned one school in each pair to the treatment.[14] Since parents cannot chose elementary schools for their children but they are assigned to a school in their district/region by a school authority, we can assume that children within school pairs are more similar concerning their socioeconomic background than completely random school pairs.

Due to resource constraints, we could deal with a maximum of two classes within each school. When a recruited school had more than two classes in grade three, we chose the two classes with the lowest classroom numbers; that is, the number that identifies the room that a specific class in elementary school uses for all or the majority of lessons. In the following, we describe the recruitment for Vienna and Linz/Muehlviertel in more detail.

In Vienna, we focused the recruitment on the inner city districts (districts two to nine) for logistical reasons. Since there is a small possibility of systematic socioeconomic differences between children from these different districts, we stratified recruitment by district, i.e., the share of schools per district in our sample resembles the true share of schools in the district. More specifically, we randomly chose one or two schools in each district, depending on its size. In total, ten out of 61 schools were chosen. For these schools, we selected the nearest other elementary school. If either the selected school or the nearest other school did not want to participate, we went down the list and contacted the nearest neighboring school. We contacted a total of 35 schools of which 19 took part in our study.[15]

[14] In Vienna, one school had already used the control app. This school was taken out of the randomization process and directly assigned to the treatment group. As a consequence, the other school in the pair was assigned to the control group.

[15] Out of the 16 schools that eventually decided not to take part in the study, one school was excluded because classes had already used the control app. The majority of the remaining schools did not give us a reason for why they did not want to participate. Those schools that provided reasons for their decline, stated a lack of time, concerns about the capability of the scholars to participate because of language barriers, and bad experiences

In the city of Linz and the rural area Muehlviertel, both in the state of Upper Austria, we sorted the school list by postal code and selected ten schools randomly from a total of 131 schools. For the ten schools, we consecutively contacted the schools closest to the initial draw, similar to the recruitment in Vienna. If a school did not want to participate, we went down the list and contacted the next school on the list. We ended up contacting a total of 28 schools out of which 20 schools, with the sufficient class size, agreed to take part in the study.[16] Four schools in Upper Austria are situated in the city of Linz and 16 in the region Muehlviertel.

The parents' information brochure was handed out to half of the schools. More precisely, half of the control group and half of the treatment group were randomly assigned to the brochure condition.

3.5 Data

In total, 1,133 children participated in our RCT. We had to drop a few observations in our main analysis for the following reasons. Some children did not participate in both data collections, i.e., in the baseline and end measurements, mainly due to sickness absence. This reduces the sample by 155 observations. Moreover, we had to conduct a few sessions using pen and paper due to technical issues. This produced missing data for four children that did not answer all the survey questions. Seven children did not answer all socioeconomic questions, and three children had to leave sessions early. We excluded another two observations from children that stated that they had no interest in the six job descriptions at all. This left us with 962 observations for the main data analysis. The excluded children do not differ significantly from those included in the analysis in the variables that we specified in the pre-analysis plan (e.g., gender). Results are presented in Appendix B. We also replicate our main results (N=962) with the entire sample (N=1133) in the Online Appendix.

Table 1 presents a balance table, showing results from ordinary least squares regressions of the treatment dummy on the variables displayed in the first column, clustering standard errors by schools. The columns labeled "control" present the means of the control group. The columns labeled "difference" display the differences between the control group and the treatment group. Table 1 shows that the majority of the comparisons in observable variables between treatment and control are not significant at conventional levels. There is one exception: Girls in the treatment exhibit marginally significantly stronger explicit stereotypes than girls in the control group. In addition to the comparisons shown in Table 1, we also check for differences between the treatment and the control groups at the class level. Within classes, the share of girls is, on average, 50% and similar in the treatment and the control groups ($p = 0.852$, test of proportions).

with other studies (e.g., not getting informed about results after the study was completed).

[16]Out of the eight schools that did not participate, we excluded three schools that had fewer than 15 pupils in third grade (this was our ex-ante self-chosen cut-off number for classes to have enough statistical power in the analysis and to manage resources efficiently), three schools did not mention any reasons for not wanting to take part, and in one school the parents decided against participation.

Table 1: Internal validity balancing tests

	Full sample		Girls		Boys	
	(1)	(2)	(3)	(4)	(5)	(6)
Variable	control	difference	control	difference	control	difference
Age[a]	8.49	0.07	8.45	0.09	8.53	0.04
	(0.69)	(0.06)	(0.62)	(0.07)	(0.75)	(0.09)
Language[b]	0.44	0.04	0.43	0.09	0.45	0.00
	(0.50)	(0.09)	(0.50)	(0.10)	(0.50)	(0.09)
Socio-economic status[c]	2.18	0.09	2.17	0.06	2.20	0.12
	(1.30)	(0.18)	(1.20)	(0.18)	(1.39)	(0.23)
Parents with STEM job[d]	0.29	0.01	0.34	-0.02	0.25	0.04
	(0.46)	(0.04)	(0.47)	(0.06)	(0.43)	(0.06)
Explicit stereotypes [e]	0.54	0.00	0.53	0.01*	0.55	-0.01
	(0.09)	(0.01)	(0.09)	(0.01)	(0.09)	(0.01)
Growth mindset[f]	0.57	-0.00	0.59	-0.01	0.56	0.00
	(0.15)	(0.01)	(0.14)	(0.01)	(0.16)	(0.02)
Competitiveness[g]	0.58	-0.02	0.47	0.00	0.68	-0.05
	(0.49)	(0.04)	(0.50)	(0.06)	(0.47)	(0.05)
Math confidence[h]	6.25	-0.08	5.18	-0.13	7.32	-0.06
	(7.58)	(1.09)	(7.09)	(1.30)	(7.92)	(1.09)
Take-up rate[i]	0.59	0.07	0.62	0.05	0.56	0.08
	(0.49)	(0.05)	(0.49)	(0.07)	(0.50)	(0.06)
Usetime[j]	112.53	-6.49	126.38	-10.19	98.54	-2.51
	(162.61)	(16.10)	(169.50)	(20.11)	(154.50)	(17.48)
Observations	418	962	210	480	208	482
Class Level Variables						
Rural area[k]	0.37	-0.05				
	(0.49)	(0.16)				
Public School[l]	1.00	-0.07				
	(0.00)	(0.07)				
STEM focus[m]	0.27	0.00				
	(0.55)	(0.13)				
Prior app usage[n]	0.46	-0.01				
	(0.51)	(0.14)				
Observations	27	58				

Notes: Numbers in parentheses indicate standard errors clustered by schools.

Variable definitions (see questionnaires/instructions for additional details): [a] *Children's age* in years, [b] *Spoken language at home*, 0=parents speak only German at home, 1=parents speak at least one other language than German at home, [c] *Proxy for socioeconomic status*, children estimate how many books there are at their homes, 0=0-10 books, 1=11-25 books, 2=26-100 books, 3=101-200 books, 4=more than 200 books, [d] 1=if parents indicated that they work in a STEM profession; 0 otherwise. $N = 531$ as not all parents answered the survey. We use ISCO-08 coding to identify STEM and non-STEM professions based on a study by the European Commission (2015), [e] Aggregate measure from six different questions ranging from 0 to 1, where 0.5 means that the child does not exhibit stereotypical thinking, values below 0.5 indicate anti-stereotypical thinking, and values above 0.5 stereotypical thinking in line with existing stereotypes, [f] Aggregate measure by Blackwell et al. (2007) based on four survey questions ranging from 0 to 1; increasing values indicate a more pronounced growth mindset, [g] 1=preferences for a math competition, 0=preference for piece-rate in the math exercise rather than competition, [h] Difference between the number of correct answers and the child's guessed number of correct answers in a math task [i] 1=child logged into the app at least once, 0=child did not log into the app at all, [j] Total number of minutes the child used the app. [k] *Location of the school (urban or rural)*, 1= rural area, 0=urban area, [l] 1=school is a public school; 0 otherwise, [m] 1=teacher states that they actively promote STEM-related school activities; 0 otherwise, [n] 1=teacher states that she/he has previously used a learning app; 0 otherwhise.

Moreover, teachers reported the following specializations of their classes: 1) active learning and a focus on English, 2) multi-grade classroom, 3) multi-language classroom, 4) music, and 5) Montessori. The majority of the participating classes (74% in both treatment and control) did not have a specialization. The specializations are not distributed equally between treatment and control with specializations 1, 2, and 4 being over-represented in the treatment, and 3) and 5) being over-represented in the control group. We therefore control for class specializations in the regression analysis.

4 Empirical results

We first test for baseline gender differences in our behavioral mechanism variables (Hypothesis 3). Second, we run a manipulation check by testing whether the treatment alters the behavioral mechanism variables. Third, we present the main treatment effects, i.e., effects of the intervention on STEM interest. In all regressions, we either use no control variables or the full set of control variables that include children's age, spoken language at home, location of the school (urban or rural), class specialization, and a proxy for socio-economic status.[17] To better understand the behavioral determinants of interest in STEM we, fourth, examine how the mechanism variables are associated with interest in STEM and, fifth, conduct a mediation analysis.

4.1 Baseline gender differences

Previous literature predominantly shows that girls are less confident and less competitive than boys in stereotypical male tasks (e.g., Buser et al., 2014; Dahlbom et al., 2011; Dreber et al., 2014; Sutter and Glätzle-Rützler, 2015). In line with this research, we find that, while about 66% of boys are willing to enter a math competition, only 47% of girls are ($p < 0.001$, test of proportions). As a proxy for baseline confidence, we asked children to guess the number of tasks they had solved correctly in the math assignment. Whereas the actual performance of boys and girls is very similar, around seven correctly solved tasks, boys overestimated their performance by about seven and girls by about five tasks ($p < 0.001$; Wilcoxon-Mann-Whitney test). Hence, we confirm gender differences in confidence and competitive preferences (see Hypothesis 3c and Hypothesis 3d), and both girls' and boys' expectations of performance are far from being well-calibrated. Besides, we also observe gender differences in stereotypical thinking and growth mindset. Boys have significantly more pronounced stereotypical beliefs than girls (girls: 0.53, boys: 0.55, $p = 0.011$; Wilcoxon-Mann-Whitney test). Girls have a more pronounced growth mindset than boys in our sample (girls: 0.58 and boys 0.56, $p = 0.029$; Wilcoxon-Mann-Whitney test).

[17] In the paper, we present model specifications using control variables based on children or teacher surveys to simplify reading the main paper. Results remain similar with different sets of control variables in a step-wise approach (see the Online Appendix).

Girls may, in general, have a lower interest in STEM than boys already at the age of our participants. To test this, we focus on differences in the interest in STEM in our control group. We use data from the control group, as STEM interest is only measured after the intervention to reduce experimenter demand effects. We find that, indeed, girls choose a STEM book less often than boys (share of books chosen which are STEM books; girls: 0.56 and boys: 0.70, $p = 0.003$; test of proportions), and they also exhibit a substantially lower relative STEM interest (girls: 0.42 and boys: 0.61, $p < 0.001$; Wilcoxon-Mann-Whitney test).

As girls have a lower initial level of STEM interest than boys, there is more room for increasing STEM interest *directly* (Hypothesis 2). Concerning our behavioral mechanism variables, we find small absolute but significant differences in the level of the growth mindset and stereotypical beliefs between boys and girls, but substantial differences in confidence and the willingness to enter a competition. Therefore, there is more scope of improvement in confidence and competitiveness for girls than for boys, which might *indirectly* increase STEM interest (Hypotheses 3 and 4), in addition to the direct increase.

4.2 Manipulation check

In Table 2, we examine whether the treatment was successful in altering the behavioral mechanism variables, separately for boys and girls. We provide an analysis for the pooled sample in Appendix A (Table A.2). In the regressions, we control for the baseline levels of the behavioral mechanism variables where applicable in addition to the general controls described above.

In columns (1)-(2) and (6)-(7), we investigate the treatment effects on stereotypical thinking and differentiate between explicit stereotypical thinking and implicit stereotypical thinking (IAT). We find no significant effect on neither explicit nor implicit stereotypes when looking at boys and girls separately. However, there is a significant decrease in explicit stereotypes in the pooled sample (see Table A.2).

In columns (3) and (8), we test whether the treatment fosters children's growth mindset. We find that the treatment significantly increases the growth mindset of girls, but there is no effect for boys. Precisely, the measure for girls' growth mindset increases by 0.026 points through the treatment, which is equivalent to an increase of 0.17 standard deviations ($p = 0.021$, Wald test).

Columns (4) and (9) present linear probability models on the decision to enter the math competition (*Compet.*). We find that the chance of a girl entering competition is 7.4 percentage points higher in the treatment than in the control group ($p = 0.056$, Wald test). We do not find a significant effect on boys' willingness to enter the math competition, and the relative increase is significantly larger for girls than for boys (see bottom row in Table 2). The large treatment effect on girls is also reflected in a marginally significant increase in competitiveness in the pooled sample (see Table A.2). In the pooled sample, we find that children in the treatment group are 3 percentage points more likely to choose competition than children in the control group ($p = 0.094$, Wald test).

Table 2: Treatment effects on behavioral mechanism variables by gender

	Girls					Boys				
	(1) Explicit stereotypes	(2) IAT	(3) Growth Mindset	(4) Compet.	(5) STEM Confidence	(6) Explicit stereotypes	(7) IAT	(8) Growth Mindset	(9) Compet.	(10) STEM Confidence
Treatment	-0.009	0.292	0.026**	0.072*	0.047***	-0.015	-0.248	0.008	-0.015	-0.033*
	(0.006)	(0.817)	(0.011)	(0.036)	(0.014)	(0.010)	(0.747)	(0.015)	(0.032)	(0.017)
	[0.356]	[0.802]	[0.149]	[0.337]	[0.040]	[0.505]	[0.852]	[0.861]	[0.911]	[0.366]
Math performance				0.012					0.008	
				(0.007)					(0.006)	
Baseline levels										
Explicit stereotypes	0.035					0.094**				
	(0.046)					(0.046)				
Growth mindset			0.260***					0.294***		
			(0.039)					(0.048)		
Competitiveness				0.371***					0.418***	
				(0.048)					(0.039)	
Constant	0.405***	1.970	0.375***	-0.441*	0.463***	0.534***	5.223*	0.401***	-0.097	0.724***
	(0.049)	(2.067)	(0.091)	(0.248)	(0.078)	(0.047)	(3.070)	(0.095)	(0.303)	(0.093)
Observations	480	480	480	480	480	482	482	482	482	482
R-squared	0.056	0.023	0.102	0.225	0.040	0.033	0.011	0.121	0.177	0.048
Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Gender differences	0.009	0.525	0.022	0.091*	0.063***					
in treatment effects	(0.011)	(0.583)	(0.016)	(0.054)	(0.021)					

Notes: OLS regressions with behavioral mechanisms variables as the dependent variables. Columns (4) and (10) show linear probability models. Columns (1) to (5) only include girls. Columns (6) to (10) only include boys. Definitions of control variables are described in Table 1 and include children's age, spoken language at home, location of the school (urban or rural), class specialization, and a proxy for socio-economic status. Numbers in parentheses indicate standard errors clustered by schools. Numbers in brackets are Romano-Wolf p-values controlling for multiple hypotheses testing. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Finally, columns (5) and (10) investigate the treatment effects on children's relative STEM confidence. Girls' STEM confidence increases significantly in the treatment group (difference: 0.047 points or 0.28 standard deviations, $p = 0.002$, Wald test), and the effect for girls is significantly larger than the effect for boys. Interestingly, we find a marginally significant negative effect on boys' STEM confidence (difference: -0.033 points or 0.19 standard deviations, $p = 0.068$, Wald test). Note that STEM confidence is a relative measure of confidence in STEM fields to confidence in non-STEM areas (see Section 3). The negative effect may, therefore, originate from the treatment building up boys' confidence in social jobs rather than losing confidence in STEM. The data provides suggestive evidence in line with this argument but the effect is not significant. In the treatment group, boys' confidence in non-STEM subjects and social jobs increases by 0.13 points on a Likert scale from 1-5 ($p = 0.133$, Wilcoxon-Mann-Whitney test). In contrast, boys' confidence in STEM subjects was not significantly affected by the treatment (-0.08 points, $p = 0.423$, Wilcoxon-Mann-Whitney test).

To sum up, the intervention decreased explicit stereotypes and the growth mindset for the pooled sample and increased girls' willingness to enter a competition and their STEM confidence significantly. We conclude that the treatment was particularly successful in altering behavioral mechanism variables for girls.

4.3 Estimation of treatment effects

We use two different outcome variables to investigate the treatment effects on STEM interest, *STEM interest* and *STEM book*. *STEM interest* is defined as the interest in three STEM jobs relative to the sum of stated interest, i.e., the relative interest in the three STEM jobs *and* the three non-STEM jobs (see also the definition of SSI in Section 3). STEM interest is 0 if a child indicated no interest in all three STEM jobs and can be at a maximum of 1 if children indicated interest only in the STEM jobs but no interest in the non-STEM jobs. Consequently, a value of 0.5 indicates equal interest in STEM and non-STEM jobs. Overall, the children were slightly more interested in STEM jobs. The average score for STEM interest is 0.52, which is, given our large sample size, significantly larger than 0.5 ($p = 0.002$, sign-test). The variable *STEM book* is a dichotomous variable that takes the value 1 when a child chooses a STEM book and 0 for a non-STEM book. Overall, 62% of the children chose a STEM book, which is again significantly above 50% ($p < 0.001$, test of proportions).

In Table 3, we analyze the treatment effect on both measures for interest in STEM.[18] The

[18] In Table A.4 in the Appendix, we present regressions controlling for parents' socioeconomic status that is proxied by parents' level of education and their household income. These measures are based on parents' surveys instead of using the proxy based on the children's survey. Participation in the survey was voluntary, and 58% of the parents returned the survey. The size of the treatment coefficients is not much smaller than in the main regressions but they are estimated less precisely and are not statistically significant due to the smaller sample size. Our proxy for socio-economic status in the main analysis, measured by asking the children how many books they have at home, is significantly correlated with the parents' socio-economic status from the survey, i.e., level of education (ρ=0.36, p-value < 0.001, $N = 562$) and household income ($\rho = 0.28$, p-value < 0.001, $N = 509$).

Table 3: Direct treatment effects on interest in STEM

	STEM interest					
	(1) Pooled	(2) Pooled	(3) Girls	(4) Girls	(5) Boys	(6) Boys
Treatment	0.016	0.026*	0.040**	0.060***	-0.009	-0.009
	(0.014)	(0.014)	(0.016)	(0.017)	(0.020)	(0.021)
	[0.500]	[0.168]	[0.049]	[0.009]	[0.649]	[0.706]
Girls	-0.163***	-0.163***				
	(0.012)	(0.013)				
Constant	0.594***	0.644***	0.417***	0.423***	0.608***	0.739***
	(0.012)	(0.061)	(0.010)	(0.101)	(0.013)	(0.062)
Observations	962	962	480	480	482	482
R-squared	0.205	0.221	0.016	0.044	0.001	0.048
Controls	no	yes	no	yes	no	yes
Gender differences		(3)-(5)	0.049**		(4)-(6)	0.055**
in treatment effects			(0.033)			(0.022)
	Chooses a STEM book					
	(1) Pooled	(2) Pooled	(3) Girls	(4) Girls	(5) Boys	(6) Boys
Treatment	-0.029	0.001	0.020	0.031	-0.078	-0.045
	(0.043)	(0.044)	(0.054)	(0.061)	(0.059)	(0.056)
	[0.534]	[0.951]	[0.736]	[0.640]	[0.340]	[0.706]
Girls	-0.085**	-0.083**				
	(0.040)	(0.040)				
Constant	0.674***	0.804***	0.562***	0.746**	0.702***	0.802***
	(0.032)	(0.190)	(0.038)	(0.287)	(0.032)	(0.266)
Observations	962	962	480	480	482	482
R-squared	0.008	0.031	0.000	0.031	0.007	0.058
Controls	no	yes	no	yes	no	yes
Gender differences		(3)-(5)	0.097		(4)-(6)	0.104
in treatment effects			(0.073)			(0.073)

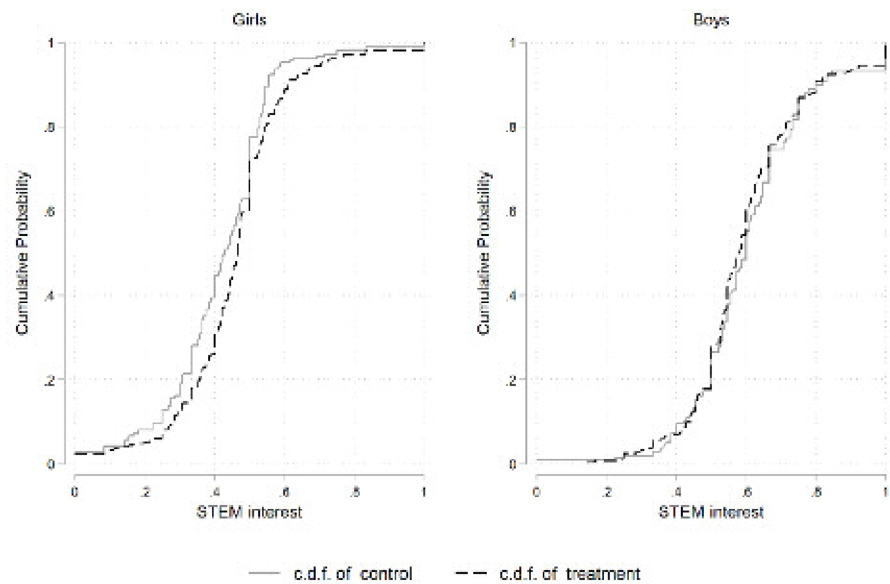
Notes: The upper panel shows OLS regressions with robust standard errors clustered at the school levels and STEM interest as the dependent variable. The lower panel shows a linear probability model with robust standard errors clustered at the school level and choice of a STEM book as the dependent variable. Definitions of control variables are described in Table 1 and include children's age, spoken language at home, location of the school (urban or rural), class specialization, and a proxy for socio-economic status. Numbers in parentheses indicate standard errors. Numbers in brackets are Romano-Wolf p-values controlling for multiple hypotheses testing. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

upper panel shows the treatment effect on *STEM interest*. We find that boys are, on average, more interested in STEM jobs than girls. The relative interest for STEM is approximately 0.16 points lower for girls than for boys (see variable *Girls* in columns (1) and (2)).

Girls' STEM interest increases significantly in the treatment, compared to the control group by +0.04 to +0.06 points, which is equivalent to 0.21 to 0.31 standard deviations (see variable *Treatment* in columns (3) and (4)). Moreover, the treatment effect is significantly larger for girls than for boys (Wald tests at the bottom of the upper panel), which is in line with Hypothesis

2.[19] In Figure 3, we illustrate this result by plotting the cumulative distribution functions of STEM interest for girls and boys. We do not find significant treatment effects in the pooled sample, except for a marginally significant effect when including all control variables, nor for boys separately (see variable *Treatment* in columns (1), (2), (5), and (6)).

Figure 3: Cumulative distribution functions of STEM interest in control and in treatment



The lower panel of Table 3 presents results from a linear probability model with the decision to choose a STEM book as the dependent variable.[20] We find that boys are generally more interested in STEM books than girls; they are 8 percentage points more likely to choose a STEM book (see variable *Girls* in columns (1) and (2)). We do not find significant treatment effects (see variable *Treatment*) for the book choice. We conclude that our treatment did not affect the children's book choice and, therefore, we focus on the variable *STEM interest* when analyzing the contribution of the behavioral mechanism variables on the treatment in greater depth in the subsequent analyses. To sum up, we reject Hypothesis 1 and cannot reject Hypothesis 2. A more detailed discussion on the results follows in Section 5.

Result 1:
Girls' are generally less interested in STEM professions than boys, and the treatment increases their STEM interest significantly. The treatment has no significant effect on children's STEM interest overall or for boys specifically.

[19]While we use relative STEM interest as the outcome variable in the main part of the paper, we also provide an analysis looking at average interest in STEM jobs without accounting for interest in non-STEM-related jobs in Appendix C. We find that the increase in relative STEM interest is driven by an increase in interest in STEM jobs, whereas interest in other jobs is not affected by the treatment.

[20]We present results from a logistic regression in Appendix A in Table A.3.

We consider intention-to-treat effects in the main body of the paper, as the *offer* of the treatment is policy-relevant rather than the treatment effect on the treated. We discuss estimates for the treatment effect on the treated for the main results in section E of the Appendix. As expected, accounting for compliance, i.e., the take-up rate of 65%, the treatment effect size increases by 35%. Moreover, we provide a heterogeneity analysis that controls for exposure levels, i.e., app usage time, in the Online Appendix. The treatment effect does not vary significantly with usage time. Details can be found in the Online Appendix.

4.4 Effect of behavioral mechanisms on STEM interest

To explore the effect of our behavioral mechanisms on interest in STEM, we conduct a correlation analysis between STEM interest and the mechanism variables presented in Table 4. We only consider behavioral mechanisms that we measured in the baseline data collection in this project, this is stereotypical thinking, growth mindset, and competitiveness.

As STEM fields are stereotypically male, in particular girls (but not boys) may have trouble expressing interest in STEM because of existing stereotypical beliefs and the resulting lack of identification (Kahn and Ginther, 2018; Miller et al., 2018). Consequently, we focus on girls in the regression that analyzes the effect of stereotypical thinking on STEM interest (column (1)), besides regressions on the pooled data in columns (2) and (3).

Table 4: Determinants of STEM interest (baseline mechanism variables)

	(1) Girls	(2) Pooled	(3) Pooled
Girls		-0.164***	-0.159***
		(0.013)	(0.013)
Explicit stereotypes	0.035		
	(0.073)		
Growth mindset		0.018	
		(0.038)	
Competitiveness			0.025**
			(0.011)
Constant	0.414***	0.642***	0.643***
	(0.108)	(0.064)	(0.056)
Observations	480	962	962
R-squared	0.019	0.218	0.222
Controls	yes	yes	yes

Notes: OLS regressions with robust standard errors clustered at the school levels. STEM interest is the dependent variable. Behavioral mechanism variables are measured at the baseline data collection. Definitions of control variables are described in Table 1 and include children's age, spoken language at home, location of the school, class specialization, and a proxy for socio-economic status. Numbers in parentheses indicate standard errors. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

We find that competitiveness is significantly positively correlated with STEM interest. The index for relative STEM interest is 0.03 points or 0.16 standard deviations higher for children who decide to enter a competition ($p = 0.029$, Wald test, Table 4). This finding corroborates results from recent studies that demonstrate the importance of competitive aptitudes for education choices toward STEM fields (Buser et al., 2017, 2021). In Appendix A, we disaggregate the underlying model specifications from Table 4 by gender (see Table A.5). We find that the general positive effect of competitiveness in the regression is driven by girls. For girls that choose to compete, STEM interest increases by 0.04 points ($p = 0.019$, Wald test), and there is no increase for boys.

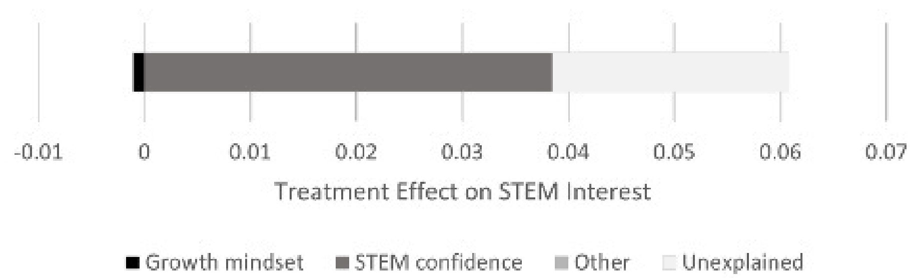
Other behavioral mechanism variables are not significantly correlated with STEM interest, i.e., we do not find support for Hypotheses 3a and b, i.e., we do not find evidence that stereotypical thinking and growth mindset are correlated with children's interest in STEM.

Result 2:
Children's competitiveness is positively associated with children's interest in STEM. We do not find evidence that stereotypical thinking and a growth mindset is associated with STEM interest.

4.5 Mediation analysis

To learn more about the influence of the behavioral mechanisms and their explanatory power for the treatment effect (Hypothesis 4) we conduct a mediation analysis. For this, we decompose the average treatment effect in a direct effect of the treatment on STEM interest and indirect effects through changes in the behavioral mechanism variables. We only focus on girls as this is the group that was affected by the treatment.

Figure 4: Total treatment effect on STEM interest (Girls)



Notes: Decomposition of the treatment effect for girls ($N = 480$). The treatment effect is estimated using OLS regressions with standard errors clustered by schools. Definitions of control variables are described in Table 1 and include children's age, spoken language at home, location of the school, class specialization and a proxy for socioeconomic status. The size of the bar corresponds to the unconditional treatment effect, and each section of the bar represents the share of the treatment effect that is explained by our behavioral mechanisms. All mechanisms are included separately but combined to "other" after the estimation, as competitiveness and stereotypes exhibit no mediating effect.

As a causal interpretation would require the assumption of sequential ignorability to hold, which cannot be tested, we abstain from claiming to be identifying causal effects. However, the decomposition of the treatment effect may still provide interesting insights on the relative importance of each of the mechanism variables (see for example Carneiro et al., 2020; Bandiera et al., 2020). We decompose the effect using the method by Gelbach (2016). This method treats each of the mechanisms as an omitted variable and uses the formula for omitted variable bias to decompose the total treatment effect into a share explained by the mechanisms (indirect) and an unexplained (direct) treatment effect. Thereby, it takes into account the fact that the mechanisms might be correlated with each other. Figure 4 shows the results of this decomposition analysis, focusing on girls. Similar to the results from the correlation, we find that the treatment effect is mainly driven by a change in STEM confidence. However, we do not find a significant effect of the willingness to enter a competitive environment, nor of stereotypical thinking. The growth mindset's coefficient on STEM interest is negative but not statistically significant. Around 30% of the treatment effect remains unexplained by our behavioral mechanism variables. This share may be accounted for by the direct effect (Hypothesis 1).

Result 3:
The increase in girls' STEM confidence can explain a large part of the treatment effect.

5 Discussion

Behavioral mechanism variables

In our study, we find that girls' confidence is positively associated with their STEM interest and is the major mediator of our treatment effect. This finding is in line with previous studies showing that women demonstrate lower performance, contribute less to discussions, are less confident in their performance, and enter competitive environments less frequently in stereotypical male tasks (Günther et al., 2010; Balafoutas and Sutter, 2012; Coffman, 2014; Bordalo et al., 2019; Exley and Kessler, 2022; Gneezy et al., 2003; Niederle and Vesterlund, 2010), which can reduce their persistence and interest in these tasks (Buser and Yuan, 2019). We therefore suggest to focus on promoting (non-economic) prerequisites such as STEM confidence that help women to develop STEM interest and to persevere and perform well in a competitive STEM environment (and other competitive environments). Girls' lower self-confidence compared to boys that we find in our study may be a double burden since girls may need even more self-confidence to maintain interest in STEM fields than boys.

Competitiveness is positively associated with STEM interest in our study. However, competitiveness does not mediate the treatment effect on girls' STEM interest. The treatment effect on competitiveness is seven percentage points and is not strong enough to show significant results as a mediator. However, we still infer that competitiveness is an important component when designing interventions to increase girl's interest in STEM and should be addressed more directly in future interventions.

Interestingly, we do not find a strong association for the other two mechanism variables of stereotypical thinking and children's growth mindset with interest in STEM. While previous literature focused on the influence of these mechanisms for career choices in older children and adults, there is little evidence on the effect of career aspirations at an early age, comparable to the age of the participants in our study (e.g., Gottfredson, 1981; Ambady et al., 2001). We might lack effects for the growth mindset because the belief that one's skills are malleable by working hard and the resulting increase in the willingness to seek challenging tasks might not be connected to STEM interest in young children, yet. Their perception of STEM fields and subjects as "hard and challenging" might be less pronounced compared to adolescents (Archer et al., 2020). Thus, believing that you can actually learn from challenging tasks might be less important for young children's interest in STEM.

Obviously, our study does not cover the universe of potential behavioral mechanisms. Furthermore, it is natural that some of the variables are correlated. Future experiments could vary different potential behavioral drivers exogenously within an intervention to pin down drivers based on the cleanest possible identification. Following up on our study's results, future work may want to focus particularly on competitiveness and STEM confidence in greater detail. For instance, varying potential drivers within the intervention such as giving feedback or rewards could give insights on which specific tools work to alter the prerequisites for STEM interest.

Addressing potential identification threats

For our study design, we took precautions to mitigate identification threats including a potential demand effect. First, we use a control group with a similar app and introduce it in the exact same way as the treatment app, e.g., the oral description in class to the children and the information letters to teachers and parents about the app are identical for the treatment and the control group. The pairwise randomization further contributes to a clean identification. Second, we made sure not to refer to STEM in any way before and during the intervention phase. The apps were introduced as a way to "facilitate learning in an environment without social prejudices." Neither the term "STEM" nor anything related to the study focus was mentioned in either the control or the treatment group until the end measurement. Third, we took several precautionary actions to mitigate the risk of letting participants know what the study was about until the end by, e.g., providing the research assistants, principals and other parties with information similar to that for the participants; telling them about the main variable of interest only at the very end of the study. Hence, any social desirability bias in responses is very unlikely.

Timing of the intervention and persistence of outcomes

Past studies in economics have mainly focused on interventions targeting adolescents shortly before their decision for tertiary education (e.g., Porter and Serra, 2020; Breda et al., 2020).

However, our study shows that there are underlying behavioral mechanisms that contribute to the gender gap in STEM and exist already very early in life. Such predispositions are likely to persist, if not even aggravated, and to shape later life outcomes (Francesconi and Heckman, 2016). Therefore, there should also be a focus on interventions before school track choices that lay the foundation for occupational choices later on. Our approach is a first example of such an early intervention.

Obviously, the sustainability of the intervention still has to be proved in long-term studies. The fact that we observed effects after a relatively long break between the treatment and the second measurement makes us confident that changes in STEM interest and in the behavioral mechanism variables are not only transitory but are likely to persist. Whether they are persistent over many years (and potentially, how they can be made persistent over many years), is a research question still to be addressed. Unfortunately, due to strict data protection policies in Austria, it is impossible for us to track children's education choices and link it with our experimental data. However, increasing girls' interest in STEM is clearly a first step toward increasing the share of girls selecting into STEM educations, since higher STEM interest is associated with actual choices for STEM tertiary education and interest in STEM jobs (shown by our auxiliary validation study and by Drescher et al., 2020, respectively).

Assessment of expected effects by scaling up the intervention

The results of this study caught the attention of policymakers in Austria. They see the potential to implement our intervention on a larger scale by including it in the school syllabus. However, research suggests that there can be "scalability problems" as soon as one rolls out an intervention that has been tested on a significantly smaller scale (Al-Ubaydli et al., 2017). First, interventions may not scale because the initial study has false-positive results. Since we pre-registered our study, executed a rigorous experimental design, and ran several robustness checks, this seems fairly unlikely. Second, the study sample can be different from the general targeted population. In our study, we drew a random sample from a rural and an urban region in Austria and randomly assigned the treatment and control groups within this sample. Although these regions do not represent the whole country, we believe that other regions may be comparable concerning culture, and children's educational and social environment. Third, the situation matters. In a study context, the principal investigators are in charge and invest time and effort to achieve high take-up rates of the intervention and comply with their investigation protocol. When interventions are rolled out, however, usually institutions are in charge of many pupils that may not have the resources to monitor the implementation as closely. In our study, however, there was fairly lose monitoring, too: Our intervention was an out-of-school intervention in which children and their parents could choose whether they wanted to use the app or not. The research team had only two major points of contact with the children - once at the beginning and once at the end of the study for about an hour each. When public authorities recommended the app or even incorporated it in the school curriculum, teachers as trusted people of parents and their children could potentially leverage an even stronger effect than in our study. Moreover, the nature of the intervention allows us to easily experiment with the content within the app and, this way, continuously improve on the outcomes for the target group. Taken together, we expect that our results will scale up.

6 Conclusion

Increasing girls' STEM interest already in elementary school may be essential for filling the science, technology, engineering, and math job pipeline. Informal and formal segregation of schools affect the propensity for a STEM degree choice later on in life. Studies suggest that an early focus on STEM in school is often a prerequisite for choosing to graduate in a STEM field later (e.g., Delaney and Devereux, 2019; Card and Payne, 2021). Particularly in Austria, formal school segregation starts as early as age ten. Therefore, particularly in the Austrian context, it is important to intervene early in education so as to engage younger children with STEM-related learning content.

In our study, we find that a web application can increase girls' interest in STEM significantly. Already the baseline level of interest in STEM is lower for girls than boys. This leaves more room for girls to increase interest in STEM under our treatment through a direct treatment effect. Moreover, girls exhibit lower initial levels for some of the ex ante identified potential behavioral mechanisms, such as STEM confidence and competitiveness, than boys. Besides the direct treatment effect in STEM interest of exposing girls to STEM content in a fun and interactive way, the web application in the treatment addresses behavioral mechanisms, such as confidence, which indirectly increase interest in STEM. In line with previous research, we find that competitiveness is positively associated with STEM interest and our treatment successfully increases competitiveness. We validated our measure for STEM interest in a survey with high-school graduates and demonstrate that higher STEM interest corresponds to a higher likelihood of specializing in STEM. Hence, our measure provides valid results.

Empirical evidence suggests that women's occupational sorting can be explained by academic skills and comparative advantages to a lesser extent than men's (Saltiel et al., 2019; Aucejo and James, 2021). Hence, other underlying factors than innate capabilities and acquired skills impede women's occupational choice for STEM. Our study provides evidence that prerequisites such as confidence are malleable and can contribute to a higher level of women's STEM interest. This may ultimately promote women's decision to choose a STEM career and, this way, decrease the gender STEM gap.

From a policy perspective, a digital intervention that increases interest in STEM may be attractive since it has the potential to be scaled up, i.e., it can be distributed, extended, and tested with relative ease. It can be more cost-effective and generally easier to integrate in school teaching, compared to more traditional interventions such as role models visiting classrooms or information events for parents. Therefore, we think that similar interventions are a promising path to decrease the gender STEM gap and ultimately foster gender equality in education and the labor market.

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Appendix

A Additional figures and tables

Figure A.1: Data collection in class

Figure A.2: Histogram of *STEM interest* in the validation study by gender (N=339)

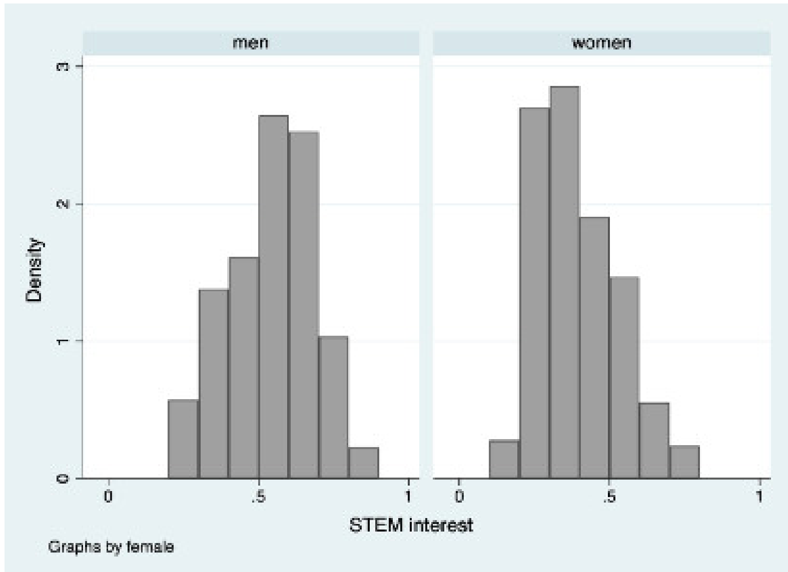

Figure A.3: Boxplot of *STEM interest* by *STEM occupation* in the validation study (N=345)

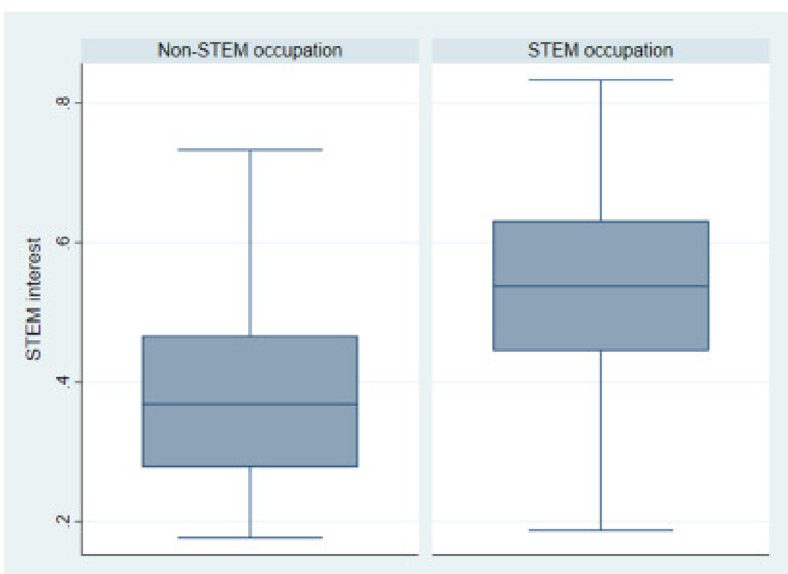

Table A.1: Logistic regressions to validate measure *STEM interest*

	Decision for STEM occupation					
	(1)	(2)	(3)	(4)	(5)	(6)
	Pooled	Pooled	Women	Women	Men	Men
STEM interest	7.486***	7.451***	7.612***	7.521***	7.774***	7.488***
	(0.994)	(1.092)	(1.280)	(1.292)	(2.009)	(2.015)
Age		-0.280*		-0.277		-0.273
		(0.140)		(0.179)		(0.219)
Women		0.0874				
		(0.322)				
Vocational training		0.132		0.194		-0.0911
		(0.398)		(0.478)		(0.705)
Constant	-4.295***	-3.482***	-4.298***	-3.409***	-4.489***	-3.490**
	(0.488)	(0.771)	(0.579)	(0.799)	(1.136)	(1.326)
Observations	339	339	258	258	87	87
adj. R-squared	0.179	0.189	0.152	0.160	0.163	0.179

Notes: Vocational training is a dummy variable and 1 if graduate wants to pursue vocational training and 0 if s/he wants to enroll in a study program. Columns 1 and 2 present results of a logistic regression for the pooled sample, columns 3 and 4 for women and 5 and 6 for men. Numbers in parentheses indicate standard errors. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table A.2: Treatment effects on mechanisms in the pooled sample

	(1) Explicit stereotypes	(2) IAT	(3) Growth mindset	(4) Compet.	(5) STEM Confidence
Treatment	-0.013**	-0.009	0.017*	0.031*	0.009
	(0.006)	(0.717)	(0.009)	(0.018)	(0.010)
Girls	-0.021***	-0.059	0.007	-0.096***	-0.147***
	(0.006)	(0.321)	(0.008)	(0.031)	(0.012)
Math performance				0.010**	
				(0.005)	
Baseline measurements					
Explicit stereotypes	0.071**				
	(0.033)				
Growth mindset			0.280***		
			(0.034)		
Competitiveness				0.398***	
				(0.033)	
Constant	0.483***	3.528*	0.384***	-0.224	0.655***
	(0.033)	(1.817)	(0.067)	(0.184)	(0.066)
Observations	962	962	962	962	962
R-squared	0.046	0.005	0.105	0.214	0.221
Controls	yes	yes	yes	yes	yes

Notes: Math ability represents the number of correctly solved tasks in the math task without competition. Control variables are described in Table 1 and include children's age, spoken language at home, location of the school (urban or rural), class specialization, and a proxy for socioeconomic status. Numbers in parentheses indicate standard errors clustered by school. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.3: Treatment effect on book choice (logit)

	Decision for a STEM book (in odds-ratios)					
	(1) Pooled	(2) Pooled	(3) Girls	(4) Girls	(5) Boys	(6) Boys
Treatment	0.883	1.002	1.083	1.137	0.705	0.814
	(0.162)	(0.193)	(0.240)	(0.291)	(0.183)	(0.209)
Girls	0.697**	0.696**				
	(0.118)	(0.121)				
Constant	2.063***	3.667	1.283	2.797	2.355***	3.831
	(0.294)	(3.027)	(0.198)	(3.320)	(0.360)	(4.733)
Observations	962	962	480	480	482	482
Pseud. R^2	0.006	0.024	<0.001	0.024	0.005	0.045
Controls	no	yes	no	yes	no	yes
Gender differences in treatment effects		(3)-(5)	1.536		(4)-(6)	1.590
			(0.478)			(0.503)

Notes: Odds ratios based on a logistic regression with the choice of a STEM book as the dependent variable. Control variables are described in Table 1 and include children's age, spoken language at home, location of the school (urban or rural), class specialization, and a proxy for socioeconomic status. Numbers in parentheses indicate standard errors clustered by school. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.4: Direct treatment effects on interest in STEM using parents reported SES

	STEM interest					
	(1) Pooled	(2) Pooled	(3) Girls	(4) Girls	(5) Boys	(6) Boys
Treatment	0.020	0.010	0.034	0.030	0.002	-0.014
	(0.016)	(0.017)	(0.021)	(0.023)	(0.023)	(0.026)
Girls	-0.179***	-0.167***				
	(0.015)	(0.016)				
Education	-0.008		-0.009		-0.007	
	(0.005)		(0.007)		(0.007)	
log HH inc.	-0.008		-0.009		-0.007	
	(0.005)		(0.007)		(0.007)	
Constant	0.671***	0.569***	0.522***	0.353*	0.640***	0.603***
	(0.096)	(0.122)	(0.121)	(0.175)	(0.151)	(0.151)
Observations	562	509	292	257	270	252
R-squared	0.261	0.237	0.036	0.035	0.057	0.045
Controls	yes	yes	yes	yes	yes	yes
Gender differences		(3)-(5)	0.007		(4)-(6)	0.015
in treatment effects			(0.029)			(0.030)
	Decision for a STEM book					
	(1) Pooled	(2) Pooled	(3) Girls	(4) Girls	(5) Boys	(6) Boys
Treatment	-0.068	-0.030	-0.039	0.014	-0.110*	-0.090
	(0.050)	(0.055)	(0.067)	(0.070)	(0.064)	(0.074)
Girls	-0.095**	-0.111**				
	(0.042)	(0.046)				
Education	0.032*		0.005		0.056**	
	(0.016)		(0.021)		(0.022)	
log HH Inc.		0.006		0.008		0.003
		(0.022)		(0.041)		(0.022)
Constant	0.798**	0.921**	0.767*	0.777	0.761*	1.063**
	(0.339)	(0.432)	(0.420)	(0.591)	(0.382)	(0.489)
Observations	562	509	292	257	270	252
R-squared	0.053	0.048	0.033	0.031	0.119	0.087
Controls	yes	yes	yes	yes	yes	yes
Gender differences		(3)-(5)	0.076		(4)-(6)	0.120
in treatment effects			(0.081)			(0.086)

Notes: The upper panel shows OLS regressions with robust standard errors clustered at the school level and STEM interest as the dependent variable. The lower panel shows a linear probability model with robust standard errors clustered at the school level and choice of a STEM book as the dependent variable. Control variables are described in Table 1 and include children's age, spoken language at home, class specialization, and location of the school (urban or rural). All children participating in both data collections whose parents also answered the question concerning level of education (columns (1), (3) and (5)) or concerning household income (columns (2), (4), (6)) are included in the sample. Numbers in parentheses indicate standard errors.* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.5: Determinants of STEM interest by gender (Baseline mechanisms)

	(1) Girls	(2) Girls	(3) Girls	(4) Boys	(5) Boys	(6) Boys
Explicit stereotypes	0.035			0.068		
	(0.073)			(0.084)		
Growth mindset		-0.007			0.035	
		(0.054)			(0.048)	
Competitiveness			0.040**			0.006
			(0.016)			(0.016)
Constant	0.414***	0.439***	0.449***	0.695***	0.712***	0.731***
	(0.108)	(0.102)	(0.094)	(0.079)	(0.067)	(0.060)
Observations	480	480	480	482	482	482
R-squared	0.019	0.018	0.033	0.049	0.049	0.048
Controls	yes	yes	yes	yes	yes	yes
Gender differences in treatment effects	(1)-(4)	-0.054 (0.115)	(2)-(5)	-0.064 (0.071)	(3)-(6)	0.036 (0.023)

Notes: OLS regressions with robust standard errors clustered at the school level. STEM interest is the dependent variable. Mechanism variables are measured at the baseline data collection. In columns (1)-(3) only girls are included in columns (4) - (6) only boys are included. Control variables are described in Table 1 and include children's age, spoken language at home, location of the school, class specialization, and a proxy for socioeconomic status. Numbers in parentheses indicate standard errors. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

B Attrition

As described in Section 3.5, we exclude 171 of 1133 observations (15%) from our main data analysis. To check if attrition may bias our results, we do the following. First, we investigate how attrition is distributed across schools and, second, we test if attrition differs between treatment groups, gender, and school location (urban or rural). In general, attrition varies between schools. This is not surprising because, e.g., diseases spread within schools and children stay at home for recovery. For a few schools, there are other explanations. In one school, it was not possible to conduct the first data collection due to technical problems. In another school, the different class-grade structure made it more demanding to have all children present for the data collection. There, the share of children who did not participate in both data collections is 42% higher than in the other schools (15%).

To test for biased attrition, we compare the ratio of girls, treated children, and children living in an urban area between the children included in the analysis (sample) and those who are not (dropouts). The share of treated children is 57% in our sample and 54% in the dropouts ($p = 0.599$, test of proportions), the share of girls in our sample is 50% and the share of girls in the dropouts is 49% ($p = 0.852$, test of proportions), and the share of children living in an urban area is 31% in our sample and 26% in the dropouts ($p = 0.264$, test of proportions). In Table B.1, we use a logistic regression to confirm non-parametric results and test for joint significance. We do not find that any of the variables nor all of them together predict who participates in both data collections ($p = 0.927$, Chi-square test). Hence, we do not find evidence that there is biased attrition in our study.

Table B.1: Determinants of inclusion in the sample

	Probability to be in the sample
Treatment	1.110
	(0.438)
Girls	1.027
	(0.188)
Rural	1.243
	(0.548)
Constant	4.925***
	(1.355)
Observations	1,133
Pseud. R^2	0.002

Notes: Logistic regression with robust standard errors clustered at the school level and inclusion in the sample as the dependent variable. We present odd ratios in this table.

C Robustness checks

Decomposition of relative STEM interest measurement

Table C.1: Treatment effects on average interest in STEM and non-STEM jobs

	Average interest in STEM jobs					
	(1) Pooled	(2) Pooled	(3) Girls	(4) Girls	(5) Boys	(6) Boys
Treatment	0.129	0.188**	0.300**	0.377***	-0.043	-0.011
	(0.102)	(0.090)	(0.140)	(0.118)	(0.097)	(0.102)
Girls	-0.551***	-0.545***				
	(0.075)	(0.072)				
Constant	2.635***	2.603***	1.987***	2.288***	2.732***	2.508***
	(0.072)	(0.312)	(0.084)	(0.571)	(0.069)	(0.587)
Observations	962	962	480	480	482	482
R-squared	0.080	0.121	0.021	0.101	0.001	0.030
Controls	no	yes	no	yes	no	yes
Gender differences		(3)-(5)	0.34**		(4)-(6)	0.33**
in treatment effects			(0.130)			(0.122)
	Average interest in non-STEM jobs					
	(1) Pooled	(2) Pooled	(3) Girls	(4) Girls	(5) Boys	(6) Boys
Treatment	0.053	0.065	0.037	0.020	0.068	0.132
	(0.100)	(0.089)	(0.087)	(0.092)	(0.147)	(0.139)
Girls	0.695***	0.693***				
	(0.069)	(0.066)				
Constant	1.913***	1.797***	2.616***	3.098***	1.904***	1.105**
	(0.083)	(0.360)	(0.041)	(0.518)	(0.102)	(0.508)
Observations	962	962	480	480	482	482
R-squared	0.117	0.162	0.000	0.040	0.001	0.082
Controls	no	yes	no	yes	no	yes
Gender differences		(3)-(5)	-0.03		(4)-(6)	-0.08
in treatment effects			(0.137)			(0.133)

Notes: Definitions of control variables are described in Table 1 and include children's age, spoken language at home, location of the school (urban or rural), class specialization, and a proxy for socioeconomic status. Numbers in parentheses indicate standard errors. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

In the main paper, we use a composite measurement for STEM interest relative to interest in non-STEM jobs. While this relative measurement is relevant for career decisions (see validation study), it is still interesting to learn more about whether the treatment app increases interest in STEM jobs, as intended, or rather decreases interest in non-STEM jobs. In Table C.1, we replicate the upper panel of Table 3 using children's average interest in STEM jobs in the upper panel and children's average interest in non-STEM jobs in the lower panel. Both variables range from 0-3. We find that an increase in interest in STEM jobs drives the treatment effect reported in the main part of the paper. The average interest in STEM jobs increases significantly in the pooled sample when we include controls (column (2)) and for girls (columns (3-4)). We find no evidence for a treatment effect on boys. Moreover, we find no evidence that the treatment affects the interest in non-STEM jobs (see lower panel).

Explicit stereotypes - Who can

In addition to the variables presented in the main text, we have elicited a second measurement for explicit stereotypes, implicit stereotypes, and confidence. We will discuss the robustness of our results with respect to using these alternative measurements in this section. Explicit stereotypes are measured by asking children who, men or women, are better at doing certain jobs. We call the variable *Who can* and it ranges from 0 anti-stereotypical views to 1 very stereotypical views. Table C.2 shows the manipulation check for *Who can*. We find that the treatment reduces stereotypical thinking for the pooled sample (column (1)) as well as for girls looking at the gender-disaggregated data (columns (2) and (3)).

Table C.2: Explicit Stereotypes - Who can

	(1) Pooled	(2) Girls	(3) Boys
Treatment	-0.017*	-0.018**	-0.014
	(0.009)	(0.008)	(0.015)
Girls	-0.042***		
	(0.007)		
Constant	0.594***	0.590***	0.580***
	(0.044)	(0.061)	(0.062)
Observations	962	480	482
R-squared	0.103	0.059	0.083
Controls	yes	yes	yes

Notes: OLS regressions with robust standard errors clustered at the school level. *Who can* is the dependent variable. Column (1) shows the aggregated and columns (2) and (3) show results for girls and boys separately. Control variables are described in Table 1 and include children's age, spoken language at home, location of the school, class specialization, and a proxy for socioeconomic status. Numbers in parentheses indicate standard errors. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Implicit stereotypes

As a second measurement for implicit stereotypes, we have asked children who ranked in the top three in the math task. We compare their ranking of boys and girls with the actual ranking in class. The children earn points if their guess was correct. The variable *Implicit stereotypes* is an integer variable and ranges from -3 to +3 and is -3 when a child ranked only girls in the top three but only boys were among the top three performers. In contrast, when they ranked only boys in the top three, but the top three performers were only girls, the variable takes the value +3. The authors have developed this elicitation method and it relies on the assumption that children have some (biased) idea about who are the best performing children in the math task. Using a Poisson regression to identify determinants of the children's guessed rankings in the baseline measurement, we find that the number of boys or girls that are among the top 15 percentile of their class (with, on average, 20 children per class, this should represent the top three) does not affect the number of boys that children rank in the top three. To put it differently, actual performance is not informative for the children's guessed ranking.

Similar to the results for explicit stereotypical thinking, we find that implicit stereotypes are more pronounced for boys than for girls in the baseline measurement (average girls: -0.68 and average boys:

0.10, $p < 0.001$, Wilcoxon-Mann-Whitney test). Table C.3 presents results of a manipulation check for implicit stereotypes. We find that the intervention did not reduce children's implicit stereotypes. In contrast, implicit stereotypes increase in the treatment compared to the control group. The increase in the pooled sample is driven by an increase for boys (see column (3)). A closer look shows that this effect is driven by a decrease in stereotypical thinking in the control rather than an increase in the treatment group. While the average ranked number of boys in the top three does not change for children in the treatment group (baseline: 1.93 vs. end: 1.92, $p = 0.813$, signtest), it decreases in the control group (baseline: 1.90 vs. end: 1.79, $p = 0.010$, signtest).

We test if the change might be explained by improvement in the math task. While children on average improve their performance between the two data collections (+0.43 tasks, $p < 0.001$, paired t-test), there is no gender difference in the improvement, neither in the treatment (average gender difference in improvement: +0.03 tasks, $p = 0.928$, two-sided t-test), nor in the control group (average gender difference in improvement: +0.11 tasks, $p = 0.670$, two-sided t-test).

Table C.3: Implicit stereotypes

	(1) Pooled	(2) Girls	(3) Boys
Treatment	0.462**	0.383	0.572***
	(0.204)	(0.237)	(0.199)
Girls	-0.532***		
	(0.115)		
Baseline	0.189**	0.234**	0.118
	(0.086)	(0.091)	(0.090)
Constant	-0.379	-1.081	-0.371
	(0.449)	(0.711)	(0.457)
Observations	924	454	470
R-squared	0.272	0.235	0.204
Controls	yes	yes	yes

Notes: OLS regressions with robust standard errors clustered at the school level. Implicit stereotypes is the dependent variable. We do not observe baseline implicit stereotypes for three classes due to technical problems during the baseline data collection. This reduces the sample to 924 observations. Column (1) shows the results for aggregated data, and columns (2) and (3) show results for girls and boys separately. Control variables are described in Table 1 and include children's age, spoken language at home, location of the school, class specialization, and a proxy for socioeconomic status. Numbers in parentheses indicate standard errors. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

There are several potential other explanations for this finding. First, the control intervention may decrease children's implicit stereotypical thinking. Second, our elicitation method was not successful to measure implicit stereotypes. We find no positive correlation between implicit stereotypes and explicit stereotypes or children's interest in STEM in the control group. Hence, explanation 1 seems unlikely. The second explanation is to some extent supported by our data as implicit stereotypes are not correlated with the results from the IAT, a validated measurement of implicit stereotypes. However, our analysis cannot provide a final explanation for this finding.

Math confidence

In addition to the confidence in STEM ability, we also consider differences in confidence in the math task. We find that girls are, on average, less confident than boys in the baseline measurement. Boys overestimate their performance on average by seven tasks, and girls overestimate their math performance on average by five tasks ($p < 0.001$, Wilcoxon-Mann-Whitney test). Table C.4 shows a manipulation check for children's confidence in math. Columns (1) - (3) use a continuous measure, i.e., the difference between the guessed and the actual number of correctly solved tasks, and columns (4) - (6) show the share overstating their ability as registered in the pre-analysis plan. We do not find a significant treatment effect.

Table C.4: Math confidence

	Continuous variable			Share overestimating		
	(1) Pooled	(2) Girls	(3) Boys	(4) Pooled	(5) Girls	(6) Boys
Treatment	-0.497	-0.834	-0.213	-0.020	-0.021	-0.020
	(0.704)	(0.801)	(0.838)	(0.043)	(0.049)	(0.052)
Girls	-0.183			0.015		
	(0.423)			(0.025)		
Baseline	0.274***	0.346***	0.209***	0.211***	0.210***	0.211***
	(0.042)	(0.052)	(0.046)	(0.039)	(0.050)	(0.056)
Constant	-4.711	-1.842	-7.785**	0.356**	0.785***	0.017
	(3.225)	(4.740)	(3.613)	(0.169)	(0.248)	(0.214)
Observations	962	480	482	962	480	482
R-squared	0.144	0.197	0.125	0.065	0.082	0.069
Controls	yes	yes	yes	yes	yes	yes

Notes: OLS regressions with robust standard errors clustered at the school level. Math confidence is the dependent variable in columns (1) -(3), where we use the difference between guessed and actual numbers of correct answers in the math task, and in columns (4) - (6), the dependent variable is a binary variable with value one if the child overestimated his or her performance and zero otherwise. Columns (1) and (4) shows the aggregated results, while we show results for girls and boys separately in the other columns. Control variables are described in Table 1 and include children's age, spoken language at home, location of the school, and a proxy for socio-economic status. Numbers in parentheses indicate standard errors. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

D The effects of the brochure

In addition to the app, we distributed a brochure to the parents that explained why STEM skills are important for children and how they can support their children in acquiring them. The authors of the study were skeptical from the start regarding whether information brochures provide effective treatments since parents are often hard to reach, and it seemed challenging that parents will receive, read, and understand the (German) brochure. However, public policy partners insisted on an intervention that addressed not only the children but also their parents. While we think that more specific and expensive treatments of parents could have effects, distributing brochures seemed unlikely to work well. We distributed the brochures to 50% of the participating schools. The brochure was distributed after the main intervention ended. To investigate the effect of the brochure on parents' and children's interest in STEM, we offer parents to participate in a lottery to win a voucher for summer camp in the end questionnaire. The summer camp offers four different themes: being creative, sports activities, the city of Vienna, and STEM. We only consider children from Vienna as the summer camp was only available in Vienna. We measure the effect of the brochure by comparing the workshop choices between parents that did and did not receive the brochure. In total, 135 parents participated in the lottery. We find that parents of sons are more likely to sign up for the STEM workshop than parents of daughters. For boys, 46% of the parents chose the STEM theme; for girls, only 27% chose the STEM theme ($p = 0.019$, Fisher's exact test).

Concerning treatment effects, we find that 32% of parents in the control and 39% of parents in the treatment group sign their children up for a STEM workshop. This difference is not statistically significant based on a test of proportions ($p = 0.446$). The result does not change when including control variables in logistic regressions shown in Table D.1. We include controls for children's age, spoken language at home, and a proxy for socioeconomic status.[21] We do not find evidence that the brochure significantly affected the likelihood that parents signed up for the STEM theme (column (1)). The same holds when we consider boys and girls separately (columns (2) and (3)). However, our sample is reduced significantly, given the relatively low number of participants, not giving us the statistical power to detect an effect size of 0.33 standard deviations that we observe in our sample. We conclude that we do not find evidence that the brochure had a large effect (i.e., an effect of more than 0.60 standard deviations, which we would be powered to measure) on the share of children who were signed up for STEM-themed summer camp.

Table D.1: Decision for STEM workshop (odds ratios)

	(1) pooled	(2) Girls	(3) Boys
Brochures	1.213	1.234	0.924
	(0.511)	(0.686)	(0.429)
Girls	0.423**		
	(0.142)		
Constant	2.368	0.269	6.160
	(8.240)	(0.955)	(32.429)
Observations	135	75	60
Pseud. R^2	0.044	0.027	0.024
Controls	Yes	Yes	Yes

Notes: Logistic regressions with robust standard errors clustered at the school level. The decision for a STEM-related workshop is the dependent variable and the estimates are reported as odds-ratios. Column (1) shows the aggregated and columns (2) and (3) show results for girls and boys separately. Control variables are described in Table 1 and include children's age, spoken language at home, and a proxy for socioeconomic status. As only children in Vienna were able to join the workshop, we do not control for school location. Numbers in parentheses indicate standard errors. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

E Treatment effect on the treated

In line with the previous literature, we estimate the local average treatment effect (LATE) on the treated. While children from the control group could not receive the treatment by design, there are children in treated classes who did not use the app. To estimate the local treatment effect, we use treatment assignment as an instrument for app usage. Table E.1 shows the estimates for the LATE, i.e., the second stage results of the regressions. As the take-up rate is 65% in the treatment group (see Table 1), the LATE estimator is approximately 35% larger than the intent-to-treat estimator reported in the main part of the paper.

[21]We do not control for class specialization in this regression as the number of parents who signed up for a workshop is very low for some classes leading to perfect separability. As the workshop was only offered in Vienna, we do not control for the school's location.

The treatment was implemented on class level. Therefore, we can also use a class-level definition of “treated” individuals. We define a class as treated if the children in this class used the app on average on more days than the app has been used on average across all treatment classes (see also our pre-analysis plan). Using this definition of treated classes, we run the LATE analysis again as described above in Table E.2. We find that 17% of the children in the treatment group are classified as treated under this definition. Consequently, the LATE is around 83% larger than the intent-to-treat estimator reported in the main part of the paper.

Table E.1: LATE on STEM interest (individual level)

	(1) Pooled	(2) Pooled	(3) Girls	(4) Girls	(5) Boys	(6) Boys
LATE	0.025	0.045**	0.061**	0.087***	-0.014	-0.015
	(0.023)	(0.021)	(0.025)	(0.026)	(0.030)	(0.033)
Constant	0.512***	0.524***	0.417***	0.442***	0.608***	0.741***
	(0.009)	(0.064)	(0.010)	(0.092)	(0.013)	(0.062)
Observations	962	962	480	480	482	482
Controls	no	yes	no	yes	no	yes

Table E.2: LATE on STEM interest (class level)

	(1) Pooled	(2) Pooled	(3) Girls	(4) Girls	(5) Boys	(6) Boys
LATE	0.115	0.148	0.241*	0.257**	-0.075	-0.057
	(0.113)	(0.096)	(0.144)	(0.125)	(0.166)	(0.126)
Constant	0.512***	0.532***	0.417***	0.457***	0.608***	0.738***
	(0.009)	(0.058)	(0.010)	(0.090)	(0.013)	(0.060)
Observations	962	962	480	480	482	482
Controls	no	yes	no	yes	no	yes

Below we estimate the LATE for the book choice. In Table E.3, we use treatment assignment as an instrument for app usage at the individual level. In Table E.4, we focus on treated classes as described above. While the estimates increase in size comparably to the results for STEM interest, we do not find a significant local average treatment effect under these model specifications.

Table E.3: LATE on STEM book (individual level)

	(1) Pooled	(2) Pooled	(3) Girls	(4) Girls	(5) Boys	(6) Boys
LATE	-0.044	0.003	0.030	0.045	-0.122	-0.072
	(0.066)	(0.067)	(0.081)	(0.087)	(0.093)	(0.090)
Constant	0.632***	0.742***	0.562***	0.756***	0.702***	0.812***
	(0.027)	(0.180)	(0.038)	(0.280)	(0.032)	(0.256)
Observations	962	962	480	480	482	482
Controls	no	yes	no	yes	no	yes

Table E.4: LATE on STEM book (class level)

	(1) Pooled	(2) Pooled	(3) Girls	(4) Girls	(5) Boys	(6) Boys
LATE	-0.200	0.011	0.117	0.134	-0.646	-0.276
	(0.326)	(0.223)	(0.323)	(0.261)	(0.614)	(0.379)
Constant	0.632***	0.743***	0.562***	0.764***	0.702***	0.799***
	(0.027)	(0.180)	(0.038)	(0.280)	(0.032)	(0.267)
Observations	962	962	480	480	482	482
Controls	no	yes	no	yes	no	yes