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CAN DEFAULTS CHANGE BEHAVIOR WHEN POST-INTERVENTION EFFORT IS REQUIRED?

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Abstract

Nothing is known about the effectiveness of defaults when moving the target outcome requires substantial post-intervention effort. With two field experiments in higher education, we investigate how defaults fare in such situations. We change the exam sign-up procedure to "opt-out" for a single exam (Exp 1), and for many exams (Exp2). Both interventions increase sign-up at the beginning of the semester. Downstream, at the end of the semester, opt-out increases task completion (exam participation) for a single exam but not for many exams. For the single exam, effects on passing (successful task completion) are heterogeneous: students responsive to unrelated university requests convert increased sign-ups into passed exams. For non-responsive students, increased sign-ups result in failed exams due to no-shows. Defaults can thus be effective but need to be carefully targeted.

Keywords: Default, Randomized Field Experiment, Higher Education

JEL Classification: C93, D02, D91, I23, J18, J24

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1 Introduction

Default options affect the choices people make: the probability of choosing an option often increases when it is pre-selected. Evidence for this behavioral effect comes from diverse contexts such as organ donation (Abadie and Gay, 2006, Johnson and Goldstein, 2003, Johnson and Goldstein, 2004), retirement savings (Choi et al., 2004, Madrian and Shea, 2001), environmental-friendly behavior (Egebark and Ekström, 2016), or charitable giving (Altmann et al., 2019). In two meta-analyses, Jachimowicz et al. (2019) and Mertens et al. (2022) estimate the effect size of changing the default setting from opt-in to opt-out to be 0.68 and 0.62 (Cohen's d and Cohen's h).

Default interventions are typically characterized by a reduction in the future cost of performing a target behavior and a short lag between intervention and target behavior (Rogers and Frey, 2014). In fact, most of the evidence on default effects comes from interventions where staying with the default and not taking further post-intervention action is sufficient to reach the target outcome, e.g. automatically contributing to a retirement savings plan avoids the effort and attention costs otherwise needed to continue saving in the future. Nothing is known, however, about whether defaults can also be effective when neither of these features is present, and decision makers must repeatedly invest substantial amounts of time and effort after the intervention in order to reach the target outcome.[1]

Opt-out defaults could provide a policy alternative in many relevant domains that require effort and which typically rely on individuals opting into voluntary participation. Examples include on-the-job training and training programs for the unemployed. Take-up of such programs is often low, especially among those who need them most (Salamon et al., 2021; Sousa-Ribeiro et al., 2018; Scherer et al., 2021). Similarly, higher level corporate goals such as diversity, inclusion and equity are often a top priority, but results turn on encouraging management to take part in coaching and leadership development (G. Cox and Lancefield, 2021). On a more general level, there is a societal need for policy measures to increase participation in volunteer work, especially with aging populations (Niebuur et al., 2018), and opt-out could be one such measure. Finally, in the education domain, engaging in extracurriculars in school can increase social mobility, and applying an opt-out rule could potentially help close existing "engagement gaps" (Snellman et al., 2015).

Our paper is the first to investigate how defaults fare in such situations by focusing on the economically and socially important area of higher education. This makes our paper also the first to evaluate whether defaults can directly affect choices and outcomes in education.[2] Our intervention changes the choice architecture when students sign up for exams.

[1] For example, none of the experiments in the meta-analysis by Jachimowicz et al. (2019) require continuous and substantial investment of time and effort.

[2] A few papers examine the *indirect* effects of defaults in education, e.g. automatic enrollment for student

Many students do not sign up for and pass the necessary number of exams per semester, leading to delayed graduation – a problem in colleges and universities worldwide.[3] [4] Typically, students are required to actively enroll for exams. We change this standard opt-in rule to an opt-out rule, where students are automatically enrolled for scheduled exams but can drop them if they want to. In a first step, we assess whether the opt-out rule increases the number of exam sign-ups at the beginning of the semester, i.e. whether *standard default effects* can be found in an education setting. We use the term standard default effect when the desired outcome can be achieved without expending substantial effort, i.e. even if the decision maker stays largely passive. Opting into more exams is, however, not sufficient for staying on track to graduation; rather the exams need to be taken and passed. In a second step, we thus go beyond standard default effects and assess effects on exam participation and passing – outcomes which require considerable continuous post-intervention investment of time and effort in the form of participating in lectures and studying for the exam. We label these effects *downstream default effects*.

We conduct two separate preregistered field experiments. In the first experiment, the treatment consists of signing up first-semester business administration students for all exams that the curriculum of their program recommends in the first semester. The university recommendation corresponds to the standard 30 credits courseload in the European university system.[5] In the following we refer to this approach as a *broad default* as it affects not only a single course or exam and aims to have students stay on track by collecting all first semester credits.[6] The second experiment is a conceptual replication, where we examine the effect of a *targeted default* intervention in a new cohort of business students: we automatically sign up second semester students only for the statistics exam, a second semester principles class that is viewed as challenging.

Our first finding is that standard default effects can be observed when the target task requires substantial effort. The broad and the targeted default increase exam sign-ups after the sign-up period by 0.27 exams and 5 percentage points (pp), respectively. With a broad

loans and its effects on academic progress; see the literature section.

[3]In the US and other OECD countries less than 40% of bachelor students graduate within the scheduled time (see OECD, 2019). Similarly, in Germany only 46% of students in bachelor's degree programs graduate within the designated time period (see Federal Statistical Office of Germany, `https://www.destatis.de/DE/Presse/Pressemitteilungen/2016/05/PD16_181_213.html`, retrieved on October 28th, 2021).

[4]This has also motivated interventions, especially in the United States, that encourage students to aim for taking the prescribed number of credits per semester, such as *15 to Finish* (see `https://completecollege.org/strategy/15-to-finish/`, retrieved on August 20, 2021).

[5]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/european-credit-transfer-and-accumulation-system-ects_en`, retrieved on November 12th, 2021.

[6]Only 38% of students in the control group did not fall behind the recommended 30 credit points in the first semester.

default this effect on sign-ups vanishes by exam day, whereas it persists with the targeted default. This already indicates aiming defaults at specific tasks as a necessary condition with effortful tasks. We discuss potential reasons in the paper.

Beyond the standard default effects, we find that further downstream, when investment of time and effort is necessary to alter outcomes, the broad default shows no effects. The targeted default, however, significantly increases actual participation in the statistics exam by 6.5 pp, and the point estimate of the effect on passing is positive (3.6 to 3.8 pp) but imprecisely estimated.

In an explorative analysis, we analyze the heterogeneity of the effects in the targeted default experiment. Recent research shows that (i) in the lab the alignment of interests between defaultee and default setter strongly predicts default effectiveness (Altmann et al., 2021); (ii) high responsiveness is linked to larger nudge effects (Heffetz et al., 2021). We therefore focus on the group of *responsive* students, which comprises those who responded to unrelated requests from the university to take part in a survey collecting feedback on students' study experience (40% of students responded). Responding to the survey request shows that these students are open and responsive to communication from the university, and motivated to provide feedback. We argue that the interests of these students are likely also better aligned with the interests of the default setter (the university) than the interests of those who are unresponsive to the requests. Specifically, we argue that the university and the responsive students have aligned interests when it comes to (quick) completion of the program. We thus expect the exam opt-out rule to be more effective for the responsives.

We find that for responsives in addition to standard default effects (up to 8 pp more sign-ups), the automatic sign-up also increases (successfull) task completion, i.e. participation (16.5 pp) and passing (11.2 to 12.2 pp). On top of the standard default effects on sign-ups, for responsives the default can therefore have strong effects on downstream outcomes which require substantial post-treatment investments from the individual.

It is interesting that exam participation of responsives increases by more than just their increase in sign-ups (16 pp vs 8 pp). We show that this is driven by a large reduction in exam no-shows among the treated responsives. This is relevant on a more general level, because it means that defaults can positively affect the outcomes downstream even for individuals who would have signed up under an opt-in rule anyway. The implication is that not all sign-ups are equally binding: own sign-ups result in no-shows at a higher rate than sign-ups initiated by the university; apparently the barrier to opting out is higher when this overrides the selection made by the choice architect. A choice generated by a default is thus more binding than the same choice which is made under opt-in, i.e. one of the channels that makes defaults effective is that defaults can increase the bindingness of a choice even without changing the choice.

The finding that the default changes outcomes for responsive students does not mean that only strong performers benefit. In fact, our results suggest that responsiveness is a category distinct from ability: among the responsives, those who benefit most had a lower first semester performance. For these students we see much larger effects on sign-up, participation and passing of the statistics exam. Among the non-responsive students we see a similar pattern concerning sign-ups: the lowest performers in the first semester have the highest increases in sign-ups. However, the consequences of the increased sign-ups downstream are vastly different between non-responsive and responsive students. For the low-achieving responsive students the increase in sign-ups translates into a higher rate of participation and passing. For the low-achieving non-responsive students, on the other hand, increased sign-ups do not turn into higher participation but rather into a higher rate of failed exams due to no-shows. Defaults are thus beneficial to low-achieving responsives but they do not help (and might even hurt) low-achieving individuals who are not responsive in the first place. This suggests that responsiveness is indeed the driving factor, and it stresses that it is important to target default interventions.

Survey data also shows that effort is the mechanism behind the effects we see for responsives: in line with the positive effects on exam passing, automatic sign-up for the statistics exam increases attendance in the statistics course/tutorial and time spent preparing for statistics outside of class. This plausibly contributes to the increased pass rates we observe.

Because in the targeted default the automatic sign-up only affects one specific task (the statistics exam), it is also important to consider the entire universe of performance and check for potential substitution effects. We find no evidence that the responsives obtain a worse statistics grade or a lower overall semester grade point average, and no evidence that they sign up for fewer exams or obtain fewer credits in classes other than statistics – their observed response to the opt-out default can therefore be interpreted as a net positive effect.

Contribution to the literature. This paper contributes to the literature in three main ways. First, our study investigates for the first time whether and under which conditions defaults can affect target outcomes that require substantial and ongoing post-treatment investment of time and effort by individuals. In contrast, the literature to date has focused on defaults that meet two main conditions: reduction in the future cost of performing a target behavior and a short lag between intervention and target behavior (see Rogers and Frey, 2014). For example, none of the 58 experiments (35 papers) included in the review article by Jachimowicz et al. (2019) require continuous and substantial investment of time and effort. In fact, only six of the studies need any post-intervention action, and, unlike attending class, studying, and taking an exam, all of these activities are one-off and demand very little effort (Chapman et al. (2010) and Narula et al. (2014) automatically schedule doctor's appointments, Elkington et al. (2014), Jin (2011), and Trevena et al. (2006) default people into survey participation, Loeb et al. (2017) recruit individuals for a one-time behavior that benefits health).

Second, there is no research on whether defaults can *directly* affect academic choices and outcomes in education. The literature in this field has so far only indirectly targeted academic outcomes: Bergman et al. (2020) use an opt-out rule to sign up parents of high-school students for a program in which they receive weekly text messages when their child's performance drops. Automatic enrollment of parents subsequently also improves student achievement in terms of grades and course passing. Kramer II et al. (2021) investigate default effects on financial choices of students and find that automatic enrollment for education loans increases the likelihood of borrowing; this has no effects on academic performance. In a lab experiment, J. C. Cox et al. (2020) find that changing the default student loan repayment plan to the less risky option strongly increases choosing that plan.

Our result that changing the exam sign-up procedure in higher education from an opt-in rule to an opt-out rule leads to more sign-ups can be interpreted as the equivalent of standard default effects in other contexts, which require no post-intervention action of the defaulted person (e.g. Abadie and Gay, 2006, Choi et al., 2004, Dinner et al., 2011, Johnson and Goldstein, 2003, Madrian and Shea, 2001). The magnitude of our effects is small compared to the literature on defaults (see, e.g., Jachimowicz et al., 2019 and Mertens et al., 2022), consistent with the finding that behavioral interventions in general exhibit smaller effect sizes in education settings (see, e.g., DellaVigna and Linos, 2020 and Kraft, 2020). Our study is also the first to show that for a specific group, defaults can improve downstream outcomes which require effort, and the first to show that in this group of responsive individuals important education outcomes benefit from the changed default.

Third, the finding that responsive students react particularly well to defaults contributes to the research on the mechanisms driving default effects. Recently, Altmann et al. (2021) have shown in the lab that defaults are more effective in changing behavior when the interests of the choice architect and the decision maker are aligned.[7] Our results provide evidence from the field in support of these findings. Our study also contributes to the literature which investigates heterogeneous effects of behavioral interventions, specifically responsiveness and its consequences for the effectiveness of nudges (see, e.g., Heffetz et al., 2021 for a reminder setting).

The remainder of this paper is structured as follows: Section 2 reports the design, procedure, and results of the broad default intervention. Section 3 reports the same for the targeted default and explores mechanisms. Section 4 concludes.

[7] A similar mechanism is shown in Tannenbaum et al. (2017): defaults are less effective when the decision maker believes the default-setter to have a misaligned position on the issue in question.

2 Field Experiment I: Broad Default

Both experiments were conducted at one of the largest universities of applied sciences in Germany.[8] The interventions were implemented and outcomes collected before any Corona-related restrictions.

The first experiment included the entire first-semester cohort of the bachelor's program Business Administration (BuA). BuA is one of the largest programs offered at our university and also the most popular program in all of German higher education – roughly 8% of all first year students in German higher education choose BuA (Statistisches Bundesamt Deutschland, 2020).

2.1 Research Design

Students in the BuA first-semester cohort were randomized into two exam sign-up regimes: opt-in (the standard procedure) and opt-out (automatic sign-up, i.e. the treatment group). Randomization was carried out by stratifying on high school GPA and balancing on the covariates displayed in Table A.1 in the Appendix (Morgan and Rubin, 2012).[9] The table shows that all variables are balanced between the control and the treatment group.

Students in the treatment group were automatically signed up for all six exams (= broad default) that the university recommends in the study plan for the first semester: Mathematics, Business Administration, Corporate Management, Accounting, Microeconomics, and Business Informatics.[10] Students are generally free to defer exams to later semesters without immediate consequences. Only Math and Business Administration are part of the orientation exams. If students do not sign up for and take the orientation exams in the first semester, these exams will count as failed.

The study plan is made salient by the university at the beginning of the semester, in introductory lectures, by tutors, in documents on the website and through the letter sent to the control group as well as the treatment group as part of the experiment (details below). The information is therefore familiar to all students in the control and treatment group and should not have any effects.

Students in the opt-out group could de-register from the exams they were signed up for. In the control group students had to actively sign up for exams themselves (opt-in), which is

[8]The university consists of 13 faculties and offers more than 20 bachelor's degree programs and a variety of master's programs. It has a student population of more than 13,000 students, and more than 2,700 full-time, first-time undergraduate students enroll each year.

[9]Variable descriptions are provided in Table A.2 in the Appendix.

[10]Each of the six exams yields 5 credit points. The recommendations are in accordance with the standard 30 credits courseload in the European university system see also also `https://ec.europa.eu/education/resources-and-tools/european-credit-transfer-and-accumulation-system-ects_en`, retrieved on November 12th, 2021.

the standard procedure in German higher education.

2.1.1 Procedure

In the week before the semester we informed students in both the treatment and the control group about the exam registration procedure that applies to them (via postal mail and e-mail; the letters are displayed in in the Online Material; a timeline of the broad default experiment is provided in Figure A.1 in the Appendix). The letters for both the treatment and control groups also included an outline of the study plan for the first and second semester.

Students in the control group could sign up for exams online during a two-week period, three weeks into the semester. During the same time interval and via the same online tool, students in the treatment group had the opportunity to de-register from the exams they were automatically signed up for and could also sign up for additional exams. In the tables and figures we refer to this period as the sign-up period. Three weeks after the end of the sign-up period, during a week-long de-registration period students could withdraw from exams they signed up for (or were signed up for by default). After this point, an exam registration can still be dropped if a doctor's note is provided. A sign-up on exam day will be graded as failed if the student does not participate in the exam.

2.1.2 Outcomes

We study the process from exam sign-up to passing or failing the actual exams. An important distinction we make is between *standard default effects* and *downstream effects*. We call a standard default effect one where no further action is required by the individual to reach the desired outcome: in our setting the relevant outcome for the standard default effect is the number of exam sign-ups. We measure exam sign-ups at two points in time: 1) five weeks into the semester after the sign-up period and, 2) on the day of the exam.[11] Staying signed up does not require any post-default action or effort from the individuals, and a higher number of exam registrations can be considered desirable (as registration is a prerequisite for passing an exam).

Downstream effects go beyond the standard default effects, and we use the number of passed exams to measure them. Changing the number of passed exams requires significant post-intervention effort by students, in the form of studying and taking the exam.

To investigate potentially negative spillover effects on other performance dimensions we also preregistered to study treatment effects on the 1st semester GPA, failed exams and the overall number of acquired credit points (see Table 3). Failed exams comprise fails due to

[11] Note that because some students do not show up for the exams, sign-up on the day of the exam is not equivalent to actual exam participation. Information on participation is only available to the lecturer of a course and not available in the administrative data provided by the university.

insufficient performance upon participation, fails due to no-shows, and failed exams due to non-sign-ups for orientation exams.[12]

2.2 Results

The official recommendation of the university is that students in the first semester sign-up for, and pass the six exams mentioned in the study plan. Overall, only 81% of control group students sign up for all six exams (see Figure A.2). This means that a few weeks into the first semester already at least 19% of the students are not on track to graduate in the recommended time frame. Further downstream, at the end of the semester, rate of successful task completion is substantially lower: only 38% pass all six recommended exams.

On top of the more general question whether defaults work when effort is required, from an education perspective, our intervention can prevent students from falling behind early on and keep them on track towards a timely and successful degree completion.[13] Signing up is a prerequisite for passing an exam and so we will first assess the effect the opt-out sign-up procedure has on the number of exams signed up for (standard default effect). We then evaluate whether a potentially higher number of exam registrations can lead to more passed exams (downstream default).

We report results based on the following OLS specification:

$$Y_i^k = \alpha_0 + \alpha_1 Treatment_i + \boldsymbol{x_i \alpha_2} + \boldsymbol{z_i \alpha_3} + \varepsilon_i, \tag{1}$$

where Y_i^k denotes the outcome k for individual i. $Treatment_i$ is a binary indicator for being randomized into the treatment group and α_1 identifies the effect of the opt-out sign-up rule. We provide estimates that control for the method of randomization (see e.g., Bruhn and McKenzie, 2009), by reporting a preregistered specification using strata dummies x_i, as well as a second preregistered specification that adds a covariate vector z_i consisting of the balancing variables accounting for the ability and background of students (high-school GPA, gender, age, application date, enrollment date, German citizenship, and university semesters prior to the current study program).

[12] As stated above Math and Business Administration are orientation exams which are graded as failed if students do not sign up for these exams. Although this should be common knowledge among students, there were a few who did not sign up for at least one of the two exams and consequently received a fail. As can be seen in Figure A.2 and Tables 1 and 3 this results in a small difference between the number of sign-ups on the day of the exam and the sum of passed and failed exams.

[13] For example, Angrist et al. (2020) show that an increase in credits earned in the first year of college translates into higher degree completion for students studying towards a Bachelor's degree.

2.2.1 Standard Default Effects

Figure 1 shows the mean number of exam sign-ups in the opt-out and the opt-in group after the sign-up period. All sign-ups are set to zero by the university as soon as a student drops out, which avoids an upward bias in the standard default effect due to inactive students who can no longer de-register. Students in the control group are signed up on average for 5.41 exams. The mean number of exams signed up for is roughly 0.27 exams higher in the treatment group, at 5.69. Regression results in Table 1 confirm these raw descriptive comparisons: we find a statistically significant increase of sign-ups in the opt-out group after the sign-up period of roughly 0.27 exams. As shown in Table 2, the effects on sign-up are positive for all six exams, and statistically significant for four of the six exams.

It is important to stress again that sign-up effects may be interpreted as conceptually analogous to most default effects in the literature (which require no further actions to reach the desired outcome). We thus show that such standard default effects can be found with tasks where post-intervention effort is necessary downstream. The effect size of roughly 0.21 (Cohen's d) is small compared to the literature on defaults (Jachimowicz et al., 2019 report an average effect size of 0.68). This is, however, consistent with the finding that behavioral interventions exhibit smaller effect sizes in education settings (see, e.g., DellaVigna and Linos, 2020 and Kraft, 2020). One reason specific to our setting could be exactly the prospect of having to exert extra effort later due to the automatic sign-up, making students more likely to deviate from the default setting than in situations where only little investment of time and effort is needed after the intervention.

The initial default effects do not last, however. As can be seen in Figure 1, Table 1 and Table 2, on the day of the exam, we do not observe statistically significant differences between treatment and control group sign-ups, neither overall nor for any of the six exams individually (Table 2). This implies that after the sign-up period, students in the treatment group actively de-register from exams.

By initially sticking to the default, students in the opt-out group retain their options and postpone the decision which exams to take until later in the semester. During the sign-up period, only two weeks into the first semester, students may not yet know how much ability and effort is required to pass the exams and it therefore makes sense to stay with the default number of sign-ups until further information about the choice environment may suggest a change. Over time, they gain knowledge about how many exams they will be able to prepare for. If students then believe to have better information in this respect than the university, they should change the number of sign-ups during the de-registration period and the initial default effects should vanish (see also the lab experiments in Altmann et al., 2021; defaults conflicting with private information should have no effect).

2.2.2 Downstream Default Effects

Not surprisingly then, the default intervention does not lead to effects on outcomes further downstream (see Figure 1). The mean number of passed exams is 4.40 in both the control group and in the treatment group, the number of failed exams is 0.78 and 0.73, respectively. The corresponding regression results are shown in Table 3. There is also no evidence that the automatic sign-up has a negative effect on the overall GPA or on the overall number of credit points (Table 3).[14]

3 Field Experiment II: Targeted Default

The second experiment is a conceptual replication (Nosek and Errington, 2017) of our first default study with a new cohort of BuA students. The goal is to test again whether an opt-out rule can generate standard default effects and whether it can move outcomes further downstream. However, this time we investigate a targeted default. Compared to the broad default, we implement the following changes: the intervention now takes place in the second semester instead of the first semester; also, instead of signing up students for all six exams that the study curriculum recommends for the second semester, we register students for only one of these, the statistics exam which is a principles class that many students view as challenging.

We hypothesized that the automatic sign-up in statistics should be more effective in changing downstream behavior than the broad default. The reason is that while students may feel they have better information than the default-setter on how many exams they are able to take (in the broad default), this may not be the case for the choice of which specific exams to take. This is a question that is particularly relevant (i) for all students who decide to take fewer than all of the scheduled six first semester exams, and who therefore have to choose specific exams rather than go with the full schedule; (ii) for those second semester students who did not pass all exams of the first semester (this applies to more than half the cohort) – these students need to (re-)take some of the first semester exams and also have to decide which of the scheduled second semester exams they should take. However, the university does not provide any guidance or recommendations on which exams to prioritize, or how to combine the 30 credits recommended by the curriculum for the second semester with exams from the first semester that have been postponed or need to be retaken. A targeted default should be informative in that respect, as it stresses the importance of one specific task – the statistics course – it should thus elicit behavioral change.

[14] We also do not find an effect on the grades of any of the individual exams.

3.1 Research Design

The cohort in this experiment consists of students who study towards a bachelor's degree in BuA in the second semester (as the statistics course is scheduled for the second semester). Randomization was carried out by stratifying on the credit points obtained in the first semester, and whether a student applied for the program after the median application date, and by balancing on the covariates displayed in Table A.3 in the Appendix.[15] The table shows that all variables are balanced between the control and the treatment group.

Students in the treatment group were automatically signed up for the statistics exam. As in the broad default experiment, students in the opt-out group could de-register from the statistics exam, and students in the control group (opt-in) had to actively sign up if they wanted to take statistics.

3.1.1 Procedure

Prior to the start of the second semester, students were informed (via postal mail and e-mail; letters are displayed in the Online Material) about the registration procedure for the statistics exam that applies to them. During the sign-up period (a timeline of the 2nd experiment is displayed in Figure A.3 in the Appendix) students in the control group were able to register for all exams online. Students in the treatment group were already automatically signed up for the statistics exam and had the opportunity de-register from statistics and to register for additional exams. During the de-registration period, about three weeks later, students in both groups could de-register from exams. After this point, de-registration and deferring the exam is still possible if a doctor's note is provided, otherwise statistics will be graded as failed.

3.1.2 Outcomes

In order to test for standard default effects, we use the sign-ups for the statistics exam at two different points in time, after the initial sign-up period and on the day of the exam. The latter differs from initial sign-ups due to de-registrations during the de-registration period and de-registrations with a doctor's note.

We again also evaluate downstream outcomes which go beyond standard default effects because they require students to invest time and effort: participation in the statistics exam, as well as passing and failing. Unlike in the first experiment, for statistics we also have data on actual exam participations.[16] This allows us to differentiate between exam failures due to no-shows (as not taking part in a registered exam counts as a fail) and failing grades due to actually failing the exam after taking part.

[15] Variable descriptions are provided in Table A.4 in the Appendix.

[16] Information on actual participation is only available to the individual lecturer and not included in the administrative data. For statistics, the instructor kindly provided us with this data.

Since statistics is not the only exam scheduled in the 2nd semester, we also monitor possible spillover effects. Students may prioritize the statistics exam because of the treatment but at the same time sign up for and pass fewer other exams. Similarly, the overall GPA may be affected by the default if treated students take more classes overall and therefore can allocate less study time to each. In order to make sure we do not miss such side effects, we preregistered to also analyze effects on the total number of exams signed up for, all passed exams, overall achieved credit points in the second semester, statistics grade, the overall GPA, and dropouts.

3.2 Full sample results

We graphically report raw treatment effects and also provide OLS estimates from the following specification:

$$Y_i^k = \alpha_0 + \alpha_1 Treatment_i + \boldsymbol{x_i \alpha_2} + \boldsymbol{z_i \alpha_3} + \varepsilon_i, \qquad (2)$$

where the outcomes Y_i^k are binary variables indicating whether students sign up for, participate in, pass or fail statistics. As preregistered, we use one specification with strata dummies, as well as one that adds a vector of the balancing variables from the randomization. To analyze spillover effects we use the same covariates and Y_i^k now represents the total overall outcomes described in Section 3.1.2.

3.2.1 Standard Default Effects

Analogous to the broad default, Figure 2 shows the rates at which BuA students signed up for the statistics exam in the opt-out and the opt-in group. During the sign-up period 81.7% of the students in the control group sign up for the exam, and being registered by default increases this number by about 4.5 pp.

Columns (1) and (2) of Table 4 show the corresponding regression coefficients: being part of the opt-out group increases sign-ups by about 5.2 to 5.4 pp (Cohen's h = 0.15). The replication experiment thus confirms our findings from the first experiment, where we also observe this standard default effect.

In contrast to the broad default, the effect of the targeted default persists beyond the sign-up period. On the day of the exam the mean sign-up rate is 7.33 pp higher in the opt-out group (Figure 2), and regression results in Columns (3) and (4) of Table 4 show an increase of roughly 8 pp (Cohen's h=0.19). The higher numbers compared to the sign-up period indicate that students in the control group de-register from the exam at a higher rate than students in the treatment group.

In Table 4 we also report persuasion rates. The persuasion rate relates the changes in sign-ups and participation to the base rates of these variables in the control group (see DellaVigna and Kaplan, 2007 and DellaVigna and Gentzkow, 2010).[17]. Our results indicate that about 30% of the students who would not have signed up in the sign-up period under the opt-in regulation were persuaded to do so by the opt-out regulation. For sign-ups on the exam day, the persuasion rate is about 31%. Both numbers indicate that the opt-out rule is rather effective in changing the behavior of those who can potentially be persuaded to do so (i.e. whose behavior can be changed).

In sum, in the targeted experiment we again find standard default effects. Effect sizes are typical for successful education interventions but smaller than what is often reported for default interventions. Compared to the broad default, which only affected sign-ups in the sign-up period, the targeted default leads to sustained increases in sign-up for the opt-out group until the day of the exam.

3.2.2 Downstream Default Effects

Regarding the outcomes further downstream which require effort, Figure 2 displays that the participation rate in the statistics exam is 69% in the control group and 75% in the treatment group. Regressions in Table 5 show that being signed up by default elicits a statistically significant effect of roughly 6.5 pp on exam participation (Columns 1 and 2), corresponding to a persuasion rate of 21%. The point estimate for the treatment effect on the passing rate (Columns 3 and 4) is almost 4 pp (roughly 63% in the treatment vs. 59% in the control group, not statistically significant), tentatively indicating that about 60% of those whom the treatment caused to participate also passed. Overall, fails are 4.4 pp higher than in the control group (Columns 5 and 6, also not statistically significant), consisting of failed exams due to non-participation (1.6 pp, Columns 7 and 8) and failed exams upon participation.[18] We find no effect on grades in the statistics exam (Columns 9 and 10).

Table A.5 in the Appendix shows the secondary outcomes we preregistered in order to check for potentially negative spillovers. The total number of exams (net of statistics) signed up for is not affected by the opt-out treatment (Columns 1 and 2). The effects on the total number of other exams passed and overall credits (both without statistics) are insignificant, yet the positive coefficients, if anything, tentatively indicate positive spillover effects (Columns 3 and 4). The overall GPA and the number of students who dropped out of the study program after the treatment are not affected.

[17]The persuasion rate is calculated as $\frac{y_T - y_C}{e_T - e_C} \cdot \frac{1}{1-y_0}$, where y_T and y_C are the shares in the treatment and control group exhibiting the behavior of interest; e_T and e_C are the shares in treatment and control receiving the treatment. Y_0 denotes the share of individuals that adopt the behavior of interest absent treatment. We set $e_T = 1$ and $e_C = 0$, and as in DellaVigna and Gentzkow (2010), we set $y_0 = y_C$.

[18]Exams are graded as failed if students sign up but do not participate, i.e. participation rate = pass rate + fail rate − fail rate no show.

Overall, the targeted default – in addition to the standard default effects – also leads to a downstream effect on exam participation. The effects on passing statistics are not statistically significant. However, the point estimates on passing should be considered relevant in an education setting, where outcomes are typically hard to move – but to be sure, the intervention would have to be replicated at larger scale with more power to statistically significantly detect effects of this magnitude. Overall, our findings strongly suggest that default effect sizes on effortful outcomes will be much smaller than the standard default effects typically reported in the default literature.

3.3 Responsive Individuals

The recent literature shows that default effects can be rather heterogeneous (see, e.g., Jachimowicz et al., 2019 and Mertens et al., 2022), more generally: Bryan et al., 2021), and Tannenbaum et al. (2017) as well as more recently Altmann et al. (2021) point out that alignment of interests between the default-setter and the defaulted individual makes defaults more effective. In addition, Heffetz et al. (2021) suggests that nudges can be more effective for individuals who have shown responsive behavior in the past.

In addition to the preregistered analyses, we therefore evaluate the effectiveness of the opt-out rule for *responsive* students, defined as those who responded to unrelated requests from the university to take part in a survey collecting feedback on students' study experience (40% of students responded).[19]

Responsive students participated in a voluntary online-survey, the "Student Satisfaction Monitor" (see Heffetz et al., 2021 for a similar approach to responsiveness in a setting with a reminder nudge). This survey is regularly conducted and asks a series of general questions regarding the study program, life/study satisfaction, stress etc., and in this iteration of the survey we added some questions about the statistics lectures, which will help us explore the channels behind the treatment effects (see Section 3.5). The dean of the faculty of Business Administration invited students via e-mail to take part in this survey. The decision to participate in the survey is thus independent of the default intervention as neither the invitation letter, the name of the survey, nor the person sending the invitation have any connection to the intervention. Students who did not respond to the initial request to participate in the survey and to two further reminder e-mails are classified as "non-responsive".

Responding to the survey request shows that these students are open and responsive to communication from the university, and are motivated to provide feedback. The group of responsives may then show a higher propensity to act in accordance with the default due to their generally higher responsiveness to defaults as implicit endorsements or recommenda-

[19]All other analyses in this paper are preregistered, see the Online Material for a summary of the preregistered analyses.

tions (Beshears et al., 2009, Carroll et al., 2009, Dinner et al., 2011, Jachimowicz et al., 2019, Madrian and Shea, 2001, McKenzie et al., 2006, Sunstein, 2013). In addition, we argue that the university and the responsives have aligned interests when it comes to (quick) completion of the program. As one of the main objectives of a university is to graduate its students (on time), the interests of the responsive students in our sample are likely to be more aligned with the goals of the university than is the case for the non-responsive students.[20] The alignment of interests should further contribute to the exam opt-out rule being more effective for responsives.

The survey took place post-treatment, but we will in the following show evidence that participation is independent of treatment. Overall, 145 students (40% of the full sample) participated in the survey. Table A.6 in the Appendix shows that responding to the survey is not significantly affected by treatment. This is the first condition to allow a credible estimation of treatment effects in this sample. The second condition is that among the responsives, those in the treatment and control group do not differ in their characteristics. We find that all covariates are balanced between treatment and control in the subsample of responsive students (Table A.7 in the Appendix). Overall, this makes us confident that we can estimate causal treatment effects for the responsives.

Note that responsiveness or alignment of interest are not the same as high ability. While a comparison of the high school GPA between responsive and non-responsive students (2.41 vs. 2.51; p-value: 0.02) shows that the responsives are a positively selected sample in terms of their ability, there are also lower achieving students who have aligned interests and are responsive. As we will show in Section 3.4, the lowest achieving students (in terms of pre-treament credits) among the responsive students actually benefit most from the default intervention (this is not the case for the non-responsives, which also underscores that ability and alignment of interests are distinct concepts).

In the following, we report results using the same OLS specification as in equation 2. We estimate the parameters for the sample of responsives and the sample of non-responsives.

3.3.1 Standard Default Effects

Figure 3 shows that among responsives, the mean sign-up rate in the control group after the sign-up period is 88%, and on the day of the exam 84% are still registered for the exam. Despite these high base levels, the opt-out treatment is able to increase sign-up by 8 pp, and on the day of the exam the mean sign-up rate for treated responsives is 9 pp higher than in

[20]Funding for universities in Germany is linked to the number of students who graduate, `https://eacea.ec.europa.eu/national-policies/eurydice/content/higher-education-funding-31_en`, retrieved on November 26th, 2021. It is also closely watched in other countries such as the US (see National Center for Education Statistics, `https://nces.ed.gov/FastFacts/display.asp?id=569`, retrieved on November 26th, 2021.

the control group. The regression results in Table 6 (upper panel) confirm this: responsive students who were automatically signed up for the exam have a 6.4 to 6.9 pp higher sign-up rate after the sign-up period. On the day of the exam it is 8 to 8.4 pp higher (Cohen's h = 0.26).

While the size of the point estimates are similar for the non-responsive students (Table 6, lower panel), this does not mean that the default is equally effective at changing their behavior. The persuasion rates for the responsives are 53% to 58% after the sign-up period and 50% to 53% on the day of the exam. Half the students in the treatment group who would not have signed up under the opt-in regime are persuaded to do so by the opt-out rule. Among the non-responsive students we find much lower persuasion rates of about 18% after the sign-up period and 22% to 25% on the day of the exam – indicating lower effectiveness of the default for the students whose interests are likely less aligned with the default-setter.

3.3.2 Downstream Default Effects

Figure 3 shows that the participation rate among treated responsives is 93% – the same as the sign-up rate on exam day. In the control group, this share declines from 84% to 76% and together this leads to a 16 pp treatment effect on exam participation (see also Columns 1 and 2 in Table 7). While roughly 8 pp of the non-responsive students who are signed up on exam day do not participate in the exam, all responsive students in the opt-out group who stayed signed up until exam day participated. This is also reflected in the 8.4 to 8.5 pp lower percentage of failed exams due to no-shows (Columns 7 and 8). Overall, among responsives, the opt-out treatment persuades 69% of those who otherwise would not have participated in statistics to attend the exam.

This result is of note because it implies that defaults can positively affect the outcomes downstream, even for individuals who would have signed up under an opt-in rule anyway, i.e. in the absence of an intervention. This shows that not all sign-ups are equally binding: own sign-ups result in no-shows at a much higher rate than the automatic sign-ups that were put into place by the university. It seems that the barrier to opting out of the exam (via non-participation) is higher when this overrides the selection made by the university. The opt-out rule leads students to actually take the exam which they would not have done under the opt-in rule (though they would have signed up in both cases). Opt-out defaults may thus increase the bindingness of the same choice versus opt-in.

For responsives the treatment also increases successfull task completion, i.e. passing rate by 11 pp (see Figure 3, 70% in the opt-in group versus 81% in the opt-out group). Regression results confirm this and show a statistically significant increase in the passing rate of 11.2 to 12.2 pp (no persuasion rate reported, as we do not consider outcomes beyond participation to be the result of persuasion). This highlights that for responsive individuals, defaults may greatly improve even outcomes which require considerable post-treatment investment of time and effort. Defaults may therefore be an attractive choice in a broader range of settings than has previously been shown.

By contrast, for non-responsive students the participation rate in the control and the treatment group is equal, at 64%, and the passing rate is 52% in the control and 51% in the treatment group (Figure 4). The regression results in Table 7 also show that there are no treatment effects on participation (the persuasion rate is effectively zero) or passing for non-responsives. However, we see an increase in overall fails of 9.6 to 9.7 pp (Columns 5 and 6), which can be explained almost entirely by students who did not show up for the exam (8.4 to 8.5 pp, Columns 7 and 8). This shows that for non-responsive students the higher sign-up rate does not lead to higher exam participation or passing. On the contrary, the increases in sign-up translate into fails due to no-shows.

Table A.8 in the Appendix shows the preregistered secondary outcomes for responsive and non-responsive students; we again check for negative spillovers. For responsive students none of the overall outcomes is significantly changed by treatment. Initial overall sign-ups (net of statistics) slightly decrease for responsives, the overall number of passed exams increases, leading to an increase in overall credits (both net of statistics) of more than one credit point, tentatively indicating, if anything, a positive spillover effect for responsive students. Treated non-responsives sign up for and pass slightly more exams, however the difference is not statistically significant. In addition, the overall GPA of non-responsives is somewhat lower with treatment.

Overall, our results are in line with Altmann et al. (2021) who show that while individuals benefit from defaults when interests are aligned with the default setter, under misaligned interests, individuals may stick to defaults too often. Ultimately this can lead to detrimental consequences. In our case, the interests of at least some of the non-responsives are probably not aligned with the interests of the default setter. Still, they stick with the default, which leads to fails due to no-shows.

3.4 Effects on High and Low Achieving Individuals

An important question for interventions in general, and specifically education interventions is what their "distributional" implications are. In our setting, this means evaluating whether they can particularly help low-achieving individuals make better progress in their studies. So far we have found that the targeted default has strong effects on the important downstream outcomes for responsive students. Despite the fact that responsive students are a positively selected group in terms of e.g. past performance, this does not necessarily mean that it is the high achievers who benefit most from the targeted default. As we will show below, on many dimensions the treatment effects are, in fact, larger for those who rank lower in the performance distribution.

In Figure 5 we show interactions of the treatment effect with pre-treatment performance, i.e. credits obtained in the previous semester (a measure of passed exams). For clarity of exposition, we estimate the parameters in three samples: the full sample, the responsive sample, and the non-responsive sample. More specifically, we estimate in these samples the following equation:

$$Y_i^k = \alpha_0 + \alpha_1 Treatment_i + \alpha_2 CP_i + \alpha_{1,2} Treatment_i \cdot CP_i + \boldsymbol{x_i \alpha_3} + \boldsymbol{z_i \alpha_4} + \varepsilon_i, \tag{3}$$

where CP_i is a discrete variable denoting the number of credit points (net of transferred credits) a student obtained in the first semester. All other variables and parameters are defined as before.

Full Sample. For the full sample, the distribution of credits in the pre-treatment semester is shown in the top left corner of Panel A in Figure 5 (corresponding regression estimates are shown in Tables A.9 and A.10 in the Appendix). Next to the distribution of credits, the treatment effects across the distribution of credits for the considered outcomes are visualized. Standard default effects are largest for the lower achieving students and taper out around 30 credits earned in the first semester (Columns 1 and 2). Further downstream, however, these large sign-up effects for the lowest 1st semester performers do not translate into above average participation or passing (Columns 3 and 4) but do lead to higher fail rates (Column 5). This is entirely driven by fails due to no-show (Column 6). However, we show below that a more nuanced picture arises once we differentiate between responsive and non-responsive students.

Responsive Students. In the first row of graphs of Panel B in Figure 5, we see an even more pronounced picture for the standard default effects: the effects on sign-up are much larger for the lower achieving students. For example, for students who obtained 20 credits in the first semester the treatment effect on initial (exam day) sign-ups is 21 pp (25 pp) (the corresponding regression estimates are in Table A.10 in the Appendix). The effects fade out again around 30 credits. It is important to note that there is not much support in the lowest part of the performance distribution, so e.g. the large main effect of treatment on a person with zero credits in the pre-treatment semester should be interpreted with caution. The main difference to the overall sample is that the increased sign-ups for the weaker students do not result in higher fail rates. Quite the contrary, they translate into higher participation and passing, and a drop in fails due to no-shows: for a student with 20 credits in the first semester, the probability of participating increases by 40 pp, the probability of passing by 26 pp.

Non-Responsive Students. The results we have shown so far imply that the effect on the overall fail rate we saw in the full sample is driven by the non-responsive students. The last row of graphs of Panel B in Figure 5 shows that the standard default effect seems to be somewhat stronger for the lower achieving non-responsive students in comparison to high achievers, and that the rise in sign-ups leads to increased fails, all of which is due to no-shows – supporting the idea that this is a group of students whose interests are not aligned with the default setter and who therefore cannot be moved to exert more post-intervention effort. Sticking with the default in this case does not lead to beneficial outcomes.

Overall, this analysis shows that the targeted default particularly increases the sign-ups for lower achieving students, but that downstream the consequences of this standard default effect are vastly different. Weaker non-responsive students become no-show fails at high rates, whereas weaker responsive students can convert the higher sign-ups into participation and, ultimately, passing of the exam. While being responsive correlates with higher pre-treatment achievement, the important message is that the lower achieving individuals among the responsive students are the ones who benefit most from changing the default. Due to the nature of our outcome – which requires post-intervention effort – the non-responsive students cannot convert the standard default effects into better academic performance.

3.5 Drivers of the Downstream Effects

The "Student Satisfaction Monitor" not only enables us to identify the responsive students, but for this edition of the survey we asked three questions about the statistics course.[21] These questions can shed some light on the mechanisms that drive the effects among the responsive students. In particular, we inquired whether the respondents attended the statistics class and/or the accompanying tutorial – and if so, how often. We also asked how many hours per week the respondents spent preparing for the statistics class, on top of lectures and tutorials.[22] The effects of the opt-out treatment on the above variables are shown in Table 8.

The data show that automatic sign-up for the statistics exam increases effort, as measured by attendance in the statistics course/tutorial by around 11 pp (Columns 1 and 2). Conditional on attending at all, the frequency of attendance may be somewhat higher but the estimates are not statistically significant. Finally, we see that treated students spend more hours per week preparing for the statistics class on top of lectures and tutorials, again indicating increased effort.

Table A.11 in the Appendix displays the remaining preregistered survey outcomes. We also observe that the automatic sign-up increases lecture attendance overall. An estimation controlling for frequency of statistics attendance reduces the effects in Columns (1) and (2) to

[21]The survey took place in the second half of the semester.

[22]The exact wording of the questions is shown in Table A.16 in the Appendix.

-0.055 (SE: 0.060) and -0.033 (SE: 0.059). This very tentatively suggests that the positive effect on the overall attendance may be mainly due to the increase in attendance in the statistics lecture – though of course these results come with the caveat that we are controlling for an outcome ("bad control", see Angrist and Pischke, 2008). In addition, we find the treatment has no effects on study time on top of lectures, satisfaction with the study program, life satisfaction and stress (Table A.11 in the Appendix).

Overall this suggests a rather straightforward mechanism where the automatic sign-up leads responsive students to subsequently increase lecture attendance and study time, in order to be able to pass the exam. This finding is very relevant not only from an education policy perspective, but for the default literature in general, because it shows that for a substantial share of individuals, default settings can lead to active behavior changes and elicit substantial investments of effort and time.

3.6 Further Evidence on Alignment of Interests: Statistics in the IB Program

So far we discussed results for Business Administration (BuA) students. We also preregistered to analyze effects of the targeted default on students in a different, small study program, where incentives of students and default setters are likely less aligned: International Business (IB). The IB and BuA programs and their students are quite different, and we will discuss how this affects the results.

The IB program is much more selective, as can be seen by the high school GPA of IB students (1.86) which is 0.62 grade points better (p-value: 0.0) than the BuA average. Accordingly, in Figure A.4 we see that 94% of the control students register for statistics during the sign-up period, and the sign-up rate stays at this level until the day of the exam. For the remaining few students who do not sign up, it is likely that they have non-aligned interests with the default setter: according to the study curriculum, BuA students have to take all first and second semester exams of the study plan, including statistics, at least once by the third semester. In the IB curriculum, no such rule exists. Specifically, this means that statistics can be taken later on, or during the mandatory semester abroad at a foreign university, where it may be less challenging. Therefore, the interests of the IB students who do not sign up under opt-in and the default setter are likely less closely aligned than the interests of the BuA students who do not sign up under opt-in. We would thus expect smaller effects – of course the caveat is that there is also little room for an increase in sign-ups due to the already high levels in the control group.

We find no statistically significant standard default effects for the IB students (see Table A.13 in the Appendix). For the initial sign-up period the estimate is 3 pp, for the day of the exam it is -5 to -6 pp, both imprecisely estimated. Downstream we find no statistically significant effects either (see Table A.14 in the Appendix). In the control group the participation (passing) rate is 88% (82%). We estimate negative treatment parameters of -11 pp for participation and since students who did not participate cannot pass the exam, similar parameters for passing. These estimates can almost entirely be explained by a 8 to 9 pp increase of students sick on the day of the exam in the treatment group (Columns 11 and 12).[23] The small sample size in the IB program makes it likely that this is due to statistical chance (three sick students in the treatment group account for the effect), as we have no reason to believe that treated students are more likely to obtain "fake" doctor's notes to opt out of the exam (there was no sign-up effect to begin with).

We find no statistically significant effects on any secondary outcomes either (Table A.15 in the Appendix). Overall, we cautiously take the absence of effects in this small group of students as further evidence that alignment of interests matters for the effectiveness of default interventions.

4 Conclusion

Many tasks are characterized by a need to invest substantial amounts of time and effort in order to complete them. In this paper we have investigated whether default interventions can be effective policy tools to increase the take-up of such tasks and, importantly, subsequent (successful) task completion rates. We have shown in a higher education setting that: (i) task take-up (i.e. exam sign-up) increases when an opt-out rule applies – perhaps unsurprisingly so, as simply staying signed up for the demanding task does not require effort, and is therefore conceptually similar to standard default effects reported in the literature; (ii) when the opt-out applies to a specific task (one predetermined exam) rather than many tasks (all exams), downstream effects on exam participation, i.e. task completion, can be observed; (iii) among the large group of previously responsive individuals, the opt-out rule increases successful task completion (passing the exam), supporting recent research which shows that the alignment of interests between default-setter and defaultee is an important driver of default effects (Altmann et al., 2021, Tannenbaum et al., 2017); (iv) the effect on successful task completion is driven by increased investment of time and effort in the months leading up to task completion (attending class and studying).

These results open up new potential fields of application for opt-out rules. We have shown that in higher education, they can be an interesting addition to more traditional measures aimed at improving the outcomes of weaker students. Other examples for substantially effortful tasks where take-up is typically optional and where policy seeks to increase participation and completion include training programs for employees and the unemployed,

[23] For BuA students in Section 3, we find no effects on being sick with a doctor's note (not shown).

volunteer work or extracurricular activities in school.

Heterogeneous responses should be expected. Successful use in policy then requires focusing on individuals whose interests are likely to be aligned with those of the policy-maker, e.g. by identifying individuals who have displayed responsiveness in the past (see also Heffetz et al., 2021). As we have seen, others may well leave the default setting in place, but ultimately this may not be in their best interest.

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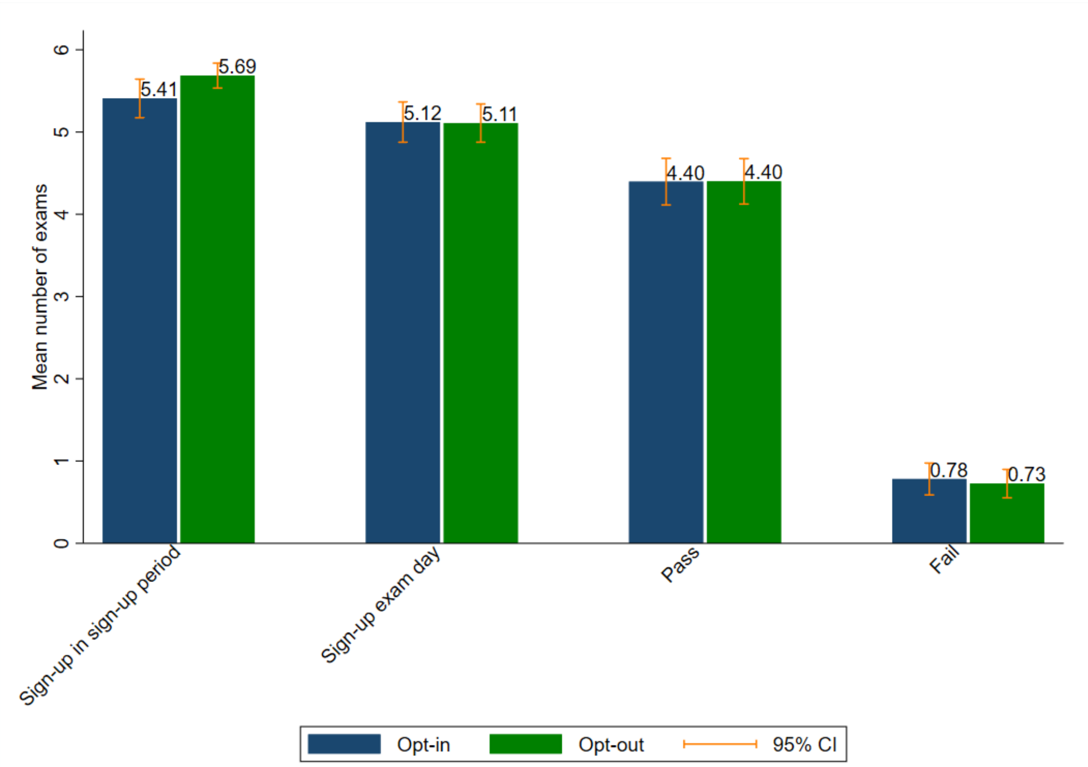
Tables and Figures

Broad Default

Figure 1: Broad default – mean outcomes in the control vs. treatment group



Note: $N = 349$; N(opt-in) = 174; N(opt-out) = 175. The sum of "pass" and "fail" is larger than "sign-up exam day " because (the very few) students who don't sign up for the orientation exams (Math and Business Administration) in the first semester receive a fail.

Table 1: Broad default – standard default effect

	Sign-up			
	Sign-up period		Exam day	
	(1)	(2)	(3)	(4)
Treatment (opt-out)	0.275*	0.258*	−0.018	−0.027
	(0.140)	(0.137)	(0.169)	(0.160)
Strata FE	Yes	Yes	Yes	Yes
Balancing variables	No	Yes	No	Yes
Mean control	5.41		5.12	
(SD)	(1.58)		(1.65)	
N	349		349	

Note: OLS estimates. Dependent variables are the number of exams (recommended by the study curriculum) signed up for after the sign-up period and on exam day. Strata fixed effects (FE) based on high school GPA; balancing variables: high school GPA, age, age squared, age to the power of three, gender, date of enrollment, application date, dummy for German citizenship, completed university semesters prior to current program. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 2: Broad default – standard default effect on individual exams

	Math	Bus. Adm.	Micro-economics	Informatics	Management	Accounting
	(1)	(2)	(3)	(4)	(5)	(6)
Sign-up: sign-up period						
Treatment (opt-out)	0.050**	0.040*	0.030	0.060**	0.041*	0.037
	(0.024)	(0.023)	(0.030)	(0.026)	(0.023)	(0.036)
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
Balancing variables	Yes	Yes	Yes	Yes	Yes	Yes
Mean control	0.92	0.93	0.89	0.90	0.93	0.84
(SD)	(0.27)	(0.25)	(0.31)	(0.31)	(0.26)	(0.36)
Sign-up: exam day						
Treatment (opt-out)	0.000	−0.017	−0.011	0.035	0.019	−0.053
	(0.033)	(0.032)	(0.041)	(0.037)	(0.026)	(0.044)
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
Balancing variables	Yes	Yes	Yes	Yes	Yes	Yes
Mean control	0.89	0.90	0.80	0.82	0.92	0.80
(SD)	(0.32)	(0.30)	(0.40)	(0.39)	(0.27)	(0.40)
N	349	349	349	349	349	349

Note: OLS estimates. Dependent variables are the dummy variables for being signed up for the individual exams after the sign-up period and on exam day. Strata FE based on high school GPA; balancing variables: high school GPA, age, age squared, age to the power of three, gender, date of enrollment, application date, dummy for German citizenship, completed university semesters prior to current program. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

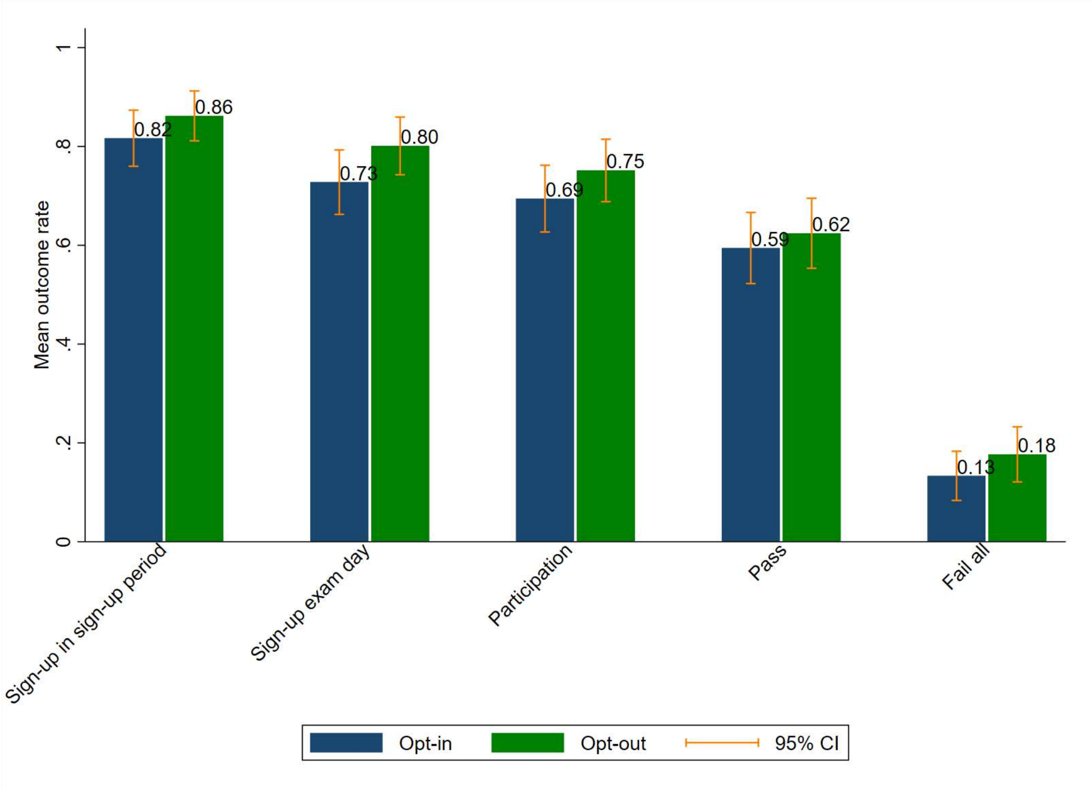
Table 3: Broad default – downstream default effect

	Pass		Fail		GPA		All CP	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment (opt-out)	−0.007	−0.011	−0.050	−0.058	0.009	−0.003	−0.114	−0.087
	(0.195)	(0.180)	(0.130)	(0.127)	(0.057)	(0.055)	(0.978)	(0.911)
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Balancing variables	No	Yes	No	Yes	No	Yes	No	Yes
Mean control	4.40		0.78		2.36		22.95	
(SD)	(1.90)		(1.30)		(0.53)		(9.43)	
N	349		349		328		349	

Note: OLS estimates. Dependent variables are the number of exams passed (net of transferred credits), failed exams, the overall GPA (only passing grades) and the number of all credits (all CP) net of transferred credits. N(GPA) = 328 because only passing grades are included. Strata FE based on high school GPA; balancing variables: high school GPA, age, age squared, age to the power of three, gender, date of enrollment, application date, dummy for German citizenship, completed university semesters prior to current program. The sum of "pass" and "fail" is larger than "sign-up exam day " because (the very few) students who do not sign up for the orientation exams (Math and Business Administration) in the first semester receive a fail. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Targeted Default

Figure 2: Targeted default – mean outcomes control and treatment



Note: N = 361; N(opt-in) = 180; N(opt-out) = 181. The sum of "pass" and "fail all" differs from "sign-up exam day" due to rounding. "Fail all" consists of failed exams due to insufficient performance plus fails due to non-participation upon sign-up.

Table 4: Targeted default – standard default effect

	Sign-up rate			
	Sign-up period		Exam day	
	(1)	(2)	(3)	(4)
Treatment (opt-out)	0.052*	0.054*	0.080**	0.083**
	(0.030)	(0.030)	(0.037)	(0.037)
Strata FE	Yes	Yes	Yes	Yes
Balancing variables	Yes	No	Yes	No
Persuasion rate	0.29	0.30	0.30	0.31
Mean control	0.82		0.73	
(SD)	(0.39)		(0.45)	
N	361		361	

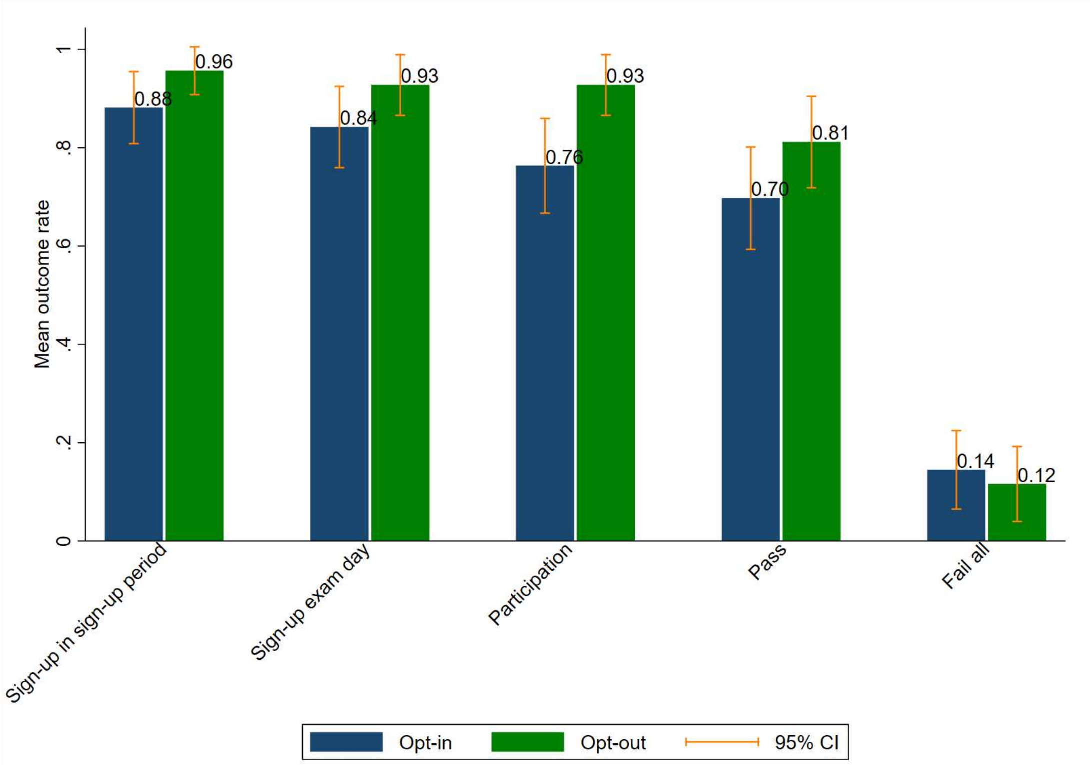
Note: OLS estimates. Dependent variables are the indicators for sign-up in statistics after the sign-up period and on exam day. Strata FE: 1st semester CP FE×a dummy for early/late application; balancing variables: high school GPA, age, gender, application date. The persuasion rate is calculated as $\frac{y_T - y_C}{1-y_C}$ where y_T and y_C are the outcomes in the treatment (T) and control (C) group. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 5: Targeted default – downstream default effect

	Participation[1]		Pass		Fail all		Fail no show		Statistics grade	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment (opt-out)	0.064*	0.066*	0.036	0.038	0.044	0.044	0.016	0.016	0.159	0.151
	(0.038)	(0.038)	(0.042)	(0.041)	(0.037)	(0.037)	(0.021)	(0.021)	(0.137)	(0.134)
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Balancing variables	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Persuasion rate	0.21	0.21	-	-	-	-	-	-	-	-
Mean control	0.69		0.59		0.13		0.03		2.80	
(SD)	(0.46)		(0.49)		(0.34)		(0.18)		(1.33)	
N	361		361		361		361		276	

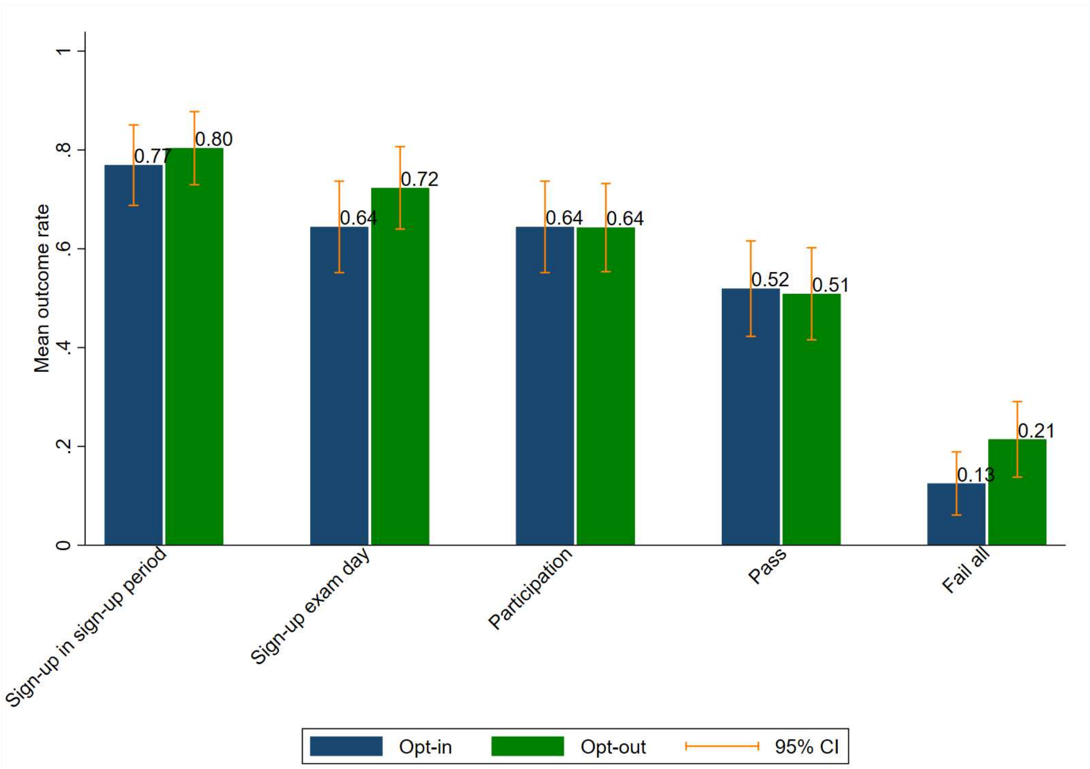
Note: OLS estimates. Dependent variables are the statistics participation rate, pass rate, fail rate, fail rate because of not showing up and the statistics grade (includes fails). Strata FE: 1st semester CP FE×a dummy for early/late application; balancing variables: high school GPA, age, gender, application date. The persuasion rate is calculated as $\frac{y_T - y_C}{1 - y_C}$ where y_T and y_C are the outcomes in the treatment (T) and control (C) group. There is no persuasion rate for pass as students can only be "persuaded" to sign up and participate. [1]The exam is graded as failed if students are signed up but do not show up, i.e. participation rate = pass rate + fail rate - fail rate no show. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Figure 3: Targeted default (responsive students) – mean outcomes in control and treatment



Note: N = 145; N(opt-in) = 76; N(opt-out) = 69. The sum of "pass" and "fail all" differs from "sign-up exam day" due to rounding. "Fail all" consists of failed exams due to insufficient performance plus fails due to non-participation upon sign-up.

Figure 4: Targeted default (non-responsive students) – mean outcomes control and treatment



Note: N = 216; N(opt-in) = 104; N(opt-out) = 112. The sum of "pass" and "fail all" differs from "sign-up exam day" due to rounding. "Fail all" consists of failed exams due to insufficient performance plus fails due to non-participation upon sign-up.

Table 6: Targeted default – standard default effect, (non)responsives

	Sign-up rate			
	Sign-up period		Exam day	
	(1)	(2)	(3)	(4)
Responsive students				
Treatment (opt-out)	0.069*	0.064	0.084*	0.080*
	(0.040)	(0.040)	(0.048)	(0.048)
Strata	Yes	Yes	Yes	Yes
Balancing variables	No	Yes	No	Yes
Persuasion rate	0.58	0.53	0.53	0.50
Mean control	0.88		0.84	
(SD)	(0.33)		(0.37)	
N	145		145	
Non-responsive students				
Treatment (opt-out)	0.038	0.042	0.079	0.090*
	(0.043)	(0.043)	(0.054)	(0.054)
Strata	Yes	Yes	Yes	Yes
Balancing variables	No	Yes	No	Yes
Persuasion rate	0.17	0.18	0.22	0.25
Mean control	0.77		0.64	
(SD)	(0.42)		(0.48)	
N	216		216	

Note: OLS estimates. Dependent variables are the indicators for sign-up in statistics after the sign-up period and on exam day. Strata FE: 1st semester CP FE×a dummy for early/late application; balancing variables: high school GPA, age, gender, application date. The persuasion rate is calculated as $\frac{y_T - y_C}{1-y_C}$ where y_T and y_C are the outcomes in the treatment (T) and control (C) group. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table 7: Targeted default – downstream default effect, (non)responsives

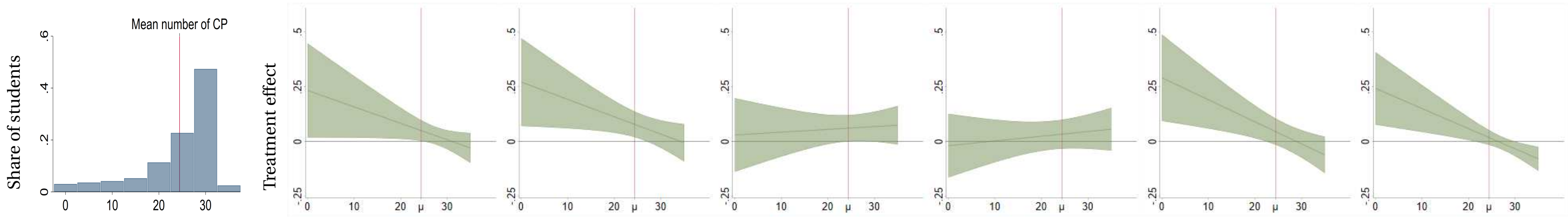
	Participation[1]		Pass		Fail all		Fail no show		Statistics grade	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Responsive students										
Treatment (opt-out)	0.168***	0.165***	0.122*	0.112*	−0.038	−0.032	−0.084***	−0.085**	−0.026	−0.016
	(0.052)	(0.053)	(0.062)	(0.063)	(0.054)	(0.054)	(0.032)	(0.033)	(0.195)	(0.191)
Strata	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Balancing variables	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Persuasion rate	0.70	0.69	-	-	-	-	-	-	-	-
Mean control	0.76		0.70		0.14		0.08		2.65	
(SD)	(0.43)		(0.46)		(0.35)		(0.27)		(1.35)	
N	145		145		145		145		128	
Non-responsive students										
Treatment (opt-out)	−0.000	0.006	−0.017	−0.007	0.096*	0.097*	0.080***	0.083***	0.316	0.300
	(0.053)	(0.054)	(0.056)	(0.057)	(0.050)	(0.049)	(0.025)	(0.025)	(0.192)	(0.192)
Strata	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Balancing variables	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Persuasion rate	0	0.02	-	-	-	-	-	-	-	-
Mean control	0.64		0.52		0.13		0		2.95	
(SD)	(0.48)		(0.50)		(0.33)		(0)		(1.31)	
N	216		216		216		216		148	

Note: OLS estimates. Dependent variables are the indicators for the participation rate, pass rate, fail rate, the fail rate because of not showing up, and the statistics grade (includes fails). Strata FE: 1st semester CP FE×a dummy for early/late application; balancing variables: high school GPA, age, gender, application date. The persuasion rate is calculated as $\frac{y_T - y_C}{1 - y_C}$ where y_T and y_C are the outcomes in the treatment (T) and control (C) group. There is no persuasion rate for pass as students can only be "persuaded" to sign up and participate. [1]The exam is graded as failed if students are signed up but do not show up, i.e. participation rate = pass rate + fail rate - fail rate no show. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Figure 5: Targeted default – Treatment effect, interaction with previous performance (CP)

	Standard default effect		Downstream default effect			
	Sign-up: sign-up period	Sign-up: day of exam	Participation[1]	Pass	Fail all	Fail no show
	(1)	(2)	(3)	(4)	(5)	(6)

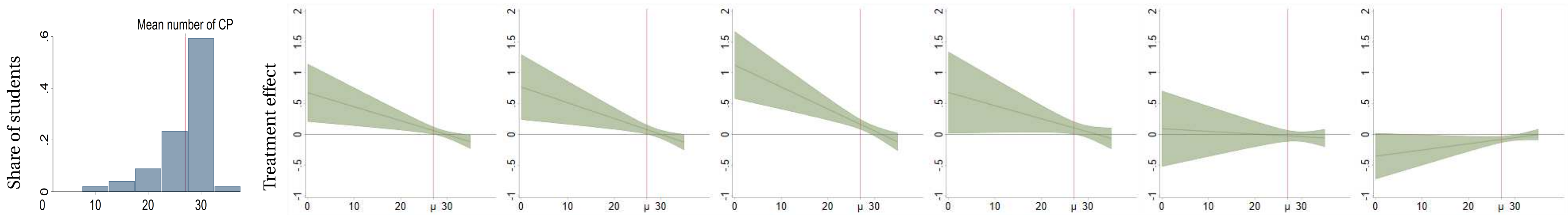
Panel A: Full sample



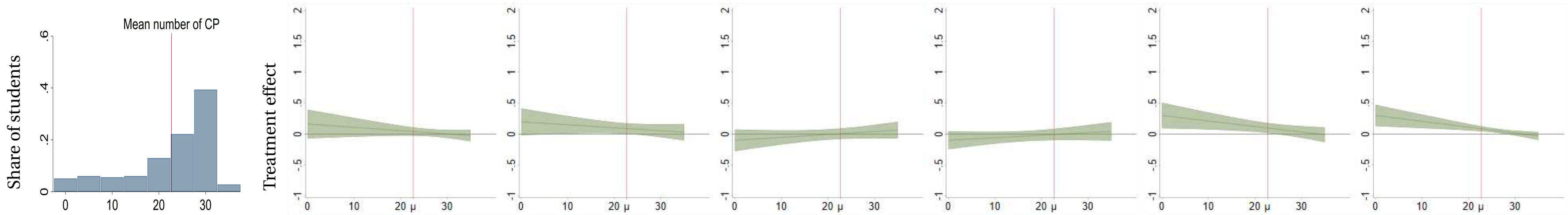
1st Semester CP

Panel B: Responsive vs. non-responsive students

Responsives



Non-responsives



1st Semester CP

Note: N(full sample)=361; N(responsives)=145; N(non-responsives)=216. Leftmost graph in each row displays the distribution of 1st semester CP (in bins of 5) with the share of students on the y-axis. The remaining graphs display the treatment effect (90% CI) on the y-axis and the 1st semester CP on the x-axis. Corresponding regression estimates are in Tables A.9, A.10, and A.11 in the Appendix. The vertical red line corresponds to the mean number of 1st semester CP (μ). [1]The exam is graded as failed if students are signed up but do not show up, i.e. participation rate = pass rate + fail rate - fail rate no show.

Table 8: Targeted default – mechanism

	Statistics attendance yes/no		Statistics attendance frequency		Time spent on statistics	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment (opt-out)	0.107*	0.109*	0.216	0.227	0.376	0.417*
	(0.060)	(0.059)	(0.158)	(0.156)	(0.251)	(0.240)
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
Balancing variables	No	Yes	No	Yes	No	Yes
Mean control	0.77		4.07		1.06	
(SD)	(0.42)		(0.84)		(1.09)	
N	138		114		109	

Note: OLS estimates. Strata FE: 1st semester CP FE×dummy for early/late application; balancing variables: high school GPA, age, gender, application date. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$. Question for statistics attendance (yes/no): Do you visit the lecture and/or tutorial in Statistics in the current semester? [1=Yes, 0=No]; question for statistics attendance (frequency): How often have you attended the Statistics lectures (including exercises and tutorials) in the current semester? [0 - never, i.e. 0 % of all classes, 1 - 1 % - 25 % of all classes, 2 - 26 % - 50%, 3 - 51 % - 75%, 4 - 76% - 99%, 5 - 100%"]; question for time spent on statistics: On average, how many hours per week did you spend studying Statistics this semester, lectures and tutorials not included? [0 - up to one hour per week; 1 - over 1 up to 2 hours per week; 2 - over 2 up to 3 hours per week; 3 - over 3 up to 4 hours per week; 4 - over 4 up to 5 hours per week; 5 - more than 5 hours per week].

Appendix

A Additional Tables and Figures

Table A.1: Broad default – balancing properties

	Treatment	Control	p-value
	(1)	(2)	(3)
Age	21.50	21.60	0.79
	(3.13)	(3.77)	
Male	0.42	0.45	0.63
	(0.50)	(0.50)	
High school GPA	2.38	2.38	0.98
	(0.47)	(0.47)	
German citizenship	0.90	0.91	0.72
	(0.30)	(0.28)	
Completed university semester	1.35	1.41	0.52
prior to current program	(0.84)	(0.89)	
Enrollment date (days left)	40.49	40.38	0.80
	(4.35)	(3.75)	
Application date (days left)	36.70	37.29	0.83
	(25.00)	(25.59)	
N	175	174	349

Note: Columns (1) and (2) display the means in the control and treatment groups. Standard deviations (SD) in parentheses. Column (3) displays t-tests of equality of means. Application date and enrollment date are coded in reverse, with the highest number corresponding to the earliest application/enrollment; the variable can be interpreted as number of days left at the time of application/enrollment.

Table A.2: Variable description: broad default

Variable	Description
Treatment Variables	
Treatment	Random assignment to the treatment group.
Stratification Variables	
HS GPA	Indicators for final high school grade point average (1.0-1.9; 2.0-2.1; 2.2; 2.3; 2.4; 2.5; 2.6; >2.6)
Control Variables	
Age	Age in years at randomization.
Male	Indicator for being male.
HS GPA	Final high school grade point average (1=best, 4=worst).
German citizenship	Indicator for being a German citizen.
Completed university semester	Number of university semester completed prior to the start the study program.
Enrollment date	Days left of the enrollment period on which student enrolled in the study program.
Application date	Days left of the application period on which student enrolled in the study program.
Outcome Variables	
Sign-up sign-up period	Number of recommended exams signed up for during the sign-up period.
Sign-up sign-up period (individual exams)	Sign-up rate of each of the recommended exams after the sign-up period.
Sign-up exam day	Number of recommended exams still signed up for on the day of the exam.
Sign-up exam day (individual exams)	Sign-up rate of each of the recommended exams on the day of the exam.
Pass	Number of recommended passed exams in the first semester.
Pass (individual exams)	Pass rate of each of the recommended exams in the first semester.
Fail	Number of recommended failed exams in the first semester.
Fail (individual exams)	Fail rate of each of the recommended exams in the first semester.
Grade (individual exams)	Grade of each of the recommended exams in the first semester.
GPA	Grade point average first semester (1=best, 4=worst); failed exams are not included in calculation.
All CP	Number of overall credit points achieved in the first semester (net of recognitions). Also including exams that were not recommended by the university to be taken in the first semester.

Figure A.1: Timeline of broad default experiment

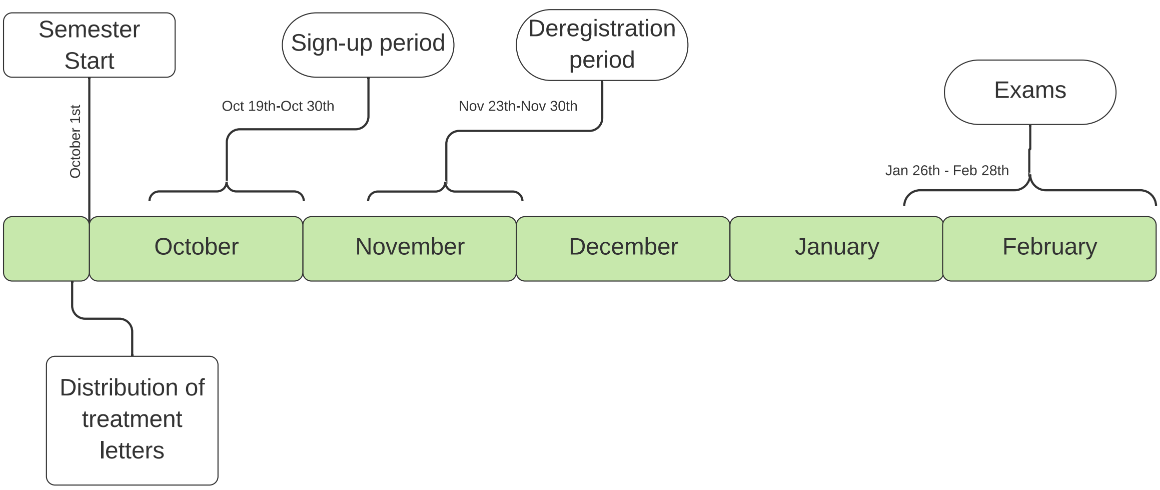
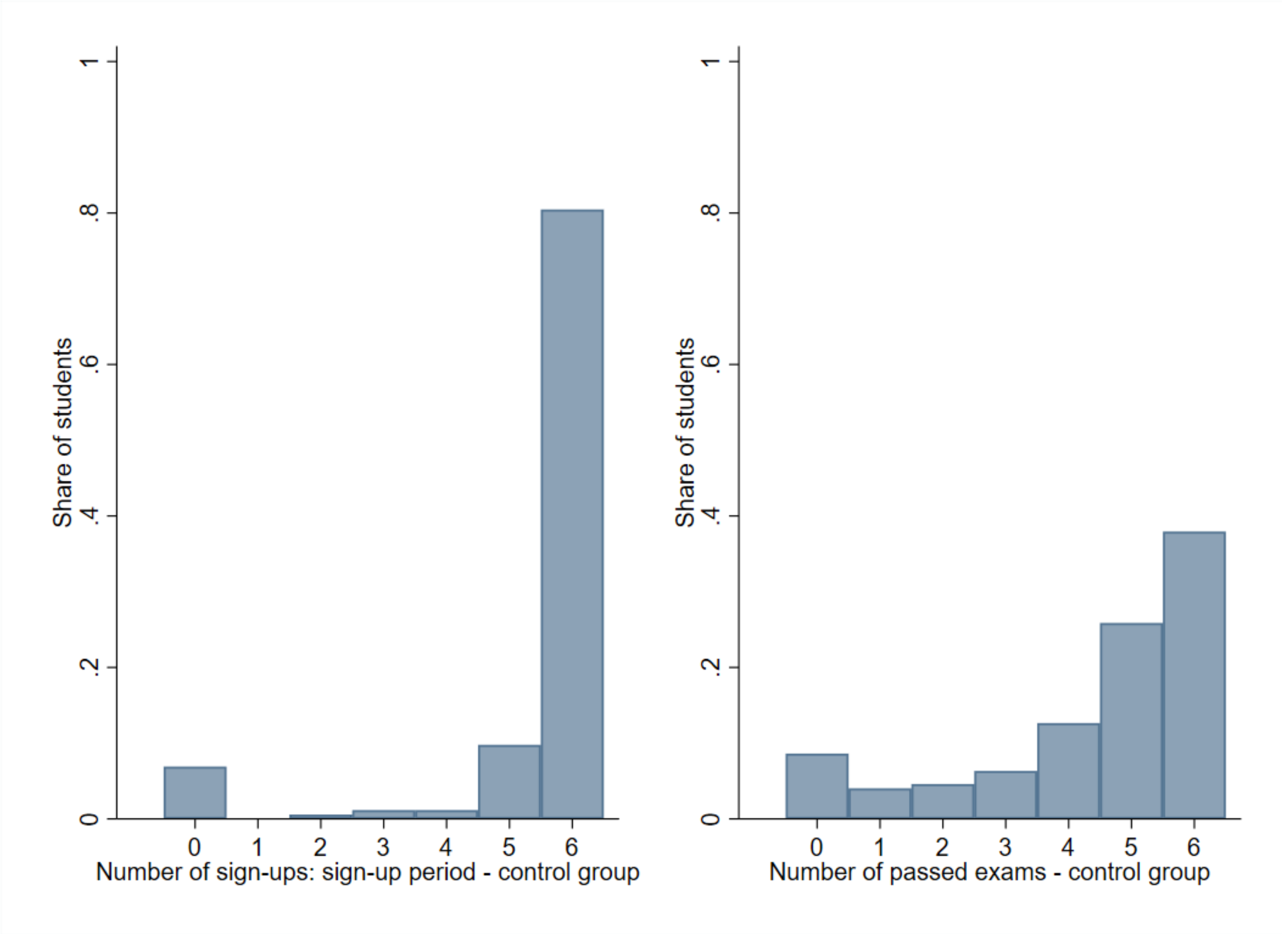


Figure A.2: Broad default – number of exam sign-ups and passes in the control group



Note: $N = 174$. The left panel displays the share of students that signed up for each number of the six exams recommended for the first semester. The right panel displays the share of students that passed each number of the six exams recommended for the first semester.

Table A.3: Targeted default – balancing properties

	Treatment	Control	p-value
	(1)	(2)	(3)
Age	21.47	21.49	0.94
	(3.59)	(2.86)	
Male	0.45	0.46	0.88
	(0.50)	(0.50)	
High school GPA	2.48	2.47	0.90
	(0.44)	(0.46)	
Application date (days left)	30.18	31.02	0.75
	(24.92)	(25.39)	
Early/late application	0.50	0.49	0.87
	(0.50)	(0.50)	
N	181	180	361

Note: Columns (1) and (2) display the means in the control and treatment groups. Standard deviations (SD) in parentheses. Column (3) displays t-tests of equality of means. Early/late application is a dummy where 1 corresponds to a student who applied after the median application date. Application date is coded in reverse, with the highest number corresponding to the earliest application; the variable can be interpreted as number of days left in the application period.

Table A.4: Variable description: targeted default

Variable	Description
Treatment Variables	
Treatment	Random assignment to the treatment group.
Stratification Variables	
1st semester CP	First semester credit points (3 blocks in each study program. BuA: <15CP; 15-30CP; 30-40CP. IB: <24CP; =24CP; >24CP).
Early/late application	Indicator for being a procrastinator (1 if someone applied in second half of the application period).
Control Variables	
Age	Age in years at randomization.
Male	Indicator for being male.
HS GPA	Final high school grade point average (1=best, 4=worst).
Application date	Days left of the application period on which student enrolled in the study program.
Outcome Variables	
Sign-up: sign-up period	Indicator for being signed up for the statistics exam after the sign-up period.
Sign-up: exam day	Indicator for being signed up for the statistics exam on the day of the exam.
Pass	Indicator for passing of the statistics exam.
Fail all	Indicator for failing of the statistics exam.
Fail no show	Indicator for failing of the statistics exam due to not showing up to the exam.
Statistics grade	Grade of the statistics exam; only students who participated in the exam are included.
Doctor's note	Indicator for withdrawing from the exam with a doctor's certificate for being unfit to take the exam.
All exams without statistics: sign-ups sign-up period	Number of exam sign-ups during sign-up period (net of statistics).
All exams without statistics: pass	Number of passed exams (net of statistics).
GPA	Grade point average in the second semester (1=best, 4=worst); failed exams are not included in calculation.
All exams without statistics: CP	Number of overall credit points achieved in the second semester (net of statistics).
Dropout	Indicator for having dropped out of the study program after treatment.

Figure A.3: Timeline of targeted default experiment

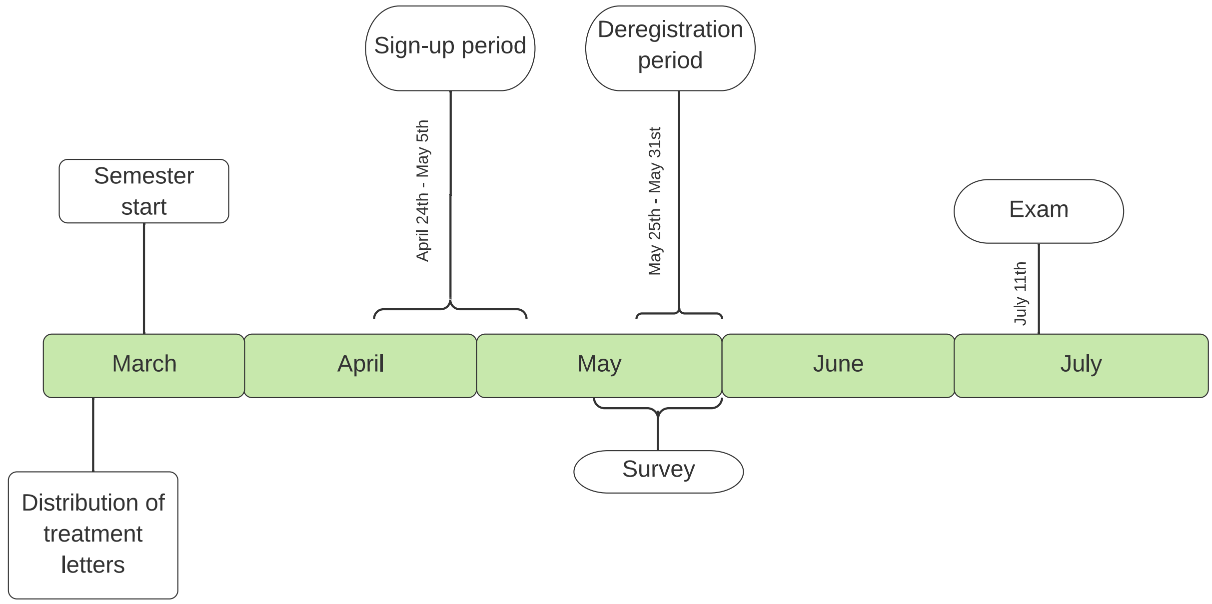


Table A.5: Targeted default – secondary outcomes

	All exams without statistics									
	Sign-up sign-up period		Pass		CP		Overall GPA		Dropout	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment (opt-out)	0.014	0.015	0.155	0.155	0.774	0.775	0.052	0.051	−0.021	−0.020
	(0.155)	(0.156)	(0.143)	(0.144)	(0.715)	(0.721)	(0.062)	(0.058)	(0.031)	(0.031)
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Balancing variables	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Mean control	5.00		3.75		18.77		2.63		0.12	
(SD)	(1.71)		(1.79)		(8.96)		(0.63)		(0.32)	
N	361		361		361		321		361	

Note: OLS estimates. Dependent variables are the number of exams signed up for after the sign-up period, passed exams (net of recognitions), number of credit (CP) net of recognitions – all variables are net of statistics – ,overall GPA (only passing grades), and a dummy for whether a student dropped out of the study program. Sign-ups, passes and GPA are weighted by the number of credits of the respective exams. Strata FE: 1st semester CP FE×dummy for early/late application; balancing variables: high school GPA, age, gender, application date. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.6: Treatment status and survey participation

	Survey participation (1)	(2)
Treatment	-0.036	-0.035
	(0.049)	(0.049)
Strata	Yes	Yes
Balancing variables	No	Yes
N	361	

Note: OLS estimates. Dependant variable is a dummy for survey participation. N(survey participants)= 145; Strata FE: 1st semester CP FE×a dummy for early/late application; balancing variables: high school GPA, age, gender, application date. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.7: Targeted default (responsives) – Balancing properties

	Treatment (1)	Control (2)	p-value (3)
Age	21.25	21.59	0.60
	(4.55)	(3.23)	
Male	0.38	0.37	0.92
	(0.49)	(0.49)	
High school GPA	2.40	2.42	0.79
	(0.45)	(0.51)	
Application days left	39.94	37.42	0.56
	(25.77)	(25.78)	
Early/late application	0.35	0.37	0.80
N	69	76	145

Note: Columns (1) and (2) display the means in the control and treatment groups. Standard deviations (SD) in parentheses. Column (3) displays t-tests of equality of means. Early/late application is a dummy where 1 corresponds to a student who applied after the median application date. Application date is coded in reverse, with the highest number corresponding to the earliest application; the variable can be interpreted as number of days left in the application period.

Table A.8: Targeted default (responsive/non-responsive students) – secondary outcomes

	All exams without statistics									
	Sign-up sign-up period		Pass		CP		Overall GPA		Dropout	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Responsive students										
Treatment (opt-out)	−0.133	−0.139	0.249	0.238	1.247	1.189	−0.062	−0.065	−0.034	−0.033
	(0.160)	(0.158)	(0.201)	(0.201)	(1.150)	(1.142)	(0.092)	(0.088)	(0.038)	(0.038)
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Balancing variables	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Mean control	5.25		4.23		21.17		2.57		0.08	
(SD)	(1.20)		(1.45)		(7.26)		(0.65)		(0.27)	
N	145		145		145		142		145	
Non-responsive students										
Treatment (opt-out)	0.154	0.158	0.120	0.134	0.600	0.672	0.143*	0.147*	−0.011	−0.002
	(0.231)	(0.237)	(0.190)	(0.193)	(1.095)	(1.115)	(0.082)	(0.078)	(0.044)	(0.045)
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Balancing variables	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Mean control	4.81		3.40		17.02		2.68		0.14	
(SD)	(1.98)		(1.94)		(9.69)		(0.60)		(0.35)	
N	216		216		216		179		216	

Note: OLS estimates. Dependent variables are the overall number of exams signed up for after the initial sign-up period, passed exams (net of recognitions), number of credits (CP) net of recognitions – all variables are net of statistics – ,overall GPA (only passing grades), and a dummy for whether a student dropped out of the study program. Sign-ups, passes, and GPA are weighted by the number of credits of the respective exams. Strata FE: 1st semester CP FE×a dummy for early/late application; balancing variables: high school GPA, age, gender, application date. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table A.9: Targeted default – interaction with 1st semester CP, full sample

	Standard default effect		Downstream default effect			
	Sign-up sign-up period	Sign-up day of exam	Participation[1]	Pass	Fail all	Fail no show
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	0.233^{*}	0.271^{**}	0.029	−0.020	0.291^{**}	0.242^{**}
	(0.130)	(0.122)	(0.102)	(0.088)	(0.120)	(0.101)
1st semester CP	0.030^{***}	0.031^{***}	0.031^{***}	0.029^{***}	0.002	0.000
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.005)
Treatment* 1st sem CP	-0.008^{*}	-0.008^{*}	0.001	0.002	-0.010^{**}	-0.009^{**}
	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Strata	Yes	Yes	Yes	Yes	Yes	Yes
Balancing variables	Yes	Yes	Yes	Yes	Yes	Yes
N	361	361	361	361	361	361

Note: OLS estimates. Dependent variables are the statistics sign-up rate after the initial sign-up period and on exam day, the participation rate, pass rate, fail rate and the fail rate because of not showing up. The 1st semester CP variable is net of transferred credits. Strata FE: 1st semester CP FE×a dummy for early/late application; balancing variables: high school GPA, age, gender, application date. [1]The exam is graded as failed if students are signed up but do not show up, i.e. participation rate = pass rate + fail rate - fail rate no show. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.10: Targeted default – interaction with 1st semester performance, (non)responsive students

	Standard default effect		Downstream default effect			
	Sign-up sign-up period	Sign-up day of exam	Participation[1]	Pass	Fail all	Fail no show
	(1)	(2)	(3)	(4)	(5)	(6)
			Responsives			
Treatment	0.674**	0.767**	1.122***	0.677*	0.090	−0.356
	(0.283)	(0.320)	(0.330)	(0.400)	(0.371)	(0.225)
1st semester CP	0.029**	0.031**	0.039***	0.043***	−0.012	−0.009
	(0.013)	(0.013)	(0.011)	(0.010)	(0.013)	(0.008)
Treatment* 1st sem CP	−0.023**	−0.026**	−0.036***	−0.021	−0.004	0.010
	(0.010)	(0.011)	(0.011)	(0.014)	(0.013)	(0.008)
Strata	Yes	Yes	Yes	Yes	Yes	Yes
Balancing variables	Yes	Yes	Yes	Yes	Yes	Yes
N	145	145	145	145	145	145
			Non-responsives			
Treatment	0.160	0.195	−0.104	−0.103	0.298**	0.299***
	(0.140)	(0.131)	(0.106)	(0.088)	(0.125)	(0.104)
1st semester CP	0.033***	0.034***	0.032***	0.026***	0.008	0.002
	(0.008)	(0.009)	(0.009)	(0.009)	(0.008)	(0.005)
Treatment* 1st sem CP	−0.005	−0.005	0.005	0.004	−0.009*	−0.009**
	(0.005)	(0.005)	(0.004)	(0.004)	(0.005)	(0.004)
Strata	Yes	Yes	Yes	Yes	Yes	Yes
Balancing variables	Yes	Yes	Yes	Yes	Yes	Yes
N	216	216	216	216	216	216

Note: OLS estimates. Dependent variables are the statistics sign-up rate after the initial sign-up period and on exam day, the participation rate, pass rate, fail rate and the fail rate because of not showing up. The 1st semester CP variable is net of transferred credits. Strata FE: 1st semester CP FE×a dummy for early/late application; balancing variables: high school GPA, age, gender, application date. [1]The exam is graded as failed if students are signed up but do not show up, i.e. participation rate = pass rate + fail rate - fail rate no show. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.11: Targeted default – survey answers

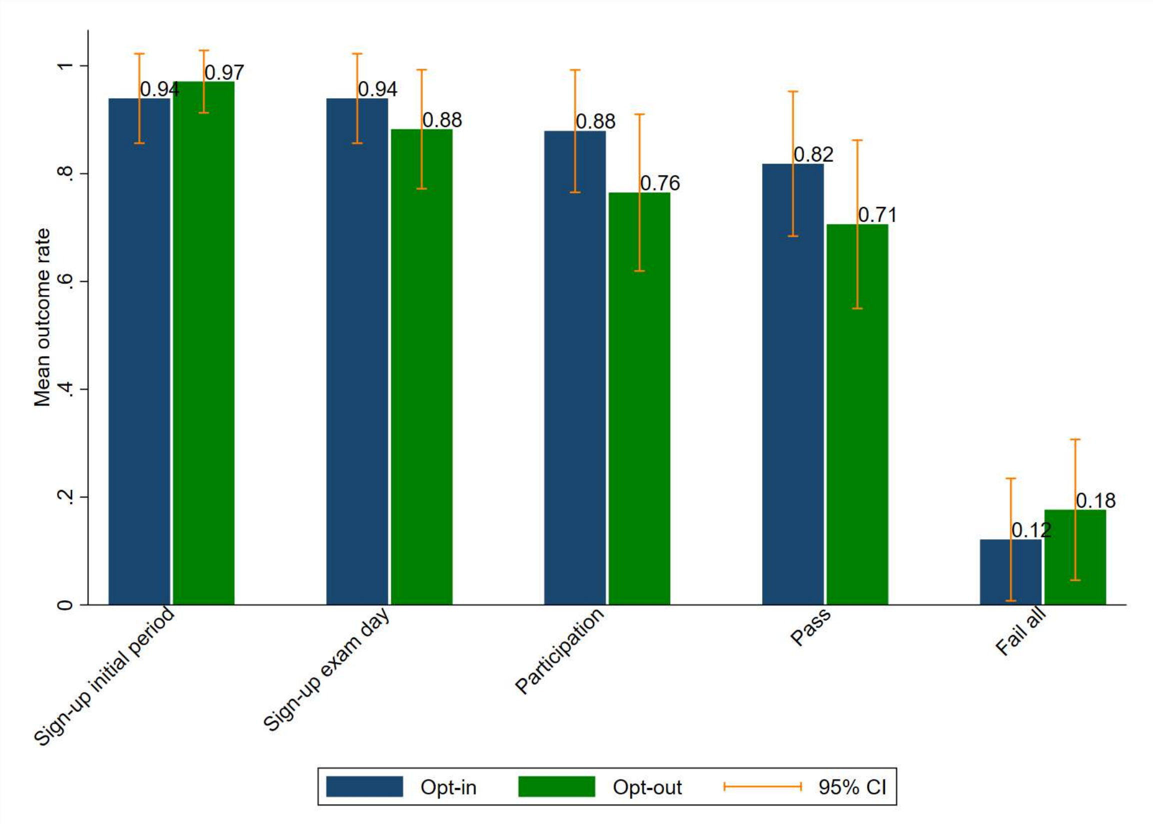
	Lecture attendance overall		Study time overall		Satisfaction with study program		Life satisfaction		Stress	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment (opt-out)	0.201	0.206*	0.377	0.360	0.286	0.275	0.184	0.200	0.034	0.058
	(0.126)	(0.122)	(0.252)	(0.246)	(0.283)	(0.282)	(0.267)	(0.267)	(0.227)	(0.223)
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Balancing variables	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Mean control	3.68		1.46		6.46		7.28		3.77	
(SD)	(0.80)		(1.28)		(1.57)		(1.74)		(1.36)	
N	142		133		139		142		142	

Note: OLS estimates. Dependent variables are the respective survey answers. Strata FE: 1st semester CP FE×a dummy for early/late application; balancing variables: high school GPA, age, gender, application date. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Questions: Lecture attendance overall: How often did you attend the lectures (including exercise classes and tutorials) in the current semester? [0 - never, i.e. 0 % of all classes, 1 - 1 % - 25 % of all classes, 2 - 26 % - 50%, 3 - 51 % - 75%, 4 - 76 % - 99 %, 5 - always, i.e. 100% of all classes]. Study time overall: On average, how many hours per week did you spend studying this semester, lectures, exercise classes and tutorials NOT included? [0 - up to 3 hours per week; 1 - over 3 up to 6 hours per week; 2 - over 6 up to 9 hours per week ; 3 - over 9 up to 12 hours per week; 4 - over 12 up to 15 hours per week ; 5 - more than 15 hours per week]. Satisfaction with study program: How satisfied are you with your studies, all things considered? [Scale from 0-10 (0=completely unsatisfied, 10=completely satisfied)]. Life satisfaction: Before we get to the actual topic of the survey, we would like to ask you about your satisfaction with your life in general: How satisfied are you with your life, all things considered? [Scale from 0-10 (0=completely unsatisfied, 10=completely satisfied)]. Stress: If you think of the current semester: To what degree do you agree with the following statements about your studies? With my studies I associate stress. [Scale from 0-6 (0=completely disagree, 6=completely agree)].

IB Study Program

Figure A.4: Targeted default (IB) – mean outcomes in the control vs. treatment group



Note: $N = 67$; N(opt-in) = 33; N(opt-out) = 34. The sum of "pass" and "fail all" differs from "sign-up exam day" due to rounding. "Fail all" consists of failed exams due to unsufficient performance plus fails due to non-participation upon sign-up.

Table A.12: Targeted default (IB) – balancing properties

	Treatment	Control	p-value
	(1)	(2)	(3)
Age	20.21	20.06	0.77
	(1.89)	(2.08)	
Male	0.35	0.42	0.56
	(0.49)	(0.50)	
High school GPA	1.87	1.84	0.82
	(0.53)	(0.58)	
Application date (days left)	28.24	27.94	0.96
	(23.19)	(22.39)	
Early/late application	0.50	0.52	0.90
	(0.51)	(0.51)	
N	34	33	67

Note: Columns (1) and (2) display the means in the control and treatment groups. Column (3) displays t-tests of equality of means. Standard deviations (SD) in parentheses. Early/late application is a dummy where 1 corresponds to a student who applied after the median application date. Application date is coded in reverse, with the highest number corresponding to the earliest application; the variable can be interpreted as number of days left in the application period.

Table A.13: Targeted default (IB) – standard default effect

	Sign-up rate			
	Sign-up period		Exam day	
	(1)	(2)	(3)	(4)
Treatment (opt-out)	0.030	0.029	−0.059	−0.051
	(0.047)	(0.047)	(0.066)	(0.063)
Strata FE	Yes	Yes	Yes	Yes
Balancing variables	No	Yes	No	Yes
Persuasion rate	0.50	0.48	0	0
Mean control	0.94		0.94	
(SD)	(0.24)		(0.24)	
N	67		67	

Note: OLS estimates. Dependent variables are the indicators for sign-up in statistics after the sign-up period and on exam day. Strata FE: 1st semester CP FE×a dummy for early/late application; balancing variables: high school GPA, age, gender, application date. The persuasion rate is calculated as $\frac{y_T - y_C}{1 - y_C}$ where y_T and y_C are the outcomes in the treatment (T) and control (C) group. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.14: Targeted default (IB) – downstream default effect

	Participation[1]		Pass		Fail all		Fail no show		Statistics grade		Doctor's note	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treatment (opt-out)	−0.112	−0.110	−0.106	−0.104	0.047	0.052	0.053	0.059	0.393	0.402	0.089*	0.080*
	(0.087)	(0.087)	(0.087)	(0.085)	(0.082)	(0.083)	(0.071)	(0.074)	(0.320)	(0.317)	(0.051)	(0.045)
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Balancing variables	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Persuasion rate	0	0	-	-	-	-	-	-	-	-	-	-
Mean control	0.88		0.82		0.12		0.06		2.24		0.00	
(SD)	(0.33)		(0.39)		(0.33)		(0.24)		(1.35)		(0.00)	
N	67		67		67		67		61		67	

Note: OLS estimates. Dependent variables are the statistics participation rate, pass rate, fail rate, fail rate because of not showing up and the statistics grade (includes fails). Strata FE: 1st semester CP FE×a dummy for early/late application; balancing variables: high school GPA, age, gender, application date. The persuasion rate is calculated as $\frac{y_T - y_C}{1 - y_C}$ where y_T and y_C are the outcomes in the treatment (T) and control (C) group. There is no persuasion rate for pass as students can only be "persuaded" to sign up and participate. [1]The exam is graded as failed if students are signed up but do not show up, i.e. participation rate = pass rate + fail rate - fail rate no show. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.15: Targeted default (IB) – Secondary outcomes

	All exams without statistics									
	Sign-up sign-up period		Pass		CP		Overall GPA		Dropout	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment (opt-out)	0.423	0.416	−0.083	−0.113	−0.417	−0.566	−0.040	−0.034	0.053	0.057
	(0.344)	(0.335)	(0.365)	(0.373)	(1.825)	(1.864)	(0.135)	(0.137)	(0.069)	(0.074)
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Balancing variables	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Mean control	5.38		4.23		21.15		2.34		0.06	
(SD)	(2.27)		(1.59)		(7.94)		(0.63)		(0.24)	
N	67		67		61		67		67	

Note: OLS estimates. Dependent variables are the number of exams signed up for after the sign-up period, passed exams (net of recognitions), number of credit (CP) net of recognitions – all variables are net of statistics – ,overall GPA (only passing grades), and a dummy for whether a student dropped out of the study program. Sign-ups, passes and GPA are weighted by the number of credits of the respective exams. Strata FE: 1st semester CP FE×dummy for early/late application; balancing variables: high school GPA, age, gender, application date. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.16: Survey questions targeted default

No.	Question
1	*Before we get to the actual topic of the survey, we would like to ask you about your satisfaction with your life in general: How satisfied are you with your life, all things considered?* [Scale from 0-10 (0=Completely unsatisfied, 10=Completely satisfied); -1 "No answer"]
2	*Now we would like to know more about what it means to study at our faculty: How often did you attend the lectures (including exercise classes and tutorials) in the current semester?* [0 - never, i.e. 0 % of all classes, 1 - 1 % - 25 % of all classes, 2 - 26 % - 50%, 3 - 51 % - 75%] [4 - 76 % - 99 %, 5 - always, i.e. 100% of all classes, -1 "No answer"]
3	*On average, how many hours per week did you spend studying this semester, lectures, exercise classes and tutorials NOT included?* [0 - up to 3 hours per week; 1 - over 3 up to 6 hours per week] [2 - over 6 up to 9 hours per week; 3 - over 9 up to 12 hours per week] [4 - over 12 up to 15 hours per week; 5 - more than 15 hours per week; -1 "No answer"]
4	*If you think of the current semester: To what degree do you agree with the following statements about your studies? With my studies I associate... ...Stress* [Scale from 0-6 (0=Completely disagree, 6=Completely agree); -1 "No answer"]
5	*Now we would like to ask you about your satisfaction with studying in general: How satisfied are you with your studies, all things considered?* [Scale from 0-10 (0=Completely unsatisfied, 10=Completely satisfied); -1 "No answer"]
6	*Do you visit the lecture and/or tutorial in Statistics in the current semester?* [Yes (1), No (0); -1 "No Answer"]
7	*How often have you attended the Statistics lectures (including exercises and tutorials) in the current semester?* [0 - never, i.e. 0 % of all classes, 1 - 1 % - 25 % of all classes, 2 - 26 % - 50%, 3 - 51 % - 75%] [4 - 76 % - 99 %, 5 - always, i.e. 100% of all classes, -1 "No answer"]
8	*On average, how many hours per week did you spend studying [BW: Business-] Statistics this semester, lectures and tutorials NOT included?* [0 - up to one hour per week; 1 - over 1 up to 2 hours per week] [2 - over 2 up to 3 hours per week; 3 - over 3 up to 4 hours per week] [4 - over 4 up to 5 hours per week; 5 - more than 5 hours per week; -1 "No answer"]

Figure A.5: Broad default: letter students in the control group received prior to the initial sign-up period

[redacted]

[redacted]
Postfach ▪ [redacted]
##

Frau / Herrn
Vorname Nachname
Straße Hausnummer
PLZ Wohnort

[redacted]

[redacted]
Anfahrtsskizze unter: [redacted]

Ihr Zeichen:
Ihre Nachricht vom

Unser Zeichen/AZ:

Ansprechpartner/in:
[redacted]

[redacted]

[redacted]

Prüfungsanmeldung im Wintersemester [redacted]

Sehr geehrte/r Frau/Herr Nachname,

hiermit erhalten Sie wichtige Informationen zur Prüfungsanmeldung im Wintersemester [redacted]. Die Fakultät [redacted] prüft derzeit verschiedene Anmeldeverfahren. In diesem Zusammenhang wurden Sie zufällig dem Verfahren „**Selbstanmeldung**“ zugeordnet. Das bedeutet:

Für alle Prüfungen, die Sie im Wintersemester [redacted] ablegen möchten, müssen Sie sich im Zeitraum der Prüfungsanmeldung ([redacted] bis [redacted]) selbstständig im Onlineportal der Hochschule [redacted] anmelden.

Rücktritte von der Prüfungsanmeldung sind im Zeitraum [redacted] bis [redacted] ebenfalls über das Onlineportal möglich. Der Studienplan empfiehlt den umseitig abgedruckten Studienaufbau.

Sollten Sie noch Fragen haben, besuchen Sie bitte die „Infoveranstaltung I“ der Prüfungskommission BW am [redacted], Beginn: 13:10 Uhr, im Raum [redacted]

Ich wünsche Ihnen einen guten Start in Ihr Studium!

Mit freundlichen Grüßen

[redacted]

Fakultät [redacted]

1/2

[redacted] ▪ [redacted] ▪ [redacted] ▪ Telefon: [redacted] ▪ Telefax: [redacted]
Internet: [redacted] ▪ E-Mail: [redacted] ▪▪▪ Konto: [redacted] ▪ IBAN: [redacted] ▪ Swift/BIC: [redacted] ▪ Bankname: [redacted] ▪ USt-ID-Nr.: [redacted] ▪ Steuer-Nr.: [redacted] ▪ EORI-Nr.: [redacted]

Figure A.6: Broad default: letter students in the control group received prior to the initial sign-up period - english translation

[redacted]

[redacted]
##

Frau / Herrn
Vorname Nachname
Straße Hausnummer
PLZ Wohnort

Fakultät [redacted]

[redacted]

Ihr Zeichen:
Ihre Nachricht vom

Unser Zeichen/AZ:

Ansprechpartner/in:
[redacted]

[redacted]

Exam registration in the winter semester [redacted]

Dear Mr/Ms,

This letter provides you with important information about the exam registration procedure in the winter semester [redacted] The faculty of [redacted] is currently testing different registration procedures. In this context you were randomly assigned to the procedure „**self-registration**". This means:

For all exams, that you want to take in the winter semester [redacted], you have to self-register during the registration period ([redacted] bis [redacted]) via the web portal of the University [redacted]

De-registrations from signed-up exams are also possible via the web portal during the time period between [redacted] bis [redacted] The study plan recommends the study curriculum on the back site of this letter.

If you have any questions, please visit the "Information-event 1" of the examination board BW on [redacted], starting time: 01:10 pm, [redacted]

I wish you a good start into your studies!

Yours sincerely

[redacted]

Fakultät [redacted]

1/2

[redacted]

Figure A.7: Broad default: letter students in the treatment group received prior to the initial sign-up period

[redacted]

[redacted]
Postfach ▪ [redacted]
##

Frau / Herrn
Vorname Nachname
Straße Hausnummer
PLZ Wohnort

[redacted]

Anfahrtsskizze unter: [redacted]

Ihr Zeichen:
Ihre Nachricht vom

Unser Zeichen/AZ:

Ansprechpartner/in:
[redacted]

Prüfungsanmeldung im Wintersemester [redacted]

Sehr geehrte/r Frau/Herr Nachname,

hiermit erhalten Sie wichtige Informationen zur Prüfungsanmeldung im Wintersemester [redacted] Die Fakultät [redacted] prüft derzeit verschiedene Anmeldeverfahren. In diesem Zusammenhang wurden Sie zufällig dem Verfahren „**Automatische Anmeldung**" zugeordnet. Das bedeutet:

Wir haben Sie mit Beginn des Wintersemesters [redacted] automatisch für die laut Studienplan empfohlenen Prüfungen des 1. Semesters (siehe blaue Tabelle, Rückseite) angemeldet. Für diese Prüfungen müssen Sie sich daher im Zeitraum der Prüfungsanmeldung nicht selbstständig anmelden. Die Plätze in den Prüfungen haben wir bereits für Sie reserviert.

Im Zeitraum der Prüfungsanmeldung ([redacted] bis [redacted]) können Prüfungen über das Onlineportal der Hochschule [redacted] hinzuzufügt oder abgewählt werden. Rücktritte von der Prüfungsanmeldung sind im Zeitraum [redacted] bis [redacted] ebenfalls über das Onlineportal möglich. Der Studienplan empfiehlt den umseitig abgedruckten Studienaufbau.

Sollten Sie noch Fragen haben, besuchen Sie bitte die „Infoveranstaltung II" der Prüfungskommission BW am [redacted] Beginn: 13:10 Uhr, im Raum [redacted]

Ich wünsche Ihnen einen guten Start in Ihr Studium!

[redacted]

Fakultät [redacted]

1/2

[redacted] ▪ [redacted] ▪ [redacted] ▪ Telefon: [redacted] ▪ Telefax: [redacted]
Internet: [redacted] ▪ E-Mail: [redacted] ▪▪▪ Konto: [redacted] ▪ IBAN: [redacted] ▪ Swift/BIC: [redacted] ▪ Bankname: [redacted] ▪ USt-ID-Nr.: [redacted] ▪ Steuer-Nr.: [redacted] ▪ EORI-Nr.: [redacted]

Figure A.8: Broad default: letter students in the treatment group received prior to the initial sign-up period - english translation

[redacted]

[redacted]

##

Frau / Herrn
Vorname Nachname
Straße Hausnummer
PLZ Wohnort

Fakultät [redacted]

[redacted]

Ihr Zeichen:
Ihre Nachricht vom

Unser Zeichen/AZ:

Ansprechpartner/in:
[redacted]

Exam registration in the winter semester [redacted]

Dear Mr/Ms,

This letter provides you with important information about the exam registration procedure in the winter semester [redacted] The faculty of [redacted] is currently testing different registration procedures. In this context you were randomly assigned to the procedure „**automatic-registration** “. This means:

With the start of the winter semester [redacted] we automatically registered you for all recommended exams according to the study plan for the first semester (see the blue table on the back site). For these exams you <u>don't</u> have to self-register anymore. We already reserved spots in the exams for you.

During the registration period ([redacted] bis [redacted]) exams can be added or dropped via the web portal of the University [redacted]. De-registrations from registered exams are also possible via the web portal during the time period between [redacted] bis [redacted] The study plan recommends the study curriculum on the back site of this letter.

If you have any questions, please visit the "Information-event 1" of the examination board BW on [redacted], starting time: 01:10 pm, room [redacted]

I wish you a good start into your studies!

Yours sincerely

[redacted]

Fakultät [redacted]

1/2

[redacted]

Figure A.9: Broad default: treatment and control letter - page 2

Studienplan erster Studienabschnitt

	Studienplan 1. Semester
G1	Allgemeine BWL [1]
G2	Wirtschaftsmathematik [1]
G3	Buchführung und Bilanzierung
G4	Virtuelle Unternehmensführung
G5	Mikroökonomie
G6	Wirtschaftsinformatik

	Studienplan 2. Semester
G7	Betriebliche Steuern [2]
G8	Betriebsstatistik
G9	Kosten- und Leistungsrechnung [2]
G10	Wirtschaftsenglisch
G11	Makroökonomie
G12	Wirtschaftsprivatrecht

Nähere Informationen entnehmen Sie bitte der Studien- und Prüfungsordnung [redacted] bzw. dem Studienplan, die Sie auf unserer Web-Seite finden.

---

1) Teil der Grundlagen- und Orientierungsprüfung nach § [redacted].

2) Die Fächer Betriebliche Steuern sowie Kosten- und Leistungsrechnung werden auch im Wintersemester [redacted] im Format der [redacted] angeboten und können nach erfolgter Anmeldung bereits im 1. Semester abgelegt werden.

2/2

Figure A.10: Broad default: treatment and control letter - page 2 - english translation

[redacted]

Study plan first study section

Study plan 1. semester	
G1	Business Administration [1)]
G2	Math [1)]
G3	Accounting
G4	Management
G5	Microeconomics
G6	Informatics

Studienplan 2. Semester	
G7	Taxation[2)]
G8	Statistics
G9	Cost- and Activity Accounting[2)]
G10	Business English
G11	Macroeconomics
G12	Business Law

For more information, please find the study- and examination regulations [redacted] and the study curriculum, which you can find on our website.

1) Part of the orientation phase according to [redacted].

2) The subjects Taxation and Cost- and Activity Accounting will also be offered in the winter semester [redacted] according to [redacted] and can be already completed in the first semester upon successful registration.

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Figure A.11: Targeted default: letter students in the control group received prior to the initial sign-up period

[redacted]

[redacted]

[redacted]

Fakultät [redacted]

[redacted]

Ihr Zeichen:
Ihre Nachricht vom

Unser Zeichen/AZ: [redacted]

Ansprechpartner/in:

[redacted]

Prüfungsanmeldung im Sommersemester [redacted]

Sehr geehrte Frau [redacted],

mit diesem Brief erhalten Sie wichtige Informationen zur Prüfungsanmeldung im Sommersemester [redacted] Unsere Fakultät prüft derzeit im Rahmen der Veranstaltung Betriebsstatistik verschiedene Verfahren der Prüfungsanmeldung. Ihnen wurde per Zufallsauswahl das selbständige Anmeldeverfahren zugeordnet. Das bedeutet:

Im Fach Betriebsstatistik sind Sie nicht zur Klausur angemeldet. Wenn Sie Betriebsstatistik im Sommersemester [redacted] ablegen möchten, können Sie sich im Zeitraum der Prüfungsanmeldung über [redacted] anmelden.

Für alle anderen Fächer gilt ebenfalls das selbständige Anmeldeverfahren. Dies bedeutet, Sie müssen sich über [redacted] auch für alle weiteren Prüfungen anmelden, die Sie ablegen möchten.

Im Zeitraum der Prüfungsanmeldung ([redacted] bis [redacted]) können Sie sich über [redacted] zu Prüfungen anmelden oder von Prüfungen abmelden. Rücktritte von der Prüfungsanmeldung sind zudem im Zeitraum [redacted] bis [redacted] über [redacted] möglich.

Ich wünsche Ihnen einen guten Start ins 2. Semester!

Mit freundlichen Grüßen

[redacted]

[redacted]

Figure A.12: Targeted default: letter students in the control group received prior to the initial sign-up period

[redacted]

[redacted]
Postfach ▪ [redacted]
##

Frau / Herrn
Vorname Nachname
Straße Hausnummer
PLZ Wohnort

[redacted]

Ihr Zeichen:
Ihre Nachricht vom

Unser Zeichen/AZ: [redacted]

Ansprechpartner/in:
[redacted]

Exam registration in the summer semester [redacted]

Dear Mr/Ms,

This letter provides you with important information about the exam registration procedure in the summer semester [redacted]. Our faculty is currently testing different registration procedures in the Statistics course. You have been randomly assigned the self-registration procedure. This means:

In the Statistics course you are not registered for the exam. If you wish to take the Statistics exam in the summer semester [redacted] you can register via [redacted] during the exam registration period.

For all other courses the self-registration procedure applies as well. This means, you must also register via [redacted] for all further exams you wish to take.

During the exam registration period ([redacted] to [redacted]) you can register for exams or de-register from exams via [redacted] De-registration from exams is also possible via [redacted] in the period from [redacted] to [redacted]

I wish you a good start into the 2nd semester!

Yours sincerely

[redacted]

1/1

[redacted] ▪ [redacted] ▪ [redacted] ▪ Telefon: [redacted] ▪ Telefax: [redacted]
Internet: [redacted] ▪ E-Mail [redacted] ▪▪▪ Konto: [redacted] ▪ IBAN: [redacted] ▪ Swift/BIC: [redacted] ▪ Bankname: [redacted] ▪ USt-ID-Nr.: [redacted] ▪ Steuer-Nr.: [redacted] ▪ EORI-Nr.: [redacted]

Figure A.13: Targeted default: letter students in the treatment group received prior to the initial sign-up period

[redacted]

[redacted]

Fakultät [redacted]

[redacted]

Ihr Zeichen:
Ihre Nachricht vom

Unser Zeichen/AZ: [redacted]

Ansprechpartner/in:

[redacted]

Prüfungsanmeldung im Sommersemester [redacted]

Sehr geehrter Herr [redacted],

mit diesem Brief erhalten Sie wichtige Informationen zur Prüfungsanmeldung im Sommersemester [redacted] Unsere Fakultät prüft derzeit im Rahmen der Veranstaltung Betriebsstatistik verschiedene Verfahren der Prüfungsanmeldung. Ihnen wurde per Zufallsauswahl das automatische Anmeldeverfahren zugeordnet. Das bedeutet:

Im Fach Betriebsstatistik sind Sie bereits zur Klausur angemeldet.* Wenn Sie Betriebsstatistik im Sommersemester [redacted] nicht ablegen möchten, können Sie sich im Zeitraum der Prüfungsanmeldung über [redacted] abmelden.

Für alle anderen Fächer gilt das selbständige Anmeldeverfahren. Dies bedeutet, Sie müssen sich über [redacted] für alle weiteren Prüfungen anmelden, die Sie ablegen möchten.

Im Zeitraum der Prüfungsanmeldung ([redacted] bis [redacted]) können Sie sich über [redacted] zu Prüfungen anmelden oder von Prüfungen abmelden. Rücktritte von der Prüfungsanmeldung sind zudem im Zeitraum [redacted] bis [redacted] über [redacted] möglich.

Ich wünsche Ihnen einen guten Start ins 2. Semester!

Mit freundlichen Grüßen

[redacted]

* Stand: [redacted] Falls Sie sich während des Sommersemesters in einem Urlaubssemester befinden, werden Sie von uns nicht automatisch zur Prüfung in Betriebsstatistik angemeldet. Allgemeine Informationen zum Urlaubssemester erhalten Sie vom Studienbüro.

[redacted]

Figure A.14: Targeted default: letter students in the treatment group received prior to the initial sign-up period

[redacted]

[redacted]
Postfach ▪ [redacted]
##

Frau / Herrn
Vorname Nachname
Straße Hausnummer
PLZ Wohnort

[redacted]
Anfahrtsskizze unter: [redacted]

Ihr Zeichen:
Ihre Nachricht vom

Unser Zeichen/AZ: [redacted]

Ansprechpartner/in:
[redacted]

Exam registration in the summer semester [redacted]

Dear Mr/Ms,

This letter provides you with important information about the exam registration procedure in the summer semester [redacted] Our faculty is currently testing different registration procedures in the Statistics course. You have been randomly assigned the automatic registration procedure. This means:

In the Statistics course you are already registered for the exam.* If you do not wish to take the Statistics exam in the summer semester [redacted] you can de-register via [redacted] during the exam registration period.

For all other courses the self-registration procedure applies. This means, you must register via [redacted] for all further exams you wish to take.

During the exam registration period ([redacted] to [redacted] you can register for exams or de-register from exams via [redacted] De-registration from exams is also possible via [redacted] in the period from [redacted] to [redacted]

I wish you a good start into the 2nd semester!

Yours sincerely

[redacted]

Dean Faculty Business Administration

* As of [redacted] If you are on academic leave (*Urlaubssemester*) during the summer semester we will not automatically register you for the Statistics exam. General information on the *Urlaubssemester* provides the student office.

1/1

[redacted] ▪ [redacted] ▪ [redacted] ▪ Telefon: [redacted] ▪ Telefax: [redacted]
Internet: [redacted] ▪ E-Mail: [redacted] ▪▪▪ Konto: [redacted] ▪ IBAN: [redacted] ▪ Swift/BIC: [redacted] ▪ Bankname: [redacted] ▪ USt-ID-Nr.: [redacted] ▪ Steuer-Nr.: [redacted] ▪ EORI-Nr.: [redacted]