Social Information and Educational Investment - Nudging Remedial Math Course Participation

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13 May 2022

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May 13, 2022

Abstract

Using randomized field experiments, I investigate the effectiveness of two social information interventions at increasing participation in a voluntary remedial math course for university students. In Intervention 1, incoming students receive invitation letters with information about the course sign-up rate in a previous semester. In Intervention 2, the students who signed up for the course receive reminder letters that include information on how helpful the course has been evaluated by previous students. On average, neither intervention increases participation in the course, but further analyses reveal that the effects of Intervention 1 are heterogeneous along two dimensions: First, by increasing the salience of the course, it raises attendance among students who enroll late in their study program, which in turn increases their first-year performance and closes the achievement gap to early enrollees. Second, the effect of the information about the past sign-up rate depends on the predicted ex-ante sign-up probability. Students for whom the prediction falls just short of the past sign-up rate increase sign-up and participation, while the opposite is true for students whose sign-up probability exceeds the social information. Along this dimension, however, the changes in attendance do not carry over to academic achievements.

Keywords: Social Information; Higher Education; Randomized Field Experiment; Remedial Courses

JEL Classification: D83, I21, I23, C93

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I would like to thank Oliver Himmler, Robert Schwager, Robert Jäckle, Cansın Arslan, Viviana Urueña, referees, and seminar participants at the University of Göttingen and the VfS Annual Conference for valuable comments and discussions. This research was approved by the University of Göttingen ethics committee and was pre-registered under `https://osf.io/47syt`.

1 Introduction

Decisions about educational investment are often characterized by uncertainty about the associated returns, which can lead to non-optimal decision-making (Altonji, 1993; Altonji, Blom and Meghir, 2012). One way to address this is to provide individuals with the relevant information, which has, for example, been shown to change the amount of time individuals stay in school (Jensen 2010), which college major they choose (Wiswall and Zafar 2015*b*,*a*), or their educational aspirations (Bleemer and Zafar 2018; Lergetporer, Werner and Woessmann 2021). But in many cases, policy makers themselves do not have access to the information that is required to inform individuals. For instance, because the educational investments have not yet been evaluated or because the returns consist of non-pecuniary benefits that are difficult to measure.[1]

In such cases, providing information about the behavior of others could be a promising alternative: First, it may provide a signal about returns, if individuals believe that the decision of others is linked to the expected utility of the investment (Coffman, Featherstone and Kessler, 2017). Second, and more general, the behavior of others may be perceived as a descriptive norm, i.e., individuals might want to invest in education because they expect others to do the same (Bicchieri and Dimant, 2019).

This paper studies if social information can indeed be used to influence an educational investment decision, and whether individuals subsequently benefit from their investment. The context is a voluntary remedial math course for economics and business students at a large German university that takes place at the beginning of the first semester; a setting characterized by the features described above. A considerable number of students does not participate in the course, even among those who initially signed up for it. Conditional on a large set of observable characteristics, these students perform worse in their first year of studies, suggesting that their decision is not optimal. This could be rooted in the fact that the course is an investment with uncertain returns, since students are usually not aware of the exact content of their study program in advance and whether it is really necessary to attend the course, given their prior knowledge. Because the course has not been causally evaluated yet, students could not be informed of its returns directly.

Against this background, I conducted field experiments with two consecutive cohorts of incoming students, to evaluate the effectiveness of two different social information interventions at increasing sign-up for and participation in the remedial math course: i) In Intervention 1 (N = 789), shortly after enrolling in their study program, students in the social information treatment receive a postal invitation letter that includes the information that 85% of students signed up for the remedial math course in the previous semester (throughout the paper, the term "enrollment" refers to enrolling in a study program or at the university and the term "sign-up" refers to signing-up for the remedial math course). In both cohorts, I compare the sign-up and participation behavior of this treatment group

[1] See e.g., Oreopoulos and Salvanes (2011) and Hout (2012) for reviews on the non-pecuniary benefits of education. Delavande and Zafar (2019) provide evidence that non-pecuniary benefits can play an important role in educational investment decisions.

to a control group that receives no invitation letter at all. To explore whether the information on past sign-up rates or an increase in the salience of the course is driving potential effects of the social information treatment, in the second cohort, I add a second treatment group that receives an invitation letter without the social information. ii) To increase attendance among students who initially sign up for the course I run Intervention 2 (N = 574), in which students in the social information group receive a postal reminder letter with the information that the course made it easier to get started with university mathematics for 95% of the students in the past. I compare the outcomes of this group to a control group that receives no reminder letter at all, and, in the second cohort, I again add another treatment group that receives a reminder letter without the social information to explore potential salience effects.

My main findings are as follows: First, I find that, on average, neither of the two interventions affects students' decision to sign up for or participate in the remedial math course. Second, further analyses provide evidence that the effects of Intervention 1 are heterogeneous along two dimensions: i) Both the social information and the salience treatment are more effective for students who enroll late in their degree program, i.e., students who enroll in their program within the last month before the beginning of the remedial math course. The treatments offset more than half of the roughly 9 and 16 percentage point (pp) lower sign-up and participation rates that I observe for these students in the control group relative to students who enroll before the last month. The fact that both treatments have similar effects suggests that the lower attendance among late enrollees in the control group is at least partly driven by a lack of (relative) salience of the course. ii) To explore effect heterogeneity along the outcome dimension, I use background characteristics – e.g., degree program, sex, and age as well as grade, type, date, and place of the high school degree – and the control group to predict all students' ex-ante sign-up probability; i.e., I perform endogenous stratification (Abadie, Chingos and West, 2018). I find that the information about the past sign-up rate – but not the salience treatment – leads to a decrease in sign-up and participation by about 10 pp among those with the highest predicted ex-ante probability, while the opposite is true for students whose probability falls just short of the past sign-up rate; students with the lowest predicted sign-up probabilities show no behavioral response. This pattern is broadly consistent with the idea that treated students update their beliefs about the behavior of others, which in turn affects beliefs about the descriptive norm or the expected utility of the course.

Third, I investigate whether these heterogeneous effects on remedial math course participation carry over to academic achievements in the first year of studies. My findings suggest that this only holds true with respect to students' timing of enrollment. For late enrolling students, whose overall performance in the absence of treatment is about 0.26 standard deviations worse compared to early enrollees, the increase in remedial math course participation is able to almost completely close the gap in academic achievement. The remarkably large effects on first-year performance appear to be driven by large increases in average course participation among a small group of individuals. My findings are in line with the notion that early engagement with their studies in form of the remedial math course increases academic and social integration among these students and prevents them from dropping out early.

The context and results of this paper are related to the following strands of the literature. First, my study contributes to the sparse literature on social information and educational decision-making in higher education. To my knowledge, it is the first to apply social information with the goal to affect an educational investment decision.[2] Silva and John (2017) investigate effects of providing information about how many students have already paid their university tuition fees and find that this does not improve payment of late fees. Page et al. (2019) test the effectiveness of different informational interventions at improving refiling for federal student aid. One treatment arm adds social pressure to the basic information by emphasizing the rates at which other students file for financial aid. Overall they find little to no impact for any of their informational interventions.[3] Attempting to nudge students to participate in online teaching evaluations, Neckermann et al. (2022) include a treatment that provides information about the participation rates in past evaluations; this also fails to be effective. Consistent with these results, I also find no evidence for a significant average effect of my social information interventions. In contrast to these studies, however, I am able to uncover heterogeneities in the behavioral response to the social information provided in Intervention 1 by employing endogenous stratification. In conjunction with findings showing that the effects of social information can depend on prior beliefs (Coffman, Featherstone and Kessler, 2017; Cantoni et al., 2019), my results suggest that future studies in higher education may need to be designed with such heterogeneities in mind.

Second, my and the other above-mentioned papers are also related to the more general literature on the effects of social information and norms. Studies from other contexts, however, have generally reported significant positive effects, which is in stark contrast to the results that I and other authors report for educational decisions in college or university. Most closely related is a substantial literature that uses social norms to improve the on-campus behavior of students, i.e., decisions that are arguably only indirectly related to educational outcomes. The majority of those studies is targeted at students' drinking behavior (e.g., Perkins, 2002; Turner, Perkins and Bauerle, 2008; Burger et al., 2011), but other topics such as mental health (Turetsky and Sanderson, 2018), cyberbullying (Doane, Kelley and Pearson, 2016), and hygiene measures (Lapinski et al., 2013) are also addressed (see the appendix of Rhodes, Shulman and McClaran (2020) for a comprehensive list of studies). In their meta-analysis on social norms, Rhodes, Shulman and McClaran (2020) estimate a positive descriptive norm effect on behavioral outcomes of $d = 0.105$ for studies using college-aged participants, which is very close to their overall estimate of $d = 0.097$, but much smaller than the effect size they estimate for field ex-

[2]The literature on the provision of relative performance feedback in higher education also makes use of social information (Azmat et al., 2019; Brade, Himmler and Jäckle, 2021; Dobrescu et al., 2021). However, there, information is usually given about the contemporaneous performance of similar others, which can, for instance, create positive effects through competitive preferences and learning about own ability. Such mechanisms are unlikely to play a role for the type of social information that I study in this paper. In addition, the studies on relative feedback typically aim at affecting decisions and efforts at the intensive instead of the extensive margin.

[3]Studying the effects of student debt letters on student loan decisions, Darolia and Harper (2018) combine information on the median total loan debt of recent graduates with a summary of a student's borrowing to date and an estimate of expected future dept payments. They do not find effects for this combined information treatment.

periments (d = 0.30). The meta-analyses on the effects of nudging by Hummel and Maedche (2019) and Mertens et al. (2022) estimate relative and standardized effects sizes for social reference nudges of 20% and d = 0.40, respectively (see Section 5 for further discussion of my estimates and the evidence from several meta-analyses).[4] However, given the lack of studies from (higher) education, both in the samples of these meta-analyses and beyond, more evidence is needed to establish whether educational decisions are indeed more difficult to influence by social information.

Third, my study relates to the literature that uses nudges to improve decision-making in education (see, e.g., Damgaard and Nielsen 2018 for a review). More specifically, I contribute to research that aims to improve outcomes of students in higher education by providing information via low touch channels, such as text-messages, e-mails, and postal letters. Initial studies showed promising results, especially with respect to enrolling in college and applying for financial help (see French and Oreopoulos 2017 and Bird et al. 2021 for reviews). However, results from recent large-scale field experiments suggest that these interventions may not necessarily scale up, creating the need for alternative approaches (Bird et al., 2021; Bergman, Denning and Manoli, 2019; Gurantz et al., 2021). Furthermore, attempts to improve persistence in college with the help of low-touch information interventions have so far not provided the desired results (e.g., Oreopoulos and Petronijevic, 2018, 2019; Huntington-Klein and Gill, 2019). My study extends this literature by adding the following: First, as mentioned above, it is the first to explicitly test whether information about the past behavior of others affects an educational investment decision in higher education. Given the promising results in other areas, surprisingly little attention has been paid to this approach so far. Second, this study investigates if (social) information can be used to influence smaller educational investment decisions. Previous studies have often focused on educational investments that are likely more difficult to influence, such as whether to enroll in college at all. Third, my results also illustrate that in some contexts a targeted provision of (social) information nudges may be necessary to achieve desired results. This is consistent with Bryan, Tipton and Yeager (2021), who argue that most treatment effects in behavioral science are heterogeneous and that a more systematic approach to their analysis is required.

Finally, the paper contributes to the ongoing discussion on college remediation (e.g., Holzer and Baum 2017; Oreopoulos 2021). My setting allows me to report on the effectiveness of remediation based on experimentally induced variation in attendance, thus providing evidence from complier populations that have not been studied so far. Due to the voluntary nature of remedial education in Germany, the context of my study is also notably different from the existing literature. There, studies mostly report results based on natural experiments that occur when participation is based on performance in a placement test (e.g., Martorell and McFarlin Jr. 2011; Scott-Clayton and Rodriguez 2015; Boatman and Long 2018). In contrast to this type of remediation, voluntary courses are arguably less likely to be stigmatizing and discouraging. Because they are usually scheduled at the beginning of

[4] Evidence for positive effects of social information comes from contexts such as charitable giving (Frey and Meier 2004; Croson and Shang 2008; Martin and Randal 2008; Shang and Croson 2009), public good contributions in the lab (Keser and Van Winden 2000; Fischbacher, Gächter and Fehr 2001), environmentally friendly behavior (Goldstein, Cialdini and Griskevicius 2008; Allcott and Rogers 2014; Byrne, Nauze and Martin 2018; Brent et al. 2020), tax compliance (Hallsworth et al., 2017), job applications (Gee, 2019), and job take-ups (Coffman, Featherstone and Kessler, 2017).

the study program, they may also contribute to students' early academic and social integration, and the effects that I report for late enrolling students are consistent with this idea. However, similar to Boatman and Long (2018), my findings also tentatively suggest that not all changes in course participation translate into respective changes in academic achievements and that the returns to course participation may depend on the complier population. More general, the effectiveness of voluntary remediation (in Germany) is still largely unknown. To my knowledge, the only other more rigorous evidence comes from Büchele (2020*a*,*b*), who uses survey data and difference-in-difference designs and finds some evidence for positive effects on students' skills and performance in math.

The paper continues as follows. In Section 2, I first describe the organization of remedial education in Germany and at the Faculty of Business and Economics and provide some descriptive evidence on the correlates of remedial math course participation and its association with academic achievement. Afterwards, I describe the design of the two interventions as well as the data and empirical approach used for the analysis. In Sections 3 and 4, I present the results of Intervention 1 and 2, respectively. Section 5 concludes by discussing the findings and their implications for policy and future research.

2 Institutional background and research design

2.1 Remedial math education in Germany

Math remediation in Germany is organized differently than in many other countries, such as the US (see, e.g., Büchele (2020*a*,*b*) for further details). Remedial courses are generally offered on a voluntary basis, i.e., they are neither tied to performance in a placement test nor are they a prerequisite for enrolling in the regular study program. The courses are typically organized in a decentralized way by individual departments (mostly for STEM majors), there is no participation fee, and they do not award course credits or factor into a students' grade point average (GPA). Most of the time, they come in the form of preparatory block courses that take place before or at the beginning of the first semester and last roughly two to four weeks, depending on the math intensity of the study program. In terms of content, the courses generally aim to prepare students for their particular study program by reviewing relevant secondary school mathematics. The effectiveness of remedial education in Germany is, however, still an open question, as it has hardly been subjected to rigorous causal evaluation (see the discussions in Büchele (2020*a*,*b*)).

The different organization of remediation has implications for the underlying mechanisms through which it can affect students' academic achievements. Two hypotheses are commonly discussed in the literature (see, e.g., Scott-Clayton and Rodriguez, 2015; Boatman and Long, 2018): On the one hand, remedial courses are supposed to help students develop or refresh the skills they need to succeed in their studies, and it is reasonable to assume that this mechanism applies to courses in Germany as well. On the other hand, it is argued that remediation can be discouraging as it often lengthens the time to graduation by design, and because the placement procedures can be stigmatizing, which may negatively affect students' self-confidence and self-efficacy. Since these discouraging features are not present in the organization of German remedial education, it is arguably less likely that such adverse effects will occur (cf. Büchele, 2020*a*). Instead, students in Germany also perceive the remedial courses as an opportunity to get to know peers and the university at an early stage (Voßkamp and Laging, 2014), and the courses may therefore play an important role in students' academic and social integration. Given the extensive literature on the importance of integration for success in college dating back to Tinto (1975, 1993), this is an interesting avenue for future research.

2.2 Remedial math course at the Faculty of Business and Economics

The focus of the present study is a voluntary remedial math course organized by the Faculty of Business and Economics at one of the largest universities in Germany, which is offered to prepare freshman students of the five bachelor's degree programs for the mandatory math exam and other mathematically demanding compulsory courses.[5] The course takes place at the beginning of the first semester – in the two weeks prior to the start of the official lecture period – and is organized as an eight-day, 42- to 44-hour block course (see Figure A.1 for a detailed course timeline for the two cohorts studied in this paper). Lectures make up about one-third of the course and are mostly held in the morning, while the rest of the time is spent in tutorials to practice the concepts taught in the lectures. All lectures are given by the same instructor, while students are divided into smaller groups of about ten to twenty students for the parallel tutorials, which are taught mainly by students from higher semesters. The course aims at refreshing secondary math knowledge and filling potential gaps, and covers topics such as numbers, arithmetic, summation, binomial formulas, (in)equations and systems of linear equations, exponentiation, root extraction, logarithms, functions, and differential calculus. Study skills, testing strategies, or specific content from the upcoming study programs are not part of the course.

To facilitate the organization of the course, in particular the prior formation of the tutorial groups, students are asked to sign up for the course in advance via a web portal. When students enroll in their study program, which most do during the two months prior to the start of the first semester, they receive information about the course via the following channels: First, information is publicly available on a website that also includes a syllabus and a test that students can use to self-assess their math knowledge prior to signing up. Second, incoming students receive a letter from the student body of the faculty that provides information about the (social) activities that are planned at the beginning of their studies, including the remedial math course. Third, the organizers of the course themselves email the students, inviting them to participate in the course. Students who sign up for the course get access to an online platform on which further details about content and structure are provided about one week before the start of the course. Via the platform, a few days before the course starts, students

[5]The math exam is mandatory in all but one of the five bachelor programs and students have to pass it by the end of the second semester in order to continue with their program. Due to the coronavirus pandemic, this requirement was relaxed and students in my sample were allowed to pass the math exam in later semesters. However, students were not aware of this when enrolling their program. The lecture associated with the math exam is taught by a different instructor than the remedial course. It is therefore unlikely that students will expect to gain instructor-specific knowledge in the remedial course about how to pass the math exam.

are also informed about the tutorial group to which they have been assigned.[6]

2.3 Descriptive evidence on remedial math course participation and its association with academic performance

Given all the available information, students should be well aware of the remedial math course. Nevertheless, a considerable number of students does not sign up for the course and attendance is even lower: for instance, in the control group of Intervention 1, only 76% of the incoming students sign up for the course, and the participation rate in the first tutorial and the average participation across all tutorials – i.e., the share of tutorials a student participates in – are 70 and 60%, respectively (see bottom row of Table 1).[7]

But there may be good reasons for students to refrain from participation. First and foremost, students may be certain that they already possess the mathematical skills needed for their studies. To gain some insights into what is driving students' decision-making, Table 1 shows estimates of regressing sign-up and participation in the course on several background characteristics using the control group of Intervention 1 (see Section 2.5 and Table A.1 for further details on all variables). The results show that most of the covariates have no statistically significant effect. Notably, this includes ability or academic preparedness, as measured by students' high school GPA (p = 0.237, 0.568, and 0.271 in Columns 1, 3, and 5). Instead, the most relevant correlates of the participation decision are the following: First, students for whom this is the first semester at any university are 22.4 to 28.5 pp more likely to sign up for and participate in the course. This is plausible, because students who have already studied at this or another institution may have already attended a similar course or be confident that their math skills are sufficient. Second, students who enrolled in their study program within the last month before the beginning of the math course are somewhat less likely to sign-up for the course (9.0 pp, p = 0.100; Column 1), but are particularly less likely to participate in it (13.9 and 16.3 pp, p = 0.019 and 0.002, respectively; Columns 3 and 5). For these students the course may be less salient, because they are still busy with organizational issues such as looking for or moving into their accommodation. Additionally, some of them may be unable to attend because they have not yet moved to the city where the university is located.[8]

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[6] During the sign-up process students are asked to self rate their math knowledge. The organizers use this information and try to form homogeneous tutorial groups.

[7] Because my intervention sample comprises only students who enroll in their study program until about one week before the beginning of the remedial course, these numbers likely overestimate the true extent of sign-up and participation. Students who enroll later, which is also possible after the official start of the semester, are less likely to participate or may not be able to participate at all at some point.

[8] The place – i.e., the federal state, – in which students' obtained their high school degree is also a significant predictor of sign-up and participation, as indicated by the p-value of the F-test for joint significance. The predictive margins for each category are presented in Table A.2. They show that students who obtained their high school degree abroad or outside of the federal state in which the university is located and two of its neighboring states are more likely to participate in the course. Possible explanations are that these students were previously located farther away and thus use the course as an opportunity to meet new peers or that these students are less certain about whether their math knowledge matches up with what is required for their studies (note that education policy in Germany is mostly under the jurisdiction of the federal states and that the secondary school curricula can therefore differ between states).

However, the observed covariates exclude other factors that may be important for students' participation decision such as further external constraints[9], their beliefs about the participation decision of others, or their beliefs about the effectiveness of the course. Moreover, given the lack of robust causal evidence on the effects of remediation in Germany, it is not even clear whether attendance should be a priority for most of the students. To provide some suggestive evidence on this, in Table 2, I use the same sample as above and regress different measures of academic achievement on average participation, controlling for all covariates presented in the previous table; the effects of the standardized and inverse-scaled high school GPA are shown as comparison (on the original scale 1.0 is the highest and 4.0 the lowest possible GPA). The results show that going from 0 to 100% participation is related to a significant increase in the likelihood to attempt and pass the math exam within the first semester (year) by 51.8 (49.1) pp and 42.5 (44.9) pp, respectively. It is also related to an increase in obtained credits by 9.7 (17.7), and a reduction in the probability to drop out of the study program by 12.9 (17.2) pp. For these outcomes, the effects of participation are more pronounced and significant than the effects of a better high school GPA. For the grade in the math exam and the GPA in university, on the other hand, participation in the math course appears to be of little value. If one is willing to assume that grades depend more on ability and skill development, while the credit load and the decision to drop out depend more on a successful social and academic integration, these results provide some tentative evidence that the remedial math course may be beneficial to students because it helps with the latter.

In sum, the evidence presented in this section suggests that in the absence of my interventions a substantial number of students does not sign up for or participate in the course, even though it might actually be beneficial for them to do so.

2.4 Design of the interventions

Against this background, I partnered with the organizers of the remedial math course and designed two social information interventions with the goal of increasing the share of students that sign up for and participate in the remedial math course: i) an invitation letter that includes information on a previous sign-up rate for the course (Intervention 1), and ii) a reminder letter for students who signed up for the course, containing information about how helpful students have evaluated the course in the past (Intervention 2). To test their effectiveness, I conducted field experiments with a cohort of incoming first-year students who enrolled in the summer term and the subsequent cohort in the winter term (the summer and winter terms in Germany are generally equivalent to the spring and fall terms in other countries). The general design and timing of the interventions was the same in both cohorts and is summarized in Figure 1.[10]

[9] Another potential reason for non-attendance is that students are working to finance their studies. Staneva (2018) provides evidence from a representative student sample and shows that at the beginning of their studies, only about 20% of German students are working, and that they spend about 11 hours per week on doing so. It is thus unlikely that work prevents many students from participating in the course.

[10] The research design for both cohorts was approved by the IRB and the data protection officer of the university. The first experiment (summer term) is pre-registered under `https://osf.io/tm7k3` and the second experiment (winter term)

2.4.1 Intervention 1: information on past sign-up rate

Starting five to seven weeks before the beginning of the remedial math course until one week before, I used administrative data on the incoming students provided by the university to randomize a total of 789 students into a control and one (summer term) or two (winter term) treatment groups. Randomization was carried out using stratification and re-randomization (Morgan and Rubin, 2012); Appendix B provides details on the randomization and shows that the samples are well balanced.

In both cohorts, students who were randomized into the **control** group (=**S0**, $N = 296$) received no information about the remedial math course beyond the sources mentioned in Section 2.2.

Both in the summer and the winter cohort, students who were randomized into the **social information** group (=**S2**, $N = 299$) received a letter that I sent on behalf of the organizers of the course (see Figure A.2). The letter stated that "[...] in order to help you get off to a good start in your studies, we would like to invite you to the remedial math course for students of business and economics. The course provides mathematical knowledge that is required in the mathematics lecture and in numerous other courses." and quoted the website where students could sign up for the course and get additional information. The letter continued by stating that "85% of the first-year students who, like you, were enrolled in a business or economics degree program in the last semester have signed up for the remedial math course. Only a small minority of students does not sign up for the remedial course". The figure of 85% was based on the sign-up rate for the remedial math course in the winter term that preceded the first experiment and was calculated among all students who enrolled in a study program at the Faculty of Business and Economics for the first time.[11] The aim of this information was to signal to students that the vast majority of students sign up for the math course and that they should thus do the same.

From a theoretical perspective, at least two arguments can be made why information on past sign-up rates should lead to an increase in sign-ups and participation. First, one can follow the model in Coffman, Featherstone and Kessler (2017) and assume that incoming students believe that higher sign-up rates signal higher returns to the remedial math course, and that individuals will only sign-up for and participate in the course if their beliefs about the expected sign-up rate are above some personal threshold. For individuals who are not going to sign up and participate, as their initial beliefs are below their threshold, treatment could lead to an increase in sign-up and subsequent participation, if the information on the past sign-up rate shifts the beliefs above their personal threshold.[12]

under `https://osf.io/vqa84`. The interventions in both cohorts and the respective math courses took place before the coronavirus pandemic. For the first cohort, the pandemic started shortly before the exam period of the second semester, and for the second cohort it started shortly before the exam period of the first semester. Exams during the pandemic were in part held online and most lectures during the second semester of the second cohort took place online.

[11] Including students who did not enroll for a study program at the faculty for the first time results in a lower sign-up rate, as it includes for example students who simply switched programs and thus already had the possibility to participate in the math course at a previous point in time.

[12] In principle, it is also possible that the social information shifts beliefs downwards, resulting in a decrease in sign-up and participation rates. A more general model on the effects of social information taking this into account is provided by Coffman, Featherstone and Kessler (2015).

Second, and more generally, the sign-up rate can be understood as a descriptive norm if students prefer to sign up and participate in the course when they expect other students to do the same (see, e.g., Bicchieri and Dimant 2019). The social information included in the letter could then increase the sign-up and participation rate if it leads to an upwards shift in the beliefs about the descriptive norm. The descriptive norm interpretation is also the reason why I included the sentence "Only a small minority of students does not sign up for the remedial course". Recent results in the literature on social norms have shown that presenting behavior as a minority activity can increase the effectiveness of descriptive norms (e.g., Hallsworth et al. 2017).

Due to all the available information, the remedial math course should have been salient to students, even in the absence of the social information treatment. However, it could still be the case that the invitation letter and its personal nature increase the salience of the course or lead to an increase in the sign-up rate and ultimately participation through some channel that is unrelated to the information on the past sign-up rate. To explore if this is the case, in the second cohort, I included a **salience** group (=**S1**, $N = 194$), which also received an invitation letter, but without the social information; i.e., it excluded the parts highlighted in gray in Figure A.2.

2.4.2 Intervention 2: information on past evaluation

About one week before the start of the course (see Figure 1), all students ($N = 574$) who signed up for the course up to that point were randomized into a control and one (summer term) or two (winter term) treatment groups. I stratified the randomization on the treatment status in Intervention 1 such that the two randomizations are orthogonal to each other (see Appendix B for details and the balancing properties).

While students in the **control** group (=**E0**, $N = 215$) received no letter, students in the **social information** group (=**E2**, $N = 211$) received a reminder letter that I sent on behalf of the organizers of the course (see Figure A.3). The letter stated that "[...] you have signed up for the remedial math course. We have therefore already reserved a seat for you and look forward to your participation. The course starts on ⟨*date*⟩ at ⟨*location*⟩" and mentioned the website where students could find the information on the tutorial group they were allocated to. Instead of social information similar to the one used in Intervention 1, the reminder included information about how helpful students evaluated the course in the past, as it stated that "95% of students who, like you, are enrolled in a business or economics degree program say that the remedial course in mathematics has made it easier for them to get started with university mathematics." This figure was based on one of the questions that was asked in a survey that was carried out a few years earlier by the course organizers among students who attended the mathematics lecture. Students were asked "[...] whether the remedial math course made it easier to get started with university mathematics?". On a scale from "1=no, not at all" to "7=yes, very much", 95% of 290 survey-participants had chosen answer category 5 or higher.

I expect that the information on how previous students have evaluated the course leads to a decrease in attrition between sign-up for and participation in the course. A similar argument as before can be made. The treatment should provide a direct signal about the (subjective) returns to the remedial math course and should thus influence the participation decision of students who are unsure about the utility of the course, and for whom this signal leads to a sufficiently large upward shift in the expected utility.

Following the reasoning for Intervention 1, I again wanted to be able to explore if potential effects of the reminder letter are driven by the social information or the letter itself. Therefore, in the second cohort, I also included a **salience** group (=**E1**, $N = 148$), which received a reminder letter without the social information (parts highlighted in gray in Figure A.3).

2.5 Data and estimation

2.5.1 Data

For the randomization and the analyses presented throughout this paper I use data from three sources (Table A.1 describes all variables in detail): First, I use administrative information on background characteristics for covariates. Second, to assess the immediate impact of my interventions, I received data from the organizers of the remedial math course about sign-ups for the course and participation in each of the tutorials. Third, I use administrative data from the student office about students' academic achievements in the first year of studies to investigate whether potential effects on participation translate into higher academic achievement.[13] For the analyses I report in the main paper, I pool the data from both cohorts (Appendix C presents results separated by cohort – following the respective pre-registrations.)

My main outcome variables are sign-up for the remedial math course, participation in the first tutorial, and average participation – i.e., the share of tutorials a student participated in. I use both measures of participation, as the participation in later tutorials might be affected by the content of the course and its interaction with the treatments. For example, students may learn that the content of the course is not as useful as the information in the letters suggested. On the other hand, average participation is arguably the more relevant outcome with respect to the later performance in the study program.

In follow-up analyses, I also study effects on academic achievement after the first semester and the first year of studies. Since the remedial course aims at improving math knowledge, I expected that attendance primarily affects whether students attempt and pass the math exam, and what grade they receive. In addition, I am interested in students' overall academic performance. For this, I consider the number of passed course credits[14], whether they dropped out of their study program[15], and their

[13] The analysis of the effects on academic achievement were pre-registered after the analyses of the effects on the sign-up and participation rates but before data on academic achievement was available to me. The pre-registration can be found under `https://osf.io/tv9yf`.

[14] Europe-wide, universities use a standardized point system (European Credit Transfer and Accumulation System, ECTS), under which a full-time academic year consists of 60 credits, with the typical workload for one credit equaling 25-30 study hours. See also `https://education.ec.europa.eu/levels/higher-education/inclusion-connectivity/european-credit-transfer-accumulation-system`, retrieved on March 23, 2022.

[15] Dropout captures both students who left the university system completely and students who merely switched the study program and/or university. However, my data does not allow me to differentiate between those cases.

GPA. These variables were pre-registered as secondary outcomes, and I initially had no clear hypothesis as to which of these dimensions should be most influenced by remedial math course attendance. Therefore, and to reduce potential concerns regarding multiple hypothesis testing, I follow the approach suggested by Anderson (2008) and additionally construct an inverse-covariance weighted index of the three variables using the Stata program *swindex* by Schwab et al. (2020), which I use as an outcome when investigating the effects on academic achievement (the use of this index was not included in the pre-registration for the effects on academic achievement).

2.5.2 Analysis of main effects

Regarding the main effects of the interventions, I provide intention-to-treat effects from OLS estimations that compare the average outcomes of the control group with the outcomes of the treatment groups. In the baseline specification, I control for the random assignment within blocks:

$$Y_i^k = \alpha_0 + \alpha_1 Salience_i + \alpha_2 SocialInformation_i + \mathbf{x_i}\boldsymbol{\alpha_3} + \varepsilon_i, \tag{1}$$

where Y_i^k denotes the level of outcome measure k for individual i. $Salience_i$ is an indicator for being randomized into the groups that receive the invitation or reminder letter without social information (**S1** or **E1**, respectively). $SocialInformation_i$ is an indicator for being randomized into the treatment groups that receive the invitation or reminder letter including the respective social information (**S2** or **E2**). The vector $\mathbf{x_i}$ controls for the method of randomization by including study program fixed effects, a winter term dummy, and the interaction between the study program fixed effects and the winter term dummy. Additionally, it includes invitation letter date fixed effects when analyzing Intervention 1 and indicators for the treatment status in Intervention 1 when analyzing the effects of Intervention 2.

In additional specifications, I add a vector $\mathbf{z_i}$, which includes further covariates:

$$Y_i^k = \alpha_0 + \alpha_1 Salience_i + \alpha_2 SocialInformation_i + \mathbf{x_i}\boldsymbol{\alpha_3} + \mathbf{z_i}\boldsymbol{\alpha_4} + \varepsilon_i. \tag{2}$$

I follow two different approaches for selecting the variables that I include in this vector. First, I simply include all variables that were pre-registered as controls for the second experiment. This includes the first university and female dummies[16], the age at the beginning of the first semester, the high school GPA, an indicator if the high school degree was obtained within the last year before the beginning of the first semester, a dummy for the type of high school degree, indicators for the place where the high school degree was obtained, and the distance over which the letter was sent in kilometers (see Table A.1 for more information).[17]

[16] Although I was sometimes able to stratify on those variables as planned in the pre-registration, this was not possible in the majority of the randomizations. Thus, I include those variables in the control vector and not in the baseline specification.

[17] The distance over which the letter was sent was only pre-registered as a control variable in the second experiment. I consider this variable to potentially improve the precision of my estimates, as it is conceivable that students who live farther away when they receive the letter are less likely to sign-up for and participate in the course, as they may still need to move

Second, I would like to employ an approach in which covariates that were not pre-registered are included in a non-arbitrary way and that furthermore leads to parsimonious specifications which only include covariates that either increase the precision of my treatment effect estimates or account for imbalances that are observed despite the randomization. For this purpose, I use the double-post LASSO approach suggested by Belloni, Chernozhukov and Hansen (2014) to select the covariates to be included in $\mathbf{z_i}$. In the covariate selection process, I consider the pre-registered covariates and, for estimations of the effects of Intervention 1, I additionally consider an indicator if the student signed up for the remedial math course before I sent, or would have sent in case of the control group, the invitation letter.

2.5.3 Analysis of heterogeneities

In addition to the main analysis, I pre-registered to explore heterogeneities along the following dimensions. First, based on the institutional background and information by the organizers of the remedial math course I expected that students who were previously enrolled at this or another university would be less likely to participate in the remedial math course, as they may have participated in a similar course previously (see results presented in Section 2.3). It thus appeared plausible to suppose that effects would be concentrated among students for whom this is the first semester at any university.

Second, I expected that students with an enrollment date closer to the remedial math course would be less likely to sign up for and participate in the course (also see Section 2.3). These students may still be busy with organizational matters such as looking for accommodation, resulting in a lower (relative) salience or importance of the course. By increasing the salience or expected utility of the course, treatment may thus be particularly effective in this group. However, if external constraints are too strong, such as students not having accommodations at the beginning of the course, treatment effects could also be smaller in this group. By design, the randomization of Intervention 1 was stratified by the timing of enrollment – i.e., the invitation letter date – anyway, giving me further reasons to pre-register this dimension.

Lastly, initial results from the first cohort tentatively suggested heterogeneity with respect to students' sex, i.e., men seemed more responsive to treatment. Given the existing evidence for greater math anxiety and lower self-efficacy among women as well as large cross-country variability in gender differences in math anxiety and achievement (e.g., Else-Quest, Hyde and Linn, 2010; Huang, 2013), heterogeneity on this dimension seemed plausible, and I thus pre-registered to explore sex as a potential source of heterogeneity for the field experiments in the winter term.

To estimate heterogeneous effects of my treatments for the pre-registered covariates, I employ the

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to the city where the university is located.

following regression specification:

$$\begin{aligned} Y_i^k = \alpha_0 + \alpha_1 Salience_i + Salience_i \mathbf{c_i} \boldsymbol{\alpha_2} + \alpha_3 SocialInformation_i \\ + SocialInformation_i \mathbf{c_i} \boldsymbol{\alpha_4} + \mathbf{c_i} \boldsymbol{\alpha_5} + \mathbf{x_i} \boldsymbol{\alpha_6} + \mathbf{z_i} \boldsymbol{\alpha_7} + \varepsilon_i. \end{aligned} \tag{3}$$

Where Y_i^k, $Salience_i$, and $SocialInformation_i$ are defined as before. The vector $\mathbf{c_i}$ includes one or all of the covariates for which I want to study the treatment effect heterogeneity. In case of Intervention 1, this includes the first university dummy, the female dummy, and a dummy that indicates if a student received, or could have received in case of the control group, the invitation letter within the last month before the beginning of the remedial course.[18] For Intervention 2, it includes the female and the first university dummy. The vector $\mathbf{x_i}$ again controls for the method of randomization. When I study the heterogeneity of Intervention 1 with respect to the timing of enrollment, this vector now excludes the invitation letter date indicators. The vector $\mathbf{z_i}$ includes the pre-registered covariates, with the exception of those for which I investigate the heterogeneous effects of treatment.

2.5.4 Statistical power of the analysis

I performed power calculations for my interventions after the field experiments in the summer term, as it was clear that for the first cohort alone, I would not have enough power to detect reasonable effect sizes, and because this enabled me to gather additional information on important parameters. Table 3 shows minimal detectable effect sizes (MDES) for $1-\beta = 0.6$ and $1-\beta = 0.8$, assuming $\alpha = 0.05$, in terms of control group standard deviations, percentage points, and relative effects – i.e., percentage changes between the dependent variable of the treatment group and the control group.

For Intervention 1, I have 60% power to detect effects sizes of 0.16 to 0.17 standard deviations, which, taking the observed sign-up and participation rates in the control group of the summer cohort into account, corresponds to effects of 7.3 to 7.7 pp and relative effect sizes of 11.0 to 13.8%. With a power of 80%, I am able to detect effects of 0.20 to 0.21 standard deviations or 9.3 to 9.8 pp (14.0 to 17.4%). For Intervention 2, my MDES corresponds to 0.21 (0.26 and 0.27) standard deviations or 5.7 and 6.0 (7.2 and 7.6) pp and 6.2 and 7.8% (7.4 and 9.3%), with a power of 60% (80%).

To evaluate if these MDESs are reasonable in light of previous findings on the effects of social information, I initially compared them to effects sizes that Hummel and Maedche (2019) report in their meta-analysis on the effectiveness of nudging. They report relative effect sizes and find that the median (average) effect size is 20% (29%) for social references nudges and 8% (28%) for reminders. My study is well powered enough to detect these kind of effect sizes.

However, my power calculations are subject to three important caveats. First, Hummel and Maedche (2019) state that due to publication bias their effect sizes likely present an upper bound for the effects of nudging. I come back to this in the discussion of my results in Section 5, where I draw on several additional meta-analyses. Second, because I introduced the salience treatments only in the

[18] I define the timing of the letter/enrollment in this way, because the respective cells get increasingly small in the last weeks before the beginning of the course, making a more fine-grained heterogeneity analysis difficult (see Table B.2).

second cohort, I have less statistical power to detect differences between the two treatment groups of each intervention than I do to detect differences between the social information treatments and the control groups, which are included in both cohorts. The salience treatments therefore mainly serve an exploratory purpose. Third, I pre-registered my heterogeneity analyses as exploratory instead of confirmatory and did not perform power analyses for them, i.e., the pre-registration mainly served as a commitment to limit my analyses to the aforementioned dimensions.

3 Effects of Intervention 1

In this section, I first report the main effects of Intervention 1, i.e., the invitation letter with information on the past sign-up rate for the remedial math course. I then study if the average treatment effects mask heterogeneous responses and whether effects on participation in the course carry over to students' academic performance in the first year of studies. The results of Intervention 2 are presented in Section 4.

3.1 Main effects

Table 4 presents estimates for the effects of Intervention 1 on remedial math course outcomes. Columns (1) to (3) present the effects on sign-up. In the control group, 76% of the students sign up for the remedial math course. Among students who receive the invitation letter with social information, the sign-up rate is 1.5 pp ($p = 0.646$) higher. Adding controls in Columns (2) and (3) increases the coefficient to 2.4 pp ($p = 0.447$) and 2.1 pp ($p = 0.494$), respectively. The salience treatment, i.e., the invitation letter without social information, decreases the sign-up rate by about 3 pp across all three specifications ($p = 0.386$ in Column 3).

Effects on participation follow a similar pattern. In the absence of treatment, 70% of the students participate in the first tutorial of the remedial math course. Students who receive the social information treatment increase participation by 0.3 to 1.1 pp ($p = 0.943 - 0.745$; Columns 4 to 6). The salience treatment decreases participation in the first tutorial by 4.1 to 4.5 pp ($p = 0.251$ in Column 6). Regarding average participation, I observe that students in the control group participate in 60% of the tutorials (Columns 7 to 9). Even though this leaves more room for improvement, treatment effects are similar to those of the first two outcomes.

Overall, this suggests that, on average, the information on the past sign-up rate is not able to increase sign up for and participation in the remedial math course. Treatment effects are neither statistically significant at any conventional level, nor are they particularly large from a practitioners' point of view. Invitation letters without social information may even decrease sign-up and participation in the course. One reason for the latter may be that sending students an invitation letter, in addition to all the other information on the course they already receive, could signal that participation in the course was too low in the past; thus acting similarly to social information about low sign-up rates.

3.2 Heterogeneity

Next, I study whether the main effects mask heterogeneity. Along the pre-registered dimensions, I find the most robust evidence for the timing of enrollment, which I discuss in detail below. With respect to students' sex and the first university dummy, I find the following (see Table A.3 for details): Students in the control group who were not enrolled at any university before are about 30 pp more likely to sign up for and participate in the remedial math course, but I find no evidence of heterogeneous treatment effects. Students' sex, on the other hand, is not predictive of sign up and participation in the absence of treatment, but I find that effects of the social information (salience) treatment on participation are between 3.1 to 6.7 pp (10.6 to 13.6 pp) larger for women compared to men; these effects, however, are not estimated precisely.[19]

Timing of enrollment. Panel a) in Table 5 presents effects by timing of enrollment. About 31% of the sample enrolled late and were sent the invitation letters within the last month before the beginning of the remedial course (see Table B.2 for the exact timing and number of observations). The first important observation is that students who enroll late are less likely to sign up for and participate in the course: their sign-up rate is 9.1 to 9.3 pp ($p = 0.102 - 0.085$) lower compared to early enrollees, and average participation is decreased by 15.7 to 16.8 pp ($p = 0.003 - 0.002$). Looking at the treatment effects and their interaction with the last month dummy, I find that the social information and the salience treatment are both more effective for late enrollees: compared to students who receive the letter before the last month, the effects on sign up and, more importantly, average participation are 5.3 to 8.6 pp ($p = 0.494 - 0.321$) and 13.6 to 17.2 pp ($p = 0.098 - 0.013$) larger, respectively.

The fact that the two treatments produce the same pattern and magnitude of results suggests that the information on the past sign-up rate provided in the social information treatment plays little role. Rather, it is plausible that the remedial math course is not salient enough among students who enroll late, but that invitation letters are able to mitigate this, independent of the social information. In light of this finding, and to increase the statistical power of my analysis, in Panel b) of Table 5, I present results that pool observation from both treatment arms: sending students any of the two letters is 15.5 to 15.8 pp ($p = 0.024 - 0.014$) more effective at increasing average participation for students who enroll late compared to those who enroll early; treatment effects in the two groups are 9.4 to 10.4 pp ($p = 0.108 - 0.057$) and −5.4 to −6.2 pp ($p = 0.119 - 0.096$), respectively.

Endogenous stratification. So far, I have presented evidence that, on average, the invitation letter intervention does not affect sign up for and participation in the remedial math course, but that effects are heterogeneous with respect to the timing of enrollment. Going beyond the pre-registered dimensions, the goal of the following analysis is to explore heterogeneous effects in a more general way (the following approach was only included in the pre-registration for the effects on academic

[19] Related to the literature on gender differences in math achievements, I also do not find evidence for significant differences in math performance or overall academic achievements between male and female students after controlling for other characteristics. If anything, women tend to perform somewhat better than men.

achievement). Specifically, it is conceivable that the effects of the social information about the past sign-up rate depend on the ex-ante sign-up probability of students: First, students with a low sign-up probability simply have more room for improvement. Second, one might assume that some students refrain from participation because their beliefs about the sign-up rates of other students, and thus also about the utility of the course or the descriptive norm, are too low (see theoretical considerations in Section 2.4). In this case, the information about the past sign-up rate may provide a signal that shifts students' beliefs about the utility of the remedial math course or the descriptive norm upwards, thereby increasing sign-up and participation.

To study heterogeneity along this dimension, I construct endogenous strata employing an approach similar to Abadie, Chingos and West (2018): First, in the control group, I regress sign-up on all pre-registered controls, the strata variables, and their interactions with the winter cohort dummy. Next, I use the estimates to predict the sign-up probabilities in the control and treatment groups. For the control group, I use the leave-one-out predictions to avoid "overfitting bias" (see Abadie, Chingos and West 2018). Finally, within the two cohorts, I divide my sample into terciles to obtain three endogenous strata (low, middle, and high ex-ante sign-up probability). I then run the following regression specification to estimate treatment effects by strata:

$$\begin{aligned} Y_i^k = \alpha_0 + \alpha_1 Salience_i + Salience_i \mathbf{e_i} \boldsymbol{\alpha_2} + \alpha_3 SocialInformation_i \\ + SocialInformation_i \mathbf{e_i} \boldsymbol{\alpha_4} + \mathbf{e_i} \boldsymbol{\alpha_5} + \mathbf{x_i} \boldsymbol{\alpha_6} + \mathbf{z_i} \boldsymbol{\alpha_7} + \varepsilon_i, \end{aligned} \tag{4}$$

where Y_i^k, $Salience_i$, $SocialInformation_i$, $\mathbf{x_i}$, and $\mathbf{z_i}$ are defined as in Equation 2. $\mathbf{e_i}$ includes the endogenous strata dummies. Based on this equation, I provide unadjusted estimates of the treatment effects in each endogenous strata by only including the vector $\mathbf{x_i}$, and adjusted estimates by also including the vector $\mathbf{z_i}$ with additional covariates.

Besides allowing me to study heterogeneity with respect to students (counterfactual) sign-up probabilities, this approach has further advantages: First, compared to exploring heterogeneities along multiple covariates and sample splits, it reduces issues associated with multiple hypotheses testing and the selective presentation of significant results. Second, from a policy perspective, identifying heterogeneities on this dimension could provide an easy way to target the intervention in future cohorts. This might be necessary, for instance, if there are negative effects for some students, but it could also help to further reduce the already low cost of the intervention.

Results are reported in Figure 2 and Table 6. Among students in the highest tercile of predicted sign-up probabilities, 86.8% (88.4% without any controls) of controls sign up for the remedial math course. In this group, both the salience and the social information treatment decrease sign-up by 5.8 pp ($p = 0.290$) and 9.6 pp ($p = 0.070$), respectively (see Column 2 in Table 6). Importantly, these effects persist: students in the two treatment groups are 6.9 pp ($p = 0.292$) and 12.3 pp ($p = 0.042$) less likely to participate in the first tutorial of the course, and show an 8.5 pp ($p = 0.162$) and 8.9 pp ($p = 0.118$) lower average participation rate across all tutorials (see Columns 4 and 6 in Table 6). Adjusting for covariates in Columns (3), (5), and (7) leads to attenuated estimates, in particular for the salience treatment.

In the middle tercile, 78.5% (78.6% without any controls) of control group students sign up for the course. While the salience treatment has no effect on either sign-up or participation in this group, my estimates indicate that the social information on the past sign-up rate is able to increase sign-up for the course by 10.4 pp (p = 0.042; see Column 2 in Table 6). This effect translates into a higher participation rate in the first tutorial and across all tutorials by 12.8 pp (p = 0.026) and 10.4 pp (p = 0.054), respectively (see Columns 4 and 6 in Table 6). Here, Columns (3), (5), and (7) indicate that the estimated effects are robust to the inclusion of covariates.

For students in the lowest tercile – among whom 65.4% (62.1% without any controls) of controls sign up for the remedial math course – my estimates indicate that neither treatment is able to affect the decision to sign up for or participate in the math course. Both for the salience and the social information treatment, I test if the interaction with the endogenous strata, i.e., α_2 and α_4 in Equation 4, are equal to zero. The p-values of the corresponding F-tests are depicted in the bottom rows of Table 6. For the social information treatment, the null hypothesis can be rejected at the 5 to 10%-level in all but one of the specifications.

Turning back to the theoretical considerations, the pattern of results found for the invitation letter with information on the past sign-up rate can be explained in the following way: For students in the lowest tercile, the social information may not be able to increase sign-up, because the signal does not increase beliefs sufficiently, or because these students simply do not expect to gain any value from the course, e.g., because they have previously participated in a similar course. The latter notion is supported by the observation that the share of students for whom this is the first semester at any university is particularly low in this strata (36.4%, compared to 77.6 and 95.8% in the middle and highest strata). The middle tercile, on the other hand, may consist of marginal students – i.e., students for whom the social information nudge leads to an increase in beliefs that is large enough to induce sign-up and participation. For the highest tercile, the opposite could be driving the results. Some students might have expected a higher sign-up rate than the letter suggests, and the signal may thus have led to a downward adjustment in beliefs, leading to lower sign-up and participation rates.

3.3 Effects on academic achievement

The goal of the remedial math course is to prepare students for the mathematically more demanding subjects of their studies. To provide some evidence on whether the course is successful in doing so, in this section, I study if the heterogeneous effects on average participation presented above translate into increased performance in the math exam and higher overall achievement in the first year of studies.

Timing of enrollment. Tables 7 and 8 present heterogeneous treatment effects on academic achievement with respect to students' timing of enrollment. To increase statistical power, and because the effects on average participation were similar, I again pool observations from both treatment arms. Both tables show that control students who enroll late suffer from lower academic achievement compared to early enrollees: After the first semester, they are 13.9 pp (p = 0.016) and 12.4 pp

(p = 0.032) less likely to have attempted and passed the math exam, respectively (Columns 1 and 2 of Table 7). Further, their overall performance index – i.e., the standardized inverse-covariance weighted average of obtained credits, dropout, and GPA – is 0.256 standard deviations lower (p = 0.070), they obtain 4.23 credits less (p = 0.008), and are 6.2 pp (p = 0.115) more likely to have dropped out of their study program (Columns 1 to 3 in Table 8).[20] After the first year of studies, they are even less likely to have attempted or passed the math exam (−17.0 pp, p = 0.003 and −15.2 pp, p = 0.009; Columns 4 and 5 of Table 7), and their overall performance is still 0.257 standard deviations (p = 0.046) lower compared to students who enroll early (Column 5 of Table 8).

For all dimensions of academic achievement presented in Tables 7 and 8, I find that the pooled treatments are able to offset all or almost all of the disadvantage in academic achievement that I observe for students who enroll late. Given that my intervention increased average participation of these students by about 10 pp, it may seem difficult at first to rationalize these large effects on performance: for example, if I assume that 10% of the students in the treatment group go from 0 to 100% participation in the course, the estimated effect on first year credits (Column 6 in Table 8) would imply that participation in the course increases obtained first year credits by about 56, which is close to the course load of a full academic year (see Footnote 14).

This raises the question as to how the large increase in achievement among late enrollees comes about. One possible explanation is that in the absence of treatment these students suffer from low motivation and low academic and social integration. The remedial math course may then be particularly beneficial because it leads to higher engagement with the university, other students, and their studies, thereby leading to higher motivation and preventing students from dropping out of their program early on. This may be of particular relevance in the German context, where tuition fees are generally very low, and students thus face very low direct costs of studying.[21] The higher dropout rate among late enrollees in the control group and the decrease in dropout due to treatment presented in Table 8 already provides some evidence that is consistent with that notion.

To study this idea further, Figure 3 depicts histograms of average participation and obtained credits by whether students received the invitation letters within the last month before the beginning of the course, separately by treatment status (the distribution of the control group is shown in dark blue and is overlaid by the distribution of the combined treatment group in transparent green). The top plot in Panel b) first provides evidence that – among late enrollees – the invitation letters do indeed lead to a large increase in average participation among few individuals rather than a small increase among many individuals. Second, the middle and the bottom plot in Panel b) provide evidence that in this group of students, about 35% (28%) of the controls obtain fewer than 5 credits in the first semester

[20] Effects on the grade in the math exam and the GPA go in the same direction (in the German system 1.0 is the best, and 4.0 the worst passing grade). They should, however, be interpreted with caution, since the outcomes are only observed for students who have attempted the math exam at least once or passed at least one graded exam, respectively.

[21] At the time of the field experiments, the tuition fees at the university were around 350€ per semester. The tuition fees include free use of public transportation in the city where the university is located, and students can also take regional trains in the federal state for free. This is effective from the start of the semester and therefore typically also includes the entire period of the remedial math course. In the second cohort of my study, the course started one day before the official start of the semester and the first day of the course was thus not covered by the ticket.

(year) – providing evidence for the low engagement with their studies. Among treated students, this share is decreased by roughly 15 pp and the distribution of credits instead is very similar to the ones that I observe for students that enroll early (shown in Panel a).

Overall, these results provide evidence that increasing remedial math course participation among late enrollees results in higher academic performance in the first year of studies. Given that these students make up around 31% of the sample, my findings suggest that the remedial math course is beneficial for a considerable number of students. In fact, since my intervention only includes students who enroll in their study program up to one week before the math course begins, my results may represent a lower bound.

Endogenous strata. Studying treatment effect heterogeneity across the endogenous strata suggests that increased remedial math course participation does not translate into higher academic achievement among all subgroups. Based on Equation 4, Tables A.4 and A.5 report effects of the salience and the social information treatment on the academic achievement dimensions across the three endogenous strata. Overall, I find little to no robust evidence that the effects on participation presented in Table 6 carry over to academic performance. For one, there is very little evidence for any significant effects in the first place. Given the large number of estimates, some of the few significant ones may simply arise from multiple hypothesis testing. In addition, given that the different dimensions of academic achievement are usually highly correlated with each other, I would expect changes in average participation to translate into consistent changes in academic achievement across the different outcomes – similar to what I found with respect to the timing of enrollment. However, this is not the case, further suggesting that the few significant estimates do not represent a robust pattern.

This raises the question as to why the effects on participation that I found for the social information treatment do not carry over to academic achievement. One plausible explanation is that the academic performance of students who are identified by the endogenous strata is simply not so easily changed by remedial math course participation. This may for example be the case because the students in the middle tercile – i.e., those who increase participation in response to the social information treatment – already possess the knowledge that is necessary to pass the exams. Table A.6 depicts the means of my outcomes by endogenous strata among control group students and provides some evidence that supports this idea: with respect to almost all outcomes, students in the middle tercile have a higher performance compared to students in the highest tercile; they are, e.g., more likely to have passed the math exam after the first year of studies, they obtain more credits, and they are similarly likely to drop out of their program. In contrast to the heterogeneities with respect to the timing of enrollment – where math course attendance and academic achievements are related to each other in the absence of treatment – it thus appears to be the case that the heterogeneous pattern of sign-up and participation among control group students across the endogenous strata does not carry over to academic achievement.

4 Effects of Intervention 2

In this section, I present the results of Intervention 2 – i.e., the reminder letter with information on the past evaluation of the course – which I conducted among students who signed up for the remedial math course.

Main effects. Table 9 reports the main effects of Intervention 2. The bottom row shows that 90% of control group students who signed up for the course go on to participate in the first tutorial, implying an attrition of 10 pp. Sending students social information about the helpfulness of the course increases participation by 2.8 pp, independent of the exact specification ($p = 0.293$ in Column 3). The salience treatment also increases participation by about 2.6 pp ($p = 0.407$ in Column 3), indicating that it is the reminder letter itself, and not the social information that leads to an increase in participation in the first tutorial. These estimates imply a substantial reduction in attrition of nearly 30%; however, they are not estimated precisely.

Next, I look at average participation in the math course across all tutorials (Columns 4 to 6). Among control group students, the average participation rate is 79%, leaving substantially more room for improvement. However, the effects on participation in the first tutorial do not translate into a higher participation across all tutorials. Students who receive the social information treatment increase their participation rate by 1.3 to 1.6 pp ($p = 0.658 - 0.582$), while students that receive the salience treatment decrease their participation rate by 1.8 to 2.4 pp ($p = 0.612 - 0.486$). A reason for this could be that students learn over time that the course is not as helpful as the reminder letters suggested.

Overall, these results suggest that sending reminder letters, with and without social information on the helpfulness of the course, to students who already signed up for the course, does, on average, not lead to a sustained change in participation rates.

Heterogeneity. In Table A.7, I investigate whether the average treatment effects presented above hide heterogeneous effects along the pre-registered dimensions (first university and sex). Across both dimensions, I find little to no evidence for heterogeneous treatment effects.[22] Similar to the invitation letter intervention, I also tried to employ endogenous stratification. However, since already 90% of control group students participate in the first tutorial, the predictive model based on the pre-registered covariates did not perform well and produced very little heterogeneity in estimated participation probabilities. To me, this indicates that among students who initially signed up for the course, reasons other than those that I can capture with my covariates drive the participation decision.

[22] For the salience treatment, the estimates suggest a very large interaction between treatment and the first university dummy (about 20 pp). However, this differential treatment effect is driven by a large negative treatment effect of about 19 pp among students who have already studied at this or any other university. Since this subgroup consists only of 24 students, these estimates should be interpreted with great caution.

5 Discussion and conclusions

In this section, I conclude the paper by discussing my key findings and their implications for future research, policy, and practice.

The **first key result** is that both social information interventions in this paper have no significant overall effect on students' decision to sign-up for and participate in the remedial math course. While my estimates are consistent with findings from previous social information interventions in higher education (Silva and John, 2017; Page et al., 2019; Neckermann et al., 2022), the null effects are not estimated very precisely. This raises the questions as to what can be learned from these results, in particular in comparison to the positive effects of social information that have been reported for other contexts.

To provide more evidence on this, in Figure 4, I compare the relative (Panel a) and standardized (Panel b) effect size of my most precise social information effects from Tables 4 and 9 to effect sizes from meta-analyses on the effects of social references, reminders, descriptive norms, and education interventions. This provides the following insights: First, ignoring the CIs, almost all my point estimates fall well below the effect sizes reported in the literature. Second, taking the 95% CIs of my coefficients into account, I can rule out relative (standardized) effects sizes greater than roughly 10% (.20 SD), and thus the effects that are reported for social cues by Hummel and Maedche (2019), Mertens et al. (2022), and DellaVigna and Linos (2022, journal sample), effects of descriptive norms on behavioral outcomes in field experiments (Rhodes, Shulman and McClaran, 2020), effects of reminders as reported by Mertens et al. (2022) and DellaVigna and Linos (2022, journal sample), as well as effect sizes that would be considered large for an education intervention (Kraft, 2020). Third, the figure also shows that the ex-ante power of my study reported in Table 3 was large enough to detect those kinds of effects. Fourth, as Hummel and Maedche (2019) and Mertens et al. (2022) discuss, their estimates may be partly driven by publication bias, and the true effects of nudges are likely lower. This is supported by DellaVigna and Linos (2022), who show that the relative effect size of nudges published in journals is about 33.4%, while it is only 8.0% for interventions conducted by nudge units. According to them, about 60 to 70% of this difference is due to publication bias, and the remainder due to nudge characteristics, suggesting that the relative effect size of nudges in academic journals without publication bias could be around 15%; an effect size that I can still exclude based on my CIs. Fourth, however, I cannot rule out the effects sizes that DellaVigna and Linos (2022) report for their nudge unit samples, the effects of descriptive norms on behavior that Rhodes, Shulman and McClaran (2020) report (overall and for college-aged participants), or effects that would be considered small to medium in an educational context (Kraft, 2020).

Taken together, my results – and those from other studies in higher education – therefore suggest that future research on the effects of social information in higher education should not expect to find the kind of effect sizes that have previously been found in other contexts, and that power analyses for average treatment effects should be conducted with comparatively small effect sizes in mind.

Related to the absence of an overall effect, but in contrast to previous results on social information in higher education, my **second key result** is the presence of substantial heterogeneities in the effects of Intervention 1. This is consistent with the notion that most treatment effects are likely to be heterogeneous (Bryan, Tipton and Yeager, 2021; Smith, 2022), which is, for instance, often reported for social comparison interventions (Allcott, 2011; Byrne, Nauze and Martin, 2018; Brent et al., 2020; Brade, Himmler and Jäckle, 2021). However, the heterogeneous effects I report are subject to an important limitation that should be kept in mind when interpreting the results. I pre-registered the analyses as exploratory and not as confirmatory, and I did not perform power calculations for them. Consequentially, the standard errors are in many cases rather large – in particular when it comes to the effects on academic achievements – increasing the likelihood for false negatives; this might, e.g., be the case for the heterogeneity by students' sex or the question as to whether the heterogeneity across endogenous strata carries over to academic achievements.

Nevertheless, I believe that the specific results of my heterogeneity analyses still provide some important insights, which brings me to the **third key result**: I find that control students who enroll late in their study program, i.e., within the last month before the beginning of the remedial math course, are 16 pp less likely to participate in the remedial math course and their overall academic performance is 0.26 standard deviations lower compared to students who enroll early. There are at least three reasons why late enrolling students show lower academic achievements and may thus be in need of supportive measures: i) Late enrollment can be due to a late offer of a place in the program, which is usually negatively related to students' high school grades. ii) Students may enroll late because they did not receive an offer from their preferred program or university, leaving them with the choice for which they are likely less motivated. iii) Late enrollment could also be the results of procrastination tendencies, which are typically negatively correlated with academic performance (Kim and Seo, 2015). However, identifying and separating these underlying mechanisms is a question for future research.

My results further show that both the social information and the salience treatment in Intervention 1 are able to almost completely close the gap in remedial course participation between early and late enrolling students, and, subsequently, also in their academic achievements. A plausible explanation is that the remedial math course helps with the academic and social integration of these students. This raises the question as to why late enrolling students are less likely to participate in the remedial math course and what policy makers or practitioners can do to address this. First, my results suggest that the salience of the course may be too low among these students and that additional information, such as my invitations letters, can mitigate this problem at low cost. Second, the beginning of the semester and the remedial math course are directly adjacent to the period in which students enroll in their study program, and some students enroll even after the start of the semester. For students who enroll late, organizational matters or external constraints may therefore make participation in the course less of a priority, difficult, or even impossible. Policy makers should therefore also work to ensure that the enrollment process and the introductory phase of a degree program, including voluntary remedial courses, are designed such that all students can participate easily.

My **last key result** is that the effect of the social information about the past sign-up rate in Intervention 1 – but not the salience treatment – is asymmetric with respect to students' predicted ex-ante sign-up probability. I argue that this measure might proxy students' initial beliefs about the behavior of others, which has been shown to be a source of heterogeneity in studies on social information (cf. Coffman, Featherstone and Kessler, 2017; Cantoni et al., 2019). Such asymmetric behavioral responses may also provide an explanation for the absence of an overall effect and finding ways to identify such asymmetries – for instance, by using measures or proxies of individuals' beliefs about the behavior of others – is thus an important building block for future research. Although the evidence is only tentative, because the effects are not estimated precisely, I further find that this heterogeneity in course participation does not translate into respective changes in academic achievements. For one, this shows the importance of investigating whether downstream outcomes are affected in the desired way. Second, similar to prior research on remediation (Boatman and Long, 2018), it suggests that participation in voluntary remedial math courses itself is likely to have heterogeneous effects that depend on the complier population. It should therefore probably not be a priority for policy makers to persuade everyone to attend, for example, by making the course mandatory. In particular, because doing so may run the risk of introducing discouragement effects that have so far been prevented by the voluntary nature of the course.

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Tables and figures

Table 1: Correlates of remedial math course sign-up and participation – control group of Intervention 1

	Sign-up		Part. 1st tutorial		Avgerage part.	
	(1)	(2)	(3)	(4)	(5)	(6)
Female	0.012	0.015	-0.079	-0.073	-0.042	-0.038
	(0.052)	(0.052)	(0.057)	(0.058)	(0.053)	(0.054)
Age	-0.035**	0.192	-0.020	0.211	-0.015	0.115
	(0.014)	(0.131)	(0.015)	(0.153)	(0.014)	(0.143)
High school GPA (std.)	-0.035	-0.032	0.017	0.023	0.030	0.034
	(0.029)	(0.031)	(0.030)	(0.032)	(0.027)	(0.029)
Fresh high school degree	0.004	0.041	0.043	0.080	-0.011	0.009
	(0.060)	(0.062)	(0.064)	(0.067)	(0.058)	(0.061)
High school degree Abitur	-0.010	0.006	0.074	0.088	0.105	0.110
	(0.071)	(0.074)	(0.075)	(0.077)	(0.065)	(0.068)
First university	0.224***	0.227***	0.270***	0.272***	0.285***	0.285***
	(0.065)	(0.065)	(0.068)	(0.068)	(0.061)	(0.061)
Last month	-0.090	-0.085	-0.139**	-0.134**	-0.163***	-0.160***
	(0.055)	(0.055)	(0.059)	(0.059)	(0.052)	(0.053)
Distance (in 100 km)	-0.004	-0.010	-0.003	-0.006	-0.002	-0.000
	(0.003)	(0.010)	(0.002)	(0.010)	(0.002)	(0.009)
Winter term	0.029	0.028	-0.007	-0.007	0.003	0.003
	(0.056)	(0.056)	(0.059)	(0.059)	(0.055)	(0.055)
Age*age		-0.005*		-0.005		-0.003
		(0.003)		(0.003)		(0.003)
HS GPA (std.)*HS GPA (std.)		0.002		-0.005		-0.005
		(0.024)		(0.025)		(0.022)
Distance (in 100 km)*distance (in 100 km)		0.000		0.000		-0.000
		(0.000)		(0.000)		(0.000)
P-value F-test place of HS degree FE	[0.001]	[0.002]	[0.002]	[0.005]	[0.003]	[0.008]
P-value F-test study program FE	[0.439]	[0.573]	[0.398]	[0.593]	[0.664]	[0.757]
N	296	296	296	296	296	296
R2	0.17	0.18	0.18	0.19	0.20	0.20
Unadjusted mean of dep. var.	0.76	0.76	0.70	0.70	0.60	0.60

Note: The high school GPA is standardized and inverse-scaled. Originally, in the German system 1.0 is the best, and 4.0 the worst high school GPA. Fresh high school degree indicates if the high school degree was obtained within the last year before the beginning of the first semester. First university indicates if this is the first semester at any university. Last month indicates whether the letter in Intervention 1 was sent within the last month before the beginning of the remedial math course, and thus also captures students' timing of enrollment. Distance in 100 km (rescaled for easier interpretation) is the distance over which the invitation letter in Intervention 1 would've been sent, which coincides with the distance to the place of residence at the timing of enrollment. Predictive margins for the place of HS degree FE and the study program FE are shown in Table A.2. Table A.1 provides further details on all variables. *Outcome variables:* sign-up for remedial math course, participation in first tutorial of remedial math course, and average participation is the share of tutorials a student participated in. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 2: REGRESSION OF ACADEMIC ACHIEVEMENT ON REMEDIAL MATH COURSE PARTICIPATION – CONTROL GROUP OF INTERVENTION 1

	Math exam			Overall performance			
	Attempted	Passed	Grade	Index	Credits	Dropout	GPA
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
a) First semester							
Average participation	0.518***	0.425***	-0.202	0.477***	9.735***	-0.129***	0.042
	(0.066)	(0.066)	(0.192)	(0.160)	(1.774)	(0.045)	(0.133)
High school GPA (std.)	-0.016	0.053*	-0.417***	-0.069	1.851**	0.026	-0.227***
	(0.029)	(0.030)	(0.072)	(0.075)	(0.856)	(0.020)	(0.053)
Controls	yes	yes	yes	yes	yes	yes	yes
N	296	296	195	296	296	296	197
R^2	0.34	0.29	0.41	0.12	0.25	0.12	0.26
a) First year							
Average participation	0.491***	0.449***	-0.264	0.590***	17.730***	-0.172***	-0.084
	(0.064)	(0.068)	(0.180)	(0.151)	(3.200)	(0.057)	(0.108)
High school GPA (std.)	-0.019	0.037	-0.404***	0.044	3.188**	0.012	-0.205***
	(0.029)	(0.030)	(0.067)	(0.065)	(1.498)	(0.023)	(0.046)
Controls	yes	yes	yes	yes	yes	yes	yes
N	296	296	209	296	296	296	224
R^2	0.34	0.30	0.37	0.19	0.27	0.15	0.25

Note: Average participation is the share of tutorials a student participated in. The high school GPA is standardized and inverse-scaled. Originally, in the German system 1.0 is the best, and 4.0 the worst high school GPA. *Outcome variables:* math exam attempted, math exam passed, grade in the math exam includes failing grades and is only observed for students who attempted the math exam at least once (highest passing grade is 1.0; lowest passing grade is 4.0; failing grade is 5.0), obtained credits, dropout indicates if a student dropped out of their study program, grade point average includes passing grades only and is unobserved for students who have not obtained a passing grade yet (highest passing grade is 1.0, lowest passing grade is 4.0); *controls:* other variables included in Columns 1, 3, and 5 of Table 1, i.e., female dummy, age, fresh HS degree dummy, HS degree abitur dummy, first university dummy, last month dummy, distance, winter term dummy, place of HS degree FE, and study program FE. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 3: MINIMUM DETECTABLE EFFECT SIZES (= d; α = 0.05)

Intervention 1					Intervention 2				
a) *Sign-up*									
Control mean = 0.683, SD = 0.468; R^2=0.211									
$1-\beta$	N	d in SD	d in pp	d in %					
0.6	600	0.16	7.53	11.03					
0.8	600	0.20	9.54	13.98					
b) *Participation 1st tutorial*									
Control mean = 0.635, SD = 0.484, R^2=0.218					Control mean = 0.923, SD = 0.269; R^2=0.101				
$1-\beta$	N	d in SD	d in pp	d in %	$1-\beta$	N	d in SD	d in pp	d in %
0.6	600	0.16	7.74	12.20	0.6	400	0.21	5.69	6.17
0.8	600	0.20	9.82	15.48	0.8	400	0.27	7.17	7.77
c) *Average participation*									
Control mean = 0.533, SD = 0.445; R^2=0.167					Control mean = 0.820, SD = 0.292; R^2=0.133				
$1-\beta$	N	d in SD	d in pp	d in %	$1-\beta$	N	d in SD	d in pp	d in %
0.6	600	0.17	7.33	13.75	0.6	400	0.21	6.03	7.36
0.8	600	0.21	9.29	17.42	0.8	400	0.26	7.64	9.32

Note: The power calculations were performed after the results for the summer cohort were available. The depicted control group means and standard deviations (SD) are therefore from the summer cohort. The assumed R^2 are based on control group OLS regressions of the outcome variables on the covariates that were pre-registered for stratification in the field experiment with the winter cohort. N is based on the number of observations that I expected to gather for the comparisons of control versus the social information treatments after pooling observations from both cohorts. Power calculations were performed with Optimal Design (Spybrook et al., 2011).

Table 4: EFFECT OF INTERVENTION 1 (INFORMATION ON PAST SIGN-UP RATE)

	Sign-up			Participation 1st tutorial			Average participation		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
S1: Salience	-0.030	-0.028	-0.031	-0.044	-0.041	-0.045	-0.034	-0.029	-0.036
	(0.040)	(0.038)	(0.036)	(0.044)	(0.041)	(0.039)	(0.041)	(0.038)	(0.037)
S2: Social information	0.015	0.024	0.021	0.003	0.011	0.009	0.000	0.008	0.006
	(0.034)	(0.032)	(0.031)	(0.037)	(0.035)	(0.034)	(0.034)	(0.032)	(0.031)
Strata	yes	yes	yes	yes	yes	yes	yes	yes	yes
Controls	no	yes	lasso	no	yes	lasso	no	yes	lasso
N	789	789	789	789	789	789	789	789	789
Control mean	0.76	0.76	0.76	0.70	0.70	0.70	0.60	0.60	0.60
(SD)	(0.43)	(0.43)	(0.43)	(0.46)	(0.46)	(0.46)	(0.43)	(0.43)	(0.43)

Note: Outcome variables: sign-up for remedial math course, participation in first tutorial of remedial math course, and average participation is the share of tutorials a student participated in; *strata:* study program FE, winter term dummy, interaction between study program FE and winter term dummy, and invitation letter date FE; *controls:* first university and female dummies, age, HS GPA, fresh HS degree dummy, HS degree abitur dummy, place of HS degree dummies, and the distance over which the letter was sent; the double-post LASSO specification considers all controls as well as a dummy if sign-up took place before the letter was sent. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 5: EFFECT OF INTERVENTION 1 (INFORMATION ON PAST SIGN-UP RATE) – BY TIMING OF ENROLLMENT

	Sign-up		Part. 1st tutorial		Avgerage part.	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel a)						
S1: Salience	-0.060	-0.051	-0.075	-0.064	-0.083*	-0.072
	(0.048)	(0.045)	(0.052)	(0.048)	(0.049)	(0.045)
S2: Social information	-0.003	0.004	-0.026	-0.019	-0.050	-0.045
	(0.041)	(0.039)	(0.044)	(0.042)	(0.041)	(0.039)
Last month	-0.091	-0.093*	-0.140**	-0.132**	-0.168***	-0.157***
	(0.056)	(0.054)	(0.060)	(0.058)	(0.054)	(0.052)
S1*last month	0.086	0.072	0.090	0.072	0.148*	0.136*
	(0.087)	(0.085)	(0.094)	(0.089)	(0.087)	(0.082)
S2*last month	0.053	0.061	0.090	0.100	0.160**	0.172**
	(0.077)	(0.071)	(0.084)	(0.077)	(0.076)	(0.069)
Strata	yes	yes	yes	yes	yes	yes
Controls	no	yes	no	yes	no	yes
N	789	789	789	789	789	789
Panel b)						
S1: Salience + S2: Soc. info.	-0.023	-0.016	-0.043	-0.035	-0.062*	-0.054
	(0.036)	(0.034)	(0.039)	(0.037)	(0.037)	(0.035)
Last month	-0.091	-0.093*	-0.140**	-0.132**	-0.168***	-0.157***
	(0.056)	(0.054)	(0.060)	(0.058)	(0.054)	(0.052)
(S1+S2)*last month	0.065	0.065	0.089	0.089	0.155**	0.158**
	(0.069)	(0.066)	(0.075)	(0.071)	(0.069)	(0.064)
(S1+S2)+(S1+S2)*last month	0.042	0.049	0.046	0.054	0.094	0.104*
	(0.059)	(0.056)	(0.064)	(0.061)	(0.058)	(0.054)
Strata	yes	yes	yes	yes	yes	yes
Controls	no	yes	no	yes	no	yes
N	789	789	789	789	789	789

Note: Last month indicates whether the invitation letter was sent within the last month before the beginning of the remedial math course. Panel b) reports results for pooling the salience and the social information treatment. *Outcome variables:* sign-up for remedial math course, participation in first tutorial of remedial math course, and average participation is the share of tutorials a student participated in; *strata:* study program FE, winter term dummy, and interaction between study program FE and winter term dummy; *controls:* first university and female dummies, age, HS GPA, fresh HS degree dummy, HS degree abitur dummy, place of HS degree dummies, and the distance over which the letter was sent. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 6: EFFECT OF INTERVENTION 1 (INFORMATION ON PAST SIGN-UP RATE) – BY ENDOGENOUS STRATA

	Tercile	Sign-up		Part. 1st tutorial		Average part.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
S1: Salience	Lowest	-0.021	-0.011	-0.077	-0.069	-0.035	-0.030
		(0.076)	(0.074)	(0.080)	(0.076)	(0.073)	(0.070)
	Middle	-0.016	-0.032	0.009	-0.007	0.014	-0.003
		(0.067)	(0.062)	(0.073)	(0.068)	(0.070)	(0.066)
	Highest	-0.058	-0.025	-0.069	-0.028	-0.085	-0.041
		(0.055)	(0.056)	(0.065)	(0.065)	(0.061)	(0.060)
S2: Social information	Lowest	0.017	0.051	-0.021	0.019	-0.039	-0.001
		(0.067)	(0.062)	(0.070)	(0.064)	(0.064)	(0.059)
	Middle	0.104**	0.095*	0.128**	0.118**	0.104*	0.088*
		(0.051)	(0.049)	(0.058)	(0.056)	(0.054)	(0.051)
	Highest	-0.096*	-0.066	-0.123**	-0.096*	-0.089	-0.061
		(0.053)	(0.051)	(0.061)	(0.058)	(0.057)	(0.055)
Strata		yes	yes	yes	yes	yes	yes
Controls		no	yes	no	yes	no	yes
N		789	789	789	789	789	789
S1: P-value F-test int. term		0.862	0.976	0.649	0.824	0.559	0.908
S2: P-value F-test int. term		0.026	0.065	0.011	0.032	0.040	0.136

Note: The table depicts treatment effect estimates for the three endogenous strata based on Equation 4. The endogenous strata group students into terciles of the predicted sign-up probability (see Section 3.2). F-tests in the bottom rows test the hypothesis that all interaction terms between the respective treatment indicator and the endogenous strata, i.e., α_2 and α_4 in Equation 4, are equal to zero. *Outcome variables:* sign-up for remedial math course, participation in first tutorial of remedial math course, and average participation is the share of tutorials a student participated in; *strata:* study program FE, winter term dummy, interaction between study program FE and winter term dummy, and invitation letter date FE; *controls:* first university and female dummies, age, HS GPA, fresh HS degree dummy, HS degree abitur dummy, place of HS degree dummies, and the distance over which the letter was sent. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 7: EFFECT OF INTERVENTION 1 (INFORMATION ON PAST SIGN-UP RATE) ON PERFORMANCE IN MATH EXAM – BY TIMING OF ENROLLMENT

	First semester			First year		
	Attempted	Passed	Grade	Attempted	Passed	Grade
	(1)	(2)	(3)	(4)	(5)	(6)
S1: Salience + S2: Soc. info.	-0.048	-0.018	-0.005	-0.069*	-0.062	0.043
	(0.040)	(0.043)	(0.095)	(0.038)	(0.042)	(0.086)
Last month	-0.139**	-0.124**	0.201	-0.170***	-0.152***	0.119
	(0.058)	(0.058)	(0.144)	(0.057)	(0.058)	(0.130)
(S1+S2)*last month	0.160**	0.122*	-0.106	0.184***	0.128*	0.028
	(0.070)	(0.073)	(0.179)	(0.069)	(0.073)	(0.164)
(S1+S2)+(S1+S2)*last month	0.112*	0.105*	-0.110	0.115**	0.065	0.071
	(0.059)	(0.060)	(0.153)	(0.058)	(0.061)	(0.142)
Strata	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
N	789	789	519	789	789	549

Note: Last month indicates whether the invitation letter was sent within the last month before the beginning of the remedial math course. *Outcome variables:* math exam attempted, math exam passed, grade in the math exam includes failing grades and is only observed for students who attempted the math exam at least once (highest passing grade is 1.0; lowest passing grade is 4.0; failing grade is 5.0); *strata:* study program FE, winter term dummy, and interaction between study program FE and winter term dummy; *controls:* first university and female dummies, age, HS GPA, fresh HS degree dummy, HS degree abitur dummy, place of HS degree dummies, and the distance over which the letter was sent. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 8: EFFECT OF INTERVENTION 1 (INFORMATION ON PAST SIGN-UP RATE) ON OVERALL PERFORMANCE – BY TIMING OF ENROLLMENT

	First semester				First year			
	Index	Credits	Dropout	GPA	Index	Credits	Dropout	GPA
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
S1: Salience + S2: Soc. info.	-0.059	0.187	0.014	0.067	-0.118	-0.550	0.046	0.057
	(0.083)	(1.120)	(0.023)	(0.059)	(0.085)	(1.977)	(0.031)	(0.054)
Last month	-0.256*	-4.226***	0.062	0.096	-0.257**	-7.174***	0.071	0.092
	(0.141)	(1.582)	(0.039)	(0.098)	(0.129)	(2.731)	(0.048)	(0.080)
(S1+S2)*last month	0.361**	3.821*	-0.091**	-0.169	0.349**	6.170*	-0.112*	-0.145
	(0.159)	(1.970)	(0.044)	(0.119)	(0.158)	(3.461)	(0.058)	(0.102)
(S1+S2)+(S1+S2)*last month	0.303**	4.008**	-0.077**	-0.102	0.231*	5.620*	-0.067	-0.088
	(0.136)	(1.647)	(0.038)	(0.105)	(0.133)	(2.867)	(0.049)	(0.088)
Strata	yes	yes	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes	yes	yes
N	789	789	789	550	789	789	789	596

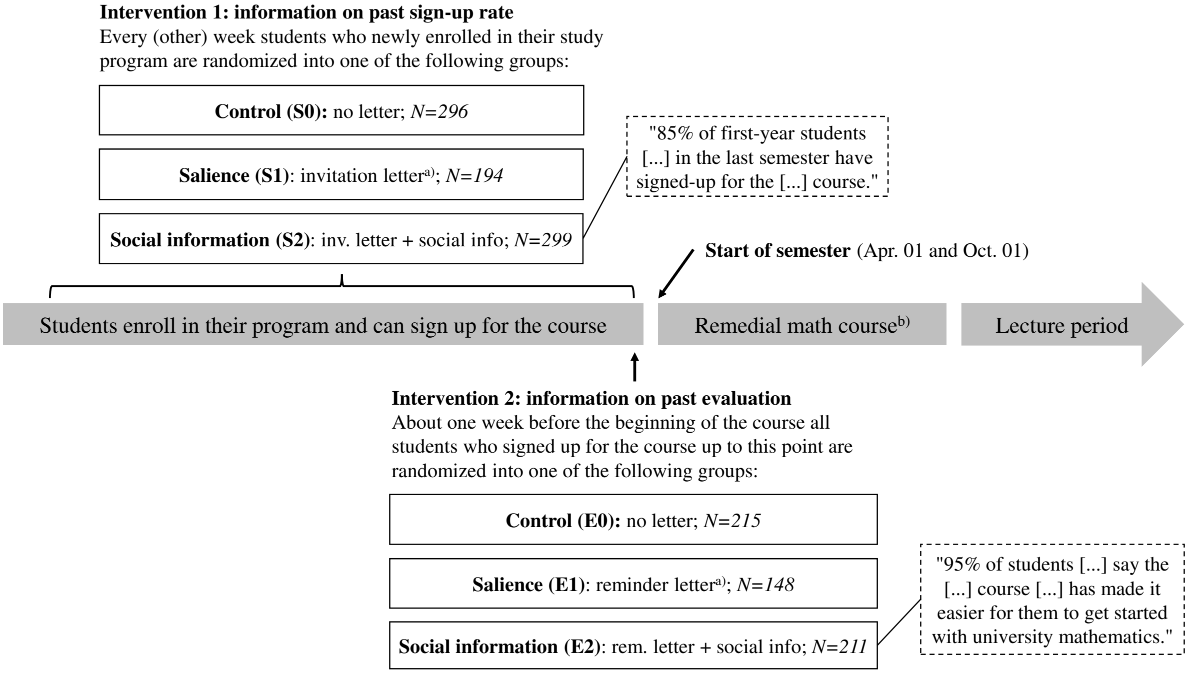
Note: Last month indicates whether the invitation letter was sent within the last month before the beginning of the remedial math course. *Outcome variables:* the index is the standardized inverse-covariance weighted average of the three overall performance measures (following Anderson (2008) and using the Stata program by Schwab et al. (2020)), obtained credits, dropout indicates if a student dropped out of their study program, grade point average includes passing grades only and is unobserved for students who have not obtained a passing grade yet (highest passing grade is 1.0, lowest passing grade is 4.0); *strata:* study program FE, winter term dummy, and interaction between study program FE and winter term dummy; *controls:* first university and female dummies, age, HS GPA, fresh HS degree dummy, HS degree abitur dummy, place of HS degree dummies, and the distance over which the letter was sent. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 9: EFFECT OF INTERVENTION 2 (INFORMATION ON PAST EVALUATION)

	Participation 1st tutorial			Average participation		
	(1)	(2)	(3)	(4)	(5)	(6)
E1: Salience	0.027	0.023	0.026	-0.018	-0.024	-0.019
	(0.033)	(0.032)	(0.032)	(0.036)	(0.034)	(0.034)
E2: Social information	0.028	0.028	0.028	0.013	0.016	0.014
	(0.027)	(0.027)	(0.026)	(0.030)	(0.029)	(0.028)
Strata	yes	yes	yes	yes	yes	yes
Controls	no	yes	lasso	no	yes	lasso
N	574	574	574	574	574	574
Control mean	0.90	0.90	0.90	0.79	0.79	0.79
(SD)	(0.30)	(0.30)	(0.30)	(0.32)	(0.32)	(0.32)

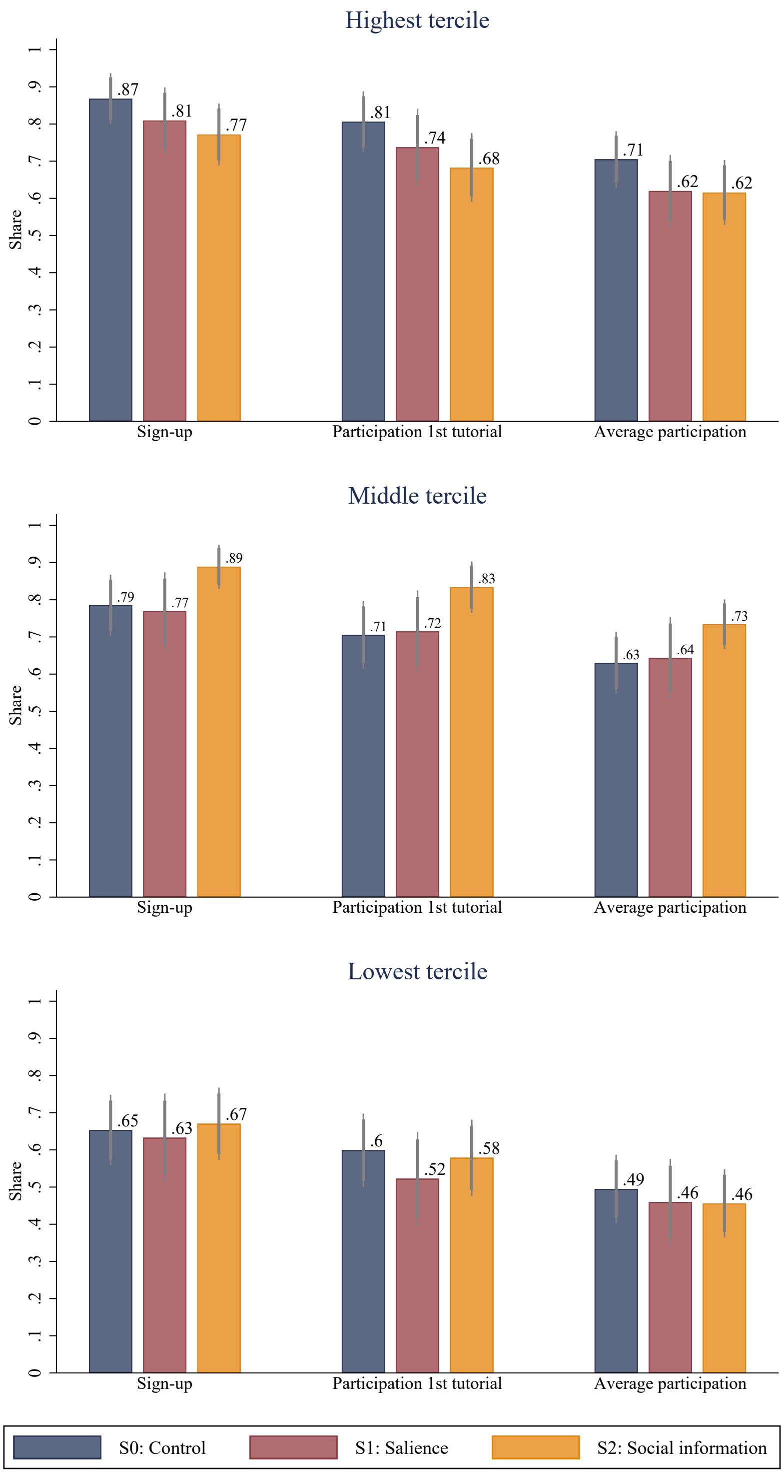
Note: *Outcome variables:* participation in first tutorial of remedial math course and average participation is the share of tutorials a student participated in; *strata:* study program FE, winter term dummy, interaction between study program FE and winter term dummy, and Intervention 1 treatment status FE; *controls:* first university and female dummies, age, HS GPA, fresh HS degree dummy, HS degree abitur dummy, place of HS degree dummies, and the distance over which the letter was sent; the double-post LASSO specification considers all controls. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Figure 1: TIMELINE AND EXPERIMENTAL DESIGN



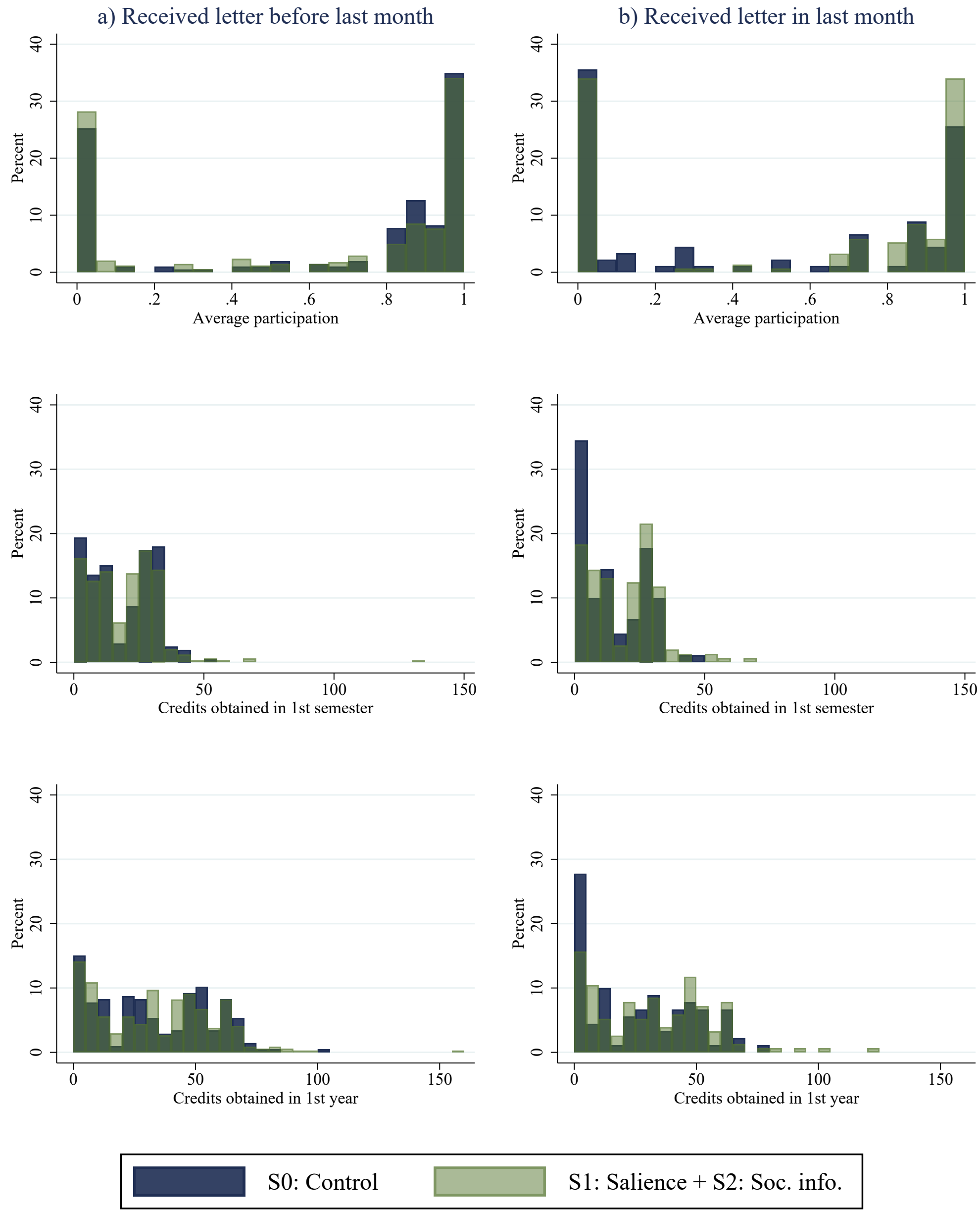
Note: a) As described in Section 2 the experimental groups who receive an invitation or reminder letter without the social information (**S1** and **E1**) are only included in the winter cohort. *b*) Additional information on the timeline and structure of the remedial math course itself is shown in Figure A.1. The full letters are shown in Figures A.2 and A.3 and the exact timing of the invitation letters in Intervention 1 and the respective number of observations are reported in Table B.2.

Figure 2: EFFECT OF INTERVENTION 1 (INFORMATION ON PAST SIGN-UP RATE) – BY ENDOGENOUS STRATA



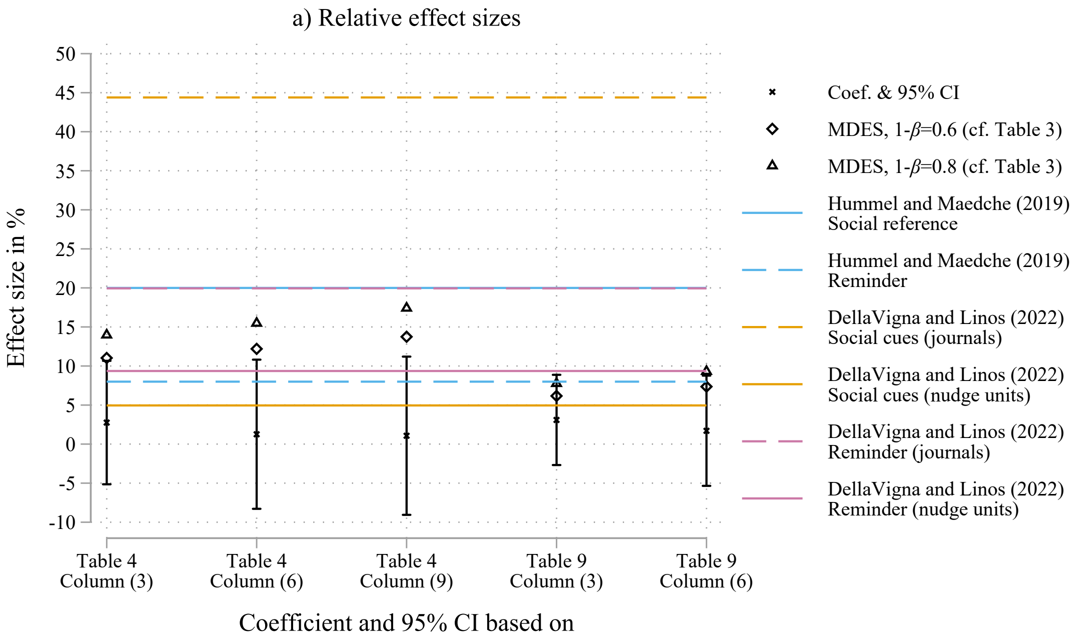
Note: The figure depicts estimates for the three endogenous strata based on Equation 4. The endogenous strata group students into terciles of the predicted sign-up probability (see Section 3.2). *Outcome variables:* sign-up for remedial math course, participation in first tutorial of remedial math course, and average participation is the share of tutorials a student participated in; *strata:* study program FE, winter term dummy, interaction between study program FE and winter term dummy, and invitation letter date FE. 90% (thick) and 95% (thin) confidence intervals based on robust standard errrors are shown.

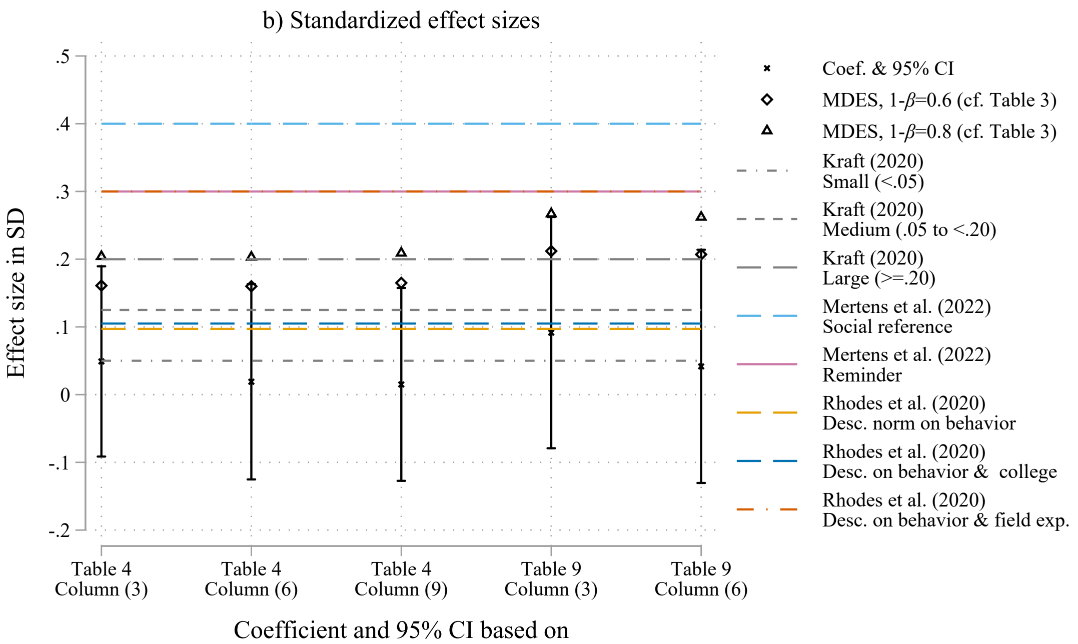
Figure 3: HISTOGRAMS BY TIMING OF ENROLLMENT (INTERVENTION 1: INFORMATION ON PAST SIGN-UP RATE)



Note: Last month indicates whether the invitation letter was sent within the last month before the beginning of the remedial math course. The histograms of the control group (dark blue) are overlaid by the distribution of the combined treatment group (transparent green). Histograms start at 0 and have a binwidth of 0.05 (average participation) and 5 (obtained credits).

Figure 4: COMPARISON OF SOCIAL INFORMATION EFFECTS TO META-ANALYTIC ESTIMATES





Note: The relative effect sizes in Panel a) are percentage changes between the dependent variable of the treatment group and the control group. Relative effect sizes for DellaVigna and Linos (2022) are calculated based on the information on control group take-up rates reported in Appendix Table A.II. Standardized effects of my estimates in Panel b) are based on the control group standard deviations of the dependent variables, which is also referred to as Glass' Δ. Kraft (2020) reports effects sizes for education interventions in terms of standardized achievement outcomes and Mertens et al. (2022) and Rhodes, Shulman and McClaran (2020) report effects sizes in terms of Cohen's *d*.

Appendix

A Additional tables and figures

Table A.1: DESCRIPTION OF VARIABLES

Variable	Description
Treatment Variables	
Salience (S1 or E1)	Random assignment to the treatment group that receives an invitation (Intervention 1) or a reminder (Intervention 2) letter without the social information. Treatments only included in the winter cohort.
Social information (S2 or E2)	Random assignment to the treatment group that receives an invitation letter with information on a past sign up rate (Intervention 1) or a reminder letter with information on past evaluation (Intervention 2).
Stratification Variables	
Study program	BA=Business Administration, BIS=Business and Information Systems, BHRE=Business and Human Resource Education, E=Economics, 2SE=Two-Subject Economics.
Winter term	Dummy for the second cohort.
Invitation letter date	Date at which the invitation letter was sent to a student (see Table B.2). Only included in analyses of the effects of Intervention 1.
Intervention 1 treatment status	Indicators for the treatment status in Intervention 1, such that the two randomizations are orthogonal to each other. Only included in analyses of the effects of Intervention 2.
Control Variables	
First university$^{a)}$	Indicates if this is the first semester at any university.
Female$^{a)}$	Indicator for being female.
Age	Age in years at the beginning of the first semester.
HS GPA	Final high school grade point average (1.0=highest, 4.0=lowest).
Fresh HS degree	Indicates if the high school degree was obtained within the last year before the beginning of the first semester.
HS degree Abitur	Indicator for a general track degree ("Abitur"); reference category includes vocational track degree ("Fachhochschulreife") and students who hold other degrees.
Place of HS degree	NI=Lower Saxony, NW=North Rhine-Westphalia, HE=Hesse, other=another federal state in Germany, and abroad.
Distance letter$^{b)}$	Distance over which the letter was sent (in kilometers).
Sign-up before letter$^{c)}$	Indicates if a student signed up for the remedial math course before the letter in Intervention 1 was sent to them, or could theoretically be sent to them in case of the control group.
Last month	Indicates whether the letter in Intervention 1 was sent within the last month before the beginning of the remedial math course.
Outcome Variables	
Sign-up	Indicates if a student signed up for the remedial math course.
Participation first tutorial	Indicates if a student participated in the first tutorial of the remedial math course.
Average participation	Share of tutorials that a student participated in.
Math attempted	Indicates whether a student attempted the math exam by the end of the first semester/year.
Math passed	Indicates whether a student passed the math exam by the end of the first semester/year.
Math grade	Grade in the math exam by the end of the first semester/year including failing grades (1.0 = highest and 4.0 = lowest passing grade, 5.0 = failing grade). Only observed if the exam was attempted at least once.
Credits	Number of credits obtained in the first semester/year.
Dropout	Indicates whether a student dropped out of their study program in the first semester/year.
GPA	Grade point average at end of first semester/year (passing grades only; 1.0=highest, 4.0=lowest). Only observed if a student passed at least one graded exam.
Performance index	Standardized inverse-covariance weighted average of credits, dropout, and GPA in the first semester/year (following Anderson (2008) and using the Stata program by Schwab et al. (2020)).

Note: $^{a)}$ As explained in Section 2, and as it was intended in the pre-registrations, the first university and female dummies were in some cases used during stratification. However, due to the number of observations per cell, they could not be included in most of the randomizations, and I therefore include them with the other controls. $^{b)}$ The distance over which the letter was sent was not listed as a control variable in the pre-registration of the first experiment. $^{c)}$ Sign-up before letter was not listed as a control variable in the pre-registration of either experiment.

Table A.2: PREDICTIVE MARGINS OF CORRELATES OF REMEDIAL MATH COURSE SIGN-UP AND PARTICIPATION – CONTROL GROUP OF INTERVENTION 1

	Sign-up		Part. 1st tutorial		Avgerage part.	
	(1)	(2)	(3)	(4)	(5)	(6)
Place of HS degree						
NI	0.703	0.700	0.661	0.659	0.589	0.589
	(0.034)	(0.034)	(0.035)	(0.035)	(0.032)	(0.033)
NW	0.739	0.743	0.612	0.616	0.471	0.471
	(0.084)	(0.086)	(0.092)	(0.094)	(0.080)	(0.082)
HE	0.785	0.783	0.673	0.673	0.544	0.545
	(0.071)	(0.072)	(0.080)	(0.081)	(0.080)	(0.082)
Other	0.873	0.879	0.799	0.802	0.682	0.681
	(0.045)	(0.045)	(0.052)	(0.052)	(0.049)	(0.049)
Abroad	1.004	1.018	0.971	0.979	0.890	0.890
	(0.074)	(0.081)	(0.079)	(0.086)	(0.088)	(0.097)
P-value F-test	[0.001]	[0.002]	[0.002]	[0.005]	[0.003]	[0.008]
Study program						
BA	0.759	0.758	0.704	0.701	0.594	0.593
	(0.033)	(0.033)	(0.035)	(0.036)	(0.034)	(0.034)
BIS	0.745	0.748	0.669	0.674	0.625	0.629
	(0.070)	(0.071)	(0.076)	(0.076)	(0.065)	(0.066)
BHRE	0.877	0.869	0.809	0.799	0.683	0.676
	(0.074)	(0.076)	(0.078)	(0.080)	(0.067)	(0.068)
E	0.679	0.684	0.589	0.600	0.544	0.550
	(0.075)	(0.080)	(0.081)	(0.087)	(0.072)	(0.079)
2SE	0.754	0.757	0.688	0.692	0.586	0.590
	(0.075)	(0.075)	(0.072)	(0.072)	(0.065)	(0.065)
P-value F-test	[0.439]	[0.573]	[0.398]	[0.593]	[0.664]	[0.757]
N	296	296	296	296	296	296
R2	0.17	0.18	0.18	0.19	0.20	0.20
Unadjusted mean of dep. var.	0.76	0.76	0.70	0.70	0.60	0.60

Note: This table shows the predictive margins for the place of high school degree FE and study program FE that are included in the regressions shown in Table 1. *Outcome variables:* sign-up for remedial math course, participation in first tutorial of remedial math course, and average participation is the share of tutorials a student participated in. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.3: EFFECT OF INTERVENTION 1 (INFORMATION ON PAST SIGN-UP RATE) – HETEROGENEITIES

	Sign-up		Part. 1st tutorial		Avgerage part.	
	(1)	(2)	(3)	(4)	(5)	(6)
First university	0.279***	0.272***	0.313***	0.311***	0.300***	0.320***
	(0.059)	(0.061)	(0.060)	(0.063)	(0.055)	(0.057)
S1*first university	0.027	0.006	0.056	0.027	0.026	0.000
	(0.101)	(0.101)	(0.101)	(0.099)	(0.091)	(0.090)
S2*first university	0.005	-0.012	0.002	-0.014	0.064	0.043
	(0.080)	(0.079)	(0.082)	(0.082)	(0.073)	(0.072)
Female	0.032	0.015	-0.033	-0.061	-0.004	-0.030
	(0.050)	(0.051)	(0.055)	(0.056)	(0.051)	(0.052)
S1*female	-0.017	-0.005	0.106	0.126	0.116	0.136*
	(0.078)	(0.079)	(0.086)	(0.085)	(0.081)	(0.080)
S2*female	-0.015	0.021	0.031	0.061	0.046	0.067
	(0.069)	(0.068)	(0.076)	(0.075)	(0.070)	(0.069)
Last month	-0.082	-0.093*	-0.133**	-0.134**	-0.159***	-0.159***
	(0.054)	(0.054)	(0.058)	(0.058)	(0.053)	(0.053)
S1*last month	0.088	0.071	0.103	0.086	0.165*	0.150*
	(0.085)	(0.085)	(0.091)	(0.089)	(0.085)	(0.082)
S2*last month	0.056	0.061	0.096	0.102	0.168**	0.176**
	(0.072)	(0.071)	(0.078)	(0.077)	(0.071)	(0.069)
Strata	yes	yes	yes	yes	yes	yes
Controls	no	yes	no	yes	no	yes
N	789	789	789	789	789	789

Note: S1=salience treatment, S2=social information treatment. *Outcome variables:* sign-up for remedial math course, participation in first tutorial of remedial math course, and average participation is the share of tutorials a student participated in; *strata:* study program FE, winter term dummy, and interaction between study program FE and winter term dummy; *controls:* age, HS GPA, fresh HS degree dummy, HS degree abitur dummy, place of HS degree dummies, and the distance over which the letter was sent. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.4: EFFECT OF INTERVENTION 1 (INFORMATION ON PAST SIGN-UP RATE) ON PERFORMANCE IN MATH EXAM – BY ENDOGENOUS STRATA

		First semester			First year		
	Tercile	Attempted	Passed	Grade	Attempted	Passed	Grade
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
S1: Salience	Lowest	0.019	-0.032	0.144	-0.001	-0.039	0.114
		(0.073)	(0.076)	(0.224)	(0.072)	(0.075)	(0.199)
	Middle	-0.020	0.008	-0.180	-0.016	-0.025	-0.070
		(0.073)	(0.079)	(0.178)	(0.071)	(0.079)	(0.169)
	Highest	0.074	0.137*	-0.179	0.069	0.080	-0.065
		(0.070)	(0.077)	(0.176)	(0.066)	(0.076)	(0.161)
S2: Social information	Lowest	0.018	-0.047	0.323*	-0.013	-0.063	0.337**
		(0.064)	(0.065)	(0.169)	(0.063)	(0.065)	(0.153)
	Middle	-0.053	-0.055	0.010	-0.045	-0.089	0.111
		(0.065)	(0.069)	(0.144)	(0.062)	(0.068)	(0.134)
	Highest	0.007	0.144**	-0.360**	-0.018	0.036	-0.190
		(0.058)	(0.066)	(0.151)	(0.055)	(0.064)	(0.137)
Strata		yes	yes	yes	yes	yes	yes
Controls		yes	yes	yes	yes	yes	yes
N		789	789	519	789	789	549
S1: P-value F-test int. term		0.637	0.261	0.444	0.632	0.474	0.731
S2: P-value F-test int. term		0.697	0.054	0.009	0.924	0.362	0.033

Note: The table depicts treatment effect estimates for the three endogenous strata based on Equation 4. The endogenous strata group students into terciles of the predicted sign-up probability (see Section 3.2). F-tests in the bottom rows test the hypothesis that all interaction terms between the respective treatment indicator and the endogenous strata, i.e., α_2 and α_4 in Equation 4, are equal to zero. *Outcome variables:* math exam attempted, math exam passed, grade in the math exam includes failing grades and is only observed for students who attempted the math exam at least once (highest passing grade is 1.0; lowest passing grade is 4.0; failing grade is 5.0); *strata:* study program FE, winter term dummy, interaction between study program FE and winter term dummy, and invitation letter date FE; *controls:* first university and female dummies, age, HS GPA, fresh HS degree dummy, HS degree abitur dummy, place of HS degree dummies, and the distance over which the letter was sent. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.5: EFFECT OF INTERVENTION 1 (INFORMATION ON PAST SIGN-UP RATE) ON OVERALL PERFORMANCE – BY ENDOGENOUS STRATA

		First semester				First year			
	Tercile	Index	Credits	Dropout	GPA	Index	Credits	Dropout	GPA
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
S1: Salience	Lowest	-0.062	-0.212	0.014	0.082	-0.045	-1.860	0.005	0.118
		(0.140)	(2.196)	(0.040)	(0.111)	(0.141)	(3.847)	(0.049)	(0.105)
	Middle	0.081	1.649	-0.018	-0.097	0.059	2.916	-0.014	-0.076
		(0.166)	(1.971)	(0.047)	(0.106)	(0.178)	(3.611)	(0.065)	(0.094)
	Highest	0.071	1.793	-0.018	0.100	0.085	2.568	-0.023	-0.007
		(0.159)	(1.769)	(0.045)	(0.106)	(0.161)	(3.363)	(0.061)	(0.095)
S2: Social information	Lowest	0.019	0.978	-0.008	0.147	-0.074	0.646	0.028	0.157
		(0.110)	(1.921)	(0.030)	(0.111)	(0.122)	(3.242)	(0.044)	(0.104)
	Middle	0.072	-0.255	-0.021	-0.010	-0.079	-1.221	0.029	0.008
		(0.144)	(1.716)	(0.040)	(0.100)	(0.152)	(3.102)	(0.056)	(0.085)
	Highest	0.101	4.170**	-0.022	-0.087	0.045	5.166*	0.014	-0.116
		(0.147)	(1.789)	(0.041)	(0.101)	(0.149)	(3.066)	(0.056)	(0.088)
Strata		yes	yes	yes	yes	yes	yes	yes	yes
Controls		yes	yes	yes	yes	yes	yes	yes	yes
N		789	789	789	550	789	789	789	596
S1: P-value F-test int. term		0.738	0.751	0.814	0.340	0.808	0.602	0.928	0.369
S2: P-value F-test int. term		0.900	0.190	0.950	0.272	0.794	0.326	0.975	0.126

Note: The table depicts treatment effect estimates for the three endogenous strata based on Equation 4. The endogenous strata group students into terciles of the predicted sign-up probability (see Section 3.2). F-tests in the bottom rows test the hypothesis that all interaction terms between the respective treatment indicator and the endogenous strata, i.e., α_2 and α_4 in Equation 4, are equal to zero. *Outcome variables:* the index is the standardized inverse-covariance weighted average of the three overall performance measures (following Anderson (2008) and using the Stata program by Schwab et al. (2020)), obtained credits, dropout indicates if a student dropped out of their study program, grade point average includes passing grades only and is unobserved for students who have not obtained a passing grade yet (highest passing grade is 1.0, lowest passing grade is 4.0); *strata:* study program FE, winter term dummy, interaction between study program FE and winter term dummy, and invitation letter date FE; *controls:* first university and female dummies, age, HS GPA, fresh HS degree dummy, HS degree abitur dummy, place of HS degree dummies, and the distance over which the letter was sent. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.6: MEAN OF OUTCOMES OF CONTROL GROUP STUDENTS BY ENDOGENOUS STRATA (INTERVENTION 1: INFORMATION ON PAST SIGN-UP RATE)

	(1)	(2)	(3)
Tercile	Lowest	Middle	Highest
Sign-up	0.621	0.786	0.884
Participation first tutorial	0.563	0.714	0.821
Average participation	0.463	0.632	0.721
Math attempted first semester	0.563	0.704	0.716
Math attempted first year	0.621	0.735	0.768
Math passed first semester	0.408	0.551	0.453
Math passed first year	0.485	0.643	0.589
Math grade first semester	3.490	3.623	3.850
Math grade first year	3.428	3.465	3.653
Performance index first semester	0.106	-0.031	-0.083
Performance index first year	0.113	0.002	-0.125
Credits first semester	16.325	17.607	15.137
Credits first year	30.165	32.852	28.463
Dropout first semester	0.058	0.092	0.105
Dropout first year	0.107	0.173	0.189
GPA first semester	2.876	3.052	3.057
GPA first year	2.840	2.939	3.056

Table A.7: EFFECT OF INTERVENTION 2 (INFORMATION ON PAST EVALUATION) – HETEROGENEITIES

	Part. 1st tutorial		Avgerage part.	
	(1)	(2)	(3)	(4)
First university	0.052	0.059	0.108*	0.137**
	(0.055)	(0.057)	(0.059)	(0.058)
E1*first university	0.172	0.164	0.199*	0.201**
	(0.112)	(0.108)	(0.105)	(0.096)
E2*first university	-0.011	-0.007	0.004	0.017
	(0.072)	(0.070)	(0.078)	(0.075)
Female	-0.026	-0.036	0.060	0.046
	(0.046)	(0.046)	(0.046)	(0.045)
E1*female	-0.016	-0.025	-0.049	-0.047
	(0.068)	(0.068)	(0.069)	(0.067)
E2*female	0.060	0.048	-0.027	-0.047
	(0.058)	(0.058)	(0.062)	(0.060)
Strata	yes	yes	yes	yes
Controls	yes	yes	yes	yes
N	574	574	574	574

Note: E1=salience treatment, E2=social information treatment. *Outcome variables:* participation in first tutorial of remedial math course and average participation is the share of tutorials a student participated in; *strata:* study program FE, winter term dummy, interaction between study program FE and winter term dummy, and Intervention 1 treatment status FE; *controls:* age, HS GPA, fresh HS degree dummy, HS degree abitur dummy, place of HS degree dummies, and the distance over which the letter was sent. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Figure A.1: Timeline and structure of remedial math course

Summer term

Apr. 01:
Start of the semester

Apr. 15:
Start of the lectures

Apr. 03 – 05 and Apr. 08 – 12:
Remedial math course

Day 1	Day 2	Day 3	Day 4	Day 5	Day 6	Day 7	Day 8
8.15 - 10.30 Lecture	8.15 - 10.30 Lecture	8.15 - 10.30 Lecture	8.15 - 10.00 Lecture	8.15 - 10.00 Lecture	8.15 - 10.00 Lecture	8.15 - 10.00 Lecture	8.15 - 10.00 Tutorial
11.00 - 13.00 Tutorial	11.00 - 13.00 Tutorial	11.00 - 13.00 Tutorial	10.30 - 12.30 Tutorial	10.30 - 12.30 Tutorial	10.30 - 12.30 Tutorial	10.30 - 12.30 Tutorial	10.30 - 12.30 Tutorial
14.15 - 16.00 Tutorial	14.15 - 15.45 Tutorial	14.15 - 15.45 Tutorial	13.45 - 15.15 Tutorial	13.45 - 15.15 Tutorial	13.45 - 15.15 Tutorial	13.45 - 15.15 Lecture	13.15 - 13.45 Tutorial

Total: 42.5 hours
Lectures: 15.25 hours
Tutorials: 27.5 hours

Winter term

Oct. 01:
Start of the semester

Oct. 15:
Start of the lectures

Sept. 30 – Oct. 02 and Oct. 07 – 11:
Remedial math course

Day 1	Day 2	Day 3	Day 4	Day 5	Day 6	Day 7	Day 8
8.15 - 10.30 Lecture	8.15 - 10.30 Lecture	8.15 - 10.30 Lecture	8.15 - 10.00 Lecture	8.15 - 10.30 Lecture	8.15 - 10.30 Lecture	8.15 - 10.15 Lecture	8.15 - 10.00 Tutorial
11.00 - 13.00 Tutorial	11.00 - 13.00 Tutorial	11.00 - 13.00 Tutorial	11.00 - 13.00 Tutorial	11.00 - 13.00 Tutorial	11.00 - 13.00 Tutorial	10.30 - 12.30 Tutorial	10.30 - 12.30 Tutorial
14.15 - 16.00 Tutorial	14.15 - 15.45 Tutorial	14.15 - 15.45 Tutorial	14.15 - 15.45 Tutorial	14.15 - 15.45 Tutorial	14.15 - 15.45 Tutorial	13.45 - 15.15 Lecture	13.15 - 13.45 Tutorial

Total: 43.75 hours
Lectures: 16.5 hours
Tutorials: 27.5 hours

Figure A.2: INVITATION LETTER (INTERVENTION 1) – SOCIAL INFORMATION HIGHLIGHTED IN GRAY (TRANSLATION)

University • postbox <number> • <zip code> <place>

Ms. / Mr.
<first name> <last name>
<street> <number>
<zip code> <place of residence>

Organizer of math course

Phone no. <number>
<e-mail>

<place>, <date>

Remedial math course for students of business and economics

Dear Ms./Mr. <last name>,

in order to help you get off to a good start in your studies, we would like to invite you to the remedial math course for students of business and economics. The course provides mathematical knowledge that is required in the mathematics lecture and in numerous other courses.

85% of the first-year students who, like you, were enrolled in a business or economics degree program in the last semester have signed up for the remedial math course.[1] Only a small minority of students does not sign up for the remedial course.

The remedial math course starts on <date> - we look forward to your participation! Please sign up at <website>.

Kind regards,

the remedial math course team

[1] The calculation of the sign-up rate is based on all students who enrolled in <year> for the winter term <year>.

Figure A.3: REMINDER LETTER (INTERVENTION 2) – SOCIAL INFORMATION HIGHLIGHTED IN GRAY (TRANSLATION)

University • postbox <number> • <zip code> <place>

Ms. / Mr.
<first name> <last name>
<street> <number>
<zip code> <place of residence>

Organizer of math course

Phone no. <number>
<e-mail>

<place>, <date>

Remedial math course for students of business and economics

Dear Ms./Mr. <last name>,

you have signed up for the remedial math course. We have therefore already reserved a seat for you and look forward to your participation. The course starts on <date> at <location>.

95% of students who, like you, are enrolled in a business or economics degree program say that the remedial course in mathematics has made it easier for them to get started with university mathematics.[1]

Before the course starts, please inform yourself about the tutorial group you have been assigned to and the room in which your tutorial will take place at <website>.

Kind regards, see you on <date>!

the remedial math course team

[1] The data is taken from a survey among students attending the mathematics lecture, which was conducted in the winter term <year>.

B Randomization and balancing properties

Intervention 1: information on past sign-up rate. In both cohorts, starting five to seven weeks before the beginning of the remedial math course until one week before, I used administrative data on the incoming students provided to me by the university to randomize students into a control and one (summer term) or two (winter term) treatment groups. Randomization was carried out using stratification and re-randomization (Morgan and Rubin, 2012). Each week in the summer term and about every other week in the winter term[23], I randomized within study programs and, if possible, i.e., if there were enough observations in the respective cells, within a dummy variable that indicates if this is students' first semester at any university and a female dummy. If the number of observations in the strata allowed it, I additionally re-randomized up to 5,000 times, keeping the randomization with the best balancing properties with respect to the age, the high school grade point average (GPA), and, if they were not used for stratification, the first university and female dummies. In total, I randomized 789 (208 in the summer and 581 in the winter term) students into the control and treatment groups (Tables B.1 and B.2 show the number of observations by study program and date of the randomization, respectively). Tables B.3 and B.4 show that in both cohorts the samples are well balanced.

Intervention 2: information on past evaluation. About one week before the start of the course (see Figure 1), I randomized all students who signed up for the course up to that point into a control and one (summer term) or two (winter term) treatment groups. Again, I performed the randomization using stratification and re-randomization. Strata were constructed based on study program, the information about the treatment status Intervention 1 – such that the two randomizations are orthogonal to each other – and, whenever possible, based on first university and female dummies. Re-randomization was conducted as before. Overall, 574 (129 in the summer and 445 in the winter term) students were randomized into treatment and control groups (Tables B.5 and B.6 show the respective balancing properties and Table B.1 the number of observations by study program).

Table B.1: NUMBER OF OBSERVATIONS BY STUDY PROGRAM

	Summer Term					Winter Term					
Study program	BA	BIS	BHRE	E	N	BA	BIS	BHRE	E	2SE	N
Intervention 1											
S0: Control	60	18	12	14	104	101	25	18	16	32	192
S1: Salience	-	-	-	-	-	101	26	19	15	33	194
S2: Social information	56	20	13	15	104	103	26	17	17	32	195
N	116	38	25	29	**208**	305	77	54	48	97	**581**
Intervention 2											
E0: Control	40	11	8	6	65	80	19	17	12	22	150
E1: Salience	-	-	-	-	-	79	21	14	14	20	148
E2: Social information	37	12	9	6	64	79	19	14	13	22	147
N	77	23	17	12	**129**	238	59	45	39	64	**445**

Note: BA=Business Administration, BIS=Business Information Systems, BHRE=Business and Human Resource Education, E=Economics, 2SE=Two-Subject Economics.

[23]For the second experiment I moved to a larger interval between randomizations in order to have access to a larger number of observations at each point in time.

Table B.2: NUMBER OF OBSERVATIONS BY TIMING OF INVITATION LETTER (INTERVENTION 1: INFORMATION ON PAST SIGN-UP RATE)

	Summer Term						Winter Term					
Days until course	37	29	23	16	9	N	49	40	28	14	7	N
S0: Control	74	17	7	4	2	104	79	53	44	13	3	192
S1: Salience	-	-	-	-	-	-	84	49	44	13	4	194
S2: Social information	74	16	6	5	3	104	83	50	46	12	4	195
N	148	33	13	9	5	**208**	246	152	134	38	11	**581**

Table B.3: DESCRIPTIVE STATISTICS AND BALANCING PROPERTIES – INTERVENTION 1 (INFORMATION ON PAST SIGN-UP RATE), SUMMER TERM

	(1) S0: Control mean (std. dev.)	(2) S2: Soc. info. coefficient (robust SE)	(3) p-Value
First university	0.510	-0.004	0.955
	(0.502)	(0.070)	
Female	0.298	-0.023	0.712
	(0.460)	(0.061)	
Age	21.654	-0.123	0.736
	(2.625)	(0.364)	
HS GPA	2.520	0.037	0.598
	(0.485)	(0.070)	
Fresh HS degree	0.423	0.008	0.911
	(0.496)	(0.069)	
HS degree Abitur	0.817	0.031	0.552
	(0.388)	(0.052)	
HS degree NI	0.577	-0.072	0.298
	(0.496)	(0.069)	
HS degree NW	0.125	-0.021	0.637
	(0.332)	(0.045)	
HS degree HE	0.096	0.088*	0.068
	(0.296)	(0.048)	
HS degree other	0.183	-0.034	0.509
	(0.388)	(0.051)	
HS degree abroad	0.019	0.039	0.163
	(0.138)	(0.028)	
Distance letter	172.433	-98.836	0.230
	(798.424)	(82.121)	
Sign-up before letter	0.067	-0.010	0.729
	(0.252)	(0.030)	
N	104	104	

Note: Column (1) presents the unadjusted control group means and standard deviations of the covariates. Column (2) presents the estimated coefficients of regressing the covariates on the treatment indicator using Equation 1. Column (3) tests the null hypothesis of no treatment effect. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table B.4: DESCRIPTIVE STATISTICS AND BALANCING PROPERTIES – INTERVENTION 1 (INFORMATION ON PAST SIGN-UP RATE), WINTER TERM

	(1) S0: Control mean (std. dev.)	(2) S1: Salience coefficient (robust SE)	(3) p-Value	(4) S2: Soc. info. coefficient (robust SE)	(5) p-Value	(6) S1−S2 = 0 (robust SE)	(7) p-Value
First university	0.776	-0.000	0.991	-0.022	0.601	0.022	0.610
	(0.418)	(0.042)		(0.042)		(0.042)	
Female	0.375	-0.012	0.794	0.001	0.990	-0.013	0.785
	(0.485)	(0.048)		(0.048)		(0.048)	
Age	20.809	0.218	0.344	0.160	0.509	0.058	0.827
	(2.022)	(0.230)		(0.242)		(0.266)	
HS GPA	2.345	-0.003	0.959	0.000	0.998	-0.003	0.957
	(0.533)	(0.056)		(0.054)		(0.056)	
Fresh HS degree	0.432	-0.021	0.673	-0.070	0.157	0.049	0.320
	(0.497)	(0.050)		(0.049)		(0.049)	
HS degree Abitur	0.807	0.041	0.281	0.065*	0.070	-0.024	0.489
	(0.395)	(0.038)		(0.036)		(0.035)	
HS degree NI	0.552	-0.007	0.895	0.005	0.921	-0.012	0.813
	(0.499)	(0.051)		(0.050)		(0.049)	
HS degree NW	0.073	0.041	0.168	-0.010	0.681	0.051*	0.074
	(0.261)	(0.030)		(0.025)		(0.029)	
HS degree HE	0.115	-0.007	0.836	-0.001	0.976	-0.006	0.860
	(0.319)	(0.032)		(0.032)		(0.032)	
HS degree other	0.224	-0.026	0.530	-0.007	0.861	-0.019	0.641
	(0.418)	(0.042)		(0.042)		(0.040)	
HS degree abroad	0.036	-0.001	0.942	0.014	0.500	-0.015	0.458
	(0.188)	(0.019)		(0.020)		(0.020)	
Distance letter	170.863	-59.722	0.307	10.341	0.898	-70.064	0.195
	(814.427)	(58.351)		(80.627)		(54.031)	
Sign-up before letter	0.099	0.021	0.409	0.004	0.869	0.017	0.510
	(0.299)	(0.026)		(0.025)		(0.026)	
N	192	194		195			

Note: Column (1) presents the unadjusted control group means and standard deviations of the covariates. Columns (2) and (4) present the estimated coefficients of regressing the covariates on the treatment indicators using Equation 1. Columns (3) and (5) test the null hypotheses of no treatment effects. Columns (6) and (7) test for the equality of the two treatment effects. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table B.5: DESCRIPTIVE STATISTICS AND BALANCING PROPERTIES – INTERVENTION 2 (INFORMATION ON PAST EVALUATION), SUMMER TERM

	(1) E0: Control mean (std. dev.)	(2) E2: Soc. info. coefficient (robust SE)	(3) p-Value
First university	0.646	-0.034	0.693
	(0.482)	(0.087)	
Female	0.277	0.001	0.991
	(0.451)	(0.078)	
Age	21.055	0.073	0.842
	(1.951)	(0.364)	
HS GPA	2.525	0.012	0.884
	(0.489)	(0.083)	
Fresh HS degree	0.508	-0.052	0.561
	(0.504)	(0.089)	
HS degree Abitur	0.908	-0.058	0.314
	(0.292)	(0.057)	
HS degree NI	0.538	0.022	0.808
	(0.502)	(0.089)	
HS degree NW	0.108	0.029	0.628
	(0.312)	(0.060)	
HS degree HE	0.108	0.053	0.372
	(0.312)	(0.059)	
HS degree other	0.231	-0.102	0.129
	(0.425)	(0.067)	
HS degree abroad	0.015	-0.002	0.924
	(0.124)	(0.021)	
Distance letter	97.072	-14.394	0.351
	(94.000)	(15.363)	
N	65	64	

Note: Column (1) presents the unadjusted control group means and standard deviations of the covariates. Column (2) presents the estimated coefficients of regressing the covariates on the treatment indicator using Equation 1. Column (3) tests the null hypothesis of no treatment effect. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table B.6: DESCRIPTIVE STATISTICS AND BALANCING PROPERTIES – INTERVENTION 2 (INFORMATION ON PAST EVALUATION), WINTER TERM

	(1) E0: Control mean (std. dev.)	(2) E1: Salience coefficient (robust SE)	(3) p-Value	(4) E2: Soc. info. coefficient (robust SE)	(5) p-Value	(6) E1−E2 = 0 (robust SE)	(7) p-Value
First university	0.827	0.011	0.794	0.010	0.818	-0.001	0.977
	(0.380)	(0.043)		(0.043)		(0.043)	
Female	0.400	-0.004	0.948	0.018	0.752	-0.021	0.707
	(0.492)	(0.055)		(0.055)		(0.056)	
Age	20.667	0.056	0.810	-0.019	0.944	0.075	0.760
	(2.292)	(0.235)		(0.261)		(0.245)	
HS GPA	2.347	-0.006	0.921	-0.006	0.923	0.000	0.995
	(0.520)	(0.059)		(0.065)		(0.065)	
Fresh HS degree	0.427	-0.023	0.683	0.042	0.467	-0.066	0.258
	(0.496)	(0.057)		(0.058)		(0.058)	
HS degree Abitur	0.880	-0.042	0.286	-0.017	0.644	-0.024	0.547
	(0.326)	(0.039)		(0.038)		(0.040)	
HS degree NI	0.560	-0.027	0.633	0.021	0.712	-0.048	0.403
	(0.498)	(0.057)		(0.057)		(0.058)	
HS degree NW	0.120	-0.058*	0.080	-0.040	0.256	-0.018	0.547
	(0.326)	(0.033)		(0.035)		(0.030)	
HS degree HE	0.067	0.036	0.271	0.083**	0.021	-0.047	0.224
	(0.250)	(0.033)		(0.036)		(0.039)	
HS degree other	0.233	0.016	0.749	-0.065	0.156	0.081*	0.089
	(0.424)	(0.050)		(0.046)		(0.048)	
HS degree abroad	0.020	0.033	0.128	-0.000	0.985	0.033	0.134
	(0.140)	(0.022)		(0.017)		(0.022)	
Distance letter	138.545	111.244	0.280	-39.699	0.132	150.944	0.137
	(285.019)	(102.897)		(26.280)		(101.325)	
N	150	148		147			

Note: Column (1) presents the unadjusted control group means and standard deviations of the covariates. Columns (2) and (4) present the estimated coefficients of regressing the covariates on thetreatment indicators using Equation 1. Columns (3) and (5) test the null hypotheses of no treatment effects. Columns (6) and (7) test for the equality of the two treatment effects. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

C Pre-registered analyses by cohort

Since the main paper reports the results of the two interventions for the pooled samples only, in this appendix, I present results separated by cohorts, following the respective pre-registrations.

C.1 Summer cohort

The field experiments with the summer cohort are pre-registered under `https://osf.io/tm7k3`.

Main analyses. To address the main research questions posed in the pre-registration, Table C.1 reports effects of Intervention 1 on sign up for the remedial math course and Table C.2 reports effects of Intervention 2 on participation in the course.

Secondary analyses. Since I did not receive the respective information, I do not report effects on the performance in placement tests that took place at the beginning and the end of the remedial math course. Table C.3 reports results regarding the interaction of the two interventions. Tables C.4 and C.5 report heterogeneous effects of the interventions with respect to the time at which the invitation letter in Intervention 1 was sent and whether this is the first semester at any university.

Table C.1: EFFECT OF INTERVENTION 1 (INFORMATION ON PAST SIGN-UP RATE)

	Sign-up	
	(1)	(2)
S2: Social information	0.052	0.057
	(0.058)	(0.057)
Strata	yes	yes
Controls	no	yes
N	208	208
Control mean	0.68	0.68
(SD)	(0.47)	(0.47)

Note: Outcome variable: sign-up for remedial math course; *strata:* study program FE, invitation letter date FE (= matriculation date FE), and first university dummy; *controls:* female dummy, age, HS GPA, fresh HS degree dummy, HS degree abitur dummy, and place of HS degree dummies. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table C.2: EFFECT OF INTERVENTION 2 (INFORMATION ON PAST EVALUATION)

	Part. 1st tutorial		Average part.	
	(1)	(2)	(3)	(4)
E2: Social information	0.031	0.025	-0.038	-0.039
	(0.042)	(0.046)	(0.050)	(0.049)
Strata	yes	yes	yes	yes
Controls	no	yes	no	yes
N	129	129	129	129
Control mean	0.97	0.97	0.82	0.82
(SD)	(0.18)	(0.18)	(0.28)	(0.28)

Note: Outcome variables: participation in first tutorial of remedial math course and average participation is the share of tutorials a student participated in; *strata:* study program FE, Intervention 1 treatment status (the pre-registration mistakenly states matriculation date FE), and first university dummy; *controls:* female dummy, age, HS GPA, fresh HS degree dummy, HS degree abitur dummy, and place of HS degree dummies. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table C.3: EFFECT OF INTERVENTION 2 (INFORMATION ON PAST EVALUATION) – BY INTERVENTION 1 TREATMENT STATUS

	Part. 1st tutorial		Average part.	
	(1)	(2)	(3)	(4)
E2: Social information	0.067	0.030	-0.016	-0.065
	(0.046)	(0.040)	(0.075)	(0.072)
S2: Social information	-0.030	-0.058	-0.037	-0.066
	(0.068)	(0.065)	(0.071)	(0.071)
E2*S2	-0.042	0.018	-0.018	0.075
	(0.082)	(0.082)	(0.102)	(0.107)
Strata	yes	yes	yes	yes
Controls	no	yes	no	yes
N	127	127	127	127

Note: Only includes students that were part of both interventions. *Outcome variables:* participation in first tutorial of remedial math course and average participation is the share of tutorials a student participated in; *strata:* study program FE and first university dummy; *controls:* female dummy, age, HS GPA, fresh HS degree dummy, HS degree abitur dummy, and place of HS degree dummies. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table C.4: EFFECT OF INTERVENTION 1 (INFORMATION ON PAST SIGN-UP RATE) – HETEROGENEITIES

	Sign-up			
	Last month		First university	
	(1)	(2)	(3)	(4)
S2: Social information	0.070	0.087	0.032	0.039
	(0.070)	(0.069)	(0.095)	(0.097)
Last month	-0.169*	-0.160*		
	(0.093)	(0.095)		
S2*last month	-0.052	-0.098		
	(0.125)	(0.120)		
First university			0.321***	0.283***
			(0.086)	(0.092)
S2*first university			0.041	0.036
			(0.117)	(0.119)
Strata	yes	yes	yes	yes
Controls	no	yes	no	yes
N	208	208	208	208

Note: Outcome variable: sign-up for remedial math course; *strata:* study program FE, invitation letter date FE (= matriculation date FE, only Columns 3 and 4), and first university dummy (only Columns 1 and 2); *controls:* female dummy, age, HS GPA, fresh HS degree dummy, HS degree abitur dummy, and place of HS degree dummies. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table C.5: EFFECT OF INTERVENTION 2 (INFORMATION ON PAST EVALUATION) – HETEROGENEITIES

	Last month				First university			
	Part. 1st tutorial		Average part.		Part. 1st tutorial		Average part.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
E2: Social information	0.082*	0.072	0.022	0.007	0.042	0.033	-0.003	-0.016
	(0.048)	(0.049)	(0.058)	(0.056)	(0.072)	(0.071)	(0.092)	(0.087)
Last month	0.127**	0.131**	0.118*	0.105				
	(0.054)	(0.054)	(0.063)	(0.071)				
E2*last month	-0.236**	-0.217**	-0.279**	-0.212				
	(0.100)	(0.105)	(0.114)	(0.133)				
First university					0.017	0.036	0.146*	0.181**
					(0.071)	(0.076)	(0.081)	(0.084)
E2*first university					-0.017	-0.012	-0.056	-0.036
					(0.090)	(0.089)	(0.111)	(0.108)
Strata	yes	yes	yes	yes	yes	yes	yes	yes
Controls	no	yes	no	yes	no	yes	no	yes
N	129	129	129	129	129	129	129	129

Note: Outcome variables: participation in first tutorial of remedial math course and average participation is the share of tutorials a student participated in; *strata:* study program FE, Intervention 1 treatment status (the pre-registration mistakenly states matriculation date FE), and first university dummy (only Columns 1 to 4); *controls:* female dummy, age, HS GPA, fresh HS degree dummy, HS degree abitur dummy, and place of HS degree dummies. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

C.2 Winter cohort

The field experiments with the winter cohort are pre-registered under `https://osf.io/vqa84`.

Main analyses. To address the main research questions posed by the pre-registration, Table C.6 reports effects of Intervention 1 on sign up for and participation in the remedial math course and Table C.7 reports effects of Intervention 1 on participation in the course.

Secondary analyses. Since I did not receive the respective information, I do not report effects on the performance in placement tests that took place at the beginning and the end of the remedial math course. Tables C.8, C.9, and C.10 report on the heterogeneous effects of Intervention 1 and Tables C.9 and C.10 on the heterogeneous effects of Intervention 2.

Table C.6: EFFECT OF INTERVENTION 1 (INFORMATION ON PAST SIGN-UP RATE)

	Sign-up		Part. 1st tutorial		Average part.	
	(1)	(2)	(3)	(4)	(5)	(6)
S1: Salience	-0.038	-0.038	-0.049	-0.043	-0.042	-0.038
	(0.039)	(0.039)	(0.043)	(0.042)	(0.040)	(0.039)
S2: Social information	0.002	0.002	-0.001	0.002	-0.010	-0.007
	(0.038)	(0.038)	(0.042)	(0.042)	(0.040)	(0.038)
S2−S1	0.041	0.041	0.048	0.045	0.032	0.030
	(0.039)	(0.039)	(0.043)	(0.041)	(0.040)	(0.039)
Strata	yes	yes	yes	yes	yes	yes
Controls	no	lasso	no	lasso	no	lasso
N	581	581	581	581	581	581
Control mean	0.80	0.80	0.73	0.73	0.64	0.64
(SD)	(0.40)	(0.40)	(0.45)	(0.45)	(0.42)	(0.42)

Note: Outcome variables: sign-up for remedial math course, participation in first tutorial of remedial math course, and average participation is the share of tutorials a student participated in; *strata:* study program FE, invitation letter date FE as well as first university and female dummies; *controls:* the double-post LASSO specification considers age, HS GPA, fresh HS degree dummy, HS degree abitur dummy, place of HS degree dummies, and the distance over which the letter was sent. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table C.7: EFFECT OF INTERVENTION 2 (INFORMATION ON PAST EVALUATION)

	Part. 1st tutorial		Average part.	
	(1)	(2)	(3)	(4)
E1: Salience	0.024	0.024	-0.008	-0.009
	(0.034)	(0.033)	(0.037)	(0.036)
E2: Social information	0.025	0.025	0.034	0.034
	(0.035)	(0.033)	(0.036)	(0.035)
E2−E1	0.001	0.001	0.043	0.043
	(0.033)	(0.031)	(0.034)	(0.033)
Strata	yes	yes	yes	yes
Controls	no	lasso	no	lasso
N	445	445	445	445
Control mean	0.90	0.90	0.79	0.79
(SD)	(0.30)	(0.30)	(0.31)	(0.31)

Note: Outcome variables: participation in first tutorial of remedial math course and average participation is the share of tutorials a student participated in; *strata:* study program FE, Intervention 1 treatment status FE as well as first university and female dummies; *controls:* the double-post LASSO specification considers age, HS GPA, fresh HS degree dummy, HS degree abitur dummy, place of HS degree dummies, and the distance over which the letter was sent. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table C.8: EFFECT OF INTERVENTION 1 (INFORMATION ON PAST SIGN-UP RATE) – BY DATE OF ENROLLMENT

	Sign-up		Part. 1st tutorial		Average part.	
	(1)	(2)	(3)	(4)	(5)	(6)
S1: Salience	-0.059	-0.057	-0.071	-0.064	-0.082*	-0.078
	(0.047)	(0.046)	(0.051)	(0.050)	(0.048)	(0.048)
S2: Social information	-0.037	-0.043	-0.042	-0.046	-0.070	-0.079*
	(0.047)	(0.047)	(0.051)	(0.050)	(0.048)	(0.047)
Last month	-0.040	-0.061	-0.092	-0.105	-0.114*	-0.125*
	(0.066)	(0.066)	(0.073)	(0.074)	(0.066)	(0.066)
S1*last month	0.054	0.052	0.061	0.055	0.117	0.113
	(0.093)	(0.092)	(0.101)	(0.100)	(0.093)	(0.091)
S2*last month	0.111	0.130	0.121	0.135	0.184**	0.197**
	(0.086)	(0.085)	(0.097)	(0.095)	(0.088)	(0.086)
Strata	yes	yes	yes	yes	yes	yes
Controls	no	yes	no	yes	no	yes
N	581	581	581	581	581	581

Note: Outcome variables: sign-up for remedial math course, participation in first tutorial of remedial math course, and average participation is the share of tutorials a student participated in; *strata:* study program FE as well as first university and female dummies; *controls:* age, HS GPA, fresh HS degree dummy, HS degree abitur dummy, place of HS degree dummies, and the distance over which the letter was sent. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table C.9: EFFECT OF INTERVENTION 1 (INFORMATION ON PAST SIGN-UP RATE) – BY FIRST UNIVERSITY

	Sign-up		Part. 1st tutorial		Average part.	
	(1)	(2)	(3)	(4)	(5)	(6)
S1: Salience	-0.098	-0.089	-0.096	-0.081	-0.049	-0.039
	(0.104)	(0.103)	(0.105)	(0.103)	(0.094)	(0.091)
S2: Social information	0.011	0.020	0.006	0.010	-0.030	-0.030
	(0.099)	(0.098)	(0.104)	(0.102)	(0.090)	(0.088)
First university	0.248***	0.234***	0.321***	0.307***	0.323***	0.327***
	(0.078)	(0.080)	(0.084)	(0.086)	(0.075)	(0.079)
S1*first university	0.076	0.067	0.061	0.046	0.009	-0.002
	(0.112)	(0.111)	(0.115)	(0.113)	(0.104)	(0.102)
S2*first university	0.012	-0.025	-0.010	-0.016	0.027	0.021
	(0.107)	(0.106)	(0.113)	(0.112)	(0.100)	(0.098)
Strata	yes	yes	yes	yes	yes	yes
Controls	no	yes	no	yes	no	yes
N	581	581	581	581	581	581

Note: Outcome variables: sign-up for remedial math course, participation in first tutorial of remedial math course, and average participation is the share of tutorials a student participated in; *strata:* study program FE, invitation letter date FE, and female dummy; *controls:* age, HS GPA, fresh HS degree dummy, HS degree abitur dummy, place of HS degree dummies, and the distance over which the letter was sent. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table C.10: EFFECT OF INTERVENTION 1 (INFORMATION ON PAST SIGN-UP RATE) – BY SEX

	Sign-up		Part. 1st tutorial		Average part.	
	(1)	(2)	(3)	(4)	(5)	(6)
S1: Salience	-0.011	-0.015	-0.068	-0.072	-0.072	-0.079
	(0.049)	(0.049)	(0.054)	(0.054)	(0.049)	(0.049)
S2: Social information	-0.006	-0.012	-0.023	-0.027	-0.044	-0.051
	(0.050)	(0.050)	(0.053)	(0.054)	(0.048)	(0.049)
Female	0.074	0.056	0.008	-0.023	0.017	-0.016
	(0.056)	(0.058)	(0.064)	(0.065)	(0.060)	(0.060)
S1*female	-0.075	-0.063	0.050	0.072	0.082	0.105
	(0.081)	(0.083)	(0.091)	(0.090)	(0.085)	(0.084)
S2*female	0.023	0.037	0.059	0.067	0.091	0.098
	(0.077)	(0.076)	(0.088)	(0.088)	(0.083)	(0.081)
Strata	yes	yes	yes	yes	yes	yes
Controls	no	yes	no	yes	no	yes
N	581	581	581	581	581	581

Note: Outcome variables: sign-up for remedial math course, participation in first tutorial of remedial math course, and average participation is the share of tutorials a student participated in; *strata:* study program FE, invitation letter date FE, and first university dummy; *controls:* age, HS GPA, fresh HS degree dummy, HS degree abitur dummy, place of HS degree dummies, and the distance over which the letter was sent. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table C.11: EFFECT OF INTERVENTION 2 (INFORMATION ON PAST EVALUATION) – BY FIRST UNIVERSITY

	Part. 1st tutorial		Average part.	
	(1)	(2)	(3)	(4)
E1: Salience	-0.100	-0.102	-0.182	-0.189*
	(0.122)	(0.117)	(0.114)	(0.106)
E2: Social information	0.025	0.022	0.024	0.018
	(0.107)	(0.104)	(0.109)	(0.105)
First university	0.081	0.081	0.103	0.125
	(0.083)	(0.083)	(0.086)	(0.084)
E1*first university	0.149	0.148	0.208*	0.214*
	(0.127)	(0.121)	(0.121)	(0.112)
E2*first university	0.001	0.005	0.013	0.027
	(0.114)	(0.111)	(0.116)	(0.112)
Strata	yes	yes	yes	yes
Controls	no	yes	no	yes
N	445	445	445	445

Note: Outcome variables: participation in first tutorial of remedial math course and average participation is the share of tutorials a student participated in; *strata:* study program FE, Intervention 1 treatment status FE, and female dummy; *controls:* age, HS GPA, fresh HS degree dummy, HS degree abitur dummy, place of HS degree dummies, and the distance over which the letter was sent. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table C.12: EFFECT OF INTERVENTION 2 (INFORMATION ON PAST EVALUATION) – BY SEX

	Part. 1st tutorial		Average part.	
	(1)	(2)	(3)	(4)
E1: Salience	0.043	0.044	0.029	0.026
	(0.041)	(0.041)	(0.046)	(0.046)
E2: Social information	0.008	0.014	0.057	0.073
	(0.046)	(0.047)	(0.048)	(0.048)
Female	0.004	-0.009	0.099*	0.083
	(0.054)	(0.054)	(0.056)	(0.055)
E1*female	-0.049	-0.060	-0.094	-0.093
	(0.072)	(0.074)	(0.076)	(0.074)
E2*female	0.042	0.026	-0.056	-0.082
	(0.071)	(0.072)	(0.074)	(0.072)
Strata	yes	yes	yes	yes
Controls	no	yes	no	yes
N	445	445	445	445

Note: Outcome variables: participation in first tutorial of remedial math course and average participation is the share of tutorials a student participated in; *strata:* study program FE, Intervention 1 treatment status FE, and first university dummy; *controls:* age, HS GPA, fresh HS degree dummy, HS degree abitur dummy, place of HS degree dummies, and the distance over which the letter was sent. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.