Information Framing and Student Decisions: Evidence from an Opportunity Cost Intervention

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Abstract: Opportunity costs are central to economic decision-making but often neglected. In a pre-registered experiment with 2,222 German university freshmen, one treatment provides salary information; another additionally frames it as the opportunity cost of delayed graduation. Only the opportunity cost framing causes students to update salary expectations. We find no effect on academic progress but a 2.8 percentage points increase in first-semester dropout ($p = 0.080$), concentrated among high-dropout-probability students (5.9 pp, $p = 0.025$). For these marginal students, dropping out instead of progressing faster is the actionable margin. By semester three, dropout rates converge, suggesting acceleration of eventual exits rather than additional dropouts.

Keywords: Natural Field Experiment, Opportunity Cost Neglect, Earnings Expectations, Academic Achievement

JEL Classification: C93, D84, D91, I21, I23

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1 Introduction

Opportunity costs – the value of the best alternative forgone – are central to economic decision-making. Yet a growing number of experiments in low-stakes laboratory settings suggest that individuals often fail to consider them spontaneously and that making them salient affects choices (Frederick et al., 2009; Maguire et al., 2023). Whether such neglect extends to high-stakes decisions outside the lab remains an open question. We extend the approach of making opportunity costs salient to a field setting where opportunity costs are large and decisions consequential.

Higher education is an ideal setting. For most individuals, the largest part of the private cost of attending university is the income they give up by not working (Becker, 1993). This cost compounds, as each additional semester that it takes a student to complete their university degree implies a potential loss of income that can be earned with that degree. Yet across OECD countries, fewer than 40% of students graduate on time (OECD, 2022).[1] If students do not consider opportunity costs in their decisions, making them salient could affect how quickly they progress. Indeed, initiatives like “15 to finish" in the US, which promote taking 15 credits per semester, emphasize the opportunity cost of delayed graduation to encourage on-time completion.[2]

In a pre-registered field experiment at a German university, we test whether providing information about the opportunity costs of delayed graduation increases academic progress. The institutional context is well-suited: students enroll in a specific program from the outset rather than choosing a major later;[3] there are no tuition fees, therefore opportunity costs account for a large portion of the total private cost of delaying graduation;[4] currently only about 30% of German students graduate on time, so there is sufficient scope for the information to change behavior.[5]

At the beginning of their first semester, we randomly assigned $2,222$ students pursuing 21 different bachelor’s degrees to control and two treatment groups. In the *T1: salary in-*

[1] In the United States, four-year graduation rates of students entering first-time, full-time four-year bachelor programs in 2014 were only 47% (`https://nces.ed.gov/programs/digest/d21/tables/dt21_326.10.asp`, accessed on January 28, 2026).

[2] See `https://completecollege.org/our-approach/momentum/15-to-finish-stay-on-track/`, or `https://uknow.uky.edu/research/undergraduate/15-finish-simple-success-strategy-uk-students`, accessed on February 02, 2026.

[3] We can therefore provide freshmen with information that remains relevant throughout their studies, and isolate its effects on academic achievement. When information is provided just before students declare a major, it becomes difficult to disentangle achievement effects from effects on major choice.

[4] This is true for the majority of German higher education institutions. There are some private institutions that charge tuition fees, but their market share is low (11.6% in 2021). See `https://www.destatis.de/DE/Presse/Pressemitteilungen/2023/10/PD23_N054_21.html`, accessed on October 14, 2025.

[5] See `https://www.destatis.de/DE/Themen/Gesellschaft-Umwelt/Bildung-Forschung-Kultur/Bildungsindikatoren/absolventen-regelstudienzeit-tabelle.html`, accessed on January 28, 2026.

formation group, students received letters with information about the first-year full-time salary typically earned by graduates of their bachelor's program at this university. In the *T2: salary and opportunity cost information* group, students additionally received the information that each additional semester of studying can result in the loss of approximately half of this salary.[6] Framing salaries as the opportunity cost of delayed graduation can make a difference because even students aware of future salaries may not spontaneously categorize them as a cost tied to their present decisions (Costa-Ramón et al., forthcoming). By making this connection explicit at a critical stage for on-time graduation (Angrist et al., 2022), the intervention aimed to help students stay on track. Since earnings information alone could already change behavior (Wiswall and Zafar, 2015; Conlon, 2021), we include the pure salary information treatment T1. This two-treatment design allows us to isolate the role of opportunity cost framing from information content.

Our first finding is that the framing of salary information affects how students process it. We collected annual salary expectations from incoming students during the application and enrollment process, but before university started. 80% underestimate the typical first-year salary; on average, by €9,968. In a follow-up survey six weeks after treatment, T2 participants report more accurate and more confident salary expectations, indicating that they have processed and also retained the information. Students in T1, on the other hand, show no updated salary expectations when given the same information as T2 but without the opportunity cost framing.

If updated salary expectations translate to behavior, T2 students should progress faster toward graduation, as higher expected salaries should increase effort. But even without updating, the opportunity cost framing could affect behavior if it increases the salience of the cost dimension by making the connection of future wages to current choices explicit. We find no effect on academic pace. Despite updated expectations, T2 has no effect on course credits signed up for, attempted, or passed. We can rule out positive effects above 0.05 SD, an effect size typically considered small, even in educational settings (Kraft, 2020, 2023). We also pre-registered dropout as a secondary outcome. We find that the opportunity cost treatment T2 increases dropout by 2.8 pp ($p = 0.080$), relative to a control mean of 10%. Consistent with the lack of expectation updating, T1 shows no effect on any academic outcome.

Why does the opportunity cost framing lead to higher dropout rather than faster progress? There are two ways to avoid the opportunity costs of delayed graduation: speed up, or exit and enter the labor market sooner (directly, or after finishing a different degree – which will also be obtained sooner if dropout occurs sooner). In a labor market where incomplete degrees carry little value (Jaeger and Page, 1996; Stans et al., 2025), students unlikely to graduate gain little from continued enrollment. They accumulate costs without real-

[6]To keep the treatment brief and concise, we do not refer to loss of end-of-career earnings or explain the underlying logic that delayed graduation shifts the income stream forward in time. For details see Footnote 20.

istic prospects of obtaining the degree premium. For them, exiting to enter the labor market sooner is a more actionable way of avoiding opportunity cost than speeding up, which requires sustained effort against institutional constraints and ability limits. We develop a simple model of limited attention to opportunity costs that formalizes this intuition and yields a testable prediction: treatment effects should concentrate among marginal students.

Consistent with the model, treatment effects concentrate among students with high predicted dropout probability: T2 increases dropout by 5.9 pp in this group ($p = 0.025$), with no effect among students with low predicted dropout probability ($p = 0.041$ for the difference). T1 shows no heterogeneous effects. This pattern also helps distinguish mechanisms. If T2 worked simply by making students process the salary information, we would expect faster progress toward the degree premium, not higher dropout among those least likely to earn it.

To assess the implications of this result for policy, we must evaluate whether the opportunity cost framing in T2 causes additional dropout or accelerates eventual exits. By the third semester, dropout rates converge across treatment and control, suggesting accelerated dropout, not additional attrition.

We contribute novel evidence on opportunity cost neglect from a consequential field setting. Laboratory experiments show that emphasizing opportunity costs of a choice reduces willingness to pay for that choice (Frederick et al., 2009), with similar findings in charitable giving and public policy (Zhang et al., 2017; Moche et al., 2020; Plantinga et al., 2018; Persson and Tinghög, 2020), and in intertemporal choices (Zhao et al., 2015; Read et al., 2017; Spiller, 2019). A meta-analysis (Maguire et al., 2023) confirms the phenomenon is robust across domains (Cohen's $d = 0.22$).

Our findings complicate this picture. In laboratory settings, highlighting opportunity costs often produces straightforward effects. In complex environments like university, effects are less predictable. We designed a pre-registered intervention to accelerate graduation, but it accelerated dropout instead. This behavioral response is rational, as exit is probably the only way to avoid ongoing opportunity costs for marginal students. One other field study reports null effects of an opportunity cost intervention (Kristal and Whillans, 2020). Even in controlled laboratory settings, opportunity cost reminders can affect unintended subgroups or produce effects opposite to expectations (Thunström et al., 2018). Our results suggest that de-biasing to change behavior in real-world environments can work, but anticipating how individuals respond requires a deep understanding of the actionable margins.

In addition, we contribute to research on how information about labor market returns affects the decisions of (prospective) college students. Both theoretical and empirical work shows that salary expectations affect enrollment and major choices, yet they are often inaccurate (Altonji et al. (2012) and Giustinelli (2023) for reviews). Several recent studies inform prospective students or students who have yet to specify a major about the returns to tertiary education and examine the impact on their enrollment and major choice (Wiswall and

Zafar, 2015; Baker et al., 2018; Bleemer and Zafar, 2018; Conlon, 2021). Our study is the first to emphasize the opportunity cost component implicitly contained in such salary information. We show that students who have already decided on their major do not process or retain salary information unless its implications for current decisions are emphasized. We are also the first to examine effects of salary information on the intensive margin of educational investment, i.e., academic achievement.[7] Our T1 shows no evidence that providing pure salary information affects student performance.

Sections 2 and 3 describe the institutional background and research design. In Section 4, we present the pre-registered main effects on expectations and academic outcomes. In Section 5, guided by a simple theoretical model, we use pre-registered and exploratory analyses to investigate the mechanisms behind the increase in dropout, as well as the absence of the hypothesized effects on academic progress. Section 6 concludes.

2 Institutional Background

We conducted the experiment in the fall of 2021 with almost all incoming bachelor's students at the Technische Hochschule Nürnberg Georg Simon Ohm (Nuremberg Tech).[8] Nuremberg Tech is one of the largest German public universities of applied sciences (UAS), and is broadly representative of UAS in Germany. About 40% of German freshmen enroll at UAS (Destatis, 2022), which are more practice-oriented than research universities; e.g., students often complete mandatory internships. The setting has three features relevant to our study.

First, students enroll in a specific program from the outset rather than choosing a major later. Therefore, our treatment cannot affect major choice, which would complicate the interpretation of results. Our sample includes 2,222 students across 21 mostly STEM-related programs (Table A.1).[9] The five largest programs (Business Administration, Social Work, Mechanical Engineering, Electrical Engineering and Information Technology, and Civil Engineering) account for over half of our sample.

Second, there are no tuition fees at public universities in Germany, so opportunity costs

[7] Prior interventions have provided combined information about returns, costs, and financial aid options, making it difficult to determine which information drives the effects (Oreopoulos and Dunn, 2013; McGuigan et al., 2016; Peter and Zambre, 2017; Kerr et al., 2020; Peter et al., 2021; Ballarino et al., 2022; Berkes et al., 2022). Even among these studies, few consider success in college as an outcome (Peter et al., 2021; Ballarino et al., 2022).

[8] See `https://www.th-nuernberg.de/en/university-region/organization-and-structure`, accessed on February 11, 2026. We excluded two study programs, Design and Architecture, because they have very different study structures.

[9] All programs are organized according to the European Credit Transfer System (ECTS), with 30 credits required per semester (210 total). A full-time academic year consists of 60 credits, with a typical workload of 25–30 hours per credit. See `https://education.ec.europa.eu/education-levels/higher-education/inclusive-and-connected-higher-education/european-credit-transfer-and-accumulation-system`.

account for most of the private costs of studying.[10] Monthly living expenses for the median German student were €800 in 2021, of which only €33 was tuition and €41 semester fees (Kroher et al., 2023). Despite low direct costs, returns to higher education are substantial: bachelor's graduates earn 67% more than those with only secondary education (OECD, 2022).

Third, on-time graduation is rare. Only 15% of students at this university graduate within the standard duration of seven semesters (81% within standard duration plus one year). Nationwide, 30% of German bachelor's graduates finish on time, and 72% within standard duration plus one year.[11]

3 Research Design

3.1 Overview

At the start of the winter term 2021, we randomly assigned 2,222 incoming bachelor's students at the Nuremberg Tech to a control group and two treatment groups. Randomization was performed using threshold blocking within programs (Higgins et al., 2016); the pre-analysis plan in Appendix C provides details. Figure 1 shows the timeline of the intervention.[12]

The intervention was deployed by mail. All students received a letter with the university seal shortly after the semester began, but before signing up for exams. A second identical letter was sent approximately six weeks before the exam period. The letters differed across the experimental groups only in the information provided about salaries and opportunity costs.

3.2 Hypotheses and Treatments

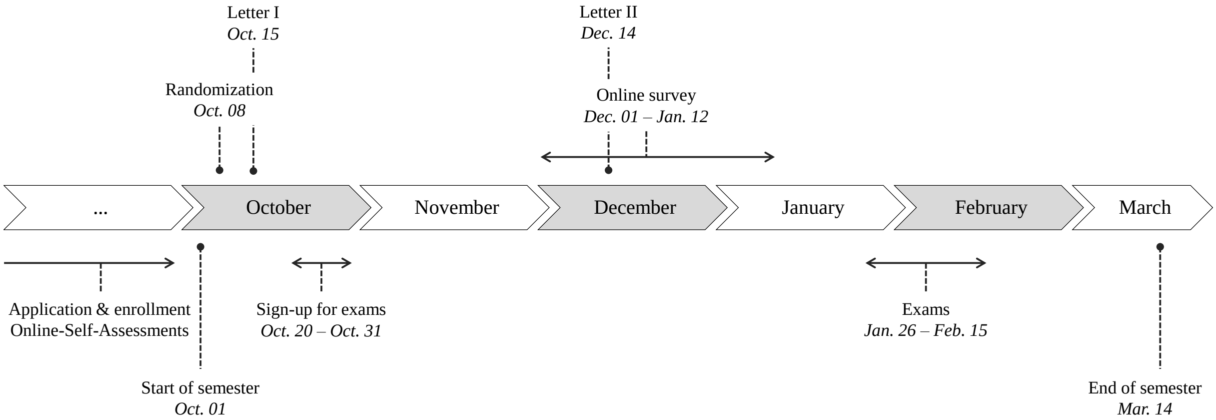
We designed this intervention to test two pre-registered main hypotheses (see Appendix C). First, whether making the opportunity cost of delayed graduation salient accelerates academic progress, measured by the pre-registered primary outcomes signed-up, attempted, and passed course credits. Second, whether opportunity cost framing is more effective than

[10] Some private institutions charge tuition, but their market share is low (11.6% in 2021). Low direct costs have been shown to contribute to delayed graduation; Bietenbeck et al. (2023) find that moderate tuition fees of €500 per semester accelerated university completion.

[11] See `https://www.destatis.de/DE/Themen/Gesellschaft-Umwelt/Bildung-Forschung-Kultur/Bildungsindikatoren/absolventen-regelstudienzeit-tabelle.html`, accessed on January 28, 2026.

[12] Teaching and examinations were face-to-face again after Covid, and the data show that study behavior does not differ from the following 2022 cohort: the dropout rate in the 2021 control group is 10%, identical to that of the 2022 cohort. First semester credits are also similar (15.7 vs. 16.7).

Figure 1: Timeline of the intervention in the fall semester 2021



salary information alone.

Several recent studies show that students often have misguided expectations about their potential future salaries (Wiswall and Zafar, 2015; Conlon, 2021). Since salary information updates alone could then affect behavior, we need to separate the effect of learning about salaries from the effect of additionally framing these salaries as opportunity costs. Our two-treatment design accomplishes this.

T0: Control. The control letter welcomed students to the university, provided general information about their studies, and described counseling services (Figures B.1 and B.4). The letter also stated that "A survey among your fellow students has shown that many of you would like to receive more information on planning your studies and on career prospects after graduation. To this end, we are currently testing different types of information." This was to ensure the controls received comparable attention from the university as the treatment groups, and to prevent control students from feeling disadvantaged if they learned that other students were receiving different information.[13]

T1: Salary information. Students in T1 received the identical text as T0, but the letter also provided information about the salaries of graduates from the recipient's degree program at this institution. This information is likely novel and relevant to students: "The average gross annual salary (full-time) of similar students during the first year after graduating with a bachelor's degree in [study program] is **€ XX,XXX**" (Figure B.2). The letter was personalized with the student's name and student ID number to discourage sharing.[14]

[13]In Section 4.1, we provide evidence of minimal belief updating in the control group, suggesting that the control letter did not induce substantial information search about salaries.

[14]The salary information was obtained from surveys of recent graduates at this university (2009/10 to 2018/19 cohorts; 1,660 respondents). The average across programs was €48,745, comparable to regional benchmarks (Heming et al., 2020). See Appendix C for calculation details and Table A.1 for program-specific salaries.

T2: Salary and opportunity cost information. Students received the identical information as T0. The salary information from T1 was also provided, and the following text was added to it: "**How does this affect the further planning of your studies?** Each additional semester of studying can lead to the loss of approximately half of that salary" (Figure B.3).[15]

The key difference between T1 and T2 is framing. Both groups receive identical salary information, but only T2 frames salaries as the opportunity cost of delayed graduation. This design allows us to isolate the effect of the opportunity cost frame on information processing and behavior by comparing T2 to T1.

3.3 Data

Our data sources map onto the causal chain we aim to examine, i.e., pre-treatment characteristics and expectations, post-treatment expectations, and academic outcomes.

Pre-treatment: Online self-assessments. During the application and enrollment periods, students could complete program-specific online self-assessments (OSA) that included questions on salary expectations, general opportunity cost consideration, and time preferences (Appendix B.1). About 53% of our sample participated, with no differential participation across treatment groups (Table A.2, Columns 1 and 2).[16] These measures allow us to examine heterogeneity in treatment effects by prior expectations and preferences.

Post-treatment: Online survey. About six weeks after the first letter, we invited students to a follow-up survey. The main purpose was to collect updated salary expectations to assess treatment effects on expectations. We also collected non-cognitive outcomes related to the psychological costs of studying (e.g., satisfaction, stress, and organizational freedom; see Appendix B.2) that are pre-registered as secondary outcomes. Participation was 18%, with no differential participation across groups (Table A.2, Columns 3 and 4).

Outcomes: Administrative data. We obtained administrative data on course credits signed up for, attempted, passed (pre-registered primary academic outcomes), GPA, and dropout (pre-registered as secondary academic outcomes) after semesters one and three. GPA allows us to check whether any credit gains come at the cost of lower grades. Dropout captures two possibilities: (i) students attempting to accelerate may overreach and exit, (ii) for students unlikely to graduate, recognizing opportunity costs may prompt dropout as the actionable behavioral response rather than acceleration.

[15] A footnote told students: "This applies when entering the workforce after earning a bachelor's degree (BA). In the case of a subsequent master's degree (MA), this amount increases by the difference in salary between MA and BA graduates."

[16] When analyzing salary expectations, we use only responses from students who enrolled in the program for which they completed the OSA. For other questions, we use information from all OSAs that a student completed, averaging if multiple.

3.4 Empirical Approach

Balancing properties. Table 1 reports descriptive statistics for the three experimental groups and p-values from F-tests for the joint significance of the treatment dummies. These are based on regressions of the respective covariates on treatment group indicators, controlling for randomization strata fixed effects (FE). The table confirms that randomization produced balanced groups across all covariates.[17] Tables A.3 and A.4 demonstrate that the observable covariates remain well-balanced, even when we later restrict the sample to those who participated in the OSA or the online survey.

Table 1: Descriptive statistics and balancing properties

	T0: control		T1: salary info		T2: salary & OC info		p-value
	Mean	SD	Mean	SD	Mean	SD	F-test
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Covariates used in randomization							
High school GPA	2.538	0.609	2.527	0.603	2.508	0.599	0.219
Procrastination index	0.008	1.014	-0.034	0.998	0.026	0.989	0.098
Women	0.367	0.482	0.362	0.481	0.363	0.481	0.677
Other covariates							
Age	21.683	3.796	21.617	3.399	21.607	3.451	0.918
Time since HS degree	1.805	2.819	1.743	2.670	1.808	2.527	0.873
First university semester	0.732	0.443	0.739	0.439	0.708	0.455	0.337
HS degree "Abitur"	0.521	0.500	0.522	0.500	0.514	0.500	0.916
N	739		740		743		

Notes: Columns (1) to (6) report the means and standard deviations of the covariates, separately for the three experimental groups. The p-values from the F-tests of joint significance reported in Column (7) are based on regressions that control for strata FE and use robust standard errors. In Germany, 1.0 is the best and 4.0 is the worst possible grade in the high school GPA (= grade of the university entrance qualification). The procrastination index is the standardized inverse-covariance-weighted average of the date of application to the study program and the date of enrollment. The latter was first standardized within study programs to account for differences in the enrollment periods. First university semester indicates whether this is the first semester at any university. High school degree "Abitur" refers to the German general track high school degree. It is one of the two main secondary school degrees in the tracked school system in Germany that qualifies students to study at a university of applied sciences; the other being the vocational track degree ("Fachhochschulreife").

Main analysis. We estimate treatment effects using the following specification:

$$Y_i = \alpha_0 + \alpha_1 T1_i + \alpha_2 T2_i + \boldsymbol{x_i}\boldsymbol{\alpha_3} + \boldsymbol{s_i} + \varepsilon_i, \tag{1}$$

where Y_i is the outcome, $T1_i$ and $T2_i$ are treatment indicators, $\boldsymbol{s_i}$ are strata fixed effects to control for the randomized assignment within strata. In another specification, we include the vector $\boldsymbol{x_i}$, which contains all covariates in Table 1. We test $\alpha_1 = 0$, $\alpha_2 = 0$, and $\alpha_2 - \alpha_1 = 0$. The letters carried the university's official seal to ensure that students would open and read them. We do not have information on compliance, but students are likely to open offi-

[17]The low p-values from the F-tests for joint significance of the treatment dummies for the high school GPA ($p = 0.219$) and the procrastination index ($p = 0.098$) are partly due to the fact that these covariates – and gender – were used to construct the randomization strata, resulting in very high R^2 (0.70 and 0.63, respectively) and thus a very high statistical power for the balance tests of these variables.

cial communication from the university. Nonetheless, all effects should be interpreted as intention-to-treat.

Depending on the assumed R^2 (0.00 to 0.40), our study has 80% (60%) power for effect sizes between 0.114 and 0.146 (0.090 and 0.115) standard deviations (SD); see Appendix C for details. Thus, we are well-powered to detect effect sizes previously reported in studies on opportunity cost neglect (Cohen's d in the meta-analysis on opportunity cost neglect by Maguire et al. (2023) is 0.22).

We pre-registered heterogeneity analyses along blocking dimensions (high school GPA, gender, procrastination, study program) and OSA measures (prior salary expectations, opportunity cost consideration, time preferences). We did not perform power analyses for them. Results should be interpreted as exploratory.

When estimating effects on survey outcomes and in the heterogeneity analyses using information from the OSAs, we deviate from the pre-registration and adjust the estimation equation to include study program FE instead of strata FE.[18]

4 Main results

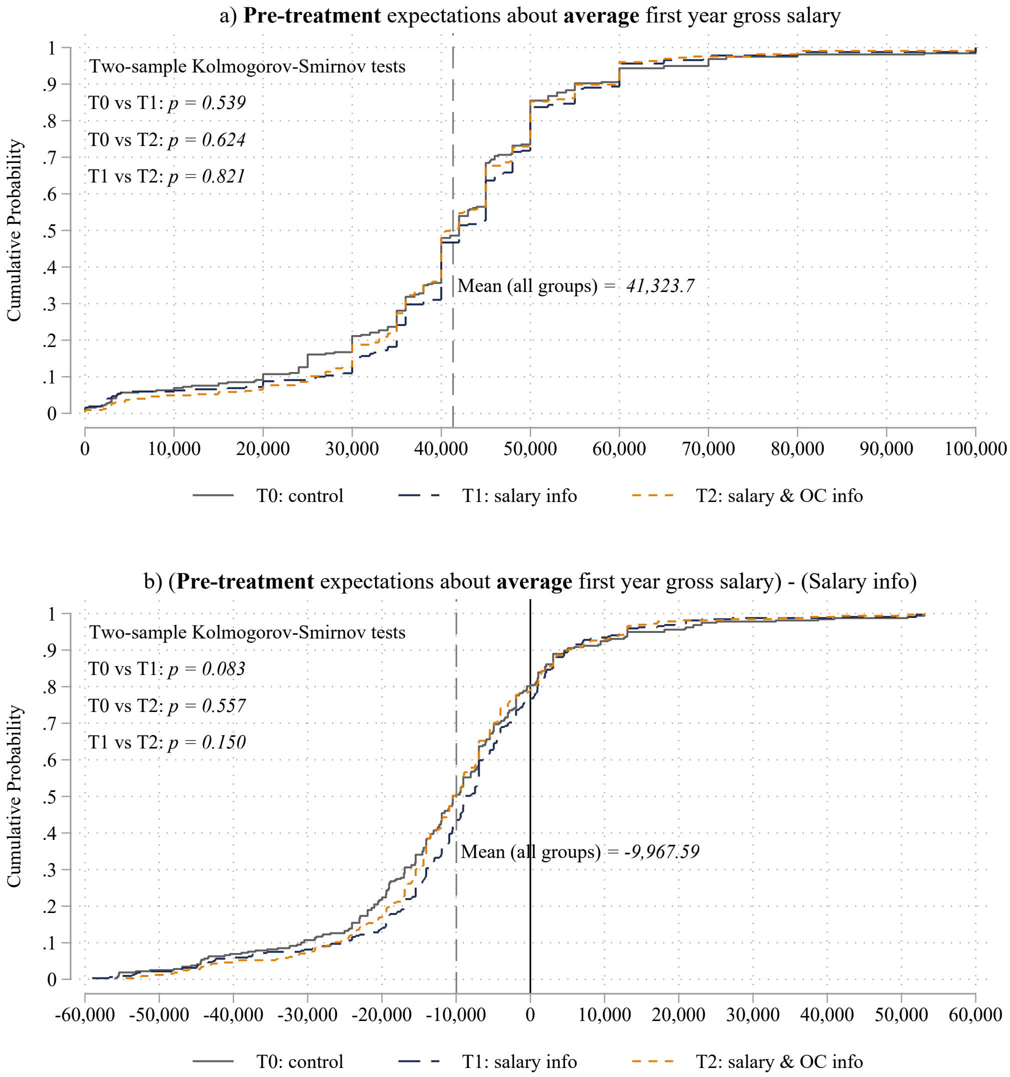
4.1 Information processing and updating of expectations

T1 and T2 provide identical salary information but differ in framing: T1 states salaries directly, while T2 additionally frames them as opportunity costs of delayed graduation. Before examining behavioral outcomes, we assess whether students process and retain the salary information at all. If students forget or dismiss the information, it cannot affect decisions through updated salary expectations. We therefore elicited pre- and post-treatment salary expectations in online surveys, measuring (i) expected average graduate salaries by program, and (ii) own expected salaries.

However, updated expectations are not the only channel through which treatments may operate. The opportunity cost framing in T2 can change how students evaluate decisions by making timing costs salient and connecting abstract future salaries to today's choices. This is true even in the absence of expectations updating. We return to this distinction when interpreting the behavioral effects of the intervention.

[18]We do this in light of the reduced number of observations in the survey and OSA samples. Because of the fine-grained nature of the strata, we would otherwise effectively lose the observations of those strata in which there is not enough remaining variation in treatment assignment.

Figure 2: Pre-treatment expectations about average first-year salary



Notes: Panel a) shows the cumulative distributions of students' expectations about the average first-year salary (winsorized at €100,000) based on the OSA question "What do you believe is the current **average gross annual salary** for full-time employment in the **first year after graduating with a bachelor's degree** in the degree program for which you are answering these OSA questions?" (students could choose to provide "no answer"). Panel b) shows the cumulative distributions of the difference between students' expectations about the average first-year salary minus the salary information provided to students in the treatment groups. $N = 961$.

4.1.1 Baseline expectations and the scope for updating

In the OSAs, we elicited **pre-treatment salary expectations**. Specifically, we asked students to estimate the current average gross annual salary for full-time employment in the first year after graduation from their program. Panel (a) of Figure 2 depicts the cumulative distributions of expectations. On average, students across all groups believe that the first-year salary with a bachelor's degree is €41,324.

Panel (b) plots the difference between expectations and the information provided in the treatments. The figure indicates that our intervention should lead to an upward adjustment of expectations for most students: 80% have expectations about first-year salaries that are lower than the information provided, and overall, students underestimate the average first-year salary by €9,968. Both panels also provide evidence that pre-treatment expectations are well-balanced across the three experimental groups.

4.1.2 Do students process and retain the salary information?

To assess whether students process and retain the salary information from the treatments, we elicited expectations about average graduate salaries again, approximately six weeks after the first treatment letter. Figure 3 reveals striking differences across treatment groups. T2 (salary & OC info) strongly shifts expectations toward the provided information. Panel (b) shows that the post-treatment expectations in T2 are aligned with the treatment information, as the distribution is compressed sharply around zero. Panel (a) confirms that this represents an upward shift in the level of expectations. Students in T2 process and retain the salary information.

T1 (salary info) generates no detectable updating of expectations. Despite receiving explicit information about graduate salaries with identical prominence and formatting as T2, students in T1 show expectations distributions statistically indistinguishable from control. The information is therefore either not processed at all or not retained.

These differential outcomes are remarkable. The information content is identical in T1 and T2, as is the letter format, delivery method, and timing. The only difference is that T2 additionally frames salaries as an opportunity cost.

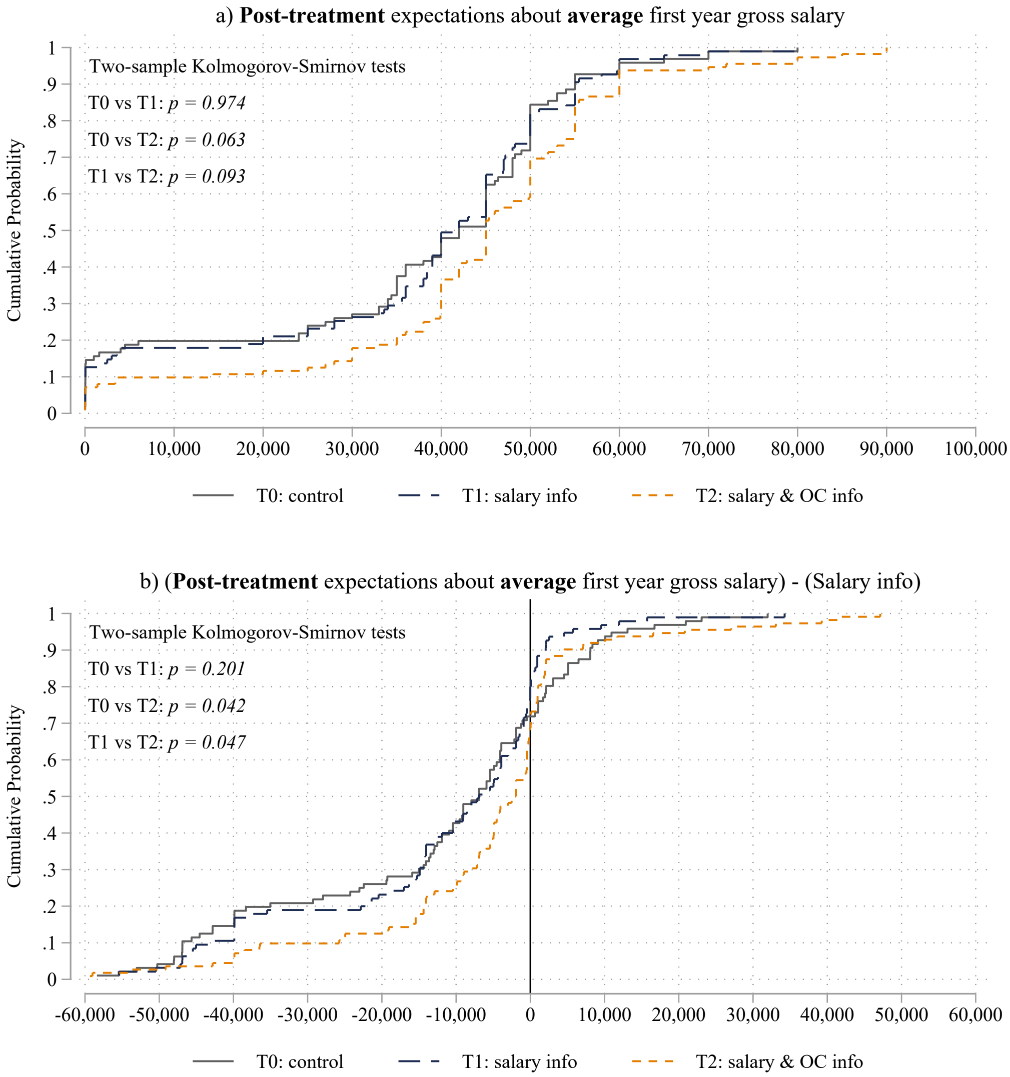
Control group expectations remain largely unchanged: Panel b) shows that approximately 70% of control students continue to underestimate average salaries relative to the information provided in treatment letters. This is evidence against widespread dissemination of the salary information through informal channels. If treatment information had spread to the control group through, e.g., peer networks, we should observe control expectations shifting toward the treatment information.

Table 2 (Columns 1 and 2) provides formal estimates confirming the visual patterns. T2 increases expectations about average first-year salaries by €6,189 to €6,788 ($p = 0.021$ and $p = 0.009$). The effects of T1, by contrast, are never statistically distinguishable from zero. Moreover, the T2 effect is statistically significantly larger than the T1 effect. The difference ranges from €5,646 to €6,297 ($p = 0.026$ and $p = 0.013$).

4.1.3 Do students personalize the information?

Students must ultimately translate population-level information into expectations about their own prospects in order for the information to affect decisions. We therefore also elicited

Figure 3: Post-treatment expectations about average first-year salary



Notes: Panel a) shows the cumulative distributions of students' expectations about the average first-year salary (winsorized at €100,000) based on the survey question "What do you believe is the current **average gross annual salary** for full-time employment in the **first year after graduating with a bachelor's degree** in your current degree program?" (students could choose to provide "no answer"). Panel b) shows the cumulative distributions of the difference between students' expectations about the average first-year salary minus the salary information provided to students in the treatment groups. $N = 303$.

post-treatment expectations about their own expected first-year salaries.

When considering expectations about one's first-year salary, we observe pre-treatment patterns similar to expectations about average salaries (see Figure A.1). However, the evidence on personalized updating of expectations is less definitive than for the processing of average salaries. Columns (3) and (4) of Table 2 show that T2 increases own-salary expectations by €4,339 to €4,742, with p-values of 0.124 and 0.079 (see also Figure A.2). While the point estimates are somewhat smaller than the average salary effect and the precision is limited, the magnitudes are not dramatically different. T1 again shows no effects, with

Table 2: Effects on post-treatment expectations about first-year salary

	Salary expectations				Confidence in expectations			
	Average salary		Own salary		Average salary		Own salary	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
T1: salary info	491.3	542.8	-3444.7	-2664.9	0.840	0.948	-0.693	-0.537
	(2841.5)	(2871.0)	(2870.6)	(2853.4)	(3.317)	(3.406)	(3.497)	(3.541)
T2: salary & OC info	6787.8***	6189.2**	4741.8*	4339.3	6.252**	5.948*	6.116*	5.675*
	(2594.7)	(2659.0)	(2691.4)	(2812.1)	(3.135)	(3.192)	(3.116)	(3.192)
T2-T1	6296.5**	5646.3**	8186.5***	7004.2***	5.413*	4.999	6.809**	6.211*
	(2516.6)	(2526.4)	(2614.6)	(2646.4)	(3.177)	(3.268)	(3.213)	(3.287)
Study program FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes	No	Yes
N	303	303	286	286	299	299	278	278
Control mean	36,215	36,215	39,129	39,129	60.96	60.96	56.40	56.40
(SD)	(20,075)	(20,075)	(18,817)	(18,817)	(20.90)	(20.90)	(21.60)	(21.60)

Notes: Salary expectations are the answers to the survey questions “What do you believe is the current **average gross annual salary** for full-time employment in the **first year after graduating with a bachelor’s degree** in your current degree program?” (Columns 1 and 2) and “Now imagine that **you** received your Bachelor’s degree in the program you are currently studying. What do you believe is the **gross annual salary** that you would earn **during the first [...] year after graduating** if you worked **full time**?” (Columns 3 and 4), winsorized at €100,000 (for both questions, students could choose to provide “no answer”). Confidence in *average salary* and *own salary* (Columns 5 to 8) are the answers to the survey question “How certain are you about this estimate?” that were asked after students reported their estimates of the average and their own future salary in the post-treatment survey (answers from “0% = Not sure at all” to 100% = “Completely sure” and “no answer”). *Controls:* high school GPA, procrastination index, age, time since graduation, and dummies for women, high school degree Abitur, and first semester at any university. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

coefficients significantly smaller than T2 ($p = 0.009$ and $p = 0.002$).

Given statistical power limitations in the survey sample, we interpret this evidence cautiously. The results suggest that, to the extent students process population-level information in T2, they likely translate it into updated expectations about their own prospects, though we cannot pin down the magnitude with precision. What is clear is that T1 fails to shift expectations about either average or own salaries.

4.1.4 Confidence in expectations

Beyond shifting point estimates, T2 also affects confidence in salary expectations. Table 2 shows T2 increases confidence in both average salary estimates (5.9 to 6.3 pp, $p = 0.047$ and 0.063) and own salary estimates (5.7 to 6.1 pp, $p = 0.077$ and 0.051). T1 shows no confidence effects, consistent with the information failing to be processed. The T2-T1 difference in confidence ranges from 5.0 to 6.8 percentage points. Even though population uncertainty remains substantial when measured by the dispersion of reported expectations, these confidence effects indicate that the treatment increases within-individual confidence in expected salaries.

4.1.5 Summary: Framing determines information retention

The information processing evidence reveals three key insights. They are important for interpreting the behavioral effects presented in subsequent sections.

First, framing determines whether identical information is retained. T1 and T2 provide the same salary information in the same format and with the same prominence, yet only T2 generates clear updates of expectations and confidence. Framing future salaries as decision-relevant opportunity costs ties them to present choices and fundamentally affects how information is processed and remembered. Information framed as abstract facts (T1) is not processed and retained.

Second, processing the treatment information is necessary for personalization. T1 fails to shift expectations about either average or own salaries. T2 successfully shifts average salary expectations and shows suggestive evidence of updating own salary expectations as well. However, the smaller, less precisely estimated shifts in own salary expectations suggest that students recognize that population averages may not apply directly to their individual circumstances.

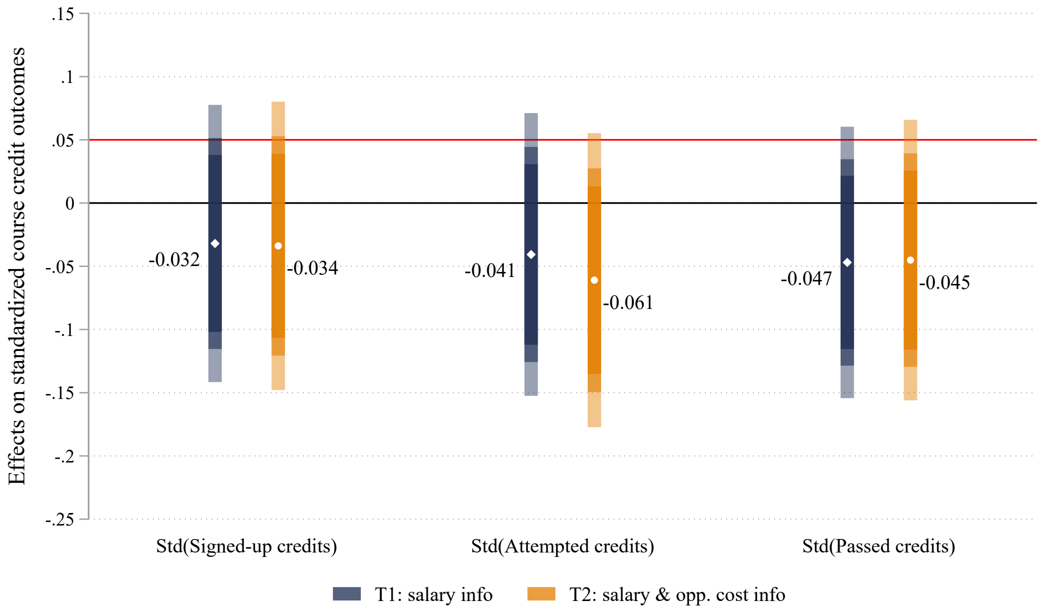
Third, expectation updating is not the only channel through which T2 can operate. In T1, the failure to update expectations rules out pure information provision as a driver of behavior. In T2, the treatment can affect behavior through multiple channels: updated expectations about salary levels, increased psychological costs from framing study progress as a matter of forgone earnings, or generally increased attention to all types of timing costs prompted by the opportunity cost framing. To distinguish these channels, we now turn to examining the behavioral outcomes and additional survey evidence.

4.2 Effects on academic achievement

How do the updated expectations in T2 (salary & OC info) affect behavior? Students can respond by progressing faster toward graduation (intensive margin) or by reconsidering whether to continue at all (extensive margin). We examine both, along with GPA, to check whether pace gains come at the cost of lower grades.

Signed-up, attempted, and passed course credits. Table A.5 shows that the estimated effects on all three primary pre-registered outcomes are negative for both treatments, but none are statistically significant at conventional levels. To assess whether these are merely noisy estimates or whether we can rule out economically meaningful positive effects, Figure 4 reports effects on standardized credit outcomes. The coefficients range from −0.032 to −0.061 standard deviations (SD). Based on the confidence intervals, we can rule out the average effect size of 0.22 reported in a meta-analysis of opportunity cost neglect studies (Maguire et al., 2023). Additionally, we can even confidently rule out positive effects above 0.05 SD, i.e., what Kraft (2020) and Kraft (2023) consider a small effect size in educational contexts. However, we cannot rule out negative effects of about −0.10 SD.

Figure 4: Effects on standardized signed-up, attempted, and passed course credits



Notes: The figure reports treatment effects based on Equation 1 on the standardized number of signed-up, attempted, and passed course credits. Estimates on the non-standardized outcomes are reported in Table A.5. The red line indicates what is typically considered a small effect size in educational contexts (Kraft, 2020, 2023). *Controls:* high school GPA, procrastination index, age, time since graduation, and dummies for women, high school degree Abitur, and first semester at any university. 99%, 95%, and 90% confidence intervals based on robust standard errors are shown.

GPA. We pre-registered GPA to check whether potential credit gains come at the cost of lower grades. We find no effect on either. The effects on standardized GPA follow the same pattern as the primary outcomes. Columns (1) and (2) in Table 3 show negative coefficients ranging from −0.021 to −0.038 SD for both treatments but are never close to being statistically significant. Since GPA is only observed for students who passed at least one graded exam, we assess the robustness of this result in Table A.6. Columns (1) and (2) first report estimated effects of treatment on not observing a GPA. Students in T1 and T2 are 1.7 to 2.3 pp ($p = 0.393$ to 0.247) and 2.9 to 3.1 pp ($p = 0.155$ to 0.112) more likely to have no observable GPA, relative to a control mean of 27%. Importantly, weighing observations by the inverse of the probability that the GPA is observed (Columns 3 and 4) suggests this does not meaningfully bias the estimates of the effects of treatment on GPA.

Dropout. While the treatments did not generate effects on the intensive margin, we do find significant effects on the extensive margin. Columns (3) and (4) in Table 3 show that students in T2 are 2.8 percentage points more likely to drop out by the end of the first semester ($p =$ 0.080 and 0.085). While these effects are only marginally significant, they are economically meaningful: relative to the control group and T1 dropout rate of 10%, providing and

Table 3: Effects on standardized GPA, dropout, and academic achievement index

	Std(GPA)		Dropout		Achiev. index	
	(1)	(2)	(3)	(4)	(5)	(6)
T1: salary info	-0.026	-0.030	-0.002	0.002	-0.001	-0.014
	(0.055)	(0.053)	(0.016)	(0.015)	(0.051)	(0.051)
T2: salary & OC info	-0.021	-0.038	0.028*	0.028*	-0.097*	-0.095*
	(0.055)	(0.053)	(0.016)	(0.016)	(0.053)	(0.052)
T2-T1	0.005	-0.008	0.030*	0.026	-0.096*	-0.082
	(0.056)	(0.054)	(0.016)	(0.016)	(0.053)	(0.053)
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
N	1,599	1,599	2,222	2,222	2,222	2,222
Control mean	0.01	0.01	0.10	0.10	0.00	0.00
(SD)	(1.03)	(1.03)	(0.30)	(0.30)	(1.00)	(1.00)

Notes: Std(GPA) is the standardized and reverse-scaled grade point average at the end of the semester based on passing grades only (1.0 = best, 4.0 = worst on original German scale). *Dropout* indicates whether a student dropped out of their initial study program by the end of the semester. *Achievement index* is the standardized inverse-covariance-weighted average of the number of passed course credits, the GPA, and dropout. *Controls:* high school GPA, procrastination index, age, time since graduation, and dummies for women, high school degree Abitur, and first semester at any university. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

framing the salary information as opportunity costs (T2) causes a 28% increase in dropout. The pure salary information in T1 has no effect on dropout (−0.002 to 0.002 pp; $p = 0.910$ to 0.912), and the difference between the two treatments is itself marginally significant (2.6 to 3.0 pp; $p = 0.109$ to 0.064).

In exploratory analysis, we examine whether the observed behavioral adjustment on the extensive margin dropout is supported by related outcomes. Table A.7 shows that consistent with the dropout result, students in T2 are 1.7 to 2.0 pp more likely to sign up for zero credits ($p = 0.243$ to 0.179), 3.2 to 3.3 pp more likely to attempt zero credits ($p = 0.085$ to 0.087), and 3.1 to 3.3 pp more likely to pass zero credits ($p = 0.119$ to 0.084). All of these outcomes are leading indicators of dropout.

Overall academic achievement. In order to address concerns about multiple hypothesis testing, we go beyond the pre-registration and provide a single summary measure of academic achievement. We construct the standardized inverse-covariance-weighted average of passed credits, GPA, and the dropout indicator; see Anderson (2008) and Schwab et al. (2020). Columns (5) and (6) of Table 3 show that T1 has no effect on overall achievement, while T2 reduces achievement by 0.095 to 0.097 SD ($p = 0.069$ and 0.068). The difference between treatments is 0.082 to 0.096 SD ($p = 0.120$ and 0.072).

5 Mechanisms

The results reveal a striking asymmetry. T2 shifts salary expectations upward and increases confidence in those expectations. Such changes might typically suggest improved motivation and academic outcomes. Instead, we observe increased dropout and no improvement in credit accumulation or GPA. Meanwhile, providing the same salary information without the opportunity cost framing in T1 has no effect on either expectations or behavior.

To organize our interpretation and derive testable predictions, we develop a simple theoretical framework based on limited attention to opportunity costs. The model shows that if framing salary information as opportunity costs also increases attention to the timing costs of dropout, this will raise the continuation threshold and cause some marginal students to drop out despite improved salary expectations (Section 5.1). The null effect on academic pace reflects a different mechanism. Here, binding institutional constraints, ability limits, and convex effort costs prevent acceleration regardless of attention (Section 5.2). The model also clarifies why pure salary information fails to influence behavior.

We first present the model and explain why dropout increases. We use both preregistered and exploratory analyses, clearly labeling each.

5.1 Why does dropout increase?

5.1.1 Setup: the dropout decision

In a two-period (semester) model, at the end of period 1, student i decides whether to continue or drop out to maximize expected utility. Continuing yields graduation with probability π_i or eventual dropout with probability $(1-\pi_i)$, where $\pi_i \in [0,1]$ captures the individual's prospects of graduating with a degree.

Depending on the decision, the student faces the following potential payoffs: i) $w_i^D(1)$, if they drop out in period 1, ii) $w_i^D(2) < w_i^D(1)$, if they continue and then drop out in period 2, and iii) $w_i^G > w_i^D(\cdot)$, if they continue and graduate. $w_i^D(1)$, $w_i^D(2)$, and w_i^G are present values of lifetime earnings after dropout in period 1 or 2, and graduation, respectively.

Continuing also imposes net effort cost $C_i > 0$ (relative to the cost imposed by the alternative career path after dropout).[19] These costs are individual-specific and affected by typically unobservable factors such as motivation, grit, and resilience.

The expected payoff from continuing then is:

$$V_i^C = \pi_i \cdot w_i^G + (1-\pi_i) \cdot w_i^D(2) - C_i,$$

[19] We assume that alternative career paths will be less costly in terms of required effort, especially for those students who already struggle.

and the payoff from dropping out immediately is:

$$V_i^D(1) = w_i^D(1).$$

Student i continues if $V_i^C \geq V_i^D(1)$, which yields the continuation condition:

$$\pi_i \cdot w_i^G + (1-\pi_i)\cdot w_i^D(2) - C_i \geq w_i^D(1).$$

Rearranging:

$$\pi_i[w_i^G - w_i^D(2)] \geq C_i + [w_i^D(1) - w_i^D(2)]. \tag{2}$$

The left side is the expected degree premium. The right side includes the effort cost C_i plus the opportunity cost of delayed dropout: if a student ultimately drops out, delaying dropout to period 2 reduces lifetime earnings due to fewer working years.[20]

Limited attention. Because the effort costs are tangible and because the forgone salaries are comparatively abstract and complex (they require estimating counterfactuals), the opportunity cost term $[w_i^D(1) - w_i^D(2)]$ can have low salience. As a consequence, students may pay limited attention to it and will then not appropriately consider the opportunity cost in their decisions. We model limited attention to the opportunity cost of delayed dropout as a binary indicator $\lambda_i^D \in \{0,1\}$ multiplying the opportunity cost term. This follows models of salience (Bordalo et al., 2012) and shrouded attributes (Gabaix and Laibson, 2006); opportunity costs are thus either in the consideration set or not (for a review of the literature see Gabaix (2019)).[21]

$$\pi_i[w_i^G - w_i^D(2)] \geq C_i + \lambda_i^D[w_i^D(1) - w_i^D(2)] \tag{3}$$

When $\lambda_i^D = 0$, students do not consider the opportunity costs of delayed dropout. We apply λ_i^D only to the opportunity cost term $[w_i^D(1) - w_i^D(2)]$. That is, we assume that the

[20] Salaries $w_i^D(1)$ and $w_i^D(2)$ are present values of lifetime earnings with labor market entry at different times. The difference $w_i^D(1) - w_i^D(2)$ reflects that earlier dropout provides more working years; this already incorporates appropriate discounting of future salary flows.

Delaying dropout by one year shifts the entire subsequent earnings stream back by one year. For example, if a student pursues an alternative degree after dropping out and completes it in four years, the one-year delay means degree completion occurs in year five instead of year four. This creates an immediate substantial loss: roughly one year of graduate salaries forgone in year five (present value: $w_i/(1+r)^5 \approx 0.86 \cdot w_i$ at $r = 3\%$). Additionally, all subsequent earnings are delayed by one year. With salary growth at rate g (e.g., 5% annually), the student earns less than their counterfactual earlier-dropout self in every subsequent year, though these additional losses are more heavily discounted. The dominant component of the opportunity cost $w_i^D(1) - w_i^D(2)$ is the near-to-medium-term loss (years five to ten), not distant-future losses, making standard discounting concerns minimal.

[21] While attention may vary continuously in some situations, the discrete model captures the key insight that opportunity cost considerations are either actively part of the decision calculus or not. The framework naturally extends to the aggregate level where $\Lambda^D \in [0,1]$ represents the fraction of students with $\lambda_D = 1$.

degree premium $[w_i^G - w_i^D(2)]$ is salient to everyone, since mass media, university marketing, parental encouragement, and career advising all emphasize returns to degree completion.

Individual continuation thresholds. The continuation condition defines student-specific thresholds:

$$\bar{\pi}_i = \frac{C_i + \lambda_i^D [w_i^D(1) - w_i^D(2)]}{[w_i^G - w_i^D(2)]} \tag{4}$$

Student i continues if $\pi_i \geq \bar{\pi}_i$ and drops out if $\pi_i < \bar{\pi}_i$. Individual continuation thresholds (and thus dropout probability) depends on the continuation cost C_i, the opportunity cost of delayed dropout $[w_i^D(1) - w_i^D(2)]$, whether students pay attention to the opportunity cost term (λ_i^D), and the degree premium $[w_i^G - w_i^D(2)]$.

5.1.2 Insights on salary expectations, opportunity cost neglect, and continuation costs

The continuation condition provides insights on how the intervention can affect dropout decisions.

Salary expectations. The continuation threshold depends on both the degree premium $[w_i^G - w_i^D(2)]$ and the opportunity cost of delayed dropout $[w_i^D(1) - w_i^D(2)]$. The intervention provided information about graduate salaries w_i^G, which enters only through the degree premium in the denominator. Higher perceived w_i^G should lower the continuation threshold, making students less likely to drop out. Indeed, Section 4.1 showed that T2 (salary & OC info) significantly increased expectations about average and own first-year salaries, as well as the confidence in those expectations. T1 (salary info) had no effect. Yet T2 increases dropout by 2.8 pp. This implies that other mechanisms must dominate the salary expectation channel.

Attention to opportunity costs. The key lies in how T2 frames the information. While both treatments provided salary data, T2 explicitly presents graduate salaries as per-semester opportunity costs. This framing appears to trigger a broader cognitive shift: students recognize that studying has opportunity costs in general. Critically, this includes the cost of delaying dropout. Formally, T2 switches the attention parameter λ_i^D in Equation 4 from 0 to 1, raising the continuation threshold.

This mechanism explains (i) why T2 affects dropout behavior while T1 does not (recall that T1 fails to shift salary expectations, and λ_i^D is 0 for T1), and (ii) why the effect of T2 is negative despite positive salary updates. When students recognize the full cost (C_i plus lost salaries) of persisting in a degree they may not complete, marginal students rationally exit.

Directly testing whether T2 increased the salience of the opportunity costs of dropout is difficult with our data. We argue that for first-semester students who have previously dropped out of another program (27.4% of our sample), the opportunity cost of delaying dropout is already salient, as they have considered the dropout decision when leaving their first program (or in its aftermath). That is, a large fraction of these students should already have $\lambda_i^D = 1$ at baseline. Treatment cannot increase λ_i^D further, so for these students it generates no behavioral effects. For many first-time students, on the other hand, λ_i^D will be responsive, because at baseline their $\lambda_i^D = 0$. As a consequence, we expect larger treatment effects among first-semester students who have no prior experience in higher education. In an exploratory heterogeneity analysis, we therefore interact the treatment indicators with a dummy for whether students have previously dropped out of another program.

Table 4: Treatment effects on dropout and achievement index by having previously dropped out

	Dropout		Achiev. index	
	(1)	(2)	(3)	(4)
T1: salary info	0.004	0.008	-0.017	-0.031
	(0.018)	(0.018)	(0.058)	(0.058)
T2: salary & OC info	0.044**	0.043**	-0.148**	-0.148**
	(0.019)	(0.019)	(0.063)	(0.062)
T1*Previously dropped out	-0.022	-0.024	0.059	0.065
	(0.038)	(0.038)	(0.125)	(0.124)
T2*Previously dropped out	-0.056	-0.055	0.184	0.185
	(0.039)	(0.037)	(0.127)	(0.123)
T1+T1*Previously dropped out	-0.018	-0.016	0.042	0.034
	(0.033)	(0.033)	(0.109)	(0.108)
T2+T2*Previously dropped out	-0.012	-0.012	0.036	0.038
	(0.033)	(0.032)	(0.108)	(0.104)
Strata FE	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes
N	2,222	2,222	2,222	2,222
Not previously dropped out: control mean	0.09	0.09	0.03	0.03
(SD)	(0.29)	(0.29)	(0.96)	(0.96)
Previously dropped out: control mean	0.13	0.13	-0.07	-0.07
(SD)	(0.33)	(0.33)	(1.11)	(1.11)

Notes: The table reports effects on academic achievements by whether students have previously dropped out of studying, i.e., whether this is the first semester at any university or not. *Dropout* indicates whether a student dropped out of their initial study program by the end of the semester. *Achievement index* is the standardized inverse-covariance-weighted average of the number of passed course credits, the GPA, and dropout. *Controls:* high school GPA, procrastination index, age, time since graduation, and dummies for women, and high school degree Abitur. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

We report the results of this analysis in Table 4. We find that first-time students in T2 are 4.3 to 4.4 pp more likely to drop out (p = 0.023 and 0.022), translating into 0.148 SD lower overall academic achievement (p = 0.018 and 0.016). For students who have previously studied, the estimated treatment effect coefficients are close to zero, and they are 5.5 to 5.6 pp (p = 0.139 and 0.148) lower for dropout and 0.184 to 0.185 SD (p = 0.147 and p = 0.132) higher for the achievement index than the effects for first-time students. These results are consistent with the intuition described above, and with λ_i^D being malleable through both experience and interventions.

Continuation costs. Third, the continuation threshold is sensitive to continuation costs C_i, which may be affected by psychological costs of studying; for instance, stress, satisfaction, and freedom of choice. An increase in psychological costs of studying then increases C_i and thus leads to a higher $\bar{\pi}_i$.

Table 5: Effects on psychological cost index

	(1)	(2)
T1: salary info	0.176	0.160
	(0.131)	(0.130)
T2: salary & OC info	0.289**	0.286**
	(0.121)	(0.123)
T2-T1	0.113	0.126
	(0.140)	(0.141)
Study program FE	Yes	Yes
Controls	No	Yes
N	379	379
Control mean	0.00	0.00
(SD)	(1.00)	(1.00)

Notes: The outcome is the standardized inverse-covariance-weighted average of standardized survey answers on students' life satisfaction, performance pressure, study freedom, personal development, study stress, and study satisfaction (see Table A.8 for effects on the individual outcomes and the question texts). Life satisfaction, personal development, study freedom, and study satisfaction enter the index reverse-scaled, such that higher index values indicate higher psychological costs. *Controls:* high school GPA, procrastination index, age, time since graduation, and dummies for women, high school degree Abitur, and first semester at any university. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

We use outcomes collected in the post-treatment online survey to quantify treatment effects on psychological costs. We asked students about their life and study satisfaction, and how much performance pressure, study stress, organizational freedom, and personal development they experience in their studies. Table 5 shows the effects on the standardized inverse-covariance-weighted average of the responses to the questions. Informing students about their potential future salary in T1 has an imprecisely estimated effect on the index of 0.160 to 0.176 SD ($p = 0.221$ to 0.181). Additionally making the opportunity costs of delayed graduation explicit in T2 results in a stronger and statistically significant increase in psychological costs of 0.286 to 0.289 SD ($p = 0.021$ to 0.017). Columns (5) to (8) in Table A.8 show that these effects are driven by a reduction in the organizational freedom and personal development that students experience in their studies. The evidence for psychological costs is somewhat mixed, as the estimated effects of the two treatments are not statistically significantly different from each other, and organizational freedom responds to both treatments.

Taken together, the empirical evidence in the context of the theoretical framework suggests that T2 increases dropout through two channels. First, and most importantly, opportunity cost framing directs attention to the previously neglected cost of delayed dropout.

Evidence from students with prior university experience supports this mechanism: they show no response to treatment, consistent with having already deliberated about opportunity costs. Second, the framing may also increase psychological costs of studying and further raise continuation costs, though this evidence is more tentative. The two effects dominate the impact of higher salary expectations, which would otherwise lower dropout.

5.2 Why are there no effects on graduation speed?

Having established how opportunity cost framing affects the dropout decision, we now turn to a complementary question: why does the same framing not accelerate graduation? After all, the treatment explicitly highlights the opportunity costs of delayed graduation, i.e., the forgone salaries from studying longer than necessary.

Consider the decision that a student who is choosing effort e to affect their time to graduation faces (formalizing this choice would require extending the model to three periods; we sketch the intuition here). Students will expend the effort required to graduate fast e^F, rather than the effort required to graduate slow e^S, if:

$$\lambda_i^F [w_i^F - w_i^S] \geq C_i(e^F) - C_i(e^S), \tag{5}$$

where w_i^F and w_i^S are present values of lifetime earnings after graduating fast and graduating slow, and λ_i^F is an indicator that captures attention to the speed premium of graduating faster, i.e., the opportunity cost of delaying graduation. We assume convex effort cost, so that $C_i(e^F) >> C_i(e^S)$.

In this decision, there are two main channels through which our intervention could accelerate graduation: First, via information about first-year salaries, both treatments could affect the belief about the magnitude of the speed premium $[w_i^F - w_i^S]$. Second, T2 (salary & OC info) is designed to increase attention to the speed premium (λ_i^F).

We observe no effects on signed-up, attempted, and passed course credits for either T1 or T2, which indicates that students do not attempt to graduate earlier. For T1, this is consistent with the lack of change in salary expectations. For T2 the lack of acceleration is surprising, as the upward shift in salary expectations should increase the perceived speed premium $[w_i^F - w_i^S]$, and the opportunity cost framing should increase attention to this premium λ_i^F.

Why are students unresponsive on the speed of progress margin? If students could freely adjust graduation speed, those receiving larger information shocks should have the strongest response and increase their credits per semester. Yet even students whose salary expectations were far below the provided information, and who thus received the largest positive updates, show no differential credit accumulation (Tables A.9 and A.10). The overall null effect holds even in programs with especially high graduate salaries (Table A.11), where incentives to accelerate should be strongest. The complete absence of credit responses across all subgroups and specifications points to binding constraints.[22]

Three types of constraints are likely to play a role: (i) **Ability**: students have finite cognitive capacity and time. Substantially increasing course loads or study intensity may be infeasible regardless of effort or motivation. (ii) **Convexity of cost**: even if speeding up is possible, the marginal cost of effort $C_i'(e)$ may rise sharply. If treatment increases λ_i^F by making the speed premium salient, convexity in the cost of effort could imply that $C_i(e^F) - C_i(e^S)$ exceeds any plausible increase in the perceived benefit. (iii) **Institutional constraints**: course prerequisites, limited course offerings per semester, and program requirements that specify sequencing may make it difficult to compress time to degree. Moreover, speed-up is discrete rather than continuous: only reaching 30 additional credits leads to faster graduation, as this is the equivalent of one semester in the ECTS. Due to uncertainty about future performance and schedules, this could make it less attractive to marginally increase credits in early semesters.

While we cannot definitively identify which constraint binds, two results point toward institutional rather than individual limitations (such as ability and the cost of effort). First, Table A.13 shows that the treatment effects are not heterogeneous with respect to high school GPA, a proxy for cognitive ability that is predictive of the number of passed course credits in the control group. Second, Table A.14 shows that the treatment effects are also not heterogeneous with respect to the index for students' procrastination tendencies, which is highly predictive of credit outcomes in the control group and may be interpreted as a proxy for how costly it is for students to invest additional effort.[23]

5.3 Validating the dropout mechanisms: Effects concentrate among marginal students

Our theoretical mechanism in Section 5.1 makes a sharp prediction: treatment effects on dropout should concentrate among marginal students, i.e., those who are near their continuation threshold ($\pi_i \approx \bar{\pi}_i$). When T2 increases attention to opportunity costs (raising λ_i^D from 0 to 1) and potentially also psychological costs (increasing C_i), it shifts the continuation threshold upward. Students who were previously above their threshold but close to it may now find themselves below the new, higher threshold and decide to drop out. Students far

[22] We also examined pre-registered heterogeneity by baseline opportunity cost awareness, finding no moderation of credit responses (Table A.12).

[23] In our pre-analysis plan (see Appendix C), we specified additional exploratory analyses to assess heterogeneous effects of our interventions. Tables A.15, A.16, A.17, and A.18 show that there are little to no heterogeneous effects with respect to students' gender, whether they are enrolled in a STEM program, and by survey-based measures of time preferences. An additional pre-specified analysis by current financial situation (as elicited in the post-treatment survey) is not reported due to a lack of statistical power.

above their baseline threshold remain unaffected even after the threshold shift induced by T2 (salary & OC info).

We cannot directly observe who is marginal, but students with low graduation probability (high predicted dropout) should, on average, be more likely to be near their thresholds. We therefore expect treatment effects to concentrate among students with high predicted dropout probability. We find exactly this pattern. Among students with high predicted dropout probability, T2 increases dropout. Students with low predicted dropout probability show zero response.

This result comes from the following exploratory analysis in which we construct a proxy for $1-\pi_i$ (i.e., the probability that a student will eventually drop out) by predicting individual ex-ante probabilities of dropping out by the end of the first semester, using data from an adjacent cohort.

Treatment effects by predicted first-semester dropout probability. We use an approach in the spirit of Dynarski et al. (2021): First, in the cohort of students who enrolled one year later, we run a logistic regression of a first-semester dropout indicator on study program FE and the same set of control variables as in Equation 1. Based on these estimates, we predict the first-semester dropout probabilities in our intervention cohort (see Figure A.3 for the distribution of the predicted probabilities). For treated students, this can be thought of as the probability of dropping out in the absence of our intervention. We split our sample at the median of the predicted dropout probabilities and estimate conditional average treatment effects based on the following equation (Table A.19 shows descriptive statistics for the low and high dropout probability groups and provides evidence that the experimental groups are also well-balanced within these groups):

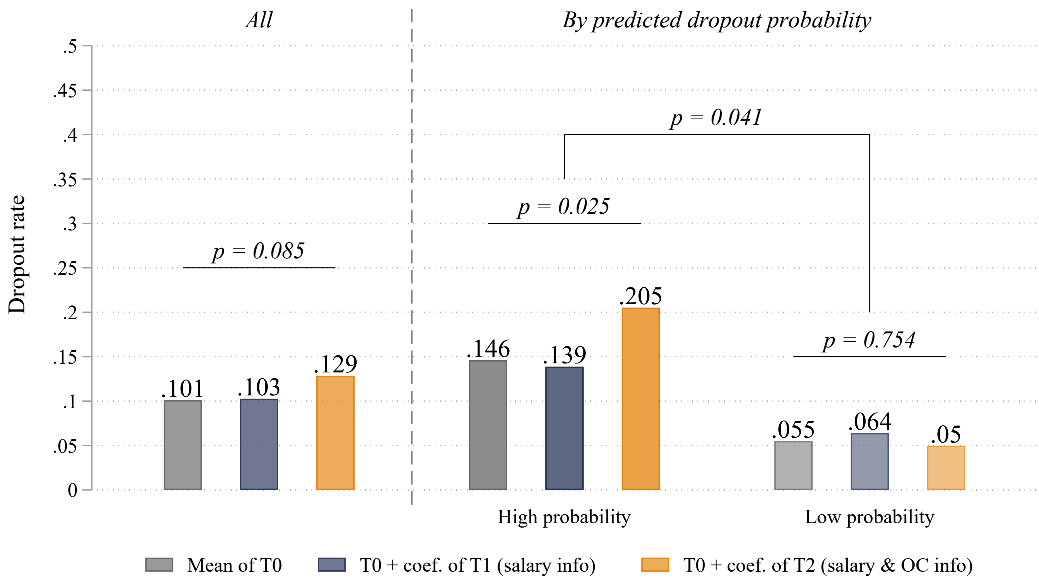
$$\begin{aligned} Y_i^k = \alpha_0 + \alpha_1 \mathit{T1}_i + \alpha_2 \mathit{T2}_i + \alpha_3 \mathit{Low\ Pr(Dropout)}_i + \alpha_{13} \mathit{T1}_i \times \mathit{Low\ Pr(Dropout)}_i \\ + \alpha_{23} \mathit{T2}_i \times \mathit{Low\ Pr(Dropout)}_i + \boldsymbol{x_i \alpha_4} + \boldsymbol{s_i} + \varepsilon_i, \end{aligned} \tag{6}$$

where $\mathit{Low\ Pr(Dropout)}_i$ is a dummy indicating students with a below median estimated dropout probability, and all other variables are defined as before.

The results confirm the prediction (Figure 5; Table A.20, Columns 5 and 6). Among high-dropout-probability students, T2 increases dropout by 5.9 pp, a 40% increase relative to the 14.6% control mean ($p = 0.025$ to 0.029). Low-dropout-probability students show zero response, and the difference between high- and low-dropout-probability groups is significant ($p = 0.041$ to 0.050). T1 has no effect in either group. This pattern extends to overall academic achievements (Columns 7 and 8): T2 reduces the index by 0.20 SD for high-probability students ($p = 0.017$ to 0.023), with a significant difference across groups ($p = 0.032$ to 0.043).[24]

[24] Effects on course credits and GPA are qualitatively similar but not statistically significant.

Figure 5: Effects on dropout by predicted dropout probability



Notes: The left part of the figure plots first semester dropout rates (plus treatment effects) based on Equation 1 and the right part plots first semester dropout rates (plus treatment effects) by students' predicted dropout probability based on Equation 6. *Dropout* indicates whether a student dropped out of their initial study program by the end of the first semester. Regressions include the following *controls:* high school GPA, procrastination index, age, time since graduation, and dummies for women, high school degree Abitur, and first semester at any university.

This heterogeneity pattern is difficult to explain through alternative mechanisms. Effects concentrate exactly where the model predicts, among marginal students near the continuation threshold. Students far from the threshold show no response.

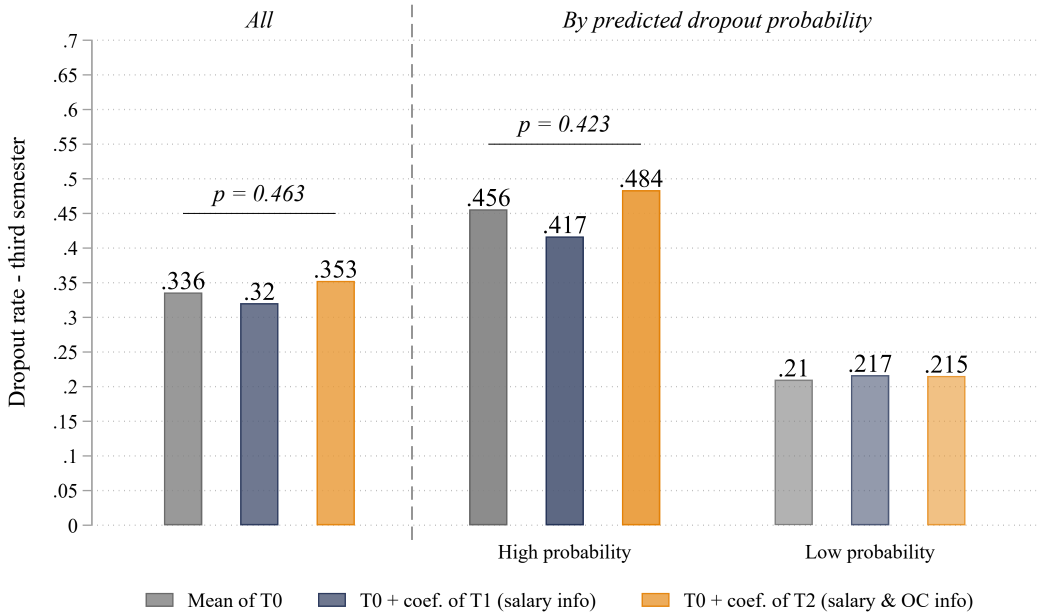
Medium-term effects. We collected additional outcome data at the end of the third semester to address two exploratory questions: (i) does our dropout proxy predict eventual dropout, and (ii) does T2 increase total dropout or accelerate it?

The proxy performs well. By semester three, control-group dropout among high-dropout-probability students has risen to 46%, compared to 21% among low-dropout-probability students (Figure 6; Table A.22).

Treatment effects have largely disappeared by semester three. Average effects are small and insignificant: T2 increases dropout by 1.5 to 1.7 pp relative to a control mean of 34% (Figure 6; Table A.21). Among high-dropout-probability students, T2 now increases dropout by 2.2 to 2.8 pp ($p = 0.423$ to 0.533), compared to 5.9 pp in semester one (Figure 6; Table A.22). T1 also shows no significant effects; results are similar for other outcomes. We cannot reject equality of the semester one and three T2 coefficients, but the point estimates suggest that T2 accelerates dropout rather than increasing it.

A plausible interpretation is that marginal control students who continue past the first semester face subsequent continuation decisions, switching from $\lambda_i^D = 0$ to $\lambda_i^D = 1$ endogenously. T2 forces early deliberation, whereas marginal control students arrive gradually at the same conclusion (similar as in Stinebrickner and Stinebrickner (2014) or Arcidiacono et al. (2025)). In the end, the same students exit in both T2 and control, just at different times. Empirically, cumulative dropout in T2 and control converges by the third semester.

Figure 6: Effects on dropout by predicted dropout probability – third semester



Notes: The left part of the figure plots third semester dropout rates (plus treatment effects) based on Equation 1 and the right part plots third semester dropout rates (plus treatment effects) by students' predicted dropout probability based on Equation 6. *Dropout* indicates whether a student dropped out of their initial study program by the end of the third semester. Regressions include the following *controls:* high school GPA, procrastination index, age, time since graduation, and dummies for women, high school degree Abitur, and first semester at any university.

5.4 Welfare considerations

The convergence finding has a direct implication for the students who respond to treatment. If cumulative dropout rates equalize by semester three, then T2 accelerates exit rather than causing additional attrition. The same students who would eventually drop out do so earlier. For these students, earlier exit is beneficial. They avoid continuation costs and enter the labor market sooner. Formally, the gain in individual welfare W_i from dropping out in period 1 rather than period 2 is:

$$\Delta W_i = [w_i^D(1) - w_i^D(2)] + C_i > 0. \tag{7}$$

The first term captures additional lifetime earnings due to earlier labor market entry following an earlier dropout. The second term reflects avoided effort costs from an additional semester of study. Students with high π_i are unaffected by the treatment and graduate regardless.

This interpretation rests on two assumptions. First, there are substantial sheepskin effects. Under sheepskin effects (Hungerford and Solon, 1987; Jaeger and Page, 1996), partial college has limited value. There is ample evidence that labor market returns in Germany accrue primarily to completed credentials rather than years of study (Hungerford and Solon, 1987; Berlingieri and Bolz, 2025; Stans et al., 2025). As a result, salary rates will be very similar whether dropping out early or late. What changes with later dropout is the number of working years, leading to $w_i^D(1) > w_i^D(2)$.

Second, continuing has limited option value. Convergence shows that control students do not succeed despite staying enrolled longer. They eventually exit anyway. This also addresses whether T2 might cause "inefficient" dropouts: if the same students exit by semester three regardless of treatment, T2 is accelerating eventual dropouts rather than inducing premature ones.

Our analysis abstracts from effects we cannot quantify, including potential costs of earlier exit (signaling effects, forgone human capital from additional coursework), and potential (social) benefits (higher lifetime tax revenue from earlier labor market entry). However, under the assumptions above, making opportunity costs salient helps marginal students avoid investing further in a path unlikely to lead to graduation, while at the same time not affecting students who would succeed regardless.

6 Conclusion

The standard remedy for opportunity cost neglect is straightforward: make the forgone alternative salient. Laboratory experiments confirm this works, as reminding individuals that money spent on one option cannot be spent on another reduces willingness to pay. Our results suggest that in complex, high-stakes environments with multiple margins of adjustment, the effects depend on which margins are actionable.

In a field experiment, we informed university students about graduate salaries and framed this information as the opportunity cost of delayed graduation. The intervention did not accelerate progress toward degree completion. Instead, it increased first-semester dropout by 2.8 percentage points. The effect was driven entirely by students with high predicted dropout probability, whose dropout rate increased by 5.9 percentage points. Students likely to graduate showed no response. By the third semester, dropout rates had converged across treatment and control. The students we designed the intervention for did not react. Those who did react made an improved decision, albeit a different one than we intended.

Three insights emerge from the experiment. First, in complex environments with multiple margins of adjustment, individuals respond where response is feasible. Speeding up graduation requires sustained effort against institutional constraints and ability limits.

Dropping out requires a single decision. For marginal students, exit is the actionable margin.

Second, the welfare implications are not obvious ex ante. The heterogeneity analysis and longer-term data were essential for proper interpretation – without them, we might have concluded the intervention backfired. We find that making opportunity costs salient helped marginal students avoid investing further in a path unlikely to lead to graduation, and it did so without affecting students who would succeed regardless. Convergence suggests acceleration of eventual exits, not additional attrition.

Third, framing determines whether information is processed. A separate treatment arm provided identical future salary information without the opportunity cost frame, but had no effect on expectations or behavior. Students ignored abstract salary facts without context. They responded when the same information was framed as a cost relevant to their present decisions. This suggests that salary information interventions depend not just on content but on whether framing triggers deliberation by connecting abstract payoffs to present choices.

Our setting is a German university with no tuition, which makes opportunity costs the dominant private cost of studying. In the US and other countries where tuition is substantial, students may already attend more to direct costs. However, even with tuition as a salient expense, the opportunity cost of time likely remains neglected, as it requires counterfactual reasoning that direct costs do not. Our core findings should qualitatively generalize. Regardless of context, framing can make opportunity costs salient and trigger dropout among marginal students, while institutional constraints may limit acceleration.

For policy, our results speak to the limits of universal informational nudges. Blanket provision of opportunity cost information is unlikely to accelerate graduation. But targeted interventions could reduce inefficient enrollment: interventions could aim to identify marginal students and provide guidance on alternative pathways. The challenge is that targeting requires ex ante identification of who is marginal, and whether such targeting is feasible and ethical at scale is unclear.

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Appendices (for online publication)

A Additional tables and figures

Table A.1: Characteristics of the study programs

Study program	(1) STEM	(2) Numerus clausus	(3) Salary info	(4) Own OSA	(5) OSA Resp. rate	(6) *N*
Applied Chemistry	yes	no	48,028	yes	71.11%	90
Applied Materials Science	yes	no	49,963	yes	43.14%	51
Applied Mathematics and Physics	yes	no	48,028	yes	73.81%	42
Building Services Engineering	yes	no	51,608	no	7.27%	55
Business Administration	no	yes	46,925	yes	91.25%	377
Civil Engineering	yes	yes	49,061	yes	28.93%	159
Computer Science	yes	yes	54,260	yes	26.04%	96
Computer Science and Media	yes	yes	48,267	no	27.27%	44
Electrical Engineering and Information Technology	yes	no	55,450	yes	83.42%	193
Energy Process Engineering	yes	no	49,192	yes	64.71%	34
Information Systems and Management	yes	yes	52,153	no	35.00%	80
International Business	no	yes	42,852	yes	48.91%	92
International Business and Technology	yes	yes	52,934	no	25.35%	71
Journalism of Technology	no	no	40,526	yes	70.27%	74
Management in Organic and Sustainability Business	no	yes	46,925	no	26.09%	23
Mechanical Engineering	yes	no	59,027	yes	82.32%	198
Mechatronics/Precision Engineering	yes	no	59,541	yes	36.23%	69
Media Engineering	yes	yes	45,742	no	37.70%	61
Medical Engineering	yes	no	59,388	yes	78.65%	89
Process Engineering	yes	no	47,195	yes	51.85%	27
Social Work	no	yes	39,906	no	3.70%	297

Table A.2: Treatment effect on OSA and survey participation

	OSA participation		Survey participation	
	(1)	(2)	(3)	(4)
T1: salary info	-0.023	-0.026	-0.015	-0.017
	(0.020)	(0.020)	(0.019)	(0.019)
T2: salary & OC info	-0.004	-0.002	0.012	0.011
	(0.020)	(0.020)	(0.020)	(0.020)
T2-T1	0.019	0.025	0.027	0.028
	(0.019)	(0.019)	(0.019)	(0.019)
Strata FE	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes
N	2,222	2,222	2,222	2,222
Control mean	0.54	0.54	0.18	0.18
(SD)	(0.50)	(0.50)	(0.38)	(0.38)

Notes: Outcomes indicate whether a student participated in the online self assessments (OSA) and online survey. *Controls:* high school GPA, procrastination index, age, time since graduation, and dummies for women, high school degree Abitur, and first semester at any university. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.3: Descriptive statistics and balancing properties – OSA sample

	T0: control		T1: salary info		T2: salary & OC info		p-value
	Mean	SD	Mean	SD	Mean	SD	F-test
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Covariates used in randomization							
High school GPA	2.639	0.616	2.633	0.589	2.566	0.578	0.302
Procrastination index	0.038	0.894	0.007	0.884	-0.051	0.911	0.393
Women	0.329	0.471	0.333	0.472	0.304	0.461	0.678
Other covariates							
Age	21.245	3.045	21.444	3.043	21.332	3.041	0.588
Time since HS degree	1.373	2.101	1.588	2.478	1.678	2.420	0.138
First university semester	0.766	0.424	0.727	0.446	0.723	0.448	0.238
HS degree “Abitur”	0.464	0.499	0.469	0.500	0.489	0.500	0.819
N	401		384		401		

Notes: Columns (1) to (6) report the means and standard deviations of the covariates, separately for the three experimental groups. The p-values from the F-tests of joint significance reported in Column (7) are based on regressions that control for study program FE and use robust standard errors. In Germany, 1.0 is the best and 4.0 is the worst possible grade in the high school GPA (= grade of the university entrance qualification). The procrastination index is the standardized inverse-covariance-weighted average of the date of application to the study program and the date of enrollment. The latter was first standardized within study programs to account for differences in the enrollment periods. First university semester indicates whether this is the first semester at any university. High school degree “Abitur” refers to the German general track high school degree. It is one of the two main secondary school degrees in the tracked school system in Germany that qualifies students to study at a university of applied sciences; the other being the vocational track degree (“Fachhochschulreife”).

Table A.4: Descriptive statistics and balancing properties – online survey sample

	T0: control		T1: salary info		T2: salary & OC info		p-value
	Mean	SD	Mean	SD	Mean	SD	F-test
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Covariates used in randomization							
High school GPA	2.329	0.587	2.276	0.610	2.284	0.581	0.677
Procrastination index	-0.242	1.015	-0.319	1.037	-0.211	0.954	0.663
Women	0.527	0.501	0.546	0.500	0.432	0.497	0.067
Other covariates							
Age	21.776	5.106	21.415	3.299	21.679	4.104	0.809
Time since HS degree	2.002	3.635	1.597	2.503	1.973	2.894	0.631
First university semester	0.786	0.412	0.807	0.397	0.727	0.447	0.345
HS degree “Abitur”	0.603	0.491	0.529	0.501	0.547	0.500	0.614
N	131		119		139		

Notes: Columns (1) to (6) report the means and standard deviations of the covariates, separately for the three experimental groups. The p-values from the F-tests of joint significance reported in Column (7) are based on regressions that control for study program FE and use robust standard errors. In Germany, 1.0 is the best and 4.0 is the worst possible grade in the high school GPA (= grade of the university entrance qualification). The procrastination index is the standardized inverse-covariance-weighted average of the date of application to the study program and the date of enrollment. The latter was first standardized within study programs to account for differences in the enrollment periods. First university semester indicates whether this is the first semester at any university. High school degree “Abitur” refers to the German general track high school degree. It is one of the two main secondary school degrees in the tracked school system in Germany that qualifies students to study at a university of applied sciences; the other being the vocational track degree (“Fachhochschulreife”).

Table A.5: Effects on number of signed-up, attempted, and passed course credits

	Signed-up credits		Attempted credits		Passed credits	
	(1)	(2)	(3)	(4)	(5)	(6)
T1: salary info	-0.217	-0.363	-0.358	-0.533	-0.446	-0.623
	(0.494)	(0.483)	(0.579)	(0.568)	(0.569)	(0.552)
T2: salary & OC info	-0.440	-0.385	-0.809	-0.799	-0.505	-0.598
	(0.511)	(0.502)	(0.601)	(0.591)	(0.587)	(0.570)
T2-T1	-0.223	-0.022	-0.451	-0.266	-0.059	0.025
	(0.512)	(0.501)	(0.605)	(0.595)	(0.580)	(0.566)
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
N	2,222	2,222	2,222	2,222	2,222	2,222
Control mean	24.24	24.24	19.37	19.37	15.67	15.67
(SD)	(11.13)	(11.13)	(12.89)	(12.89)	(13.27)	(13.27)

Notes: Controls: high school GPA, procrastination index, age, time since graduation, and dummies for women, high school degree Abitur, and first semester at any university. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.6: Effects on standardized GPA – robustness

	GPA N/A		Std(GPA) IPW	
	(1)	(2)	(3)	(4)
T1: salary info	0.017	0.023	-0.001	-0.025
	(0.020)	(0.019)	(0.061)	(0.057)
T2: salary & OC info	0.029	0.031	-0.008	-0.035
	(0.020)	(0.020)	(0.059)	(0.056)
T2-T1	0.012	0.009	-0.007	-0.011
	(0.020)	(0.020)	(0.059)	(0.058)
Strata FE	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes
N	2,222	2,222	1,599	1,599
Control mean	0.27	0.27	-0.11	-0.11
(SD)	(0.44)	(0.44)	(1.04)	(1.04)

Notes: The estimates, control mean, and control SD in Columns (3) and (4) are based on reweighting observations by the treatment group specific inverse of the probability of observing a GPA. The probabilities are estimated based on a probit regression including study program FE and all other controls. *GPA N/A* indicates whether the GPA is observed for a student. *Std(GPA)* is the standardized and reverse-scaled grade point average at the end of the semester based on passing grades only (1.0 = best, 4.0 = worst on original German scale). *Controls:* high school GPA, procrastination index, age, time since graduation, and dummies for women, high school degree Abitur, and first semester at any university. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.7: Effects on signing-up, attempting, and passing zero credits

	Zero signed-up credits		Zero attempted credits		Zero passed credits	
	(1)	(2)	(3)	(4)	(5)	(6)
T1: salary info	0.001	0.006	0.008	0.014	0.019	0.024
	(0.014)	(0.014)	(0.019)	(0.019)	(0.019)	(0.019)
T2: salary & OC info	0.020	0.017	0.033*	0.032*	0.031	0.033*
	(0.015)	(0.014)	(0.019)	(0.019)	(0.020)	(0.019)
T2-T1	0.019	0.011	0.025	0.019	0.012	0.009
	(0.015)	(0.014)	(0.020)	(0.019)	(0.020)	(0.020)
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
N	2,222	2,222	2,222	2,222	2,222	2,222
Control mean	0.09	0.09	0.19	0.19	0.25	0.25
(SD)	(0.29)	(0.29)	(0.39)	(0.39)	(0.43)	(0.43)

Notes: The outcome variables indicate whether a student signed-up, attempted, or passed zero course credits, respectively. *Controls:* High school GPA, procrastination index, age, time since graduation, and dummies for women, high school degree Abitur, and first semester at any university. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.8: Effects on psychological costs

	Std(Life sat.)		Std(Perf. pres.)		Std(Study freedom)		Std(Personal dev.)		Std(Study stress)		Std(Study sat.)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
T1: salary info	-0.058	-0.053	0.027	0.007	-0.288**	-0.280**	-0.112	-0.116	0.202	0.163	-0.284**	-0.272*
	(0.130)	(0.128)	(0.133)	(0.132)	(0.129)	(0.131)	(0.132)	(0.133)	(0.132)	(0.129)	(0.137)	(0.138)
T2: salary & OC info	-0.090	-0.109	0.081	0.098	-0.378***	-0.350***	-0.267**	-0.234*	0.067	0.096	-0.204	-0.177
	(0.121)	(0.125)	(0.126)	(0.128)	(0.124)	(0.125)	(0.126)	(0.125)	(0.125)	(0.122)	(0.127)	(0.128)
T2-T1	-0.032	-0.057	0.053	0.091	-0.090	-0.070	-0.155	-0.118	-0.135	-0.067	0.080	0.095
	(0.137)	(0.135)	(0.136)	(0.136)	(0.134)	(0.135)	(0.141)	(0.147)	(0.130)	(0.128)	(0.148)	(0.151)
Study program FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
N	370	370	361	361	357	357	359	359	362	362	349	349
Control mean	0.04	0.04	-0.02	-0.02	0.21	0.21	0.11	0.11	-0.05	-0.05	0.16	0.16
(SD)	(0.98)	(0.98)	(0.98)	(0.98)	(0.96)	(0.96)	(0.91)	(0.91)	(1.00)	(1.00)	(0.86)	(0.86)

Notes: Std(life satisfaction) are the standardized answers to the question "How satisfied are you with your life, all things considered?" (answers from "0 = Completely unsatisfied" to 10 = "Completely satisfied" and "no answer"). *Std(performance pressure)* are the standardized answers to the statement "With my studies I associate performance pressure". *Std(freedom)* are the standardized answers to the statement "With my studies I associate freedom to organize studying according to my plans". *Std(personal development)* are the standardized answers to the statement "With my studies I associate personal development". *Std(stress)* are the standardized answers to the statement "With my studies I associate stress" (answers for the last four statements from "1 = Completely disagree" to 7 = "Completely agree" and "no answer"). *Std(study satisfaction)* are the standardized answers to the question "How satisfied are you with your studies, all things considered?" (answers from "0 = Completely unsatisfied" to 10 = "Completely satisfied" and "no answer"). *Controls:* high school GPA, procrastination index, age, time since graduation, and dummies for women, high school degree Abitur, and first semester at any university. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.9: Effects on number of signed-up, attempted, and passed course credits, by pre-treatment expectations about average salary

	Signed-up credits		Attempted credits		Passed credits	
	(1)	(2)	(3)	(4)	(5)	(6)
T1: salary info	1.374	1.119	1.239	0.915	1.290	0.707
	(0.972)	(0.940)	(1.196)	(1.144)	(1.213)	(1.095)
T2: salary & OC info	0.000	-0.281	0.754	0.018	1.741	0.518
	(1.049)	(1.063)	(1.188)	(1.162)	(1.212)	(1.140)
E(Avg. salary) dev. from info (in 10,000)	0.086	-0.029	0.306	0.138	0.233	-0.014
	(0.297)	(0.297)	(0.331)	(0.326)	(0.315)	(0.297)
T1*E(Avg. salary) deviation	0.607	0.555	0.550	0.613	0.675	0.703
	(0.523)	(0.492)	(0.646)	(0.603)	(0.594)	(0.528)
T2*E(Avg. salary) deviation	0.093	-0.093	0.599	0.298	0.842	0.436
	(0.516)	(0.506)	(0.584)	(0.562)	(0.557)	(0.521)
Study program FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
N	961	961	961	961	961	961

Notes: Deviation E(Avg. salary) is the difference between students' pre-treatment expectations about the average first-year gross salary and the information provided by our treatments (in €10,000). *Controls:* high school GPA, procrastination index, age, time since graduation, and dummies for women, high school degree Abitur, and first semester at any university. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.10: Effects on number of signed-up, attempted, and passed course credits, by pre-treatment expectations about own salary

	Signed-up credits		Attempted credits		Passed credits	
	(1)	(2)	(3)	(4)	(5)	(6)
T1: salary info	0.901	0.765	1.111	0.880	1.342	0.882
	(1.163)	(1.125)	(1.353)	(1.292)	(1.374)	(1.249)
T2: salary & OC info	-0.644	-0.820	0.244	-0.322	1.200	0.322
	(1.128)	(1.150)	(1.277)	(1.251)	(1.303)	(1.234)
E(Own salary) dev. from info (in 10,000)	0.290	0.129	0.519	0.301	0.430	0.122
	(0.299)	(0.295)	(0.350)	(0.348)	(0.339)	(0.326)
T1*E(Own salary) deviation	0.228	0.318	0.281	0.497	0.562	0.760
	(0.653)	(0.613)	(0.752)	(0.712)	(0.716)	(0.645)
T2*E(Own salary) deviation	-0.392	-0.439	0.304	0.163	0.542	0.359
	(0.571)	(0.562)	(0.656)	(0.631)	(0.638)	(0.596)
Study program FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
N	940	940	940	940	940	940

Notes: Deviation E(Own salary) is the difference between students' pre-treatment expectations about their own first-year gross salary and the information provided by our treatments (in €10,000). *Controls:* high school GPA, procrastination index, age, time since graduation, and dummies for women, high school degree Abitur, and first semester at any university. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.11: Effects on number of signed-up, attempted, and passed course credits, by high salary study programs

	Signed-up credits		Attempted credits		Passed credits	
	(1)	(2)	(3)	(4)	(5)	(6)
T1: salary info	-0.299	-0.372	-0.435	-0.534	-0.642	-0.736
	(0.798)	(0.777)	(0.909)	(0.889)	(0.893)	(0.864)
T2: salary & OC info	-0.132	-0.065	-1.028	-1.072	-0.677	-0.811
	(0.830)	(0.811)	(0.938)	(0.924)	(0.926)	(0.901)
T1*High salary prog.	0.167	0.018	0.157	0.001	0.400	0.229
	(0.983)	(0.957)	(1.155)	(1.130)	(1.134)	(1.099)
T2*High salary prog.	-0.623	-0.646	0.444	0.552	0.351	0.432
	(1.018)	(0.999)	(1.198)	(1.178)	(1.170)	(1.139)
T1+T1*High salary prog.	-0.132	-0.353	-0.278	-0.532	-0.242	-0.506
	(0.573)	(0.561)	(0.712)	(0.699)	(0.699)	(0.679)
T2+T2*High salary prog.	-0.756	-0.712	-0.584	-0.520	-0.327	-0.379
	(0.589)	(0.584)	(0.745)	(0.731)	(0.716)	(0.696)
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
N	2,222	2,222	2,222	2,222	2,222	2,222
Low salary prog.: control mean	26.22	26.22	21.78	21.78	18.75	18.75
(SD)	(12.57)	(12.57)	(13.55)	(13.55)	(13.95)	(13.95)
High salary prog.: control mean	22.20	22.20	16.88	16.88	12.49	12.49
(SD)	(8.97)	(8.97)	(11.69)	(11.69)	(11.73)	(11.73)

Notes: High salary indicates programs for which our treatment informs students about an average annual gross starting salary above €49,000 (see Table A.1). *Controls:* high school GPA, procrastination index, age, time since graduation, and dummies for women, high school degree Abitur, and first semester at any university. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.12: Effects on number of signed-up, attempted, and passed course credits, by general opportunity cost consideration

	Signed-up credits		Attempted credits		Passed credits	
	(1)	(2)	(3)	(4)	(5)	(6)
T1: salary info	-0.375	-0.454	-0.183	-0.380	-0.041	-0.395
	(0.712)	(0.695)	(0.859)	(0.817)	(0.859)	(0.770)
T2: salary & OC info	-0.654	-0.949	-0.363	-0.945	0.588	-0.341
	(0.760)	(0.754)	(0.900)	(0.865)	(0.907)	(0.837)
Std(Opp. cost consideration)	0.123	0.052	-0.120	-0.231	0.170	-0.047
	(0.500)	(0.487)	(0.650)	(0.608)	(0.618)	(0.543)
T1*Std(OC consideration)	-1.061	-1.075	-0.530	-0.481	-0.728	-0.596
	(0.838)	(0.808)	(1.039)	(0.976)	(1.033)	(0.911)
T2*Std(OC consideration)	0.066	0.053	-0.402	-0.255	-1.231	-0.882
	(0.713)	(0.691)	(0.914)	(0.841)	(0.870)	(0.763)
Study program FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
N	1,135	1,135	1,135	1,135	1,135	1,135

Notes: Std(OC consideration) is the standardized first principal component of the answers to the following four statements: "When I'm faced with an opportunity to make a purchase, I try to imagine things in other categories I might spend that money on.", "Before spending time on a particular activity, I consider other specific activities that I would not be able to spend time on.", "Before I make a particular purchase, I consider other specific items that I would not be able to buy.", and "When I'm faced with the decision to spend time on a particular activity, I try to imagine other activities I might spend my time on" (answers from "0 = Does not describe me at all" to 10 = "Describes me perfectly" and "no answer"); prior to the principal component analysis, missing values for observations for which at least one other response was not missing were imputed with the sample mean. *Controls:* high school GPA, procrastination index, age, time since graduation, and dummies for women, high school degree Abitur, and first semester at any university. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.13: Effects on number of signed-up, attempted, and passed course credits, by high school GPA

	Signed-up credits		Attempted credits		Passed credits	
	(1)	(2)	(3)	(4)	(5)	(6)
T1: salary info	-0.214	-0.358	-0.382	-0.525	-0.505	-0.623
	(0.493)	(0.482)	(0.577)	(0.567)	(0.562)	(0.553)
T2: salary & OC info	-0.453	-0.389	-0.892	-0.800	-0.658	-0.597
	(0.511)	(0.502)	(0.600)	(0.591)	(0.581)	(0.570)
Std(HS GPA)	-0.141	-0.566	1.280**	0.555	3.040***	2.425***
	(0.474)	(0.493)	(0.558)	(0.574)	(0.532)	(0.548)
T1*Std(HS GPA)	0.139	0.155	0.219	0.333	-0.086	0.028
	(0.480)	(0.466)	(0.555)	(0.544)	(0.527)	(0.518)
T2*Std(HS GPA)	0.708	0.692	0.624	0.670	-0.104	-0.089
	(0.512)	(0.500)	(0.592)	(0.581)	(0.555)	(0.544)
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
N	2,222	2,222	2,222	2,222	2,222	2,222

Notes: Controls: procrastination index, age, time since graduation, and dummies for women, high school degree Abitur, and first semester at any university. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.14: Effects on number of signed-up, attempted, and passed course credits, by procrastination index

	Signed-up credits		Attempted credits		Passed credits	
	(1)	(2)	(3)	(4)	(5)	(6)
T1: salary info	-0.346	-0.359	-0.514	-0.535	-0.592	-0.621
	(0.487)	(0.484)	(0.571)	(0.569)	(0.561)	(0.552)
T2: salary & OC info	-0.379	-0.376	-0.750	-0.801	-0.443	-0.594
	(0.501)	(0.500)	(0.591)	(0.590)	(0.578)	(0.570)
Procras. index	-2.526***	-2.581***	-3.209***	-3.098***	-2.974***	-2.631***
	(0.458)	(0.467)	(0.513)	(0.524)	(0.469)	(0.482)
T1*Procras. index	-0.206	-0.170	-0.060	-0.050	-0.114	-0.108
	(0.528)	(0.531)	(0.553)	(0.559)	(0.496)	(0.496)
T2*Procras. index	-0.484	-0.500	0.089	0.103	-0.197	-0.246
	(0.498)	(0.498)	(0.560)	(0.562)	(0.520)	(0.520)
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
N	2,222	2,222	2,222	2,222	2,222	2,222

Notes: Controls: high school GPA, age, time since graduation, and dummies for women, high school degree Abitur, and first semester at any university. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.15: Effects on number of signed-up, attempted, and passed course credits, by gender

	Signed-up credits		Attempted credits		Passed credits	
	(1)	(2)	(3)	(4)	(5)	(6)
T1: salary info	-0.428	-0.477	-0.376	-0.538	-0.090	-0.318
	(0.620)	(0.614)	(0.734)	(0.729)	(0.716)	(0.705)
T2: salary & OC info	-0.795	-0.621	-1.357*	-1.241	-0.808	-0.767
	(0.639)	(0.634)	(0.774)	(0.765)	(0.749)	(0.733)
T1*Women	0.535	0.311	0.041	0.011	-0.979	-0.839
	(1.030)	(1.015)	(1.205)	(1.188)	(1.188)	(1.161)
T2*Women	0.947	0.648	1.500	1.216	0.837	0.471
	(1.066)	(1.037)	(1.228)	(1.194)	(1.208)	(1.162)
T1+T1*Women	0.107	-0.166	-0.335	-0.527	-1.069	-1.157
	(0.820)	(0.798)	(0.951)	(0.926)	(0.944)	(0.908)
T2+T2*Women	0.152	0.027	0.143	-0.025	0.029	-0.296
	(0.853)	(0.822)	(0.951)	(0.919)	(0.946)	(0.903)
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
N	2,222	2,222	2,222	2,222	2,222	2,222
Men: control mean	24.08	24.08	18.59	18.59	14.38	14.38
(SD)	(10.92)	(10.92)	(13.00)	(13.00)	(13.22)	(13.22)
Women: control mean	24.54	24.54	20.72	20.72	17.92	17.92
(SD)	(11.50)	(11.50)	(12.62)	(12.62)	(13.09)	(13.09)

Notes: Controls: high school GPA, procrastination index, age, time since graduation, and dummies for high school degree Abitur, and first semester at any university. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.16: Effects on number of signed-up, attempted, and passed course credits, by STEM study programs

	Signed-up credits		Attempted credits		Passed credits	
	(1)	(2)	(3)	(4)	(5)	(6)
T1: salary info	0.343	0.292	-0.253	-0.359	-0.395	-0.467
	(0.941)	(0.920)	(1.076)	(1.061)	(1.064)	(1.042)
T2: salary & OC info	0.106	0.102	-1.018	-1.124	-0.674	-0.778
	(1.000)	(0.988)	(1.120)	(1.112)	(1.116)	(1.097)
T1*STEM prog.	-0.916	-1.072	-0.173	-0.286	-0.084	-0.257
	(1.087)	(1.060)	(1.260)	(1.239)	(1.241)	(1.210)
T2*STEM prog.	-0.892	-0.795	0.341	0.531	0.276	0.294
	(1.138)	(1.124)	(1.310)	(1.298)	(1.291)	(1.268)
T1+T1*STEM prog.	-0.573	-0.780	-0.426	-0.645	-0.479	-0.723
	(0.543)	(0.528)	(0.656)	(0.639)	(0.640)	(0.614)
T2+T2*STEM prog.	-0.786	-0.693	-0.677	-0.592	-0.398	-0.484
	(0.544)	(0.533)	(0.678)	(0.664)	(0.649)	(0.626)
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
N	2,222	2,222	2,222	2,222	2,222	2,222
Non-STEM prog.: control mean	27.35	27.35	23.12	23.12	20.36	20.36
(SD)	(12.74)	(12.74)	(13.74)	(13.74)	(13.87)	(13.87)
STEM prog.: control mean	22.28	22.28	17.01	17.01	12.72	12.72
(SD)	(9.48)	(9.48)	(11.75)	(11.75)	(11.98)	(11.98)

Notes: Controls: high school GPA, procrastination index, age, time since graduation, and dummies for women, high school degree Abitur, and first semester at any university. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.17: Effects on number of signed-up, attempted, and passed course credits, by patience

	Signed-up credits		Attempted credits		Passed credits	
	(1)	(2)	(3)	(4)	(5)	(6)
T1: salary info	-0.391	-0.484	-0.160	-0.383	0.041	-0.335
	(0.707)	(0.688)	(0.854)	(0.814)	(0.855)	(0.768)
T2: salary & OC info	-0.726	-1.022	-0.423	-0.969	0.468	-0.419
	(0.752)	(0.745)	(0.895)	(0.863)	(0.904)	(0.837)
Std(GPS: patience)	0.185	-0.075	0.631	0.222	0.809	0.315
	(0.469)	(0.475)	(0.576)	(0.538)	(0.541)	(0.469)
T1*Std(GPS: patience)	-0.229	0.038	-1.322	-0.743	-1.831**	-1.090
	(0.650)	(0.643)	(0.831)	(0.786)	(0.806)	(0.710)
T2*Std(GPS: patience)	-0.252	0.027	-0.846	-0.226	-0.630	0.240
	(0.702)	(0.692)	(0.838)	(0.795)	(0.806)	(0.729)
Study program FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
N	1,143	1,143	1,143	1,143	1,143	1,143

Notes: Std(GPS: patience are the standardized answers to the question “How willing are you to give up something that is beneficial for you today in order to benefit more from that in the future?” (answers from “0 = completely unwilling to do so” to “10 = very willing to do so” and “no answer”). *Controls:* high school GPA, procrastination index, age, time since graduation, and dummies for women, high school degree Abitur, and first semester at any university. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.18: Effects on number of signed-up, attempted, and passed course credits, by procrastination

	Signed-up credits		Attempted credits		Passed credits	
	(1)	(2)	(3)	(4)	(5)	(6)
T1: salary info	-0.260	-0.324	-0.069	-0.226	0.084	-0.227
	(0.693)	(0.676)	(0.848)	(0.811)	(0.852)	(0.766)
T2: salary & OC info	-0.981	-1.280*	-0.409	-0.980	0.482	-0.428
	(0.761)	(0.754)	(0.904)	(0.875)	(0.915)	(0.848)
Std(GPS: procrastination)	-0.597	-0.190	-1.631**	-0.830	-1.595**	-0.613
	(0.562)	(0.561)	(0.657)	(0.630)	(0.676)	(0.616)
T1*Std(GPS: procrastination)	0.297	0.143	0.989	0.465	1.271	0.578
	(0.790)	(0.767)	(0.918)	(0.873)	(0.931)	(0.834)
T2*Std(GPS: procrastination)	0.839	0.517	1.593*	0.947	1.484	0.459
	(0.823)	(0.822)	(0.941)	(0.911)	(0.943)	(0.872)
Study program FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes
N	1,123	1,123	1,123	1,123	1,123	1,123

Notes: Std(GPS: procrastination are the standardized answers to the statement "I tend to postpone tasks even if I know it would be better to do them right away." (answers from "0 = does not describe me at all" to "10 = describes me perfectly" and "no answer"). *Controls:* high school GPA, procrastination index, age, time since graduation, and dummies for women, high school degree Abitur, and first semester at any university. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.19: Descriptive statistics and balancing properties by predicted dropout probability

	T0: control		T1: salary info		T2: salary & OC info		p-value
	Mean	SD	Mean	SD	Mean	SD	F-test
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
a) High predicted dropout probability							
Predicted Pr(Dropout)	0.158	0.088	0.158	0.089	0.156	0.094	0.814
High school GPA	2.750	0.588	2.741	0.580	2.700	0.578	0.340
Procrastination index	0.604	0.797	0.573	0.833	0.607	0.816	0.315
Women	0.358	0.480	0.370	0.484	0.355	0.479	0.649
Age	22.154	4.376	22.166	3.734	22.180	3.919	0.898
Time since HS degree	1.790	3.287	1.805	2.998	1.909	2.953	0.895
First university semester	0.759	0.428	0.782	0.413	0.737	0.441	0.200
HS degree “Abitur”	0.427	0.495	0.429	0.496	0.413	0.493	0.841
N	377		354		380		
b) Low predicted dropout probability							
Predicted Pr(Dropout)	0.048	0.016	0.049	0.017	0.048	0.016	0.689
High school GPA	2.318	0.551	2.331	0.556	2.307	0.553	0.402
Procrastination index	-0.612	0.826	-0.591	0.790	-0.582	0.764	0.396
Women	0.376	0.485	0.355	0.479	0.372	0.484	0.696
Age	21.192	3.005	21.114	2.977	21.006	2.762	0.637
Time since HS degree	1.821	2.235	1.687	2.331	1.703	1.984	0.554
First university semester	0.704	0.457	0.699	0.459	0.678	0.468	0.570
HS degree “Abitur”	0.619	0.486	0.606	0.489	0.620	0.486	0.974
N	362		386		363		

Notes: Columns (1) to (6) report the means and standard deviations of the covariates, separately for the three experimental groups. The p-values from the F-tests of joint significance reported in Column (7) are based on regressions that control for strata FE and use robust standard errors. In Germany, 1.0 is the best and 4.0 is the worst possible grade in the high school GPA (= grade of the university entrance qualification). The procrastination index is the standardized inverse-covariance-weighted average of the date of application to the study program and the date of enrollment. The latter was first standardized within study programs to account for differences in the enrollment periods. First university semester indicates whether this is the first semester at any university. High school degree “Abitur” refers to the German general track high school degree. It is one of the two main secondary school degrees in the tracked school system in Germany that qualifies students to study at a university of applied sciences; the other being the vocational track degree (Fachhochschulreife).

Table A.20: Treatment effects on academic achievements by predicted dropout probability

	Passed credits		Std(GPA)		Dropout		Achiev. index	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
T1: salary info	-0.548	-0.713	0.072	0.072	-0.010	-0.007	0.030	0.020
	(0.833)	(0.804)	(0.089)	(0.088)	(0.026)	(0.025)	(0.084)	(0.082)
T2: salary & OC info	-1.056	-1.294	-0.071	-0.085	0.059**	0.059**	-0.200**	-0.204**
	(0.848)	(0.823)	(0.086)	(0.086)	(0.027)	(0.026)	(0.088)	(0.086)
T1*Low Pr(Dropout)	0.033	0.241	-0.167	-0.158	0.020	0.016	-0.073	-0.060
	(1.164)	(1.137)	(0.114)	(0.111)	(0.031)	(0.031)	(0.103)	(0.102)
T2*Low Pr(Dropout)	1.140	1.411	0.079	0.082	-0.063*	-0.065**	0.213**	0.222**
	(1.173)	(1.143)	(0.114)	(0.111)	(0.032)	(0.032)	(0.105)	(0.103)
T1+T1*Low Pr(Dropout)	-0.516	-0.472	-0.095	-0.086	0.010	0.009	-0.043	-0.040
	(0.796)	(0.782)	(0.071)	(0.067)	(0.018)	(0.018)	(0.059)	(0.060)
T2+T2*Low Pr(Dropout)	0.084	0.116	0.009	-0.003	-0.004	-0.005	0.012	0.018
	(0.806)	(0.789)	(0.072)	(0.069)	(0.017)	(0.017)	(0.057)	(0.058)
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes	No	Yes
N	2,222	2,222	1,599	1,599	2,222	2,222	2,222	2,222
High Pr(Dropout): control mean	11.85	11.85	-0.18	-0.18	0.15	0.15	-0.18	-0.18
(SD)	(12.89)	(12.89)	(1.07)	(1.07)	(0.35)	(0.35)	(1.15)	(1.15)
Low Pr(Dropout): control mean	19.66	19.66	0.15	0.15	0.06	0.06	0.19	0.19
(SD)	(12.49)	(12.49)	(0.97)	(0.97)	(0.23)	(0.23)	(0.77)	(0.77)

Notes: The table reports effects on academic achievements by students' predicted dropout probability based on Equation 6. *Passed credits* is the number of course credits passed by the end of the semester. *Std(GPA)* is the standardized and reverse-scaled grade point average at the end of the semester based on passing grades only (1.0 = best, 4.0 = worst on original German scale). *Dropout* indicates whether a student dropped out of their initial study program by the end of the semester. *Achievement index* is the standardized inverse-covariance-weighted average of the number of passed course credits, the GPA, and dropout. *Controls:* high school GPA, procrastination index, age, time since graduation, and dummies for women, high school degree Abitur, and first semester at any university. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

Table A.21: Effects on academic achievements – third semester

	Passed credits		Std(GPA)		Dropout		Achiev. index	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
T1: salary info	0.702	0.072	-0.023	-0.033	-0.021	-0.016	0.028	0.013
	(1.606)	(1.540)	(0.050)	(0.047)	(0.023)	(0.023)	(0.045)	(0.044)
T2: salary & OC info	-1.066	-1.389	0.000	-0.019	0.015	0.017	-0.031	-0.041
	(1.643)	(1.585)	(0.052)	(0.049)	(0.023)	(0.023)	(0.046)	(0.044)
T2-T1	-1.768	-1.461	0.024	0.014	0.036	0.032	-0.059	-0.054
	(1.664)	(1.611)	(0.051)	(0.048)	(0.023)	(0.023)	(0.046)	(0.045)
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes	No	Yes
N	2,222	2,222	1,707	1,707	2,222	2,222	2,222	2,222
Control mean	46.45	46.45	0.01	0.01	0.34	0.34	0.00	0.00
(SD)	(36.75)	(36.75)	(1.01)	(1.01)	(0.47)	(0.47)	(1.00)	(1.00)

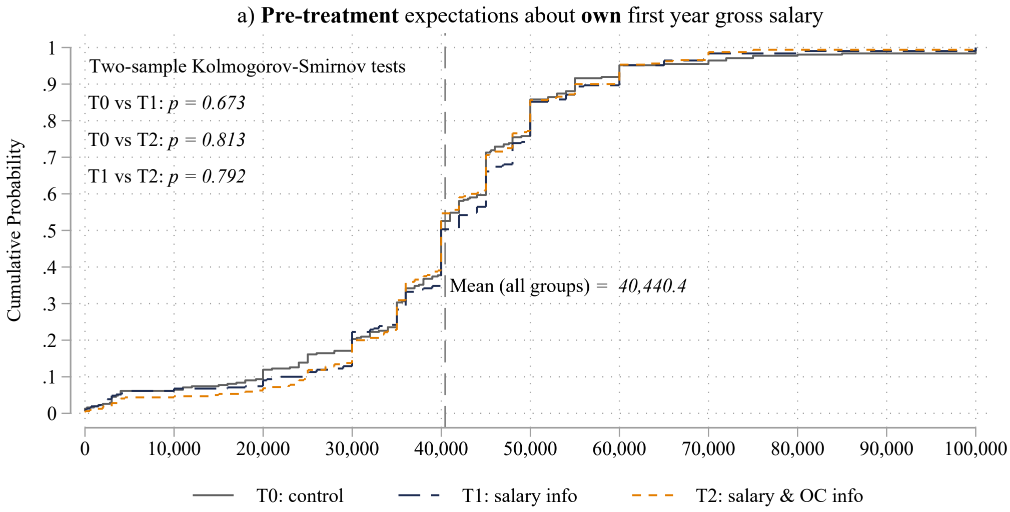
Notes: Passed credits is the number of course credits passed by the end of the semester. *Std(GPA)* is the standardized and reverse-scaled grade point average at the end of the semester based on passing grades only (1.0 = best, 4.0 = worst on original German scale). *Dropout* indicates whether a student dropped out of their initial study program by the end of the semester. *Achievement index* is the standardized inverse-covariance-weighted average of the number of passed course credits, the GPA, and dropout. *Controls:* high school GPA, procrastination index, age, time since graduation, and dummies for women, high school degree Abitur, and first semester at any university. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

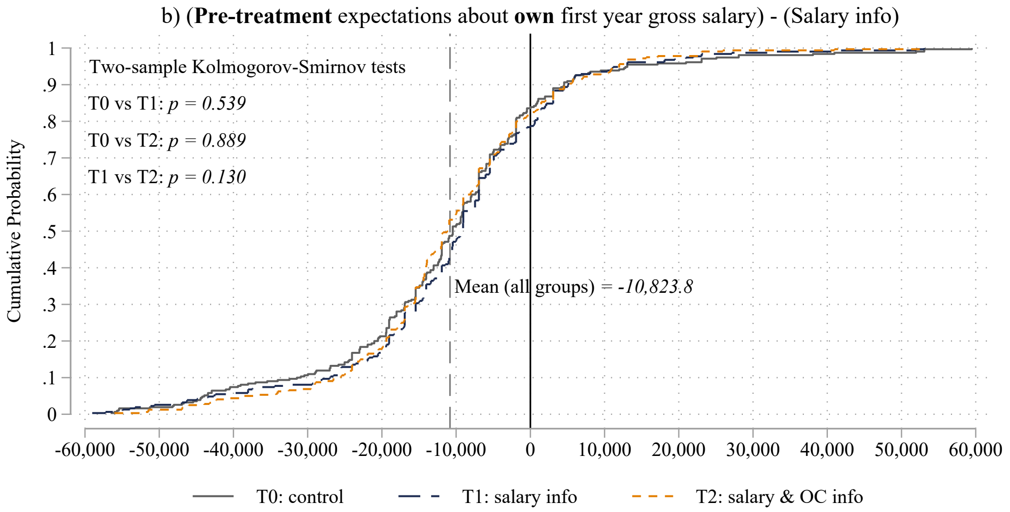
Table A.22: Treatment effects on academic achievements by predicted dropout probability – third semester

	Passed credits		Std(GPA)		Dropout		Achiev. index	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
T1: salary info	0.096	-0.474	0.025	0.011	-0.044	-0.039	0.090	0.078
	(2.284)	(2.192)	(0.078)	(0.075)	(0.035)	(0.034)	(0.067)	(0.065)
T2: salary & OC info	-2.116	-2.852	-0.067	-0.091	0.022	0.028	-0.059	-0.080
	(2.286)	(2.222)	(0.080)	(0.077)	(0.035)	(0.034)	(0.066)	(0.065)
T1*Low Pr(Dropout)	0.437	1.110	-0.093	-0.068	0.052	0.046	-0.141	-0.125
	(3.229)	(3.123)	(0.103)	(0.097)	(0.046)	(0.046)	(0.091)	(0.089)
T2*Low Pr(Dropout)	2.198	2.992	0.117	0.124	-0.015	-0.022	0.059	0.080
	(3.309)	(3.214)	(0.106)	(0.101)	(0.046)	(0.046)	(0.092)	(0.089)
T1+T1*Low Pr(Dropout)	0.533	0.636	-0.067	-0.057	0.008	0.007	-0.051	-0.047
	(2.262)	(2.199)	(0.066)	(0.061)	(0.030)	(0.030)	(0.062)	(0.060)
T2+T2*Low Pr(Dropout)	0.082	0.140	0.050	0.034	0.006	0.005	-0.001	-0.001
	(2.358)	(2.292)	(0.069)	(0.064)	(0.030)	(0.030)	(0.063)	(0.061)
Strata FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes	No	Yes
N	2,222	2,222	1,707	1,707	2,222	2,222	2,222	2,222
High Pr(Dropout): control mean	33.73	33.73	-0.22	-0.22	0.46	0.46	-0.33	-0.33
(SD)	(35.45)	(35.45)	(1.03)	(1.03)	(0.50)	(0.50)	(1.01)	(1.01)
Low Pr(Dropout): control mean	59.70	59.70	0.18	0.18	0.21	0.21	0.34	0.34
(SD)	(33.28)	(33.28)	(0.96)	(0.96)	(0.41)	(0.41)	(0.87)	(0.87)

Notes: The table reports effects on academic achievements by students' predicted dropout probability based on Equation 6. *Passed credits* is the number of course credits passed by the end of the semester. *Std(GPA)* is the standardized and reverse-scaled grade point average at the end of the semester based on passing grades only (1.0 = best, 4.0 = worst on original German scale). *Dropout* indicates whether a student dropped out of their initial study program by the end of the semester. *Achievement index* is the standardized inverse-covariance-weighted average of the number of passed course credits, the GPA, and dropout. *Controls:* high school GPA, procrastination index, age, time since graduation, and dummies for women, high school degree Abitur, and first semester at any university. Robust standard errors in parentheses. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

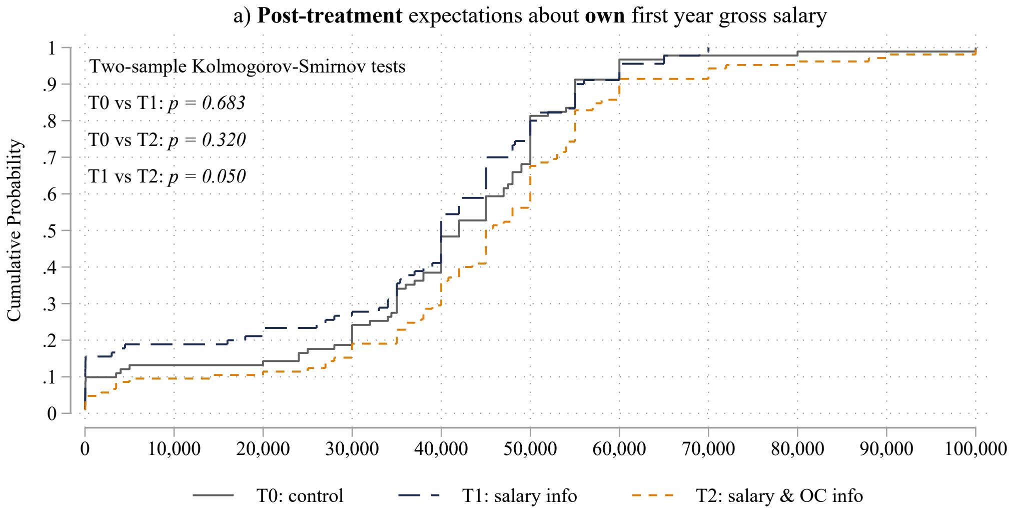
Figure A.1: Pre-treatment expectations about own first-year salary

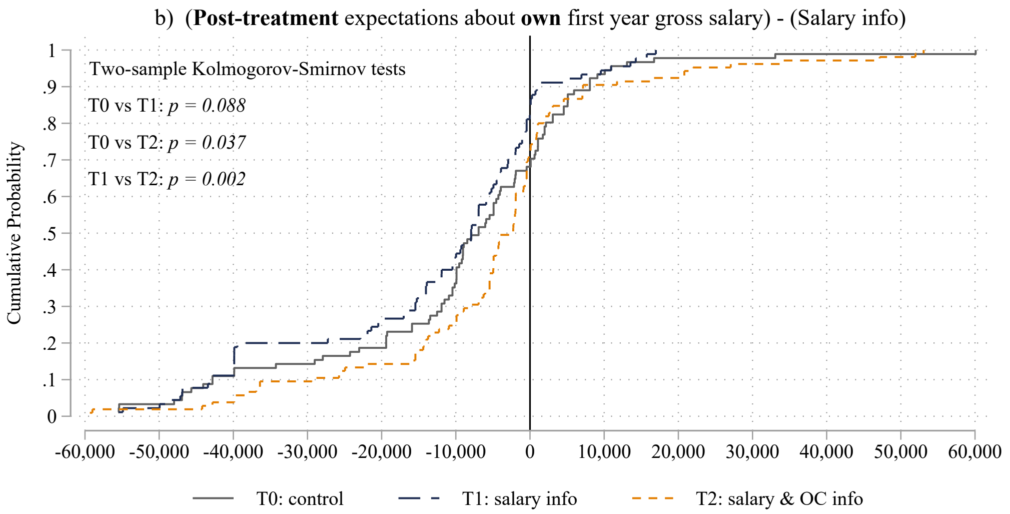




Notes: Panel a) shows the cumulative distributions of students' expectations about their own first-year salary (winsorized at €100,000) based on the OSA question "Now imagine that **you** received your bachelor's degree in the program for which you are answering these OSA questions. What do you believe is the **gross annual salary** that you would earn **during the first [...] year after graduating** if you worked **full time**?" (students could choose to provide "no answer"). Panel b) shows the cumulative distributions of the difference between students' expectations about their own first-year salary minus the salary information provided to students in the treatment groups. $N = 940$.

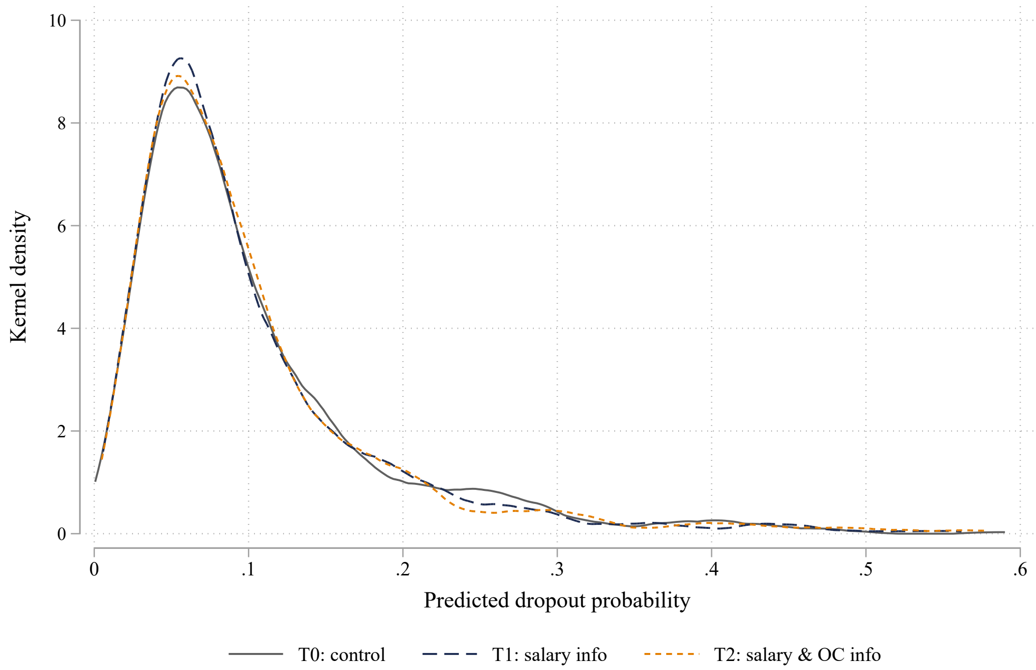
Figure A.2: Post-treatment expectations about own first-year salary





Notes: Panel a) shows the cumulative distributions of students' expectations about their own first-year salary (winsorized at €100,000) based on the survey question "Now imagine that **you** received your Bachelor's degree in the program you are currently studying. What do you believe is the **gross annual salary** that you would earn **during the first [...] year after graduating** if you worked **full time**?" (students could choose to provide "no answer"). Panel b) shows the cumulative distributions of the difference between students' expectations about their own first-year salary minus the salary information provided to students in the treatment groups. $N = 286$.

Figure A.3: Distribution of the predicted dropout probability



Notes: The figure plots the kernel density of the predicted dropout probability by experimental groups using the Epanechnikov kernel and a bandwidth of 0.015.

B Experimental materials and survey questions

Figure B.1: First Page of Letter I – T0: control (example from one of the two programs taught in English)

[redacted] 77

[redacted]

[redacted]

Contact:
[redacted]

12.10.2021

Your studies of the program [redacted]

Dear [redacted]

We are pleased that you are studying at the [redacted] and we would like to support you during your studies. A survey among your fellow students has shown that many of you would like to receive more information on planning your studies and on the career prospects after graduation. To this end we are currently testing different types of information.[1]

A lot of important information about your studies as well as about counseling and information services offered by the university can be found on the back of this letter.

We wish you all the best for your studies at our university and hope that you enjoy your time in [redacted]

Yours sincerely

[redacted]

1) If you no longer wish to receive this information in the future, please write an email from your [redacted] account with the subject "No information Bachelor" to [redacted]

Figure B.2: First Page of Letter I – T1: salary info (example from one of the two programs taught in English)

[redacted] 77

[redacted]

[redacted]

[redacted]

Contact:
[redacted]

12.10.2021

Your studies of the program [redacted]

Dear [redacted]

We are pleased that you are studying at the [redacted] and we would like to support you during your studies. A survey among your fellow students has shown that many of you would like to receive more information on planning your studies and on the career prospects after graduation. To this end we are currently testing different types of information.[1]

Information for [redacted] **in the Bachelor's program** [redacted] **Mtknr.** [redacted]

- The average gross annual salary (full-time) of similar students during the first year after graduating with a bachelor's degree in [redacted] is **€ 42,852**.[2]

- A lot of additional important information about your studies as well as about counseling and information services offered by the university can be found on the back of this letter.

We wish you all the best for your studies at our university and hope that you enjoy your time in [redacted]

Yours sincerely

[redacted]

1) If you no longer wish to receive this information in the future, please write an email from your [redacted] account with the subject "No information Bachelor" to [redacted]

2) Source: BAS and BAP, survey at the [redacted] cohorts 2012/13-18/19, responses of similar students to a question about starting salaries. Similar students studied the same or a related bachelor's degree program at the [redacted] The gross salary is the annual salary before deduction of taxes and social security contributions for full-time employment (38.2 hours per week including an end-of-year bonus of 0.25 monthly salaries) referring to the base year 2020.

Figure B.3: First Page of Letter I – T2: salary & OC info (example from one of the two programs taught in English)

[redacted] 77

[redacted]

[redacted]

[redacted]

Contact:
[redacted]

12.10.2021

Your studies of the program [redacted]

Dear [redacted]

We are pleased that you are studying at the [redacted] and we would like to support you during your studies. A survey among your fellow students has shown that many of you would like to receive more information on planning your studies and on the career prospects after graduation. To this end we are currently testing different types of information.[1]

Information for [redacted] **in the Bachelor's program** [redacted] **Mtknr.** [redacted]

- The average gross annual salary (full-time) of similar students during the first year after graduating with a bachelor's degree in [redacted] is **€ 42,852**.[2]

- **How does this affect the further planning of your studies?** Each additional semester of studying can lead to the loss of approximately half of that salary.[3]

- A lot of additional important information about your studies as well as about counseling and information services offered by the university can be found on the back of this letter.

We wish you all the best for your studies at our university and hope that you enjoy your time in [redacted]

Yours sincerely

[redacted]

1) If you no longer wish to receive this information in the future, please write an email from your [redacted] account with the subject "No information Bachelor" to [redacted]
2) Source: BAS and BAP, survey at the [redacted] cohorts 2012/13-18/19, responses of similar students to a question about starting salaries. Similar students studied the same or a related bachelor's degree program at the [redacted] The gross salary is the annual salary before deduction of taxes and social security contributions for full-time employment (38.2 hours per week including an end-of-year bonus of 0.25 monthly salaries) referring to the base year 2020.
3) This applies when entering the workforce after earning a bachelor's degree (BA). In the case of a subsequent master's degree (MA), this amount increases by the difference in salary between MA and BA graduates.

Figure B.4: Second Page of Letter I – all groups (example from one of the two programs taught in English)

Information:

- For a bachelor's degree you need to obtain **210 credit points** (so-called ECTS points). According to the study plan of your program, the regular duration for this is **7 semesters**. You can find the study plan and further information about your degree program here: [redacted]
- The first point of contact for all questions about studying is the **Studierendenservice:** [redacted]
- The **Allgemeine Prüfungsordnung** of the [redacted] can be found here: [redacted]
- You can retrieve the **Studienprüfungsordnung** for your specific program at [redacted]

Counseling services:

- The **Servicestelle Lernen** provides further information and interesting programs concerning the broad issue of learning at: [redacted]
- If you have general questions about your studies at the [redacted] please contact the **Zentrale Studienberatung**: [redacted]
- Mentoring by students from higher semesters is provided by the **Studienberatungsportal**: [redacted]
- You can find the **faculty advisor** on the website of your program under Program Advising & Guidance: [redacted]
- The **psychologische Studienberatung** provides counseling for personal problems that are rooted in or related to your studies [redacted]
- The Studentenwerk also offers **psychological counseling**: [redacted]

B.1 Online self assessment questions

1. What do you believe is the current **average gross annual salary** for full-time employment in the **first year after graduating with a bachelor's degree** in the degree program for which you are answering these OSA questions?
   Please provide the **gross annual salary (NOT: monthly salary!)**, i.e., the salary before taxes.
   If you do not want to answer the question, please enter −1 for"no answer".
   ______ *Euro;* −1 *= no answer*

   - How certain are you about this estimate?
     0% means "you are not sure at all" and 100% means "you are completely sure". You can also use any numbers between 0 and 100 to indicate where you fall on the scale.
     0%; 10%; 20%; 30%; 40%; 50%; 60%; 70%; 80%; 90%; 100%; no answer

2. Now imagine that **you** received your bachelor's degree in the program for which you are answering these OSA questions. What do you believe is the **gross annual salary** that you would earn **during the first and the tenth year after graduating** if you worked **full time**?
   Please provide the **gross annual salary (NOT: monthly salary!)**, i.e., the salary before taxes.
   If you do not want to answer the question, please enter −1 for "no answer".

   - Gross annual salary **during the first year** after graduating:
     ______ *Euro;* −1 *= no answer*
   - How certain are you about this estimate?
     0% means "you are not sure at all" and 100% means "you are completely sure". You can also use any numbers between 0 and 100 to indicate where you fall on the scale.
     0%; 10%; 20%; 30%; 40%; 50%; 60%; 70%; 80%; 90%; 100%; no answer
   - Gross annual salary **during the tenth year** after graduating:
     ______ *Euro;* −1 *= no answer*
   - How certain are you about this estimate?
     0% means "you are not sure at all" and 100% means "you are completely sure". You can also use any numbers between 0 and 100 to indicate where you fall on the scale.
     0%; 10%; 20%; 30%; 40%; 50%; 60%; 70%; 80%; 90%; 100%; no answer

3. Now suppose there is a free concert that lasts 90 minutes.

   - To get to the concert, you ride your bike for 20 minutes. When the concert starts, you realize that you don't like the music. Would you stay until the end?
     Yes, No, no answer
   - If you cycled 40 minutes to the same concert: would you stay until the end?
     Yes, No, no answer
   - And if you cycled 5 minutes to the same concert: would you stay until the end?
     Yes, No, no answer

4. Following next are four statements about yourself. How well do they describe you?
   Please indicate your answer on a scale from 0 to 10, where 0 means "does not describe me at all" and 10 means "describes me perfectly". You can also use any numbers between 0 and 10 to indicate where you fall on the scale.
   0 = does not describe me at all; 1; 2, 3; 4; 5, 6; 7; 8; 9; 10 = describes me perfectly; no answer

   - When I'm faced with an opportunity to make a purchase, I try to imagine things in other categories I might spend that money on.

- Before spending time on a particular activity, I consider other specific activities that I would not be able to spend time on.
- Before I make a particular purchase, I consider other specific items that I would not be able to buy.
- When I'm faced with the decision to spend time on a particular activity, I try to imagine other activities I might spend my time on.

5. Now suppose you bought a bottle of juice for €2.
   - When you start to drink it, you realize you do not really like the taste. Would you finish drinking it?
     Yes, No, no answer
   - Now suppose you bought exactly the same bottle (brand, quantity and quality) of juice for €5. Would you finish drinking it?
     Yes, No, no answer
   - And if you bought exactly the same bottle of juice for €1? Would you finish drinking it?
     Yes, No, no answer
   - Now suppose you got exactly the same bottle of juice for free at the checkout as part of a marketing promotion. Would you finish drinking it?
     Yes, No, no answer
6. We now ask for your willingness to act in a certain way.
   Please indicate your answer on a scale from 0 to 10, where 0 means you are "completely unwilling to do so" and 10 means you are "very willing to do so". You can also use any numbers between 0 and 10 to indicate where you fall on the scale
   0 = completely unwilling to do so; 1; 2; 3; 4; 5; 6; 7; 8; 9; 10 = very willing to do so; no answer
   - In general, how willing are you to take risks?
   - How willing are you to give up something that is beneficial for you today in order to benefit more from that in the future?
   - How willing are you to punish someone who treats **you** unfairly, even if there may be costs for you (e.g., in the form of money, time, or reputation)?
   - How willing are you to punish someone who treats **others** unfairly, even if there may be costs for you (e.g., in the form of money, time, or reputation)?
   - How willing are you to give to good causes without expecting anything in return?
7. Finally, we would like to know how well the following statements describe you as a person?
   Please indicate your answer on a scale from 0 to 10, where 0 means "does not describe me at all" and 10 means "describes me perfectly". You can also use any numbers between 0 and 10 to indicate where you fall on the scale.
   0 = does not describe me at all; 1; 2; 3; 4; 5; 6; 7; 8; 9; 10 = describes me perfectly; no answer
   - When someone does me a favor, I am willing to return it.
   - If I am treated very unjustly, I will take revenge at the first occasion, even if there is a cost to do so (e.g., in the form of money, time, or reputation).
   - When I meet new people, I assume they have only the best intentions.
   - I tend to postpone tasks even if I know it would be better to do them right away.

B.2 Online survey questions

1. First, we would like to ask you to indicate your age:
   __; No answer: -1
2. Are you ...
   Male; Female; Divers; No answer
3. 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?
   0 - Completely unsatisfied; 1; 2; 3; 4; 5; 6; 7; 8; 9; 10 - Completely satisfied; No answer
4. Now we would like to know more about your attitude towards your studies.
   What motivates you to learn during your studies? To what extent do you agree with the following statements? I study, ...
   1 - Completely disagree; 2; 3; 4; 5; 6; 7 - Completely agree; No answer
   - ... because I am intrinsically motivated, e.g. out of interest and enthusiasm for the content of my studies, out of curiosity, or because I like that the content of my studies is challenging.
   - ... in order to have greater opportunities later in life, e.g. higher chances of employment or financial security.
   - ... because I want to be among the best, e.g. I want to perform better than others in the exams and in my studies.
5. Please think about the current Semester: **On average**, how many hours **per week** do you dedicate to your studies?
   Please include all study activities, such as seminars or lectures you attend in person, streaming lectures, watching dubbed presentations, or video tutorials as well as your own study of lecture notes, textbooks, etc.
   ___ hours per week; No answer: -1
6. If you think about the current semester. To what extent do you agree with the following statements? With my studies I associate...
   1 - Completely disagree; 2; 3; 4; 5; 6; 7 - Completely agree; No answer
   - performance pressure.
   - freedom to organize studying according to my plans.
   - personal development.
   - stress.
7. Regardless of how your studies are going right now, how many semesters will it **ideally** take you to complete the Bachelor's degree in your current study program?
   If you plan to drop out of your current study program, please answer with "does not apply (-2)".
   __ semesters; No answer: -1*; Does not apply:* -2
8. And **realistically**, how many semesters do you think it will take you to complete the Bachelor's degree in your current study program?
   If you plan to drop out of your current study program, please answer with "does not apply (-2)".
   __ semesters; No answer: -1*; Does not apply:* -2
9. Next, we would like to know more about your interaction with other students in your study program.
   With how many students from your current study program are you in contact so closely that you regularly exchange or discuss course materials or plan on studying for exams together?
   __; No answer: -1

10. In the following we would like to ask you some questions about how you finance your studies. What sources do you use to finance your living in the current semester?
    Check any that apply
    Financial support from parents, partner, or relatives; Student financial aid according to BAföG; Bank loan for student finance, e.g., a student loan from the KfW banking group; Own income from employment; Vocational training pay, e.g. from a dual course of study; Own resources that were acquired or saved up before studying; Scholarship, except of BAföG; State benefits, e.g. child allowance, housing allowance, orphan's allowance or orphan's pension, but not student financial aid (BAföG); Other sources of finance; No answer

11. How much money do you have at your disposal on average **each month** during the current semester? Please think about all of the previously mentioned financial sources.
    Important: Please also take into account sums that other people pay directly to third parties for you, e.g. transfers of rent to your landlord.
    ____ *euros per month; No answer:* −1

12. Do you intend to pursue a Master's degree after completing your Bachelor's degree?
    Choose one of the following answers
    No; Yes, as directly as possible after completion of the Bachelor's degree; Yes, but not before I have acquired some professional experiences; Yes, but I am not sure yet at what time; No answer

13. And how satisfied are you with your studies, all things considered?
    0 - Completely unsatisfied; 1; 2; 3; 4; 5; 6; 7; 8; 9; 10 - Completely satisfied; No answer

14. Now follow some questions about the labor market prospects after a Bachelor's degree.
    What do you believe is the current **average gross annual salary** for full-time employment in the **first year after graduating with a bachelor's degree** in your current degree program?
    Please provide the **gross annual salary (NOT: monthly salary!)**, i.e., the salary before taxes.
    ______ *euros; No answer:* −1

    - How certain are you about this estimate?
      0% Not sure at all; 10%; 20%; 30%; 40%; 50%; 60%; 70%; 80%; 90%; 100% - Completely sure; No answer

15. Now imagine that **you** received your Bachelor's degree in the program you are currently studying: What do you think, how likely is it that you will find a job within the first 6 months after graduating?
    0%; 10%; 20%; 30%; 40%; 50%; 60%; 70%; 80%; 90%; 100%; No answer

16. Now imagine that **you** received your Bachelor's degree in the program you are currently studying. What do you believe is the **gross annual salary** that you would earn during the first and the tenth year after graduating if you worked **full time**?
    Please provide the **gross annual salary (NOT: monthly salary!)**, i.e., the salary before taxes.
    Gross annual salary **during the first year** after graduating:
    ______ *euros; No answer:* −1

    - How certain are you about this estimate?
      0% Not sure at all; 10%; 20%; 30%; 40%; 50%; 60%; 70%; 80%; 90%; 100% - Completely sure; No answer

17. Gross annual salary **during the tenth year** after graduating:
    ______ *euros; No answer:* −1

    - How certain are you about this estimate?
      0% Not sure at all; 10%; 20%; 30%; 40%; 50%; 60%; 70%; 80%; 90%; 100% - Completely sure; No answer

18. Finally, we would like to ask you some general questions about yourself.
    Now suppose there is a free concert that lasts 90 minutes.
    - To get to the concert, you ride your bike for 20 minutes. When the concert starts, you realize that you don't like the music. Would you stay until the end?
      Yes, No, No answer
    - If you cycled 40 minutes to the same concert: would you stay until the end?
      Yes, No, No answer
    - And if you cycled 5 minutes to the same concert: would you stay until the end?
      Yes, No, No answer
19. Following next are four statements about yourself. How well do they describe you?
    0 - Does not describe me at all; 1; 2, 3; 4; 5, 6; 7; 8; 9; 10 - Describes me perfectly; No answer
    - When I'm faced with an opportunity to make a purchase, I try to imagine things in other categories I might spend that money on.
    - Before spending time on a particular activity, I consider other specific activities that I would not be able to spend time on.
    - Before I make a particular purchase, I consider other specific items that I would not be able to buy.
    - When I'm faced with the decision to spend time on a particular activity, I try to imagine other activities I might spend my time on.
20. Now suppose you bought a bottle of juice for €2.
    - When you start to drink it, you realize you do not really like the taste. Would you finish drinking it?
      No, Yes, No answer
    - Now suppose you bought exactly the same bottle (brand, quantity and quality) of juice for €5. Would you finish drinking it?
      No, Yes, No answer
    - And if you bought exactly the same bottle of juice for €1? Would you finish drinking it?
      No, Yes, No answer
    - Now suppose you got exactly the same bottle of juice for free at the checkout as part of a marketing promotion. Would you finish drinking it?
      No, Yes, No answer
21. We now ask for your willingness to act in a certain way.
    0 - Completely unwilling to do so; 1; 2, 3; 4; 5, 6; 7; 8; 9; 10 - Very willing to do so; No answer
    - In general, how willing are you to take risks?
    - How willing are you to give up something that is beneficial for you today in order to benefit more from that in the future?
    - How willing are you to punish someone who treats **you** unfairly, even if there may be costs for you (e.g., in the form of money, time, or reputation)?
    - How willing are you to punish someone who treats **others** unfairly, even if there may be costs for you (e.g., in the form of money, time, or reputation)?
    - How willing are you to give to good causes without expecting anything in return?
22. How well the following statements describe you as a person?
    0 - Does not describe me at all; 1; 2, 3; 4; 5, 6; 7; 8; 9; 10 - Describes me perfectly; No answer
    - I tend to postpone tasks even if I know it would be better to do them right away.
    - When someone does me a favor, I am willing to return it.

- If I am treated very unjustly, I will take revenge at the first occasion, even if there is a cost to do so (e.g., in the form of money, time, or reputation).
- When I meet new people, I assume they have only the best intentions.

23. You now have the opportunity to let us know what kind of additional information and support you need from the university or faculty for successful studies.

C Pre-analysis plan

Pre-Analysis Plan (PAP): Opportunity Cost Neglect in Higher Education

Lars Behlen[1], Raphael Brade[2], Oliver Himmler[3], and Robert Jäckle[4]

October 19, 2021

1. Motivation and Research Questions

University students in many countries often take much longer than the prescribed time to graduate with a degree. For example, in Germany and in other OECD countries only about 40% of students manage to graduate within the regular study duration (Statistisches Bundesamt 2018; OECD 2019). From an individual perspective, long study durations imply directs costs, e.g., in the form of tuition fees, but also opportunity costs such as the foregone earnings due to later employment. Contrary to standard economic theory, recent literature suggests that individuals often only account for opportunity costs in their decision making when these costs are made salient (Frederick et al., 2009; Plantinga et al., 2018). It is therefore conceivable that opportunity costs are also neglected when it comes to study related decisions. Given that the opportunity costs of a longer study duration lie in the future, it seems particularly likely that those costs are not taken into account by students when deciding on their optimal effort level at the beginning of their studies.

Against this background, the intervention presented in this PAP tests whether explicitly pointing out opportunity costs of a prolonged study duration increases academic performance in the first semester. To this end, treated students are provided with information about the gross annual starting salary from recent graduates of the same or a similar study program and they are informed that each additional semester until graduation can imply the loss of half of that potential salary. Since research shows that students often have biased and inaccurate expectations about future earnings and that correcting those beliefs may lead to behavioral changes (Wiswall & Zafar, 2015; Conlon, 2021), we also include a treatment group that only receives information on the potential earnings without explicitly pointing out the potential loss of income that can accompany a longer study duration. This

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allows to test to what extent the effects of the first treatment are driven by the earnings information.

With the intervention and the analysis presented below, we plan to answer the following main research questions:

1. Does information on the opportunity cost of a prolonged study duration lead to increased academic achievement in the first semester on performance dimensions that are directly related to the duration of studies, i.e., course credits signed-up for, course credits attempted, and, most importantly, course credits passed?
2. Is explicitly stating that a long study duration can imply a loss of income more effective than just providing students with information on the gross annual starting salary of recent graduates?

2. Sample

We conduct our intervention at a German university of applied sciences with 2,222 incoming first semester students who enroll in one of 21 bachelor's programs in the winter semester 2021/22. Table 1 shows the number of students per study program:

Table 1: Observations by study program

Study program	**Freq.**
Applied Chemistry	90
Applied Mathematics and Physics	42
Civil Engineering	159
Business Administration	377
Electrical Engineering and Information Technology	193
Building Services Engineering	55
Energy Process Engineering	34
Computer Science	96
International Business	92
International Business and Technology	71
Management in Organic and Sustainability Business	23
Mechanical Engineering	198
Mechatronics/Precision Engineering	69
Media Engineering	61
Computer Science and Media	44
Medical Engineering	89
Social Work	297
Journalism of Technology	74
Process Engineering	27
Applied Materials Science	51
Information Systems and Management	80
Total	2,222

We will not exclude students from the analysis sample who drop out at any point after the treatment.

3. Design of the Intervention

Figure 1: Intended timeline of the intervention

Figure 1 shows the intended timeline of our intervention, which starts at the beginning of the winter semester 2021/22. Using administrative data on students' background characteristics, on October 08, we randomized 2,222 students into three different treatment groups (see Section 4 for information on the randomization procedure). On October 15, we sent a first unannounced (physical) letter by mail to students of all treatment groups (we describe the contents of the letters for the different treatment conditions in detail below). Around December 20, i.e., about four weeks before the beginning of the exam period, students will receive a second letter. The informational content of the second letter will be the same. The goal is to make the information salient at a time when students start preparing for their exams. In addition, it is planned to invite students to a post-treatment online survey between the first and second letter.

Depending on the experimental group, the letters include the following information:

Control group (**T0**): Letters for students in the control group contain information about counseling and information services offered by the university. This information is also publicly available on the web page of the university.

Earnings information (**EI**): The letters include the same information that the control group receives. In addition, they contain information on the average gross starting salary per year of recent graduates who studied the same or a similar program as the individual that receives the letter. Specifically the letter states that "the average gross annual salary (full-time) of similar students during the first year after graduating with a bachelor's degree in *study program* is **€ XX,XXX**".[5]

[5] The salary is based on aggregated data from surveys among graduates from previous cohorts that provide information on average gross hourly starting salaries. Based on this data we calculated gross annual salaries for full-time employment (38.2 hours per week including an end-of-year bonus of 0.25 monthly salaries) referring to the base year 2020.

Opportunity cost (**OC**): The letters include the same information that the earnings information (EI) group receives. In addition, directly after the earnings information, the letter states the following: "**How does this affect the further planning of your studies?** Each additional semester of studying can lead to the loss of approximately half of that salary."

4. Randomization Procedure

Students were assigned to one of the three experimental groups within blocks that we constructed by performing threshold blocking within study programs using the *R quickblock* package (Higgins et al., 2016). As a distance measure for the creation of blocks, we used the Mahalanobis distance with respect to students' high school GPA[6], their gender, and a proxy for procrastination of which we know that it is highly predictive of passed course credits.[7] To allow for the formation of multiple homogeneous blocks in all study programs, minimal block sizes range between 21 (larger study programs) and 6 (smaller programs). In total, we construct 120 Blocks across the 21 study programs. Figures 2 and 3 illustrate the formation of blocks for the study programs Business Administration and Process Engineering. The subsequent within-block randomization using equal assignment probabilities was performed with *Stata*'s *randtreat* command (Carril, 2017).

Table 2 shows the number of observations per experimental group as well as balancing characteristics for the variables used to construct the blocks and for four additional variables (age, time since high school graduation in years, a dummy for whether it is the first semester at a university at all, and a high school degree "Abitur" dummy[8]). The F-tests used for the construction of the p-values are based on regressions that control for block dummies and robust standard errors.

[6] The high school GPA was missing for 12 observations. To keep the sample complete, we imputed those values based on a linear regression of the high school GPA on age, a female dummy, time since high school graduation in years, a high school degree Abitur dummy, the procrastination index, a first semester at any university dummy as well as study program dummies, and the interaction of the study program dummies with the other variables.

[7] To construct the proxy, we used *Stata*'s *swindex* command by Schwab et al. (2020) to calculate the standardized inverse-covariance weighted average (Anderson, 2008) of the date of application for the study program and the date of enrollment. The date of enrollment was first standardized within study programs, due to differences in the timelines of the enrollment periods between study programs.

[8] High school degree Abitur refers to the German general track degree. It is one of the two main secondary school degrees in the tracked school system in Germany that qualifies students to study at a University of Applied Sciences; the second being the vocational track degree (Fachhochschulreife).

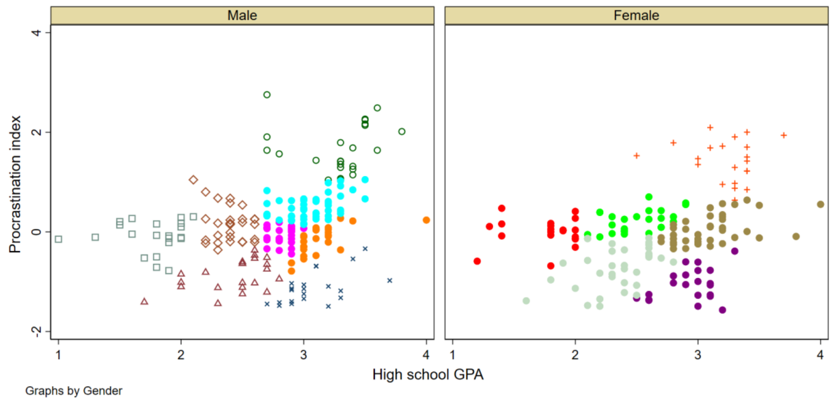

Figure 2: Threshold blocking in Business Administration (minimal allowed block size = 21)

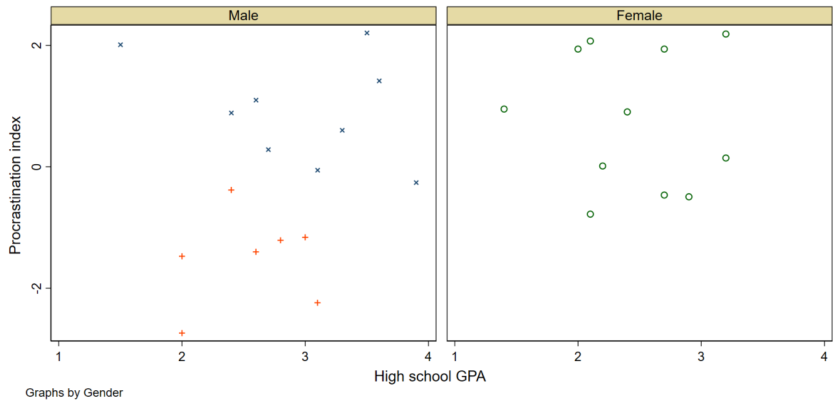

Figure 3: Threshold blocking in Process Engineering (minimal allowed block size = 6)

Table 2: Summary statistics and balancing properties

	TO	**EI**	**OC**	**p-value F-test**
HS GPA	2.538	2.527	2.508	0.219
Procrastination index	0.008	-0.034	0.026	0.098
Female	0.367	0.362	0.363	0.677
Age	21.683	21.617	21.607	0.918
Time s. grad. (years)	1.805	1.743	1.808	0.873
First university semester	0.732	0.739	0.708	0.337
HS degree Abitur	0.521	0.522	0.514	0.916
N	739	740	743	

5. Statistical Power

Assuming alpha = 0.05, we calculated effect sizes for comparisons between the experimental groups using the Stata *power twomeans* command for an R^2 of 0.00 (Column 3) and, using the *Optimal Design* software (Spybrooks et al., 2011), for assumed R^2 of 0.20 and 0.40 (Columns 4 and 5). The two latter R^2 are based on analyses with previous cohorts that show that the variables used for blocking (study program dummies, the procrastination index, high school GPA, and the female dummy) explain up to 40% of the variance in passed first semester credits.

Table 3: Minimum detectable effect sizes

Power	**N**	**Delta (R^2 = 0.00)**	**Delta (R^2 = 0.20)**	**Delta (R^2 = 0.40)**
0.6	1480	0.115	0.105	0.090
0.8	1480	0.146	0.130	0.114

6. Data Sources

For the analyses of the effects of the intervention, we plan to use data from the following sources:

Administrative data: The university provides us with administrative data on students' background characteristics and information from the application process. Some of the information from those sources was used in the randomization procedure and we plan to use some of it as covariates and for potential heterogeneity analyses.

The university will also provide us with information on the number of exams/credits that students sign up for[9] and with information on students' academic achievements, e.g., number of attempted and passed course credits, GPA, and dropout. We will use information from these sources for our outcome variables.

Online-Self-Assessments (OSAs): During the enrollment process, students of 9 study programs are obliged to complete a subject specific online self-assessment. Students from the other programs can also take those subject specific self-assessments or a voluntary general self-assessment. We were allowed to include a short module in the OSAs that takes about 5 minutes to complete. The module includes questions on subjects such as time preferences, procrastination tendencies, opportunity cost consideration, and earnings expectations. We plan to match the data from the OSAs with the administrative data.

Online surveys: We will invite students to participate in a voluntary online survey. Among others, it will include questions on expected earnings, the, students' current financial

[9] To take exams students have to sign up for them in advance during the sign-up period (see Figure 1). However, depending on the study program, students can later deregister from taking the exams that they signed up for; either during a specific deregistration period or by simply not showing up to the exam.

situation, the expected and intended study duration, as well as questions on non-cognitive outcomes such as intrinsic and extrinsic motivation, stress, and life and study satisfaction.

7. Variables

Primary outcome(s): The primary outcomes of the intervention are the number of course credits signed-up for, attempted, and passed in the first semester.

Explanatory outcome(s): Students' beliefs about expected earnings and the confidence in those beliefs from the post-treatment online survey.

Secondary outcomes: To study the net effects of our interventions, i.e., whether students trade off performance gains on the credit dimension with losses on other dimensions, we will also study effects on students' GPA, their dropout behavior, and on non-cognitive outcomes measured with the online surveys. When studying multiple non-cognitive outcome measures, we will also construct indices based on the standardized inverse-covariance weighted average of those outcomes (Anderson, 2008; Schwab et al., 2020).

Covariates: In some of our regression specifications we will not only include block fixed effects (FE) but also additional covariates (see Section 8). Currently, this includes all covariates shown in Table 2. For the selection and inclusion of any additional covariates in the specifications of our main analyses beyond those just mentioned, e.g., to increase the precision of the estimates, we will rely on the double post-lasso approach proposed by Belloni et al. (2014).

8. Analyses

8.1 Main Analyses

In our main analyses we will focus on the effects on the number of course credits signed-up for, attempted, and passed in the first semester. We will perform those analyses using OLS regressions with the following baseline specification:

$$y_i^k = \alpha_0 + \alpha_1 EI_i + \alpha_2 OC_i + \boldsymbol{s_i} + \varepsilon_i,$$

where y_i^k is the outcome of interest, EI_i and OC_i are dummies for being randomized in the respective treatment groups, and $\boldsymbol{s_i}$ are FE that control for the random assignment within blocks. In an additional specification, we will include a vector $\boldsymbol{x_i}$ that includes the covariates specified in Section 7.

Based on those specifications, we will test the following hypotheses:

1. $H_0: \alpha_1 = 0; H_1: \alpha_1 \neq 0.$
2. $H_0: \alpha_2 = 0; H_1: \alpha_2 \neq 0.$
3. $H_0: \alpha_2 - \alpha_1 = 0; H_1: \alpha_2 - \alpha_1 \neq 0.$

8.2 Explanatory and Secondary Analyses

We are planning to run the following explanatory and secondary analyses:

1. Using the respective survey outcomes, we will study treatment effects on students' expectations about future earnings as well as the accuracy of and the confidence in those beliefs.

2. We plan to use data from the OSAs to study whether the treatment effects depend on students' pre-treatment earnings expectations and their opportunity cost consideration.

3. To study the net effects of our intervention, i.e., whether students buy gains on the course credit dimension with declines in performance on other outcome dimensions, we will re-run the main analyses with our secondary outcomes (GPA, dropout, and non-cognitive outcomes).

4. Since the GPA is only observed for students who pass at least one graded module and because all outcomes from the online survey are only observed for students who answer the respective question, we will study whether observing these outcomes is affected by treatment and, if applicable, control for potential differences using inverse probability weighting.

8.3 Exploratory Analyses

For exploratory analyses we are mainly interested in the following:

1. We will explore whether treatment effects are heterogeneous with respect to the dimensions used in the threshold blocking procedure. I.e., we will study if treatment effects are heterogeneous with respect to students' procrastination tendencies, their high school GPA (= a proxy for ability), their gender, and across study programs. Since many study programs have only a small number of observations (see Table 1), we will group study programs into broader fields of study.

2. We plan to explore heterogeneity with respect to time preferences and procrastination tendencies which we measure based on questions in the OSAs.

3. We plan to explore heterogeneity with respect to students' current financial situation, which we measure in the online survey. Since the online survey is conducted post-treatment, we will first study whether treatment affects item nonresponse and the answering behavior.

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