Post-Secondary Education and Information on Labor Market Prospects: A Randomized Field Experiment*

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Abstract

We examine the impact of an information intervention offered to 97 randomly chosen high schools on post-secondary education applications and enrollment in Finland. Graduating students in treatment schools were surveyed and given information on the labor market prospects associated with detailed post-secondary programs. We find that students who were the most likely to update their beliefs due to the intervention started to apply to programs associated with higher earnings. However, this subgroup is too small to give rise to a statistically or economically significant impact on the overall application or enrollment patterns.

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1 Introduction

The choice of how long and what to study is among the most important investment decisions that a typical person makes during her lifetime. Thus it is not surprising that many policy makers, commentators and parents worry about students' ability to make the "right" choices. Indeed, there appears to be widespread concern that many students lack information about the economic consequences of their educational choices and thus do not acquire the type of skills for which there is demand in the labor market. Many governments have responded to these concerns by running schemes that aim to improve the information available to students.[1]

In this paper, we argue that while information interventions can yield substantial belief updating—and may affect some students' behavior—they may have a limited impact on the allocation of students into post-secondary programs. We reach this conclusion with the help of a large randomized field experiment that provided Finnish high school students accurate information about the earnings distributions, employment rates, and the most common occupations associated with detailed post-secondary educational degrees. After receiving this information, roughly a third of the treatment group students report to have updated their beliefs. Furthermore, the intervention pushed students who were the most likely to hold unrealistically positive expectations on the labor market prospects of their preferred programs to apply to programs associated with higher earnings. However, this group of affected students appears to be small. Thus we fail to find a statistically or economically significant impacts on the overall application or enrollment patterns.

Our experiment was designed to test the hypothesis that the match between educational choices and the demand for skills in the labor market can be enhanced by providing information on the population outcomes associated with alternative degrees. This hypothesis is based on earlier work showing that information interventions can have large effects on the likelihood of continuing in secondary education in developing countries (Jensen, 2010; Nguyen, 2008) and that they affect students' beliefs and intentions in developed countries (Oreopoulos and Dunn, 2013; Wiswall and Zafar, 2015; McGuigan et al., 2016; Baker et al., 2018; Peter and Zambre, 2017). However, evidence on the effect of providing information on labor market prospects on *actual* educational choices remains

[1] For example, the U.S Bureau of Census provides infographics Pathways after a bachelors degree that helps to compare average lifetime earnings across different careers, see http://www.census.gov/library/visualizations/2012/comm/pathways-series.html

scarce.

In our experiment, we contacted student guidance counselors working in 97 randomly chosen Finnish high schools and offered them an information package and related lecture materials. In the 64 high schools that chose to participate, more than 5,000 students sat through an obligatory class given as a part of their standard curriculum. During the class, students listened to a student guidance counselor's presentation on the differences in the distribution of earnings and employment rates between different post-secondary degrees. They also completed a survey where they were asked about their preferences and expectations. Along with the survey, the students were given a leaflet reporting the distribution of earnings, employment rates, and the most common occupations among the current population of 30–34-year-old persons by 104 most common post-secondary degrees. Furthermore, they were given the supplementary material at the end of the class to consult at home. The experiment was implemented 5–6 months before the students applied to post-secondary programs.

The Finnish higher education system provides a particularly informative setting for our experiment, because students apply directly to degree programs (including professional degrees such as medicine and law). Thus, the choices that we observe at the admission stage are closely connected with the education that the students will have when they enter the labor market. The universities choose their students based on transparent and uniform criteria based solely on the credits derived from the national final high school exam and the university's own entrance examination. The importance of the entry exams—which are typically based on material not covered in high school—means that the students participating in our intervention have not yet critically limited their choice set of post-secondary degrees. In particular, pre-intervention grades or any other kind of assessment by their high school teachers are not used as admission criteria. Furthermore, credit constraints are unlikely to complicate our analysis because Finnish universities do not charge tuition fees, and the government offers generous subsidies to students who gain entry to higher education. In short, we examine the impacts of the intervention on the relevant educational choices in a transparent, flexible, and simple setting.

The choice of field of study is likely to have important implications for earnings and employment outcomes in the Finnish context. The differences in average earnings across graduates from different fields are nearly as large in Finland as the differences reported by Altonji et al. (2012) for the United States. Even though we are unable to provide the treatment school students with personalized estimates of their individual returns to education in different fields, we argue that providing them information on the earnings distribution by fields can help them form more accurate estimates of their potential earnings. Furthermore, our subsample and survey results suggest that the control group was unlikely to receive similar information through other channels. This interpretation is also supported by the fact that such information was not publicly available but had to be acquired from Statistics Finland with significant financial and effort costs.

Another major advantage of the Finnish context is that we can use the national application registry to evaluate the intervention's impact. These data cover all applications to Finnish universities and polytechnics over several years and allow us to study the effects separately on applications and final enrollments and to check for the robustness of our results when controlling for the baseline educational choices at the school level. Access to register data also means that we avoid attrition problems and obtain high statistical power. Furthermore, we do not need to convince the control schools to participate without receiving any of the potential benefits. However, these gains come with the inevitable cost of not obtaining survey data for the control group.

Our experimental results show that, on average, the information intervention did not affect the likelihood of being enrolled in a post-secondary program or the type of programs where the students were enrolled. Furthermore, the application patterns among students graduating from the treatment and control school are indistinguishable. The point estimates are close to zero and sufficiently precise to rule out economically significant effects.

To understand why the intervention had little average impact, we turn to the survey data collected as part of the experiment. The results show that while students value many dimensions of alternative degrees, four-fifths report that future earnings are an important factor in their educational choice. Importantly, these data reveal similar belief updating as has been documented in the previous literature cited above. Roughly a third of the respondents in our survey declared that they were surprised about the labor market prospects associated with the program to which they intended to apply, and approximately 19% state that these prospects were worse than they had expected. Moreover, among the students who allowed us to link their survey answers to the application register, we find that this belief updating was correlated with their later choices: those who had been negatively surprised were more likely to change the field that they actually applied to than the rest of the treatment school students. We interpret these findings as evidence of the intervention conveying information to the students.

The correlation between being negatively surprised and changing plans later in our survey data also suggests that the intervention may have affected the behavior of the subgroup of students who were disappointed about the labor market prospects of their intended field. Unfortunately, we cannot directly compare surprised students in the treatment group with those in the control group who would have been surprised if they had received the information intervention. Instead, we take an indirect approach using the linked survey-register data. Using information available in the application register for the full student population—demographics, subjects taken in the national matriculation exam, and school characteristics—we predict the likelihood of being negatively surprised about earnings in the first program of choice. We then estimate the treatment effects for subgroups that differ in this predicted likelihood. The results suggest that the students who were most likely to be negatively surprised started to apply to programs associated with higher earnings. However, this subgroup is too small to affect the average treatment effect estimates significantly.

These results add to the growing literature evaluating interventions designed to improve the efficiency of educational choice. In addition to the papers cited above, existing work closest to our own is Hastings et al. (2015), who provided information about costs and returns associated with different degrees using an online tool incorporated into the student aid application procedure in Chile. They find that the intervention decreased demand for low-return degrees—typically provided by for-profit institutions—among low-SES, low-achieving students. In contrast, there appears to be little or no effect for high-SES students. This suggests that the difference in the average effect of their study, in comparison to ours, is likely driven by differences in the distribution of students background and the supply of higher education rather the specific form of the interventions.[2]

The rest of the paper is organized as follows. In the following two sections, we describe the institutional context and the information intervention. We then discuss the findings from the survey conducted among the students in our treatment schools. The fifth section discusses the applications register and our estimation methods and reports the results. Section 6 concludes.

2 Institutional setting

Our intervention was timed to affect students who were making their post-secondary education choices, i.e., soon to be graduating high school seniors. In Finland, these choices are made at the end of the upper secondary school, typically at the age of 18–19. In this section, we describe the Finnish educational system's main features and the importance of post-secondary educational choices in the Finnish context.

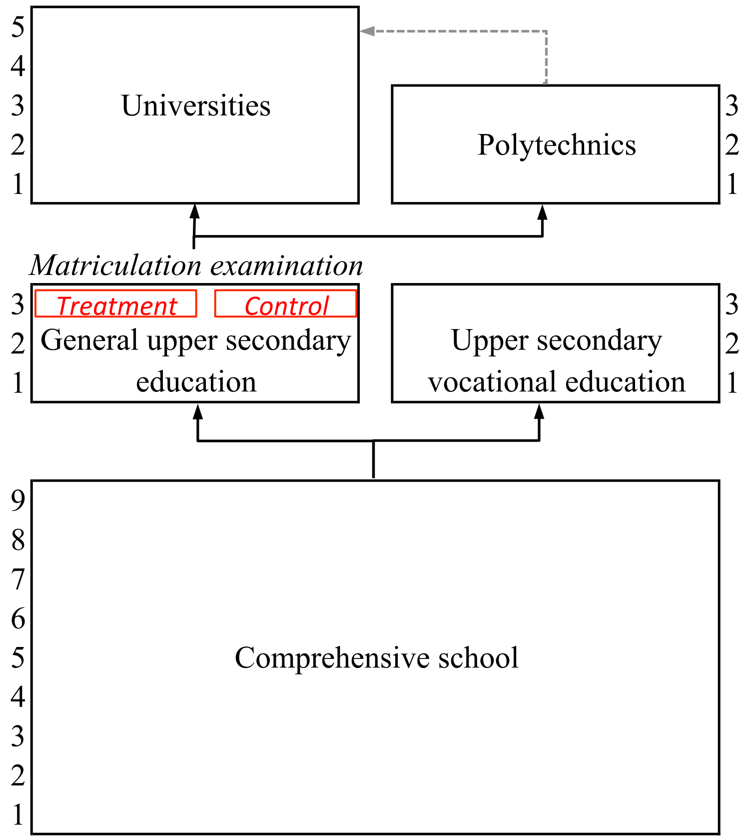
2.1 Context: Finnish upper secondary school graduates

Figure 1 describes the main features of the Finnish education system. Compulsory schooling starts at age seven and lasts for nine years. More than 90% of the cohort continues to the three-year upper secondary school, which is divided into general and vocational tracks. Roughly half of the students who continue to upper secondary school choose the general track, which is more academic in content and is the primary channel through which students continue to post-secondary education. Our intervention targeted students only in general upper secondary school. Henceforth, we refer to these schools simply as "high schools."

The three-year high school concludes with a national matriculation examination,

[2] In addition, Busso et al. (2017) find that providing tailored information about financial aid and labor market prospects affects enrollment in some Chilean colleges. However, they focus only on the non-selective segment of the Chilean education system that typically attracts students with lower high school performance, and thus their results are harder to compare with ours. Other relevant earlier work include studies showing that students are misinformed about the true costs of higher education (Hoxby and Avery, 2013) and that providing accurate information on those costs, along with assistance to apply for financial aid, can influence enrollment (Bettinger et al., 2012; Hoxby and Turner, 2013; Dinkelman and Martínez A, 2014). However, interventions providing *only* information seem to have little effect in this context (Bettinger et al., 2012; Bergman et al., 2016). Furthermore, survey evidence suggests that university students' earnings expectations are inaccurate (Betts, 1996; Carvajal et al., 2000; Dominitz and Manski, 1996; Brunello et al., 2004). Lavecchia et al. (2016) and French and Oreopoulos (2017) present throrough reviews of this literature.

Figure 1: The timing of the treatment within the Finnish educational system



which provides the general eligibility for higher education. The examinations are held each spring and autumn during a two-week period. They consist of four exams of which mother tongue (either Finnish or Swedish) is compulsory. The student can choose the other three exams from the second national language (Finnish or Swedish), foreign languages, mathematics, and science and humanities exams. In addition, students can take as many voluntary exams as they wish. The examination is national and graded externally by a centralized examination board. The results are standardized to be comparable across years.

2.2 Applying to post-secondary education

After completing the matriculation exam, the graduating students can file applications to post-secondary education. Typically around 75% of students apply the same year they graduate from high school. The Finnish tertiary education system consists of two kinds of institutions: universities and polytechnics. Universities focus on scientific research and education and have the right to award advanced degrees. Polytechnics concentrate on advanced vocational education. The prospective students apply directly to the specific degree program, and switching programs after entering is difficult.[3] Students typically obtain their final (bachelor or masters) degree from their first program.

The admission system is centralized, but most institutions base their admission on a combination of entrance examinations and national matriculation examination scores. The universities and polytechnics are free to design their program-specific entrance examinations. Typically these exams are based on material that is not taught in high schools. Personal essays, sports performance, letters of recommendation, or extra-curricular activities are not used as admission criteria.

The applicant is allowed to apply up to seven university programs and four polytechnic programs in a given year. The programs are defined by the institution and major subject, e.g., economics at the University of Helsinki. However, the need to prepare for entrance examination limits the number of applications in practice. The average number of applications per individual was 4.5 during the period we examine.

The number of available slots per program is determined in joint negotiations between the universities and the Ministry of Education on an annual basis. Popular programs are heavily oversubscribed, and it is common that students apply several times before being admitted. In 2011, for example, only 24% of the high school graduates of that year were immediately accepted to a university and 21% to polytechnics. That is, over half of the high school graduates did not gain admission in the first year that they tried.[4] However, most high school graduates succeed in gaining admission in a few years after graduating.

Admission to a university program typically gives the right to study until the master's degree. Importantly, this practice also includes professional degrees like law and medicine. Universities and polytechnics are not allowed to charge tuition from domestic students, and the primary source of funding is the state budget through the Ministry of Education. A large fraction of state funding to universities is allocated based on the number of targeted and completed master's and advanced degrees. This creates an incentive for the universities to attract the best available students. Students are provided generous study grants, highly subsidized accommodation, and access to government-guaranteed student loans. Thus credit constraints are unlikely to be important in the Finnish context.

[3] In most cases, switching programs requires one to re-apply and pass the entrance examination.

[4] There is considerable variation across fields, with natural sciences accepting 34% of the applicants whereas small fields such as theatre and arts accept only 3% of the applicants.

2.3 Characteristics of post-secondary degrees

Post-secondary degrees differ in the kind of labor market prospects that they provide and the kind of applicants they attract. Table 1 documents the application patterns in 2011 using data discussed in detail in section 5.1. The most popular degree, nursing in polytechnics, attracts 15% of the applications and has an admission rate of only 9% despite the low average earnings of the current 30–34-year-olds holding this degree. Nursing is followed in popularity by polytechnic degrees in business and engineering and university degrees in education, humanities, and natural sciences.

Importantly, annual earnings vary considerably across programs. Graduates from university-level medicine, law, engineering and business programs tend to earn, on average, almost twice as much as the graduates from nursing and education.[5] Furthermore, employment prospects are positively correlated with average earnings. For example, employment rates at age 30–34 in engineering and medicine are well above 90 percent.

Of course, these earnings differences are unlikely to be solely caused by educational degrees. For example, Kirkeboen et al. (2016) find that Norwegian students select into different programs based on their comparative advantages. However, they also present compelling evidence suggesting that completing a degree in medicine, law, business, or engineering has a substantial positive causal effect on earnings compared to other alternatives.

Figure 2 plots the average earnings of the 30–34 old individuals currently holding the degrees against the average matriculation exam scores of the students who were enrolled in these programs in 2011–2013.[6] The figure shows a clear positive association between matriculation exam scores and average earnings for a degree. However, it also shows that conditional on matriculation exam scores, large differences in expected earnings remain. For example, students enrolled in university-level engineering and humanities programs have similar average matriculation exam scores despite the almost 20,000 euros

[5]While taxes and benefits reduce the importance of gross earnings, the earnings differences imply sizeable differences also in disposable income. For example, Koerselman and Uusitalo (2014) find that Finnish men with a college degree have 2.2 times higher lifetime gross earnings than men with only a vocational degree. After taking into account taxes and benefits, college-educated men have 1.7 times higher disposable income than those with a vocational degree.

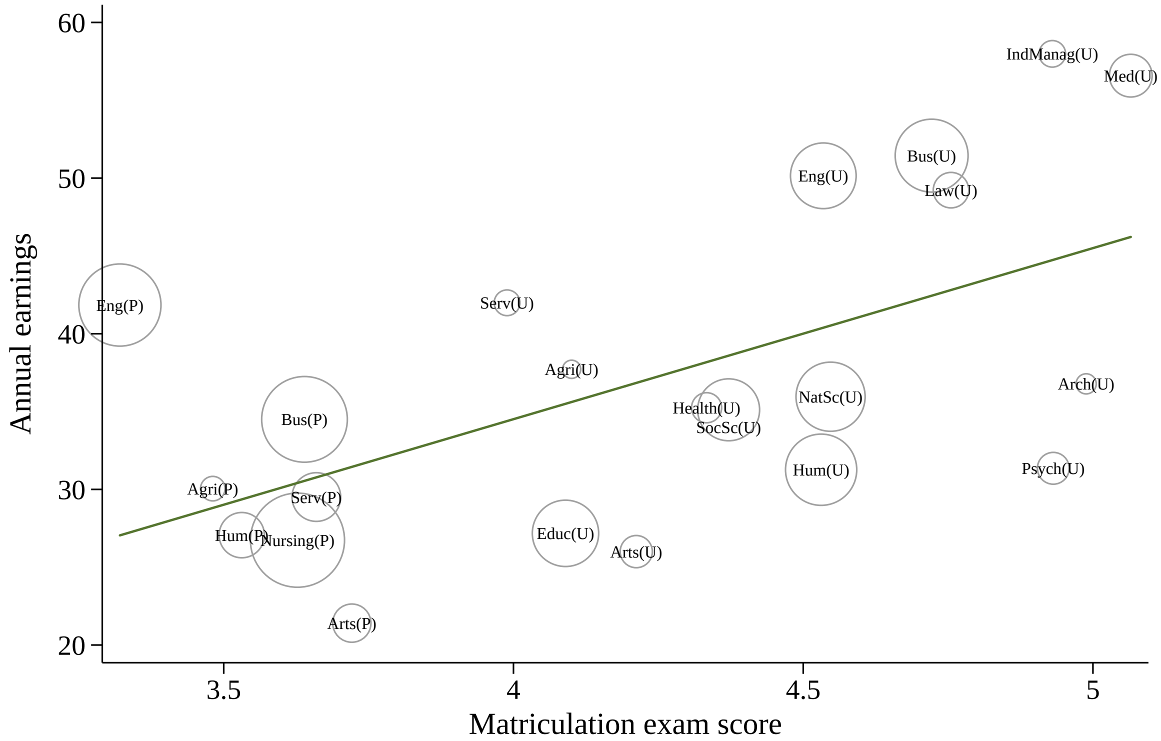
[6]The average grade was calculated based on the four compulsory subjects in the high school matriculation examination: Mother tongue, and the best three grades out of (a) mathematics (long or short curriculum), (b) foreign language, (c) the second domestic language (Swedish), and (d) the best grade in the battery of tests in humanities and sciences.

Table 1: Post-secondary degree characteristics

				Matriculation Exam Score			Employ-ment	Annual earnings		
	Applications (1)	Accepted (2)	Female (3)	Adv. Math (4)	Average (5)	Std. dev. (6)	Rate (7)	Mean (8)	p10 (9)	p90 (10)
		A: Polytechnics								
Humanities	0.03	0.09	0.70	0.19	3.76	1.01	0.80	27,060	10,860	40,740
Arts	0.03	0.10	0.80	0.24	3.82	1.01	0.55	21,408	7,740	34,680
Business	0.10	0.16	0.59	0.28	3.83	1.01	0.90	34,500	15,696	53,856
Engineering	0.08	0.26	0.19	0.55	3.51	0.91	0.94	41,844	28,392	56,568
Agriculture	0.01	0.32	0.49	0.40	3.70	0.96	0.88	30,048	17,544	42,576
Nursing	0.15	0.09	0.88	0.24	3.66	0.96	0.89	26,736	11,232	36960
Services	0.05	0.10	0.81	0.18	3.70	0.99	0.87	29,508	12,912	46,404
		B: University								
Education	0.07	0.07	0.88	0.28	3.98	1.00	0.75	27,364	11,382	38,231
Arts	0.02	0.08	0.72	0.29	4.08	1.03	0.77	26,463	9,085	42,881
Humanities	0.09	0.14	0.72	0.29	4.41	1.04	0.83	31,268	13,588	44,667
Business	0.06	0.10	0.47	0.48	4.37	1.04	0.92	51,444	20,904	80,280
Social sciences	0.05	0.08	0.63	0.38	4.30	1.04	0.86	35,112	14,964	52,272
Psychology	0.02	0.02	0.81	0.35	4.34	1.00	0.88	31,356	12,036	41,712
Law	0.01	0.13	0.61	0.42	4.46	1.09	0.93	49,224	23,100	76,164
Natural sciences	0.10	0.36	0.52	0.80	4.61	1.05	0.83	36,184	19,137	52,935
Engineering	0.07	0.20	0.21	0.95	4.38	1.06	0.95	50,148	30,912	70,044
Ind. management	0.01	0.14	0.22	0.93	4.38	1.05	0.95	57,984	32,640	85,344
Architecture	0.01	0.05	0.64	0.78	4.38	1.13	0.91	36,780	21,924	49,248
Agriculture	0.00	0.27	0.61	0.60	4.29	1.04	0.86	37,716	16,116	55,812
Medicine	0.01	0.08	0.55	0.85	4.71	1.10	0.93	56,716	25,572	82,218
Other health care	0.01	0.14	0.79	0.59	4.23	1.02	0.90	36,122	16,647	52,010
Services	0.01	0.02	0.51	0.37	3.84	0.93	0.95	41,988	27,252	55,344
All	106,150	0.15	0.62	0.43	4.06	1.08	0.86	35,210	17,137	51,251

Note: Charateristics of the applications and degrees. Columns (1) to (6) use the universe of applications into polytechnics and universities in 2011–2013. Columns (7) to (10) use data distributed to the students as part of our experiment.

Figure 2: Average earnings and matriculation exam scores among enrolled students



Note: Average earnings of the 30–34 old individuals in 2008 holding a degree (vertical axis) and the average matriculation exam scores of the students enrolled in these programs in 2011–2013 (horizontal axis). The size of the circles corresponds to the number of students enrolled in the program in 2011–2013. (P) refers to polytechnic and (U) to university programs.

(or 60%) difference in their average annual earnings. Another striking example is the polytechnics engineering programs, which combine low average matriculation exam scores and relatively high acceptance rates with high earnings and employment rates. Thus it seems likely that, for many students, the choice of post-secondary degree has a large impact on their future earnings.

3 The experiment

In this section, we describe the design and implementation of the information experiment. We start by describing how the treatment schools were selected and give background information about the student guidance counselors who implemented the intervention. We then describe the content of the information package in detail.

3.1 Research design

Our experiment was implemented through the standard high-school curriculum in order to use the expertise of the student guidance counselors and to test the effects of an intervention that could be scaled up within the Finnish high-school system. We used randomized block design and divided the 363 Finnish language high schools operating in mainland Finland into 97 blocks by province and the average matriculation examination grades of the schools in 2008–2010. We then randomly chose one school from each block to the treatment group. This approach assures that the treatment schools are representative of both in terms of average student quality and geography.

In total, 97 high schools were allocated into the treatment group and 266 to the control group.[7] Table 2 reports the average pre-treatment background characteristics and outcome variables across different groups of schools in 2011. Columns (1) to (3) show that observed background characteristics, application behavior, and enrollment outcomes were balanced across students graduating from treatment and control schools a year before our experiment. We also compare pre-treatment students of treatment schools that participated in our intervention (column 4) and schools selected to the treatment group that did not participate (column 5). This comparison suggests that, if anything, pre-treatment students graduating from schools that chose to participate in the treatment

[7] We discuss the details of the procedure in Online Appendix A.

Table 2: Pre-treatment characteristics of treatment and control schools

	Treatment and Control Schools			Treatment Schools			
	Treament Average (1)	Control Average (2)	Diff. (3)	Partici-pating (4)	Non-parti-cipating (5)	Diff. (6)	Participating vs. Control (7)
Male	0.41	0.40	0.008	0.42	0.40	0.016	0.012
			(0.013)			(0.024)	(0.014)
Advanced math	0.40	0.42	-0.015	0.39	0.44	-0.053	-0.028
			(0.017)			(0.028)	(0.020)
Matriculation exam GPA	4.01	4.02	-0.011	3.98	4.08	-0.100	-0.037
			(0.054)			(0.099)	(0.059)
Enrolled in tertiary education in 2011	0.45	0.45	-0.002	0.43	0.49	-0.059	-0.017
			(0.013)			(0.021)	(0.014)
Log av. earnings in the program where enrolled in 2011	10.5	10.5	0.008	10.5	10.5	0.007	0.010
			(0.009)			(0.017)	(0.010)
Average employment rate in the program where enrolled in 2011	0.91	0.91	0.002	0.91	0.91	-0.003	0.001
			(0.002)			(0.003)	(0.002)
Log av. earnings of the application portfolio in 2011	10.4	10.4	0.003	10.4	10.4	0.006	0.004
			(0.008)			(0.016)	(0.009)
Number of applications	4.50	4.53	-0.036	4.39	4.79	-0.403	-0.140
			(0.087)			(0.200)	(0.085)
Number of students	5,278	15,916		3,922	1,356		
Number of schools	97	265		64	33		

Note: This table reports background characteristics and main outcome variables for students who graduated from treatment and control schools a year before intervention and applied to tertiary education in 2011. Note that these numbers are smaller than total number of geaduating students as not all students apply to tertiary education immediately and are hence not found from applicant data. Columns (1), (2), (4) and (5) report averages for students that graduated from these high schools in 2011. The remaining columns report point estimates and school-level clustered standard errors (in parantheses) from OLS regressions measuring the difference between treatment and control schools (column 3), treatment schools that participated in the intervention and those that did not (column 6), and participating and control schools (column 7). Advanced math is an indicator for the student taking part in the advanced math exam in the matriculation exam. Matriculation GPA is calculated as described in footnote 6. Log average earnings and average employment rate in the program where enrolled in 2011 refer to the average earnings and employment rates of 30-34 year olds with that degree. Log average earnings of the application portfolio refers to the average of the average earnings in the each program that the individual applied to, see Section 5.2 for details.

had lower academic aptitude as measured by matriculation exam scores, the likelihood of taking advanced mathematics exam, and enrolling in higher education immediately after graduation. However, only the differences in enrollment rates and the number of applications are significant at 5% level. Furthermore, we find no statistically or economically significant pre-treatment differences between the participating schools and control schools.

For each treatment school, we visited the school website to obtain the student guidance counselors' contact details and sent them an email inviting their schools to participate. Of the 97 schools contacted, 40 responded positively and none negatively to the first invitation. The 57 schools that did not respond were contacted by email again, which resulted in 24 additional schools being recruited for the study and one refusing to participate due to the absence of student guidance counselors. The participating sample includes a total of 64 schools with altogether 5,343 students in the final year of high school. Complete survey responses were received from 59 schools by the end of 2011. These 59 schools had 4,984 final year students, and we received 3,437 responses to our survey. Importantly, we define the randomly chosen group of 97 schools as our treatment group irrespective of how they responded to our call.

3.2 Student guidance counseling

The intervention was implemented during the student guidance counselors' classes that are a mandatory part of the curriculum.[8] These classes are the most natural channel through which to distribute information related to different post-secondary degrees, because one of the main tasks of the counselors is to inform students about career choices. The counselors are teachers who have taken an additional one-year of full-time university training in counseling. The pre-requisite for this training is a Master's degree in education and a teacher qualification (see Online Appendix A.4 for details).

Importantly, the student guidance counselors do not have access to the type of information provided by our experiment. On the contrary, our review of the counselors' occupational magazine, *Opo-lehti*, suggests that the presentation of each post-secondary education option is given an equal esteem regardless of their labor market prospects. Fur-

[8]Finnish high schools students have to take 38 lessons, usually spread out over three years, in counseling.

Figure 3: Timeline

September 7, 2011	Invitations sent to the 97 treatment schools
September 20, 2011	Positive replies received from 40 treatment schools
September 21, 2011	New invitations sent to the remaining 57 treatment schools
October 5, 2011	Positive replies recieved from additional 24 treatment schools
October 6, 2011	The intervention material is sent to the treatment schools
November, 2011	Treatment schools implement the intervention in the guidance counselor classes
February–March, 2012	Matriculation examination
April, 2012	Students apply to post-secondary education programs
June, 2012	Entrance examinations
July-August, 2012	Final enrollment outcomes are realized

thermore, while some occupation-specific trade unions provide rough characterizations of typical or recommended initial wages on their websites, the kind of detailed, comparative and comprehensive information provided in our experiment is not readily available anywhere.

3.3 The intervention

Figure 3 presents the timeline of our information intervention. We contacted the student guidance counselors at the treatment schools, who were also responsible for the actual implementation of the information and survey sessions in September 2011. The intervention packages were sent to the treatment schools in October 2011. These packages came with detailed instructions. Furthermore, we quickly responded to any questions that arose during the experiment. After the intervention was implemented in November 2011, the survey forms were returned to us by the end of January, 2012.

The treatment school students retained the information packages after the information session and could, therefore, consult them anytime they wished. Hence, we expect our intervention to affect the application behavior and enrollment from spring 2012 onwards. In the spring of 2012, both the treatment and control students took their matriculation examinations and graduated from high school, after which they were able to submit applications to post-secondary programs and prepare for the entrance examinations taking place in June 2012.

The intervention was implemented in one 45-minute session and was structured as follows. First, the student guidance counselors were instructed to prepare a roughly 20-minute presentation providing general information on the value of education in the labor market. We provided a PowerPoint presentation and a separate document with suggestions about the general message the counselors might want to convey with each slide. The slides provided information on the earnings distributions by education level and broad field, the lowest and highest-earning degrees by field, information about the cost and funding of studies, and the overall acceptance probabilities and completion times for various degrees.

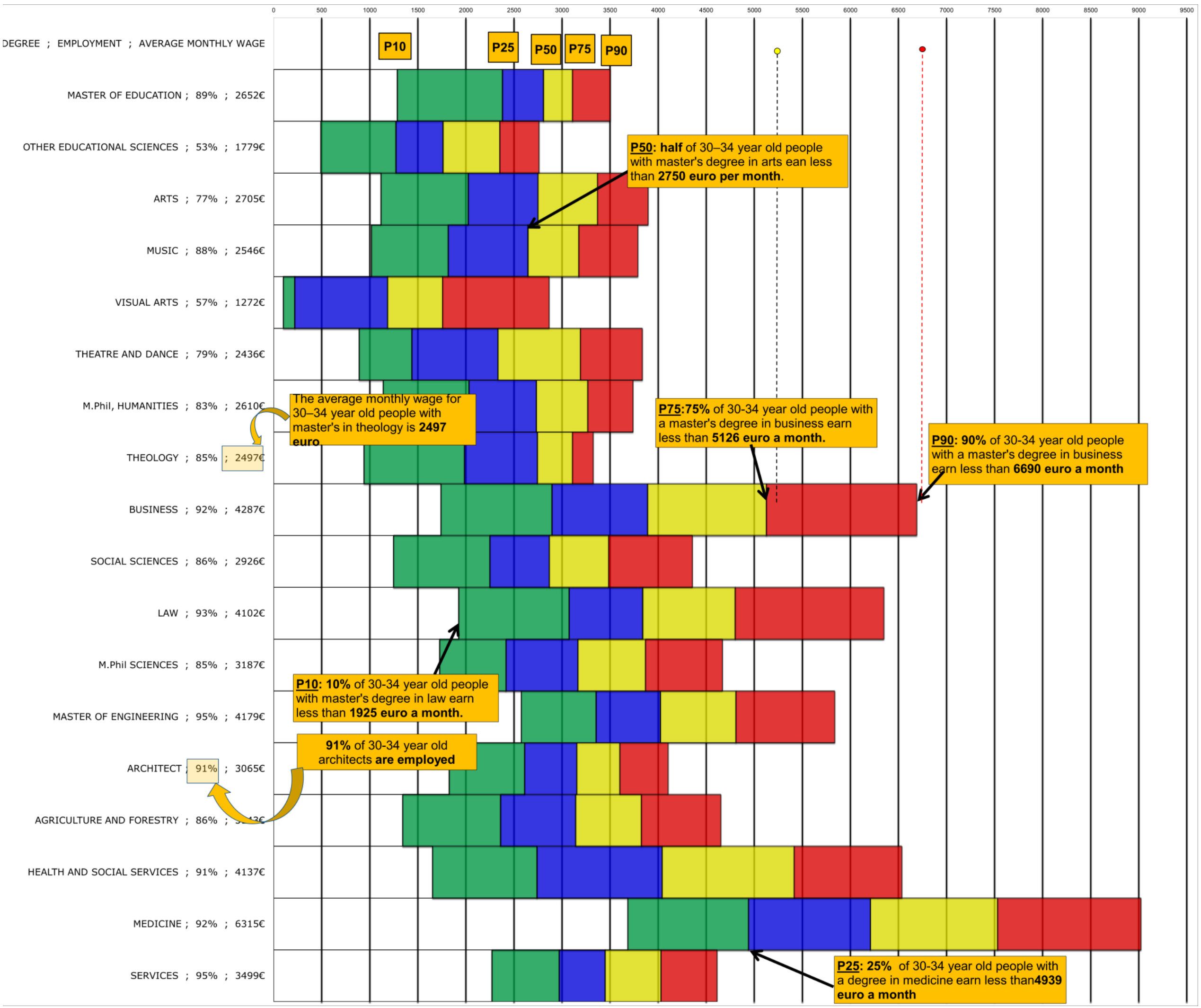
After giving the presentation, the counselors were asked to hand the information packages and questionnaire forms to the students and allow 15 to 20 minutes to fill in the questionnaires. Finally, we instructed the teachers to collect the questionnaires but let the students retain the information materials.[9]

The information package presented employment rates, average monthly earnings and a graph on the distribution of monthly earnings (first and ninth deciles, quartiles, and median) for current 30–34-year-olds holding each degree.[10] Figure 4 illustrates how we presented this information using a slide from the PowerPoint presentation provided to the counselors. We also listed the two most common occupations and the share of graduates in these occupations for each degree. To keep the package at a reasonable length, we

[9] All the materials provided to the schools, along with English translations, are available as an Online Appendix.

[10] The presence of entrance exams in the Finnish system rules out identification strategies similar to Hastings et al. (2013) and Kirkeboen et al. (2016) that would allow us to estimate causal effects of different degrees on labor market outcomes.

Figure 4: Extract from the slides provided to the student guidance councelors



Note: Slide 8 from the PowerPoint presentation provided to the student guidance councelors. Similar distributional graphs were provided in the information package regarding 104 degrees. We instructed the student guidance councelors to go through this slide slowly and provided detailed discussion about how to interpret it. See the treatment material, pages 3–4, for details (available at www.aalto-econ.fi/sarvimaki).

mostly used the 3-digit level of the education classification. However, we also reported separately those 4-digit level degrees for which the average earnings differed noticeably from the 3-digit level averages.[11] This criterion led us to use a classification of 41 secondary education degrees, 19 polytechnic degrees, and 44 university degrees. The student guidance counselors were instructed to stress that the figures refer to observed outcomes in the population and may not reflect, for example, what an individual student would earn with a particular degree. We argue that this is the most likely way in which a nation-wide policy of informing students about the labor market prospects associated with alternative educational choices would be constructed.

4 Survey results

This section summarizes the main results from the survey data collected as a part of the intervention. We ran the survey to acquire information on the students' expectations, the level of information they had about the labor market prospects associated with different educational choices, and the sources they relied on for such information. Altogether 3,418 students returned the survey, corresponding to 64% of the final year students in the schools that complied with the information experiment.

4.1 Expectations and sources of information

Our survey started with questions about the expectations regarding future education. Almost everyone responded that they intended to continue their studies after high school.[12] Figure 5a shows that 55% of women and 48% of men thought that they would obtain a Master's degree. Roughly a quarter expected to obtain a polytechnics degree, while a fifth answered that they did not yet know. Only 5% thought that they would enter the labor market with only a secondary degree.

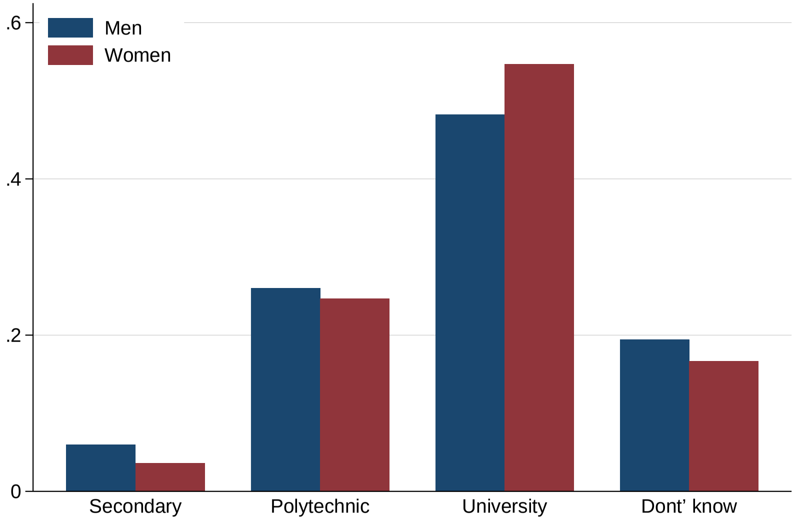
We then asked the students to list up to four programs they were planning to apply to and what they considered important factors when making this choice. Figure 5b

[11] For example, while most of the university level engineering degrees are well-described by the 3-digit level MSc in Technology, graduates from the Industrial Management program earn significantly more and graduates from the Process Technology program significantly less than the average engineering graduate.

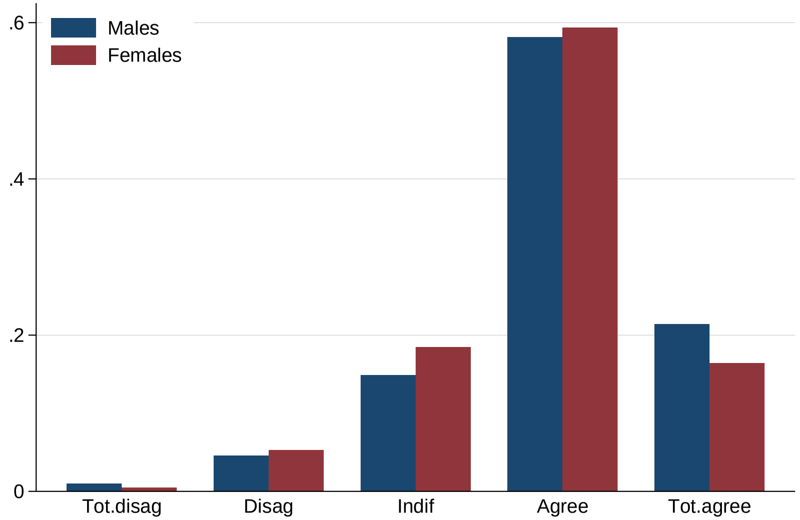
[12] 94% were planning to apply to post-secondary education, 0.4% said that they do not have such plans, and 5% stated that they were unsure. Furthermore, 60% stated that they planned to apply directly after finishing with the matriculation examination.

Figure 5: Survey results

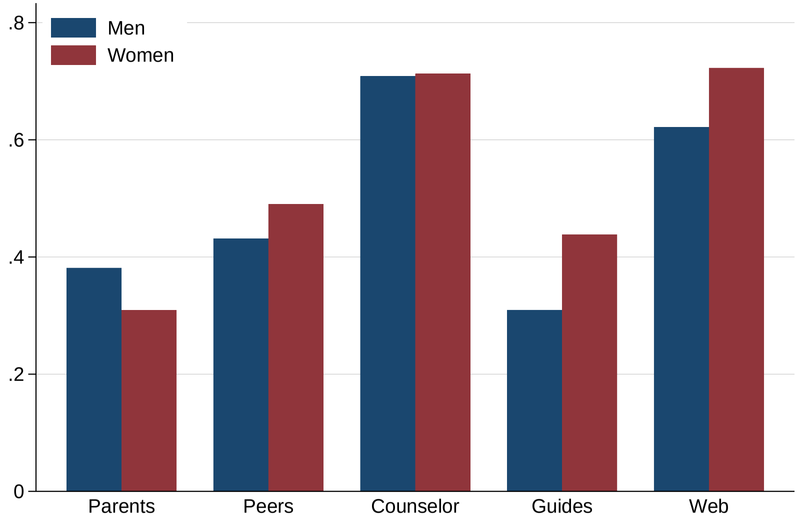
(a) "What is the highest level of education you think you will obtain?"



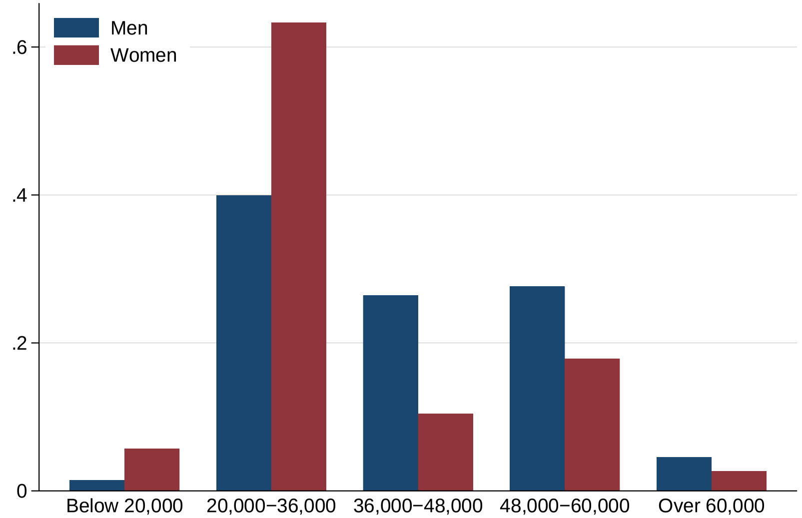
(b) "When selecting a place to study I consider [post-education earnings] to be important"



(c) "From whom have you received useful information about further study opportunities?"



(d) Average annual earnings associated with the first choice program



presents the answers for the part inquiring about the importance of post-education earnings. Roughly four-fifths of the students agree or strongly agree that future earnings are an important factor in their educational choice. Men place slightly more emphasis on earnings than women. However, gender differences are much clearer when asked about the other dimensions of programs. In particular, women put more emphasis on whether the subject that they study is interesting and whether it leads to an interesting job. (See Online Appendix Figure C.1).

The survey continued with questions about how informed the students felt about labor market prospects associated with alternative degrees and where they obtained this information. Two-thirds of men and 56% of women considered themselves to be well informed. Figure 5c shows that the most important sources of information were the student guidance counselor and the internet, with parents, peers, and study guides playing a smaller role.

Finally, Figure 5d plots the distributions of average monthly earnings of the students' first ranked programs by gender. It reveals a considerable variation in the average monthly earnings of the programs where the students are planning to apply to and shows that women are planning to apply to programs that are associated with lower-paid jobs than men.

4.2 Belief updating

The next section of the survey examined the extent to which the intervention led to belief updating. We first asked students to check the average earnings and employment rates of their preferred degree from the supplementary material and to write these numbers down.[13] We then asked them whether these averages were higher, equal to, or lower than what they had expected. Roughly 19% reported to be negatively surprised, while 18% were positively surprised about the earnings information.

We acknowledge that this approach for measuring belief updating has important limitations. Ideally, we would like to have pre- and post-treatment information on students' beliefs over average outcomes—as well as their own outcomes—in all programs. However,

[13]The primary motivation for asking this question in this form was to make sure that the students looked at the information package. It also allowed us to check the consistency of their answers: 72% and 73% of the students provided the correct earnings and employment rates, respectively, given their declared first-choice degree.

asking students to fill in this information for the 104 programs in our information leaflet would not have been feasible during the approximately 25 minutes reserved to reply to the survey during the guidance counselor class. Indeed, we would have been hesitant to ask them to engage in such a massive effort even if we had not been constrained by the length of the class.[14] Furthermore, we had no strong priors on how the students would process the information. For instance, the relevant information for some may have been that those holding a medical degree earned 1.9 times more than those with a psychology degree while others may have found it more useful to learn that the average monthly earnings of those holding a psychology degree was 2,613 euros. Given these constraints and potential complications, we simply asked the students whether monthly earnings and employment rates in their favorite program were higher or lower than they had expected.

Table 3 reports the distribution of self-reported surprises by the students' background characteristics. It shows that being negatively surprised is associated with being a woman, reporting to be poorly informed to begin with and stating that the post-education earnings are important. In addition, surprises are correlated with several measures of academic achievement. Respondents who eventually took the advanced mathematics test in the matriculation exam are less likely to be surprised than students who did not. Furthermore, students graduating from lower quality schools—defined as the school's average matriculation grades being below median—are more likely to be surprised by the information we provided. The average matriculation examination scores of the students themselves or the educational attainment of their parents do not seem to be correlated with being surprised.

Table 4 presents the surprises by the field of study.[15] It reveals that among reasonably large university fields, students who listed business, medicine, and engineering as their first choice tended to be positively surprised, whereas those planning to apply to an education or psychology degree tended to be negatively surprised. Interestingly, there is a positive correlation between the fields' average earnings and the direction of surprises, suggesting that students may underestimate cross-field earnings differences.

[14] In comparison, earlier studies that have elicited beliefs about returns to education have either focused on average returns to college education (Dominitz and Manski, 1996; Attanasio and Kaufmann, 2009) or surveyed beliefs about returns to highly aggregated fields of study among students whose choice set is already more limited than that of high school students (Zafar (2011); Zafar (2013); Wiswall and Zafar (2015)).

[15] In table 4, as well as in table 1 report descriptive statistics on more aggregated program codes that we used in the actual intervention. This higher level of aggregation is used for illustrative purposes only.

Table 3: Belief updating by background characteristics and survey responses

	Earnings less than expected	Earnings equal to expectations	Earnings larger than expected
A: Gender			
Men	14.0	64.3	21.8
Women	23.1	61.5	15.4
B: "I think I know enough about the effect of education choices on earnings"			
No	26.9	55.1	18.0
Yes	15.0	67.0	18.0
C: "When selecting a place to study I consider post-education earnings to be important"			
Disagree	15.3	63.2	21.5
Agree	20.7	62.4	16.9
D: Takes advanced math in the matriculation exam			
No	22.4	61.4	16.2
Yes	15.1	64.3	20.6
E: Own matriculation exam score			
Below median	19.7	62.0	18.3
Above median	19.1	63.2	17.7
F: School's average matriculation exam score			
Below median	22.8	60.4	16.8
Above median	17.9	63.6	18.5
G: Parents have post-secondary degrees			
No	19.5	62.1	18.4
Yes	20.3	62.3	17.5
All	19.4	62.6	18.0

Table 4: Updating Beliefs about Average Wages by Field

	Earnings less than expected	Earnings equal to expectations	Earnings larger than expected	Did not answer	Mean	Obs.
			A: Polytechnics			
Humanities	0.18	0.55	0.15	0.11	-0.03	123
Arts	0.25	0.25	0.13	0.38	-0.20	8
Business	0.16	0.68	0.12	0.04	-0.05	135
Engineering	0.09	0.64	0.21	0.05	0.13	118
Agriculture	0.26	0.57	0.13	0.04	-0.14	23
Nursing	0.19	0.69	0.08	0.05	-0.12	278
Services	0.24	0.62	0.05	0.10	-0.21	156
			B: Universities			
Education	0.37	0.55	0.05	0.02	-0.33	167
Arts	0.21	0.49	0.23	0.07	0.02	61
Humanities	0.19	0.64	0.13	0.04	-0.06	189
Business	0.04	0.62	0.31	0.03	0.28	271
Social sciences	0.25	0.61	0.08	0.07	-0.19	104
Psychology	0.57	0.35	0.05	0.04	-0.54	109
Law	0.08	0.78	0.13	0.01	0.04	159
Natural sciences	0.24	0.56	0.11	0.09	-0.14	117
Engineering	0.08	0.62	0.27	0.03	0.20	128
Ind. management	0.00	0.50	0.40	0.10	0.44	10
Architecture	0.26	0.55	0.11	0.08	-0.17	38
Agriculture	0.00	0.56	0.44	0.00	0.44	9
Medicine	0.04	0.68	0.25	0.02	0.22	221
Other health care	0.34	0.52	0.09	0.05	-0.27	93
Services	0.25	0.60	0.11	0.04	-0.14	81
Total	0.19	0.62	0.15	0.05	2,598	

Note: The average updating is calculated by assigning value -1 for negative, 1 for positive, and 0 for no surprises. The measurement of surprises by field is based on students who listed a program in that field as their number one choice at the time of survey.

5 Experimental results

This section presents the experimental results. We start with a description of the applicant register data and then present the application-level and student-level analysis of the intervention's impacts.

5.1 Application register

Our experimental analysis is based on data drawn from two centralized registers maintained by the Ministry of Education (HAREK and AMKOREK). These registers are used to allocate students to post-secondary programs. For each student, we observe her full set of applications, whether she attended the entrance examination, whether she was accepted into the program and whether she eventually chose to enter. In addition, the data contain the students' detailed matriculation exam grades and the name of the high school from which she graduated. We have access to this information for years 2011–2013.

The register data have several important strengths. They allow us to observe post-intervention outcomes without having to reach the students for a second survey. Thus we avoid attrition problems that often plague experimental designs. We can also keep track of the students who did not obtain offers in 2012 and observe their application behavior in 2013. Also, we observe the application patterns of students graduating from the control schools without having to convince these schools to be a part of the experiment. Finally, we observe the pre-intervention applications from both the treatment and control schools.

In Table 5, we report the share of applications across programs by treatment status among the graduating students. This table provides the first look at the effects of the intervention. If our intervention had major effects on the allocation of applications across programs that should be visible here. However, the distribution of applications appears very similar in the treatment and control groups. We use randomization inference to formally test whether the distributions of applications differ between the treatment and control schools (Fisher, 1935; Rosenbaum, 2002).[16] The p-value of the test for the equality

[16]This choice of inference yields correct p-values despite the potentially complex clustering structure in our data. Such clustering would arise, for example, if application behavior were affected by school-level factors such as peer behavior and geographical location. The issue is further amplified by the fact that several applications typically originate from the same student. Thus standard inference procedures could be severely misleading. In practice, we draw random placebo assignments P_s using the same randomization process over schools as was used in the real intervention. We then calculate the estimate of interest, $\hat{\delta}_P$, for each placebo assignment and obtain an empirical c.d.f distribution $F(\hat{\delta}_P)$. P-values

Table 5: Applications by Field and Treatment Group

	Control (1)	Treated (2)
A: Polytechnics		
Humanities	3.01	2.72
Arts	2.65	2.31
Business	10.28	10.85
Engineering	7.28	7.37
Agriculture	0.94	1.14
Nursing	15.02	15.48
Services	3.99	3.93
B: Universities		
Education	7.84	7.34
Arts	1.37	1.29
Humanities	8.69	8.62
Business	6.25	6.20
Social sciences	4.56	5.07
Psychology	2.14	2.02
Law	1.32	1.40
Natural sciences	10.33	10.21
Engineering	8.26	8.12
Ind. management	1.08	1.05
Architecture	0.69	0.57
Agriculture	0.36	0.40
Medicine	1.74	1.75
Other health care	1.18	1.21
Services	1.00	0.97
Applications	71,156	23,720
Randomization inference P-value	0.88	

Note: Columns 1 to 2 report the distribution of applications from the treatment and control high-schools in 2012 (post-treatment) among graduating students. Bottom row reports the p-value for the test of the equality of columns (1) and (2). This p-value is calculated by randomization inference (see the text for the dicussion of this method).

of the distribution of applications across degrees by treatment status is 0.88. That is, we cannot reject the null hypothesis that the distributions are identical.[17]

5.2 Impact on enrollment and application portfolios

We now turn to examine the impact of the intervention on enrollment patterns and individual-level application portfolios. Table 6 presents estimates for the impact of the intervention on enrollment and application outcomes. The estimates correspond to β, in regression:

$$y_{ijs} = \alpha_j + \beta D_s + \mu_s + \epsilon_{ijs} \tag{1}$$

where y_{ijs} is the outcome for student i in year j after graduating from high school and belonging to strata s, D_i is an indicator taking value one if she graduated from a high school that was offered the intervention (zero otherwise), and μ_s is a vector of strata fixed-effects.

In columns (1) to (3) of panel A, Table 6, the dependent variable is an indicator for enrolling in any post-secondary program immediately after graduating from high school. The point estimates are close to zero, and the school level clustered standard errors suggest that we can exclude economically significant effects. The randomization inference p-values show that all estimates are far from being statistically significant. In columns (4) to (6), we examine the situation one year after graduation. Again, we find no evidence on the intervention having an impact on the overall enrollment.

The next two panels repeat the analysis for the log mean earnings (B) and employment rate (C) of the field in which the accepted applicants were enrolled in. Again, the point estimates are close to zero, precisely estimated and statistically and economically insignificant.

These results suggest that our information intervention did not affect the average likelihood of enrolling in tertiary education or the types of programs in which the students enrolled. In the remainder of Table 6, we examine whether the intervention affected *applications*, even if it did not affect enrollment. This analysis complements the application level analysis reported in Table 5 by characterizing the application portfolios at

are calculated by comparing where the measured real treatment effect, $\hat{\delta}$, falls in the distribution $F(\hat{\delta}_P)$.

[17] In the Online Appendix Table C.1 we show that the applications of the treatment and control schools were equally distributed across programs also in 2011, i.e., one year before our intervention took place.

Table 6: The effect of the information intervention

		Year of graduation			Year after graduation		
		All	Men	Women	All	Men	Women
	Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)
(A)	Enrolled	-.007	-.018	.003	-.011	-.008	-.015
		(.011)	(.015)	(.012)	(.010)	(.016)	(.013)
		[0.543]	[0.301]	[0.823]	[0.372]	[0.645]	[0.324]
(B)	log Average earnings in the program where enrolled	-.006	-.008	.001	-.010	-.019	-.005
		(.008)	(.007)	(.008)	(.009)	(.010)	(.010)
		[0.503]	[0.333]	[0.919]	[0.288]	[0.116]	[0.661]
(C)	Average employment rate in the program where enrolled	-.002	-.002	-.002	-.002	-.001	-.002
		(.001)	(.002)	(.002)	(.002)	(.002)	(.002)
		[0.213]	[0.246]	[0.446]	[0.412]	[0.723]	[0.436]
(D)	log Mean earnings of the application portfolio	-.001	-.007	.006	-.002	-.003	.001
		(.007)	(.007)	(.007)	(.007)	(.007)	(.008)
		[0.953]	[0.392]	[0.529]	[0.766]	[0.708]	[0.904]
(E)	log Expected earnings of the application portfolio	-.003	.011	-.005	.006	.034	-.005
		(.024)	(.031)	(.025)	(.022)	(.033)	(.027)
		[0.922]	[0.773]	[0.886]	[0.849]	[0.399]	[0.890]
(E)	Number of applications	-.063	-.044	-.076	.094	- .038	.158
		(.075)	(.092)	(.084)	(.060)	(.078)	(.077)
		[0.436]	[0.666]	[0.423]	[0.168]	[0.671]	[0.084]

Note: Cross sectional ITT estimates, school-level clustered standard errors (in parantheses) and randomization inference p-values [in brackets] using 10,000 replications. Log expected value of the application portfolio is scaled to have a standard deviation of one.

the student level. The aim is to improve statistical power and to facilitate interpretation. The challenge, however, is that it is not apparent how an application portfolio should be characterized in a setting where students can apply to as many as 11 programs out of a total of 658. This abundance of choice also makes it infeasible to directly examine each possible combination of applications.[18]

Our approach is to use two *ad hoc* but arguably reasonable measures that characterize the applicants' application portfolio. The first is simply the log mean earnings of the fields that the applicants include in their portfolio. While intuitive, the weakness of this measure is that it does not take into account that students may apply to programs where they are very unlikely to be admitted to. As an alternative measure, we use log expected earnings of the application portfolio, where the average earnings associated with each application are weighted by the student's likelihood of being accepted, as predicted by her matriculation exam results, and by taking into account that the student can enter only one program (see Online Appendix B for details). In order to ease interpretation, we scale this outcome to have a standard deviation of one.

The results presented in panels D and E, Table 6, verify the conclusions from the application level analysis. The estimated impacts on the average log earnings and expected earnings of the application portfolio are precisely estimated zeros. The point estimates for the expected earnings of the number of applications in the portfolio are similarly small and insignificant, except for marginally significant but very small increase for women in the year following the experiment.

As a robustness check, we report differences-in-differences estimates in Online Appendix Table C.2. These results come from a differences-in-differences version of regression (1), where we treat applications in the year 2011 as the baseline and add a full set of high school fixed effects as controls. The results are qualitatively similar to the cross-sectional estimates except that the effect on the average employment rate of the program where the applicants are enrolled is negative and statistically significant. However, even this estimate, at -0.003, is small. It is also very close to the cross-sectional estimate of

[18] There are $\binom{658}{11} + \binom{658}{10} + ... + \binom{658}{1} = 2.35 \times 10^{23}$ possible application combinations. Some theoretical work examining problems approaching this level of complexity exists, but existing results are not sufficient to, e.g., characterize the optimal strategy in our context. The problem is particularly hard because preparing for an entrance exam for one program decreases the likelihood of being accepted by other programs (due to time constraints). Thus the tools introduced by Chade and Smith (2006) and Chade et al. (2014) are not directly applicable in the Finnish context.

-0.002. Similarly, the estimates for the number of applications are very close to the cross-sectional results, but now the small positive second-year effect for women is statistically significant.

6 Interpretation

The results discussed above suggest that our intervention led to significant belief updating, but did not have a statistically or economically significant impact on the actual average applications or enrollment. In this section, we discuss the potential explanations for this pattern of results. We start by showing that the students' survey answers are remarkably consistent with their later application behavior, and that the students who were disappointed about their initial choice were more likely to change their plans than others. We then present experimental results showing that students who were most likely to be negatively surprised about the labor market prospects of their favorite programs appear to have been affected by the intervention. However, the affected subgroup is not sufficiently large to make the estimates for the full student population significant.

6.1 Survey responses and application behavior

In our survey, we asked the respondents to permit us to link their responses to the application registers. Many refused, and some provided names that were not found in application data. Nevertheless, we were able to make this link for 1,168 students. For them, we can check whether they ended up applying to the program that they listed as their first choice in the survey, i.e., we can examine whether their plans changed between the time of the intervention (November 2011) and the application deadline (April 2012).

Table 7 tabulates the fraction of students who applied to at least one program in the field they listed as their first choice in the survey against whether they reported to be surprised about the average earnings of recent graduates in that field. We also tabulate the fraction of students who were offered a place and the fraction accepting an offer.

The first notable fact of Table 7 is that in the spring of 2012, approximately three-quarters of the students applied to the program that they had listed as their first choice in the survey almost half a year before the actual application process started. This suggests that the survey answers are informative about the students' intentions and that many of

Table 7: Belief updating and application behavior

	Earnings less than expected	Earnings equal to expectations	Earnings larger than expected	Total	χ^2-test p-value
Applied to first choice program	67.2	75.5	75.8	73.9	0.036
Accepted to first choice program	16.8	22.2	24.6	21.6	0.108
Enrolled in first choice program	14.2	20.8	23.2	20.0	0.038
N	232	725	211	1,168	

Note: Fractions of students whose survey responses could be matched to register data and who applied to a program that they listed as their most likely choice (first row), were admitted to their first choice program (second row) and eventually enrolled in their first choice program in the fall term of High School graduation year (third row). The numbers are calculated separately for students who responded that average earnings in their first choice program in the data they were provided along with the survey were more than, equal to or less than they had expected. Differences across these groups are tested using a simple χ^2-test.

them had seriously considered their post-secondary education at the time of the survey. Roughly a fifth were eventually accepted to a program that they listed as their first choice in our survey.

Interestingly, however, compared to other students, those who were negatively surprised by the information we provided were eight percentage points less likely to apply to the field that they reported as their first choice in the survey. The difference persists in the fraction accepted and eventually enrolling in their survey-time first choice program. Column 4 of Table 7 reports the p-values of a χ^2-test for the equality of the shares of students across columns. We reject the hypothesis that the shares are equal across columns for applications and enrollments (but not for acceptances).

6.2 Subsample analysis

The results reported in Table 7 suggest that our intervention may have affected a subsample of students who were negatively surprised by the information we provided them. However, the association between changing plans and being negatively surprised could be spurious. For example, students who had the most unrealistic expectations of the field they listed as their first choice in the survey may have spent less time thinking about their future. The same students could then be more likely to change their minds between the survey and actual application even if they had not participated in the information intervention.

Ideally, we would like to estimate the intervention's impact on the subsample of students who would have been negatively surprised if they had been in the treatment group. The challenge is that we do not observe who updated (or would have updated) her beliefs. In the treatment group, we could link only a third of the responses to the register data. In the control group, we did not collect any survey data due to practical considerations and an attempt to keep the control group as "pure" as possible.[19] Given these limitations, we resort to using the linked survey-register data to predict who was most likely to be negatively surprised. As was shown in Table 3, being negatively surprised was correlated with variables such as gender and optional subjects taken in high school. We can, therefore, estimate the predicted likelihoods of being negatively surprised for all the students in treatment and control schools using the linked survey-register data and background variables included in the register data.[20] Since we observe these variables for everyone, we can use the estimates to predict the probability of being negatively surprised for the full student population.

We examine whether the effect of our information intervention is heterogeneous along predicted likelihood of being negatively surprised by estimating regressions of the form:

$$y_{ijs} = \alpha_j + \beta D_s + \gamma(D_s \times \hat{p}_i) + \rho\hat{p}_i + \mu_s + \epsilon_{ijs} \tag{2}$$

where $\hat{p}_i$ is the predicted likelihood of being negatively surprised for student i. The parameter of interest is γ, which tells us whether the intervention's effect varies across students with different levels of predicted likelihood of being negatively surprised. In order to ease the interpretation of the results, we convert $\hat{p}_i$ to deviations from mean so that the main effect of being in the treatment school, β, can be interpreted as the effect of the treatment on those with an average likelihood of being negatively surprised.

[19]A major advantage of using register data is that we did not have to contact the control schools at all and could thus minimize the risk of unintentionally affecting their students. On the practical side, our approach for eliciting belief updating—asking students whether they were surprised by the information, see Section 4.2—could not have been implemented in the control schools even if we had been able to convince them to collect survey data without offering them a meaningful intervention.

[20]Specifically, we use a linear probability model and regress an indicator for the respondent reporting to be negatively surprised on sex, a full set of indicators for the optional subjects taken in the matriculation exam, and the average matriculation grade in the school in 2011. We present the results of the regressions in Online Appendix Table C.3.

Table 8: The effect of the information intervention interacted with the probability of being negatively surprised, year of graduation

		Program where enrolled		Application portfolio		
	Enrolled (1)	log Average earnings (2)	Average employment rate (3)	log Average earnings (4)	log Expected earnings (5)	Number of applications (6)
Treatment	-0.005	-0.001	0.001	0.003	0.005	-0.063
	(0.010)	(0.007)	(0.003)	(0.006)	(0.020)	(0.075)
	[0.700]	[0.864]	[0.618]	[0.688]	[0.863]	[0.433]
Predicted surprise	-1.223	-1.553	-0.282	-1.462	-3.590	-0.381
	(0.055)	(0.038)	(0.013)	(0.026)	(0.118)	(0.345)
Treatment $\times$ predicted surprise	0.174	0.088	0.036	0.098	0.229	-0.395
	(0.114)	(0.067)	(0.024)	(0.049)	(0.239)	(0.637)
	[0.115]	[0.213]	[0.150]	[0.058]	[0.339]	[0.542]
N	20,723	8,962	8,962	20,723	20,069	20,723

Note: Cross-sectional ITT estimates, school-level clustered standard errors (in parantheses) and randomization inference p-values [in brackets] using 10,000 replications of the information intervention.

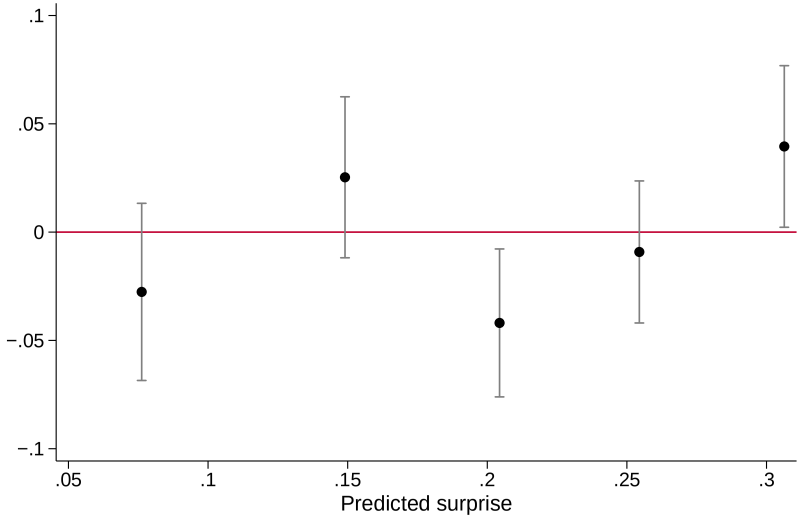
Table 8 reports the results using cross-sectional data for the year of graduation. In line with our main results, the main effect of the treatment is a precisely estimated zero for all outcomes. However, apart from the small decline for the number of applications, all point estimates for the interaction terms are positive, yet statistically significant only in the case of log average earnings of the application portfolio. The differences-in-differences estimates, reported in the Online Appendix Table C.4, are similar but stronger. That is, once we condition for high school fixed-effects, the interaction terms become significant for all outcomes except the likelihood of enrolling to any program and the number of applications.

Figure 6 reports results from an alternative approach where we split the sample into quintiles by the predicted likelihood of being negatively surprised. It shows that the resulting treatment effects are small and mostly insignificant for most values of the predicted surprises. However, among students who were the most likely to be negatively surprised, we find a positive effect on the average earnings of the application portfolio with a randomization p-value of 0.03 (Online Appendix Table C.5). These findings are in line with those from the linear specification (Table 8). Individuals who are the most likely to update their beliefs appear to change their application behavior as a result of the intervention by switching their applications towards higher wages programs. Furthermore, differences-in-differences results, reported in Online Table C.4, suggest that also enrollment patterns of these individuals were affected.

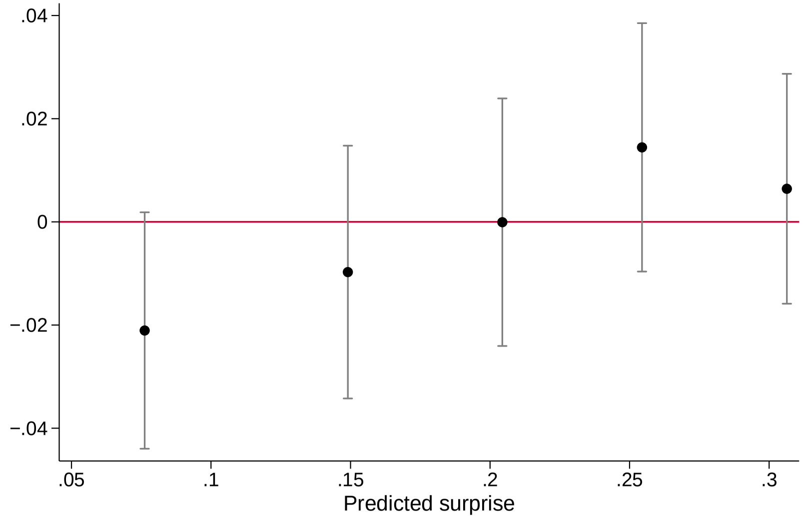
We interpret these results as suggestive evidence that our information intervention may have affected the behavior of those individuals who had overly optimistic beliefs about the labor market prospects of the fields that they were considering at the time of the intervention. However, this group appears to represent only a small fraction of the overall student population. Only 19 % of survey respondents were negatively surprised by the information given in the experiment. Incomplete compliance (only 64 schools out of 97 that were randomized to the treatment group participated in the experiment) further reduced the fraction of effectively treated students. In the end, only about 12% of treatment group students received information that could have induced them to revise their plans. This fraction appears to be too small to have a significant impact on average application behavior.

Figure 6: The effect of the information intervention by groups of predicted negative surprises

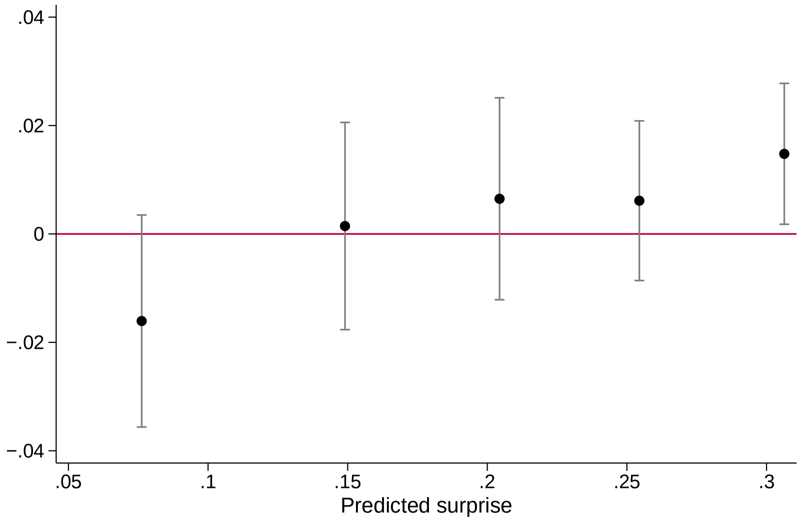
(a) Enrolled



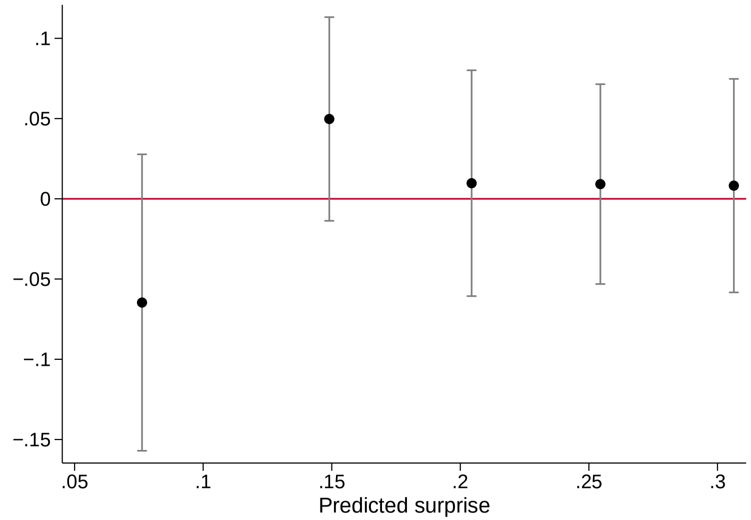
(b) log Average earnings in the program where enrolled



(c) log Mean earnings of the application portfolio



(d) log Expected earnings of the application portfolio



Note: Cross-sectional ITT estimates by predicted likelihood of being negatively surprised by the average earnings of one's first choice program. The 95% confidence intervals are based on standard errors that are clustered at high school level (see Appendix Table C.5 for randomization inference p-values).

7 Conclusions

Many commentators, politicians, and parents worry that high-school students make post-secondary educational choices that do not sufficiently prepare them for the labor market. These choices are often alleged to reflect a lack of information about actual labor market prospects associated with alternative degrees. Yet, the mere fact that some choices do not appear to maximize lifetime earnings does not necessarily mean that they are based on incomplete information. Degrees that offer lower earnings or employment prospects may attract students for other reasons. On the other hand, degrees associated with high earnings and employment rates are often heavily over-subscribed.

In this paper, we have reported results from a large randomized field experiment that provided accurate and detailed information about the earnings distribution and employment prospects associated with different post-secondary degrees in Finland. Our survey results confirm findings from previous literature suggesting that these kinds of information interventions lead to belief updating. Furthermore, our results suggest that students who were the most likely to be negatively surprised by the information we provided them switched to apply to programs that have higher average earnings. However, while the effects may be significant for these students, this subgroup represents only a small fraction of the overall student population. Thus our intervention did not have a statistically or economically significant impact on the overall application or enrollment patterns.

Our results suggest two policy lessons. First, providing accurate information on the labor market prospects associated with alternative degrees is likely to be a cost-efficient policy. Given the importance of post-secondary education choices—and the low cost of providing information—helping even a small fraction of students avoid making consequential mistakes provides a justification for this type of intervention. Nevertheless, it is important to bear in mind that, at least in the type of contexts we examine, better information alone is unlikely to have a major impact on the overall allocation of students into post-secondary programs.

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A Details of the experiment

A.1 Target schools

We started designing the experiment by collecting a list of all 431 Finnish high schools. We dropped evening and adult high schools, and other speciality schools such as religious institutes, resulting in a reduction of 32 schools. We further excluded the only high school in the autonomous Åland archipelago and another school operating in Spain for Finnish students located there, as well as any Swedish language high schools, or schools specializing in another language (e.g. French, German or Russian). Our final target group is the 2011 list of operating Finnish language schools which includes 363 high schools in the continental Finland.

A.2 Randomization

We used a randomized block design where we split the schools into bins of four school based on the province they are located in and their average matriculation examination grades during 2007–2010. When the number of schools was not divisible by four, the location of the incomplete bin in the ranking distribution was randomly selected. We then randomly selected one from each bin. Figure A.1 illustrates the research design by plotting schools againts the average grades within six provinces (out of the total 18 provinces). It shows how our treatment group consists of schools from the top and bottom of the ranking, in some cases including the very best or worst school in the province. The final treatment group consisted of 97 high schools.

A.3 Balancing tests

Table 2 examines the average characteristics of the treatment and control schools in 2011. There are no significant differences in the average matriculation grades of the treatment and control schools. The table also reports information on the background characteristics of the students using Statistics Finland's geocode data that reports the average share of high school and university graduates and the average household income by 250m x 250m squares (0.05 square miles or 30 acres). These data were linked to the application register using students' addresses. None of these background variables differ significantly between the treatment and control schools, with the borderline exception of regional unemployment. These results suggest that the randomization worked as intended.

A.4 Student guidance counselors

The student guidance counselors in high schools have multiple roles within the institution. According to a recent survey of 213 guidance counselors, their core tasks involve counseling, information sharing, planning, and "networking with stakeholders".[21] Each student works individually with the counselor to create their study plan which includes course choices, plans for the matriculation exam and plans for further education. In addition, the guidance counselors are responsible for student welfare, marketing, recruiting, development of the student counseling, and institution specific tasks assigned by the headmaster. The latter consist of monitoring exams, creating study guides, guiding visitors, a variety of control tasks, and receiving the matriculation examination registration forms. Most also maintain a website that includes links to universities and polytechnics, a link to the centralized tertiary education application system, and any other links that the counselor views relevant.

The job of student guidance counselors has evolved rapidly in recent years. Some of these changes are related to the matriculation examination reforms, changes in the organization and selection criteria in the polytechnics and universities, and the increased choices around the timing and content of the matriculation exam that has especially increased the work load with students at risk of dropping out and/or immigrant students. According to the guidance counselors, adjusting to these changes is taking up an increasing amount of their time and allowing them to spend less time on "traditional counseling activities".

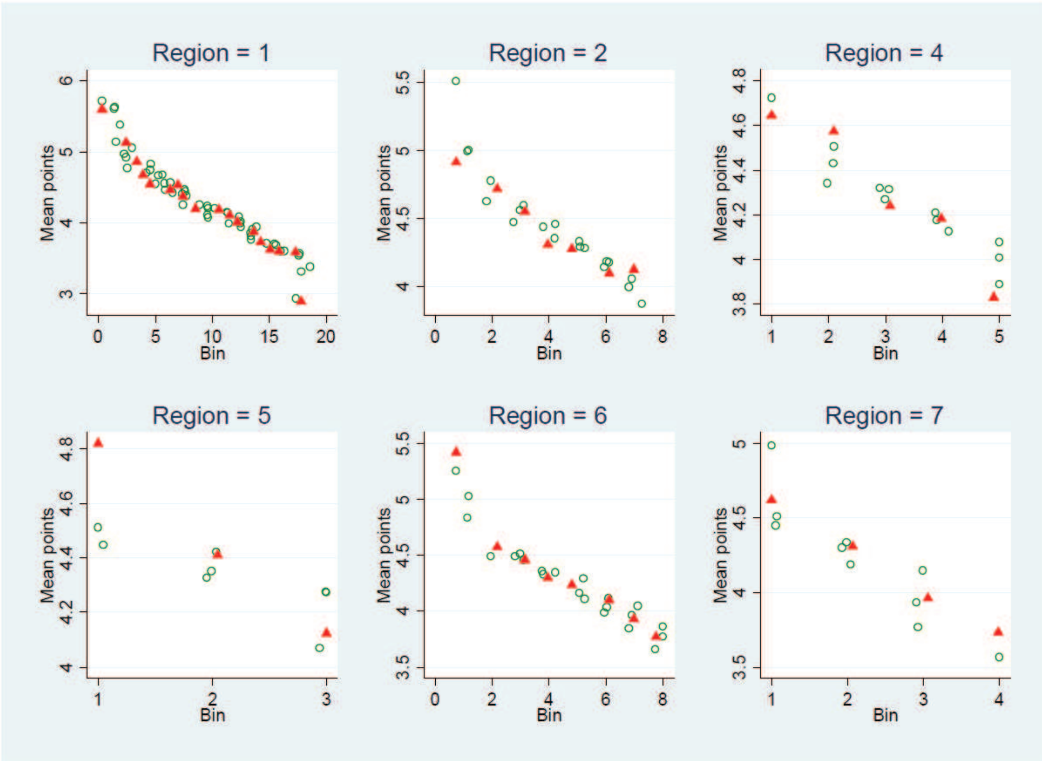
A.5 Pilot study

Before implementing the intervention in the treatment schools we piloted the entire experiment in a single high school during spring 2011. Most students (89%) in the pilot school thought the information on the labor market prospects related to education should be made available in all schools. Likewise, students in our treatment schools indicated

[21] Source: Lukion opinto-ohjaajan työnkuva- ja palkkauskysely 2011 ("High school guidance counselor job and salary survey 2011").

that the information was novel and useful, and the guidance councellors appeared enthusiastic about the materials. Based on the responses from the participating schools and the overall tone in the open-ended comments from students we expect the intervention to have been successful in communicating the message on the labor market prospects to participating students. The pilot school is excluded from our sample.

Figure A.1: Illustration of the randomized block design



Note: Distribution of high-schools in 6 example provinces. Schools are ranked according to the average matriculation grade of students graduating in 2007–2010, and divided into bins of four. Schools within the same bin are clustered together in the plot, and treatment schools are indicated with a triangle.

B Constructing expected earnings of an application portfolio

We measure the expected income associated with an application portfolio of each individual as

$$V_i = p_{i1}E_1 + \sum_{j=2}^{J_i} \left[\prod_{n=1}^{j-1} (1 - p_{in}) \right] p_{ij} E_j \tag{B1}$$

where p_{ij} is the probability that person i is admitted to her j^{th} choice, and E_j is the expected earnings associated with her j^{th} choice. The logic of this measure is the following. If a person applies to only one program, she can either be admitted and receive E_1 or be rejected and get her outside option (normalized as zero). The probability that she is admitted is p_{i1} and thus her expected income is $p_{i1}E_1$. If instead she applies to two programs, she can be admitted to her first choice and receive E_1, be rejected from the first choice but admitted to the second and get E_2, or be rejected from both and get nothing. Thus her expected income is $p_{i1}E_1 + (1 - p_{i1})\, p_{i2}E_2$. Equation (B1) generalizes this idea for a person applying to J_i programs.

A useful feature of this measure is that the p_i is person specific and thus applying to a high paying program increases expected income only to the extent that the person has a chance of being admitted. We estimate these probabilities using the 2011 application register data. For each program, we take all applicants and regress an indicator for being accepted on the matriculation exam results of the applicants using a flexible specification (dummies for each possible grade in the four subjects of the matriculation exam and interacting math grades with a dummy for long curriculum). Using estimates from these regressions, we then construct the probabilities that an application in our data would be admitted.

The approach also has three important limitations. First, it could be sensitive to the ranking of applications. We do not observe these rankings in our data and thus we have to impose them. For our baseline results, we have assumed that the students rank their applications based on the average earnings of each degree (in the order from the largest to the smallest). In order to examine the importance of this assumption, we have also experimented with a random ranking and a ranking based on the likelihood of being accepted to a program. We reach similar conclusions with all these approaches and thus conclude that the issue is not important in practice. Second, the approximation of the expected income is based on degree-level averages and therefore does not take into account any within-degree heterogeneity. Third, the calculation of the expected income of the portfolio is based on the assumption that the elements of vector p_i are independent of each other. This assumption is violated, for example, in the realistic situation where the study material tested in the entrance examinations of several programs partly overlap.

While these limitations are real, they should affect the measurement of the application portfolios of the treatment and the control groups in a similar way. Thus we consider the issues primarily as measurement error in the outcome variable, which should not bias our estimates (but will make them less precise).

C Additional results

Table C.1: Applications by Field and Treatment Group in 2011

	Control (1)	Treated (2)
A: Polytechnics		
Humanities	3.50	3.22
Arts	2.93	2.83
Business	10.06	9.83
Engineering	7.49	8.13
Agriculture	1.05	1.21
Nursing	15.42	15.20
Services	4.90	4.48
B: Universities		
Education	6.77	6.73
Arts	1.53	1.31
Humanities	9.22	8.78
Business	5.62	5.33
Social sciences	4.94	5.55
Psychology	1.88	1.91
Law	1.24	1.42
Natural sciences	10.27	10.57
Engineering	7.67	7.91
Ind. management	1.01	1.07
Architecture	0.79	0.44
Agriculture	0.43	0.46
Medicine	1.47	1.48
Other health care	1.05	1.25
Services	0.79	0.88
Applications	72,128	23,728
Randomization inference P-value	0.36	

Note: Columns 2 to 4 report the distribution of applications from the treatment and control high-schools in 2011 (pre-treatment). Bottom row reports the p-value for the test of the equality of columns (1) and (2). This p-value is calculated by randomization inference (see the text for the dicussion of this method).

Table C.2: Differences-in-differences estimates for the effect of the information on enrollment and application portfolios

		Year of graduation			Year after graduation		
		All	Men	Women	All	Men	Women
	Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)
(A)	Enrolled	-0.001	-0.014	0.011	0.007	-0.003	0.016
		(0.011)	(0.020)	(0.013)	(0.017)	(0.023)	(0.023)
		[0.938]	[0.149]	[0.243]	[0.453]	[0.721]	[0.098]
(B)	log Average earnings	-0.011	-0.008	-0.007	-0.004	-0.009	0.007
	in the program	(0.008)	(0.008)	(0.010)	(0.012)	(0.015)	(0.014)
	where enrolled	[0.074]	[0.220]	[0.302]	[0.517]	[0.131]	[0.255]
(C)	Average employment	-0.003	-0.002	-0.003	-0.002	-0.002	-0.002
	rate in the program	(0.002)	(0.002)	(0.002)	(0.003)	(0.004)	(0.003)
	where enrolled	[0.046]	[0.229]	[0.049]	[0.087]	[0.145]	[0.281]
(D)	log Mean earnings	-0.001	-0.005	0.006	0.005	0.003	0.010
	of the application	(0.006)	(0.008)	(0.006)	(0.006)	(0.010)	(0.007)
	portfolio	[0.774]	[0.378]	[0.291]	[0.367]	[0.545]	[0.057]
(E)	log Expected earnings	0.013	0.030	0.009	0.000	0.018	0.004
	of the application	(0.026)	(0.040)	(0.030)	(0.027)	(0.048)	(0.034)
	portfolio	[0.590]	[0.200]	[0.701]	[0.999]	[0.452]	[0.853]
(E)	Number of applications	0.059	0.075	0.035	0.115	0.030	0.173
		(0.087)	(0.103)	(0.110)	(0.094)	(0.119)	(0.119)
		[0.388]	[0.270]	[0.597]	[0.091]	[0.653]	[0.011]

Note: Differences-in-differences ITT estimates, school-level clustered standard errors (in parantheses) and randomization inference p-values [in brackets] using 10,000 replications. Log expected value of the application portfolio is scaled to have a standard deviation of one. Each cell reports the estimate of the coefficient of the interaction of treatment status and year 2012 dummies from a regression which uses pooled data from 2011 and 2012 application registers and controls for year 2012 main effects as well as a full set of high school dummies.

Table C.3: Regression of negative surprises on matriculation exam choices and school characteristics

	(1)	(2)
Female	0.0437	0.0366
	(0.0251)	(0.0261)
Advanced mathematics	-0.0360	-0.0334
	(0.0286)	(0.0297)
Lutheran	0.0150	0.0199
	(0.0440)	(0.0465)
Orthodox	-0.275	-0.382
	(0.391)	(0.418)
Ethics	-0.252	-0.210
	(0.196)	(0.199)
Philosophy	-0.00584	-0.0119
	(0.0608)	(0.0623)
Psychology	0.0388	0.0427
	(0.0295)	(0.0311)
History	-0.0970	-0.0818
	(0.0315)	(0.0326)
Physics	-0.0615	-0.0748
	(0.0368)	(0.0385)
Chemistry	-0.00744	0.00831
	(0.0353)	(0.0366)
Biology	0.00439	0.00244
	(0.0310)	(0.0323)
Geography	-0.0116	-0.0148
	(0.0346)	(0.0362)
Health education	-0.00424	-0.0165
	(0.0274)	(0.0291)
Social science	-0.0588	-0.0572
	(0.0302)	(0.0312)
School bl. median	-0.0697	-0.116
	(0.0343)	(0.123)
Controls for other school characteristics	No	Yes
Constant	0.230	4.274
	(0.0361)	(6.634)
Observations	1,272	1,265
R-squared	0.042	0.073

Note: Dependent variable is an indicator for survey response that the average wage of the field where the respondent was thinking of applying is less than (s)he expected. Explanatory variables are dummy variables that take value one if the respondent took the subject in the matriculation exam (except for School bl. median which takes value one if the average matriculation grade of the school was below median). Controls for other school characteristics include shares of applications from the school by field, shares of students who took the entrance exam by field, shares of students who were accepeted by field, and shares of students who were enrolled by field. We use results from column (1) to predict negative surprises in the rest of the population. School-level clustered standard errors are reported in parantheses.

Table C.4: The effect of the information intervention interacted with the probability of being negatively surprised

		Program where enrolled		Application portfolio		
	Enrolled (1)	log Average earnings (2)	Average employment rate (3)	log Average earnings (4)	log Expected earnings (5)	Number of applications (6)
Treatment × post	0.001	-0.008	-0.000	0.001	0.023	0.061
	(0.011)	(0.007)	(0.003)	(0.004)	(0.022)	(0.060)
	[0.901]	[0.284]	[0.969]	[0.807]	[0.370]	[0.419]
Predicted surprise × post	0.017	-0.082	-0.038	-0.084	-0.233	-0.458
	(0.065)	(0.043)	(0.016)	(0.025)	(0.194)	(0.359)
Treatment × predicted surprise × post	0.089	0.181	0.058	0.155	0.458	0.707
	(0.130)	(0.086)	(0.031)	(0.050)	(0.260)	(0.719)
	[0.389]	[0.010]	[0.025]	[0.001]	[0.034]	[0.237]
Post	-0.014	0.004	-0.001	0.011	-0.002	0.047
	(0.005)	(0.004)	(0.001)	(0.002)	(0.011)	(0.031)
Predicted surprise	-1.228	-1.452	-0.243	-1.355	-3.480	-0.124
	(0.048)	(0.032)	(0.012)	(0.018)	(0.096)	(0.265)
Treatment × predicted surprise	0.005	-0.148	-0.041	-0.108	-0.233	-0.575
	(0.097)	(0.064)	(0.023)	(0.037)	(0.194)	(0.536)
N	41,917	18,464	18,464	41,917	41,917	41,917

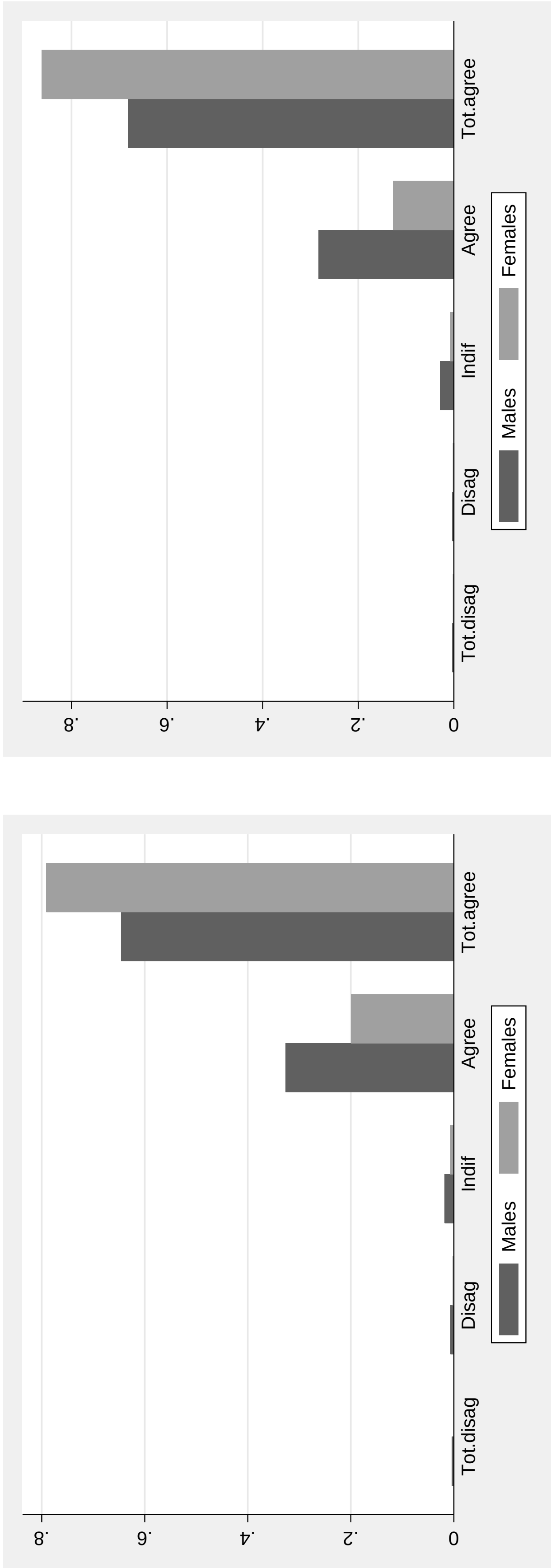
Note: Differences-in-differences ITT estimates, school-level clustered standard errors (in parantheses) and randomization inference p-values [in brackets] using 10,000 replications of the information intervention.

Table C.5: The effect of the information intervention by groups of predicted negative surprises

	Dependent variable	Predicted likelihood of being negatively surprised				
		(lowest) Group 1 (1)	Group 2 (2)	Group 3 (3)	Group 4 (4)	(highest) Goup 5 (5)
(A)	Enrolled	-0.028	0.025	-0.042	-0.009	0.040
		(0.021)	(0.019)	(0.017)	(0.017)	(0.019)
		[0.218]	[0.261]	[0.050]	[0.645]	[0.075]
(B)	log Average earnings in the program where enrolled	-0.021	-0.010	-0.000	0.014	0.006
		(0.012)	(0.012)	(0.012)	(0.012)	(0.011)
		[0.146]	[0.534]	[0.995]	[0.314]	[0.627]
(C)	Average employment rate in the program where enrolled	-0.005	-0.002	-0.000	0.002	0.012
		(0.003)	(0.004)	(0.004)	(0.006)	(0.007)
		[0.192]	[0.717]	[0.934]	[0.767]	[0.167]
(D)	log Mean earnings of the application portfolio	-0.016	0.001	0.006	0.006	0.015
		(0.010)	(0.010)	(0.010)	(0.007)	(0.007)
		[0.193]	[0.915]	[0.557]	[0.529]	[0.030]
(E)	log Expected earnings of the application portfolio	-0.065	0.050	0.010	0.009	0.008
		(0.047)	(0.032)	(0.036)	(0.032)	(0.034)
		[0.206]	[0.252]	[0.818]	[0.810]	[0.842]
Observations (full)		4,265	4,257	4,076	4,044	4,081
Observations (conditional on enrollment)		2,351	2,206	1,747	1,450	1,208

Note: Cross-sectional ITT estimates, school-level clustered standard errors (in parantheses) and randomization inference p-values [in brackets] using 10,000 replications of the information intervention. Groups are defined by predicted surprises based on the results in Table C.3. Group 1 has the lowest predicted surprise and group 5 the highest.

Figure C.1: Additional survey results



(a) "When selecting a place to study I consider an interesting post-education job important"

(b) "When selecting a place to study I consider the content of the studies important"