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ABSTRACT

Information Provision and Postgraduate Studies*

This is the first paper to experimentally examine effects of information provision on beliefs about pecuniary and non-pecuniary returns of postgraduate education, enrollment intentions and realized enrollment. We find that our treatment causally affects beliefs measured six month after treatment. The effects on beliefs differ by gender and academic background, and we find that stated enrollment intentions change accordingly: in particular males significantly adjust their beliefs and intentions to undertake postgraduate studies. We follow the students further and provide evidence on actual enrollment one year after treatment. Taken together, this study highlights the relevance of information provision on pecuniary and non-pecuniary labor market returns for postgraduate study decisions.

JEL Classification: I21, I24, J24
Keywords: postgraduate education, information, RCT, expectations

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1 Introduction

Numerous studies in the economic literature document that compared to vocational training or high school education, returns to college education are high, with Katz and Murphy (1992) being a well-known early example. Consequently, the individual decision to enroll in college or not, has been widely studied. Based on quasi-experimental and experimental studies, we know that information, costs and beliefs all play important roles in explaining college decisions.[1] Recently, studies have documented the increasing variance in earnings within the group of college-educated workers, and estimated substantial returns to postgraduate education (Lindley and Machin, 2016; Altonji et al., 2016). This suggests that not only the initial decisions to enroll in college, but also postgraduate enrollment decisions matter.[2] Yet, comparatively little is known about factors that influence individual decisions to pursue postgraduate education. In a recent study, Boneva et al. (2019) show that pecuniary and non-pecuniary factors play a role by using a choice model, but, to the best of our knowledge, experimental evidence on factors that affect the postgraduate education decisions does not exist.

This study starts to fill this gap by studying effects of information provision about pecuniary and non-pecuniary returns to postgraduate education to undergraduate college students close to completion of their bachelor's degrees, in a randomized controlled trial (henceforth, RCT). We study effects of our randomized treatment on beliefs about pecuniary and non-pecuniary returns and how this affects postgraduate enrollment intentions six month later. Moreover, we can provide evidence on realized enrollment in postgraduate education twelve months after treatment.

The treatment consists of information about pecuniary and non-pecuniary returns of postgraduate versus undergraduate degrees in the labor market, based on existing workers. The treatment is delivered at the end of an online survey. The online survey was programmed and administered by a renowned survey institute (Kantar Public) to ensure a professional interaction with survey participants. Students are invited via email to par-

[1] See detailed literature review below.

[2] Note we use "postgraduate education" in the European sense, i.e. including Masters degrees, which is called "graduate education" in the US.

ticipate in an online survey providing them with an individual link. The link works with smartphone, tablets, and PCs, as the online survey was mobile-ready. In this online survey, we then present treated students with a range of information, i.e. earnings levels and differentials for different occupations and sectors, allowing students to place themselves and to update their beliefs. This is because providing students with a single number, like average postgraduate earnings, might be misleading. The information provision was based on visual and audio material. Students were shown some informative graphs with explanatory text and helpful audio explanation transporting the depicted information. After each information slide, students were asked to answer a comprehensive question about the previous screen. Students could not continue to the next screen without listening to the audio file and without answering the short comprehensive question. However, students could go back to the previous screen. Moreover, we present information about a range of non-pecuniary labor market returns depending on degree (bachelor's vs master's degree), such as on the likelihood of having a job with high responsibility or whether the job will be easy to combine with family life, or not. Note, we do not present any information on costs and benefits of the student experience as such. Our target population already has first-hand experience on these through their undergraduate studies. In this regard, the information set available to students who decide about postgraduate enrollment differs to the information and decisions about initial college-going at the end of high school. Rather than providing students with information about the student experience, our treatment gives information about pecuniary and non-pecuniary returns on the labor market depending on undergraduate or graduate degrees, of which undergraduate students have no first-hand experience.

The study population was recruited out of an existing experimental panel study that focused on the initial college-going decision of high school students of the 2014 graduation cohort in the Berlin area, Germany (Peter and Zambre, 2017; Peter et al., 2018). Our focus on the 446 students presumably enrolled in their final years of the undergraduate program in 2017 resulted in a number of benefits, including access to information on pre-baseline characteristics that were collected in the past. In particular, pre-baseline information on postgraduate enrollment intentions was available and we used this, together with more background variables, to implement a randomization design based on pair-wise matching. Moreover, we believe the fact that the targeted students have had experiences in a previous panel study might explain the very low rates of attrition in the two follow-up surveys of this experiment.

We use the information collected in the intervention and the two follow up surveys that we conducted six and twelve month after treatment in four steps.

First, we present correlations between postgraduate enrollment intentions and pecuniary and non-pecuniary returns for our control population. This confirms the relevance of both sets of factors, in line with existing research that stresses that non-pecuniary factors matter in addition to –and potentially more than– pecuniary factors for postgraduate education (Boneva et al., 2019).

Second, we examine how the treatment has shifted individual beliefs about pecuniary and non-pecuniary returns of postgraduate education. This is interesting both, to understand later effects on intentions and enrollment, but also in its own right as it sheds light on belief updating. This is because the treatment consisted of objective information on a range of attributes of jobs, for example average earnings for different occupations – and so depends not only on existing beliefs but also on how students place themselves in the categories that we have presented. The main finding here is that many students previously either held very accurate beliefs about pecuniary and non-pecuniary differences between graduate and postgraduate jobs, or did not significantly update their beliefs due to our online information intervention. The largest, and statistically significant, updating of beliefs occurs for males, who downward adjust their expected postgraduate earnings premium.

Third, we examine how the treatment affected postgraduate enrollment intentions stated six month later. Here, we find effects that mirror the effects on belief updating documented above: males are significantly less likely to state the intention to directly enroll for a postgraduate degree following the successfully completion of their undergraduate studies. We find further heterogeneity along parental background, which however are rarely significant at conventional levels of statistical significance.

Fourth and finally, we estimate effects on postgraduate enrollment twelve months after initial treatment. Here, we again find the largest and negative estimates for male students and for students with academic parental background.[3] However, these estimates are not statistically significant, most likely due to the fact that most students are still enrolled in their undergraduate studies, taking longer than expected to complete these.

Taken together, we present causal evidence that an information treatment on pecuniary and non-pecuniary returns can have long-run consequences. We show significant effects on enrollment intentions measured six month later, and supportive evidence on enrollment one year after treatment. In addition, we document that the treatment has lead to a different updating of beliefs of students, with male students significantly downward adjusting expectations of postgraduate wage premia. These differences in belief updating from the same treatment are in line with the heterogeneity in the effects that we document on direct postgraduate study intentions and enrollment. This study therefore has two main contributions: first and foremost, we provide the first causal evidence of the role of information for postgraduate enrollment decisions. Moreover, we document that the heterogeneity that we find in the treatment effect of receiving information is in line with the heterogeneity that we find in belief updating. This means that despite our finding that male students react strongly to our information treatment, and female students do not, this does not imply that males and females place a different importance on information when making decisions. Differences in belief updating and information processing presents an alternative explanation to heterogeneity in treatment effects of information treatments.

This study is related to the large literature on the role of financial constraints or on the lack or effectiveness of information about actual costs and future monetary returns for the college enrollment decisions (see for example, Dynarski, 2002; Dynarski and Scott-Clayton, 2006; Bettinger et al., 2012; Oreopoulos and Dunn, 2013; Wales, 2013; Bettinger and Baker, 2014; Castleman et al., 2014; Kerr et al., 2015; Wiswall and Zafar, 2015;

[3]Students are considered to come from an academic parental background if at least one of their parents holds a college degree.

Castleman and Long, 2016; Oreopoulos and Ford, 2016; Carrell and Sacerdote, 2017; Dynarski et al., 2018). In the German context, Peter et al. (2018) and Peter and Zambre (2017) study the effects of providing information about returns and financing possibilities for college education to high school students. One key finding is that students of non-academic background, in particular those with intentions to enroll, are more likely to pursue college education if they have received information about its benefits. Moreover, an existing literature on individuals' beliefs about returns to educational investment shows that besides pecuniary, especially non-pecuniary returns can explain educational decisions (Boneva and Rauh, 2017; Belfield et al., 2019). This paper differs from this literature because we study postgraduate education decisions. In an important and recent paper, Boneva et al. (2019) show that both pecuniary and non-pecuniary returns also matter for postgraduate education decisions. We complement this literature by providing first experimental evidence on these, as well as on the role of information.

The remainder of this paper is structured as follows. The next section describes the institutional context and data. In section 3 we describe the treatment, randomization and compliance. Section 4 describes our estimation strategy and outcome variables. Section 5 presents the estimates on stated beliefs, enrollment intentions and enrollment. Here, we also provide descriptive evidence on the association between pecuniary and non-pecuniary postgraduate returns and enrollment intentions. Section 6 concludes.

2 Institutional context and data

2.1 Institutional Context

Germany has a well-established two-tier setting where students first enroll for a bachelor's degree that typically lasts for four years.[4] Overall, about 60 percent of bachelor's graduates move on to study for an additional two years to earn a master's degree (Spangenberg and Quast, 2016). These percentages are higher for university students compared to those at universities of applied sciences, which usually offer more practically oriented degrees.

[4]Before the Bologna-reform in the 2000s Germany had a system of longer single-tier degrees.

Moreover, they are higher for students with academically educated parents and for male students. For these groups the transition rates are between 70 and 80 percent, while the others have lower rates (50 to 60 percent).

Most bachelor's students who continue with a master's program do this without an interruption. Only about 20 percent perform or plan a transition after a short interruption (Spangenberg and Quast, 2016). The main reason for an interruption among university students are internships. In a survey among bachelor degree students this is stated by 36 percent. The reported main reason among other students is the intention to gain work experience: 42 percent. This is also the main reason for those who finish their higher education with a Bachelors degree. Another important reason for no transition to a master's program are attractive job offers (Autorengruppe Bildungsberichterstattung, 2018).

In general, higher education in Germany (at public institutions) is free of charge, with students paying only a small administrative fee each term. There are no fee-differences between bachelor's and master's programs.

2.2 Data

A central design feature of this RCT is that we sample the students from a population of students who are likely to pursue postgraduate studies or to enter the labor market after their undergraduate degree. We exploit existing knowledge about students from the *Berliner-Studienberechtigten-Panel* (Best Up) to sample our study population. This panel study provides us the necessary target population, as it comprises vast information about students starting from their enrollment in undergraduate studies until students' early intentions of postgraduate enrollment.[5] The Best Up data contains very detailed information about students of the cohort that graduated from high school either in summer 2014 or one year later. These students come from a relatively homogeneous environment and are followed from the last year prior to high school graduation (*Abitur* in German) to the first two years of college or vocational training. Although the Best Up data provides

[5]For further information on the Best Up study and data see Ehlert et al. (2017); Peter and Zambre (2017); Peter et al. (2018).

us with undergraduate students from the same high school cohort, not all directly enrolled in college after high school graduation in 2014. Around 30% of the Best Up participants took a gap year after high school. Thus students in our sample are progressing at different speeds through their undergraduate studies. In addition, the speed varies, because students enrolling in universities of applied sciences take on average one year longer to finish their bachelor's degree due to different program structures compared to university majors.

In the Best Up data, a majority of students who started studying for a bachelor's degree, start their final year of undergraduate studies in winter 2017. Thus, in winter 2018 these students are likely to transit to postgraduate studies or to enter the labor market. Out of the Best Up data we identify 446 students who are likely to be studying in winter 2017 as our target population of which 371 students (83%) participated in the baseline survey (see section 3 for more information about the RCT and the survey).

In table 1 we provide further descriptive evidence looking at students from the first survey of our study. We show means of all matching (pre-trial) variables as well as baseline study and background characteristics. The sample consists of a majority of students from a non-academic background, who are slightly less likely to enroll in postgraduate degrees. Students are on average 23 years old and in their fifth semester.[6] This shows that the majority of students in our sample is at the end of their undergraduate degree. As described in section 2.1 some majors are still organized under the old degree system prior to the change to the two-tier structure. In our sample about 6% of students are enrolled in such a major. The majority of students (78%) are enrolled in a bachelor's degree. 46% students in our sample intend to directly enroll in postgraduate studies in December 2017 (baseline) and 48% in May 2018 (first follow-up). In December 2018, 26% of those students who participated in the first and the last survey of our study (N=293) are enrolled in postgraduate studies (see last row in table 1).

[Table 1 about here]

A first comparison of our initial target sample with a nationwide representative study

[6] In Germany one year of college is divided in two terms called semester.

already shows, that students are fairly compatible in terms of age, final GPA, intentions to enroll in postgraduate studies to the average German student (see table A1 in the Appendix A).[7] The former comprises students that also graduated from high school in 2014, in this group of students, 50% come from a nonacademic background, i.e. are first generation students, compared to 59% in our baseline sample, thus we have a slightly higher share of students from nonacademic backgrounds. In the SC5 cohort, students were sampled in the winter term 2010/2011 at German universities and universities of applied sciences, these students graduate earlier than students in our sample. In this sample 63% of students are first generation students and 83% are enrolled in a bachelor program, compared to 78% in our baseline sample. All in all, the baseline sample is – with very few exceptions – very similar to students from NEPS, which is a representative survey for students (see table A1 in Appendix A).

3 Details of intervention

We conducted three online surveys to accompany bachelor students from the *Best Up panel* at the transition to postgraduate studies or the labor market. In the first survey in December 2017/January 2018 we routed students according to their treatment status and presented to those in the treatment group a series of screens with information about realized pecuniary and non-pecuniary returns on the labor market differentiated by college degree. The following section introduces the treatment in detail and discusses timeline and set up of the randomized controlled trial.

3.1 Treatment

The information treatment consists of an online learning module that informed students about different aspects relevant for the postgraduate decision. The learning module comprised visual and audio information and addressed three topics: realized pecuniary and non-pecuniary labor market returns by college degree, and funding options for postgrad-

[7]We compare our sample to two so-called starting cohorts of the National Educational Panel Study (NEPS): SC4 and SC5 (see Blossfeld et al., 2011, for more information).

uate studies.

In figure 1 we show two exemplary visual information that students received in the online learning module.[8] The figure at the top of figure 1 presents a pecuniary example of realized labor market returns by college degree type, whereas the bottom figure depicts a non-pecuniary example, namely probability to work in high-skilled occupations. For all measures depicting pecuniary returns examples we used data from the Microcensus (*Mikrozensus* in German).[9] The measures for the non-pecuniary examples were constructed using another large nationwide household survey the German Socio-Economic Panel Study (SOEP).[10] Using large representative data sets to construct the measures for the treatment allowed us to tailor the information to students close to finish their bachelor's degree and to provide students with information not widely available. Providing information by different college qualifications is important, as students might not observe pecuniary and non-pecuniary returns in their environment. Numbers on realized labor market returns are not widely available in newspapers or on the web for different college degree types given that the two tier system is still young.

[Figure 1 about here]

The realized earnings by college degree and occupational group shown in figure 1, for example, depict average realized earnings of bachelor graduates in jobs in natural sciences as well as those of master graduates and explicitly mark the difference between both qualifications. Earnings over time and separated by gender were shown in other graphs. Although the two tier system is still young, master graduates have been around long enough in the labor market to describe realized wage differences in the first years of employment. These first five to ten years after college graduation are exactly those years of realized labor market returns we are interested in to support near-bachelor-graduates at the transition to postgraduate studies or the labor market with relevant information.

[8] Examples of programmed screens as seen by students are included in Appendix B.

[9] The Microcensus is an annual household survey providing nationwide representative statistics on the population and the labour market in Germany. It surveys 1% of the population in Germany.

[10] The SOEP has been carried out since 1984 and in 2017 more than 30,000 individuals in approximately 17,000 households participated in (see Wagner et al., 2007).

For non-pecuniary labor market returns the online learning module comprised for example information about the likelihood to work in high-skilled occupations (see bottom figure in figure 1). This visual material shows the percentage of employees with bachelor or master's degree working in highly skilled occupations, i.e. in team/department leading positions or managerial position.

Apart from pecuniary and non-pecuniary realized labor market returns the online learning module also comprised information about funding possibilities of postgraduate studies. We informed students about different funding sources in Germany and highlighted for example that students can also apply for student aid (*BAföG (Bundesausbildungsförderungsgesetz)* in German) for a master degree, as many eligible students tend to believe the support covers only the first degree. Appendix B comprises example screenshots of programmed visual material shown in the online learning module. Together with a professional field institute, we worked out ways to provide the information on multiple screens to make the content reader-friendly and to monitor students' behavior: they were only able to continue to the next screen after listening to the figure-guiding audio message and answering a simple knowledge-based question. These questions were implemented to ensure that students had looked at the material and understood the visualized information, as it is otherwise very difficult to know for certain that students looked at the information with online or handout based provision compared to information provided in person (see for example, Oreopoulos and Dunn, 2013; Peter and Zambre, 2017).

The information treatment consists of various pecuniary and non-pecuniary labor market returns, providing also information by different fields and by gender, as we do not know what type of returns the individual will experience. Showing realized returns differentiated by college degree helps students to place themselves using their own best guess.[11]

3.2 Implementation, timing of intervention

We implemented in total four online surveys which were optimized to smartphones, tablets and computers for easy access to participation. In a first very short pre-trial survey we

[11] The study has been approved by an IRB (see for more information the AEA RCT registry entry under `https://doi.org/10.1257/rct.2446-2.0`).

assessed how many students would still be studying in the winter term 2017. This pre-trial survey took place from August to September 2017 (see figure A1 in Appendix A). The following three online surveys took place from December 2017 to January 2018, from May to June 2018, and from December 2018 to January 2019.[12] From the pre-trial survey in fall 2017 we received a target population of 446 potential students still studying for the bachelor's degree in winter 2017. Out of these 446 students, response rates in all three trial surveys are very high and lie always clearly above 80% (see number of participants per survey in figure A1).[13]

The baseline survey in December 2017/January 2018 was conducted about 7-8 months before final year students would typically graduate with an undergraduate degree. At the end of this first online survey treated students were routed to the online learning module (see also section 3.1). The first follow-up survey was 6 months later in May/June 2018. With this first follow-up we were able to measure students intentions to enroll in postgraduate studies. These intentions measured up to 6 months after the first survey are comparatively long run intentions and most likely coincide for the majority of students with their application process for postgraduate studies. The second follow-up survey was conducted 12 months after treatment in December 2018. With this second follow-up we asked students about their actual enrollment. With the winter term 2018, we expected most students to have graduated from their undergraduate studies and have directly enrolled in master's program. As our data from 12 months after treatment shows, this second follow-up was still a little bit early to detect the full effect on actual enrollment, as students are still more likely enrolled in undergraduate programs and less likely to be enrolled in postgraduate studies.

3.3 Randomization and compliance

The randomization of students into treatment and control groups was implemented using pair-wise matching. Bruhn and McKenzie (2009) show that in small samples, other methods than pure randomization can improve the degree of balance among relevant

[12]The administration of all the surveys was carried out by a renowned survey institute (Kantar Public).
[13]Compared to other response rate of similar RCTs, this response rate is very high and satisfactory.

pre-treatment characteristics and follow-up outcomes. Pair-wise matching allowed us to balance treatment and control students matching on many variables predictive of the outcome variables and thereby increasing efficiency and power of hypothesis testing. We applied the greedy pair-wise matching algorithm mentioned and provided by Bruhn and McKenzie (2009). Since we utilize data from the *Best Up panel*, we had enough information and time to perform randomization using matching techniques, as information about, for example, pre-trial postgraduate intentions, GPA from high school graduation or gender was already available. Pair-wise matching using baseline characteristics would not have been feasible, as the treatment took place immediately after the baseline data collection. After having selected "statistical twins" based on a rich set of pre-treatment characteristics, we randomized participants in each pair into treatment and control groups.

Table 2 shows the balancing of covariates in the pre-trial survey (Aug/Sept 2017) and the baseline survey (Dec 2017/Jan 2018). We separately regress balancing variables on a treatment group dummy to calculate raw treatment group differences. To account for the ex-ante balance approach, we further regress balancing variables on a treatment group dummy and pair fixed-effects using one joint fixed effect for incomplete pairs. The actual difference between treatment and control group means is not statistically significant for the pair-wise matching variables and the variables on intentions, enrollment, as well as background characteristics. Yet, a statistically significant higher share of treated students has a migration background. The same picture emerges for the treatment group differences controlling for pair fixed effects. Overall, as confirmed by the F-tests for joint orthogonality, the randomization was successful in dividing the sample into two groups, which are highly similar in their characteristics.

[Table 2 about here]

The attrition rate is not significantly related to the treatment and equals 13.2% in the control group at the first follow-up and 21% at the second follow-up and is again insignificantly higher in the treatment group. Attrition is also not related to most matching variables and important predictors of postgraduate enrollment intentions in neither the control or treatment groups. Albeit attrition is small and does not differ between treatment and control groups, we see a small statistically significant difference between male and female participants (see table A2 in Appendix A). Females are less likely to drop out at the second follow-up. While this does not imply that treatment effect estimates are biased, we acknowledge that it might limit representativeness of our estimates for our baseline sample. We therefore also run separate regressions for females and males in the analysis below.

4 Empirical analysis

4.1 Estimation specification

The main model for the estimation of treatment effects is

$$Y_i^{post} = \alpha + \delta T_i + \gamma W_i + \beta X_i + \epsilon_i, \tag{1}$$

where Y_i^{post} is the post-treatment outcome of a student i and T_i is a binary treatment group indicator. In our main specification, we control for variables used for pair-wise matching W_i to account for the randomization procedure. In addition, we control for a set of baseline characteristics X_i to improve power (direct and general enrollment intentions and postgraduate enrollment at baseline).

As shown by Bruhn and McKenzie (2009), the most accurate way to account for the randomization procedure is to run a regression on the treatment group indicator and pair fixed effects. Otherwise, standard error estimates tend to be overly conservative. However, this leads to the omission of observations from pairs with only one follow-up observation. As this lowers the effective sample size – particularly for subgroup analyses – we control for matching variables W_i instead. In table A3 we show different specifications for our main outcome variable using pair fixed effects (assigning one fixed effect to all students from incomplete pairs) and additional control variables.

4.2 Outcome variables and effect heterogeneity

A first set of outcome variables consists of students' beliefs about pecuniary and non-pecuniary labor market returns by degree type. We asked students to rate the answers to the following question: "Please think about the time in the near future when you are 30-35 years old. Further assume, you are working full-time then. Certain aspects of your life might depend on whether you graduated with a bachelor's degree or with a master's degree. How likely do you think that you will ...". We provided students with the following five pecuniary and non-pecuniary aspects for both bachelor and master's degree and asked to rate these on a likert-type scale from 1 (very unlikely) to 7 (very likely): (1) to earn above average income, (2) to do intellectually challenging work, (3) to be able to combine work and family life, (4) to work in a highly-skilled job or with managerial responsibility, or (5) that parents are satisfied with their job.[14] Table 1 already shows how students on average rate these five dimensions for each degree type. For example, students on average rate the probability to work in a highly-skilled job or with managerial responsibility with 4.1 with a bachelor's degree and 5.5 with a master's degree.[15] We construct the perceived postgraduate return measures as the difference in perceived probabilities between a master's and bachelor's degree.

The second set of outcome variables comprises students' *intentions* to pursue postgraduate studies and their *actual enrollment* in master's programs. Given the German context, measuring students' postgraduate application behavior is somewhat difficult, similar to measuring undergraduate application (see Peter et al., 2018, for a discussion regarding bachelor's programs). Not all study programs require students to apply. In many programs, they can just enroll without any further requirements. We therefore focus in particular on students' intentions to enroll *directly* after obtaining a bachelor's degree. We measure postgraduate enrollment intentions using a binary variable measuring *direct* transition intentions. We define *direct intentions* as intending to enroll in a postgraduate

[14]We use these particular categories to elicit students' beliefs, as they have been shown to matter by Boneva and Rauh (2017), which allows us to compare our findings to the emerging literature on pecuniary and non-pecuniary returns and educational choices.

[15]While these values are for the overall sample at the first follow-up, Table A5 shows similar values for the control group only.

program immediately after completion of the bachelor's program. We code all students for those the question does not apply due to permanent study termination as 0 and students already enrolled into a postgraduate or 5-year program as 1.

Enrollment in postgraduate studies is defined for students who completed their bachelor's degree and are enrolled in a master's program at the second follow up survey. They are coded as 1 and the bachelor graduates no longer enrolled in higher education, either at universities or universities of applied sciences, are coded as 0. We are aware that the scope for finding effects 12 months after treatment is limited if students progress slower through their studies than the population average. In particular for nonacademic background students, who are more likely to not directly enroll in postgraduate studies (see section 2.1).

Besides a potential deferral of enrollment, nonacademic students might differ in their beliefs about labor market returns to a postgraduate degree compared to students from an environment where college returns are observable (see Boneva and Rauh, 2017). In addition, the effects might also vary by gender. Studies show that the expected returns to a bachelor or master degree differ for male and female students (see for example Reuben et al., 2017; Zambre, 2018).

5 Results

5.1 Beliefs about pecuniary and non-pecuniary returns

In order to elicit students' beliefs about different returns for either bachelor's or master's degrees, we asked them to rate five pecuniary and non-pecuniary aspects separately for each degree type on a likert-type scale from 1 to 7 (see section 4.2 for more details). In the following, we show associations between postgraduate enrollment intentions and a range of pecuniary and non-pecuniary return factors for the control group at the first follow-up. Table 3 presents estimates of a new regression equation in each column, with differing sets of covariates: column (1) shows that the perceived probability to earn an above average income is significantly correlated with intentions to study a master's degree. Columns

(2) to (5) introduce the non-pecuniary factors. Interestingly, parental satisfaction with job does not matter, possibly because these bachelor's students are already somewhat detached from their parental background.[16] All other factors are significant predictors in the association between non-pecuniary beliefs and direct enrollment intentions. Next, in column (6) we jointly estimate the associations between perceived pecuniary and non-pecuniary returns and enrollment intentions. Last but not least, in column (7) we combine the non-pecuniary factors into a preference-weighted index of non-pecuniary returns.[17] As before, and in line with the existing literature, we find that these two measures of perceptions of pecuniary and non-pecuniary returns matter (Boneva and Rauh, 2017; Belfield et al., 2019).

[Table 3 about here]

5.2 Causal effects on perceived returns

In figure 2 we show treatment effects on students' beliefs about pecuniary and non-pecuniary postgraduate returns. The upper panel shows the overall effect for the preferred specification, the middle panel effects by gender, and the bottom panel effects by academic background.[18] Each sub-figure in figure 2 lists the results for the one pecuniary return measure ("earn above average income") and the four non-pecuniary return measures, as described in section 4.2.

Figure 2 shows that treated students increase their beliefs about a bachelor's degree six month after treatment (see also table A4 in appendix A). Treated students mainly update their beliefs about monetary returns with an undergraduate degree, but also increase the returns to a bachelor regarding non-pecuniary returns, such as "to do more intellectually challenging work" and "to work in a highly skilled job". In particular treated male students and students with at least one parent with a university degree increase their pecuniary

[16]This finding is different to Boneva et al. (2019) who find a large and statistically significant effect for parental support. However, it might be possible that they measure another aspect of parental support.

[17]Students were asked to rate the importance of each category on a scale from 1 (not important at all) to 5 (very important). We constructed the non-pecuniary return index by weighting each of the 4 non-pecuniary return measures with the relative importance reported by the respective student.

[18]The estimates are also summarized in table format in table A4 in appendix A .

beliefs about a bachelor's degree, making a master's degree relatively less attractive.

[Figure 2 about here]

Treated male students increase their belief "to earn above average income" with a bachelor's degree by 0.55 points. This leads to a large decrease in the difference between both degrees (-0.57 points, see also column 3 in table A4 in the appendix). Students from an academic background also significantly adjust their beliefs about monetary returns of a bachelor's degree. Similar to male treated students, the difference between the beliefs by degree significantly decreases by -0.46 points.

Female students and students from non-academic backgrounds do not significantly update their beliefs about either degree due to the information treatment. In addition, we show the absolute levels of perceived returns in the control group in table A5. Males and females in the control group have fairly similar perceptions of bachelor's and master's degrees. Yet, untreated males tend to assess bachelor's degrees somewhat worse than untreated females which can be seen as suggestive evidence that males are more likely to underestimate the returns to a bachelor's degree. This pattern is not observed for students from academic vs non-academic background.

5.3 Effects on direct enrollment intentions

In table 4 we present treatment effects on students' intentions to enroll into a postgraduate program measured 6 months after the treatment, i.e. in the first follow-up survey. The first column shows the overall effect, column 2 and 3 effects differentiated by gender, and columns 4 and 6 by students' academic background. We estimate all effects in table 4 controlling for matching variables, direct enrollment intentions and postgraduate enrollment prior to treatment (at baseline).

Table 4 shows that students direct enrollment intentions decreases by 0.041 in the overall sample (see column 1). Compared to students' in the control group, where 50% intend to directly enroll in postgraduate studies, treated students are 4 percentage points (pp) less likely to pursue a master's degree directly after graduating from their bachelor's degree. Looking at the treatment effect separately by gender shows that this reduction in intentions is driven by male students. Treated male students are 16 pp less likely to intend to enroll in postgraduate studies after the treatment (see column 3 in table 4). Considering that enrollment intentions of males in the control group are 13 pp higher than those of females, the treatment led to a reduction in the gender gap of enrollment intentions. This effect mirrors the effects found in figure 2, as treated male students increase their belief of pecuniary returns to a bachelor's degree. We also estimate the treatment effects separately by academic background and continuing generation students are also less likely to intend to directly enroll in master's programs by 5 pp.

[Table 4 about here]

All effects on direct intention to enroll in postgraduate studies are statistically insignificant at conventional levels, apart from the effects on male students. Yet, the size of the overall effect is not small.

While the results presented above refer to our preferred specification, we also provide results for four alternative specifications in table A3 in Appendix A, including a regression with pair fixed effects. Column (1) shows that the mean difference in the outcome between treatment and control group is -0.038 points, when accounting for the randomization pair dummies as suggested by Bruhn and McKenzie (2009). Controlling instead of pair dummies for the randomization matching variables directly does not change standard errors, but it decreases the treatment estimate slightly to -0.031 (column 2). To control for finite sample imbalances, we are gradually adding sets of control variables to the regression in column 3 and 4. Controlling for baseline enrollment intentions and enrollment lowers standard errors considerably and increases the effect size to about -0.041. In the last column we add further background characteristics, e.g. controlling for the finite-sample imbalance in migration background, with negligible changes in point estimates and standard errors.

5.4 Effects on actual postgraduate enrollment

In table 5 we present results on the last of our main outcomes and look at treatment effects on actual postgraduate enrollment 12 months after the information treatment.

Other than the results on postgraduate enrollment intentions, these results are likely to be very dependent on the exact timing of the intervention. Initially, when the intervention was planned, it was assumed that a large share of students were likely to complete their undergraduate degree between the first and second follow-up. Yet, as the control group mean for postgraduate enrollment at the second follow-up shows, a relatively small share of students already and directly transitioned to postgraduate studies (28.2%). The majority of students are still enrolled in an undergraduate program, e.g. because they started studying later, they are more likely to be enrolled in programs of universities of applied sciences, switched majors or need longer than the population average for other reasons. While our second follow-up already presents an interesting case to study the longer term effects of the information treatment, it is possible that additional impacts of the treatment on actual postgraduate enrollment would only show even later.

Column 1 again presents the overall effect and in columns 2 and 3 we present differences by gender and in columns 4 and 5 by academic background. Treated students are 5 pp less likely to be enrolled in postgraduate studies in the winter term 2018. This translates into a reduction in enrollment by 15% compared to the control group. While substantial in size, effects on actual enrollment shown in table 5 are not statistically significant. While the pattern from the previous findings – stronger effects on males and students from academic family background – persist, the pattern is less pronounced for this outcome variable.

[Table 5 about here]

6 Conclusion

This is the first study to present estimates for effects of information provision on beliefs about postgraduate returns, enrollment intentions, and realized enrollment. We show that students significantly updated their beliefs about postgraduate returns half a year later. Moreover, we document corresponding changes in enrollment intentions six month after treatment. Moreover, we provide suggestive evidence that the effects of information on intention materialised into differences in realised postgraduate enrollment one year after initial treatment.

These results are important as they document that information frictions exist even for students already enrolled in undergraduate degrees. Moreover, the online-treatment could be scaled up at low costs.

On the other hand, we show that only groups of students for whom we find significant effects of the treatment on beliefs also show significant reactions in our enrollment measures. This highlights a general difficulty in providing systematic information about the role of beliefs in an experimental setup where research is bound in the analysis by the ethical requirement to only present truthful information to the students. One implication is that effects of information can only be estimated for groups where significant belief updating takes place. RCTs on the role of information for belief updating and postgraduate decisions as a result have particularly high demands on sample size to shed light on heterogeneity on the role of information, which requires significant belief updating across groups.

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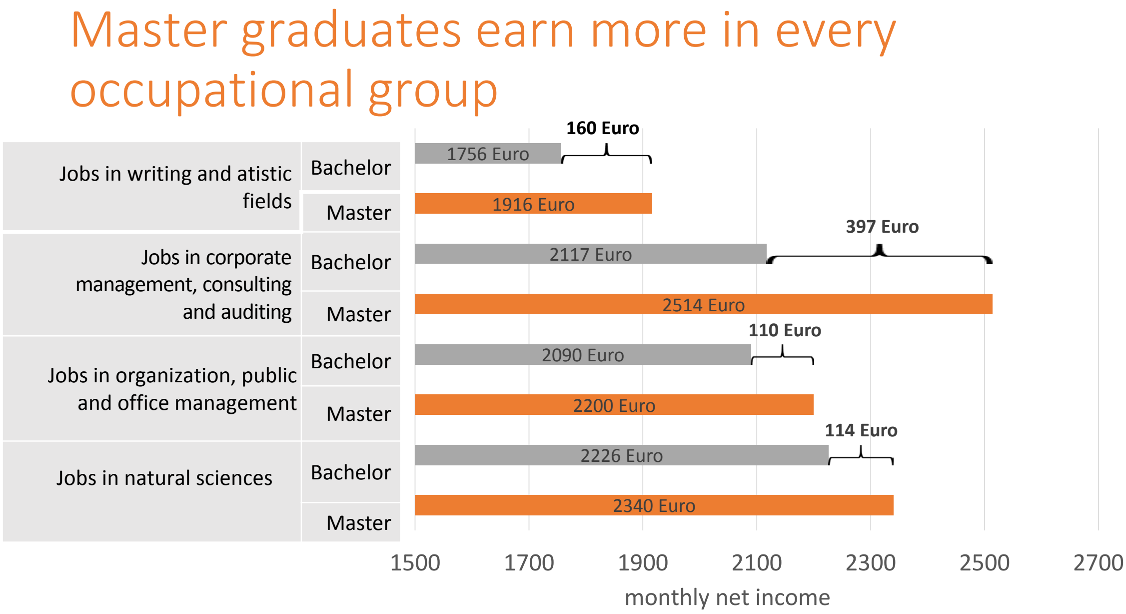
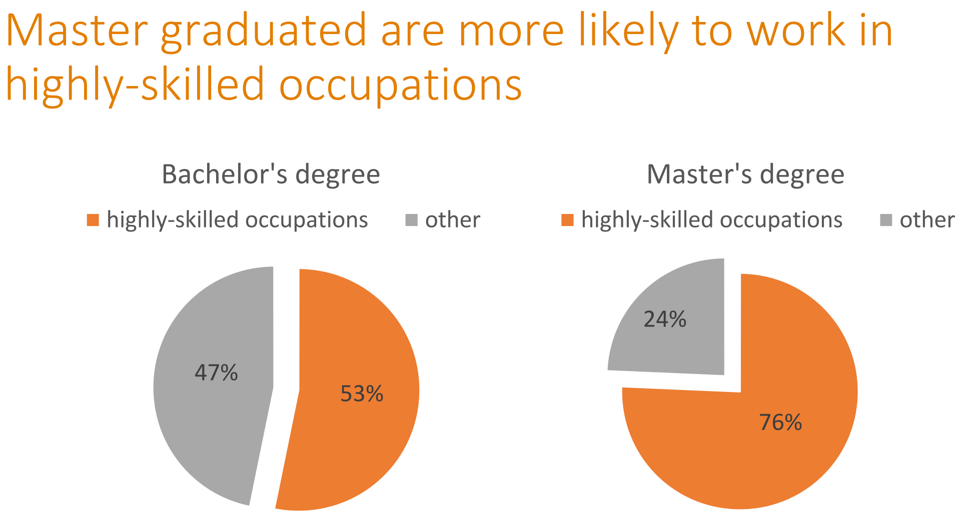
Figures

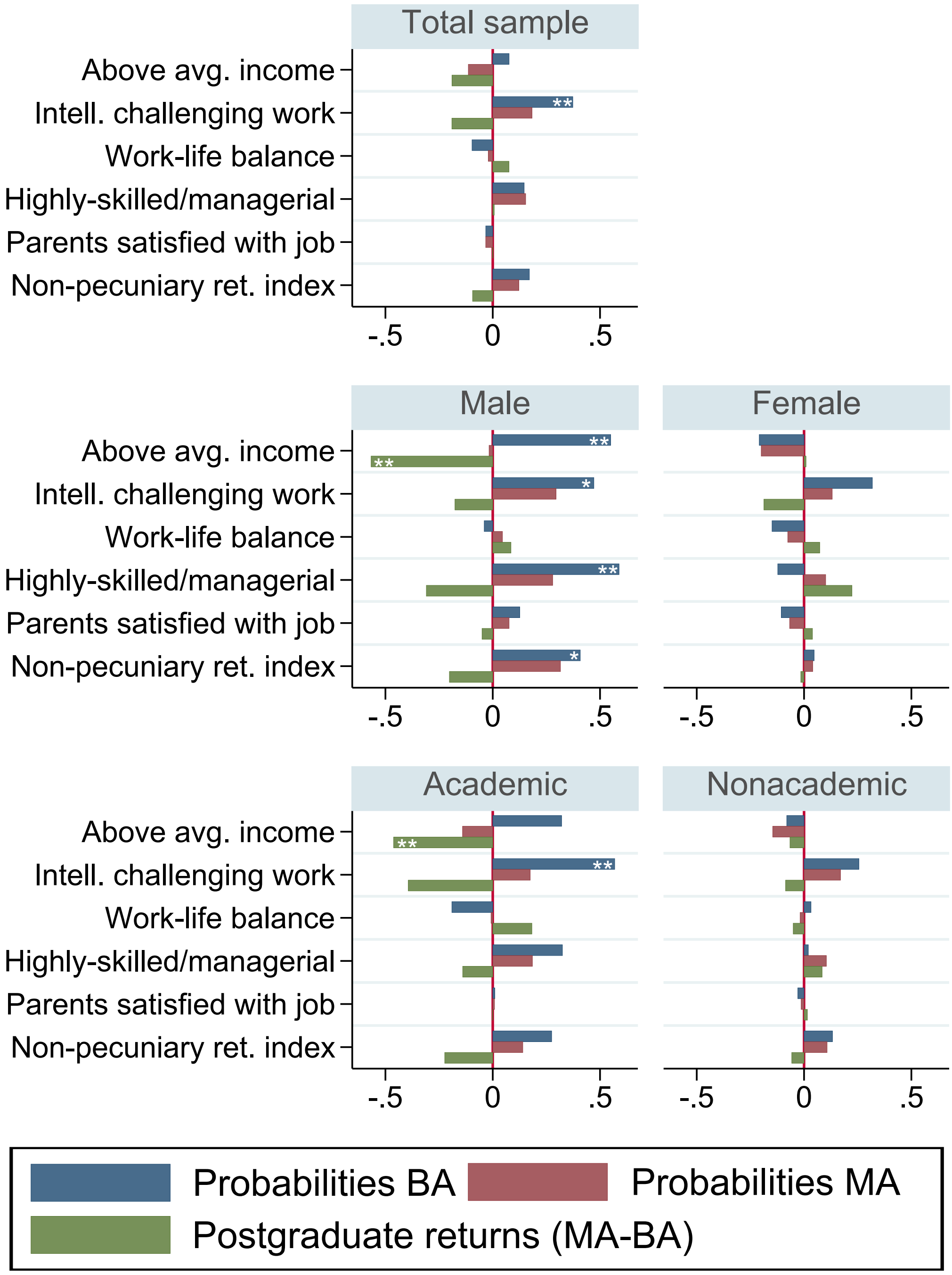
Figure 1: Example slides of online information module





Notes: This figure provides examples of the slides used in the online information module and shows two out of ten illustrative screens. The top figure shows income by field of education and degree type and the bottom figure the share of people working in a highly-skilled occupation by degree type. Both slides are translated from German. Examples of the original screens seen by students are included in appendix B.

Figure 2: Treatment effects on perceived probabilities and return



Notes: All outcome measures taken at first follow-up (6 months after treatment). Figure shows treatment effects from a regression of the outcome measure on a treatment group indicator, also controlling for matching variables (see table 2), direct and general enrollment intentions and postgraduate enrollment at baseline. Source: *Berliner-Studienberechtigten-Panel*, 2013-2018. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Tables

Table 1: Descriptive statistics

Variable	Mean	SD	N
Matching variables (pre-inquiry)			
General intention	.7708895	.420828	371
BestUp treatment group	.3045822	.4608518	371
Female	.6226415	.4853804	371
Pre-inquiry enrollment	.8652291	.3419398	371
GPA (categorical)	1.911051	.7953661	371
Baseline covariates			
Direct transition	.4609164	.4991433	371
General intention	.7681941	.4225556	371
Non-academic background	.5890411	.4926832	365
Migration background	.4673913	.4996148	368
Age (June 2018)	23.44227	.9453236	371
Academic high school	.3018868	.4596964	371
Comprehensive high school	.3692722	.4832595	371
Vocational academic high school	.328841	.4704265	371
GPA	2.326344	.5637834	371
Degree: not enrolled	.1024259	.303617	371
Degree: bachelor	.7789757	.4154971	371
Degree: *Staatsexamen/Diplom*	.0566038	.231396	371
Degree: master	.0458221	.2093815	371
Degree: art/n.a.	.0161725	.126309	371
Total semester enrolled	5.466292	1.837478	356
1. Follow-up (6 months after treatment)			
University	.4636119	.4993476	371
Applied University	.2587601	.438545	371
Lehramt (teaching)	.097035	.2964052	371
Subj.: Law, Business, Social Sci.	.309973	.4631067	371
Subj.: Natural Sci., Engineering	.2533693	.4355279	371
Subj.: Other	.1725067	.3783305	371
Above avg. Income (BA)	4.944637	1.342565	289
Above avg. Income (MA)	5.798611	1.22733	288
Intell. challenging work (BA)	4.993031	1.353558	287
Intell. challenging work (MA)	4.828671	1.394985	286
Work-life balance (BA)	4.134483	1.320264	290
Work-life balance (MA)	5.391003	1.294777	289
Highly-skilled/managerial (BA)	4.15917	1.342001	289
Highly-skilled/managerial (MA)	5.5	1.287842	288
Parents satisfied with job (BA)	5.957447	1.372699	282
Parents satisfied with job (MA)	6.298932	1.150915	281
Non-pecuniary ret. index (MA-BA)	-.0066443	1.021297	272
Direct transition	.4782609	.5003047	322
General intention	.7391304	.4397923	322
2. Follow-up (12 months after treatment)			
Postgraduate enrollment	.2559727	.4371532	293

Source: *Berliner-Studienberechtigten-Panel*, 2017-2018.

Table 2: Balance in baseline and pre-trial covariates

	Control group mean	Treatment group difference (actual)	Treatment group difference (with pair FE)	N
Matching variables (pre-inquiry)				
General intention	0.784	-0.026	-0.020	371
BestUp treatment group	0.308	-0.007	-0.029**	371
Female	0.627	-0.009	0.021	371
Pre-inquiry enrollment	0.870	-0.010	-0.022	371
GPA (categorical)	1.908	0.006	-0.008	371
Enrollment intentions (baseline)				
Direct transition	0.449	0.024	0.024	371
General intention	0.757	0.023	0.023	371
Background (baseline)				
Non-academic background	0.575	0.029	0.062	365
Migration background	0.404	0.125**	0.106**	368
Age (June 2018)	23.403	0.079	0.116	371
Academic high school	0.286	0.031	0.019	371
Comprehensive high school	0.405	-0.072	-0.071	371
Vocational academic high school	0.308	0.041	0.052	371
GPA	2.319	0.017	0.012	335
Enrollment (baseline)				
Degree: not enrolled	0.114	-0.022	-0.022	371
Degree: bachelor	0.757	0.044	0.045	371
Degree: *Staatsexamen/Diplom*	0.054	0.005	0.005	371
Degree: master	0.054	-0.016	-0.017	371
Degree: art/n.a.	0.022	-0.011	-0.011	371
Total semester enrolled	5.474	-0.016	0.038	356

Notes: Treatment group differences with pair fixed effects are based on a regression of the outcome variable on a treatment dummy and pair fixed effects. Pair fixed effects are constructed by including a binary indicator variable for each complete randomization pair and one binary indicator variable for observations from incomplete pairs. F-tests for joint orthogonality are based on a regression of a treatment dummy on all balancing variables with missing values set to the control group mean. Source: *Berliner-Studienberechtigten-Panel*, 2017-2018. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Associations of enrollment intentions and perceived return

	Dependent variable: direct transition intention						
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
Pecuniary aspect:							
Above avg. income	0.10***					0.08**	0.07**
	(0.03)					(0.04)	(0.03)
Non-pecuniary aspects:							
Intell. challenging work		0.07**				0.06*	
		(0.03)				(0.03)	
Work-life balance			0.07*			0.07**	
			(0.04)			(0.04)	
Highly-skilled/managerial				0.06**		0.02	
				(0.03)		(0.03)	
Parents satisfied with job					0.01	-0.07	
					(0.04)	(0.05)	
Non-pecuniary ret. index							0.09***
							(0.03)
Others:							
Age (June 2018)	-0.05	-0.05	-0.06	-0.03	-0.05	-0.05	-0.05
	(0.04)	(0.05)	(0.05)	(0.04)	(0.05)	(0.04)	(0.04)
Female	-0.23***	-0.24***	-0.20**	-0.22**	-0.22**	-0.23***	-0.22***
	(0.08)	(0.08)	(0.09)	(0.08)	(0.09)	(0.08)	(0.08)
Migration background	0.17**	0.18**	0.15*	0.17**	0.16*	0.19**	0.18**
	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)
Non-academic background	-0.08	-0.06	-0.08	-0.09	-0.08	-0.07	-0.07
	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)
F-test (pvalue): Joint significance of return measures						0.00	
Constant	1.61	1.65	1.95*	1.34	1.75	1.59	1.60
	(0.99)	(1.08)	(1.11)	(1.03)	(1.09)	(1.03)	(0.98)
N	147	147	147	147	147	147	147

Notes: This table shows the effects of step-wise regressions for direct transition intentions on perceived pecuniary and non-pecuniary returns. Dependent variable and return measures are from the first follow-up survey (6 months after treatment). Regressions are based on control group only. Robust standard errors in parentheses. Source: *Berliner-Studienberechtigten-Panel*, 2017-2018. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Treatment effects on direct postgraduate enrollment intentions

	Total sample	**Female**	**Male**	**Non-academic**	**Academic**
Direct transition intentions (6 months after treatment)					
Treatment effect	-0.041	0.029	-0.169**	-0.002	-0.052
	(0.042)	(0.050)	(0.077)	(0.059)	(0.061)
Control group mean	*0.497*	*0.432*	*0.560*	*0.455*	*0.523*
N	322	206	116	189	130

Notes: All regressions control for matching variables (see table 2) and direct and general enrollment intentions and postgraduate enrollment at baseline. We deal with missing information in control variables by setting these variables to a constant value and including a binary variable indicating missing values in control variables. Robust standard errors in parentheses. Source: *Berliner-Studienberechtigten-Panel*, 2013-2018. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

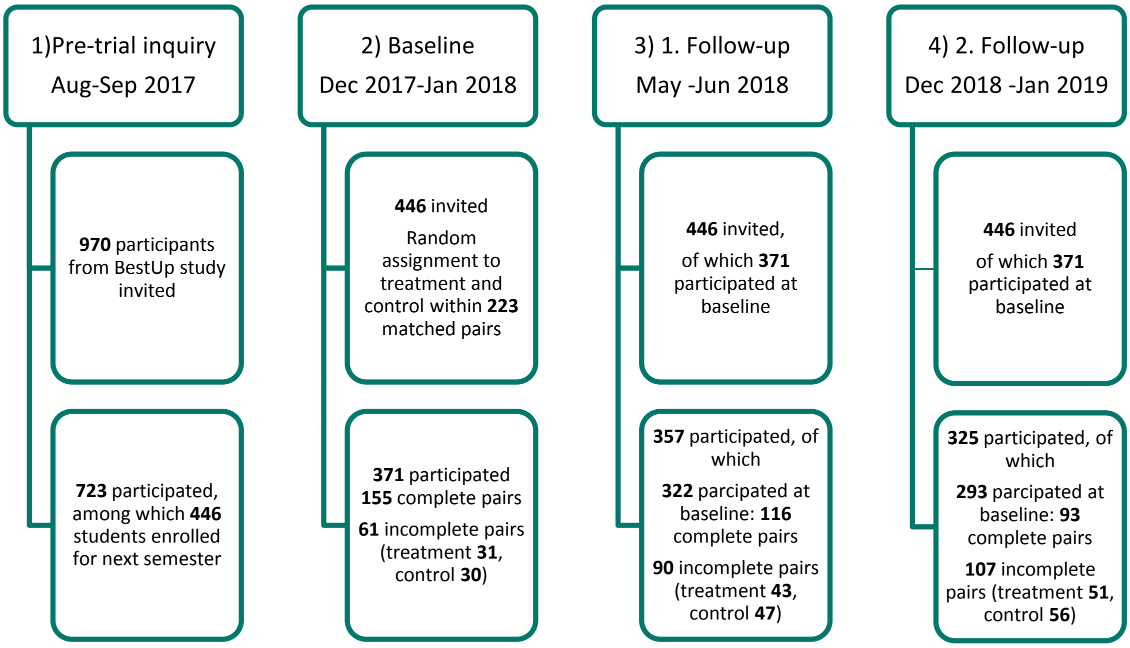
Table 5: Treatment effects on postgraduate enrollment

	Total sample	**Female**	**Male**	**Non-academic**	**Academic**
Postgraduate enrollment (12 months after treatment)					
Treatment effect	-0.043	-0.041	-0.066	-0.026	-0.043
	(0.042)	(0.052)	(0.074)	(0.057)	(0.074)
Control group mean	*0.282*	*0.255*	*0.257*	*0.256*	*0.259*
N	293	192	101	176	112

Notes: All regressions control for matching variables (see table 2) and direct and general enrollment intentions and postgraduate enrollment at baseline. We deal with missing information in control variables by setting these variables to a constant value and including a binary variable indicating missing values in control variables. Robust standard errors in parentheses. Source: *Berliner-Studienberechtigten-Panel*, 2017-2019. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

A Supplementary Material

Figure A1: Trial profile and timeline



Notes: This figure presents participation rates in each survey of the *Berliner-Studienberechtigten-Panel* from 2017-2019. We report participation rates related to the utilized randomization method pair-wise matching. We also report the number of participants per wave and in relation to baseline participation. For example, in the second follow-up 325 persons participated and of those 293 also participated in the baseline survey. The latter equals a response rate of 79% compared to baseline and 91% compared to the imminent wave (N=322). In contrast to the baseline and follow-up surveys, students did not receive any incentives to participate in the pre-trial inquiry in 2017.

Table A1: Comparison of baseline sample with NEPS

	Best Up Baseline (2017/2018)		**NEPS SC 4**		**NEPS SC 5**	
Variable	Mean	N	Mean	N	Mean	N
Direct transition	0.46	371			0.44	1129
General intention	0.77	371	0.75	631	0.82	1129
Non-academic background	0.59	365	0.50	2360	0.63	1129
Migration background	0.47	368	0.22	2360	0.19	1129
Age (June 2018)	23.44	371	22.42	2360	24.21	1129
Academic high school	0.30	371	0.78	2360	0.75	1129
Comprehensive high school	0.37	371	0.07	2360	0.03	1129
Vocational academic high school	0.33	371	0.14	2360	0.13	1129
GPA	2.33	371	2.20	2360	2.24	1129
Degree: not enrolled	0.10	371	0.00	2360	0	1129
Degree: bachelor	0.78	371	0.84	2360	0.83	1129
Degree: *Staatsexamen/Diplom*	0.06	371	0.16	2360	0.06	1129
Degree: master	0.05	371	0.00	2360	0.11	1129
Degree: art/n.a.	0.02	371	0.00	2360	0	1129
Total semester enrolled	5.47	356	3.23	2360		

Notes: Source: This table uses data from the *Berliner-Studienberechtigten-Panel*, 2017-2018 and the National Educational Panel Study (NEPS): Starting Cohort Grade 9, doi:10.5157/NEPS:SC4:10.0.0 and Starting Cohort First-Year Students, doi:10.5157/NEPS:SC5:12.0.0, own calculations. From 2008 to 2013, NEPS data was collected as part of the Framework Program for the Promotion of Empirical Educational Research funded by the German Federal Ministry of Education and Research (BMBF). As of 2014, NEPS is carried out by the Leibniz Institute for Educational Trajectories (LIfBi) at the University of Bamberg in cooperation with a nationwide network.

The starting cohort 4 of the NEPS follows the educational pathway of students in grade 9 and higher into either university or vocational training. This sample consists of students who graduated from high school in 2014 or 2015. The variable "General intention" to start a master uses a restricted sample, namely that of bachelor students only, hence the smaller sample size. The starting cohort 5 comprises first-year students who started studying at a higher education institution in 2010. This sample was restricted to students who graduated from high school in 2010 and who could have finished their bachelor in 2013 at the earliest.

Table A2: Attrition of students to 1st and 2nd follow-up

	1. Follow-up		2. Follow-up	
T: Treatment group	0.026	0.085	0.031	0.137
Direct transition intentions		-0.061		-0.067
BestUp treatment group		0.013		-0.028
Female		-0.050		-0.143**
GPA		0.061		0.048
Degree: not enrolled		-0.059		0.080
T*Direct transition intentions		0.034		0.041
T*BestUp treatment group		0.060		-0.015
T*Female		-0.033		0.050
T*GPA		-0.038		-0.069
T*Degree: not enrolled		0.156		0.122
Control group mean attrition	0.132		0.210	
Joint F-tests (p-values):				
Baseline controls (without interaction)		0.373		0.133
T interactions with baseline controls		0.737		0.881
N	371	371	371	371

Notes: This table shows OLS regressions with attrition at first and second follow-up as dependent variable using all baseline participants. Missing baseline control variables are replaced by the control group mean. Robust standard errors are used. Source: *Berliner-Studienberechtigten-Panel*, 2013-2018. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Treatment effects on direct postgraduate enrollment intentions

	(1)	(2)	(3)	(4)
Direct transition intentions (6 months after treatment)				
Treatment effect	-0.038	-0.031	-0.041	-0.037
	(0.053)	(0.053)	(0.042)	(0.042)
Control group mean		*0.497*		
N	322	322	322	322
Pair fixed effects	Yes	No	No	No
Controls: Matching variables	No	Yes	Yes	Yes
Controls: Enrolment intentions	No	No	Yes	Yes
Controls: Background	No	No	No	Yes

Notes: This table shows regressions using pair fixed effects and controlling for matching variables. In models (3) and (4) further covariates are included. Regressions with pair fixed effects include a binary indicator variable for each complete randomization pair and one binary indicator variable for observations from incomplete pairs. See table 2 for list of matching variables which are used as controls. Enrollment intentions are controlled for by controlling for direct and general enrollment intentions and postgraduate enrollment at baseline. Background control variables are migration background, non-academic family background, gender, age, high school type and GPA. We deal with missing information in control variables by setting these variables to a constant value and including a binary variable indicating missing values in control variables. Robust standard errors in parentheses. Source: *Berliner-Studienberechtigten-Panel*, 2013-2018. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Treatment effects on perceived probabilities and return

	Total sample	**Female**	**Male**	**Non-academic**	**Academic**
How do you rate the probability...					
(1 very unlikely - ... - 7 very likely)					
...to earn an above-average income with...					
bachelor's degree?	0.08	-0.21	0.55**	-0.08	0.32
master's degree?	-0.11	-0.20	-0.02	-0.15	-0.14
Difference	-0.19	0.01	-0.57**	-0.07	-0.46**
...to do intellectually challenging work with...					
bachelor's degree?	0.37**	0.32	0.47*	0.26	0.57**
master's degree?	0.18	0.13	0.30	0.17	0.17
Difference	-0.19	-0.19	-0.18	-0.09	-0.39
...to be able to combine work and family life with...					
bachelor's degree?	-0.10	-0.15	-0.04	0.03	-0.19
master's degree?	-0.02	-0.08	0.05	-0.02	-0.01
Difference	0.08	0.07	0.08	-0.05	0.18
...to work in a highly-skilled job or with managerial responsibility with...					
bachelor's degree?	0.15	-0.12	0.59**	0.02	0.32
master's degree?	0.15	0.10	0.28	0.10	0.18
Difference	0.01	0.22	-0.31	0.08	-0.14
...that parents will be satisfied with your job with...					
bachelor's degree?	-0.03	-0.11	0.13	-0.03	0.01
master's degree?	-0.03	-0.07	0.08	-0.01	0.01
Difference	-0.00	0.04	-0.05	0.02	-0.00
Non-pecuniary return index					
bachelor's degree?	0.17	0.05	0.41*	0.13	0.27
master's degree?	0.12	0.04	0.32	0.11	0.14
Difference	-0.09	-0.02	-0.20	-0.06	-0.22
N	322	206	116	189	130

Notes: All outcome measures taken from at first follow-up (6 months after treatment). Table shows treatment effects from a regression of the outcome measure on a treatment group indicator, also controlling for matching variables (see table 2), direct and general enrollment intentions and postgraduate enrollment at baseline. Source: *Berliner-Studienberechtigten-Panel*, 2013-2018. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Control group levels of perceived returns by degree

	Total sample	**Female**	**Male**	**Non-academic**	**Academic**
How do you rate the probability...					
(1 very unlikely - ... - 7 very likely)					
...to earn an above-average income with...					
bachelor's degree?	4.77	4.80	4.72	4.79	4.72
master's degree?	5.71	5.77	5.60	5.73	5.73
Difference	0.94	0.97	0.89	0.94	1.02
...to do intellectually challenging work with...					
bachelor's degree?	5.04	5.17	4.83	4.89	5.22
master's degree?	4.85	4.90	4.75	4.70	5.03
Difference	-0.20	-0.27	-0.08	-0.19	-0.19
...to be able to combine work and family life with...					
bachelor's degree?	4.10	4.12	4.07	4.04	4.15
master's degree?	5.45	5.46	5.43	5.40	5.52
Difference	1.34	1.34	1.35	1.37	1.38
...to work in a highly-skilled job or with managerial responsibility with...					
bachelor's degree?	4.08	4.22	3.85	4.06	4.10
master's degree?	5.43	5.52	5.28	5.45	5.39
Difference	1.34	1.30	1.43	1.39	1.30
...that parents will be satisfied with your job with...					
bachelor's degree?	5.97	6.11	5.73	5.95	6.02
master's degree?	6.31	6.40	6.16	6.33	6.29
Difference	0.34	0.29	0.43	0.38	0.27
Non-pecuniary return index					
bachelor's degree?	-0.08	0.04	-0.29	-0.12	-0.04
master's degree?	-0.06	0.05	-0.26	-0.09	-0.00
Difference	0.04	-0.01	0.12	0.06	0.05
N	146	92	54	83	61

Notes: All measures taken at first follow-up (6 months after treatment). Source: *Berliner-Studienberechtigten-Panel*, 2018.

B Material of the information treatment

Figure B1: Example of programmed screen (information on income by type of occupation)

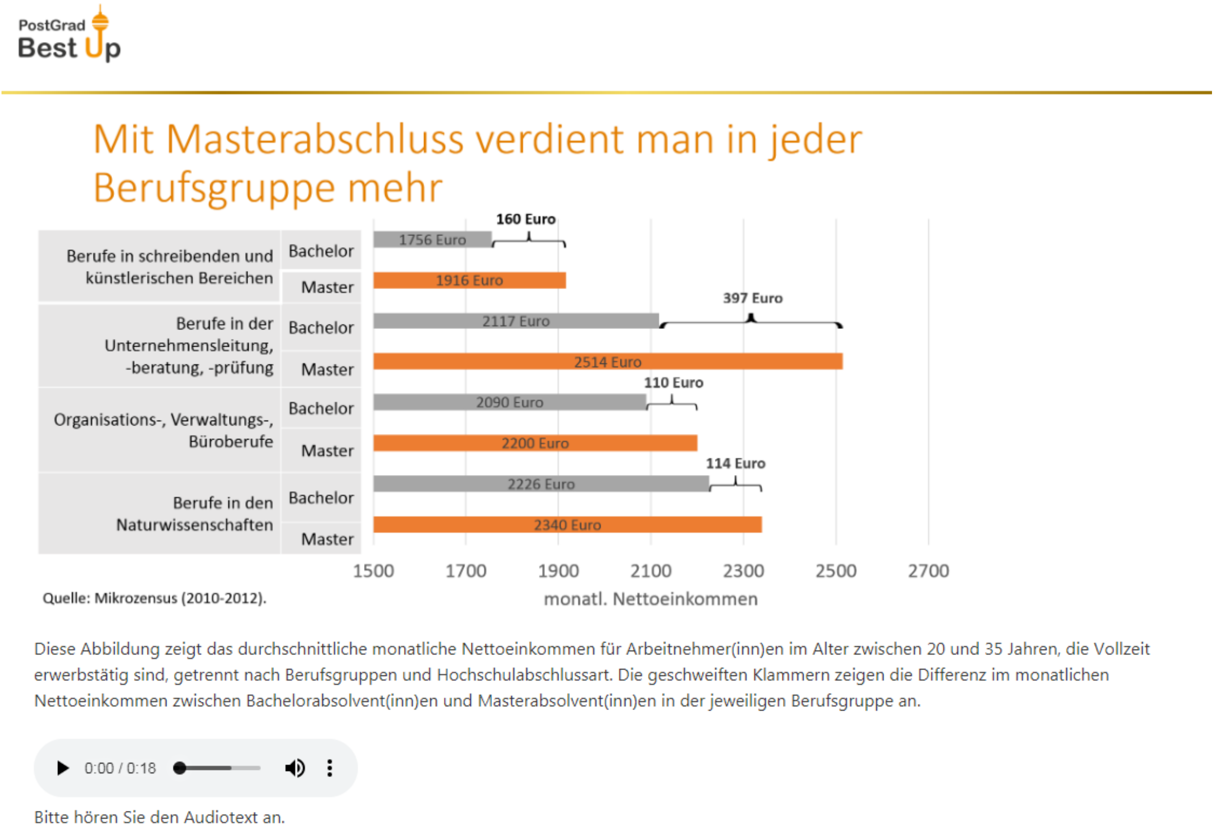


Figure B2: Example of programmed screen (comprehension question on income by type of occupation)

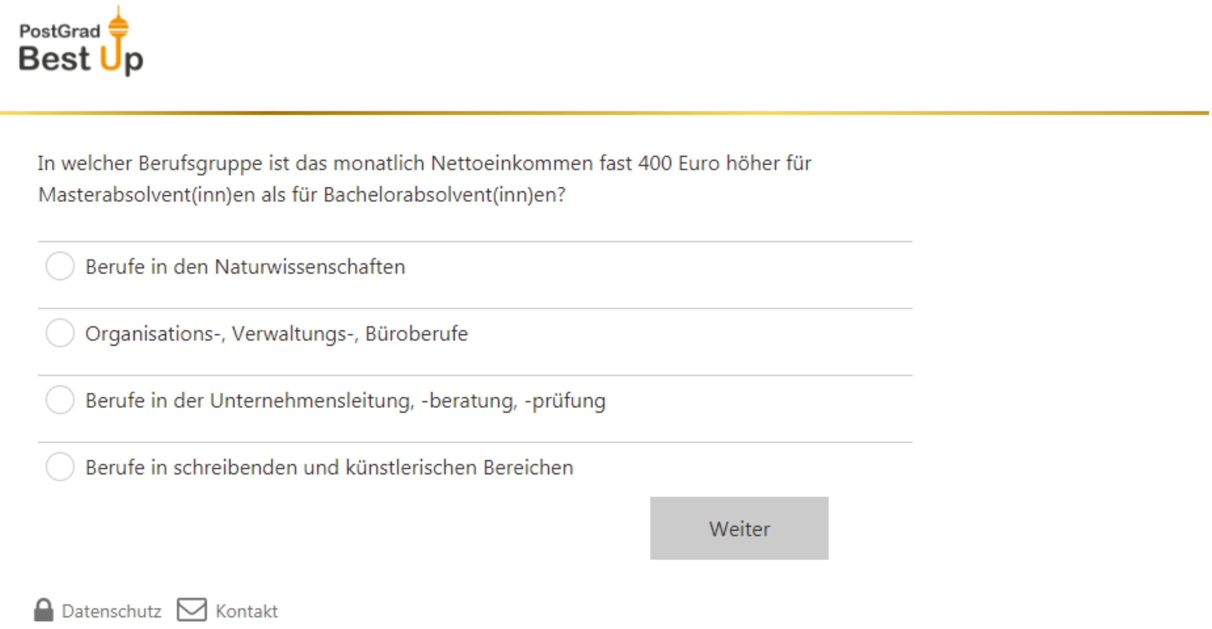


Figure B3: Example of programmed screen (information about financing of postgraduate studies)