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ABSTRACT

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

We run a field experiment in which we provide information to students about job opportunities and hourly wages of occupations they are interested in. The experiment takes place within a widely-used career orientation program in the Netherlands, and involves 28,186 pre-vocational secondary education students in 243 schools over two years. The information improves the accuracy of students' beliefs and leads them to change their preferred occupation to one with better labor market prospects. Administrative data that covers up to four years after the experiment shows that students choose (and remain in) post-secondary education programs with better job opportunities and higher hourly wages as a result of the information treatment.

JEL Classification: C93, D83, I26, J24

Keywords: education choice, labor market information, field experiment

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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 et al., 2016; Kirkeboen et al., 2016). It is also important for society, as it affects future shortages and excess supply of labor in important occupations (Smeets et al., 2025). 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 et al., 2018; Hastings et al., 2016) and careers (Arcidiacono et al., 2012; Betts, 1996).[1] As a result, many teenagers end up choosing programs that have a bleak outlook, both in terms of job opportunities and wages.

Motivated by this issue, a number of experimental studies have focused on the impact of providing information about the earnings prospects of different majors on enrollment decisions. 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 et al., 2019; Conlon, 2019; Hastings et al., 2015; Kerr et al., 2020). An explanation for this puzzling finding may be that a large part of the differences in earnings prospects by field of study can be explained by occupation choice (Altonji et al., 2012), which students seem to be well aware of (Arcidiacono et al., 2012). In line with this, the returns to skills learned in post-secondary education depend strongly on the industry of employment (Cnossen et al., 2025). Information about average earnings by field of study may further be misinterpreted because students overestimate the share of graduates ending up in a major's stereotypical occupation (Conlon and Patel, 2025). In this paper, we therefore put occupations front and center, and ask whether *occupation-specific* information about job opportunities and hourly wages affects beliefs, preferences over occupations, and educational choices of students.

We conduct a natural 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 and shifts students' preferences over occupations immediately after the

[1] See Giustinelli (2023) for a more comprehensive overview of studies on students' subjective expectations about the returns to education.

intervention, and up to one and a half year later through a post-experimental survey. Using linked administrative data, we also study whether our information influences their actual education choices up to four years after the experiment. Our multi-year field experiment involves 28,186 students at 243 different schools for pre-vocational secondary education in the Netherlands. The students take part in the experiment in grades 8 to 10 and are between 13 and 16 years old.[2]

The experiment takes place as part of a widely-used career orientation program. In this computer-based program, students perform numerous assignments that help them discover what they are interested in, what they are good at and, ultimately, which occupations would be a good fit for them. As part of one of these assignments, students take an extensive 'skills and personality' test that results in a short-list of twenty (out of 353) occupations that fit their abilities and interests best according to the answers they provided. Our experiment starts right after completing this test.

Our experiment proceeds as follows. First, we ask students in which secondary-school specializations (i.e., set of subjects; 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 the most. We then ask them to state their beliefs about the job opportunities and hourly wages for these five occupations, and to rank the occupations 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, supplement that with information about the hourly wages of the selected occupations. Our motivation for restricting information provision to job opportunities in the first treatment is possible information overload. Processing information about both job opportunities and hourly wages for five occupations may be too demanding, rendering the treatment less effective. Students at the remaining schools do not receive any information and form our control group.

In a sub-randomization arm that we explain in more detail later, we alter the 'sender' of the information to be either a labor market research institute, or a specific researcher (male or female, and experienced or less experienced) working at that institute. The identity of the 'sender' is randomized within the treatment group. This variation in sender identity is inspired by the literature on role models that we discuss below.

Next, students in both the treatment and control group watch an information video

[2]In the Dutch education system, students are tracked at an early age. Grades 8 to 10 are the second to fourth (and final) year of pre-vocational secondary education in the Netherlands. Vocational education represents 37% of student enrollment among 15 to 19 year olds in the OECD. In the Netherlands, and countries such as Austria, Switzerland, and the Czech Republic, this figure is above 50% as Table B1.2 in OECD (2023) shows.

about the Dutch economy that does not convey any information about specific occupations. This gives students some time to reflect. Finally, we ask students to again state their beliefs about wages and job prospects and to re-rank the occupations. 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 students' beliefs and preferences, and (ii) up to four years of administrative data on their education choices during and after secondary education.

Our results are as follows. In line with the earlier studies (Baker et al., 2018; Betts, 1996; Hastings et al., 2016), we find that students have highly inaccurate beliefs about the job opportunities[3] and hourly wages of the occupations that they like. They tend to overestimate both, particularly for those occupations they like the most. Our information intervention is effective in correcting beliefs. Immediately following the intervention, 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 and 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. We find, unexpectedly, that the sample of surveyed students differed from the full sample in the experiment. The former was less likely to change their favorite occupation for one with better prospects during the experiment than the latter.

Using administrative data containing three to four years of student enrollment decisions, we find that our information intervention impacts students' post-secondary education enrollment decisions. Students who receive information about labor market prospects enroll (and remain) in study programs associated with occupations that provide better labor market prospects. Job opportunities of chosen programs are up to 2.9% better for treated students, and hourly wages 1.25% higher. This impact appears to be mostly driven by shifts within sectors, rather than across. We show that the sectoral composition of secondary education enrollment remains largely unchanged, though we see a small shift away from services and education, and towards healthcare, using the ISCED classification of education programs.

[3] We elicit beliefs and provide information about the job opportunities in a qualitative manner, so inaccuracies may stem from students having beliefs that deviate from the truth, or not agreeing on the terms we assign.

Our study contributes to a growing body of literature on the role of labor market expectations in education choices. As stated, previous studies have invariably found that students have highly noisy beliefs about the labor market returns of different study programs (Baker et al., 2018; Betts, 1996; Hastings et al., 2016). There are, however, some notable heterogeneities. Students who are more concerned with the labor market prospects of programs are less likely to overestimate these prospects (Hastings et al., 2016) and there is a substantial gender difference in concerns about these prospects (Wiswall and Zafar, 2017). Men tend to care more about pecuniary outcomes, whereas women care more about job security and flexibility. In line with this, 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. Our heterogeneity analysis shows that, in response to our treatment, male students tend towards occupations with higher wages, whereas female students gravitate towards occupations with better job opportunities. 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), likely because they lack access to reliable information from, e.g., parents (Bleemer and Zafar, 2018; Lergetporer et al., 2021; Qiu, 2025). These information constraints are a plausible explanation for why students from low socioeconomic status backgrounds attend programs with lower expected earnings (Campbell et al., 2022). 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. However, in contrast to Campbell et al. (2022), we find that students from high socioeconomic status household choose occupations with lower hourly wages. There appear no large differences in treatment effects between students from low and high socioeconomic status backgrounds.

Prior evidence shows that labor market conditions affect educational decisions. In times of higher unemployment, students generally select fields of education with higher wages and better job opportunities (Blom et al., 2021). Which field students go into depends on local labor market conditions. Sector-specific local labor demand shocks drive students to choose different community college programs (Acton, 2021) and majors (Weinstein, 2022). Because of this response, labor market conditions have important implications for the composition of the workforce. For instance, when general local labor market conditions are poor, individuals with higher value-added sort into the teaching profession (Deneault, 2025). While these papers show the importance of the labor market in shaping educational decisions, they do not speak to the issue of how best to provide labor market information to students.

A number of field-experimental studies have tested the effects of different interventions aimed at improving students' knowledge about the returns to education. Evidence from the

Dominican Republic shows that providing students with information about the returns to attending secondary school increases enrollment (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 (Kerr et al., 2020; Bonilla-Mejía et al., 2019). There is some evidence that it does increase intended enrollment, particularly for students from low socioeconomic status backgrounds (Oreopoulos and Dunn, 2013; McGuigan et al., 2016; Peter and Zambre, 2017).

Most closely related to our paper are a number of studies that focus on providing information about the returns to specific study programs or institutions. These generally find some impact on enrollment decisions, though the exact margin of impact differs. Hastings et al. (2015) show that, after being provided with such information, students are more likely to enroll in higher-return study programs. Similarly, Ballarino et al. (2022) show that informed students were *less* likely to enroll in fields of study with *weak* labor market prospects, though this seems to be driven by selection out of higher education for students who would otherwise enroll in these fields. Bonilla-Mejía et al. (2019) find no such impact, but do find that students are more likely to enroll in prestigious institutions. Lastly, Conlon (2019) shows that students are more likely to enroll in a study program about which the student receives information, regardless of the content of this information. All-in-all, the results of information interventions targeting field of study are mixed.

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. 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. Lastly, Riley (2024) shows that watching a movie featuring a female role model (Queen Katwe) positively affects female students' test scores in particular. Our inclusion of the different 'information senders' (the labor market research institute or a researcher from this institute, either senior or junior, either female or male) provides a further look into how the characteristics of a person providing information affects the degree to which it is used. However, we find that the identity of the sender of the information that is mentioned in the intervention is inconsequential for the subsequent beliefs and preference ranking of occupations. In short, we find no evidence that role models play an important role in our context.

The main contribution of our study 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 broad study programs. Such information has been shown to be effective in motivating occupational transitions among unemployed job seekers (see e.g., Altmann et al., 2022; Belot et al., 2019, 2025). Our setting provides a unique opportunity to do so in education, as vocational education programs are strongly tied to occupations. Furthermore, the information we provide is highly detailed (based on 113 occupational groups), and tailored to the student's tested and self-declared interests. 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 orientation program actually used as part of students' curriculum in school, and thus more closely matches the definition of a natural field experiment (Harrison and List, 2004). The intervention is low-cost, as it relies on readily available information, and easy to replicate in similar career orientation programs. Our partner company is currently investigating the optimal way to implement occupational information provision at different levels of secondary education in its online career orientation platform.

Beyond the design of our intervention, the data we collected allows us to provide a detailed picture of how students use the information. The time-stamped experimental data allows us to show how much time students spend to process the information we provide them with, and whether this is sufficient for knowledge to persist in the long-term. The high-quality administrative data covering multiple years further enables us to follow students for up to four years, and to estimate the impact of the treatment not only on initial decisions but to also verify that these decisions are not reversed in subsequent years.

A final unique feature of our study is that we treat students in different grades; 8 to 10, specifically. This allows us to analyze what the impact of our information treatment is at different stages of students' educational careers. Students in the eighth grade still have to decide on their secondary school specialization, whereas those in the tenth grade will graduate within a year and have to soon decide on whether to go to post-secondary education and, if so, which program to enroll in.

All-in-all, our study provides evidence that labor market information about careers that students are interested in is useful in helping them choose programs that provide better prospects upon graduation. Future studies may want to explore how to further improve the effectiveness of such information. For instance, by exploring the impact of different labor market indicators (such as, e.g., wage growth, flexibility, and job satisfaction) and ways of presenting the information.

The rest of this paper is structured as follows. Section 2 explains the institutional context: the Dutch education system and career orientation 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 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.[4] 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). There are four such 'learning pathways': the basic vocational program, advanced vocational program, combined vocational-theoretical program, and theoretical program (Nuffic, 2019). In the theoretical program, students mostly take general subjects. The combined program drops one general subject in favor of four hours of vocational training, but is otherwise the same. In the basic and advanced vocational programs, students receive approximately 12 hours of vocational training instead of general subjects. General subjects are taught at a lower level compared to the combined and theoretical programs, with the level at the advanced vocational program being above that of the basic vocational program. Within the learning pathways, students also choose a 'profile', which determines the subjects they are taught (Government of the Netherlands, n.d.a).[5] Both the learning pathway and profile 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 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). The vast majority of students can therefore not leave education after graduating from their pre-vocational secondary education program. This leaves them with essentially two options: move on to post-secondary intermediate vocational

[4]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).

[5]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.

education or enroll in a different (sub)track of secondary education. Graduates from all learning pathways are eligible to enroll in intermediate vocational education. Programs in intermediate vocational education generally train students for a specific occupation. 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). In recent years, over 95% of students in the basic vocational, advanced vocational, and combined vocational-theoretical program directly enroll in intermediate vocational education. Of the remaining 5%, approximately half enrolls in intermediate vocational education at some later point in time. Among graduates from the theoretical program, about 80% directly enrolls in intermediate vocational education, with an additional 5% enrolling at a later point in time. The remaining 15% most commonly enrolls in *secondary general education*, a track that prepares students for higher vocational education (Dutch Ministry of Education, Culture and Science, n.d.).

Schools are required by law to provide career orientation counseling to students. To structure their counseling efforts, schools often make use of programs offered by private companies. For this experiment, we partner with a company called Qompas, developer of the most commonly used career orientation program for Dutch pre-vocational secondary education. As part of the program, students complete a number of assignments aimed at helping them learn more about themselves and the education choices they will have to make. While students can access the program at any time, schools generally use Qompas during their career orientation classes at set times during the school week. Usually, students individually go through the assignments in a classroom setting. 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 the so-called occupation assignment. While the Qompas system has a suggested order for doing the different assignments, schools decide in which year students actually do it. Schools usually let students do the occupation assignment in the second, third or fourth year of education. We expand on this 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 as users in the system, which comprises about a third 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 through a system message as well as an email that an experiment would take place. 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. They further explained to schools that the research would be conducted by way of an experiment within their career orientation program. Schools also received contact details of the person responsible for the experiment at Qompas in case they had any questions or complaints. Online 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 risk of spillovers between students, as they generally complete their assignments in the classroom. 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 similar 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 to the different groups. We discuss the transition from assignment to actual participation in detail in Section 4.3.

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.[6] 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 Interior and Kingdom Relations (2018). 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

[6]Data on the number of students in the school itself is provided as open data by the Dutch education executive agency (Dutch Executive Education Agency, 2018)

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. Note that in our experimental data, we have access to the quality of life indicator score of the individual student's neighborhood.

We used a block design to randomize. Because the profile choice is one of our outcome variables and largely determines the variety of occupations the students are likely to be interested in, we first sought balance on this dimension. We divided the schools into three groups: predetermined choice (only one theoretical profile available), limited choice (two or three theoretical profiles available), and an unknown number of 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 of the 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 of the 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 Online Appendix B provides a visual representation of the procedures.

4 Experimental Design

In this section, we describe each stage of the experiment in detail in chronological order. The accompanying Online Appendix D shows screen captures of the screens students in each of the treatment and control groups see in the experiment.

[7]This information is available in the data set referred to in footnote 6.

4.1 Occupation test

Our intervention is preceded by (and makes use of information collected in) an extensive occupational interest test. The test was designed by Qompas and had already been used for a number of years before our experiment. During the occupational interest test, Qompas asks students to answer 90 questions about themselves and their attitudes 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 a reflective assignment that students must do, which contains our intervention.

4.2 Elicitation of baseline information

During the reflective assignment, we collect baseline data on students' preferences and beliefs. We ask students about their intended profile choice, which 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, using a slider, we ask students to state their beliefs about the job opportunities and gross hourly wages of the five occupations they selected.[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. The Dutch income tax system features a quite sizeable tax-free sum. Consequently, for most youngsters, gross earnings equal net earnings.

4.3 Information provision

After we elicit the baseline preference ranking and beliefs about the labor market prospects, we present students at schools assigned to one of the treatment groups with information about the labor market prospects of the occupations they selected. Control group students do not receive 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 gross 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 the job opportunities of 113 different occupational groups over a period of six years; in line with the time when students who take part in the experiment are expected to enter the labor force.[10] To construct the information set, we matched the occupations in the Qompas system to these occupational groups. The job opportunities forecasts are taken from Fouarge et al. (2017). For the hourly wages, ROA relied on Dutch Labor Force data, matched to administrative data for 2016, the most recent year available when we designed the experiment.

In treatment groups 1 and 3, we tell students that the information is provided by a researcher affiliated with ROA. This sender is randomized at the individual level within these treatments. The sender takes four different identities: inexperienced male researchers, experienced male researchers, inexperienced female researchers, and experienced female researchers.[11] For each identity, one of four possible names is chosen. 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 tell students that ROA provides them with the information.

[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 for the accreditation of new study programs and several stakeholders in the Netherlands, including information websites for students such as https://www.studiekeuze123.nl/. The forecasts for job opportunities reflect the expected ratio between expected future supply of labor and demand.

[11] In Dutch: 'beginnend onderzoek(st)er' and 'ervaren onderzoek(st)er'.

[12] With their consent, we use the actual names of ROA employees.

4.4 Video

Next, we show students in all groups a short video about work in general.[13] The video mentions neither any particular occupations nor 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 five occupations for which the job opportunities are all forecasted to be "very bad", "bad" or "reasonable". We suggest to those students (in both the control and treatment groups) a set of alternative occupations with better labor market prospects and provide information on the day-to-day activities of these occupations. 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. 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.

As just described, 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 who were presented with alternative occupations also received information about the job opportunities of these occupations, as well as about their initial set of occupations. Because of this error, we remove all students who were suggested alternatives from our analyses using survey and administrative data. That also means we do not consider the alternative occupations in our analysis at all.

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.

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

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. Of the 286 school that agreed to part-take in the experiment, we received data from 249 schools. The attrition of 37 schools is not a threat to our internal validity, as schools did not know their treatment assignment before going through the experiment. In total, we have 40,176 observations from participating schools. After sequentially removing school administrators (48), first-year students (1,855; exclusion specified in our pre-analysis plan), observations without an initial ranking of occupations (1,082), and observations with a create date before August 1st, 2018 (5,023), we are left with 32,168 students from 244 schools. Of the 32,168 eligible students, 3,982 changed their initial preference ranking on a different day than they created it. We remove these students from the sample as well ('no mutation' restriction), because we cannot exclude that these students went through the experiment multiple times, which would make our data (the set of initially selected occupations as well as students' beliefs) unreliable. Online Appendix Table C1 reassuringly shows that these variables are not significantly related to treatment status.

After imposing our restrictions, we are left with 28,186 students from 243 schools.[14] Columns 4 to 7 of Table 1 show how these numbers relate to the number of assigned schools. Table 2 shows that demographic covariates are also balanced between the control and treatment groups.

5.2 Survey data

In addition to the experimental data collected through the career orientation program, we conducted a survey among graduating students in the 2019/2020 school year. For this purpose, we invited all students who took part in our experiment in their third year in the 2018/2019 school year and students who took part in their fourth year in the 2019/2020 school year. The survey was fielded between the 15^{th} of April and the 20^{th} of May, 2020, around the time at which they would need to finalize their enrollment decisions for next school year. To incentivize responses, we announced that we would raffle off 20 €25.- vouchers for a large Dutch e-tailer among survey respondents. The survey was sent to 9,510 students of which 1,061 responded, implying a response rate of 11%.

Again, we impose a number of sample restrictions. First, we only consider students who

[14]The implementation costs for programming the intervention were €39,600, or just €2.13 per treated student at its current scale.

went through the experiment on a single day to avoid those who returned to their prior ranking a later point in time ('no mutation' restriction). Second, we include only students who did not see the alternative occupations ('no alternative occupation' restriction). After we impose our sample restrictions, we are left with 3,580 survey invitees, and 355 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. Table C2 in Online Appendix C shows that answering the survey is not related to treatment status. We do observe that male students are less likely to respond to the survey. The differences in the average grade the students are in, and whether they took part in the 2018/2019 or 2019/2020 school year can be ascribed to the targeting of graduating students.

5.3 Administrative data

To study long-term effects of our intervention, we match our experimental data to administrative records at the Dutch Executive Education Agency (DUO). DUO is an agency of the Dutch Ministry of Education responsible for all administrative and informational matters related to education. DUO manages all registrations in official education programs; including for secondary and post-secondary education. The data allows us to track students' educational status at any point in the education system. DUO matched students to their administrative records using their name and zip code from the Qompas system. To ensure the resulting matches from this method are accurate, we impose a number of additional sample restrictions. First, we remove 32 experimental observations for which we find a duplicate match. Second, we drop students for whom the grade they are registered in differs between the administrative and experimental data. Last, we impose our 'no mutation' and 'no alternative occupation' restrictions. Of the 17,596 students who meet the 'no mutation' and 'no alternative occupation' restrictions in our original sample, we match 9,305 (53%) to DUO's administrative data. Table C3 in the Online Appendix shows that, like for our full experimental sample, treatment status is unrelated to any observable characteristics for the matched sample. We also do not see large differences in the characteristics of those matched and those not matched. One thing to note is that the differences in the proportion of schools that have more than three to four theoretical profiles, or more than three practical profiles available are fairly large (though not significant). This is remarkable, given that we stratified on the number of profiles available in schools. The reason this could happen is that at the time of randomizing, Qompas did not know exactly how many profiles were available in 187 out of 286 schools. In the administrative data, we can observe this cleanly based on enrolled students. Our stratification was therefore not particularly effective regarding this aspect.

Through the administrative data, we are able to observe students' 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. For all years, we observe highly detailed enrollment information for both secondary and post-secondary education. While students are in secondary education, we observe the grade, track, subtrack, learning pathway, and profile they are enrolled in. For post-secondary education, we observe the exact program they are enrolled in and match it to the occupations in the Qompas system.[15]

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.

There are some interesting patterns in the selection of the occupations. Tables C4 and C5 in Online 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 do female students. The latter finding is interesting in particular, since the literature (see e.g., Bleemer and Zafar, 2018; Lergetporer et al., 2021) shows that male students are generally less informed about earnings. The tables further show that students in the practical

[15]Each occupation in the Qompas system is associated with a study program identifier. While useful, it turned out that 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 (2024). 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 450 of 480 program identifiers we observe in the administrative data to at least one program identifier in the Qompas system.

pathway tend to pick occupations with worse job opportunities and lower hourly wages. Given their lower level of education, the opportunities of occupations available to them are likely worse. There is little further heterogeneity for job opportunities, though there is some indication that students in schools with more profiles available pick occupations with better job opportunities as their initial favorites. For wages, there is more heterogeneity. Students in later years select occupations with substantially higher hourly wages. The same is true for students in schools with more profile options, although the impact is much smaller. Notably, the coefficient of the quality of life indicator score is negative, meaning that students from high socioeconomic status backgrounds choose occupations with somewhat lower hourly wages.

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 must be due to the programming error that enabled students to correct their initial beliefs, as discussed in Section 4.3. As stated there, we will not include students from the 2018/2019 school year in our analyses whenever we make us of prior beliefs. 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, as shown in the panels on the right-hand side of Figure 2.

The figure shows left-skewed distributions, which indicates that students tend to overestimate the labor market prospects of their preferred occupations. This observation should be interpreted with some caution, however. First, job opportunities are measured using ordinal categories. Differences between students' beliefs and the provided information could thus be caused by misperceptions about the true job opportunities as well as by differences in the interpretation of what sort of job opportunities are (very) poor, reasonable and (very) good. We have no way of disentangling the two. Second, the distribution of 'true' values is right-skewed, see Figure 1. That is, occupations with very poor and poor job opportunities and hourly wages between €10.- and €17.- are more common than occupations with good and very good job opportunities and hourly wages €19.- to €26.-. If students' answers tended towards the scales' midpoints, Figure 2 may show excessive overestimations.

That said, Figure 3 does lend credibility to the idea that students overestimate the labor market prospects of the occupations they are most interested in. The graph shows that the overestimation of both job opportunities and hourly wages is most pronounced for the initially highest-ranked occupation, and gradually decreases with the occupation's rank. There are two likely explanations for this: (i) students may attempt to justify their occupational preferences by assigning better labor market prospects to those they have ranked higher, or (ii) students may be more interested in occupations with better labor market prospects, which would put those for which they draw higher beliefs at the top of their ranking.

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 $|W_{i,j}^{Prior} - W_j^{Actual}|$. Second, we analyze how often beliefs are exactly correct (i.e., $O_{i,j}^{Prior} - O_j^{Actual} = 0$ and $W_{i,j}^{Prior} - W_j^{Actual} = 0$).

Table C6 in Online Appendix C shows heterogeneity in belief accuracy among control group students.[16] The regressions in this table include occupation fixed effects to account for differences in the true prospects of chosen occupations between students. The table shows that male students tend to overestimate both job opportunities and hourly wages to a larger degree than do female students. They also make larger absolute errors and are less likely to be correct. Third-year students, but in particular fourth-year students make fewer and smaller absolute errors than do second-year students, suggesting students become better informed as they get closer to having to make a decision on which program to enroll in. As shown in Figure 3, 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.

6.1.3 Time spent processing

A natural question to ask prior to assessing the impact of the treatment is whether students in the treatment group actually paid attention to the provided information. A sensible proxy for this is the time it took students to go from the initial beliefs elicitation to the posterior ranking (i.e., the time spent on studying the information screens and watching the video about work). To process the information provided, treated students should take more time to

[16]This Table contains students from the 2018/2019 as well as the 2019/2020 school year, as we are not comparing treatment and control.

get to the posterior rankings than control students, who see no information screens. Beyond seeing whether treated students actually paid attention to the information we provided, this exercise is also useful to see how the treatments providing information about just the job opportunities differ from those where information about both the job opportunities and hourly wages are provided. If students spend equal amounts of time processing each piece of information,[17] we expect the treatment groups that received information about both the job opportunities and hourly wages to spend up to twice as much time on processing the information as the groups that just received information about the job opportunities.

Figure 4 shows how the time spent differs between the groups. The time spent variable has considerable outliers, so we trim it at the 5^{th} and 95^{th} percentile. The control group takes about 70 seconds from stating their initial beliefs to making the posterior ranking. Most of this time is likely spent on watching the video about work.[18] Students in the job opportunities treatment take about 14 more seconds to get to the posterior ranking. Since the five pieces of information is the only difference between this group and the control group, this indicates they spend a little under 3 seconds on each piece of information. Those students who additionally receive information about the hourly wages spend a little over 24 seconds longer on the information than students in the control group. F-tests show that we can reject the null hypothesis that students in the job opportunities treatments spend as much time as those in the job opportunities and hourly wages treatment ($p<0.01$), indicating that students exert additional time to learn about the hourly wages. In fact, we cannot reject that students in the job opportunities and hourly wages treatment spend *twice as much* additional time studying the information as those in the job opportunities treatment. This indicates that students spend approximately an equal amount of time on each piece of individual information.

6.2 Short-term and medium-term treatment impact

6.2.1 Posterior beliefs

The analyses of time spent clearly show that students spend time to process the information we provide. That still leaves the possibility that they do not understand or believe the information. It is therefore useful to study how beliefs change immediately after receiving the information. We preregistered our hypothesis that *"We expect that the updated beliefs of*

[17] We define a piece of information to be an information about one characteristic of one occupation (e.g., the job opportunities of the second-ranked occupation).

[18] The video lasts 1 minute and 49 seconds, see: `https://www.youtube.com/watch?v=YJ78VDQrO3c`. Since students in the control group on average take 70 seconds to arrive at the posterior ranking, it follows that not all students watched the video completely.

students in the treatment groups will be more accurate (i.e., more in line with the provided information) than those of individuals in the control group (who did not receive the information)". Figure 5 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 both treatment groups are much more likely to be correct about the job opportunities, and those who additionally received information about the hourly wages are also more likely to be correct about those. Both of these results are largely driven by the correction of overestimations. Students who initially underestimated the labor market prospects of their occupations react much less strongly than those who initially overestimated them.

We further preregistered a hypothesis that *"The degree to which beliefs are updated may depend on the distance between the prior belief and information provided. Distance and updating may relate non-linearly as higher distance may affect how reliable the student thinks the provided information is"*. Tables C7 and C8 in Online Appendix C speak to this, using the 2019/2020 cohort, where we can use students' prior beliefs in the analysis. Table C7 shows that the treatment impact on the posterior absolute error increases linearly in the initial absolute error, with a further increase when the initial mistake was an overestimation. For being exactly correct, we do see a non-linear impact: the largest positive impact of the treatment on the likelihood of being exactly correct is at an overestimation of two categories. For students who initially overestimated the job opportunities by four categories, the treatment barely has an impact. Table C8 shows a similar picture for the wages. Those who received wage information correct their beliefs more strongly when the initial absolute error was larger, and more so if it was an overestimation. However, those who made particularly large errors barely react: the impact of the treatment on the likelihood of being exactly correct hits zero at an initial absolute error of €12. Interestingly, there also seems to be a response from the group that only receive information about the job opportunities if they were very far off initially. Students likely infer that if they were far off on the job opportunities, they were off on the wages as well. This makes sense, as job opportunities and wages are strongly correlated.

We further hypothesized that students *"are more sensitive to information provided by experienced senders"* and *"more sensitive to information provided by senders of their own gender"*. We find no support for these hypotheses. Table C9 in Online Appendix C shows that the treatment is equally effective when we state that the information comes from an institute, or from a researcher at that institute. Zooming in on the specific researcher, Table C10 in Online Appendix C shows that neither whether the sender was an experienced or inexperienced researcher, nor whether the sender was a male or a female researcher matters for the degree to which beliefs are updated. This holds for both male and female students.

Next, we study how persistent the effects on posterior beliefs are. Table C11 in Online Appendix C 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. The reason is that 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. 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

For the preference ranking of occupations, we hypothesized that the *"provided labor market information influences the students' rankings of occupations"*. Specifically, we stated that *"We will analyze whether the occupation ranked number one changes more often between the first and second elicitation for the treatment groups than for the control group"*. 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 5.7% of students change their favorite occupation. In the treatment groups, this share reaches up to 7.8%, or 2.1 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 their 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 about €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, see the last row of Table 3.

We further specified that we expect that *"the better the news about an occupation's prospects compared to the news about the initial number one ranked occupation, the more likely it is that it takes over the number one spot"*. To answer this question, we estimate a conditional logit model for the probability of assigning occupation j to rank 1, as a function of its initial rank and the news received about the occupation ($O_j^{Actual} - O_{i,j}^{Prior}$ and $W_j^{Actual} - W_{i,j}^{Prior}$), or the information received about the occupation (O^{Actual} and W_j^{Actual}). Table C12 in Online Appendix C shows the results, which are in line with our hypothesis. Column 1 shows that when treated students receive positive news about the job opportunities of an occupation, they are more likely to rank that occupation at number 1 at second elicitation. The impact of the wages is more muted, though also positive. The impacts in Column 2 are larger. A 1 category increase in the job opportunities of occupation j increase the odds ratio of it ending up at rank 1 by 0.232 in the job opportunities treatment. That impact is only half as large in the group that also received information about wages (0.12), but we also observe a large positive impact of a €1 increase in the wages (a 0.09 increase in the odds ratio). Lastly, we stated that *"Moreover, we think that the higher this occupation was originally ranked, the more likely this is to happen as well"*. It is clear that occupations ranked higher initially have a larger chance of taking the top spot, but the occurrence of switches is sufficiently rare that we are not powered to test this hypothesis.

Table C13 in Online Appendix C shows there is no effect of the identity of the information sender here either, neither for male nor female students.

We do not find evidence that treated students still prefer occupations with better prospects in the survey. However, the sample size is small, and Columns 1 and 3 of Table C14 in Online Appendix C show that the treated students in the survey did not switch to occupations with better prospects directly after the intervention either. Hence, the lack of an effect at time of the survey is likely due to a lack of power and selection into survey participation, a problem we do not face when using the administrative data on actual education choices, to which we turn now.

6.3 Impact on educational decisions

We analyze the treatment impact on the profile choice in secondary school and on post-graduation education outcomes. As we have not found any evidence that the identity of the sender impacts the response to the treatment, we collapse the four treatments into two:

1. Job Opportunities Information Treatment
2. Wage and Job Opportunities Information Treatment

This makes the interpretation of the results easier, and increases power of the individual estimates as the sample size for each collapsed treatment is larger.

6.3.1 Profile choice

The outcome of interest for second-year students is whether we observe the student as enrolled in profile P in the next school year. We pre-registered the hypothesis that "*[we] expect that the more and the better the news one receives about occupations associated with a particular study profile compared to the news about occupations associated with the prior intended profile choice, the more likely it is that the profile is chosen*". We estimate a conditional logit model for the probability of choosing profile P as a function of its attributes. We study the effects of two different attributes: (i) the news and (ii) the average information provided about the job opportunities and hourly wages of occupations belonging to a profile. For this analysis, we restrict the sample to students that we first observed in their second year (i.e., before they made their profile choice).

We define the information that students received about each profile in two different ways. First, we calculate the 'news' about each profile P as $\mathcal{O}_P = \sum_{j \in P} \left(O_j^{\text{Actual}} - O_{i,j}^{\text{Prior}} \right)$ and $\mathcal{W}_P = \sum_{j \in P} \left(W_j^{\text{Actual}} - W_{i,j}^{\text{Prior}} \right)$. These values increase for every occupation j belonging to profile P for which the student received good news (i.e., they underestimated the occupation's labor market prospects). This variable therefore contains information on the amount of news the student received, as well as the direction of that news. This aligns well with our pre-registered hypothesis. The main advantage of this operationalization is that we can set this value to 0 for profiles not associated with any occupations the student selected. This means it is defined for all possible profiles the student can choose. Using $\mathcal{O}_P$ and $\mathcal{W}_P$ also comes with downsides. 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. This is because prior beliefs are essential for this analysis and a programming error led to bias in our measure of prior beliefs in the 2018/2019 school year, as discussed before.

As a robustness check, we run another analysis in which we use the average information received about the job opportunities and hourly wages of occupations belonging to a profile. Specifically, we calculate $O_P = \frac{\sum_{j \in P}\left(O_j^{\text{Actual}}\right)}{N_P}$ and $W_P = \frac{\sum_{j \in P}\left(W_j^{\text{Actual}}\right)}{N_P}$. These variables take care of the downsides that come with the news values, but do have their own drawbacks. Beyond the fact that it does not take prior beliefs into account, it is also problematic that these covariates are only defined for profiles associated with occupations that the student picked. Together, we believe they provide a comprehensive analysis of the profile choice. The last restriction we impose is that profile P has to be available at the student's school. We consider a profile P to be available if we observe at least one student enrolled in this profile at the school.

Table 4 shows the results of the analysis, with the coefficients expressed as odds ratios. The first three columns show the impact of the news, and the last three columns show the impact of the average presented information. Within each set of columns, the first column shows the overall impact. The second column shows the impact on students on the theoretical pathway (who can choose among four profiles), and the third column shows the impact for those on the practical pathways (who can choose among ten profiles). To increase precision of our estimates, we include a dummy to indicate whether the profile was part of the student's intended profile choice and a set of dummies for the number of occupations the student selected associated with the profile.

Column 1 shows that for students who received information about just the job opportunities compared to the control group, the odds ratio of choosing profile P increases by about 0.04 if the job opportunities were 'underestimated' by one category, though it is insignificant. The coefficient for students in the wage and job opportunities treatment is smaller at 0.02, and also insignificant. The χ^2 test shows that the impact of the news about the job opportunities is not jointly significant among the two treatments. We do not find any evidence that wage news impacts the profile choice either. The same conclusion holds for students in the theoretical and practical pathway separately.

When we move to the average information provided in Columns 4 to 6, we find a little more evidence that the treatment impacted the profile choice. Students who received information about both the job opportunities and wages seem to select profiles for which they observe better average job opportunities, with the estimate being marginally significant. The coefficient on average observed wages is negative for students in this treatment group, which will partially offset the impact of the information, as job opportunities and hourly wages are strongly correlated. The treatment impact appears much smaller for students who received information about just the job opportunities. All-in-all, we conclude that the treatment had little impact on the profile choice of students through news and information about the job opportunities and hourly wages.

6.3.2 Grade retention and post-graduation extensive margin decisions

Before we turn to the study program choice, we are first interested in students' academic performance (i.e., are they ever retained?) and extensive margin decisions (i.e., are they more or less likely to enroll in intermediate vocational education or general secondary education?). This is important, since it would complicate the analysis if students were to differ on these dimensions. Table C15 in Online Appendix C shows that this is not the case. Conditional on a set of baseline covariates, there is no significant impact of the treatment on ever being retained (Column 1), starting general secondary education (Column 2), or starting post-secondary vocational education (Column 3). However, there are some notable differences in who starts post-secondary vocational education, which is relevant for the sample of students for whom we observe the program choice. Students who took part in 2019/2020 are less likely to enroll in post-secondary vocational education. The same holds for students in schools with more theoretical profiles available, and the opposite for students where more practical profiles are available. Students who took part in the experiment in a later grade and, as described in Section 2, those on the practical pathway are also more likely to enroll in post-secondary vocational education.

6.3.3 Study program choice

We now turn to the impact of the treatment on the study program choice. With the above sample considerations in mind, Table 5 provides the answer to the most policy-relevant question of the paper: do students who receive information enroll in programs associated with occupations that provide better labor market prospects? A student is coded as having chosen a program associated with an occupation if 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. To calculate the labor market prospects of each program, we take the frequency-weighted[19] average of the labor market prospects of all occupations associated with the current program identifier.

We consider two programs in this analysis: (i) the first program that students enrolled in [first choice] at entry in post-secondary vocational education, and (ii) the last program in which we observe the student enrolled [final choice]. Including both provides us with a sense of the impact of the treatment on the initial choice, but ensures we are not simply picking

[19]We base the weight on the number of choice sets (i.e., top-5 occupations) the occupation was included in.

up that students initially pick programs with better prospects but then leave them for the programs they initially intended to enroll in anyway. Beyond the covariates listed in the regression table, all regressions contain fixed effects for each of the occupations the students selected to be in their top five to account for initial preferences.[20]

The results in Column 1 of Table 5 show that treated students initially enroll in programs associated with occupations that provide better job opportunities; around 0.07 to 0.08 categories on average, or a 2.2 to 2.4% increase over the control group mean. The impact of the two treatment types is jointly significant at the 5%-level. The same holds true for the hourly wages of the first choice in Column 2, which increase by €0.20.- to €0.22.-; an increase of 1.1 to 1.3% of the control group mean and also jointly significant at the 5%-level. It may seem surprising that the coefficient on the job opportunities treatment is positive as well but the correlation between job opportunities and hourly wages is around 0.5. It is therefore not surprising that those who receive information about the job opportunities and respond to it also choose programs with higher hourly wages. Columns 3 and 4 show highly similar results to Columns 1 and 2, though the impact on the job opportunities is larger (2.5 to 2.9% of the control group mean). This indicates that the treatment's impact did not exclusively impact the initial choice upon graduation but has a persistent impact. The two treatments are jointly significant at the 1% and 5% level for job opportunities and hourly wages, respectively.

It is worth comparing the treatment effect in Table 5 to that on the immediate change in job opportunities and hourly wages of the number one ranked occupation in Table 3. One might expect that, similar to the treatment impact on belief accuracy, the immediate impact would be large but decrease over time. This is not what we find. In fact, the results in Table 5 show a much larger impact of the treatment on the job opportunities and hourly wages of the chosen study programs than on the number one ranked occupation. The most likely explanation for this is that students were restricted in their ranking by their initial choice set. These restrictions do not apply for their actual program choice.[21] The results in Table 5 indicate that the treatment does more than just help students decide between occupations in their initial set of interest. The results suggest that, in response to the treatment, students gathered information about labor market prospects on a broader range of occupations and,

[20]Tables C16 and C17 in Online Appendix C show the job opportunities and hourly wages of selected occupations in the administrative sample, respectively. There is some meaningful imbalance. The job opportunities of students' initial favorite occupation is 2.5% lower in the job opportunities treatment than in the control group. For wages, the imbalance is smaller (1% of the control mean) but more persistent across choices. Controlling for the set of selected occupations is therefore important. Since these occupations were selected before the treatment, this is not a threat to internal validity.

[21]In fact, we observe that only 25% of students end up picking a program associated with an occupation that appeared in their initial five, of which 55% pick a program associated with their initial favorite.

consequently, made choices associated with better prospects.

While we consider the results in Table 5 to be the most policy relevant, they do not speak to the mechanism through which the treatment operates. In our pre-analysis plan, we describe a likely mechanism: that students are more likely to pick a study program associated with an occupation about which they receive good news. Specifically, we state we *"expect that the better the news about an occupation's prospects compared to the news about the other occupations, the more likely it is that the student decides to pursue the study program associated with this occupation"*. To this end, we estimate another conditional logit model. The outcome of interest is whether the student selected a study program associated with occupation j. The main covariates of interest are the news and information provided about the occupation, interacted with the treatment. Note that this restricts our analysis to the 25% of students who picked a program associated with one of their initial occupations of interest. Table 6 shows the results of this analysis.

Column 1 shows the impact of news about the job opportunities and wages on the first choice upon enrolling in post-secondary vocational education. None of the individual coefficients are significant, and joint significance tests of both the impact of job opportunities news in both treatments, and the impact of job opportunities and wage news in the job opportunities and wage information treatment fail to reject the null hypothesis that there is no impact. The impact on the final choice in Column 3 looks similar. Column 2, which considers only the information, and allows us to include more students in the sample, and avoids measurement error related to inaccurate belief elicitation, shows larger impacts. We observe that information about the job opportunities increases the odds ratio of picking a program associated with the occupation by 0.08 to 0.15 for the treatments, with no discernible impact of wage information. Like before, Column 4 looks similar.

All in all, Tables 5 and 6 show that the treatment has an impact on the labor market prospects of programs the students choose. Some of this impact appears to be driven by switches within the initial choice set, but it is likely that the treatment also made students more broadly aware about labor market prospects.

6.4 Impact on sectoral composition

Now that we have established that treated students actually enroll in programs with better labor market prospects, we study how this affects the sectoral composition of chosen post-secondary education programs. Are students moving across sectors to enroll in programs with better labor market prospects, or do they choose more promising programs within their initial sector of interest? One may worry that moving across sectors comes at the cost of match quality in terms of skills and preferences. Note, however, that our intervention is specifically designed to avoid this, by only providing students with labor market information about occupations they are already interested in.

We use two sector definitions. The first is the official Dutch post-secondary vocational education sector classification. In this classification, programs are divided into nine education sectors, roughly similar to the profiles in secondary vocational education.[22] The second is its international equivalent: the ISCED-F classification. The ISCED-F classification contains 11 broad fields.[23]

We conduct two analyses. First, we ask whether treated students are less likely to choose a program in the same sector as their initial main occupation of interest. Table 7 shows weak evidence for this. In columns 1 and 2, we compare the Dutch classification sector of the initial and final chosen program (respectively) to the sector associated with the initial favorite occupation. In columns 3 and 4, we use the ISCED-F classification. In both cases, the control mean is right around 50%. We find that students in the job opportunities treatment are up to 2.8 percentage points less likely to choose a program in the same sector as their initial main occupation of interest according to the Dutch sector classification, though only marginally significant for the final choice. For the combined treatment, this impact drops to 1 percentage point and is not significant at standard levels. The joint test does not allow us to reject the null hypothesis that the likelihood of choosing a program in the same sector as the main occupation of interest is the same between the control and treatment groups. Columns 3 and 4 show much smaller and insignificant impacts across the board.

Since over 50% switch sectors in both groups, the sectoral composition could still be affected by the treatment. Conditional on switching, treated students may switch towards sectors with better labor market prospects, similar to what we observed in Table 3. Figures 6a and 6b show that we find little evidence for this in the Dutch classification. There are no significant differences in the sectoral composition of chosen programs, despite notable differences in job opportunities and wages. However, Figures 6c and 6d paint a slightly different picture. First, the ISCED-F classification shows larger differences in labor market prospects between sectors than the Dutch classification. We also observe more movement in

[22] 1. Technology and the Built Environment, 2. Mobility, Transport, Logistics, and Maritime, 3. Care, Welfare, and Sports, 4. Trade, 5. ICT and Creative Industries, 6. Food, Green, and Hospitality, 7. Business Services and Security, 8. Specialist Craftsmanship 9. Entry-Level Programs. We include the first seven in our analyses, as sectors 8 and 9 are rarely chosen.

[23] 1. Generic programs and qualifications, 2. Education, 3. Arts and humanities, 4. Social sciences, journalism and information, 5. Business, administration and law, 6. Natural sciences, mathematics and statistics. 7. Information and Communication Technologies, 8. Engineering, manufacturing and construction, 9. Agriculture, forestry, fisheries and veterinary, 10. Health and welfare, 11. Services. We ignore generic programs, social sciences, and natural sciences, as these are almost entirely absent in vocational education.

the sectoral composition in Panel 6c than we do in 6a. It looks like there is some movement away from the services (in the job opportunities treatment in particular) and education (in the job opportunities and hourly wages treatment in particular) sectors and into the health sector according to the ISCED-F classification. That said, most other sectors look unaffected. We conclude that the evidence for large shift across sectors is weak and that our intervention mainly works through changing students' choices to more promising occupations within sectors they were likely to choose in absence of the intervention.

6.5 Heterogeneity

We preregistered a number of hypotheses related to treatment effect heterogeneity. Specifically, we hypothesized that the treatment would have a larger impact on low socioeconomic status students, and when more profiles are available. Moreover, we hypothesized a differential impact between male and female students. We further believe the timing of the intervention is worth analyzing (i.e., whether students took part in their second, third or fourth year of secondary education), and by school year (2018/2019 or 2019/2020). We think the timing of the intervention is especially relevant, as to our knowledge, our study is the only study in which there is variation in this. We conduct these heterogeneity analyses by splitting the sample. This means these results can be interpreted as the impact we would have found if we had conducted the experiment with the specific sample. Since we believe the analyses in Table 5 to be most policy-relevant, we mimic this in our heterogeneity analyses. Note that the coefficients of these heterogeneity analyses need not be the average of the overall analysis. Because we split the sample, the coefficients of baseline covariates may also change.

Table 8 presents the results. We first observe that the treatment has a positive impact on students from both low and high quality of life indicator score neighborhoods. The results in schools with few profiles available (defined as having 2 or fewer theoretical profiles *or* 3 or fewer basic profiles) are similar to the main results. Results for schools with many profiles available are noisy, as the sample size is small. Between male and female students, it looks like female students tend towards programs with better job opportunities following the treatment, whereas men gravitate towards occupations with higher wages.

The timing of the intervention also shows interesting heterogeneity. The treatment impact is largest for students who take it in their third (i.e., the penultimate) year of secondary school. The smaller, mostly insignificant, impacts on second year students despite the large sample size are in line with our findings in Table 4 showing hardly any impact on the profile choice. The impact for fourth year students is noisily estimated, because of the small sample size. Of course, we cannot rule out that career orientation counselors who decide to have students take the occupation test in the third year differ from those who take the test in the other years in other dimensions and affect students' study program choices in others ways. This does not threaten internal validity, but does threaten the interpretation that the third year is the right time to do this intervention. Between the two school years in which the intervention was conducted, we observe that the treatment had a larger impact on wages for the 2019/2020 school year. We do not have a strong prior for why this is the case but students who took part in this year were obviously affected by the Covid pandemic. This may have limited their opportunities to obtain information in other ways, and have therefore increased the impact of the treatment.

7 Conclusion

In this paper, we presented the results of a field experiment aimed at improving the accuracy of 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 result of students rationalizing their choices. However, 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 labor market information is effective in correcting belief errors in the short term. Survey data shows that these beliefs stick for at least a couple of months, but 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 also find clean evidence that our treatment affects actual educational decisions. Students who receive labor market information enroll in programs associated with occupations that provide better labor market prospects. The effects are sizeable, particularly considering the light-touch interventions that we study. Our intervention is relevant from a policy perspective, as it makes use of readily available information on supply-demand ratios and can thus be easily replicated and scaled up at low costs.

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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	82	10,901	81	9,259
Job Opp. Info by Researcher (Treatment 1)	1/6	47	42	5,790	42	5,098
Job Opp. Info by Research Institute (Treatment 2)	1/6	47	40	5,633	40	5,133
Job Opp. & Wage Info by Researcher (Treatment 3)	1/6	48	38	4,761	38	4,231
Job Opp. & Wage Info by Research Institute (Treatment 4)	1/6	48	42	5,083	42	4,465
Total	1	286	244	32,168	243	28,186

Table 2: Balance table

	Control		Job Opp. Info - Researcher		Job Opp. Info - Institute		Job Opp. & Wage Info - Researcher		Job Opp. & Wage Info - Institute		
	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	3.44	0.88	3.07	1.18	3.45	0.93	3.37	0.85	3.33	0.98	0.46
Male	0.52	0.50	0.53	0.50	0.56	0.50	0.52	0.50	0.50	0.50	0.44
Grade	2.46	0.64	2.45	0.64	2.55	0.68	2.43	0.60	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
Took part in 2019/2020	0.49	0.50	0.47	0.50	0.44	0.50	0.49	0.50	0.47	0.50	0.91
Practical pathway	0.32	0.47	0.39	0.49	0.46	0.50	0.45	0.50	0.42	0.49	0.38

Note: Nmbr of students and nmbr of profiles available are school level variables. Male, grade, QOL score, year of participation, and basic level are individual level variables. Last column of the Table shows p-value of joint significance tests between treatment and control groups from regression at variable level.

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

	(1)	(2)	(3)	(4)	(5)
	Pr(Fav. Change)	ΔO_j^{Actual}	ΔO_j^{Actual} (Changed)	ΔW_j^{Actual}	ΔW_j^{Actual} (Changed)
Job Opp. Info - Researcher	0.00775*	0.0190***	0.295***	0.0130	0.192
	(0.00457)	(0.00622)	(0.101)	(0.0174)	(0.292)
Job Opp. Info - Institute	0.0122**	0.0305***	0.447***	0.0303**	0.430*
	(0.00536)	(0.00650)	(0.102)	(0.0149)	(0.253)
Job Opp. & Wage Info - Researcher	0.0205***	0.0278***	0.358***	0.0876***	1.115***
	(0.00551)	(0.00577)	(0.0880)	(0.0215)	(0.295)
Job Opp. & Wage Info - Institute	0.0180***	0.0214***	0.285***	0.0904***	1.197***
	(0.00639)	(0.00640)	(0.0944)	(0.0199)	(0.278)
Observations	28186	27386	1791	27386	1791
Control mean	0.057	0.001	0.012	0.005	0.084

Note: Results from ordinary least squares regression. Column (1) shows the probability that a student changed their favorite occupation between first and second elicitation. Columns (2) and (3) show the difference between job opportunities of students' favorite occupation at second and first elicitation; for all students and those who changed favorite occupations, respectively. Coluns (4) and (5) show the same for the wages. Standard errors clustered at school level in brackets; *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table 4: Treatment impact on profile choice

	(1) All students | News	(2) Theoretical students | News	(3) Practical students | News	(4) All students | Info	(5) Theoretical students | Info	(6) Practical students | Info
Job opp. news about profile	0.932**	0.930**	0.982			
	(0.0286)	(0.0308)	(0.0417)			
Wage news about profile	0.999	1.005	0.976**			
	(0.00713)	(0.00727)	(0.00945)			
Job Opp. Info × Job opp. news about profile	1.044	1.045	1.022			
	(0.0446)	(0.0486)	(0.0615)			
Job Opp. & Wage Info × Job opp. news about profile	1.015	1.004	0.998			
	(0.0422)	(0.0521)	(0.0637)			
Job Opp. Info × Wage news about profile	1.000	0.995	1.027*			
	(0.0122)	(0.0132)	(0.0140)			
Job Opp. & Wage Info × Wage news about profile	0.985	0.998	1.005			
	(0.0109)	(0.0131)	(0.0124)			
Average observed job opportunities of profile				0.796**	0.745**	0.910
				(0.0780)	(0.0954)	(0.125)
Average observed wages of profile				0.981	0.968	1.028
				(0.0292)	(0.0279)	(0.0819)
Job Opp. Info × Average observed job opportunities of profile				1.030	0.990	1.124
				(0.146)	(0.186)	(0.226)
Job Opp. & Wage Info × Average observed job opportunities of profile				1.302*	1.040	1.448**
				(0.180)	(0.207)	(0.240)
Job Opp. Info × Average observed wages of profile				1.022	1.046	0.986
				(0.0497)	(0.0484)	(0.117)
Job Opp. & Wage Info × Average observed wages of profile				0.933	0.935	0.932
				(0.0441)	(0.0411)	(0.0999)
Intended prior choice	5.140***	3.054***	10.48***	4.388***	2.906***	11.18***
	(0.607)	(0.470)	(1.587)	(0.534)	(0.342)	(2.230)
Occupation count	Yes	Yes	Yes	Yes	Yes	Yes
Observations	8786	3514	5272	4818	2660	2154
Chi-Sq Joint Sign. of Treatments × Job Opp.	1.075	1.143	0.193	4.681	0.072	5.449
P-value Joint Sign. of Treatments × Job Opp.	0.584	0.565	0.908	0.096	0.965	0.066
Chi-Sq Joint Sign. Wage & Job Opp. Treatment × Job Opp. and Wages	2.153	0.024	0.178	4.862	2.674	4.977
P-value Joint Sign. Wage & Job Opp. Treatment × Job Opp. and Wages	0.341	0.988	0.915	0.088	0.263	0.083

Note: Table shows odds ratios based on a conditional logit model with whether profile P was chosen as the outcome variable. *Job opp. news* and *Wage news* (Columns (1) to (3)) are defined as $\mathcal{O}_P = \sum_{j \in P}\left(O_j^{\text{Actual}} - O_{i,j}^{\text{Prior}}\right)$ and $\mathcal{W}_P = \sum_{j \in P}\left(W_j^{\text{Actual}} - W_{i,j}^{\text{Prior}}\right)$, respectively. Similarly, the *Average observed job opportunities* and *Average observed wages* (Columns (4) to (6)) are defined as $O_P = \frac{\sum_{j \in P}\left(O_j^{\text{Actual}}\right)}{N_P}$ and $W_P = \frac{\sum_{j \in P}\left(W_j^{\text{Actual}}\right)}{N_P}$. $j \in P$ denotes an occupation associated with the profile. N_P denotes the number of occupations the student picked that were associated with profile P. *Intended prior choice* is a dummy that indicates whether a student intended to choose the profile pre-treatment. Specifications include controls for the amount of information the student received about the profile (i.e., how many occupations associated with the profile were part of their choice set). The sample is restricted to second-year students only. For columns (1) to (3), it is restricted to those who took part in the experiment in the 2019/2020 school year. Columns (1) and (4) show results for all students, regardless of pathway. Columns (2) and (5) show results for students in the theoretical pathway and Columns (3) and (6) show results for students in the practical pathway. The χ^2-tests test the joint significance of the two treatments interacted with the job opportunities (i.e., did the job opportunities make treated students more or less likely to pick a profile) and the joint significance of the job opportunities and the wages for the treatment that received both. Standard errors clustered at the school level in brackets; *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table 5: Treatment impact on job opportunities and wages of chosen programs

	(1) Job opp. first choice	(2) Hourly wage first choice	(3) Job opp. final choice	(4) Hourly wage final choice
Job Opp. Info	0.0689**	0.223**	0.0798**	0.219***
	(0.0325)	(0.0880)	(0.0319)	(0.0842)
Job Opp. & Wage Info	0.0765**	0.202*	0.0923***	0.184*
	(0.0320)	(0.105)	(0.0316)	(0.106)
Took part in 2019/2020	-0.0211	-0.0657	-0.0108	-0.0265
	(0.0248)	(0.0730)	(0.0248)	(0.0752)
Male	-0.0376	0.624***	-0.0243	0.596***
	(0.0427)	(0.118)	(0.0404)	(0.110)
QOL Indicator	0.00490	-0.0536*	0.00221	-0.0195
	(0.00931)	(0.0288)	(0.00843)	(0.0267)
3-4 theoretical profiles	0.0330	0.237**	0.0522*	0.206**
	(0.0305)	(0.0961)	(0.0297)	(0.0914)
4-10 practical profiles	0.0764***	0.0796	0.0770***	0.0747
	(0.0281)	(0.0874)	(0.0279)	(0.0795)
3rd year	0.0511*	0.281***	0.0201	0.190**
	(0.0278)	(0.0864)	(0.0261)	(0.0775)
4th year	0.133***	0.376***	0.0963**	0.279**
	(0.0451)	(0.112)	(0.0449)	(0.115)
Practical pathway	-0.148***	-0.764***	-0.142***	-0.699***
	(0.0282)	(0.0970)	(0.0285)	(0.0897)
Selected occupations fixed effects	Yes	Yes	Yes	Yes
Observations	7843	7843	7835	7835
Control mean	3.179	17.811	3.151	17.742
F-Stat Joint Sign. of Treatments	3.417	3.646	5.133	3.563
P-value Joint Sign. Treatments	0.035	0.028	0.007	0.030

Note: Table shows results from ordinary least squares regression. Columns (1) and (3) show impact on the job opportunities of the chosen study program. Columns (2) and (4) show the impact on the hourly wage of the chosen study programs. Columns (1) and (2) focus on the first program we observe in the data, whereas Columns (3) and (4) focus on the last program we observe in the data. All specifications include fixed effects for each selected occupation (i.e., rank 1, 2, 3, 4, and 5). The analysis is necessarily restricted to students who chose to go into post-secondary vocational education and chose a program for which we can define labor market prospects. The F-tests and P-values reported at the bottom of the table are for the joint significance of the two treatments. Standard errors clustered at the school level in brackets; *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table 6: Treatment impact on choice within initial set

	(1) First choice | News	(2) First choice | Info	(3) Final choice | News	(4) Final choice | Info
News about job opportunities	0.932		0.935	
	(0.0576)		(0.0595)	
News about wages	1.028		1.035	
	(0.0221)		(0.0245)	
Job Opp. Info × News about job opportunities	0.912		0.907	
	(0.0757)		(0.0779)	
Job Opp. & Wage Info × News about job opportunities	1.074		1.062	
	(0.100)		(0.100)	
Job Opp. Info × News about wages	1.019		1.000	
	(0.0281)		(0.0295)	
Job Opp. & Wage Info × News about wages	0.984		0.987	
	(0.0277)		(0.0290)	
Job opportunities of occupation		0.925^{*}		0.912^{*}
		(0.0437)		(0.0430)
Hourly wage of occupation		1.078^{***}		1.086^{***}
		(0.0174)		(0.0166)
Job Opp. Info × Job opportunities of occupation		1.077		1.083
		(0.0769)		(0.0718)
Job Opp. & Wage Info × Job opportunities of occupation		1.153^{*}		1.173^{**}
		(0.0855)		(0.0851)
Job Opp. Info × Hourly wage of occupation		0.995		0.984
		(0.0233)		(0.0236)
Job Opp. & Wage Info × Hourly wage of occupation		1.007		0.987
		(0.0252)		(0.0237)
Prior rank 2	0.472^{***}	0.457^{***}	0.498^{***}	0.479^{***}
	(0.0348)	(0.0249)	(0.0377)	(0.0270)
Prior rank 3	0.299^{***}	0.282^{***}	0.324^{***}	0.307^{***}
	(0.0278)	(0.0188)	(0.0287)	(0.0200)
Prior rank 4	0.230^{***}	0.215^{***}	0.254^{***}	0.245^{***}
	(0.0261)	(0.0149)	(0.0279)	(0.0182)
Prior rank 5	0.155^{***}	0.163^{***}	0.156^{***}	0.172^{***}
	(0.0184)	(0.0132)	(0.0183)	(0.0137)
Observations	5276	11632	5212	11249
Chi-Sq Joint Sign. of Treatments × Job Opp.	3.486	3.752	3.236	4.862
P-value Joint Sign. of Treatments × Job Opp.	0.175	0.153	0.198	0.088
Chi-Sq Joint Sign. Wage & Job Opp. Treatment × Job Opp. and Wages	0.711	5.745	0.467	4.900
P-value Joint Sign. Wage & Job Opp. Treatment × Job Opp. and Wages	0.701	0.057	0.792	0.086

Note: Table shows odds ratios based on a conditional logit model with whether the student chose a study program associated with occupation j as the outcome variable. Columns (1) and (2) consider the first choice we observe, and Columns (3) and (4) consider the final choice we observe. *News about job opportunities* and *wages* are defined as $\mathcal{O}_j = O_j^{Actual} - O_{i,j}^{Prior}$ and $\mathcal{W}_j = W_j^{Actual} - W_{i,j}^{Prior}$ (Columns (1) and (3)), respectively. That is, the higher this value, the better the 'news'. *Job opportunities* and *wage* are defined as usual: O_j^{Actual} and W_j^{Actual}. Dummies for the rank of the occupation in the student's first choice set are also included. The χ^2-tests test the joint significance of the two treatments interacted with the job opportunities and the joint significance of the job opportunities and the wages for the treatment that received both. Standard errors clustered at the school level in brackets; *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table 7: Likelihood of chosen program in same sector as initial favorite occupation

	(1) Sector Init.	(2) Sector Final	(3) ISCED Init.	(4) ISCED Final
Job Opp. Info	-0.0234	-0.0281*	-0.00902	-0.0118
	(0.0164)	(0.0165)	(0.0163)	(0.0168)
Job Opp. & Wage Info	0.0104	0.0111	-0.00286	-0.000140
	(0.0155)	(0.0154)	(0.0155)	(0.0151)
Took part in 2019/2020	-0.0322***	-0.0167	-0.0283**	-0.00881
	(0.0118)	(0.0113)	(0.0113)	(0.0110)
Male	0.0174	0.00975	0.0419**	0.0555***
	(0.0210)	(0.0195)	(0.0196)	(0.0198)
QOL Indicator	0.0103***	0.0107***	0.00729*	0.0119***
	(0.00360)	(0.00364)	(0.00428)	(0.00408)
3-4 theoretical profiles	-0.0115	-0.0103	-0.0104	-0.0169
	(0.0158)	(0.0160)	(0.0160)	(0.0161)
4-10 practical profiles	0.0288	0.0238	0.0295*	0.0277
	(0.0188)	(0.0185)	(0.0159)	(0.0172)
3rd year	0.0461***	0.0376***	0.0555***	0.0420***
	(0.0140)	(0.0139)	(0.0135)	(0.0148)
4th year	0.121***	0.0816***	0.144***	0.101***
	(0.0226)	(0.0221)	(0.0248)	(0.0247)
Practical pathway	0.0201	0.0220	0.0317**	0.0226
	(0.0175)	(0.0179)	(0.0141)	(0.0153)
Selected occupations fixed effects	Yes	Yes	Yes	Yes
Observations	7484	7537	7803	7807
Control mean	0.507	0.492	0.496	0.475
F-Stat Joint Sign. of Treatments	1.668	2.339	0.153	0.277
P-value Joint Sign. Treatments	0.191	0.099	0.858	0.759

Note: Results from ordinary least squares regression. Columns (1) and (2) show the likelihood of choosing a program in the same sector as the initial favorite occuaptions. In columns (3) and (4), we consider the ISCED-F categorization. Columns (1) and (3) cover the first choice we observe, while columns (2) and (4) consider the final choice. The analysis is necessarily restricted to students who chose to go into post-secondary vocational education and chose a program for which we can define labor market prospects. The F-tests and P-values reported at the bottom of the table are for the joint significance of the two treatments. Standard errors clustered at the school level in brackets; *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Table 8: Treatment heterogeneity

	N	Job Opp. Info		Job Opp. & Wage Info	
		β	SE	β	SE
Job opportunities					
Overall	7843	0.069**	0.032	0.077**	0.032
High QOL	4683	0.074*	0.038	0.100**	0.038
Low QOL	3173	0.096**	0.046	0.059	0.046
Few profiles	5971	0.057*	0.034	0.084**	0.034
Many profiles	1872	0.050	0.060	-0.039	0.060
Female	3548	0.110***	0.040	0.096**	0.040
Male	4295	0.054	0.041	0.080*	0.041
Grade 2	4752	0.050	0.038	0.056	0.038
Grade 3	2395	0.169***	0.053	0.156***	0.053
Grade 4	696	0.094	0.123	0.024	0.123
2018/2019	4164	0.110***	0.040	0.074*	0.040
2019/2020	3679	0.060	0.043	0.108**	0.043
Hourly wages					
Overall	7843	0.223**	0.088	0.202*	0.088
High QOL	4683	0.225**	0.104	0.252**	0.104
Low QOL	3173	0.323**	0.133	0.167	0.133
Few profiles	5971	0.106	0.097	0.199**	0.097
Many profiles	1872	0.347**	0.157	-0.099	0.157
Female	3548	0.128	0.099	0.127	0.099
Male	4295	0.352***	0.124	0.316**	0.124
Grade 2	4752	0.199*	0.105	0.016	0.105
Grade 3	2395	0.490***	0.152	0.607***	0.152
Grade 4	696	-0.139	0.347	0.338	0.347
2018/2019	4164	0.143	0.111	0.147	0.111
2019/2020	3679	0.367***	0.121	0.356***	0.121

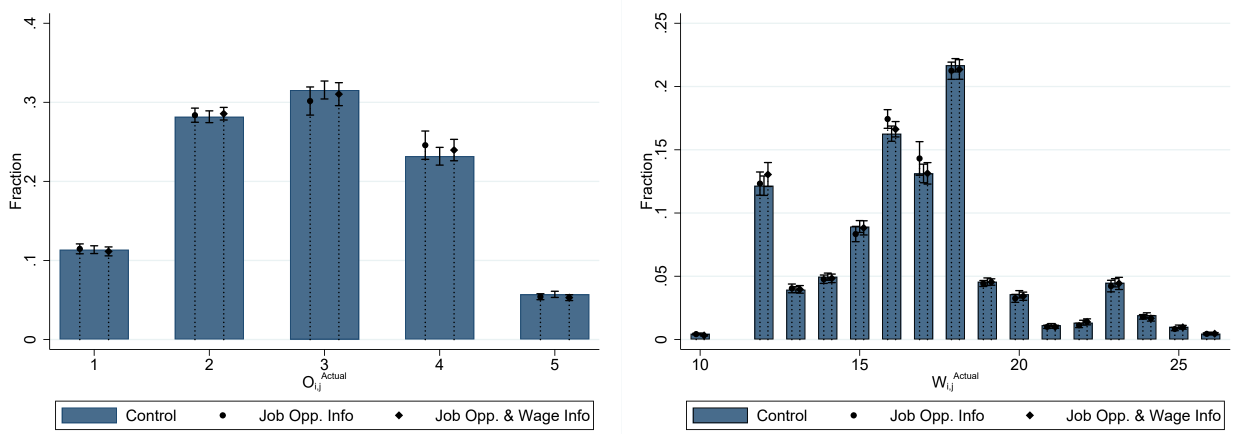
Note: Results from split sample analyses similar to those in Table 5. Controls are the same as in Table 5, except for the heterogeneity dimension of interest. The analysis is necessarily restricted to students who chose to go into post-secondary vocational education and chose a program for which we can define labor market prospects. Standard errors clustered at the school level. *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$.

Figures

Figure 1: Job opportunities and hourly wages of selected occupations

(a) Job opportunities

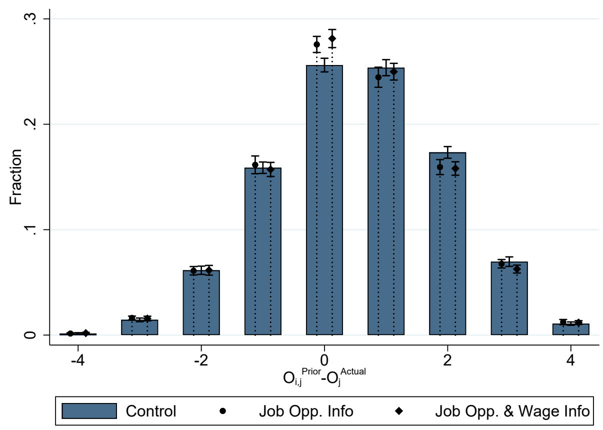
(b) Hourly wages



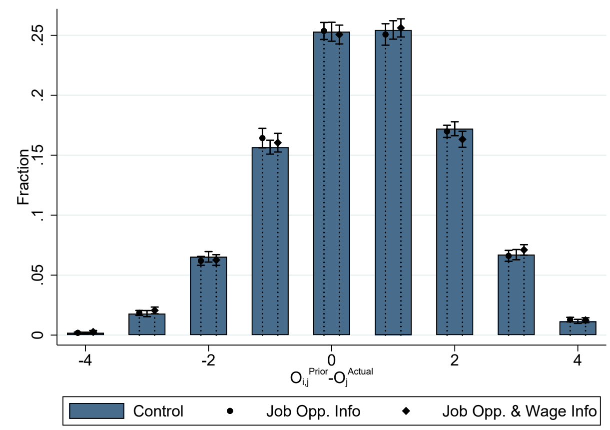
Note: graphic representation of prediction after multinomial logit estimation. Standard errors clustered at school level.

Figure 2: Prior belief accuracy by relevant group

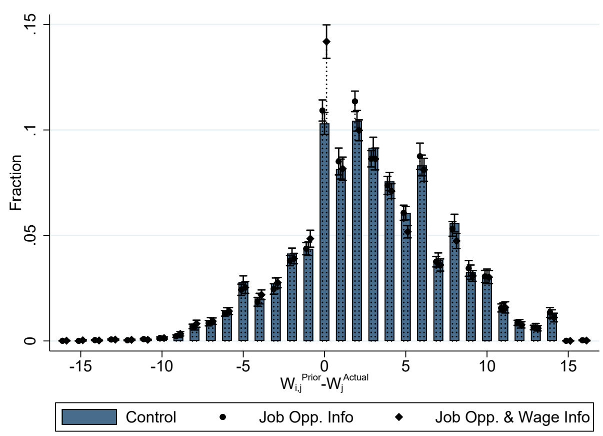
(a) Job opportunities 2018/2019



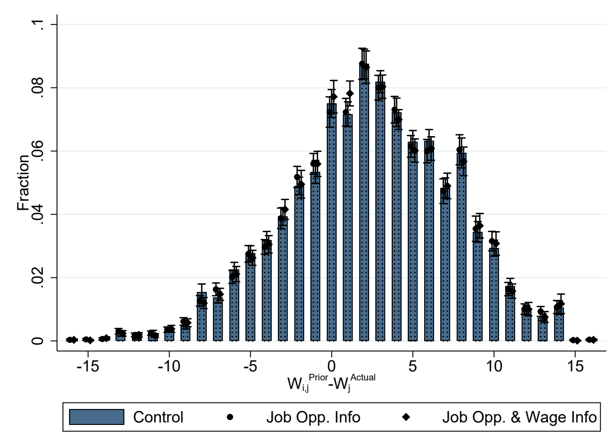
(b) Job opportunities 2019/2020



(c) Hourly wages 2018/2019



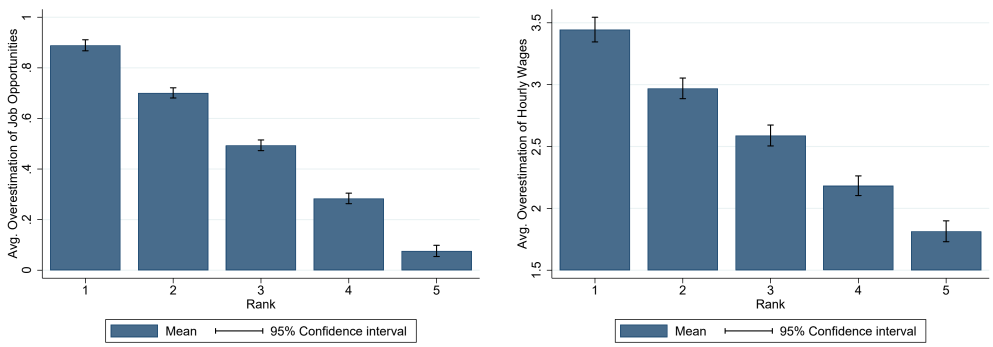
(d) Hourly wages 2019/2020



Note: graphic representation of prediction after multinomial logit estimation. Standard errors clustered at school 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.

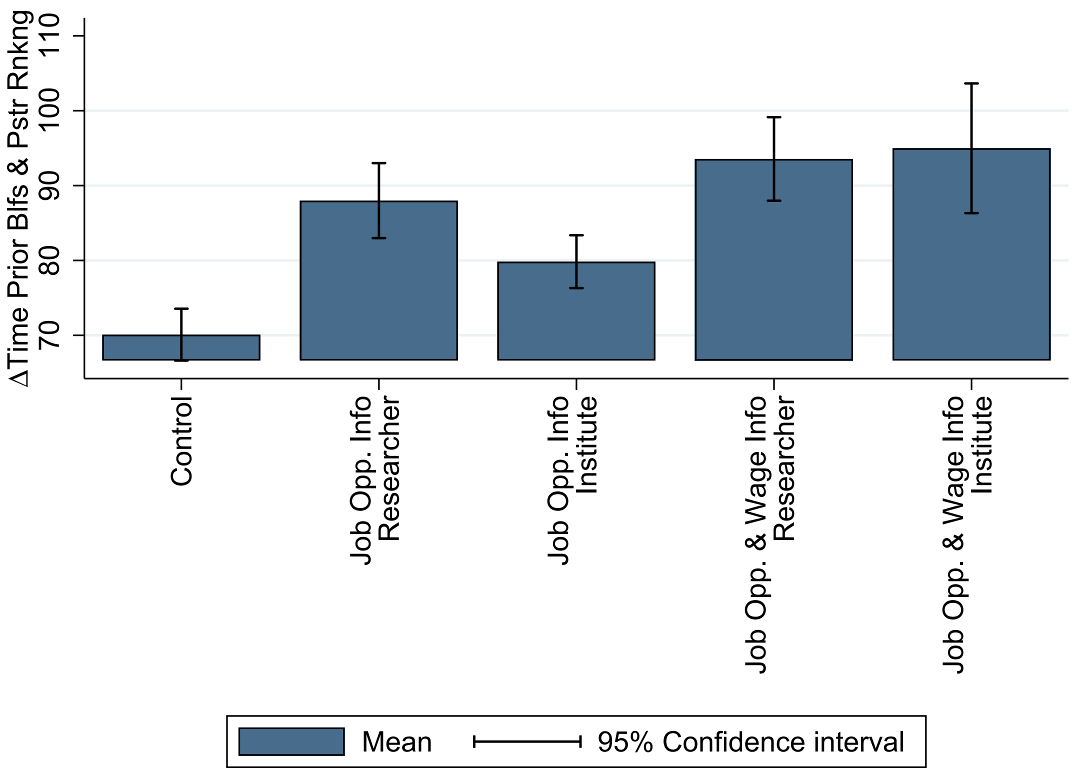
Figure 3: $O_{i,j}^{Prior} - O_j^{Actual}$ and $W_{i,j}^{Prior} - W_j^{Actual}$ by rank

(a) Job opportunities (b) Hourly wages



Note: The x-axis displays the initial rank of the occupation. The y-axis shows 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. Estimation based on students from the 2019/2020 school year.

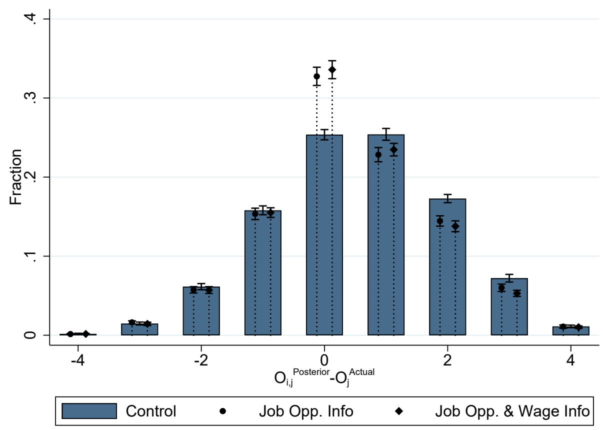
Figure 4: Time spent between prior beliefs and posterior ranking



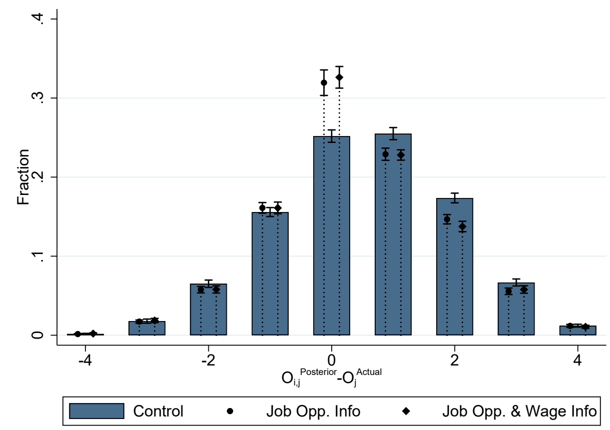
Note: Graphic representation of predictions based on ordinary least squares regression. Sample includes only students from the 2019/2020 school year. Vertical axis denotes the time spent (in seconds) on watching the video about work and processing the information. Standard errors are clustered at the school level in the regression.

Figure 5: Posterior belief accuracy by relevant group

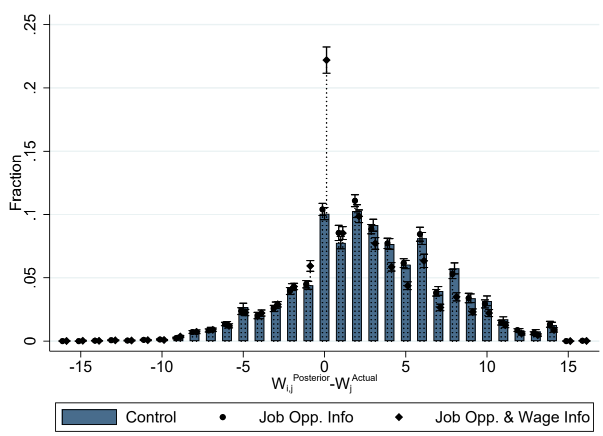
(a) Job opportunities 2018/2019



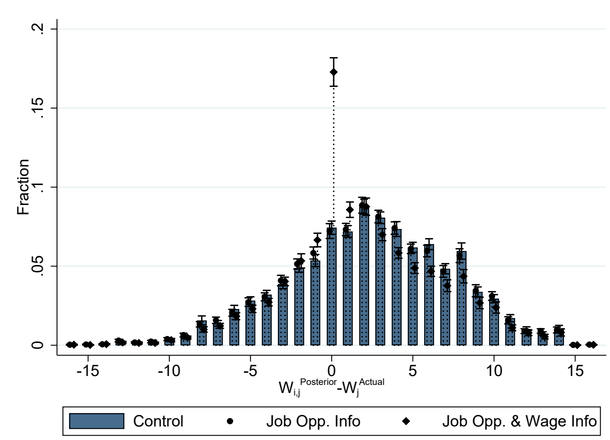
(b) Job opportunities 2019/2020



(c) Hourly wages 2018/2019

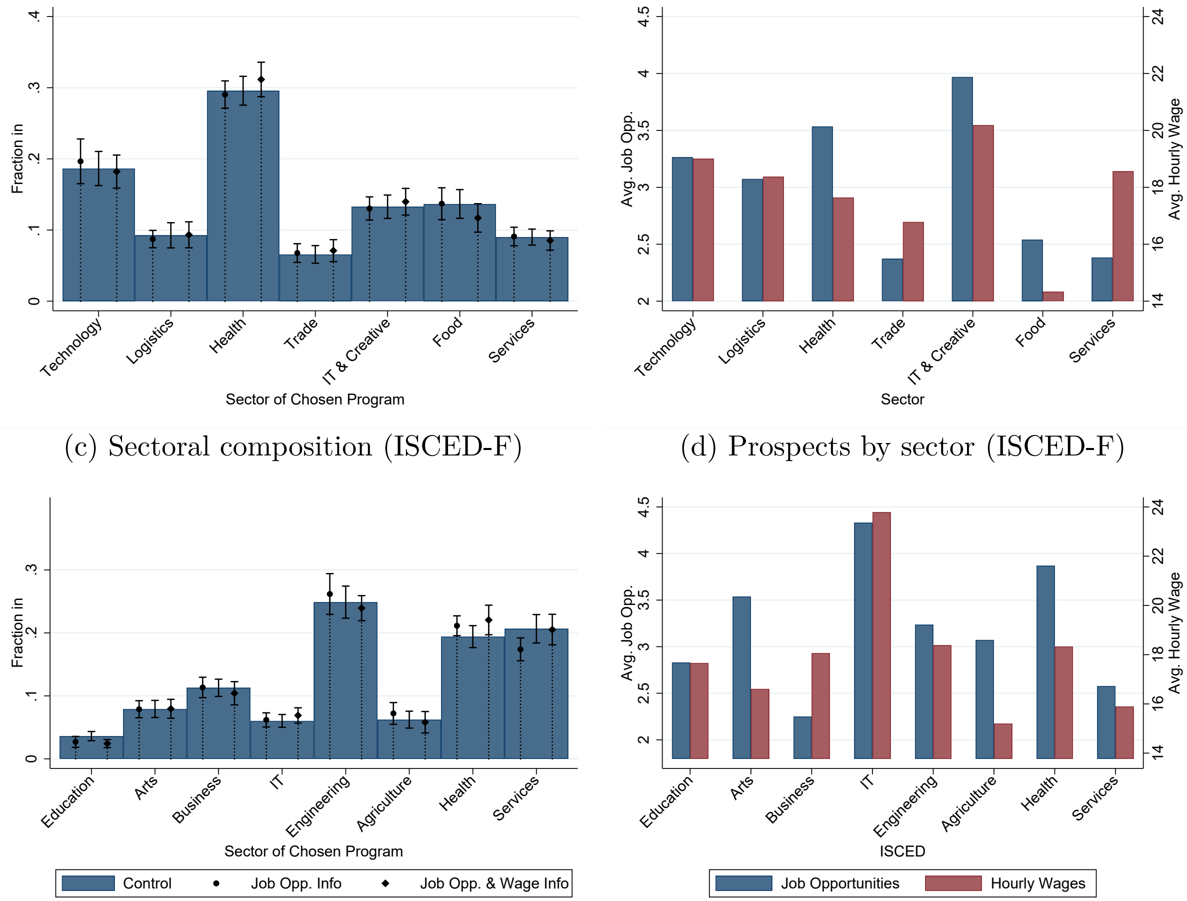


(d) Hourly wages 2019/2020



Note: graphic representation of prediction after multinomial logit estimation. The horizontal 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.

Figure 6: Sector/ISCED composition



(a) Sectoral composition (Dutch classification)

(b) Prospects by sector (Dutch classification)

(c) Sectoral composition (ISCED-F)

(d) Prospects by sector (ISCED-F)

Note: panels (a) and (c) show a graphic representation of prediction after multinomial logit estimation, with standard errors clustered at the school level. The x-axis shows the sectors according to the Dutch and ISCED-F classification, respectively. Panels (b) and (d) show the average job opportunities and hourly wages by sector.