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Social Information and Educational Investment – Nudging Remedial Math Course Participation

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Abstract

In the context of a voluntary remedial math course for university students, we conduct randomized field experiments to study whether social information can help counteract low participation rates. Incoming students receive postal invitation and reminder letters that inform them about past sign-up rates and past evaluations on the usefulness of the course, respectively. On average, we find that neither of the two interventions increases sign-up or participation. Heterogeneity analyses provide evidence that a targeted provision of the invitation letters can be beneficial: i) By increasing the salience of the course, they raise attendance among late enrolling students, which in turn increases their academic performance in the first year of studies. ii) Students whose ex-ante sign-up probability falls just short of the descriptive norm increase sign-up and participation in response to the social information, while the opposite is true for students whose sign-up probability exceeds the norm.

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

JEL Classification: D83, I21, I23, C93

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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).[1] 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). However, in many situations, the necessary information may not be available to policy makers, for instance, because the educational investments have not yet been evaluated or because the returns also encompass non-pecuniary benefits that are difficult to measure.[2]

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 (see Coffman, Featherstone and Kessler (2015) and Coffman, Featherstone and Kessler (2017) for respective models). Second, and more general, the behavior of others may also be perceived as a descriptive norm, i.e., individuals might want to invest in education because they expect others to do the same (see, e.g., Bicchieri and Dimant 2019).

This paper studies if social information can indeed be used to influence an educational investment decision, and whether individuals 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 before 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. Descriptively, 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. 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.[3] Since the course has not been causally evaluated yet, students could not be informed of its returns directly.

[1] In many contexts, beliefs about one's own academic ability and the probability to succeed also play an important role in deciding whether (further) investments in education should be made (see, e.g., Kunz and Staub 2020; Stinebrickner and Stinebrickner 2012, 2014*b*,*a*).

[2] 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.

[3] Information on the cost, i.e., the time it takes to participate, and a summary of the content of the course is available to students in advance. Furthermore, students do not have to pass a test as part of the course, which could otherwise create additional uncertainty.

Against this background, we conduct field experiments with an incoming summer cohort and the subsequent winter cohort, to evaluate the effectiveness of two different social information interventions at increasing sign-up for and participation in the remedial math course: i) After students enroll in their study program, we send a randomly selected group of students a postal invitation letter that includes the information that 85% of students signed up for the remedial math course in a previous cohort.[4] In both cohorts, we compare the sign-up and participation behavior of this treatment group with that of students who receive no invitation letter at all. To test whether the social information or salience drives potential effects, in the winter cohort, we add a treatment group that receives the same invitation letter, but without the social information. ii) To increase course attendance among students who initially sign up for it, we randomize a postal reminder letter that informs students that the course made it easier to get started with university mathematics for 95% of the students in the past. We compare the participation behavior of this group with one that receives no reminder letter at all, and, in the winter cohort, also a group that receives a reminder letter without the social information.

Our key findings are as follows: First, we find that, on average, neither the invitation nor the reminder letter intervention affect students' decision to sign up for or participate in the remedial math course, respectively. Second, further analyses provide evidence that the effects of the invitation letter intervention are heterogeneous along two dimensions: a) Both the letter with information on past sign-up rates and the letter without this information are more effective for students who enroll late in their degree program. They offset more than half of the roughly 9 and 16 percentage point (pp) lower sign-up and participation rates that we observe for these students in the absence of treatment relative to early enrollees. The fact that letters with and without social information are similarly effective suggests that low sign-up and participation among late enrollees is at least partly driven by a lack of (relative) salience of the course. b) The effect of the invitation letter with social information also depends on the ex-ante sign-up probability of students, which we predict using the control group, i.e., we use endogenous stratification (Abadie, Chingos and West, 2018). Those with the highest ex-ante probability decrease sign-up and participation by about 10 pp, while the opposite is true for students whose probability falls just short of the social information on past sign-up rates; students with the lowest predicted sign-up probabilities show no behavioral response. We argue that 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 and the expected utility of the course.

[4] Throughout the paper, we use the term "enrollment" when referring to study programs or the university and we use the term "sign-up" when referring to the remedial math course.

Third, we investigate whether these heterogeneous effects on remedial math course participation carry over to academic achievement in the first year of studies. Our findings suggest that this only holds true for the timing of enrollment. For late enrolling students, whose overall performance 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 performance. 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. Our findings are in line with the notion that early engagement with their studies in form of the remedial math course prevents these students from dropping out early, thus explaining the large gains in academic achievement.

The context and results of our study contribute to the following strands of the literature. First, our paper relates to the literature that studies whether and how the provision of social information influences individual decision-making. In contrast to this study, much of the existing evidence on positive effects comes from environments where the stakes for individuals are low, such as increasing charitable giving (e.g., Frey and Meier 2004; Croson and Shang 2008; Martin and Randal 2008; Shang and Croson 2009), public good contributions in the lab (e.g., Keser and Van Winden 2000; Fischbacher, Gächter and Fehr 2001), or environmentally friendly behavior (e.g., Goldstein, Cialdini and Griskevicius 2008; Allcott and Rogers 2014; Byrne, Nauze and Martin 2018; Brent et al. 2020). Attempts to influence decisions with higher stakes have produced mixed results: Fellner, Sausgruber and Traxler (2013) find no effect of social information on TV licensing fee compliance and Beshears et al. (2015) even find negative effects when trying to increase retirement savings. Hallsworth et al. (2017), Gee (2019), and Coffman, Featherstone and Kessler (2017) on the other hand show that social information can positively affect tax compliance, job application rates, and job take-up, respectively. Our study provides additional evidence for the effects of social information in environments with higher stakes, as the decision not to participate in the remedial math course can come at the cost of obtaining less course credits or dropping out of the study program. Moreover, to our knowledge, this is the first study that provides evidence on this type of social information from an educational context.[5][6]

Second, our study is related to the literature that uses nudges to improve decision-

[5]Darolia and Harper (2018) study the effects of student debt letters, which also include information on past borrowing behavior of others. However, they do not explicitly test the effects of the social information.

[6]The literature on relative performance feedback also makes use of social information (see Villeval 2020 for a review of the literature). However, there, information is usually given about the current performance of similar others, which can, for example, create positive effects through competitive preferences and learning about own ability (Azmat and Iriberri, 2010; Dobrescu et al., forthcoming). Such mechanisms are unlikely to play a role for the type of social information that we study in this paper. In addition, relative performance feedback usually aims at affecting effort at the intensive and not on the extensive margin.

making in education (see, e.g., Damgaard and Nielsen 2018 for a review). More specifically, we 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. 2019 for reviews). However, results from recent large-scale field experiments suggest that these interventions do not necessarily scale up, creating the need for alternative approaches (Bird et al., 2019; Bergman, Denning and Manoli, 2019; Gurantz et al., 2019). Furthermore, attempts to improve persistence in college with the help of low-touch information interventions have so far not provided the desired results (Oreopoulos and Petronijevic, 2018, 2019; Huntington-Klein and Gill, 2019). Our study extends this literature by adding the following: First, to the best of our knowledge, our study is the first to explicitly test whether information about the past behavior of others affects individual decision-making in higher education. Given the promising results in other areas, surprisingly little attention has been paid to this approach so far. Second, we are the first to study 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, our results also illustrate that in some contexts a targeted provision of (social) information nudges may be necessary to achieve the desired results.

Last, the paper contributes to the ongoing discussion on college remediation (e.g., Holzer and Baum 2017; Oreopoulos 2021). By studying whether the changes in remedial math course participation due to our intervention carry over to academic performance, we are the first who study the effects of remediation based on experimentally induced variation in attendance. Many existing studies on the effectiveness of remedial courses in higher education instead make use of natural experiments that occur when participation in remedial courses is based on performance in a placement test (see, e.g., Martorell and McFarlin Jr 2011; Boatman and Long 2018). Our study adds to this literature by providing evidence on the effectiveness of remediation from complier populations that have not been studied so far.

The paper continues as follows. Section 2 describes the institutional background, data, and design of the two interventions as well as the empirical approach. In Sections 3 and 4, we present the results of the invitation and the reminder letter intervention, respectively. Section 5 concludes.

2 Institutional background and research design

2.1 Institutional background

We conducted our field experiments at one of the largest universities in Germany with a summer and a winter cohort of incoming first-year students who enroll in one of five bachelor's degree programs offered at the Faculty of Business and Economics.[7] Adequate knowledge of mathematics is an important prerequisite: students in all but one of the programs must pass a math exam by the end of the second semester in order to continue with their program.[8] In addition, all programs include other mathematically demanding subjects, such as microeconomics, that students have to pass at some point during their studies. To prepare students for these subjects, the Faculty of Business and Economics offers a voluntary remedial math course that usually takes place two to three weeks before the start of the first term. The course aims at refreshing the math knowledge that students should have acquired by the end of secondary education and filling potential gaps.[9] The course usually lasts eight days, with each day consisting of a lecture in the morning and two tutorials later in the day, which are held in groups of ten to twenty students. In order to facilitate the organization of the course, especially the prior formation of the tutorial groups, students are asked to sign up for the course in advance via a web portal.

In the absence of our interventions, students can access or receive information on the remedial math course via the following channels: First, information about the course is publicly available on a website. Second, incoming first-year students receive a letter from the student council that provides information about the (social) activities that are planned at the beginning of their studies, including information on the remedial math course. Third, the organizers of the course themselves email all newly enrolled students, inviting them to participate in the course. In principle, the remedial math course should thus be very salient to students. Still, a considerable number of students does not sign up for the course and the organizers informed us that attrition between sign-up and participation presents an additional problem. For example, in the control group of our first intervention, only 76% of the incoming students sign up for the course, and the participation rate in the first tutorial and

[7] Students can enroll both in the summer and the winter term, but most students enter the university in the winter after graduating from high school during the summer.

[8] Due to the COVID-19 pandemic, this requirement was relaxed and students in our field experiments were allowed to pass the math exam after the second semester. However, when student enrolled in their program, they were not aware of this.

[9] The content of the course covers subjects such as numbers, arithmetic, summation, binomial formulas, (in)equations and systems of linear equations, exponentiation, root extraction, logarithms, functions, and differential calculus.

the average participation across all tutorials are even lower at around 70 and 60%, respectively (see Table 1).

However, for students there may be good reasons not to participate in the course. Most importantly, they may think that they already possess the knowledge that is taught in the course. If that is indeed the case, then it might not even be desirable to persuade these students to participate. Since neither this nor – as far as we know – any other voluntary remedial math course has been evaluated to date, we do not know for certain whether attendance is actually beneficial and whether students are making a non-optimal decision. To provide some correlational evidence on this, in Table A.2, we use the control group of our first intervention and regress different measures of academic achievement on average participation, controlling for a large set of observables; effects of the high school GPA are shown as comparison. We find that going from 0 to 100% participation significantly increases the likelihood to attempt and pass the math exam within the first semester (year) by about 50 pp, the obtained credits by roughly 10 (19), and reduces the probability to drop out of the study program by around 14 (17) pp.

This raises the question as to why students fail to participate even though the course may have been beneficial for them. First and foremost, students may misjudge their own knowledge or how useful the course is, making it difficult to assess whether they actually need to participate in order to pass the math exam and other mathematically demanding subjects in their studies. Second, before the start of their studies, students are often still busy with organizational matters, in particular the search for an accommodation, which may reduce the salience or relative importance of the course. While there may certainly be other causes for a lack of attendance, e.g., students may simply not have time to participate, our interventions aim to address the salience of the course and uncertainty about its usefulness.

2.2 Experimental design

Against this background, we 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 past sign-up rates, and ii) A reminder letter for students who signed up for the course, containing information about how useful students have perceived the course in the past. To test their effectiveness, we 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 general design and timing of the interventions was the same in both cohorts and is summarized in Figure 1.[10]

[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

Invitation letter and information on past sign-up rates. Starting five to seven weeks before the beginning of the remedial math course until one week before, we used administrative data on the incoming students provided to us 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); 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 (**I0**) received no information about the remedial math course from us, but they could still receive information on the math course from the sources mentioned above.

Both in the summer and the winter term, students who were randomized into the treatment group **I2** received a letter on behalf of the organizers of the course (see Figure A.1). 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 our 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 signs 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, we 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

experiment (winter term) under `https://osf.io/vqa84`. The interventions in both cohorts and the respective math courses took place before the Corona 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.

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 participation, if information on past sign-up rates shifts the beliefs above their personal threshold.[12]

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).[13] The social information in our letters 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 we 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 past sign-up rates that we include in the letter. To study if this is the case, in the winter term, we included a second treatment group (**I1**), which also received an invitation letter, but without the descriptive social norm; i.e., it excluded the parts highlighted in gray in Figure A.1.

Reminder letter and information on the usefulness of the course. About one week before the start of the course (see Figure 1), we 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 (see Appendix B for details and the balancing properties).

While students in the control group (**R0**) received no letter, students in treatment **R2** received a reminder letter on behalf of the organizers of the course (see Figure A.2). 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

[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).

[13] For example, students may mainly participate in the course for social reason such as meeting their future peers early.

used in the invitation letter, the reminder included information about how useful 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.

We expect that the information on how students have evaluated the course in the past 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 the invitation letter, we again wanted to be able to test if potential effects of the reminder letter are driven by the social information or the letter itself. Therefore, in the winter term, we included a second treatment group (**R1**), which received a reminder letter without the social information (parts highlighted in gray in Figure A.2).

2.3 Data and estimation

Data. To analyze the effects of the two interventions, we use data from three sources: First, we use administrative information on background characteristics for our covariates. Second, we receive information from the organizers of the remedial math course about sign-ups for the course and participation in each of the tutorials, which we use as outcomes in our analyses. Third, we use administrative information from the student office about students' academic performance in the first year of studies to investigate whether potential effects on participation translate into higher academic achievement (Table A.1 describes all variables that we use).[14] For the analyses in the main paper, we pool the data from the summer and the winter cohort.[15]

Our 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

[14]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 us. The pre-registration can be found under `https://osf.io/tv9yf`.

[15]Appendix D presents results separated by cohort – following the respective pre-registrations.

in.[16] In follow up analyses, we 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, we expect that attendance primarily affects whether students attempt and pass the math exam, and what grade they receive. In addition, we are interested in students' overall academic performance. For this, we consider the number of passed course credits[17], whether they dropped out of their study program[18], and their GPA. These variables were pre-registered as secondary outcomes, and we have 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, we 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 we use as an outcome when investigating the effects on academic achievement.[19]

Analysis of main effects. Regarding the main effects of our interventions, we 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, we control for the random assignment within blocks:

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

where Y_i^k denotes the level of outcome measure k for individual i. $Letter_i$ is an indicator for being randomized into the treatment group that receives the invitation (reminder) letter without social information. $LetterSocialInfo_i$ is an indicator for being randomized into the treatment group that receives the invitation (reminder) letter including the respective social information. The vector $\mathbf{x_i}$ controls for the method of randomization by including study program fixed effects, a winter term dummy, the interaction between the study program fixed effects and the winter term dummy. Additionally, it includes invitation letter date fixed

[16]We 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 of 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.

[17] 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://ec.europa.eu/education/resources-and-tools`, retrieved on July 28, 2021.

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

[19]The use of this index as an additional outcome was not included in the pre-registration for the effects on academic achievement. For the reasons stated above, we nevertheless believe that it is advisable to also consider effects on this aggregate performance measure.

effects or invitation letter treatment status indicators (such that the two randomizations are orthogonal) when analyzing the effects of the invitation or reminder letter, respectively (see Appendix B for details on the randomizations).

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

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

We follow two different approaches for selecting the variables that we include in this vector. First, we simply include all variables that were pre-registered as controls for the second experiment. This includes the first university and female dummies[20], 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 a detailed description of the variables).[21]

Second, we 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 our treatment effect estimates or account for imbalances that are observed despite the randomization. For this purpose, we use the double-post LASSO approach suggested by Belloni, Chernozhukov and Hansen (2014) to select the covariates to be included in the estimation. In the covariate selection process, we consider the pre-registered covariates and for estimations of the effect of the invitation letter we additionally consider an indicator if the student signed up for the remedial math course before we sent, or would have sent in case of the control group, the invitation letter.

As we show in Appendix C and Section 5, our study is generally well-powered enough to detect effects sizes that have so far been published in the literature on social information nudges. There, we also discuss more recent evidence which suggests that previous results may suffer from publication bias and overstate the true effect.

Analysis of heterogeneities. In addition to the main analysis, we pre-registered to study heterogeneity along the following dimensions: First, based on the institutional background we expected that students who were previously enrolled at this or another university would

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

[21] The distance over which the letter was sent was only pre-registered as a control variable in the second experiment.

be less likely to participate in the remedial math course, as they may have participated in a similar course previously. Treatment effects might therefore be concentrated among students for whom this is the first semester at any university. Second, we 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. 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.[22] Lastly, initial results from the field experiments in the summer term indicated that men may be more responsive to treatment, and we thus pre-registered gender as a potential source of heterogeneity for the field experiments in the winter term.

To estimate heterogeneous effects of our treatments for the pre-registered covariates, we employ the following regression specification:

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

Where Y_i^k, $Letter_i$, and $LetterSocialInfo_i$ are defined as before. The vector $\mathbf{c_i}$ includes one or all of the covariates for which we want to study the treatment effect heterogeneity. In case of the invitation letter intervention 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 course.[23] In case of the reminder letter, it includes the female and the first university dummy. The vector $\mathbf{x_i}$ again controls for the method of randomization. When we study the heterogeneity of the invitation letter intervention 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 we investigate the heterogeneous effects of treatment.

3 Effects of the invitation letter intervention

In this section, we first report the main effects of the invitation letter intervention. We then study if the average treatment effects mask heterogeneous responses and whether effects on

[22] 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.

[23] We 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).

participation in the course carry over to students' academic performance in the first year of studies.

3.1 Main effects

Table 1 shows estimates for the effects of the invitation letters with and without social information about past sign-up rates. 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. Sending students 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 invitation letter with social information increase participation by 0.3 to 1.1 pp ($p = 0.943 - 0.745$; Columns 4 to 6). The pure invitation letter decreases participation in the first tutorial by 4.1 to 4.5 pp ($p = 0.251$ in Column 6). Regarding average participation, we 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 social information 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. Pure invitation letters 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, we study whether the main effects mask heterogeneity. Along the pre-registered dimensions, we find the most robust evidence for the timing of enrollment, which we discuss in detail below. With respect to students' gender and the first university dummy, we 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 re-

medial math course – which is in line with what we expected, but we find no evidence of heterogeneous treatment effects. Students' gender, on the other hand, is not predictive of sign up and participation in the absence of treatment, but we find that treatment effects on average participation are between 4.6 to 13.6 pp higher for females compared to males; these effects, however, are not estimated precisely.

Timing of enrollment. Panel a) in Table 2 presents effects by timing of enrollment. About 31% of the sample enrolled late and were sent the invitation letter 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, we find that both types of invitation letters are 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 invitation letters produce the same pattern and magnitude of results suggests that the social information provided in our main 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 with and without social information are able to mitigate this. In light of this finding, and to increase the statistical power of our analysis, in Panel b) of Table 2, we 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 that 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, we 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.[24] Specifically, it is conceivable that the effects of the invitation letters – in particular the one including information on past sign-up rates – depend on the ex-ante sign-up probability of students: First, students with a low sign-up

[24] Studying heterogeneity through endogenous stratification was only included in the pre-registration for the effects on academic achievement.

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

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

where Y_i^k, $Letter_i$, $LetterSocialInfo_i$, $\mathbf{x_i}$, and $\mathbf{z_i}$ are defined as in Equation 2. $\mathbf{e_i}$ includes the endogenous strata dummies. Based on this equation, we 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 us 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 3. Among students in the highest tercile of predicted sign-up probabilities, 86.7% (88.4% without any controls) sign up for the remedial math course. In this group, both the letter without and the letter with social information decrease sign-up by 6.7 pp (p = 0.224) and 9.6 pp (p = 0.071), respectively (see Column 2 in Table 3). Importantly, these effects persist: students in the two treatment groups are 7.7 pp (p = 0.240) and 12.3 pp (p = 0.043) less likely to participate in the first tutorial of the course,

and show a 8.9 pp ($p = 0.140$ and $p = 0.120$) lower average participation rate across all tutorials (see Columns 4 and 6 in Table 3). Adjusting for covariates in Columns (3), (5), and (7) leads to attenuated estimates, in particular for the pure invitation letter.

In the middle tercile, 78.9% (79.0% without any controls) of control group students sign up for the course. While the invitation letter without social information has no effect on either sign-up or participation in this group, our estimates indicate that the letter with social information is able to increase sign-up for the course by 9.3 pp ($p = 0.070$; see Column 2 in Table 3). This effect translates into a higher participation rate in the first tutorial and across all tutorials by 11.5 pp ($p = 0.045$) and 10.1 pp ($p = 0.062$), respectively (see Columns 4 and 6 in Table 3). 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 64.7% (61.4% without any controls) sign up for the remedial math course – our estimates indicate that neither type of invitation letter is able to affect the decision to sign up for or participate in the math course. Both for the letter with and the letter without social information, we 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 3. For the invitation letter with social information, 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 past sign-up rates 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 (35.5%, compared to 77.8 and 95.6% 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 our 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, we 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.[25]

Timing of enrollment. Tables 4 and 5 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, we 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 4). 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 (p = 0.008) less, and are 6.2 pp (p = 0.115) more likely to have dropped out of their study program (Columns 1 to 3 in Table 5).[26] 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 4), and their overall performance is still 0.257 standard deviations (p = 0.046) lower compared to students who enroll early (Column 5 of Table 5).

For all dimensions of academic achievement presented in Tables 4 and 5, we find that the invitation letters are able to offset all or almost all of the disadvantage in academic achievement that we observe for students who enroll late. Given that our 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 we 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 5) 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 17).

[25] As mentioned in Footnote 14, the analyses of the effects on academic achievement were pre-registered after the analysis of the effects on sign up for and participation in the course, but before data on academic achievement became available to us.

[26] 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.

This raises the question as to how the large increase in achievement among late enrollees comes about. One possible explanation is that these students suffer from low motivation and engagement with their studies in the absence of treatment. 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.[27] The higher dropout rate among late enrollees in the control group and the decrease in dropout due to treatment presented in Table 5 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 letter 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 (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 we 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 late enrollees make up around 31% of the sample, our findings suggest that the remedial math course can be beneficial for a considerable number of students. In fact, since our intervention only includes students who enroll in their study program up to one week before the math course begins, our results may represent a lower bound.[28]

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 re-

[27] 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 ride regional trains in the federal state for free.

[28] Across both cohorts, about 100 additional students enrolled at an even later date, making it impossible for some of them to attend the remedial math course.

port effects of the two types of invitation letters on the academic achievement dimensions across the three endogenous strata. Overall, we find little to no robust evidence that the effects on participation presented in Table 3 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, we would expect changes in average participation to translate into consistent changes in academic achievement across the different outcomes – similar to what we 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 we found for the invitation letter with social information 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 – who increase participation in response to treatment – already possess the knowledge that is necessary to pass the exams. Table A.6 depicts the means of our 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. More generally – and in contrast to the timing of enrollment – we observe 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 the reminder letter intervention

In this section, we present the results of the reminder letter intervention that we conducted among students who signed up for the remedial math course.

Main effects. Table 6 reports the main effects of the reminder letter intervention. 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 the reminder letter with social information about the usefulness of the course increases participation by 2.8 pp, independent of the exact specification (p = 0.293 in Column 3). The pure reminder letter without social information also increases participation by about 2.6 pp (p = 0.407 in Column 3), indicating that it is the reminder 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, we 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 a letter with social information increase their participation rate by 1.3 to 1.6 pp ($p = 0.658 - 0.582$), while students that receive a letter without social information 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 useful as the reminder letters suggested.

Overall, these results suggest that sending reminder letters, with and without social information on the usefulness 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, we investigate whether the average treatment effects presented above hide heterogeneous effects along the pre-registered dimensions (first university and gender). Across both dimensions, we find little to no evidence for heterogeneous treatment effects.[29] Similar to the invitation letter intervention, we 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 us, this indicates that among students who initially signed up for the course, reasons other than those that we can capture with our covariates drive the participation decision. For example, some students may not yet have moved to the city where the university is located, or they may be subject to other external constraints.

5 Discussion and conclusions

In the context of a voluntary remedial math course at a large German university, we study whether social information nudges can be used to influence educational investment decisions. The results of our field experiments show that both an invitation letter with social information on past sign-up rates as well as a reminder letter with information on past evaluations of the course are, on average, not able to increase sign up for and participation in the

[29] For the pure reminder letter, 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.

course.

One concern with our main effects could be that our study is simply not well-powered enough to detect effects that are typically found in the literature. In their meta-analysis, DellaVigna and Linos (2020) report an average treatment effect of 13.81 pp for studies published in academic journals that make use of social cues. As we show in Appendix C, our study has enough power to detect these effects sizes, but also those that are more close to interventions that make use of reminders (5.02 pp). However, for studies conducted by nudge units, DellaVigna and Linos (2020) only report average treatment effects between 0.96 pp (social cues) and 2.56 (reminders), which is much closer to the main effects of this study. While the authors show that some of the differences in the effects can be explained by study characteristics, much of it appears to stem from publication bias. Given this knowledge, the results of our study may well be representative for the effect size that researchers should expect to find for the average treatment effect of social information nudges, although we do not have the power to estimate these kinds of effects precisely. Since this is the first study on this type of social information nudge in an educational context, our results may still serve as a basis for future studies in this area.

Studying the heterogeneity of the main effects, we find evidence that both the invitation letter with and without social information increases participation among students who enroll late, suggesting that low salience may have been causing the low attendance that we observe for these students. In addition, we employ endogenous stratification and show that marginal students increase sign-up and attendance by 10 pp in response to the invitation letter with social information. Students who have a high estimated sign-up probability, on the other hand, decrease sign-up and attendance by about 10 pp after receiving the invitation letter with social information. Our study thus highlights that a targeted provision of social information may be necessary to prevent undesired results.

One explanation for those heterogeneities is that the behavioral response of individuals depends on their beliefs about the behavior of similar others. This has been observed for other contexts, such as the labor market (Coffman, Featherstone and Kessler, 2017) and for antiauthoritarian protests (Cantoni et al., 2019). A caveat of our study is that we do not have direct information on students' beliefs. We try to address this with the help of endogenous stratification. However, this approach may be subject to concerns regarding the exact covariates and specifications that are used to construct the endogenous strata. We circumvent this by only including pre-registered covariates, even though this may result in us failing to find the specification that is most predictive of the outcome of interest. Future studies should elicit beliefs directly or study in advance which covariates are predictive of the outcome that is to be affected by the social information nudge.

Lastly, we also study whether the heterogeneous effects on course participation carry over to academic achievement in the first year of studies. By doing so, we provide first evidence on the effectiveness of (voluntary) remedial math courses based on experimentally induced variation in attendance. In situations where the intervention of interest cannot be evaluated directly, e.g., because it has to be offered to all students, such an encouragement design may be the only viable option. Because we only found increases in math course participation among subgroups, these analyses have limited statistical power, and should therefore be interpreted with caution.

We show that the increase in average participation among students who enroll late leads to robust changes in academic performance. Our results for late enrollees are consistent with the idea that an early engagement with their studies in the form of the remedial course may prevent these students from dropping out early, explaining their large gains in academic achievement. Considering that these students make up a substantial part of the sample and perform poorly in their first year of studies, they are an important population for further research. The heterogeneous effects of social information on participation for students who fall short off or exceed the descriptive norm, on the other hand, do not translate into changes in achievement. This highlights the importance of investigating whether influencing individual behavior, in this case participation in the remedial course, actually results in the desired outcomes later on.

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Tables and figures

Table 1: EFFECT OF INVITATION LETTER

	Sign-up			Participation 1st tutorial			Average participation		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
I1: Letter	-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)
I2: Letter & social info	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	d-p lasso	no	yes	d-p lasso	no	yes	d-p 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 2: EFFECT OF INVITATION LETTER BY TIMING OF ENROLLMENT

	Sign-up		Part. 1st tutorial		Avgerage part.	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel a)						
I1: Letter	-0.060	-0.051	-0.075	-0.064	-0.083*	-0.072
	(0.048)	(0.045)	(0.052)	(0.048)	(0.049)	(0.045)
I2: Letter & social info	-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)
I1*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)
I2*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)						
Any letter (I1+I2)	-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)
(I1+I2)*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)
(I1+I2)+(I1+I2)*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. *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 3: EFFECT OF INVITATION LETTER BY ENDOGENOUS STRATA

	Tercile	Sign-up		Part. 1st tutorial		Average part.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
I1: Letter	Lowest	-0.020	-0.012	-0.075	-0.068	-0.034	-0.030
		(0.077)	(0.075)	(0.081)	(0.077)	(0.075)	(0.070)
	Middle	-0.005	-0.026	0.017	-0.006	0.020	-0.002
		(0.066)	(0.062)	(0.072)	(0.068)	(0.070)	(0.067)
	Highest	-0.067	-0.028	-0.076	-0.029	-0.089	-0.039
		(0.055)	(0.055)	(0.065)	(0.064)	(0.060)	(0.059)
I2: Letter & social info	Lowest	0.027	0.059	-0.010	0.026	-0.038	-0.003
		(0.068)	(0.062)	(0.071)	(0.064)	(0.065)	(0.058)
	Middle	0.093*	0.087*	0.115**	0.109*	0.101*	0.089*
		(0.051)	(0.049)	(0.057)	(0.056)	(0.054)	(0.052)
	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
I1: P-value F-test int. term		0.745	0.983	0.565	0.829	0.485	0.915
I2: P-value F-test int. term		0.038	0.078	0.018	0.042	0.046	0.137

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 4: EFFECT OF INVITATION LETTER 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)
Any letter (I1+I2)	-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)
(I1+I2)*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)
(I1+I2)+(I1+I2)*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 5: EFFECT OF INVITATION LETTER 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)
Any letter (I1+I2)	-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)
(I1+I2)*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)
(I1+I2)+(I1+I2)*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

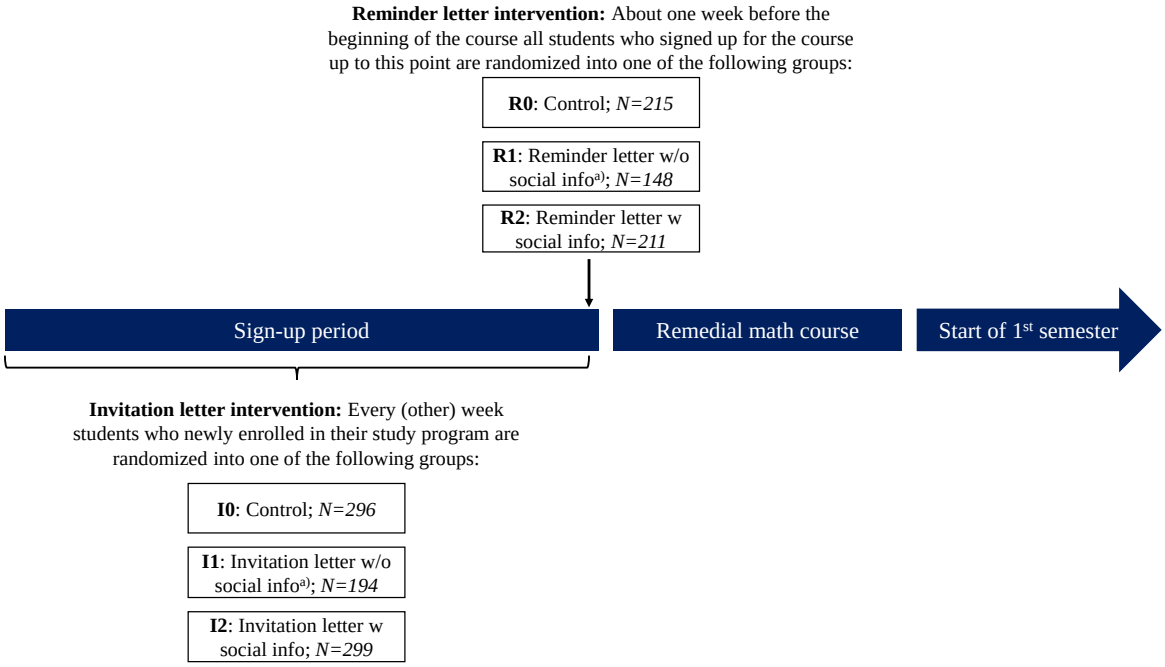
Note: Last month indicates whether the invitation letter was sent within the last month before the beginning of the remedial math course. *Outcome variables:* the index is the standardized inverse-covariance weighted average of the three overall performance measures (following Anderson (2008) and using 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 6: EFFECT OF REMINDER LETTER

	Participation 1st tutorial			Average participation		
	(1)	(2)	(3)	(4)	(5)	(6)
Letter	0.026	0.023	0.026	-0.018	-0.024	-0.019
	(0.033)	(0.032)	(0.032)	(0.036)	(0.034)	(0.034)
Letter & social info	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	d-p lasso	no	yes	d-p 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)

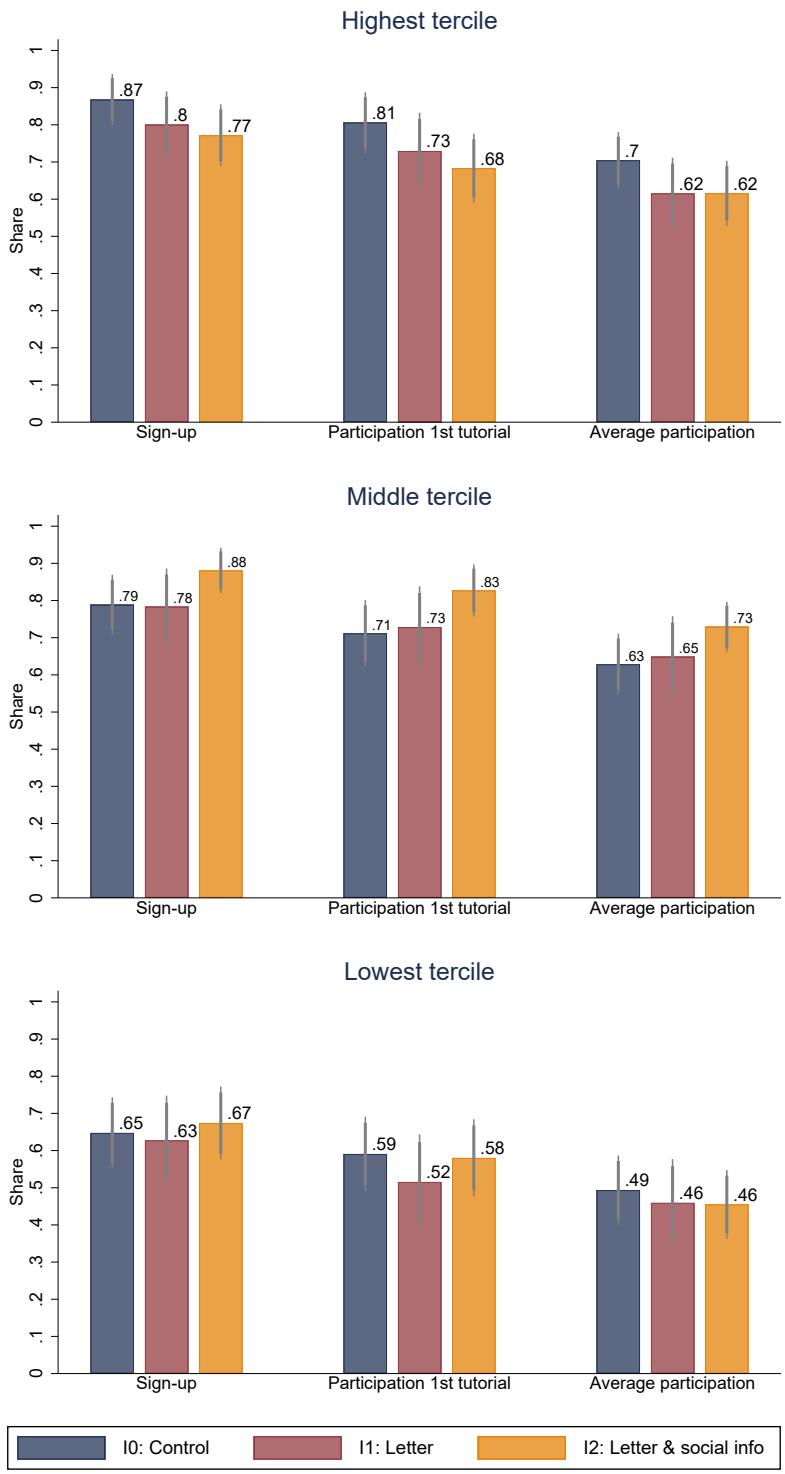
Note: Outcome variables: participation in first tutorial of remedial math course and average participation is the share of tutorials a student participated in; *strata:* study program FE, winter term dummy, interaction between study program FE and winter term dummy, and invitation letter treatment status 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



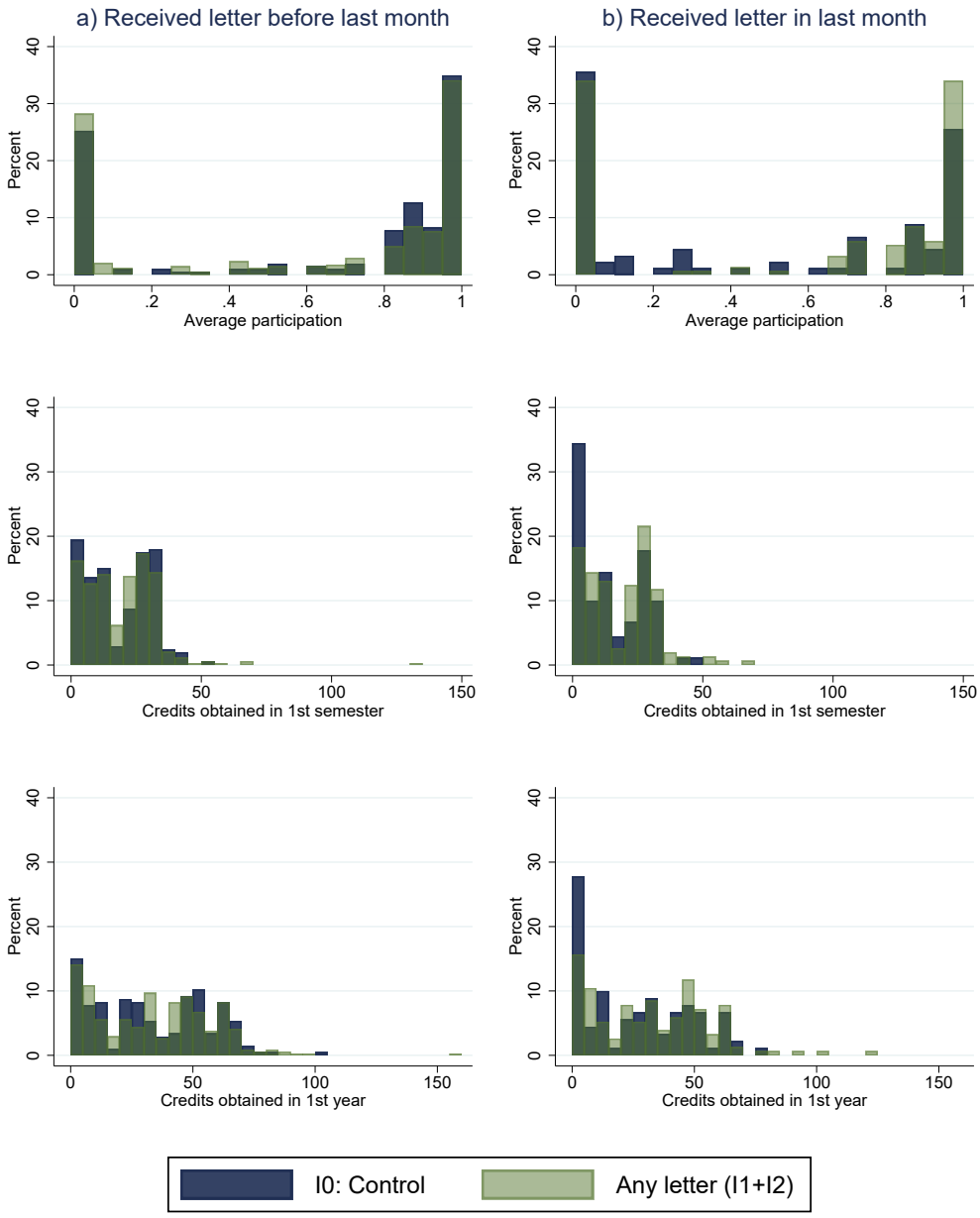
Note: [a)] As described in Section 2 the experimental groups who receive an invitation or reminder letter without the social information (I1 and R1) are only included in the winter cohort. The exact timing of the invitation letters and the respective number of observations are shown in Table B.2.

Figure 2: EFFECT OF INVITATION LETTER 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



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).

Appendix

A Additional tables and figures

Table A.1: DESCRIPTION OF VARIABLES

Variable	Description
Treatment Variables	
Letter (I1 or R1)	Random assignment to the treatment group that receives an invitation (reminder) letter without the social information. Treatments only included in the winter cohort.
Letter & social info (I2 or R2)	Random assignment to the treatment group that receives an invitation (reminder) letter that includes the social information.
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 the invitation letter.
Invitation letter treatment status	Indicators for the treatment status in the invitation letter intervention, such that the two randomizations are orthogonal to each other. Only included in analyses of the effects of the reminder letter.
Control Variables	
First university[a)]	Dummy that 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	Dummy 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 was sent to her, or could theoretically be sent to her in case of the control group.
Last month	Dummy that indicates whether the invitation letter was sent within the last month before the beginning of the remedial math course. Only used in the respective heterogeneity analysis.
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 her 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 (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 we 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: REGRESSION OF ACADEMIC ACHIEVEMENT ON REMEDIAL MATH COURSE PARTICIPATION – CONTROL GROUP OF INVITATION LETTER INTERVENTION (I0)

	Math exam			Overall performance		
	Attempted	Passed	Grade	Credits	Dropout	GPA
	(1)	(2)	(3)	(4)	(5)	(6)
a) First semester						
Average participation	0.530***	0.447***	-0.145	10.364***	-0.136***	0.012
	(0.069)	(0.068)	(0.203)	(1.766)	(0.046)	(0.137)
High school GPA	0.018	-0.108**	0.760***	-3.621**	-0.051	0.391***
	(0.052)	(0.054)	(0.132)	(1.553)	(0.035)	(0.098)
Strata	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
N	296	296	195	296	296	197
a) First year						
Average participation	0.501***	0.467***	-0.255	19.027***	-0.167***	-0.109
	(0.065)	(0.070)	(0.192)	(3.156)	(0.058)	(0.108)
High school GPA	0.026	-0.085	0.761***	-6.263**	-0.025	0.353***
	(0.053)	(0.054)	(0.124)	(2.676)	(0.041)	(0.083)
Strata	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
N	296	296	209	296	296	224

Note: Average participation is the share of tutorials a student participated in. 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); *strata:* study program FE, winter term dummy, interaction between study program FE and winter term dummy, and invitation letter date FE; *controls:* age, 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.3: EFFECT OF INVITATION LETTER – 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)
I1*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)
I2*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)
I1*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)
I2*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)
I1*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)
I2*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: 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 INVITATION LETTER 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)
I1: Letter	Lowest	-0.008	0.018	-0.175	-0.019	-0.031	-0.083
		(0.074)	(0.076)	(0.219)	(0.073)	(0.075)	(0.192)
	Middle	(0.073)	(0.079)	(0.182)	(0.071)	(0.080)	(0.176)
		0.056	0.126*	-0.179	0.064	0.082	-0.060
	Highest	(0.070)	(0.076)	(0.175)	(0.066)	(0.076)	(0.159)
		0.032	-0.053	0.400**	0.000	-0.069	0.413***
I2: Letter & social info	Lowest	0.027	-0.028	0.145	0.007	-0.035	0.109
		(0.064)	(0.065)	(0.164)	(0.063)	(0.065)	(0.147)
	Middle	-0.065	-0.048	-0.047	-0.058	-0.081	0.051
		(0.065)	(0.068)	(0.146)	(0.062)	(0.067)	(0.137)
	Highest	0.007	0.145**	-0.361**	-0.018	0.036	-0.193
		(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
I1: P-value F-test int. term		0.812	0.337	0.438	0.664	0.458	0.719
I2: P-value F-test int. term		0.539	0.054	0.003	0.797	0.373	0.010

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 INVITATION LETTER 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)
I1: Letter	Lowest	-0.093	-0.103	0.023	0.081	-0.028	-1.657	-0.002	0.116
		(0.138)	(2.203)	(0.039)	(0.112)	(0.135)	(3.848)	(0.047)	(0.105)
	Middle	0.101	1.366	-0.024	-0.119	0.027	2.160	-0.002	-0.092
		(0.170)	(2.004)	(0.048)	(0.105)	(0.184)	(3.683)	(0.066)	(0.093)
	Highest	0.078	1.945	-0.020	0.114	0.095	3.053	-0.027	0.004
		(0.156)	(1.751)	(0.044)	(0.105)	(0.159)	(3.330)	(0.060)	(0.093)
I2: Letter & social info	Lowest	-0.013	0.412	0.001	0.138	-0.104	-0.165	0.036	0.164
		(0.107)	(1.897)	(0.029)	(0.112)	(0.121)	(3.224)	(0.044)	(0.107)
	Middle	0.104	0.283	-0.029	-0.003	-0.048	-0.427	0.021	0.004
		(0.146)	(1.759)	(0.041)	(0.099)	(0.151)	(3.131)	(0.056)	(0.084)
	Highest	0.100	4.160**	-0.022	-0.088	0.046	5.154*	0.013	-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
I1: P-value F-test int. term		0.575	0.767	0.654	0.228	0.837	0.634	0.937	0.318
I2: P-value F-test int. term		0.745	0.224	0.809	0.307	0.739	0.359	0.947	0.119

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 OUTCOMES OF CONTROL GROUP STUDENTS BY ENDOGENOUS STRATA (INVITATION LETTER INTERVENTION)

	(1)	(2)	(3)
Tercile	Lowest	Middle	Highest
Sign-up	0.614	0.790	0.884
Participation first tutorial	0.554	0.720	0.821
Average participation	0.461	0.631	0.721
Math attempted first semester	0.554	0.710	0.716
Math attempted first year	0.614	0.740	0.768
Math passed first semester	0.416	0.540	0.453
Math passed first year	0.495	0.630	0.589
Math grade first semester	3.436	3.662	3.850
Math grade first year	3.377	3.507	3.653
Performance index first semester	0.137	-0.060	-0.083
Performance index first year	0.138	-0.021	-0.125
Credits first semester	16.530	17.375	15.137
Credits first year	30.564	32.395	28.463
Dropout first semester	0.050	0.100	0.105
Dropout first year	0.099	0.180	0.189
GPA first semester	2.863	3.061	3.057
GPA first year	2.832	2.946	3.056

Table A.7: EFFECT OF REMINDER LETTER – 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)
R1*first university	0.172	0.164	0.199*	0.201**
	(0.112)	(0.108)	(0.105)	(0.096)
R2*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)
R1*female	-0.016	-0.025	-0.049	-0.047
	(0.068)	(0.068)	(0.069)	(0.067)
R2*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: 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 invitation letter 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: INVITATION LETTER – 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.2: REMINDER LETTER – 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

Invitation letter intervention. In both cohorts, starting five to seven weeks before the beginning of the remedial math course until one week before, we used administrative data on the incoming students provided to us 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[30], we 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, we 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, we 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.

Reminder letter intervention. About one week before the start of the course (see Figure 1), we 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, we performed the randomization using stratification and re-randomization. Strata were constructed based on study program, the information about the treatment status in the invitation letter randomization – such that the two randomizations were 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
Invitation letter											
I0: Control	60	18	12	14	104	101	25	18	16	32	192
I1: Letter	-	-	-	-	-	101	26	19	15	33	194
I2: Letter & social info	56	20	13	15	104	103	26	17	17	32	195
N	116	38	25	29	**208**	305	77	54	48	97	**581**
Reminder letter											
R0: Control	40	11	8	6	65	80	19	17	12	22	150
R1: Letter	-	-	-	-	-	79	21	14	14	20	148
R2: Letter & social info	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.

[30]For the second experiment we 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

	Summer Term						Winter Term					
Days until course	37	29	23	16	9	N	49	40	28	14	7	N
I0: Control	74	17	7	4	2	104	79	53	44	13	3	192
I1: Letter	-	-	-	-	-	-	84	49	44	13	4	194
I2: Letter & social info	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 – INVITATION LETTER, SUMMER TERM

	(1) I0: Control mean (std. dev.)	(2) I2: Let. & 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	0.154	-0.045	0.327
	(0.363)	(0.046)	
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 – INVITATION LETTER, WINTER TERM

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	I0: Control mean (std. dev.)	I1: Letter coefficient (robust SE)	p-Value	I2: Let. & soc. info coefficient (robust SE)	p-Value	I1-I2 = 0 (robust SE)	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 – REMINDER LETTER, SUMMER TERM

	(1) R0: Control mean (std. dev.)	(2) R2: Let. & 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	0.185	-0.105*	0.076
	(0.391)	(0.059)	
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 – REMINDER LETTER, WINTER TERM

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	R0: Control mean (std. dev.)	R1: Letter coefficient (robust SE)	p-Value	R2: Let. & soc. info coefficient (robust SE)	p-Value	R1 - R2 = 0 (robust SE)	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 Statistical power

One potential concern with our study could be that it has not enough statistical power to detect effect sizes that are usually found in studies that employ social information. Table C.1 shows minimal detectable effect sizes (MDES) for $1-\beta = 0.6$ and $1-\beta = 0.8$, assuming $\alpha = 0.05$, $R^2 = 0.21$ ($R^2 = 0.10$), and $N = 600$ ($N = 400$) for the invitation (reminder) letter intervention. We performed the power calculations after the field experiment in the summer term, as it was clear that for the summer cohort alone we would not have enough power to detect reasonable effect sizes. The assumed R^2 are based on OLS estimations using the control group of the summer cohort that regress sign-up and participation in the first tutorial on the covariates that we pre-registered for the stratification procedure in the winter cohort (see Section 2.2). The assumed number of observations refers to the number of observations that we expected to have for the comparison of control versus the treatment group that received a letter with social information after pooling observations from both cohorts. In addition, we use information on the control group take-up rates in the summer cohort to calculate expected effect sizes in terms of percentage points.

For the invitation letter intervention our study has 60% power to detect effects sizes of 0.16 standard deviations, which, taking the observed sign-up and participation rates in the control group into account, corresponds to effects of 7.4 to 7.7 pp. With a power of 80%, we would be able to detect effects of 0.20 standard deviations or 9.3 to 9.6 pp. For the reminder letter intervention our MDES corresponds to 0.21 (0.27) standard deviations or 5.6 (7.2) pp, with a power of 60% (80%).

To evaluate these MDESs, we can compare them to effect sizes that are typically found in studies that make use of social information and reminders. As this is the first study that makes use of this type of social information in the context of education, we can only compare our study to other nudges that are employed in this context. For information on typical effect sizes we draw on the meta-analysis by DellaVigna and Linos (2020).[31] For studies that rely on social cues that are published in academic journals they report effect sizes of 13.81 pp or about 0.30 standard deviations (see also Table C.2). Our study is well powered to detect effects of this size. For studies that make use of pure reminders or that are set in the context of education, they report effect sizes of 5.02 and 2.56 pp (0.12 and 0.05 standard deviations), respectively. While our study does not necessarily have enough power to detect these kind of effect sizes, we should be able to detect effect sizes between those and the ones reported for social cues.

DellaVigna and Linos (2020) also report effect sizes for interventions that were conducted by Nudge Units in the US. The average effects sizes of these interventions are substantially smaller than those published in academic journals (see DellaVigna and Linos 2020 and Table C.2), and our study is not well-powered enough to detect these kind of effects sizes (see the related discussion in Section 5).

[31] The study by DellaVigna and Linos (2020) includes papers that are also covered in the meta-analysis by Hummel and Maedche (2019), which we initially used to evaluate our MDESs.

Table C.1: MINIMUM DETECTABLE EFFECT SIZES ($\alpha = 0.05$)

Invitation letter ($R^2 = 0.21$)				*Reminder letter* ($R^2 = 0.10$)			
$1-\beta$	N	MDES	in pp	$1-\beta$	N	MDES	in pp
Effect on sign up							
Control take-up=68.3%							
0.6	600	0.16	7.44				
0.8	600	0.20	9.31				
Effect on participation 1st tutorial							
Control take-up=63.5%				Control take-up=92.3%			
0.6	600	0.16	7.70	0.6	400	0.21	5.60
0.8	600	0.20	9.63	0.8	400	0.27	7.20

Note: The power calculations were performed after the results for the summer cohort were available. The assumed R^2 are based on control group OLS regressions of the outcome variables sign-up and participation in first tutorial on the covariates that we pre-registered for stratification in the field experiment with the winter cohort. For the invitation (reminder) letter $N = 600$ ($N = 400$) refers to the number of observations that we expected to have for the comparison of control versus letter with social information after pooling observations from both cohorts. The depicted control group take-up rates are from the summer cohort. The calculation of the MDES in pp is based on the following formula: $MDES \times \sqrt{(\text{control take-up}) \times (100 - \text{control take-up})}$. Power calculations were performed with Optimal Design (Spybrook et al., 2011).

Table C.2: EXPECTED EFFECT SIZES IN THE LITERATURE – BASED ON DELLAVIGNA AND LINOS (2020)

	Academic Journals			Nudge Units		
	Control take-up (%)	ATE (pp)	Effect size (sd)	Control take-up (%)	ATE (pp)	Effect size (sd)
Education	66.16	2.56	0.054	14.39	0.49	0.014
Reminders	25.17	5.02	0.116	27.29	2.56	0.058
Social cues	31.11	13.81	0.298	18.05	0.96	0.025

Note: Numbers from tables 1a and 1b in DellaVigna and Linos (2020). Effect size in standard deviations (sd) is calculated as according to $\frac{ATE}{\sqrt{(Take-up)\times(100-Take-up)}}$

D Pre-registered analyses by cohort

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

D.1 Summer cohort

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

Main analyses. To address the main research questions, Table D.1 reports effects of the invitation letter with social information on sign up for the remedial math course and Table D.2 reports effects of the reminder letter on participation in the course.

Secondary analyses. Since we did not receive the respective information, we 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 D.3 reports results regarding the interaction of the two interventions. Tables D.4 and D.5 report heterogeneous effects of the interventions with respect to the time at which the invitation letter was sent and whether this is the first semester at any university.

Table D.1: EFFECT OF INVITATION LETTER

	Sign-up	
	(1)	(2)
I2: Letter & social info	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 D.2: EFFECT OF REMINDER LETTER

	Part. 1st tutorial		Average part.	
	(1)	(2)	(3)	(4)
R2: Letter & social info	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, invitation letter 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 D.3: EFFECT OF REMINDER LETTER BY INVITATION LETTER TREATMENT STATUS

	Part. 1st tutorial		Average part.	
	(1)	(2)	(3)	(4)
R2: Reminder & social info	0.067	0.030	-0.016	-0.065
	(0.046)	(0.040)	(0.075)	(0.072)
I2: Invitation & social info	-0.030	-0.058	-0.037	-0.066
	(0.068)	(0.065)	(0.071)	(0.071)
R2*I2	-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 D.4: EFFECT OF INVITATION LETTER – HETEROGENEITIES

	Sign-up			
	Last month		First university	
	(1)	(2)	(3)	(4)
I2: Letter & social info	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)		
I2*last month	-0.052	-0.098		
	(0.125)	(0.120)		
First university			0.321***	0.283***
			(0.086)	(0.092)
I2*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 D.5: EFFECT OF REMINDER LETTER – HETEROGENEITIES

	Last month				First university			
	Part. 1st tutorial		Average part.		Part. 1st tutorial		Average part.	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
R2: Letter & social info	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)				
R2*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)
R2*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, invitation letter 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. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

D.2 Winter cohort

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

Main analyses. To address the main research questions, Table D.6 reports effects of the invitation letter intervention on sign up for and participation in the remedial math course and Table D.7 reports effects of the reminder letter intervention on participation in the course.

Secondary analyses. Since we did not receive the respective information, we 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 D.8, D.9, and D.10 report on the heterogeneous effects of the invitation letter intervention and Tables D.9 and D.10 on the heterogeneous effects of the reminder letter intervention.

Table D.6: EFFECT OF INVITATION LETTER

	Sign-up		Part. 1st tutorial		Average part.	
	(1)	(2)	(3)	(4)	(5)	(6)
I1: Letter	-0.038	-0.038	-0.049	-0.043	-0.042	-0.038
	(0.039)	(0.039)	(0.043)	(0.042)	(0.040)	(0.039)
I2: Letter & social info	0.002	0.002	-0.001	0.002	-0.010	-0.007
	(0.038)	(0.038)	(0.042)	(0.042)	(0.040)	(0.038)
I2-I1	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	d.-p. LASSO	no	d.-p. LASSO	no	d.-p. 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 D.7: EFFECT OF REMINDER LETTER

	Part. 1st tutorial		Average part.	
	(1)	(2)	(3)	(4)
R1: Letter	0.024	0.024	-0.008	-0.009
	(0.034)	(0.033)	(0.037)	(0.036)
R2: Letter & social info	0.025	0.025	0.034	0.034
	(0.035)	(0.033)	(0.036)	(0.035)
R2-R1	0.001	0.001	0.043	0.043
	(0.033)	(0.031)	(0.034)	(0.033)
Strata	yes	yes	yes	yes
Controls	no	d.-p. LASSO	no	d.-p. 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, invitation letter 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 D.8: EFFECT OF INVITATION LETTER BY DATE OF ENROLLMENT

	Sign-up		Part. 1st tutorial		Average part.	
	(1)	(2)	(3)	(4)	(5)	(6)
I1: Letter	-0.059	-0.057	-0.071	-0.064	-0.082*	-0.078
	(0.047)	(0.046)	(0.051)	(0.050)	(0.048)	(0.048)
I2: Letter & social info	-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)
I1*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)
I2*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 D.9: EFFECT OF INVITATION LETTER BY FIRST UNIVERSITY

	Sign-up		Part. 1st tutorial		Average part.	
	(1)	(2)	(3)	(4)	(5)	(6)
I1: Letter	-0.098	-0.089	-0.096	-0.081	-0.049	-0.039
	(0.104)	(0.103)	(0.105)	(0.103)	(0.094)	(0.091)
I2: Letter & social info	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)
I1*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)
I2*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 D.10: EFFECT OF INVITATION LETTER BY GENDER

	Sign-up		Part. 1st tutorial		Average part.	
	(1)	(2)	(3)	(4)	(5)	(6)
I1: Letter	-0.011	-0.015	-0.068	-0.072	-0.072	-0.079
	(0.049)	(0.049)	(0.054)	(0.054)	(0.049)	(0.049)
I2: Letter & social info	-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)
I1*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)
I2*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 D.11: EFFECT OF REMINDER LETTER BY FIRST UNIVERSITY

	Part. 1st tutorial		Average part.	
	(1)	(2)	(3)	(4)
R1: Letter	-0.100	-0.102	-0.182	-0.189*
	(0.122)	(0.117)	(0.114)	(0.106)
R2: Letter & social info	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)
R1*first university	0.149	0.148	0.208*	0.214*
	(0.127)	(0.121)	(0.121)	(0.112)
R2*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, invitation letter 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 D.12: EFFECT OF REMINDER LETTER BY GENDER

	Part. 1st tutorial		Average part.	
	(1)	(2)	(3)	(4)
R1: Letter	0.043	0.044	0.029	0.026
	(0.041)	(0.041)	(0.046)	(0.046)
R2: Letter & social info	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)
R1*female	-0.049	-0.060	-0.094	-0.093
	(0.072)	(0.074)	(0.076)	(0.074)
R2*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, invitation letter 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. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.