Correcting Beliefs about Job Opportunities and Wages: A Field Experiment on Education Choices

Bart K. de Koning[1], Robert Dur[2,4,5], and Didier Fouarge[3,4,6]

[1] *Cornell University*
[2] *Erasmus University Rotterdam*
[3] *Maastricht University*
[4] *IZA*
[5] *CESifo*
[6] *Netspar*

October 3, 2022

Preliminary version

Click here for the most recent version of the paper

Abstract

We run a large-scale field experiment in which we provide information to students at randomly selected schools about the job opportunities and hourly wages of a small set of occupations they are interested in. The experiment takes place on an online career guidance counseling platform that is widely used in the Netherlands, and involves 28,267 pre-vocational secondary education students in 243 schools over a period of 2 years. We find that the information improves the accuracy of students' beliefs, both in the short run (for job opportunities and hourly wages) and in the long run (for job opportunities only). Students who receive the information also tend to change their favorite occupation towards an occupation with better labor market prospects. Last, and most importantly, they select secondary school specializations related to occupations with better labor market prospects and choose post-secondary education programs with higher expected earnings.

Keywords: Education choice, labor market information, field experiment.
JEL codes: C93, D83, I26, J24
AEA Social Science Registry Entry: AEARCTR-0003220

Corresponding author: Bart K. de Koning (dekoning@cornell.edu). This project received funding from the Dutch Ministry of Education, Culture, and Science. Fouarge further acknowledges the financial support of NRO, UWV, Randstad, S-BB (grant: 405-17-900). This study is registered in the AEA RCT Registry and the digital object identifier (DOI) is: https://doi.org/10.1257/rct.3220

1 Introduction

Each year, millions of teenagers around the world face a choice that has far-reaching consequences, both for themselves and for society: the choice of post-secondary education program. This choice is important for themselves, as the program from which they earn a degree is an important determinant of future labor market outcomes (see e.g. Bleemer and Mehta, 2022; Ketel, Leuven, Oosterbeek, and van der Klaauw, 2016; Kirkeboen, Leuven, and Mogstad, 2016). It is also important for society, as it affects future shortages and excess supply of labor in important occupations.

Despite its huge importance, students often decide on their field of study without having accurate information about the labor market prospects of different programs (Baker, Bettinger, Jacob, & Marinescu, 2018; Conlon, 2019; Hastings, Neilson, & Zimmerman, 2015; Hastings, Neilson, Ramirez, & Zimmerman, 2016; Pekkala Kerr, Pekkarinen, Sarvimäki, & Uusitalo, 2015) and careers (Arcidiacono, Hotz, & Kang, 2012; Betts, 1996). As a result, many teenagers end up choosing programs that have a bleak outlook, both in terms of job opportunities and wages.

To help students make better choices, several large-scale field experiments have tested whether providing information to students about labor market prospects makes a meaningful difference in students' educational choices. The results of these experiments tend to be sobering. Even though students' choices move in the direction of education programs with better labor market prospects, the size of these effects tends to be limited, not seldomly statistically indistinguishable from zero (see e.g., Bonilla-Mejía, Bottan, and Ham, 2019; Conlon, 2019; Hastings et al., 2015; Pekkala Kerr et al., 2015). A possible reason might be that the information provided in earlier experiments is too coarse: interventions commonly provide information about the labor market returns to enrolling in university, or about different majors rather than about occupations. While majors are an important determinant of future earnings (Altonji, Arcidiacono, & Maurel, 2016), subsequent occupational choices explain a large part of the difference in earnings between majors (Altonji, Blom, & Meghir,

2012) and students seem well aware of that (Arcidiacono et al., 2012). Hence, a promising next step in this literature is to provide more fine-grained information to students about the labor market prospects of occupations.

In this paper, we report the results of a field experiment in which we provide a random selection of students with personally targeted information about the labor market prospects of a small set of occupations they are interested in. To our knowledge, we are the first to do so. We study whether the information leads students to correct their beliefs about the labor market prospects of these occupations, shifts students' preferences over occupations, and influences their education choices. Our multi-year field experiment involves 28,267 students at 243 different schools for pre-vocational education in the Netherlands. The students are in grades 8 to 10[1] and generally are between 13 and 16 years old.

The field experiment takes place on an online career guidance counseling platform that is market leader among pre-vocational secondary education schools in the Netherlands. On the platform, students do numerous assignments to find out what they like, what they are good at and, ultimately, which occupations would be a good fit for them. As part of one of these assignments, students also take an extensive occupation test. This test results in a short-list of twenty (out of 353) occupations for all students student that fit their interests best according to the answers they provide. Students take part in our experiment right after this test.

Our experiment proceeds as follows. First, we ask students in which secondary-school specializations (called: "profiles") they are most interested. Next, we show students their shortlist of twenty occupations and ask them to select the five that they like most from it. We then ask them to state their beliefs about the job opportunities and hourly wages for these five occupations, and to rank them based on how much they would like to work in them. Subsequently, we provide students of randomly selected schools with information about the job opportunities and, for a random subset of these schools, hourly wages of the selected

[1] The second to fourth year of pre-vocational secondary education in the Netherlands.

occupations. Students at the remaining schools do not receive any information and form our control group. To learn whether it matters who provides the information, we mention to some students that the information is provided by a labor market research institute, whereas we mention to others that a specific researcher from this institute – who is either male or female and experienced or inexperienced – provides the information. The identity of the 'sender' is randomized within the treatment group.

Next, students in both the treatment and the control group watch a video, get the opportunity to update their stated beliefs, re-rank their preferred occupations and update their interest in the different profiles. These answers are our first set of outcome measures. In addition to these data, we obtain (i) post-experimental survey data (up to one and a half year later) on the above mentioned beliefs and preferences, as well as their post-secondary education choices, and (ii) administrative data at the school level on students' profile choices. All our analyses follow the design we registered prior to the start of the experiment[2], except where indicated.

In line with the earlier studies cited above, our results show that students have highly inaccurate beliefs about the job opportunities and hourly wages of the occupations that they like. They tend to overestimate both. Interestingly, the interest of a student in an occupation is strongly positively correlated with the student's expectations about the occupation's job opportunities and hourly wages. This suggests that students' beliefs about occupations' labor market prospects play a role in students' occupational aspirations, underlining the potential importance of providing accurate information about them.

Our information intervention is effective in correcting beliefs. In the short run, treated students overestimate the job opportunities and hourly wages to a smaller degree, make smaller absolute errors, and are more likely to hold correct beliefs. The improved accuracy is mostly driven by students correcting overestimations. Our post-experimental survey data show that these effects partly persist: those who received the information in their final school

[2] AEA Social Science Registry Entry: AEARCTR-0003220.

year have more accurate expectations about the job opportunities up to seven months later.

We also find evidence that the treatment increases the likelihood that students change their favorite occupation. If students do so, they tend to substitute the initial occupation for one with better job opportunities or hourly wages. We do not find evidence that this ranking persists in the survey fielded after the experiment. However, this may be driven by selection into the survey. The sample of surveyed students differed from the full sample in the experiment in that the former was less likely to change their favorite occupation for one with better prospects during the experiment than the latter.

In contrast to our predictions, we find no evidence that the treatment impacts students' intended choice of profile right after the experiment. However, administrative data at the school level shows that in treated schools, students select profiles associated with occupations with better job opportunities and higher earnings. A possible interpretation of these two findings is that treated students need some time to make up their mind and discuss the obtained information with their parents or peers before they actually revise their profile choice. Survey data collected from students graduating secondary education shows that students who received information about hourly wages are more likely to choose a study program with higher earning prospects. This further shows that the treatment is indeed effective in altering education choices.

The identity of the sender of the information that is mentioned in the intervention — the labor market research institute or a researcher from this institute, either senior or junior, either female or male — appears inconsequential for the subsequent beliefs or preference ranking of occupations.

Our study contributes to a growing body of literature on the role of labor market expectations in education choices. Studies have invariably found that students have highly noisy beliefs about the labor market returns of different study programs (Baker et al., 2018; Hastings et al., 2015, 2016; Pekkala Kerr et al., 2015; Conlon, 2019) and earnings in different careers (Arcidiacono et al., 2012; Betts, 1996). Students who are more concerned with the labor market prospects of programs, however, are less likely to overestimate these prospects (Hastings et al., 2016). The differences in concerns about these prospects are large between men and women (Wiswall & Zafar, 2017). Men tend to care more about pecuniary outcomes, whereas women care more about job security and flexibility. Similarly, we find in our data that male students select occupations with better job opportunities and higher hourly wages. However, they are also more likely to overestimate these and make larger absolute errors. A number of studies further document that students from low socioeconomic status backgrounds have less accurate expectations (Baker et al., 2018; Hastings et al., 2015, 2016). This could be explained by their parents having less information (Bleemer & Zafar, 2018; Lergetporer, Werner, & Woessmann, 2018), thus making the process of acquiring this information more costly. We indeed confirm that students from higher socioeconomic status neighborhoods make smaller absolute errors and are more likely to be correct about the hourly wages of the occupations they select, but this does not hold for the job opportunities. Lastly, students have been shown to be uninformed about programs with good labor market prospects outside of their field of interest (Hastings et al., 2015).

A number of field-experimental studies have tested the effects of interventions aimed at improving students' knowledge about the returns to – and costs of – education. Evidence from the Dominican Republic shows that providing students with information about the returns to attending secondary school increases enrolment (Jensen, 2010). For the general secondary education student population in industrialized countries, providing information about the returns to further education does not seem to influence actual enrollment (Pekkala Kerr et al., 2015; Bonilla-Mejía et al., 2019). There is some evidence that it does increase intended enrollment, particularly for students from low socioeconomic status backgrounds (Oreopoulos & Dunn, 2013; McGuigan, McNally, & Wyness, 2016; Peter & Zambre, 2017). Most closely related to our study are a number of studies that focus on providing information about the returns to specific study programs or institutions. These generally find stronger effects. Some studies show that, after being provided with such information, students are more likely to enroll in more prestigious institutions (Bonilla-Mejía et al., 2019) and higher-return study programs (Hastings et al., 2015). It has also been documented that simply receiving information about a study program makes students more likely to enroll in them (Conlon, 2019).

Despite the differences in context and outcome measures used, it is worthwhile to consider how the effectiveness of our intervention compares to these closely related information interventions. Bonilla-Mejía et al. (2019) find no effect of information provision on student enrollment in higher education. They do find that students who receive information are 0.5 percentage points more likely to enroll in highly selective colleges. Despite high returns to attending a selective college (Hastings, Neilson, & Zimmerman, 2013), the overall impact of Bonilla-Mejía et al.'s (2019) intervention on expected earnings is likely low, since the impact only applies to a small group of students. Conlon (2019) finds no impact of the implemented intervention on expected earnings of chosen majors. Our results are most easily compared to Hastings et al. (2015). Their main result shows that their intervention increases the expected earnings of chosen degrees by 1.4% of the control group mean. Our survey results show that students who received wage information choose study programs with wages that are 2.5% higher than the control group mean. In addition, we find that, long before their study program choice, students in treated schools select profiles with weighted job opportunities and hourly wages that are 1.5% higher and 0.3% higher than the control group mean, respectively. This does not take into account any changes in occupational preferences within profiles. In short, our intervention looks to be effective compared to similar studies and shows that information about study programs or occupations can have an impact long before students have to make a decision on their degree program or major.

Our study further draws on, and contributes to, recent work on role models. Porter and Serra (2020) show that female students are more likely to enroll in economics classes when they get to listen to a female role model talk about her experiences in university, as well as her career path and achievements (Porter & Serra, 2020). Moreover, Del Carpio and Guadalupe (2021) ran an experiment studying female enrollment in a 5-month software coding program. They show that removing a 'success story' of a female participant from the information page decreases enrollment by four percentage points. Our inclusion of the different 'information senders' provides a further look into how the characteristics of a person providing information affects the degree to which it is used.

Our main contribution is that, to the best of our knowledge, we are the first to present students with information on the labor market prospects of occupations rather than specific study programs. Information about occupations may be more relevant as the true returns to education strongly depend on occupational sorting after graduation. Our setting provides a unique opportunity to do so, as vocational education programs are strongly tied to occupations. The occupations we provide information about are those that students are most interested in, which maximizes the relevance of the information. Furthermore, we do not just treat students who are close to post-secondary education, but also those who still have to decide on their specialization in secondary education. This allows us to analyze what the impact of our information treatment is at different stages of students' educational careers. Lastly, with the exception of Hastings et al. (2015), all field-experimental studies we know of required students to attend a presentation or visit a website they otherwise would not have. Our intervention is designed within an established career guidance platform actually used as part of students' curriculum in school. This provides for a more natural environment. The intervention is low-cost and easy to replicate. Based on our field-experimental results, the company that we collaborate with intends to include our intervention on the platform in the near future.

The rest of this paper is structured as follows. Section 2 explains the institutional context: the Dutch education system and career guidance practice. Section 3 shows how we recruited schools and randomized them into treatment groups. Section 4 describes the experimental design. Section 5 lays out the data specifications and Section 6 presents the results. Section 7 concludes.

2 Institutional Context

In this experiment, we focus on students enrolled in pre-vocational secondary education in the Netherlands. Pre-vocational secondary education is one of the three main tracks of Dutch secondary education[3]. As the name suggests, it is vocationally-oriented and offers a broad range of subjects. It is also the largest track in terms of student numbers: in the 2017/2018 school year, about 53% of Dutch children in secondary school attended pre-vocational secondary education (Dutch Inspectorate of Education, 2020).

The pre-vocational secondary education program takes four years to complete (Nuffic, 2019). At the end of the second year, students choose a ‘learning pathway’ and profile. Pre-vocational secondary education is divided into four ‘learning pathways’: the basic vocational program, advanced vocational program, combined program, and theoretical program (Nuffic, 2019). In the theoretical program, students mostly take general subjects. The combined program drops one general subject in favor of four hours of vocational training, but is otherwise the same. In the basic and advanced vocational programs, students receive approximately 12 hours of vocational training instead of general subjects. General subjects are taught at a lower level compared to the combined and theoretical programs, with the level at the advanced vocational program being slightly above that of the basic vocational program. Within the learning pathways, students also choose a profile[4]. This profile determines what subjects are taught (Government of the Netherlands, n.d.-a). Both the learning pathway and profile a student chooses have important consequences for the opportunities for further education at the time the student graduates, on which we expand below. At the end of the fourth year, students have to decide how to continue their education. Dutch law dictates that students cannot leave education until they are either eighteen years of age or have a

[3]Pre-vocational secondary education is known as ‘vmbo’ in Dutch. The two other tracks are higher general secondary education (havo) and pre-university education (vwo).

[4]For the basic vocational, advanced vocational, and mixed program there are ten available profiles: 1. Building, housing and interiors, 2. Engineering, fitting out and energy, 3. Transport and mobility, 4. Media, design and IT, 5. Maritime and technology, 6. Care and welfare, 7. Business and commerce, 8. Catering, baking and leisure, 9. Animals, plants and land and 10. Services and products. For the theoretical program, there are four options: 1. Care and welfare, 2. Engineering and technology, 3. Business and 4. Agriculture

‘starting qualification’ (i.e., an intermediate vocational education or senior general secondary education degree).

As students usually graduate from their initial pre-vocational education program at age sixteen, entering the labor force directly is generally not an option. This leaves them with essentially two options: move on to post-secondary intermediate vocational education or enroll in a different (sub)track of secondary education. Graduates from all learning pathways are eligible to enroll in intermediate vocational education. The exact level at which graduates can enroll depends on the chosen learning pathway. Graduates from the basic vocational program can enroll in qualification level 2 of intermediate vocational education only. Graduates from the other three programs can enroll in levels 2, 3 and 4 (Government of the Netherlands, n.d.-b). Programs in intermediate vocational education generally train students for a specific occupation.

To aid students in navigating these choices, schools are required to provided career guidance counseling. To structure their career guidance counseling efforts, most schools make use of online platforms. For this experiment, we partner with a company called Qompas, which provides an online platform to schools. The platform consists of a number of assignments that help students get to know more about themselves and the choices they will have to make. While students can access the platform at any time, the idea is that schools use Qompas during their career guidance counseling classes at set times during the week. All assignments the students complete are saved and stored in their personal file, which they are supposed to review periodically. We implement the experiment described in this paper within one of Qompas’s assignments: the occupation assignment. While the Qompas system has a suggested order for doing the different assignments, schools ultimately decide in which year students do which. Schools usually have students do the occupation assignment in the second, third or fourth year of education. We expand on the assignment in Section 4.

3 Recruitment and Randomization

We recruited schools to participate in the experiment directly through the Qompas system. At the time of recruitment, 300 schools for pre-vocational secondary education were registered in the Qompas system, which comprises about 15% of all schools of this type in the Netherlands. Of these schools, thirteen were not eligible to participate in the experiment because of missing information.

The 287 remaining schools were informed about the experiment through a system message as well as an email. Qompas informed schools that they, together with a research institute of Maastricht University, were asked by the Ministry of Education, Culture, and Science to do research into the effects of labor market information on the choices of pre-vocational secondary education students. Qompas further explained to schools that the research would be conducted by way of an experiment within the Qompas's career guidance counseling platform. Schools also received contact details of the person responsible for the experiment at Qompas in case they had any questions, complaints or did not want to participate. Appendix A provides the original version as well as an English translation of the message. Only a single school indicated that it did indeed not want to be a part of the experiment. This left us with 286 schools.

To randomize schools, we employed a stratified procedure at the school level. The reason for randomizing at the school level instead of at the student level is twofold. First, it reduces the chance of there being spillover effects between students who receive different treatments. Second, we expected that schools would be less willing to participate if some of their students were to be provided with information, whereas others were not.

We randomized schools into three main groups of approximately equal size: a control group, a treatment group that receives information about just job opportunities, and a treatment group that receives information about both job opportunities and hourly wages. The latter two groups were randomly assigned to receive information from either a research institute or a specific researcher from this institute. Columns 2 and 3 of Table 1 display the exact division of schools assigned over the different groups. We explain the difference between the treatment groups in further detail in Section 4.3

[Table 1 about here.]

We stratified schools on the basis of three characteristics: the number of broad profiles offered in the school, the number of students who completed the occupation test in the year before the experiment, and the quality of life indicator of neighborhoods the students come from. For the available profiles, we relied on data from Qompas. Qompas also registered the number of students who completed the occupation test in the previous year. However, data was not available for all schools. If no data was available, we predicted the number using the number of newly registered students in the Qompas system and the total number of students in the school itself[5]. If data on one of the two was not available, we predicted the number using just the available measure. For the quality of life in neighborhoods students came from, we relied on the quality of life indicator developed by the Dutch Ministry of the Interior and Kingdom Relations[6]. All neighborhoods (defined by their 4-digit postal code) in the Netherlands have a score, ranging from 1 (very low quality of life) to 9 (very high quality of life). For every school, we calculated the weighted average quality of life indicator score of the neighborhoods the school's student body came from[7]. If no data on the residential location of students was available, we predicted the average quality of life indicator score using the score of the school's neighborhood.

We used a block design to randomize. Because the profile choice is one of our outcome variables and largely determines the variety of occupations the students are likely to be interested in, we first sought balance on this dimension. We divided the schools into three groups: predetermined choice (only one theoretical profile available), limited choice (between one and three theoretical profiles available) and all four profiles available. Within these

[5] Data on the number of students in the school itself is provided as open data by the Dutch education executive agency; `https://duo.nl/open_onderwijsdata/databestanden/vo/leerlingen/leerlingen-vo-2.jsp`; Retrieved: 22-06-2018

[6] `https://data.overheid.nl/dataset/leefbaarometer-meting-2018`; Retrieved: 22-06-2018

[7] This information is available in the data set referred to in footnote 5

groups, we subsequently ranked schools based on the number of students who completed the occupation test last year. We split these groups into three more equal groups based on this dimension. As schools vary a lot in size, we hoped to improve balance in terms of sample size in this way. Lastly, within each of the now nine groups, we ranked schools on the basis of the weighted average quality of life indicator score. We then further split these groups into two. Increased balance on this dimension is important as we estimate heterogeneous effects based on the indicator. In the end, we were left with eighteen strata.

Within each stratum, schools were randomly assigned to the different treatment groups according to the division specified in Table 1. As not every stratum contained a perfect multitude of six schools, not all schools could be assigned in one go. We dealt with the unassigned schools by recreating strata as mentioned above, omitting the division in two based on the weighted average quality of life indicator score. Within each of the now nine strata, schools were again randomly assigned. For unassigned schools arising from this procedure, we repeated the procedure once more, now stratifying only based on the freedom of profile choice. The last ten remaining unassigned schools were sorted based on the freedom of profile choice and then assigned based on a randomly ordered list of the control and treatment groups. Figure B1 in Appendix C provides a visual representation of the procedures.

4 Experimental Design

In this section, we describe the experimental design in detail. The accompanying Appendix D (online) shows screen captures of the screens students in each of the control and treatment groups get to see in the experiment.

4.1 Occupation test

The assignment on occupation choice in the Qompas method consists of two parts: a test and a reflective assignment. Although we make no alterations to the test, we use its results in the experiment. During the test, Qompas asks students to answer 90 questions about themselves and their attitude towards a number of salient occupations (e.g., waiter/waitress, mason, mechanic). The aim of this test is to predict what sort of occupations the student might be interested in. Based on the answers, Qompas calculates a score for each of the 353 occupations in their system. This score represents how well the various occupations fit the student's preferences and abilities. Qompas subsequently uses the results of this test in the reflective assignment, which contains our intervention.

4.2 Elicitation of baseline information

Before the start of the experiment, we establish a baseline of students' preferences and beliefs. To do so, we ask students a number of questions before being exposed to the intervention. The first question we ask is about their intended profile choice, which the second year students still have to make at this point. They can pick multiple options, in case they aren't sure yet. We subsequently show students the twenty occupations that fit them best according to the test and ask them to select the five occupations they are most interested in. Students then receive information on the day-to-day activities in these occupations. After they read the information, we ask the students to rank the occupations in order of how much they would like to work in them later in life. Lastly, we ask students to state their beliefs about the job opportunities and gross hourly wages of the five occupations they selected using a slider[8]. The options for job opportunities are "very poor", "poor", "reasonable", "good", and "very good". The options for the hourly wage range between €10.- and €26.-, with €1.- intervals.

During the first year of the experiment (the 2018/2019 school year), the sliders had a default option: "reasonable" for the job opportunities and €18.- for the hourly wages. Qompas removed this default option for the 2019/2020 school year. Moreover, in the 2018/2019 school year, students were able to alter their prior beliefs later on in the experiment by returning to them after receiving the information. Qompas corrected this error for the 2019/2020

[8] We ask for gross hourly wage because many youngsters in the Netherlands have a side job, e.g., in a supermarket, and are likely to have a good understanding of what they earn per hour with this job.

school year. Because of these issues, we only consider the students who went through the experiment in the 2019/2020 school year whenever prior beliefs are relevant.

4.3 Information provision

After we elicit the baseline preference ranking and beliefs about the labor market prospects, we present treated students with information about the labor market prospects of the occupations they selected. Control group students do not get any labor market information. For treatment groups 1 and 2, we provide information about the forecasted job opportunities. In treatment groups 3 and 4 we add information about the occupations' median hourly wage levels. Maastricht University's Research Center for Education and the Labor Market (ROA)[9] provided us with the information. As part of one of its research programs, ROA develops labor market forecasts for job opportunities of 113 different occupational groups in the next six years[10]. This is what we use to inform students about the job opportunities. ROA also calculated the median hourly wage of intermediate vocational education graduates for these 113 occupations. To this end, they used data from the Dutch Labor Force survey, matched to administrative records. We match the Qompas occupations to these occupational groups.

In treatments 1 and 3, we tell students that the information is presented by a researcher affiliated to Research Center for Education and the Labor Market. We divide senders into four groups: inexperienced male researchers, experienced male researchers, inexperienced female researchers, and experienced female researchers. In this context, experience is defined by the seniority of the information sender. We consider a researcher who did not have a Ph.D. (yet) at the time of the experiment's launch to be inexperienced, and consider a researcher with a Ph.D. to be experienced. To ensure understanding, we present senders' experience as either 'beginning researcher' or 'experienced researcher'[11]. For each sender, we show the

[9] www.roa.nl

[10] For information on methods, validity, and the governance of this project, see https://roa.maastrichtuniversity.nl/research/research-projects/project-onderwijs-arbeidsmarkt-poa. These forecasts are used by the national unemployment agency and for the accreditation of new study programs.

[11] In Dutch: 'beginnend onderzoek(st)er' and 'ervaren onderzoek(st)er'. We do not present the differ-

name and experience on the screen[12]. We do not explicitly mention gender, but the names of all senders are indicative of their gender and the Dutch word for 'researcher' is different for men and women. We do not show pictures of the senders, so as to avoid bias caused by appearance unrelated to status or gender.

In treatment groups 2 and 4, we do not specify a human information sender. Instead, we tell students that the Research Center for Education and the Labor Market will provide them with the information. As we do not provide students in the control group with any information, we do not show them a sender either.

4.4 Video

Next, we show students in all treatment and control groups a short video about work in general[13]. The video does not mention any particular occupations or the importance of job opportunities and wages. The main reason to show the video is to create some time between the first and second elicitation of beliefs for the control group. Without the video, students in the control group would be asked to state their beliefs a second time right after the first.

4.5 Elicitation of posterior beliefs and ranking

To estimate the effect of the treatment on beliefs and preferences, we elicit the students' ranking and beliefs a second time after the video. We show students their initial ranking and beliefs and ask them if they want to change anything.

ent statuses as 'junior' and 'senior', respectively, because we are worried about a lack of understanding. 'Beginning' and 'experienced' are more commonly used in the scenario described above in Dutch than in English.

[12]With their consent, we use the actual names of Research Center of Education and the Labor Market employees.

[13]https://www.youtube.com/watch?v=YJ78VDQrO3c

4.6 Alternative occupations

37.7% of students select only occupations of which the job opportunities are forecasted to be “very bad”, “bad”” or “reasonable”. We suggested to those students a few alternative occupations with better labor market prospects. To treated students, we state that the labor market prospects for their chosen occupations are not very good, and that the proposed alternatives have better prospects. We do not tell control group students why we offer them alternatives. All students receive information on the day-to-day activities of these occupations. If students got to see the alternative occupations, they get the opportunity to include these occupations in their ranking. Initially, we place these alternative occupations at the bottom of the ranking in a randomized order.

Information about the labor market prospects of the alternative occupations was supposed to only be provided to students in the treatment groups. However, due to a programming error, control group students also received information about the job opportunities of the alternative occupations as well as their initial set of occupations. Because of this error, we do not consider the alternative occupations in our analysis at all and remove students who were suggested alternatives from our post-intervention analyses.

4.7 Elicitation of posterior intended profile choice

At the end of the experiment, we once again ask students what profile they intend to choose. We show them their initial selection and allow them to alter it.

5 Data

5.1 Sample

We collected data between September of 2018 and July of 2020, covering the 2018/2019 and 2019/2020 school years. 249 schools actually participated in the experiment, for a total of 40,176 individuals. At the other 37 schools, the part of the platform that included our experiment was not used by any student. As schools could not know their treatment assignment before going through the experiment, this forms no threat to our internal validity. A small fraction of the individuals who went through the experiment were either first-year students (1,855) or school administrators involved in study guidance (48). We exclude them from the data. Of the remaining group of students, 1,082 did not make a first ranking of their selected occupations. As these students bring no data worth analyzing, we also exclude them. 8,924 students changed their initial preference ranking on a different day than on the day they went through the experiment. This could be because these students went through the experiment multiple times, making the belief and ranking measures unreliable. We therefore remove these students from the sample as well. None of these sample restrictions are related to treatment status. After imposing our restrictions, we are left with 28,267 individuals from 243 schools. Columns (4) to (7) of Table 1 show how these numbers relate to the number of assigned schools. Table 2 shows that covariates are balanced between the control and treatment groups.

[Table 2 about here.]

5.2 Survey data

In addition to the experimental data, we conducted a survey among graduating students in the 2019/2020 school year. The survey was fielded between the 15^{th} of April and the 20^{th} of May, 2020. The survey was sent to 9,510 students of which 1,061 responded. Again, we impose a number of sample restrictions. In our analysis, we only consider students who went through the experiment, did not change their prior ranking on a different day than they created it, did not see the alternative occupations and were either in the second-to-last year of secondary school in the 2018/2019 school year or the final year of secondary school in 2019/2020 school year. After we impose our sample restrictions, we are left with 4,389 survey invitees, and 405 respondents. To incentivize responses, we announced that we would raffle off 20 €25.- vouchers for a large Dutch e-tailer among survey respondents. In the survey, we once again ask students to state their beliefs about the labor market prospects of the occupations they selected as well as to rank the occupations based on how much they would like to carry them out later in life. Furthermore, we ask them about their plans for next year. If students indicate they will go on to intermediate vocational education, we ask them to state what study program they enrolled in using a free-form text field. We manually match these study programs to their official study program identifier. All study program identifiers can be linked to the Research Center of Education and the Labor Market's labor market information system, which we use in our analysis. For the analysis of the survey respondents' beliefs and preferences, all above mentioned sample restrictions apply as well. For the analysis of the study program choice, we relax the restriction on the alteration of the prior ranking. Naturally, we do not include prior beliefs in these analyses. Table C1 in Appendix C shows that responding to the survey is not related to treatment. We do observe that older students and male students are less likely to respond to the survey.

5.3 Administrative data

To analyze the actual profile choice of students, we use administrative data at the school level[14]. These data provide us with information on the number of students that follow a particular profile on the 1st of October of each year from 2018 to 2020.

While these data allow us to analyze the impact of the treatment on students' profile choices, there are a number of caveats. First of all, because the data is at the school level, we are not able to restrict the sample only to those who have not seen the suggested alternative occupations. Our estimations will therefore likely be lower bounds. Secondly, we lose a lot of power by not being able to do the analyses at the individual level. We recently received permission to match our experimental data to administrative records at the individual level. Analyses using individual-level data will be performed and added once the data have become

[14]Available at `https://duo.nl/open_onderwijsdata/databestanden/vo/leerlingen/leerlingen-vo-1.jsp`.

available for us.

6 Results

6.1 Descriptive statistics

6.1.1 Selected occupations

Figure 1 shows the job opportunities and hourly wages of the occupations students selected for their top five before the intervention. Most selected occupations have job opportunities that are either poor (category 2), reasonable (3) or good (4). Hourly wages generally range between €12.- and €18.-. The Figure also shows that before the interventions there is no difference between the control and treatment groups in terms of job opportunities and hourly wages for the occupations the students selected for their top five. Tables C2 and C3 in Appendix C confirm this. Although Table C3 does show that students in the first treatment group select occupations with lower hourly wages, the joint significance tests do not allow us to reject that the selection process between the treatment and control groups is the same. These Tables also show that there is no difference in the labor market prospects of the selected occupations between the first and second year of the experiment.

There are some interesting patterns in the selection of the occupations. Tables C4 and C5 in Appendix C show that male students generally select occupations with better job opportunities and higher hourly wages than female students do. Students in later years and students from low socioeconomic status neighborhoods choose occupations with higher hourly wages, but no better job opportunities.

[Figure 1 about here.]

6.1.2 Prior beliefs

Figure 2 shows the prior belief accuracy of the control and treatment groups in the two years of the experiment. We denote the prior beliefs of individual i about the job opportunities of occupation j by $O_{i,j}^{Prior}$, and the actual job opportunities for that occupation by O_{j}^{Actual}. We apply the same logic to the hourly wages, which we denote as W. To measure belief accuracy, we first consider the difference between individual i's belief about the prospects of occupation j and its actual prospects: $O_{i,j}^{Prior} - O_{j}^{Actual}$ and $W_{i,j}^{Prior} - W_{j}^{Actual}$. These differences, which we report in Figure 2, allow us to analyze the degree of over- and underestimation of job opportunities and hourly wages. In the 2018/2019 school year, treated students show significantly more accurate expectations about the job opportunities and hourly wages than do control group students. This is likely due to the fact they could correct their initial beliefs, as discussed in Section 4.3. In the 2019/2020 school year, when the programming error was fixed, there is no difference between the beliefs of control and treatment group students. The figure also shows a left-skewed distribution, which indicates that students tend to overestimate the labor market prospects of their preferred occupations.

[Figure 2 about here.]

When using central tendency measures, errors in beliefs that have opposite directions may cancel each other out. We therefore consider two additional metrics to assess the accuracy of students' beliefs and how these differ by a number of characteristics. First, we analyze the absolute values of the belief errors: $|O_{i,j}^{Prior} - O_{j}^{Actual}|$ and $|W_{i,j}^{Prior} - W_{j}^{Actual}|$. The combination of the overestimation and absolute error allows us to infer to what degree errors are caused by overestimation and underestimation. Secondly, we analyze how often beliefs are exactly correct (i.e., $O_{i,j}^{Prior} - O_{j}^{Actual} = 0$ and $W_{i,j}^{Prior} - W_{j}^{Actual} = 0$).

Because we bound students' stated expectations by our use of sliders, students cannot overestimate occupations with good job opportunities and high hourly wages to the same degree as occupations that have worse prospects. Since there is heterogeneity in occupational preferences, we have to account for this in our analyses. We do this by adding an occupation fixed effect in our analysis of belief accuracy. This means we compare individuals' belief accuracy conditional on the occupation they selected. Table C6 in Appendix C shows that male students tend to overestimate both job opportunities and hourly wages to a larger degree. They also make larger absolute errors and are less likely to be correct. Third and fourth year students, who are closer to making a decision than second year students don't do much better when it comes to the job opportunities, but make smaller absolute errors for the hourly wages. This might be because of the fact that we present the job opportunities in a categorical manner. Even if students do have a good idea about the future job opportunities, they might not agree on the qualifications we assign to them. Students in schools where more profiles are available seem to make smaller absolute errors and are somewhat more likely to be correct about both job opportunities and hourly wages. What's most striking about the Table is the effect the initial ranking of the occupation has on the belief accuracy. Higher ranked occupations are overestimated to a much larger degree. The difference between the number one and number five ranked occupation is almost an entire category for the job opportunities and €1.50 for the hourly wages.

6.2 Treatment effects

6.2.1 Posterior beliefs

Moving to the effect of the treatment, Figure 3 shows the posterior belief accuracy for the control group and relevant treatment groups. We denote the posterior beliefs of individual i about the job opportunities of occupation j by $O_{i,j}^{Post}$ and that of the hourly wages by $W_{i,j}^{Post}$. The graphs show that in both years, students in the treatment groups are much more likely to be correct about the job opportunities and the hourly wages of their selected occupations. This is largely driven by the correction of overestimations. Treated students correct beliefs more often and more strongly than control students. Students who initially underestimated the labor market prospects of their occupations react much less strongly than those who initially overestimated them. Tables C7 and C8 in Appendix C confirms this for the 2019/2020 cohort, where we can use students' prior beliefs in the analysis.

[Figure 3 about here.]

Table C9 shows that the treatment is equally effective when a researcher is said to provide the information, compared to an institute. Zooming in on the specific researcher, Table C10 shows that neither whether a male or a female researcher provides the information, nor whether the sender was an 'experienced' or 'beginning' researcher matters for the degree to which beliefs are updated. Table C11 shows that when it comes to job opportunities, third and fourth year students react more strongly to the treatment than second year students. The same holds for male versus female students. Table C12 shows that treated fourth year students are more often correct than earlier year students, although the change is smaller than for the job opportunities and only marginally significant.

Next, we study how persistent the effects on posterior beliefs are. Table C13 shows that beliefs about the job opportunities remain more accurate at the time of the survey for students treated in the 2019/2020 school year (that is, up to seven months after treatment). This does not hold for those treated in the 2018/2019 school year (who completed the survey over a year after the treatment). However, we cannot ascribe the difference to time since treatment alone Information on job opportunities and hourly wages may become more important as students get closer to their post-secondary education decision. As we survey graduating students, the students who received the information most recently were also much closer to the end of their secondary school career when they did. As such, the reason these students better recall the information may be that they paid more attention to it, not that that they received it more recently. With our data, we cannot distinguish between these two mechanisms. For the hourly wages, we find that treated students do not have more accurate beliefs than the control group for both years of the experiment.

6.2.2 Rankings

Table 3 shows how the treatment affects the likelihood of students changing their favorite occupation between the first and second elicitation. We observe that students in the treatment group indeed change their favorite occupation significantly more often than those in the control group. The effect size is fairly small, however. In the control group, approximately 5.53% of students change their favorite occupation. In the treatment groups, this fraction is 0.88 to 2.16 percentage points higher.

The fact that students in the treatment group change their favorite occupation (slightly) more often does not tell the whole story, however. Table 3 also shows whether students in the treatment group switch towards occupations with better labor market prospects. ΔO_j^{Actual} and ΔW_j^{Actual}, respectively, denote the difference in the job opportunities and hourly wages between the number one ranked occupation at first elicitation and the number one ranked occupation at second elicitation. If a student does not change his or her favorite occupation between the first and second elicitation, $\Delta O_j^{Actual} = \Delta W_j^{Actual} = 0$. Columns (2) and (4) show the effect unconditional on actually changing the number one ranked occupation. The job opportunities in the treatment groups rise by anywhere from 0.0190 to 0.0305 categories. For the wage treatments, the hourly wages rise by €0.09. Columns (3) and (5) show the change for students who did change their favorite occupation. For students in the treatment groups, the job opportunities move up by 0.285 to 0.447 categories and hourly wages by €1.12 to €1.20. It is important to note that in both cases, the job opportunities and hourly wages do not move at all for control group students.

[Table 3 about here.]

Table C14 in Appendix C shows that treated students in schools with four profiles available are not more likely to change their favorite occupation compared to schools with fewer profiles available, but do switch to occupations with better job opportunities when they do. This may be driven by the fact that these students have a larger set of options to choose from. This Table, together with Table C15 shows that we find no further evidence for heterogeneous treatment effects. Table C16 shows there is no effect of the information sender either.

We do not find any evidence that treated students still prefer occupations with better prospects in the survey. However, Columns (1) and (3) of Table C17 show that the treated students in the survey did not switch to occupations with better prospects directly after the intervention either.

6.2.3 Profile & study program choice

Moving to the profile choice, we first study how our intervention affects the intended profile choice of students right after the intervention. Table 4 provides no evidence that the treatment impacts the likelihood second year students' intended profile choice or the number of profiles they consider right after the experiment. Table C18 in Appendix C shows that there is some heterogeneity based on the number of profiles available in the school, however. Looking at Column (3), the treatment seems to marginally narrow the scope of profiles students in the basic, advanced vocational and mixed programs are willing to consider if they are in schools that offer very few profiles. Table C19 shows that even learning that an occupation that fits with a certain profile has very good job opportunities or hourly wages does not make students more likely to include that profile in their choice set immediately after the experiment. While somewhat surprising, it may be that students require some time to process the information they received and adjust their intended choices based on this. Based on administrative data at the school level, we can study how profile choices actually materialized. Unfortunately, this decreases the number of observations and therefore statistical power, especially since not every single profile is available in every school. Because of this, we consolidate our treatment groups to 'Treated' (treatment groups 1 through 4) and 'Wage Information' (treatment groups 3 and 4).

To analyze and quantify the impact our intervention had on the actual profile choice, we assign a value for job opportunities as well as hourly wages to each profile. We do this by taking the average job opportunities and hourly wages of the occupations associated with a certain profile, weighted by how often that occupation was chosen by students. Figure 4 shows these metrics by profile, as well as the weighted average prior beliefs for the occupations associated with these profiles. It is worthwhile to note how large the difference between the profiles in terms of the actual job opportunities and hourly wages they provide are. For instance, the blue bars in Panel (a) show that the average job opportunities for the technology profile is almost a category higher than that of the economics profile. Likewise, panel (b) shows that the hourly wages for occupations associated with technology profiles are close to €3.- an hour higher than for occupations associated with the agriculture profile. Panels (c) and (d) show a similar picture for the other profiles, with even more striking differences, as these profiles are more fine-grained. The red bars, showing students' prior beliefs about the labor market prospects in these occupations, show a much flatter picture. While students do seem to judge the occupations associated with some profiles to have better prospects than others, the differences are slim. With such large differences, that students are not a priori aware of, it seems likely that some students may change their preferences over profiles once they learn more about the labor market prospects in the occupations associated with them.

To analyze this, we determine the weighted average job opportunities and hourly wages of the number of third year students enrolled in each profile at the school level. Our baseline period is the 1^{st} of October 2018. As our experiment started in the 2018/2019 school year, the intervention could not have had an impact on the profile choice yet, as students decide on their profile at the end of the previous school year. Table 5 shows the results of our analysis. Controlling for the weighted average job opportunities and hourly wages of the chosen profiles in 2018, students in the relevant treatment groups choose profiles with better job opportunities and higher hourly wages. The effects are marginally significant, but point in the same direction. The effect is 0.0424 categories (1,5% of the control group mean) for the job opportunities and approximately €0.05 (0.3% of the control group mean) for the hourly wages. We should note that, because of the way in which we calculate the job opportunities and hourly wages of the profiles, we assume that the selection of occupations is unaffected by the treatment. This is not necessarily the case, as the results from our survey will show.

Lastly, Table 6 shows how the treatments affect the study program choice. Column (1) shows that students who receive information about just the job opportunities from a researcher choose study programs with job opportunities that are 0.43 categories better on average. This is a sizeable effect. However, the effect of the other treatments are very close to – and not significantly different from – zero. A joint significance test reveals that the treatments did not lead to students choosing study programs with significantly better job opportunities. Column (2) shows the treatment effect on the hourly wages of the chosen study programs. Compared to control group students, students who receive information about both the job opportunities and the hourly wages from a researcher choose study programs of which graduates earn €0.74 an hour more on average. The other wage treatment shows a slight positive, but insignificant effect. A joint significance test of the two treatments shows that the information about the hourly wages indeed lead students to choose study program with higher earnings prospects.

[Table 6 about here.]

7 Conclusion

In this paper, we presented a field experiment aimed at improving the accuracy of Dutch pre-vocational education students' beliefs about the job opportunities and hourly wages of occupations they are interested in. In line with the literature, we find that students' prior beliefs are highly inaccurate. In our sample, both job opportunities and hourly wages are strongly overestimated, particularly for students' favorite occupations. This could be innocuous, and simply the results of students rationalizing their choices. However, since our results indicate that students do indeed attach some value to the labor market prospects of occupations when making educational decisions, another explanation is likely. If students gather noisy information and tend to gravitate towards the occupations for which they learn the labor market prospects are best, these will often be the occupations for which the information was least accurate in a winner's curse fashion. This underlines the importance of providing students with accurate information.

Our results show that providing such information is effective in correcting belief errors in the short term. However, survey data shows that these beliefs stick for at most a couple of months and only for the job opportunities. Students who receive information are more likely to change their favorite occupation between the first and second elicitation of the ranking and, if they do so, switch towards occupations with better labor market prospects. We are unable to confirm whether this change in preferences holds in the long term, however. Even though we do not see very strong effects on stated beliefs and preferences in the long term, we do see that students in treated schools enroll in profiles associated with occupations that have better labor market prospects. Similarly, students who received information about the hourly wages of occupations do enroll in post-secondary education programs that have better earnings prospects. We find no evidence that it matters whether the information is provided by a person or an institute, and in the former case whether this person is experienced, inexperienced, male or female.

A limitation of our experiment is that we either use self-reported measures of beliefs or have to rely on school level data. We intend to repeat our analysis using administrative data at the individual level on profile choice, post-secondary education program choice and degree matriculation. The big advantage of this data for the profile choice is that we can actually link the information students received about occupations associated with a certain profile to their choices. Currently, we have to rely on averages by profile. Degree matriculation is particularly important as well, as it is not something we can currently analyze. If students choose different study programs because of the information, but do not end up finishing these, the net effect of the information may still be negative. Likewise, if the information motivates students to finish their study programs, our findings are an underestimation of the true effect.

References

Altonji, J. G., Arcidiacono, P., & Maurel, A. (2016). The analysis of field choice in college and graduate school: Determinants and wage effects. In *Handbook of the economics of education* (Vol. 5, pp. 305–396). Elsevier.

Altonji, J. G., Blom, E., & Meghir, C. (2012). Heterogeneity in human capital investments: High school curriculum, college major, and careers. *Annu. Rev. Econ.*, *4*(1), 185–223.

Arcidiacono, P., Hotz, V. J., & Kang, S. (2012). Modeling college major choices using elicited measures of expectations and counterfactuals. *Journal of Econometrics*, *166*(1), 3–16.

Baker, R., Bettinger, E., Jacob, B., & Marinescu, I. (2018). The effect of labor market information on community college students' major choice. *Economics of Education Review*, *65*, 18–30.

Betts, J. R. (1996). What do students know about wages? evidence from a survey of undergraduates. *Journal of human resources*, 27–56.

Bleemer, Z., & Mehta, A. (2022). Will studying economics make you rich? a regression discontinuity analysis of the returns to college major. *American Economic Journal: Applied Economics*, *14*(2), 1–22.

Bleemer, Z., & Zafar, B. (2018). Intended college attendance: Evidence from an experiment on college returns and costs. *Journal of Public Economics*, *157*, 184–211.

Bonilla-Mejía, L., Bottan, N. L., & Ham, A. (2019). Information policies and higher education choices experimental evidence from colombia. *Journal of Behavioral and Experimental Economics*, *83*, 101468.

Conlon, J. J. (2019). Major malfunction: A field experiment correcting undergraduates' beliefs about salaries. *Journal of Human Resources*, 0317–8599R2.

Del Carpio, L., & Guadalupe, M. (2021). More women in tech? evidence from a field experiment addressing social identity. *Management Science*.

Dutch Inspectorate of Education. (2020). *De staat van het onderwijs 2020* (Tech. Rep.).

Government of the Netherlands. (n.d.-a). *Pre-vocational secondary education (vmbo).* Retrieved 2019-08-06, from https://www.government.nl/topics/secondary-education/pre-vocational-secondary-education-vmbo

Government of the Netherlands. (n.d.-b). *Wat zijn de vooropleidingseisen voor het middelbaar beroepsonderwijs (mbo)?* Retrieved 2019-08-11, from https://www.rijksoverheid.nl/onderwerpen/middelbaar-beroepsonderwijs/vraag-en-antwoord/wat-zijn-de-vooropleidingseisen-voor-het-middelbaar-beroepsonderwijs-mbo

Hastings, J., Neilson, C. A., Ramirez, A., & Zimmerman, S. D. (2016). (un) informed college and major choice: Evidence from linked survey and administrative data. *Economics of Education Review*, *51*, 136–151.

Hastings, J., Neilson, C. A., & Zimmerman, S. D. (2013). *Are some degrees worth more than others? evidence from college admission cutoffs in chile* (Tech. Rep.). National Bureau of Economic Research.

Hastings, J., Neilson, C. A., & Zimmerman, S. D. (2015). *The effects of earnings disclosure on college enrollment decisions* (Working Paper No. 21300). National Bureau of Economic Researc. Retrieved from http://www.nber.org/papers/w21300

Jensen, R. (2010). The (perceived) returns to education and the demand for schooling. *The Quarterly Journal of Economics*, *125*(2), 515–548.

Ketel, N., Leuven, E., Oosterbeek, H., & van der Klaauw, B. (2016). The returns to medical school: Evidence from admission lotteries. *American Economic Journal: Applied Economics*, *8*(2), 225–54.

Kirkeboen, L. J., Leuven, E., & Mogstad, M. (2016). Field of study, earnings, and self-selection. *The Quarterly Journal of Economics*, *131*(3), 1057–1111.

Lergetporer, P., Werner, K., & Woessmann, L. (2018). *Does ignorance of economic returns and costs explain the educational aspiration gap? evidence from representative survey experiments* (IZA Discussion Papers No. 11453). Institute for the Study of Labor (IZA). Retrieved from `https://EconPapers.repec.org/RePEc:iza:izadps:dp11453`

McGuigan, M., McNally, S., & Wyness, G. (2016). Student awareness of costs and benefits of educational decisions: effects of an information campaign. *Journal of Human Capital*, *10*(4), 482–519.

Nuffic. (2019). *The education system of the netherlands.* Retrieved 2019-08-04, from `https://www.nuffic.nl/documents/459/education-system-the-netherlands.pdf`

Oreopoulos, P., & Dunn, R. (2013). Information and college access: Evidence from a randomized field experiment. *The Scandinavian Journal of Economics*, *115*(1), 3–26.

Pekkala Kerr, S., Pekkarinen, T., Sarvimäki, M., & Uusitalo, R. (2015). *Post-secondary education and information on labor market prospects: A randomized field experiment* (IZA Discussion Papers No. 9372). Institute for the Study of Labor (IZA). Retrieved from `https://EconPapers.repec.org/RePEc:iza:izadps:dp9372`

Peter, F. H., & Zambre, V. (2017). Intended college enrollment and educational inequality: Do students lack information? *Economics of Education Review*, *60*, 125–141.

Porter, C., & Serra, D. (2020). Gender differences in the choice of major: The importance of female role models. *American Economic Journal: Applied Economics*, *12*(3), 226–54.

Wiswall, M., & Zafar, B. (2017). Preference for the workplace, investment in human capital, and gender. *The Quarterly Journal of Economics*, *133*(1), 457–507.

Tables

Table 1: Treatment assignment, participation and analysis sample overview

Treatment Group	**Frac. of Schools**	**Assigned Schools**	**Participating Schools**	**Participating Students**	**Schools in Analysis**	**Students in Analysis**
Control Group	1/3	96	83	12,544	81	9,275
Job Opp. Info by Researcher (Treatment 1)	1/6	47	42	6,917	42	5,117
Job Opp. Info by Research Institute (Treatment 2)	1/6	47	40	6,470	40	5,151
Job Opp. & Wage Info by Researcher (Treatment 3)	1/6	48	38	5,580	38	4,254
Job Opp. & Wage Info by Research Institute (Treatment 4)	1/6	48	43	5,680	42	4,470
Total	1	286	246	37,191	28,267	243

Table 2: Balance table

	Control		Treatment 1		Treatment 2		Treatment 3		Treatment 4		
	Mean	Std. Dev	Mean	Std. Dev	Mean	Std. Dev	Mean	Std. Dev	Mean	Std. Dev	P-value joint sign.
No. of students	164.14	134.24	175.55	147.33	178.00	121.59	158.21	118.49	150.10	110.95	0.82
No. of profiles available	3.37	0.99	2.93	1.30	3.30	1.14	3.21	1.07	3.14	1.20	0.38
Age	14.04	1.01	14.02	0.99	14.16	0.99	14.06	0.99	14.05	1.04	0.82
Male	0.52	0.50	0.53	0.50	0.56	0.50	0.52	0.50	0.50	0.50	0.43
Grade	2.47	0.64	2.45	0.64	2.56	0.68	2.43	0.61	2.48	0.67	0.84
QOL score	6.57	1.35	6.55	1.31	6.77	1.23	6.65	1.42	6.46	1.46	0.59

Note: No. of students and no. of profiles available are school level variables. Age, male, grade and QOL score are individual level variables. QOL score refers to the quality of life indicator score of the neighborhood the student lives in (see Section 3 in the paper). The last column of the Table indicates whether a joint significance tests shows a significant difference between the treatment groups and the control group for each of the variables considered.

Table 3: Treatment effect on likelihood changing favorite occupation and change in prospects

	(1) Pr(Fav. Change)	(2) ΔO_j^{Actual}	(3) ΔO_j^{Actual} (Changed)	(4) ΔW_j^{Actual}	(5) ΔW_j^{Actual} (Changed)
Sender: researcher					
Info: job opportunities	0.00877*	0.0190***	0.295***	0.0130	0.192
	(0.00454)	(0.00622)	(0.101)	(0.0174)	(0.292)
Sender: institute					
Info: job opportunities	0.0126**	0.0305***	0.447***	0.0303**	0.430*
	(0.00529)	(0.00650)	(0.102)	(0.0149)	(0.253)
Sender: researcher					
Info: job opp. & wages	0.0216***	0.0278***	0.358***	0.0876***	1.115***
	(0.00562)	(0.00577)	(0.0880)	(0.0215)	(0.295)
Sender: institute					
Info: job opp. & wages	0.0189***	0.0214***	0.285***	0.0904***	1.197***
	(0.00624)	(0.00640)	(0.0944)	(0.0199)	(0.278)
Constant	0.0553***	0.000666	0.0120	0.00466	0.0843
	(0.00317)	(0.00325)	(0.0590)	(0.0102)	(0.186)
Observations	27387	27387	1791	27387	1791

Note: Constant refers to the control group estimate. Each row above refers to the incremental estimate for each of the four treatment groups. Pr(Fav. Change) in Column (1) is the chance that a student changed his or her favorite occupation between second elicitation. ΔO_j^{Acutal} in Column (2) denotes the difference between the job opportunities of the student's favorite occupation at the second elicitation and the first elicitation. It is equal to 0 if the student did not change. ΔW_j^{Acutal} in Column (4) denotes the equivalent for the hourly wages. Columns (3) and (5) only contain observations where the student did switch favorite occupations between first and second elicitation. Standard errors clustered at the school level in brackets; *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Regressions at individual level.

Table 4: Treatment effect on profiles considered immediately after intervention

	(1) Pr(Same Profile Pre-Post)	(2) No. of Theoretical Profiles	(3) No. of Other Profiles
Sender: researcher			
Info: job opportunities	-0.0395	-0.0152	0.0468
	(0.0264)	(0.0188)	(0.0475)
Sender: institute			
Info: job opportunities	-0.0274	0.00781	0.00588
	(0.0229)	(0.0229)	(0.0525)
Sender: researcher			
Info: job opp. & wages	-0.0383	0.00177	0.00648
	(0.0236)	(0.0268)	(0.0443)
Sender: institute			
Info: job opp. & wages	-0.0359	-0.00870	0.0229
	(0.0311)	(0.0197)	(0.0481)
No. of theoretical profiles a priori		0.554***	
		(0.0190)	
No. of other profiles a priori			0.443***
			(0.0333)
Constant	0.692***	0.169***	0.287***
	(0.0126)	(0.0122)	(0.0412)
Observations	10671	5901	4772
F-Stat joint sign. of treatments.	1.133	0.312	0.368
P-value F-Stat joint sign. of treatments.	0.342	0.869	0.831

Note: Constant refers to the control group estimate. Each row above refers to the incremental estimate for each of the four treatment groups. No. of profiles a priori is a metric of how many profiles a student considered before the intervention. Pr(Same Profile Pre-Post) in Column (1) indicates the likelihood that student did not change his or her profile choice between the two elicitations. No. of Theoretical Profiles and No. of Other Profiles in Columns (2) and (3) denote the number of profiles a student considered at second elicitation, respectively. Only second year students, who did not see alternative occupations are included in this analysis. Standard errors clustered at the school level in brackets; *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Regressions at individual level.

Table 5: Treatment effect on labor market prospects chosen profiles

	(1) Weighted job opportunities	(2) Weighted wage level
Treated	0.0424*	0.00417
	(0.0238)	(0.0286)
Weighted job opportunities 2018	0.822***	
	(0.0377)	
Wage information		0.0421
		(0.0272)
Weighted wage level 2018		0.961***
		(0.0161)
Constant	0.473***	0.620**
	(0.0960)	(0.257)
Observations	444	444
F-Stat Treated + wage information		3.100
P-value F-Stat Treated + wage information		0.080

Note: Constant refers to the control group estimate. Treated refers to all four treatment groups. Wage information is the incremental estimate for treatment groups 3 and 4, that received wage information. 'Weighted job opportunities' in Column (1) and the second row refers to the average job opportunities of the profiles students chose, based on how often they chose them. 'Weighted wage level' refers to the same for the hourly wages. Section 6.2.3 explains the metrics in more detail. 'F-stat Treated + wage information' and the corresponding p-value refer to the test of significance for the estimate of 'Treated' + the estimate of 'Wage information', which provides the estimate for the treatment groups that received information about hourly wages as well as about job opportunities. Standard errors clustered at the school level in brackets; *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Regressions at school-year level.

Table 6: Treatment effect on job opportunities and hourly wages chosen study program

	(1) Job Opportunities Chosen Program	(2) Average Hourly Wages Chosen Program
Sender: researcher		
Info: job opportunities	0.432**	0.217
	(0.184)	(0.291)
Sender: institute		
Info: job opportunities	-0.0544	0.0660
	(0.160)	(0.259)
Sender: researcher		
Info: job opp. & wages	0.00253	0.740**
	(0.157)	(0.296)
Sender: institute		
Info: job opp. & wages	-0.0958	0.0367
	(0.253)	(0.276)
Constant	2.361***	17.43***
	(0.0801)	(0.154)
Observations	405	405
F-Stat Relevant Treatments	1.678	3.331
P-value F-Stat Relevant Treatments	0.160	0.0393

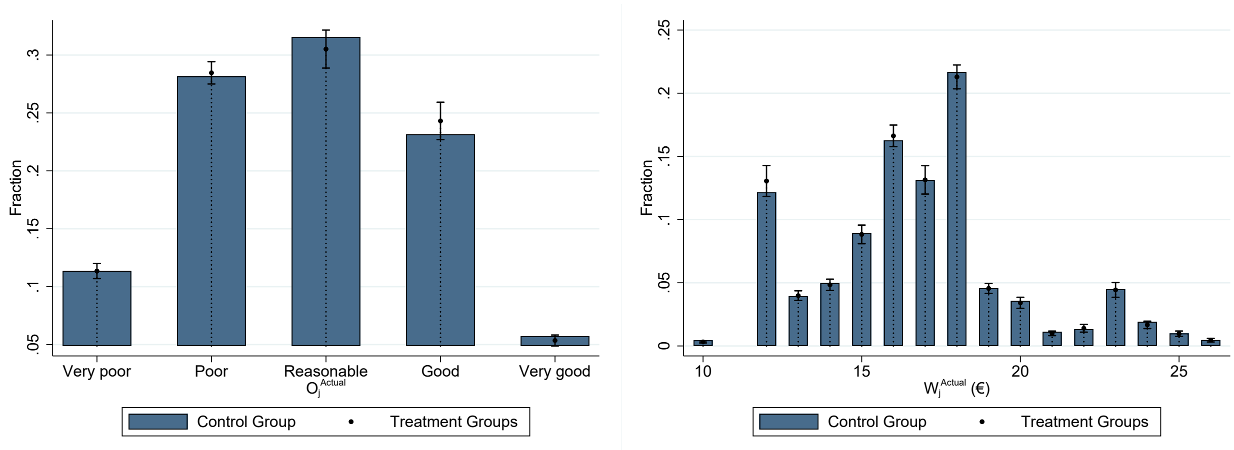
Note: Constant refers to the control group estimate. Each row above refers to the incremental estimate for each of the four treatment groups. Job opportunities chosen program in Column (1) refers to the assigned job opportunities of the student's chosen study program in the Research Centre of Education and Labor Market's research program. Average hourly wages of chosen program refers to the hourly wage level of the program, from the same source. Standard errors clustered at the school level in brackets; *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Regressions at individual level.

Figures

Figure 1: Job opportunities and hourly wages of selected occupations

(a) Job opportunities

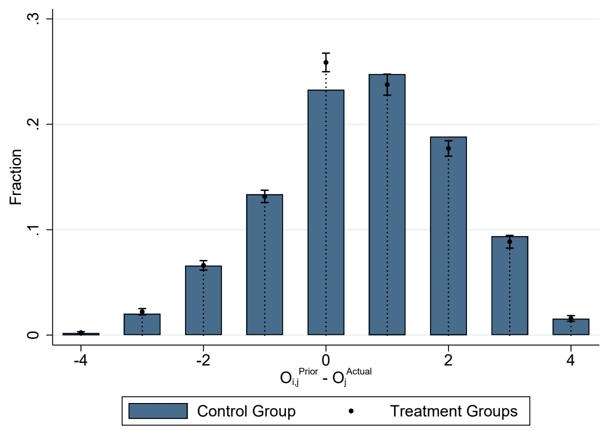
(b) Hourly wages



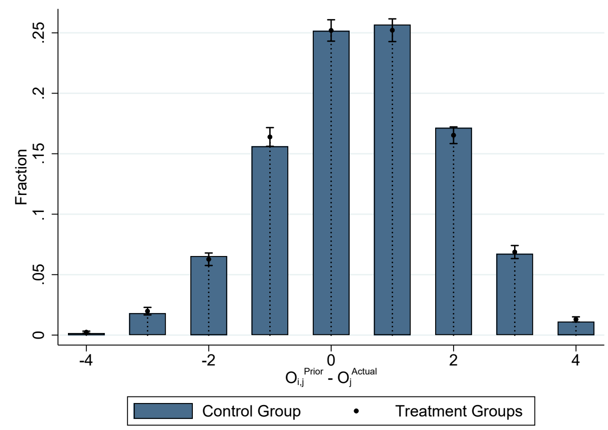
Note: graphic representation of multinomial logit estimation. Standard errors clustered at school level. Blue bars indicate level for control group. Black dots and error bars indicate level for treatment group and 95% confidence interval, respectively.

Figure 2: Prior belief accuracy by relevant group

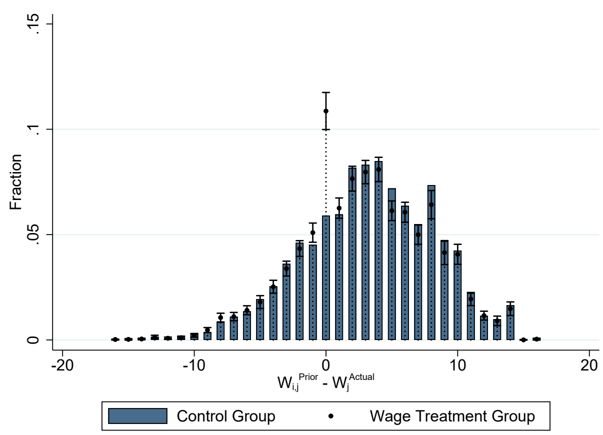
(a) Job opportunities 2018/2019



(b) Job opportunities 2019/2020



(c) Hourly wages 2018/2019

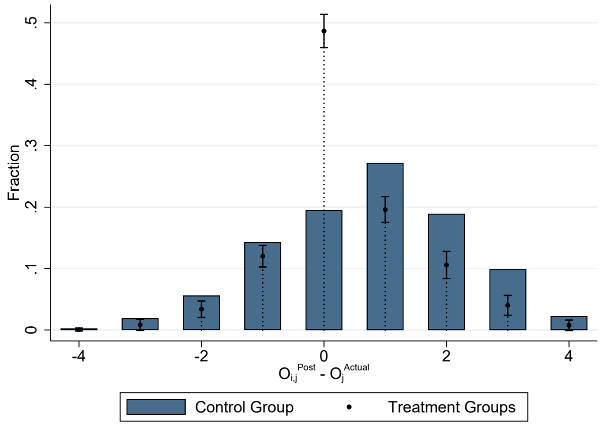


(d) Hourly wages 2019/2020

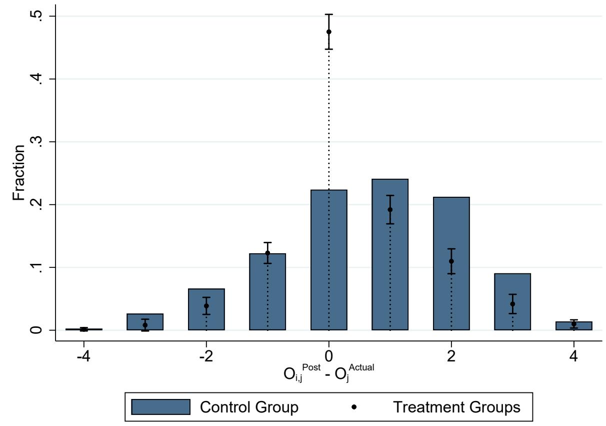
Note: graphic representation of multinomial logit estimation at occupation level. The x-axis displays the degree of overestimation. For the job opportunities, the numbers indicate the overestimation in categories (i.e., -2 denotes an underestimation of two categories, whereas +2 indicates an overestimation of two categories). For the hourly wages, the overestimation is displayed in Euros. Standard errors clustered at school level. Blue bars indicate level for control group. Black dots and error bars indicate level for treatment group and 95% confidence interval, respectively.

Figure 3: Posterior belief accuracy by relevant group

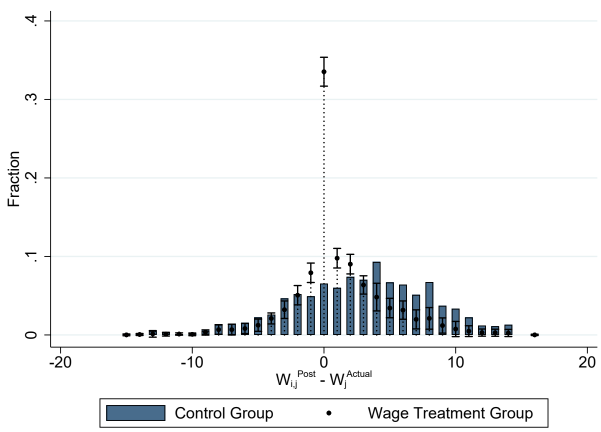
(a) Job opportunities 2018/2019



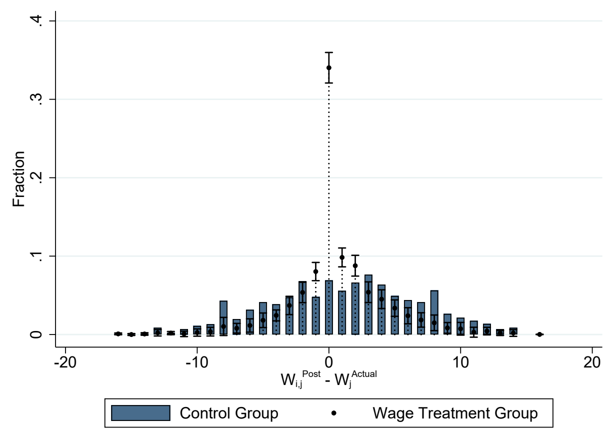
(b) Job opportunities 2019/2020



(c) Hourly wages 2018/2019



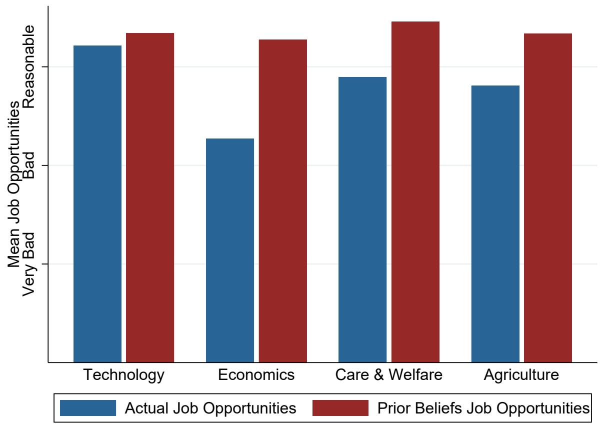
(d) Hourly wages 2019/2020



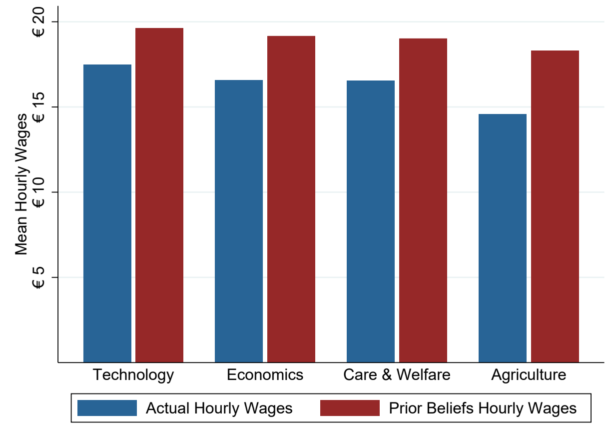
Note: graphic representation of multinomial logit estimation at occupation level. The x-axis displays the degree of overestimation. For the job opportunities, the numbers indicate the overestimation in categories (i.e., -2 denotes an underestimation of two categories, whereas +2 indicates an overestimation of two categories). For the hourly wages, the overestimation is displayed in Euros. Standard errors clustered at school level. Blue bars indicate level for control group. Black dots and error bars indicate level for treatment group and 95% confidence interval, respectively.

Figure 4: Weighted average actual prospects and prior beliefs by profile

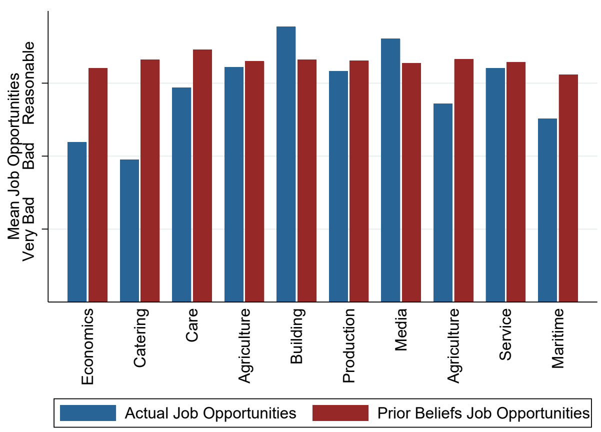
(a) Job opportunities theoretical profiles



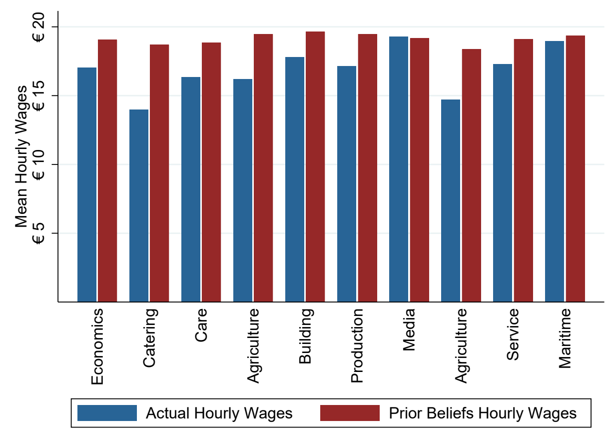
(b) Hourly wages theoretical profiles



(c) Job opportunities other profiles



(d) Hourly wages other profiles



Note: red bars display the average weighted job opportunities and hourly wages of selected occupations by profile, weighted by how often the occupation was selected. Blue bars display the average prior belief about the job opportunities and hourly wages in these profiles.

Appendix A: Recruitment Text

Dutch

ROA (Researchcentrum voor Onderwijs en Arbeidsmarkt aangesloten bij Universiteit Maastricht) en Qompas zijn samen door het Ministerie van Onderwijs, Cultuur en Wetenschap (OCW) gevraagd om onderzoek uit te voeren naar de invloed van arbeidsmarktinformatie op de keuze van vmbo-leerlingen voor een studie.

Door middel van een A/B-test in de lesmethode Qompas VMBO/Mavo gaan we onderzoeken of vmbo'ers bij het maken van hun studiekeuze letten op informatie over baankans en of die informatie ertoe bijdraagt dat zij een betere keuze maken. Met deze informatie kan Qompas haar lesmethode doorontwikkelen om scholieren in de toekomst nog beter te kunnen helpen met hun studiekeuze.

Wij hopen dat uw school meewerkt aan dit onderzoek. Alle gegevens worden anoniem verwerkt. Voor meer informatie kunt u contact opnemen met [REDACTED].

English

ROA (The Research Center for Education and the Labor Market, part of Maastricht University) and Qompas were asked by the Ministry of Education, Culture and Science (OCW) to do research on the influence of labor market information on the education choices of intermediate vocational education students.

Through an A/B-test in the Qompas system we will research whether intermediate vocational education students take information about job opportunities into account when making education choices and whether this information helps them make a better choice. With this information, Qompas can improve its platform by being even more able to help students with their education choice.

We hope your school will participate in this study. All details will be processed anonymously. For more information, you can contact [REDACTED].

Appendix B: Additional figures

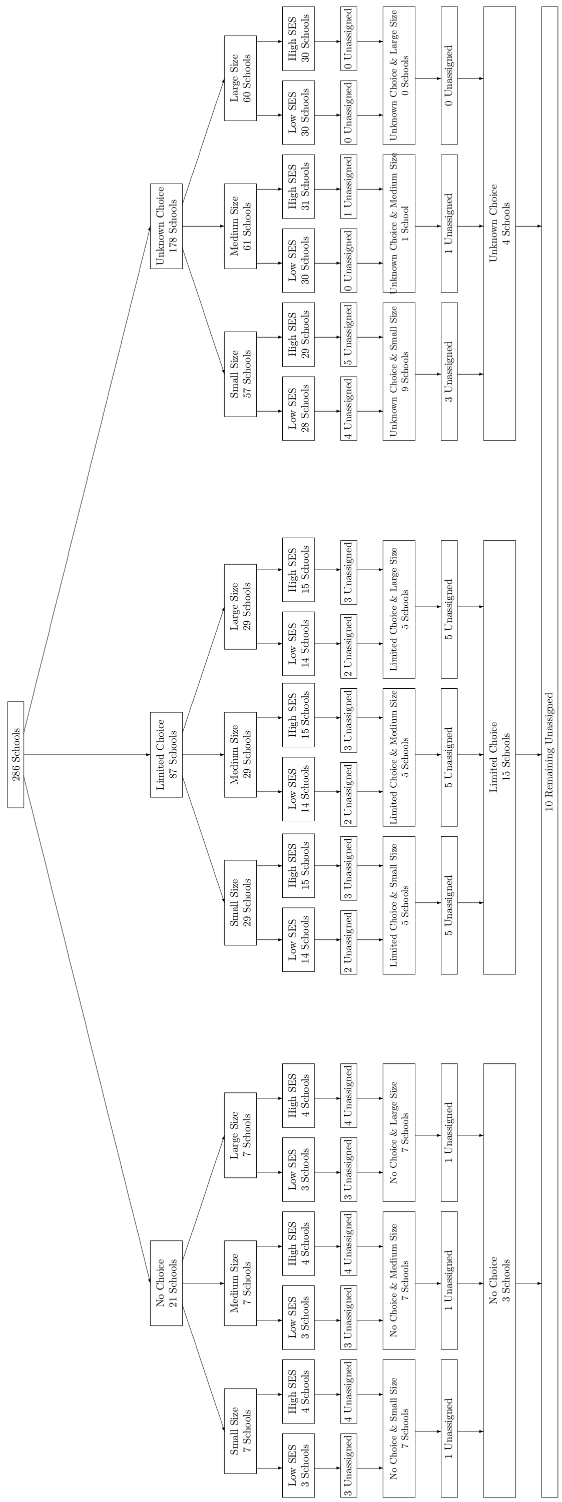


Figure B1: Graphical Representation of Randomization

Appendix C: Additional tables

Table C1: Balance check survey respondents

	(1) Answered Survey	(2) Age	(3) Grade	(4) Male	(5) QOL score
Sender: researcher					
Info: job opportunities	0.0148				
	(0.0178)				
Sender: institute					
Info: job opportunities	0.00580				
	(0.0184)				
Sender: researcher					
Info: job opp. & wages	-0.00294				
	(0.0157)				
Sender: institute					
Info: job opp. & wages	-0.0116				
	(0.0125)				
Answered Survey		-0.0840*	-0.0161	-0.192***	-0.0251
		(0.0500)	(0.0343)	(0.0273)	(0.0680)
Constant	0.0960***	14.88***	3.242***	0.563***	6.710***
	(0.00848)	(0.0482)	(0.0400)	(0.0147)	(0.0759)
Observations	4389	4012	4389	4388	4292
F-Stat Treatments	0.637				
P-value F-Stat Treatments	0.637				

Note: standard errors clustered at the school level in brackets; *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Regressions at individual level.

Table C2: Job opportunities of selected occupations by treatment group and year

		(1) Mean value	(2) Rank 1	(3) Rank 2	(4) Rank 3	(5) Rank 4	(6) Rank 5
Sender: researcher							
Info: job opportunities		-0.00181	-0.00589	-0.0207	-0.0105	0.0253	-0.000699
		(0.0223)	(0.0317)	(0.0288)	(0.0286)	(0.0294)	(0.0317)
Sender: institute							
Info: job opportunities		0.0174	-0.0187	0.0376	0.0205	0.0482	0.00394
		(0.0272)	(0.0330)	(0.0335)	(0.0354)	(0.0391)	(0.0437)
Sender: researcher							
Info: job opp. & wages		0.00634	-0.0190	0.0302	0.0441*	0.00259	-0.0236
		(0.0194)	(0.0273)	(0.0344)	(0.0239)	(0.0284)	(0.0296)
Sender: institute							
Info: job opp. & wages		-0.0285	-0.0288	0.000159	-0.0253	-0.0411	-0.0507*
		(0.0211)	(0.0365)	(0.0288)	(0.0264)	(0.0305)	(0.0281)
2019/2020		0.0160	0.0450*	0.0575***	-0.00715	0.0200	-0.0298*
		(0.0145)	(0.0250)	(0.0212)	(0.0230)	(0.0223)	(0.0173)
Sender: researcher							
Info: job opportunities	× 2019/2020	0.00402	-0.0156	-0.0202	0.0586	-0.0542	0.0415
		(0.0237)	(0.0389)	(0.0355)	(0.0378)	(0.0348)	(0.0364)
Sender: institute							
Info: job opportunities	× 2019/2020	-0.0168	-0.0180	-0.0629*	-0.0153	-0.0312	0.0353
		(0.0251)	(0.0365)	(0.0370)	(0.0343)	(0.0406)	(0.0357)
Sender: researcher							
Info: job opp. & wages	× 2019/2020	0.0395	0.0508	-0.0384	0.0233	0.0590	0.0904**
		(0.0272)	(0.0426)	(0.0447)	(0.0384)	(0.0429)	(0.0412)
Sender: institute							
Info: job opp. & wages	× 2019/2020	0.00884	-0.0310	-0.0408	0.00896	0.0160	0.0853**
		(0.0308)	(0.0491)	(0.0406)	(0.0452)	(0.0395)	(0.0345)
Constant		2.826***	2.875***	2.798***	2.813***	2.814***	2.843***
		(0.0116)	(0.0215)	(0.0174)	(0.0160)	(0.0151)	(0.0151)
Observations		28267	27805	27811	27801	27715	27598
F-Stat Non-interacted Treatments		0.799	0.230	0.848	1.927	1.289	0.955
P-value F-Stat Non-interacted Treatments		0.527	0.922	0.496	0.107	0.275	0.433
F-Stat Treatments + Treatments x 19/20		1.704	1.856	1.411	1.623	2.367	1.750
P-value F-Stat Treatments + Treatments x 19/20		0.150	0.119	0.231	0.169	0.053	0.140

Note: standard errors clustered at the school level in brackets; *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Regressions at individual level. Discrepancies in observations are caused by the fact that a few occupations could not be assigned to a level of job opportunities. We do calculate an average score for the other occupations a student selected in this case.

Table C3: Hourly wages of selected occupations by treatment group and year

		(1) Mean value	(2) Rank 1	(3) Rank 2	(4) Rank 3	(5) Rank 4	(6) Rank 5
Sender: researcher							
Info: job opportunities		-0.194**	-0.266**	-0.152	-0.236**	-0.197**	-0.0812
		(0.0906)	(0.124)	(0.107)	(0.106)	(0.0923)	(0.111)
Sender: institute							
Info: job opportunities		-0.0628	-0.0616	-0.0123	0.0110	-0.141	-0.106
		(0.0764)	(0.109)	(0.0866)	(0.0951)	(0.0916)	(0.0824)
Sender: researcher							
Info: job opp. & wages		-0.0284	-0.0886	0.0328	-0.00867	-0.0544	-0.0343
		(0.101)	(0.150)	(0.0991)	(0.109)	(0.104)	(0.120)
Sender: institute							
Info: job opp. & wages		-0.0441	-0.0531	0.126	-0.0951	-0.110	-0.0585
		(0.119)	(0.161)	(0.129)	(0.123)	(0.118)	(0.127)
2019/2020		-0.0111	-0.0542	0.0884	0.00624	-0.0298	-0.0636
		(0.0547)	(0.0774)	(0.0662)	(0.0806)	(0.0615)	(0.0806)
Sender: researcher							
Info: job opportunities	× 2019/2020	0.172	0.225*	0.0570	0.240	0.109	0.177
		(0.108)	(0.130)	(0.145)	(0.146)	(0.139)	(0.133)
Sender: institute							
Info: job opportunities	× 2019/2020	0.0426	0.0362	-0.0325	0.0129	0.0359	0.190*
		(0.0737)	(0.116)	(0.0993)	(0.113)	(0.1000)	(0.115)
Sender: researcher							
Info: job opp. & wages	× 2019/2020	-0.0672	0.0798	-0.176	-0.139	-0.0173	-0.0347
		(0.107)	(0.138)	(0.128)	(0.136)	(0.125)	(0.153)
Sender: institute							
Info: job opp. & wages	× 2019/2020	-0.0786	-0.0604	-0.396***	-0.0962	-0.0110	0.126
		(0.0939)	(0.139)	(0.122)	(0.127)	(0.112)	(0.124)
Constant		16.79***	16.86***	16.61***	16.72***	16.81***	16.85***
		(0.0573)	(0.0778)	(0.0597)	(0.0737)	(0.0633)	(0.0640)
Observations		28267	27805	27811	27801	27715	27598
F-Stat Non-interacted Treatments		1.224	1.215	1.065	1.974	1.320	0.442
P-value F-Stat Non-interacted Treatments		0.301	0.305	0.374	0.099	0.263	0.778
F-Stat Treatments + Treatments x 19/20		0.372	0.108	1.642	1.178	0.167	0.761
P-value F-Stat Treatments + Treatments x 19/20		0.828	0.980	0.164	0.321	0.955	0.551

Note: standard errors clustered at the school level in brackets; *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Regressions at individual level. Discrepancies in observations are caused by the fact that a few occupations could not be assigned to a level of hourly wages. We do calculate an average score for the other occupations a student selected in this case.

Table C4: Heterogeneity job opportunities of selected occupations

	(1) Mean value	(2) Rank 1	(3) Rank 2	(4) Rank 3	(5) Rank 4	(6) Rank 5
Age	0.000312	0.00610	0.00944	-0.0195	0.00515	-0.00153
	(0.00922)	(0.0162)	(0.0149)	(0.0157)	(0.0156)	(0.0144)
3rd year	-0.0121	-0.0122	-0.0239	0.0106	-0.0491*	0.0204
	(0.0213)	(0.0338)	(0.0390)	(0.0354)	(0.0292)	(0.0255)
4th year	-0.00236	0.0173	-0.00492	0.0565	-0.113**	0.0363
	(0.0306)	(0.0543)	(0.0555)	(0.0476)	(0.0525)	(0.0434)
Male	0.212***	0.0497*	0.178***	0.282***	0.291***	0.295***
	(0.0187)	(0.0275)	(0.0272)	(0.0263)	(0.0261)	(0.0235)
QOL score	-0.000243	0.0127	-0.00224	-0.00870	-0.00843	0.00672
	(0.00628)	(0.0104)	(0.0100)	(0.0100)	(0.00879)	(0.00975)
No. of Profiles Available=3	-0.00670	0.0462	0.0844*	-0.0768	0.0316	-0.126***
	(0.0371)	(0.0405)	(0.0467)	(0.0805)	(0.0514)	(0.0418)
No. of Profiles Available=4	-0.00275	0.0561	0.0988**	-0.0457	0.00898	-0.130***
	(0.0375)	(0.0387)	(0.0481)	(0.0824)	(0.0490)	(0.0368)
Constant	2.730***	2.658***	2.536***	3.042***	2.665***	2.766***
	(0.125)	(0.229)	(0.196)	(0.236)	(0.230)	(0.217)
Observations	8576	8425	8422	8419	8394	8350

Note: standard errors clustered at the school level in brackets; *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Regressions at individual-occupation level. Only includes control group students. 2nd year, female students in schools where only 1 profile is available are baseline.

Table C5: Heterogeneity hourly wages of selected occupations by treatment group and year

	(1) Mean value	(2) Rank 1	(3) Rank 2	(4) Rank 3	(5) Rank 4	(6) Rank 5
Age	0.0297	0.0912**	0.0926*	-0.00160	0.00327	-0.0401
	(0.0319)	(0.0431)	(0.0472)	(0.0544)	(0.0434)	(0.0556)
3rd year	0.256***	0.310***	0.198*	0.331***	0.160	0.253**
	(0.0798)	(0.108)	(0.113)	(0.114)	(0.0967)	(0.106)
4th year	0.484***	0.335**	0.522***	0.526***	0.433***	0.521***
	(0.0933)	(0.148)	(0.139)	(0.175)	(0.142)	(0.170)
Male	0.852***	0.759***	0.935***	0.950***	0.943***	0.698***
	(0.0418)	(0.0621)	(0.0735)	(0.0756)	(0.0693)	(0.0635)
QOL score	-0.0711***	-0.0713**	-0.0650***	-0.103***	-0.0592**	-0.0486
	(0.0203)	(0.0321)	(0.0244)	(0.0316)	(0.0262)	(0.0324)
No. of Profiles Available=3	0.0234	-0.0399	0.223	-0.0464	0.0768	-0.133
	(0.119)	(0.153)	(0.156)	(0.161)	(0.108)	(0.152)
No. of Profiles Available=4	0.185*	0.242*	0.408***	0.0880	0.0908	0.0615
	(0.100)	(0.143)	(0.144)	(0.127)	(0.0902)	(0.145)
Constant	16.16***	15.37***	14.88***	16.76***	16.49***	17.23***
	(0.448)	(0.652)	(0.707)	(0.767)	(0.620)	(0.782)
Observations	8576	8425	8422	8419	8394	8350

Note: standard errors clustered at the school level in brackets; *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Regressions at individual-occupation level. Only includes control group students. 2nd year, female students in schools where only 1 profile is available are baseline.

Table C6: Heterogeneity in prior beliefs

	(1) $O_{i,j}^{Prior} - O_j^{Actual}$	(2) $\lvert O_{i,j}^{Prior} - O_j^{Actual}\rvert$	(3) $O_{i,j}^{Prior} - O_j^{Actual} = 0$	(4) $W_{i,j}^{Prior} - W_j^{Actual}$	(5) $\lvert W_{i,j}^{Prior} - W_j^{Actual}\rvert$	(6) $W_{i,j}^{Prior} - W_j^{Actual} = 0$
Age	0.00421	0.000280	0.00645**	0.0371	0.0268	0.00108
	(0.00885)	(0.00744)	(0.00317)	(0.0328)	(0.0255)	(0.00224)
3rd year	0.00935	0.00441	-0.0109**	-0.130	-0.252***	0.00432
	(0.0213)	(0.0134)	(0.00547)	(0.0875)	(0.0595)	(0.00473)
4th year	0.0785***	-0.0192	-0.00269	-0.436***	-0.527***	0.0111
	(0.0287)	(0.0213)	(0.0103)	(0.150)	(0.107)	(0.00720)
Male	0.0677***	0.0471***	-0.0151***	0.644***	0.422***	-0.0167***
	(0.0147)	(0.0110)	(0.00529)	(0.0798)	(0.0487)	(0.00384)
QOL score	0.00345	-0.00286	0.00120	-0.0400	-0.0424*	0.00266**
	(0.00562)	(0.00408)	(0.00159)	(0.0315)	(0.0220)	(0.00125)
No. of Profiles Available=3	0.0419	-0.0412**	0.0209**	0.107	-0.119	0.00500
	(0.0331)	(0.0166)	(0.00861)	(0.234)	(0.0927)	(0.00537)
No. of Profiles Available=4	0.0419	-0.0294**	0.0156**	-0.0550	-0.165*	0.0122**
	(0.0271)	(0.0146)	(0.00767)	(0.223)	(0.0832)	(0.00545)
Rank=2	-0.235***	-0.105***	0.0271***	-0.584***	-0.344***	0.00623
	(0.0114)	(0.0113)	(0.00517)	(0.0356)	(0.0323)	(0.00398)
Rank=3	-0.425***	-0.174***	0.0382***	-0.902***	-0.499***	0.0113**
	(0.0131)	(0.0130)	(0.00647)	(0.0367)	(0.0314)	(0.00508)
Rank=4	-0.611***	-0.211***	0.0511***	-1.192***	-0.546***	0.0110**
	(0.0155)	(0.0128)	(0.00686)	(0.0500)	(0.0348)	(0.00466)
Rank=5	-0.818***	-0.200***	0.0438***	-1.485***	-0.583***	0.0187***
	(0.0200)	(0.0153)	(0.00759)	(0.0529)	(0.0427)	(0.00434)
Constant	-0.241	1.138***	0.111	-3.519***	4.856***	-0.0272
	(0.190)	(0.129)	(0.0678)	(0.712)	(0.540)	(0.0354)
Observations	41842	41842	41842	41825	41825	41825

Note: standard errors clustered at the school level in brackets; *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Regressions at individual-occupation level. Regressions include occupation dummies. Only includes control group students.

Table C7: Treatment effect on posterior beliefs job opportunities by prior belief accuracy

	(1) $\lvert O_{i,j}^{Post} - O_{j}^{Actual}\rvert$	(2) $O_{i,j}^{Post} - O_{j}^{Actual} = 0$
Treated	0.0319***	0.0154*
	(0.00824)	(0.00860)
$\lvert O_{i,j}^{Prior} - O_{j}^{Actual}\rvert$	0.911***	-0.241***
	(0.00456)	(0.00361)
Treated × $\lvert O_{i,j}^{Prior} - O_{j}^{Actual}\rvert$	-0.112***	0.00920*
	(0.00925)	(0.00478)
$\left(O_{i,j}^{Prior} - O_{j}^{Actual} > 0\right)$	-0.0242***	-0.339***
	(0.00600)	(0.00559)
Treated × $\left(O_{i,j}^{Prior} - O_{j}^{Actual} > 0\right)$	-0.0574***	0.0823***
	(0.0115)	(0.00782)
Wage information	0.0205**	-0.00861
	(0.00961)	(0.00949)
Wage information × $\lvert O_{i,j}^{Prior} - O_{j}^{Actual}\rvert$	-0.0197	0.00949*
	(0.0137)	(0.00548)
Wage information × $\left(O_{i,j}^{Prior} - O_{j}^{Actual} > 0\right)$	-0.0293*	0.0168
	(0.0174)	(0.0102)
Constant	0.120**	0.557***
	(0.0522)	(0.0368)
Observations	64579	64579

Note: standard errors clustered at the school level in brackets; *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Regressions at individual-occupation level. Regressions contain occupation dummies. Treated = All treatment groups. Wage info = Treatments 3 & 4.

Table C8: Treatment effect on posterior beliefs hourly wages by prior belief accuracy

	(1) $\lvert W_{i,j}^{Post} - W_j^{Actual} \rvert$	(2) $W_{i,j}^{Post} - W_j^{Actual} = 0$
Treated	0.188***	-0.0250***
	(0.0306)	(0.00909)
$\lvert W_{i,j}^{Prior} - W_j^{Actual} \rvert$	0.914***	-0.0222***
	(0.00406)	(0.000609)
Treated × $\lvert W_{i,j}^{Prior} - W_j^{Actual} \rvert$	-0.0481***	0.00294***
	(0.00660)	(0.000859)
$\left(W_{i,j}^{Prior} - W_j^{Actual} > 0\right)$	0.00122	-0.173***
	(0.0179)	(0.00466)
Treated × $\left(W_{i,j}^{Prior} - W_j^{Actual} > 0\right)$	-0.0337	0.0151**
	(0.0307)	(0.00668)
Wage information	0.0232	0.0928***
	(0.0468)	(0.0114)
Wage information × $\lvert W_{i,j}^{Prior} - W_j^{Actual} \rvert$	-0.156***	-0.00325***
	(0.0127)	(0.00121)
Wage information × $\left(W_{i,j}^{Prior} - W_j^{Actual} > 0\right)$	-0.114**	0.0293***
	(0.0536)	(0.00998)
Constant	0.458***	0.196***
	(0.159)	(0.0227)
Observations	64565	64565

Note: standard errors clustered at the school level in brackets; *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Regressions at individual-occupation level. Regressions contain occupation dummies. Treated = All treatment groups. Wage info = Treatments 3 & 4.

Table C9: Detailed treatment effect on posterior beliefs

	(1) $O_{i,j}^{Post} - O_j^{Actual}$	(2) $\lvert O_{i,j}^{Post} - O_j^{Actual} \rvert$	(3) $O_{i,j}^{Post} - O_j^{Actual} = 0$	(4) $W_{i,j}^{Post} - W_j^{Actual}$	(5) $\lvert W_{i,j}^{Post} - W_j^{Actual} \rvert$	(6) $W_{i,j}^{Post} - W_j^{Actual} = 0$
Sender: researcher						
Info: job opportunities	-0.103***	-0.135***	0.0721***	-0.173**	-0.127**	0.00334
	(0.0135)	(0.0159)	(0.00821)	(0.0722)	(0.0585)	(0.00265)
Sender: institute						
Info: job opportunities	-0.103***	-0.139***	0.0762***	-0.0627	-0.0367	0.00271
	(0.0171)	(0.0120)	(0.00627)	(0.0726)	(0.0591)	(0.00309)
Sender: researcher						
Info: job opp. & wages	-0.135***	-0.138***	0.0714***	-0.870***	-0.914***	0.113***
	(0.0137)	(0.0124)	(0.00563)	(0.0843)	(0.0685)	(0.00590)
Sender: institute						
Info: job opp. & wages	-0.127***	-0.163***	0.0854***	-0.799***	-0.920***	0.110***
	(0.0155)	(0.0140)	(0.00704)	(0.0758)	(0.0690)	(0.00488)
Constant	-0.559***	1.005***	0.249***	-3.247***	4.486***	0.0204
	(0.0706)	(0.0539)	(0.0309)	(0.287)	(0.229)	(0.0142)
Observations	136721	136721	136721	136707	136707	136707
F-Stat Researcher; Job Opp. = Institute; Job Opp.	0.000	0.039	0.185			
P-value Researcher; Job Opp. = Institute; Job Opp.	0.999	0.844	0.667			
F-Stat Researcher; Job Opp. & Wage = Institute; Job Opp. & Wage	0.249	2.038	2.824	0.568	0.005	0.219
P-value F-Stat f2 F-Stat Researcher; Job Opp. & Wage = Institute; Job Opp. & Wage	0.618	0.155	0.094	0.452	0.944	0.640

Note: standard errors clustered at the school level in brackets; *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Regressions at individual-occupation level. Regressions contain occupation dummies.

Table C10: Sender effect on posterior beliefs

	(1) $O_{i,j}^{Post} - O_j^{Actual}$	(2) $\lvert O_{i,j}^{Post} - O_j^{Actual}\rvert$	(3) $O_{i,j}^{Post} - O_j^{Actual} = 0$	(4) $W_{i,j}^{Post} - W_j^{Actual}$	(5) $\lvert W_{i,j}^{Post} - W_j^{Actual}\rvert$	(6) $W_{i,j}^{Post} - W_j^{Actual} = 0$
Sender: Female - Low status	-0.00271	-0.0100	-0.00256	0.388**	0.0337	-0.00948
	(0.0250)	(0.0198)	(0.00918)	(0.188)	(0.129)	(0.0166)
Sender: Male - High status	-0.00166	0.0103	-0.00616	0.133	-0.0303	0.00212
	(0.0263)	(0.0238)	(0.0108)	(0.122)	(0.119)	(0.0154)
Sender: Male - Low status	0.00609	0.0236	-0.00504	0.0675	-0.104	0.0188
	(0.0277)	(0.0250)	(0.0106)	(0.225)	(0.165)	(0.0151)
Male	0.100***	0.105***	-0.0412***	0.583***	0.391***	-0.0330**
	(0.0278)	(0.0240)	(0.0104)	(0.196)	(0.120)	(0.0160)
Sender: Female - Low status × Male	-0.0102	0.0163	0.00345	-0.479*	-0.214	0.0294
	(0.0405)	(0.0315)	(0.0154)	(0.244)	(0.153)	(0.0232)
Sender: Male - High status × Male	0.0312	-0.0128	0.0104	-0.223	-0.127	0.0227
	(0.0438)	(0.0400)	(0.0167)	(0.214)	(0.185)	(0.0206)
Sender: Male - Low status × Male	0.0356	-0.0196	0.0195	0.140	0.0185	-0.000800
	(0.0402)	(0.0376)	(0.0162)	(0.236)	(0.215)	(0.0217)
Constant	-0.690***	0.696***	0.373***	-3.798***	3.643***	0.101*
	(0.118)	(0.0921)	(0.0606)	(0.810)	(0.635)	(0.0587)
Observations	44964	44964	44964	20466	20466	20466

Note: standard errors clustered at the school level in brackets; *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Regressions at individual-occupation level. Regressions contain occupation dummies. Female students with female high status sender are baseline. Regressions (1), (2) and (3) contain treatments 1 & 3. Regressions (4), (5) and (6) conly contain treatment 3.

Table C11: Heterogeneous treatment effects on posterior beliefs job opportunities

	(1) $O_{i,j}^{Post} - O_j^{Actual}$	(2) $\lvert O_{i,j}^{Post} - O_j^{Actual}\rvert$	(3) $O_{i,j}^{Post} - O_j^{Actual} = 0$
Treated	-0.0783**	-0.154***	0.0759***
	(0.0374)	(0.0228)	(0.0117)
Age (demeaned)	0.00340	0.000383	0.00659**
	(0.00877)	(0.00725)	(0.00313)
Treated × Age (demeaned)	0.00432	0.00924	-0.0106**
	(0.0116)	(0.00948)	(0.00461)
3rd year	0.00995	0.00359	-0.0115**
	(0.0210)	(0.0129)	(0.00570)
4th year	0.0902***	-0.0236	-0.00475
	(0.0272)	(0.0202)	(0.00975)
Treated × 3rd year	-0.00166	-0.0449**	0.0368***
	(0.0257)	(0.0194)	(0.00941)
Treated × 4th year	-0.0585	-0.0897***	0.0587***
	(0.0385)	(0.0330)	(0.0167)
Male	0.0632***	0.0452***	-0.00691
	(0.0145)	(0.0107)	(0.00506)
Treated × Male	0.0511***	0.0566***	-0.0287***
	(0.0183)	(0.0129)	(0.00617)
QOL score (demeaned)	0.00438	-0.00309	0.000764
	(0.00609)	(0.00436)	(0.00175)
Treated × QOL score (demeaned)	-0.00702	-0.00333	0.00244
	(0.00736)	(0.00549)	(0.00239)
No. of Profiles Available=3	0.0462	-0.0433**	0.0201**
	(0.0370)	(0.0169)	(0.00983)
No. of Profiles Available=4	0.0504	-0.0330**	0.0129
	(0.0315)	(0.0159)	(0.00906)
Treated × No. of Profiles Available=3	-0.0511	0.0191	-0.0113
	(0.0421)	(0.0243)	(0.0131)
Treated × No. of Profiles Available=4	-0.0787**	-0.0178	0.00822
	(0.0362)	(0.0226)	(0.0120)
Constant	-0.676***	0.972***	0.257***
	(0.0729)	(0.0569)	(0.0319)
Observations	125647	125647	125647

Note: standard errors clustered at the school level in brackets; *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Regressions at individual-occupation level. Only includes control group students. Treated = All Treatments. 2nd year, female students in schools where only 1 profile is available are baseline.

Table C12: Heterogeneous treatment effects on posterior beliefs hourly wages

	(1) $W_{i,j}^{Post} - W_{j}^{Actual}$	(2) $\lvert W_{i,j}^{Post} - W_{j}^{Actual} \rvert$	(3) $W_{i,j}^{Post} - W_{j}^{Actual} = 0$
Wage information	-0.712***	-1.013***	0.117***
	(0.252)	(0.109)	(0.00849)
Age (demeaned)	0.0285	0.0182	0.00130
	(0.0337)	(0.0257)	(0.00217)
Wage information × Age (demeaned)	-0.0235	-0.00156	-0.00640
	(0.0508)	(0.0404)	(0.00433)
3rd year	-0.119	-0.249***	0.00474
	(0.0871)	(0.0553)	(0.00471)
4th year	-0.419***	-0.484***	0.00865
	(0.158)	(0.106)	(0.00709)
Wage information × 3rd year	0.117	-0.00494	0.0167
	(0.141)	(0.110)	(0.0114)
Wage information × 4th year	0.289	0.00619	0.0305*
	(0.220)	(0.182)	(0.0162)
Male	0.618***	0.370***	-0.0173***
	(0.0771)	(0.0493)	(0.00460)
Wage information × Male	-0.0446	0.0921	-0.00908
	(0.0995)	(0.0710)	(0.00765)
QOL score (demeaned)	-0.0390	-0.0389*	0.00185
	(0.0338)	(0.0227)	(0.00123)
Wage information × QOL score (demeaned)	0.0360	0.0216	-0.00283
	(0.0407)	(0.0322)	(0.00234)
No. of Profiles Available=3	0.0950	-0.164*	0.0119**
	(0.258)	(0.0984)	(0.00495)
No. of Profiles Available=4	-0.0871	-0.204**	0.0163***
	(0.247)	(0.0839)	(0.00506)
Wage information × No. of Profiles Available=3	-0.170	0.0753	-0.00454
	(0.280)	(0.137)	(0.00995)
Wage information × No. of Profiles Available=4	-0.261	-0.0408	-0.00568
	(0.262)	(0.115)	(0.00918)
Constant	-3.743***	4.765***	-0.00183
	(0.397)	(0.349)	(0.0213)
Observations	80042	80042	80042

Note: standard errors clustered at the school level in brackets; *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Regressions at individual-occupation level. Only includes control group students. Wage info = Treatments 3 & 4. Treatments 1 & 2 are excluded from this analysis. 2nd year, female students in schools where only 1 profile is available are baseline.

Table C13: Long-term treatment effect on posterior beliefs

	(1)	(2)	(3)	(4)	(5)	(6)
	$O_{i,j}^{Survey} - O_j^{Actual}$	$\lvert O_{i,j}^{Survey} - O_j^{Actual}\rvert$	$O_{i,j}^{Survey} - O_j^{Actual} = 0$	$W_{i,j}^{Survey} - W_j^{Actual}$	$\lvert W_{i,j}^{Survey} - W_j^{Actual}\rvert$	$W_{i,j}^{Survey} - W_j^{Actual} = 0$
Sender: researcher Info: job opportunities	-0.0173	0.00867	-0.0590**	-0.490	0.198	-0.0138
	(0.125)	(0.0612)	(0.0282)	(0.601)	(0.276)	(0.0191)
Sender: institute Info: job opportunities	0.284**	0.166**	-0.0415*	0.167	0.336	-0.0238
	(0.140)	(0.0713)	(0.0244)	(0.519)	(0.260)	(0.0200)
Sender: researcher Info: job opp. & wages	0.269*	0.0508	-0.0530*	1.189*	0.353	-0.0344
	(0.157)	(0.0760)	(0.0317)	(0.604)	(0.254)	(0.0235)
Sender: institute Info: job opp. & wages	-0.207	-0.0380	0.00785	0.121	0.0692	0.0103
	(0.161)	(0.0668)	(0.0322)	(0.543)	(0.258)	(0.0318)
2019/2020	0.238	0.184*	-0.120***	-0.464	0.643	-0.0231
	(0.208)	(0.0962)	(0.0454)	(1.224)	(0.424)	(0.0254)
Sender: researcher Info: job opportunities × 2019/2020	-0.00801	-0.397**	0.205***	-2.186	0.0274	0.115*
	(0.265)	(0.153)	(0.0709)	(1.656)	(0.885)	(0.0674)
Sender: institute Info: job opportunities × 2019/2020	-0.181	-0.389***	0.0716	0.336	-1.107**	0.0777*
	(0.234)	(0.118)	(0.0659)	(1.287)	(0.485)	(0.0396)
Sender: researcher Info: job opp. & wages × 2019/2020	-0.136	-0.302*	0.217**	-0.846	-0.765	0.0378
	(0.265)	(0.165)	(0.0846)	(1.368)	(0.693)	(0.0380)
Sender: institute Info: job opp. & wages × 2019/2020	0.471	-0.286**	0.152**	0.0694	-0.675	-0.00231
	(0.293)	(0.122)	(0.0616)	(1.497)	(0.760)	(0.0428)
Constant	0.216**	1.154***	0.284***	-0.741**	3.857***	0.0887***
	(0.0859)	(0.0422)	(0.0171)	(0.368)	(0.151)	(0.0129)
Observations	2079	2057	1928	1928	1909	1928
F-Stat Non-interacted (Wage) Treatments	2.907	2.044	1.950	2.142	0.992	1.286
P-value F-Stat Non-interacted (Wage) Treatments	0.025	0.093	0.107	0.122	0.374	0.281
F-Stat (Wage) Treatments + (Wage) Treatments x 19/20	0.570	2.622	3.496	0.066	0.805	0.222
P-value F-Stat (Wage) Treatments + (Wage) Treatments x 19/20	0.685	0.039	0.010	0.936	0.450	0.801

Note: standard errors clustered at the school level in brackets; *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. F-Stat Non-interacted Treatments is compared to 2018/2019 control group. F-Stat Treatments + Treatments x 19/20 is compared to 2019/2020 control group.

Table C14: Heterogeneous treatment effect on favorite occupation's job opportunities

	(1) Pr(Fav. Change)	(2) ΔO_j^{Actual}	(3) ΔO_j^{Actual} (Changed)
Treated	0.0249***	-0.00210	-0.226
	(0.00939)	(0.0107)	(0.183)
Age (demeaned)	-0.00159	0.00862*	0.221**
	(0.00257)	(0.00441)	(0.110)
Treated × Age (demeaned)	0.00300	-0.0111*	-0.270**
	(0.00371)	(0.00567)	(0.120)
3rd year	-0.00226	-0.00539	-0.199
	(0.00584)	(0.00781)	(0.161)
4th year	-0.0264***	-0.0103	-0.254
	(0.00896)	(0.0117)	(0.345)
Treated × 3rd year	-0.00773	0.00867	0.303
	(0.00776)	(0.0103)	(0.186)
Treated × 4th year	0.00653	0.0155	0.471
	(0.0129)	(0.0162)	(0.383)
Male	0.0160***	-0.00129	-0.0415
	(0.00528)	(0.00739)	(0.129)
Treated × Male	-0.00538	0.00515	0.0406
	(0.00675)	(0.00933)	(0.151)
QOL score (demeaned)	-0.00357	-0.00153	-0.0126
	(0.00216)	(0.00255)	(0.0401)
Treated × QOL score (demeaned)	0.0000950	0.00280	0.0445
	(0.00267)	(0.00338)	(0.0495)
No. of Profiles Available=3	0.0185*	-0.00702	-0.255
	(0.00945)	(0.00967)	(0.161)
No. of Profiles Available=4	0.0171**	-0.0185**	-0.447***
	(0.00824)	(0.00827)	(0.136)
Treated × No. of Profiles Available=3	-0.00217	0.00296	0.128
	(0.0114)	(0.0132)	(0.209)
Treated × No. of Profiles Available=4	-0.00255	0.0289***	0.528***
	(0.00971)	(0.0110)	(0.176)
Constant	0.0328***	0.0192**	0.530***
	(0.00790)	(0.00791)	(0.146)
Observations	25174	25174	1634

Note: standard errors clustered at the school level in brackets; *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Treated = All Treatments. Regressions at individual level. 2nd year, female students in schools where only 1 profile is available are baseline.

Table C15: Heterogeneous treatment effect on favorite occupation's hourly wages

	(1) Pr(Fav. Change)	(2) ΔW_j^{Actual}	(3) ΔW_j^{Actual} (Changed)
Wage information	0.0207*	0.0101	0.205
	(0.0118)	(0.0405)	(0.764)
Age (demeaned)	-0.00159	0.00141	0.0468
	(0.00257)	(0.0120)	(0.305)
Wage information × Age (demeaned)	0.00254	-0.00235	-0.133
	(0.00487)	(0.0234)	(0.410)
3rd year	-0.00226	-0.0238	-0.468
	(0.00585)	(0.0222)	(0.441)
4th year	-0.0264***	0.0352	1.472
	(0.00897)	(0.0327)	(1.118)
Wage information × 3rd year	0.00239	0.0657	1.112*
	(0.0101)	(0.0441)	(0.661)
Wage information × 4th year	0.00676	0.0222	-0.0980
	(0.0150)	(0.0611)	(1.397)
Male	0.0160***	-0.0161	-0.375
	(0.00529)	(0.0204)	(0.366)
Wage information × Male	0.00181	0.0552	0.549
	(0.00827)	(0.0363)	(0.517)
QOL score (demeaned)	-0.00357	0.00636	0.103
	(0.00216)	(0.00694)	(0.121)
Wage information × QOL score (demeaned)	0.000869	-0.0222*	-0.251
	(0.00327)	(0.0117)	(0.157)
No. of Profiles Available=3	0.0185*	0.0370	0.514
	(0.00946)	(0.0297)	(0.651)
No. of Profiles Available=4	0.0171**	0.00310	-0.00864
	(0.00825)	(0.0246)	(0.554)
Wage information × No. of Profiles Available=3	0.00197	-0.00110	-0.336
	(0.0141)	(0.0431)	(0.786)
Wage information × No. of Profiles Available=4	-0.00126	0.0472	0.461
	(0.0126)	(0.0418)	(0.739)
Constant	0.0328***	0.00990	0.346
	(0.00791)	(0.0228)	(0.581)
Observations	16036	16036	1034

Note: standard errors clustered at the school level in brackets; *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Wage info = Treatments 3 & 4. Treatments 1 & 2 are excluded from this analysis. are excluded Regressions at individual level. 2nd year, female students in schools where only 1 profile is available are baseline.

Table C16: Effect of sender on likelihood favorite occupation changing and its prospects

	(1) Pr(Fav. Change)	(2) ΔO_j^{Actual}	(3) ΔO_j^{Actual} (Changed)	(4) ΔW_j^{Actual}	(5) ΔW_j^{Actual} (Changed)
Sender: Female - Low status	-0.00203	-0.000304	0.00430	0.00653	0.126
	(0.0106)	(0.0163)	(0.252)	(0.0675)	(0.954)
Sender: Male - High status	0.00575	-0.00312	-0.0667	0.0896	1.105
	(0.0118)	(0.0169)	(0.242)	(0.0722)	(0.840)
Sender: Male - Low status	-0.00370	0.0151	0.262	0.111	2.233
	(0.0128)	(0.0146)	(0.227)	(0.0822)	(1.326)
Male	0.000274	0.00166	0.0241	0.0736	0.934
	(0.0102)	(0.0150)	(0.232)	(0.0811)	(1.047)
Sender: Female - Low status × Male	0.00811	-0.00507	-0.105	-0.0000434	-0.0565
	(0.0147)	(0.0194)	(0.293)	(0.115)	(1.493)
Sender: Male - High status × Male	-0.00110	0.0146	0.209	-0.0299	-0.482
	(0.0157)	(0.0238)	(0.334)	(0.118)	(1.383)
Sender: Male - Low status × Male	0.0239	0.00338	-0.119	-0.0904	-2.264
	(0.0172)	(0.0214)	(0.288)	(0.114)	(1.568)
Constant	0.0655***	0.0186	0.284	0.0170	0.229
	(0.00807)	(0.0121)	(0.182)	(0.0480)	(0.642)
Observations	9016	9016	629	4099	315

Note: standard errors clustered at the school level in brackets; *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Regressions at individual level. Regressions contain occupation dummies. Female students with female high status sender are baseline. Regressions (1), (2) and (3) contain treatments 1 & 3. Regressions (4), (5) and (6) conly contain treatment 3.

Table C17: Long-term treatment effect on prospects favorite occupation

	(1) ΔO_j^{Actual} (Experiment)	(2) ΔO_j^{Actual} (Survey)	(3) ΔW_j^{Actual} (Experiment)	(4) ΔW_j^{Actual} (Survey)
Sender: researcher				
Info: job opportunities	0.0416	-0.0825	0.0525	0.0150
	(0.0676)	(0.128)	(0.0865)	(0.294)
Sender: institute				
Info: job opportunities	-0.0334	0.0211	-0.0432	-0.0688
	(0.0310)	(0.126)	(0.0478)	(0.367)
Sender: researcher				
Info: job opp. & wages	0.00715	0.136	0.00715	-0.455
	(0.0353)	(0.160)	(0.0726)	(0.478)
Sender: institute				
Info: job opp. & wages	-0.0236	-0.0768	-0.0236	0.157
	(0.0213)	(0.132)	(0.0432)	(0.334)
Constant	0.0236	0.126	0.0236	0.0394
	(0.0213)	(0.0867)	(0.0432)	(0.175)
Observations	447	447	447	447

Note: standard errors clustered at the school level in brackets; *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. F-Stat Non-interacted Treatments is compared to 2018/2019 control group. F-Stat Treatments + Treatments x 19/20 is compared to 2019/2020 control group.

Table C18: Heterogeneity in change in profiles considered immediately after intervention

	(1) Pr(Same Profile Choice)	(2) No. of Theoretical Profiles	(3) No. of Other Profiles
Treated	0.0543	0.0123	-0.151**
	(0.0495)	(0.0519)	(0.0704)
3 theoretical profiles available	0.197***	-0.0491	-0.185**
	(0.0405)	(0.0506)	(0.0854)
4 theoretical profiles available	0.153***	0.00139	-0.211***
	(0.0376)	(0.0494)	(0.0711)
3 theoretical profiles available × Treated	-0.0572	-0.00201	0.219**
	(0.0550)	(0.0578)	(0.0940)
4 theoretical profiles available × Treated	-0.0816	-0.0278	0.242***
	(0.0550)	(0.0569)	(0.0921)
Wage information	-0.0202	-0.0614	-0.0250
	(0.0438)	(0.0551)	(0.0458)
3 theoretical profiles available × Wage information	-0.0400	0.0950	0.0163
	(0.0581)	(0.0665)	(0.0625)
4 theoretical profiles available × Wage information	0.0516	0.0567	0.00929
	(0.0506)	(0.0600)	(0.0724)
No. of theoretical profiles a priori		0.553***	
		(0.0190)	
No. of other profiles a priori			0.443***
			(0.0329)
Constant	0.538***	0.183***	0.451***
	(0.0356)	(0.0475)	(0.0649)
Observations	10671	5901	4772

Note: standard errors clustered at the school level in brackets; *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table C19: Impact of occupational information on likelihood choosing related profile

	(1) Pr(Chose Profile of Occupation)	(2) Pr(Chose Profile of Occupation)
Treated	-0.00154	0.0150
	(0.00870)	(0.0188)
Job opportunities	-0.00240	
	(0.00184)	
Treated × Job opportunities	-0.0000956	
	(0.00250)	
Wage information	-0.00727	-0.00130
	(0.00828)	(0.0174)
Wage information × Job opportunities	0.00111	
	(0.00244)	
Chose profile a priori	0.784***	0.784***
	(0.00657)	(0.00657)
Hourly wage		0.000731
		(0.000708)
Treated × Hourly wage		-0.000987
		(0.00108)
Wage information × Hourly wage		-0.000146
		(0.00103)
Constant	0.0609***	0.0408***
	(0.00649)	(0.0129)
Observations	52785	52785

Note: standard errors clustered at the school level in brackets; *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table C20: Treatment effect on prospects of chosen study program by gender

	(1) Job Opportunities Chosen Program	(2) Average Hourly Wages Chosen Program
Treated	0.209	0.105
	(0.177)	(0.269)
Male	0.000877	0.858**
	(0.226)	(0.387)
Treated × Male	-0.0310	0.146
	(0.280)	(0.493)
Wage information	-0.346	0.409
	(0.218)	(0.292)
Wage information × Male	0.293	-0.514
	(0.340)	(0.504)
Constant	2.361***	17.15***
	(0.108)	(0.191)
Observations	405	405

Note: standard errors clustered at the school level in brackets; *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Regressions at individual level.

Table C21: Treatment effect on prospects of chosen study program by QOL score

	(1) Job Opportunities Chosen Program	(2) Average Hourly Wages Chosen Program
Treated	0.147	0.151
	(0.160)	(0.204)
QOL score	0.0300	-0.0816
	(0.0634)	(0.0969)
Treated × QOL score	0.0363	0.0145
	(0.0883)	(0.131)
Wage information	-0.220	-0.00441
	(0.189)	(0.252)
Wage information × QOL score	-0.0242	0.313**
	(0.106)	(0.150)
Constant	2.351***	17.49***
	(0.0931)	(0.134)
Observations	399	399

Note: standard errors clustered at the school level in brackets; *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Regressions at individual level.