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

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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 up to seven months later (for job opportunities only). Students who receive the information also tend to change their favorite occupation towards an occupation with better labor market prospects. Administrative records show that students are less likely to choose a profile (i.e., set of subjects) associated with their initial favorite occupation when they receive a more positive information shock about the job opportunities of another occupation. students in the treatment groups are more likely to choose intermediate vocational education over general secondary education after graduating from pre-vocational secondary education. Among those who choose to enroll in intermediate vocational education, we see that the informational content of the treatment affects students' likelihood of enrolling in a study program associated with their initial favorite occupation.

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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 their 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). Additionally, students tend to assign a high probability of ending up in a study program's 'stereotypical' occupation (Conlon & Patel, 2023). 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 the largest online career guidance counseling platform 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 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

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

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, and (ii) administrative data on their education choices during and after secondary education. 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.

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 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.

These changes in beliefs and preferences impact students' educational decisions. Results from administrative data indicate that students are less likely to choose the profile (i.e., set of subjects) associated with their initial favorite occupation when they get a larger positive information shock about the job opportunities of an occupation that is associated with a different profile. Administrative data on enrolment in post-secondary education further shows that students in the treatment groups are more likely to enroll in post-secondary vocational education than general secondary education (i.e., the pre-academic track). This is surprising, as the

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information we provided mostly corrected overestimations about the labor market prospects of occupations associated with post-secondary vocational education. Conditional on enrolling in post-secondary vocational education, we observe that treated students whose initial favorite occupations have the best job opportunities in their choice set are more likely to enroll in study programs associated with this occupation than those in the control group, although the effect is not significant. This 'reinforcement' effect of the treatment is completely negated when students learn that another occupation in their choice set has better job opportunities.

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).

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, take a survey, 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' (i.e., level of theoretical rigor). Pre-vocational secondary education is divided into four 'learning pathways': the basic vocational program, advanced vocational program, combined vocational-theoretical program,

[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).

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 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. Notably, Dutch law dictates that students cannot leave education until they are either eighteen years of age or have a 'starting qualification' (i.e., an intermediate vocational education or senior general secondary education degree). This means that the majority of students cannot leave education after graduating from their pre-vocational secondary education program.

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, schools often make use of online platforms. For this experiment, we partner with a company called Qompas, which

[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

provides such an online platform to schools. On the platform, students can do a number of assignments aimed at helping them learn more about themselves and the choices they will have to make. While students can access the platform at any time, schools generally 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

Qompas recruited schools to participate in the experiment. 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 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 inter-

[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

ested 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 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 B 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 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 are not 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 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.

[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.

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 with ROA. 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 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

[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 different 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.

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 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 initial 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.

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 get 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

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

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. For the analysis of the survey respondents' beliefs and preferences, all above mentioned sample restrictions apply as well. In Table C1 in the Appendix, we show the results of running a regression of answering the survey on treatment status and a number of demographic characteristics for those who were invited to the survey. We show that the survey response rate is not affected by treatment status. We do observe that older students and male students are less likely to respond to the survey.

5.3 Administrative data

To study long-term outcomes of our intervention, we match our experimental data to administrative records at the Dutch Executive Education Agency. The Dutch Executive Education Agency is an agency of the government of the Netherlands responsible for all administrative and informational matters related to Dutch education. As such, they manage all registrations in official education programs; including for secondary and post-secondary education. This means that we are able to track students' educational status at any point in the education system. We matched students to their administrative records using their name and zip code from the Qompas system. We successfully matched 56% of students from the analysis sample. Table C2 in the Appendix shows that treatment status is unrelated to any observable characteristics for the matched sample.

Through the administrative data, we are able to observe their educational status from October of the academic year they went through the experiment (i.e., 2018 or 2019) until 2022. This means we have four years of data for students who went through the experiment in the 2018/2019 academic year and three years for those who went through the experiment in the 2019/2020 academic year.

We are interested in a number of outcomes. First, for the students who went through the experiment in their second year of pre-vocational secondary education, we are interested in their profile choice the following year. We observe this for all such students in our administrative sample. Beyond the profile choice, we are primarily interested in the extensive margin decision to enroll in vocational post-secondary education or general secondary education[14]. and, for those who select into vocational post-secondary education, which study program they enroll in.

A major challenge of analyzing students' decisions through the administrative data is that we observe only educational choices, not occupational choices. As such, we have to match study programs to occupations and their prospects. We do this in the following way.

Each occupation in the Qompas system is associated with a study program identifier. While useful, these identifiers are in most cases deprecated. To match these identifiers to current program identifiers, we use a crosswalk provided by the Vocational Education and Industry Partnership[15]. This crosswalk provides a list of all current and deprecated program identifiers, allowing us to match the program identifiers in the Qompas system to the current program identifiers we obtain as part of the survey and administrative data. Unfortunately, not all matches are 1-to-1 meaning that a program identifier from the Qompas system can be matched to multiple current program identifiers and vice versa. We are able to match 427 of 444 program identifiers we observe in the administrative data to at least one program identifier in the Qompas system.

For our analysis of the administrative data, we are concerned with two outcomes: (i) choosing a program associated with one of the occupations of interest, and (ii) a measure of the labor market prospects of the chosen programs. For the former, we say that a student chose a program associated with the occupation they initially ranked at place k if the program identi-

[14]We did not preregister this outcome

[15]`https://kwalificatie-mijn.s-bb.nl/Lijsten/Output/46604`; Retrieved: 25-05-2024

fier of the study program the student enrolled in had a crosswalk connection to the program identifier in the Qompas system. Note that we do not require this match to be unique. For the labor market prospects of the chosen study program, we take the unweighted average of the labor market prospects of all occupations associated with the current program identifier.

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 C3 and C4 in Appendix C confirm this. Although Table C4 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 C5 and C6 in Appendix C show that male students generally select occupations with better job opportunities and higher hourly wages than female students do. This is in line with the finding of Wiswall and Zafar (2017) that male students care more about remuneration than female students. Students in later years and students from low socioeconomic status neighborhoods choose occupations with higher hourly wages, but no better job opportunities. The latter finding is interesting in particular, since the literature (see e.g., Bleemer and Zafar, 2018; Lergetporer, Werner, and Woessmann, 2021) shows that these students are generally less informed about earnings.

[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 C7 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 Short-term and medium-term treatment impact

6.2.1 Posterior beliefs

Moving to the immediate impact 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 C8 and C9 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 C10 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 C11 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 C12 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 female versus male students. Table C13 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 C14 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 C15 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 C16 shows that we find no further evidence for heterogeneous treatment effects. Table C17 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 C18 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 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 C19 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 C20 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.

Table 5 shows the results based on the administrative data. Column (1) shows the average treatment effect. We observe a decreased likelihood in the treatment of choosing the profile associated with the initial favorite occupation. This could be a product of the fact that overestimations are increasing in the rank of the occupation, meaning that news about the initial favorite occupation is generally the most negative. However, this contrasts with our earlier findings. Column (2) shows how the likelihood of selecting the profile associated with the initial favorite occupation is affected by the treatment and the factual information the student receives about this occupation and the other occupations in their choice set. Receiving information that one of the other occupations in the choice set has better job opportunities than the initial favorite does not have a statistically significant impact on the profile choice, although the sign is as expected. Column (3) interacts the treatment with a dummy indicating whether the student received 'relevant news'. That is, they received more positive news (i.e., $O^{Actual}_{j\neq 1} - O^{Prior}_{i,j\neq 1} > O^{Actual}_{j=1} - O^{Prior}_{i,j=1}$ for some occupation $j \neq 1$, and the wage equivalent). While this method captures the 'surprise' element of the treatment, measurement error in the prior beliefs of the labor market prospects of the occupations may lead to attenuation bias. Additionally, we have to restrict the sample to the 2019/2020 treatment year, as prior beliefs are relevant. Despite these caveats, Column (3) shows a significant impact of the news shock on the likelihood of choosing a profile associated with the initial favorite occupation. Those who receive relevant news are less likely to choose this profile. A joint significance test confirms that this effect is indeed significant over all treatments.

6.2.4 Post-graduation outcomes

The first question we answer about students' educational decisions after graduation using the linked administrative data is whether treated students make different educational decisions at the extensive margin (i.e., are they more or less likely to enrol in intermediate vocational education or general secondary education). As we do not observe any notable differences between the sender arms of the treatments throughout the paper, we combine the treatments and distinguish them only by the information provided: either just job opportunities, or both job opportunities and wages. Column (1) of Table 6 shows the treatment impact on the likelihood of enrolling in vocational post-secondary education at any point after taking part in the experiment. In the control group, about 89% of students enroll in post-secondary vocational education. This number increases to between 90% and 92% in the treatment groups, being marginally significant only for the job opportunities information treatment. Column (2) shows the impact of the likelihood of enrolling in general secondary education, which prepares students for higher education. The effects are of the opposite sign and slightly larger in magnitude and statistically significant for the job opportunities treatment. Joint significance tests for both outcomes show a marginally significant effect only on the likelihood of enrolling in general secondary education.

Taking these results at face value, it looks like our treatment has moved participants from enrolling in general secondary education to enrolling in post-secondary vocational education. This is surprising, as students generally overestimated the job opportunities and hourly wages of their occupations of interest. If students revise their beliefs downwards, as some of our earlier results suggest, one would expect them to be less likely to enroll in post-secondary vocational education.

There are two potential explanations as to why this may not be the case. First, as Conlon(2019) also alludes to, it may be that the information reduces students' uncertainty (i.e., lowers their expected variance) about labor market prospects, which may make the vocational occupations more attractive under risk aversion. A second explanation is that students use the information to update more broadly about the labor market as a whole, rather than occupations specifically. If they think the earnings distribution is more compressed than they initially thought, they may be less willing to invest in obtaining a higher education degree.

Table 7 shows how the treatment and information provided impacts the likelihood of the student selecting a program associated with their initial favorite occupation. Column (1) shows the average treatment effect, which is a precise null, though this may hide substantial heterogeneity based on the information received. Column (2) shows how the likelihood of selecting a study program associated with the initial favorite occupation is affected by the treatment and the factual information the student receives about this occupation and the other occupations in their choice set. The coefficients of the non-interacted treatment dummies can be interpreted as the impact of the intervention when the student did not have any occupation with better job opportunities or wages in their choice set. The point estimate of the treatment is positive in this case, indicating that there may be a slight reinforcement effect of receiving information about the initial favorite occupation. Receiving information that one of the other occupations in the choice set has better job opportunities than the initial favorite completely negates this reinforcement impact of the treatment on likelihood of choosing a study program associated with this occupation. The sign for learning about the wage is negative, as expected, but not statistically significant. Column (3) interacts the treatment with a dummy indicating whether the student received relevant news. We find no significant effects of the information shock on the likelihood of choosing the initial favorite occupation.

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' educational decisions are affected by the treatment. Students in the treatment group are less likely to choose a profile (i.e., set of subjects) associated with their initial favorite occupation when they receive a more positive information shock about the job opportunities of an occupation associated with a different profile. Moreover, students in the treatment groups are more likely to choose intermediate vocational education over general secondary education after graduating from pre-vocational secondary education. Among those who choose to enroll in intermediate vocational education, we observe that the information provided in the treatment has an impact on the decision of whether to enroll in a study program associated with the students' initial favorite occupation.

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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.
Nmbr of Students	164.14	134.24	175.55	147.33	178.00	121.59	158.21	118.49	150.10	110.95	0.82
Nmbr 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: Likelihood of Choosing Profile Associated with Rank 1 Occupation by Info

	(1) ATE	(2) Interaction with Info	(3) Interaction with News Value
Job Opp. Info Treatment	-0.0536	-0.0397	-0.0605
	(0.0454)	(0.0596)	(0.0603)
Wage & Job Opp. Info Treatment	-0.0571	-0.0558	-0.0533
	(0.0416)	(0.0508)	(0.0563)
Better Job Opp. Top 2-5		-0.00624	
		(0.0330)	
Higher Wage Top 2-5		0.00757	
		(0.0292)	
Job Opp. Info Treatment × Top 2-5 Job Opp. Better		-0.0482	
		(0.0478)	
Wage & Job Opp. Info Treatment × Top 2-5 Job Opp. Better		-0.0767	
		(0.0494)	
Job Opp. Info Treatment × Top 2-5 Wage Better		-0.00339	
		(0.0537)	
Wage & Job Opp. Info Treatment × Top 2-5 Wage Better		0.0585	
		(0.0523)	
Relevant Job Opp. Info			0.0506
			(0.0331)
Relevant Wage Info			-0.0642*
			(0.0335)
Job Opp. Info Treatment × Relevant News Job Opp.			-0.120***
			(0.0449)
Wage & Job Opp. Info Treatment × Relevant News Job Opp.			-0.0546
			(0.0479)
Job Opp. Info Treatment × Relevant News Wage			0.0891
			(0.0540)
Wage & Job Opp. Info Treatment × Relevant News Wage			0.0492
			(0.0529)
Constant	0.385***	0.384***	0.399***
	(0.0320)	(0.0391)	(0.0430)
Observations	6510	6510	3612
F-Stat Joint Sig. Test Intereacted Treatments		1.262	3.597
P-value Joint Sig. Test Intereacted Treatments		0.286	0.0300

Note: Standard errors clustered at the school level in brackets; *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Outcome variable indicates whether the student chose a profile associated with their initial favorite occupation. Relevant News Job Opp. (Wage) means that the student learned that $O_1^{Actual} - O_{i,1}^{Prior} < O_k^{Actual} - O_{i,1}^{Prior}$ ($W_1^{Actual} - W_{i,1}^{Prior} < W_k^{Actual} - W_{i,1}^{Prior}$) for some $k \neq 1$. Top 2-5 Job Opp. (Wage) Better indicates that $O_k^{Actual} > O_1^{Actual}$ ($W_k^{Actual} > W_1^{Actual}$) for some $k \neq 1$.

Table 6: Likelihood of enrolling in intermediate vocational and general secondary education

	(1) Started Vocational Education	(2) Started General Sec. Education
Job Opp. Info Treatment	0.0309*	-0.0391**
	(0.0160)	(0.0175)
Wage & Job Opp. Info Treatment	0.0162	-0.0219
	(0.0168)	(0.0186)
Constant	0.889***	0.125***
	(0.0129)	(0.0150)
Observations	11446	11446
F-Stat Joint Significance Treatments	1.861	2.486
P-Value Joint Significance Treatments	0.158	0.0855

Note: Standard errors clustered at the school level in brackets; *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Regressions for graduating are conditional on starting and having gone through the experiment at least four years before potential matriculation.

Table 7: Likelihood of enrolling in program associated with initial favorite occupation

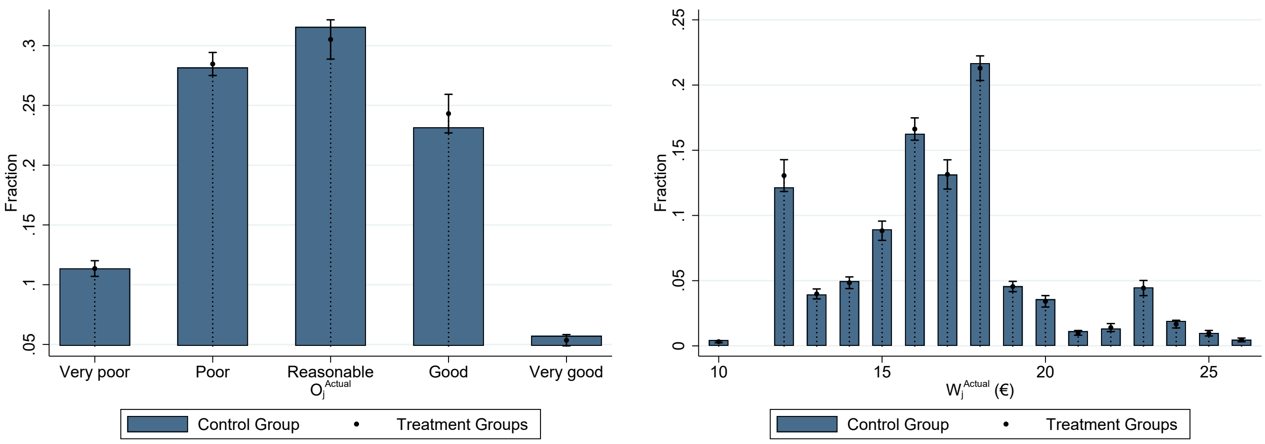
	(1) ATE	(2) Interaction with Info	(3) Interaction with News Value
Job Opp. Info Treatment	0.00901	0.0383	0.0337
	(0.0118)	(0.0233)	(0.0344)
Wage & Job Opp. Info Treatment	0.0121	0.0226	0.0300
	(0.0108)	(0.0195)	(0.0270)
Better Job Opp. Top 2-5		0.0250*	
		(0.0138)	
Higher Wage Top 2-5		-0.0408***	
		(0.0145)	
Job Opp. Info Treatment × Top 2-5 Job Opp. Better		-0.0349**	
		(0.0176)	
Wage & Job Opp. Info Treatment × Top 2-5 Job Opp. Better		0.00287	
		(0.0209)	
Job Opp. Info Treatment × Top 2-5 Wage Better		-0.0160	
		(0.0210)	
Wage & Job Opp. Info Treatment × Top 2-5 Wage Better		-0.0182	
		(0.0213)	
Relevant Job Opp. Info			-0.00401
			(0.0206)
Relevant Wage Info			0.0190
			(0.0176)
Job Opp. Info Treatment × Relevant News Job Opp.			-0.0157
			(0.0315)
Wage & Job Opp. Info Treatment × Relevant News Job Opp.			-0.000598
			(0.0282)
Job Opp. Info Treatment × Relevant News Wage			-0.0151
			(0.0254)
Wage & Job Opp. Info Treatment × Relevant News Wage			-0.0234
			(0.0268)
Constant	0.154***	0.167***	0.131***
	(0.00730)	(0.0150)	(0.0198)
Observations	10624	10624	5690
F-Stat Joint Sig. Test Intereacted Treatments		2.890	0.154
P-value Joint Sig. Test Intereacted Treatments		0.0576	0.857

Note: Standard errors clustered at the school level in brackets; *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Outcome variable indicates whether the student chose a profile associated with their initial favorite occupation. Relevant News Job Opp. (Wage) means that the student learned that $O_1^{Actual} - O_{i,1}^{Prior} < O_k^{Actual} - O_{i,1}^{Prior}$ ($W_1^{Actual} - W_{i,1}^{Prior} < W_k^{Actual} - W_{i,1}^{Prior}$) for some $k \neq 1$. Top 2-5 Job Opp. (Wage) Better indicates that $O_k^{Actual} > O_1^{Actual}$ ($W_k^{Actual} > W_1^{Actual}$) for some $k \neq 1$.

Figures

Figure 1: Job opportunities and hourly wages of selected occupations

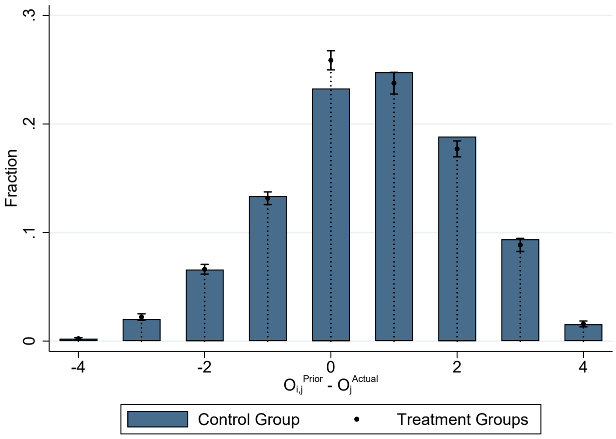
(a) Job opportunities (b) Hourly wages



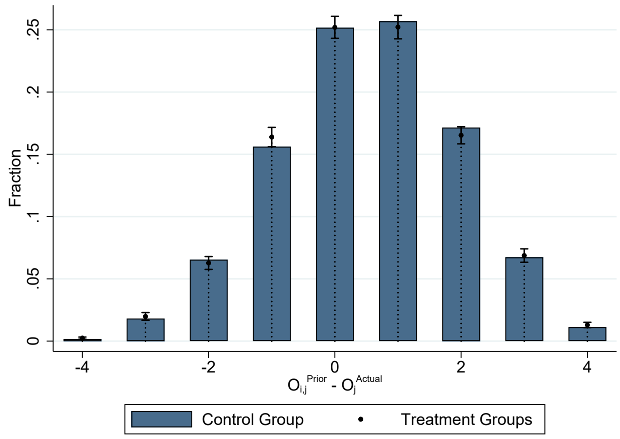
Note: graphic representation of multinomial logit estimation. Standard errors clustered at school level. Blue bars indicate level for control group. Black dots and error bars indicate level for treatment group and 95% confidence interval, respectively.

Figure 2: Prior belief accuracy by relevant group

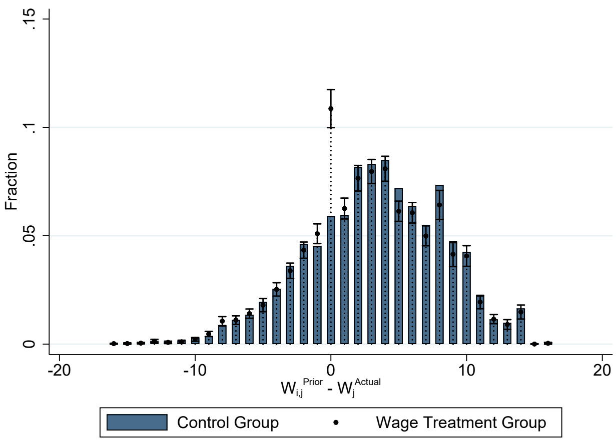
(a) Job opportunities 2018/2019



(b) Job opportunities 2019/2020



(c) Hourly wages 2018/2019

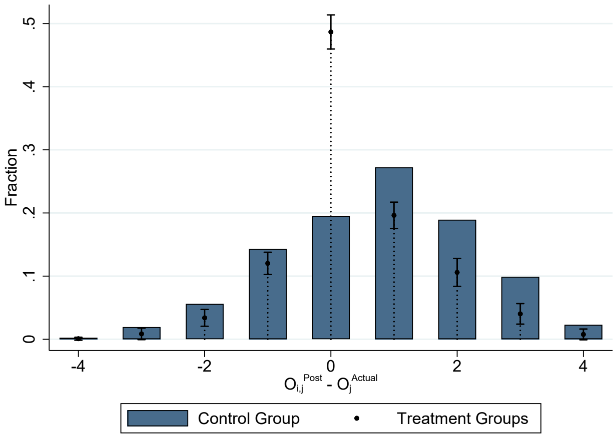


(d) Hourly wages 2019/2020

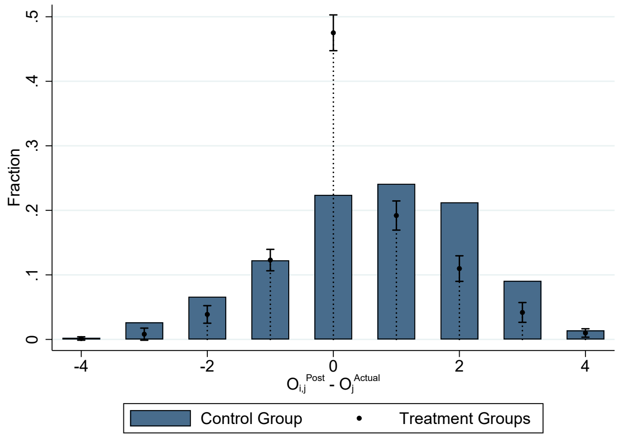
Note: graphic representation of multinomial logit estimation at occupation level. The x-axis displays the degree of overestimation. For the job opportunities, the numbers indicate the overestimation in categories (i.e., -2 denotes an underestimation of two categories, whereas +2 indicates an overestimation 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

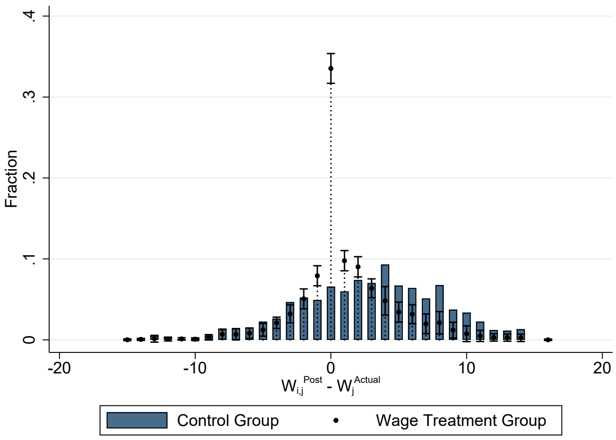
(a) Job opportunities 2018/2019



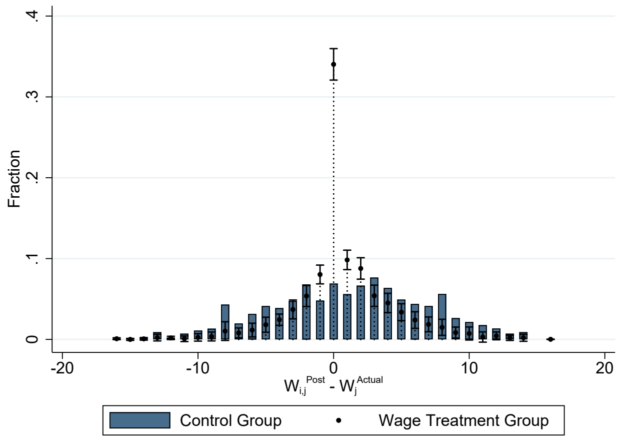
(b) Job opportunities 2019/2020



(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.

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

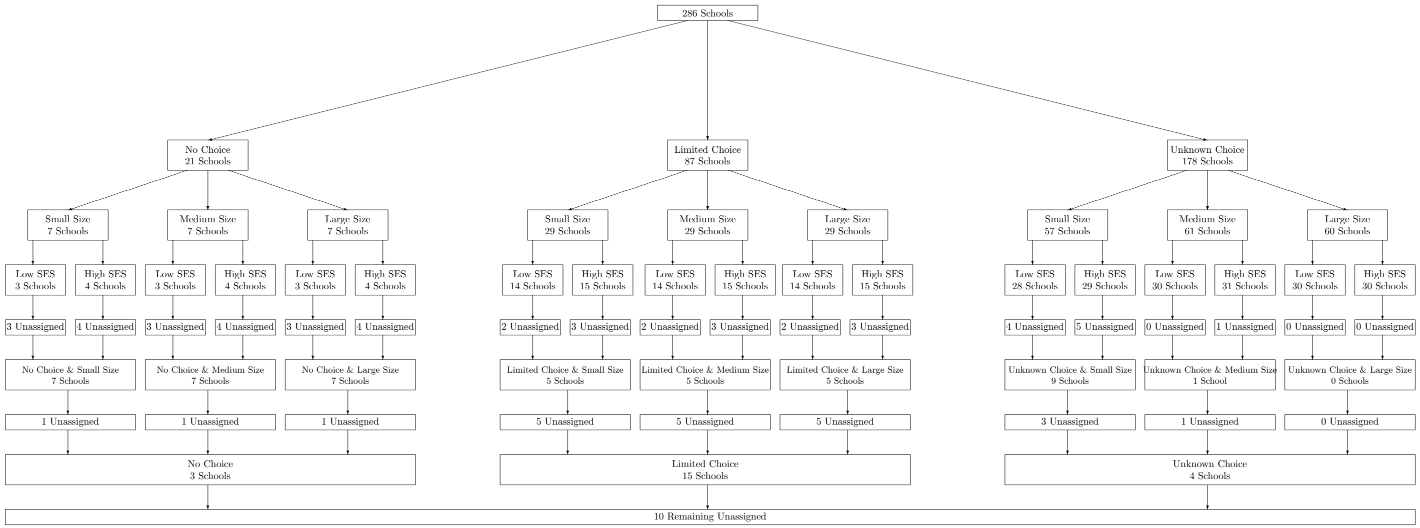
ROA (The Research Center for Education and the Labor Market, part of Maastricht University) and Qompas were asked by the Ministry of Education, Culture and Science (OCW) to do research on the influence of labor market information on the education choices of intermediate vocational education students.

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: Balance table Administrative Data

	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	169.18	134.18	190.71	146.81	182.36	119.97	171.74	117.52	150.10	110.95	0.64
No. of Profiles Available	3.35	1.00	2.92	1.30	3.28	1.15	3.24	1.05	3.14	1.20	0.48
Age	13.99	0.91	14.01	0.95	14.10	0.94	13.94	0.90	14.01	0.95	0.78
Male	0.50	0.50	0.51	0.50	0.55	0.50	0.50	0.50	0.48	0.50	0.44
Grade	2.46	0.63	2.48	0.65	2.54	0.68	2.38	0.60	2.50	0.68	0.71
QOL Score	6.61	1.32	6.57	1.29	6.88	1.19	6.77	1.45	6.47	1.47	0.30

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 C3: 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		1.825	1.421	0.602	1.845	2.291	1.675
P-value F-Stat Treatments + Treatments x 19/20		0.125	0.228	0.661	0.121	0.060	0.156

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 C4: 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		0.512	0.152	1.514	1.275	0.685	0.688
P-value F-Stat Treatments + Treatments x 19/20		0.727	0.962	0.199	0.280	0.603	0.601

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 C5: 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 level. Only includes control group students. 2nd year, female students in schools where only 1 profile is available are baseline.

Table C6: 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 level. Only includes control group students. 2nd year, female students in schools where only 1 profile is available are baseline.

Table C7: Heterogeneity in prior beliefs

	(1)	(2)	(3)	(4)	(5)	(6)
	$O_{i,j}^{Prior} - O_j^{Actual}$	$\|O_{i,j}^{Prior} - O_j^{Actual}\|$	$O_{i,j}^{Prior} - O_j^{Actual} = 0$	$W_{i,j}^{Prior} - W_j^{Actual}$	$\|W_{i,j}^{Prior} - W_j^{Actual}\|$	$W_{i,j}^{Prior} - W_j^{Actual} = 0$
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 C8: 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 $\times$ $\lvert O_{i,j}^{Prior} - O_{j}^{Actual} \rvert$	-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 $\times$ $\left(O_{i,j}^{Prior} - O_{j}^{Actual} > 0\right)$	-0.0574***	0.0823***
	(0.0115)	(0.00782)
Wage information	0.0205**	-0.00861
	(0.00961)	(0.00949)
Wage information $\times$ $\lvert O_{i,j}^{Prior} - O_{j}^{Actual} \rvert$	-0.0197	0.00949*
	(0.0137)	(0.00548)
Wage information $\times$ $\left(O_{i,j}^{Prior} - O_{j}^{Actual} > 0\right)$	-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 C9: 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 C10: 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 C11: 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 C12: 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 C13: Heterogeneous treatment effects on posterior beliefs hourly wages

	(1) $W_{i,j}^{Post} - W_j^{Actual}$	(2) $\|W_{i,j}^{Post} - W_j^{Actual}\|$	(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 C14: Medium-term treatment effect on posterior beliefs

		(1) $O_{i,j}^{Survey} - O_j^{Actual}$	(2) $\lvert O_{i,j}^{Survey} - O_j^{Actual}\rvert$	(3) $O_{i,j}^{Survey} - O_j^{Actual} = 0$	(4) $W_{i,j}^{Survey} - W_j^{Actual}$	(5) $\lvert W_{i,j}^{Survey} - W_j^{Actual}\rvert$	(6) $W_{i,j}^{Survey} - W_j^{Actual} = 0$
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 C15: 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 C16: 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 C17: 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 C18: 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 C19: 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 C20: 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; *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.