CESifo
AREA CONFERENCES 2021

Economics of Education

Munich, 3–4 September 2021

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by

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August 27, 2021

Abstract:

We estimate means and distributions of ex-ante treatment effects for obtaining university education relative to high school. To achieve this, we conducted a survey which elicited earnings expectations associated with counterfactual educational choices for a sample of students in Stockholm. We find average ex-ante returns to university to be 36%, with higher returns for females, those with high SES backgrounds and high math scores. The returns are highest for those that choose university, but also positive and sizable for those that don't. Our results imply that students sort into education based on their comparative advantage, but not absolute advantage. Our results also suggest that OLS estimates should be expected to be only somewhat downward biased estimates of the average treatment and treatment on the treated effects from university education. Additionally, we find evidence that the positive ex-ante earnings returns to high paying fields, among those that do not choose these fields, can (partly) be reconciled by individuals expecting to be compensated through higher non-pecuniary returns to those fields.

Keywords:
JEL-codes:

[*] We are grateful for comments from Joseph Hotz, Martin Nybom and from participants at seminars at IFAU, Tinbergen Institute, SOFI, JRC/EU commission, Århus University and University of Gothenburg. Mikael Lindahl acknowledges financial support from Jan Wallanders and Tom Hedelius Stiftelse, Tore Browaldh Stiftelse and Torsten Söderberg foundation.
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1. Introduction

If returns to education are heterogeneous and educational decisions are made with uncertainty, it is a prospective student's expected return, or the *ex-ante* treatment effect, that is relevant for understanding their educational decision making.[1] However, inferring ex-ante treatment effects from ex-post data requires very strong assumptions such as rational expectations and no unanticipated earnings shocks.[2] By eliciting subjective expected outcomes for counterfactual educational choices through a survey, one can overcome some of these problems and directly estimate ex-ante treatment effects.[3] Although this requires few econometric identification assumptions, it relies on high-quality survey data in which the elicited expectations are really informative about individuals' true expected outcomes.

In this paper, we use newly collected subjective expectations data for a sample of high school students from 40 public high schools in Stockholm, Sweden. Following the approach outlined in Arcidiacono et al. (2020), we use this data to expand the focus from college majors and occupational groups and estimate various ex-ante average and distributional treatment effects of *choosing* university education, including the average treatment effect (ATE), average treatment on the treated (TT) and average treatment on the untreated (TUT).[4] This is a classical returns to education topic, and we can analyze educational choices across this vertical dimension since we surveyed high school students about earnings expectations from choosing various university fields-of-study, as well as without continuing to university.

In addition, we expand the ex-ante treatment effect literature using subjective expectations data by explicitly connecting the various ex-ante treatment effects to the descriptive difference-in-means (or OLS) estimator, as well as to various sorting parameters, using the returns to education framework laid out in Heckman, Lochner and Todd (2006). The fundamental difficulty in estimating ex-post (realized) returns to education is the lack of data on counterfactual outcomes, something which can be overcome by imposing, sometimes very strong, econometric identification assumptions. Estimating mean and distributional treatment

[1] See Carneiro, Hansen and Heckman, 2003; Heckman, Lochner and Todd, 2006; Cunha and Heckman, 2007.
[2] See Arcidiacono, Hotz, Maurel and Romano (2020).
[3] As in Arcidiacono et al. (2020) and Wiswall and Zafar (2020).
[4] Rather than focusing on estimating ex-ante treatment effects from choosing different college majors (as in Wiswall and Zafar, 2020) or from choosing different occupational fields (as in Arcidiacono et al, 2020), conditional on being a college student.

effects is particularly challenging in models that allow for heterogeneous returns, and where students self-select into higher education.[5] We therefore push the usage of subjective expectations data beyond the estimation of ex-ante treatment effects, to also directly estimate the degree and sources of biases in ex-ante OLS estimates of treatment effects. Assuming students have rational expectations, these estimates can be interpreted as showing ex-post relationships net of unanticipated earnings shocks. In addition, this makes it possible to directly estimate ex-ante versions of various sorting effects and the degree of comparative earnings advantage.

To capture counterfactual outcomes, we designed and implemented a survey which elicited beliefs and expectations about future earnings and other outcomes associated with various educational choices (graduating university in various fields; not attending university). The fact that our sample population is both relatively large, and comes from a varied cross-section of high schools in a large city, is, we believe, an important aspect of this study, since it means that we provide results for a broad population of interest. Although this type of survey is growing in popularity, getting participants to provide valid responses to such hypothetical questions can be challenging (see Dominitz and Manski, 1996; Manski, 2004). To combat this, we took special care in designing the questions and conducting the survey. We have also linked administrative data on students' family background and school performance, as well as follow-up data on application and enrollment at (any Swedish) university which we use to validate the stated educational choices as well as to estimate alternative treatment effects and sorting parameters.

We provide a number of interesting findings regarding the ex-ante returns to university education. First, we estimate the ex-ante average treatment effect to be 0.36, indicating that the average prospective student expects about 9% higher earnings for each year of university study. However, these ex-ante returns vary substantially between prospective students, and are positively correlated with being female, coming from a high SES background, and test scores in math, but only weakly related to other measures of school performance. Second, we find evidence of positive sorting effects, meaning that the ex-ante return to university is higher for those choosing university than for those choosing high school on average (so that $TT > TUT$),

[5] Willis and Rosen, 1979; Card, 1999; Heckman, Lochner and Todd, 2006 Cunha and Heckman, 2007; and Carneiro, Heckman and Vytlacil, 2001 and 2011.

although there is a lot of distributional overlap. This is consistent with students sorting into educations based on their comparative earnings advantage.

Third, despite the apparent heterogeneous returns with respect to schooling choices, resulting in sorting based on comparative advantage, our ex-ante estimates predicts that the bias from estimating ATE or TT using a differences-in-means estimator, or OLS, should be expected to be fairly small. The reason is that the ex-ante selection bias and ex-ante sorting gain from choosing university (for university choosers) are of opposite sign and of somewhat limited magnitude. On the other hand, since those who choose to stop education after high school expect to gain much less from university than an average person, the ex-ante TUT estimates are typically much smaller than the OLS estimates.

Fourth, the underlying ex-ante sorting pattern can provide information about how to model "earnings ability" in earnings-education models, which has important econometric implications for returns to education estimations (see Heckman, Lochner and Todd, 2006). That the selection bias is (somewhat) negative or zero means that the perceived earnings from stopping at high school is quite similar regardless of the educational choice. However, the expected earnings from choosing university is much higher for those that actually choose university compared to those that opt for only high school education.[6] This suggests that we can reject the simple unitary ability/single skill model, since such a model would expect that those with high ability do better in both educational states. On the other hand, we do find a positive correlation between individuals' expected earnings with university and high school education (the correlation is about 0.4 and very similar regardless of educational choice). This is evidence against multiple skill models where those expected to be more "able" in high school occupations are the least able in university occupations (implying a negative correlation), which was proposed in Willis and Rosen (1986) and who have found empirical support in papers by, e.g., Carneiro, Heckman and Vytlacil (2011), using observational data for the US.

Fifth, we find that the ex-ante TUT estimates are consistently positive.[7] This contradicts the pure Roy model of earnings maximization, and instead suggests that there are other factors

[6] These patterns are notably similar to those in Cunha, Heckman and Navarro (2005), who use factor models applied to observational data for the US.
[7] This is in line with results found in Nybom (2017) using Swedish data, but different than what is found in Heckman, Lochner and Todd (2006) and Carneiro, Heckman, and Vytlacil (2011) using data for US, all using

affecting these student's choices. Since all universities in Sweden are free and living expenses are covered by generous grants and subsidized loans, financial constraints are unlikely to be important. Using survey measures of non-pecuniary costs, including enjoyment of studies and probability of graduating, we show that these measures are only weakly related to returns, indicating that psychic costs are unlikely to explain this finding. Another possibility is compensating non-pecuniary factors, so that prospective students are prepared to give up earnings gains from choosing university because of higher non-pecuniary benefits from choosing to stop with high school education. This is an issue which we come back to below.[8]

To complement the analysis above we also estimate ex-ante treatment effects of choosing high- versus low-paying university fields of study. This makes it possible to compare our ex-ante treatment and sorting earnings effects with those in Wiswall and Zafar (2020), who estimated ex-ante treatment effects for high- versus low paying college majors in the US (science/business vs social sciences/humanities). Interestingly, our findings of sorting based on comparative advantage and positive ex-ante TUT mimic the results in their study. In addition, we are then also able to provide counterfactual subjective ex-ante treatment effects for non-earnings outcomes, including expected social status, enjoyment of study and of work and work-life balance, many of which are novel in the literature on ex-ante treatment effects.[9] For the non-pecuniary outcomes we find mostly positive and large TT estimates, which are notably higher than the TUT estimates for these outcomes, the latter of which in many cases are negative.

Interestingly, for the high- and low paying dimension our results therefore show an important compensating role for many non-pecuniary factors, meaning that prospective students are prepared to sacrifice higher expected earnings and instead select a low-paying field which they expect would lead to a job where they would obtain higher non-pecuniary benefits. For instance, those individuals choosing low-paying fields expect to enjoy their jobs and sustain a better work-life balance in the occupations resulting from educations in such fields, to a higher degree than they expect to achieve had they instead chosen high paying fields. As the ex-ante

instrumental variables and the "MTE-framework" to estimate treatment effects of returns to college using observational data.

[8] Unfortunately, we do not have access to direct information about non-pecuniary factors in choosing high school.

[9] These non-pecuniary outcomes measures are very similar to those used in Zafar (2013) who investigated determinants of the college major choice across genders. In Pihl, Angelov, Johansson and Lindahl (2019) we use these measures to answer similar questions.

treatment and sorting earnings effects for high- versus low paying fields are qualitatively very similar to university versus high school, including a positive and sizable TUT, we believe these results are also informative about why we find positive TUT for university studies relative to high school.

Our paper and findings relate to several strands of literature. Two important recent papers, Arcidiacono et al. (2020) and Wiswall and Zafar (2020), have estimated ex-ante earnings treatment effects (ATE, TT, TUT) for occupational choice and college major, respectively, using subjective expectations data for students enrolled in two US universities.[10] Both studies estimate ex-ante treatment effects between various fields and find evidence of positive ex-ante ATE and positive sorting effects (TT>TUT>0) for earnings.[11] Both these studies find substantial individual heterogeneity in ex-ante earnings returns. They also find that non-pecuniary factors play an important role.[12] In these respects, our results for university choice are quite similar.

We also relate to a small literature which has used subjective expectations data to study university-going. An early set of papers by Kodde (1986, 1988) find that expecting higher post-secondary earnings in the Netherlands is positively correlated with choosing to attend post-secondary education, while expecting higher earnings with only a high school degree is negatively (but not significantly) associated with further education. More recently, Boneva and Rauh (2020) estimate ex-ante university premiums for the UK, and find that students from low SES backgrounds perceive the earnings returns to university education as being significantly lower compared to students with a high SES background.[13] This is also consistent with our findings. There is also a rapidly growing literature eliciting beliefs and stated preferences about counterfactual choices (see Altonji, Arcidiacono and Maurel, 2016, for a survey), especially

[10] Arcidicaono et al. (2020), use data on 173 (male) Duke University students, from whom they elicited data on earnings beliefs and subjective probabilities of working in five occupation groups, which were matched to actual occupation using data from the social network *LinkedIn*. Wiswall and Zafar (2020) use data on 493 enrolled New York University students and analyze various outcomes, including expected earnings, marital sorting and labor supply, associated with four groups of potential majors.

[11] Wiswall and Zafar (2020) estimate ex-ante treatment effects for Science/Business versus Humanities/Social sciences, and also versus a small sample of drop outs.

[12] Arcidiacono et al (2020) investigate this using an indirect test, whereas Wiswall and Zafar (2020) look at expected spousal earnings and fertility.

[13] Boneva and Rauh (2020) perform an online survey of 2,540 secondary school students' (where 759 students are in their final year) expected earnings and beliefs about some non-pecuniary outcomes. They use their data to estimate the socioeconomic gap in ex-ante earnings premiums and find that students from low SES backgrounds perceives the pecuniary as well as the non-pecuniary returns to university education as being significantly lower.

those eliciting earnings expectations associated with hypothetical schooling choices (see Dominitz and Manski, 1996; Zafar, 2013, and; Arcidiacono, Hotz and Kang, 2012, for important early contributions).

Our paper is structured as follows. In Section 2, we present the survey and key variables, as well as discuss the validation of these variables. Section 3 presents the conceptual framework for analyzing ex-ante treatment effects, sorting parameters and how to interpret them. Section 4 presents the results for levels and returns to university, vis-à-vis high school. Section 5 presents the results for high versus low-paying fields and a supplementary analysis using non-pecuniary outcomes. Section 6 concludes.

2. Institutions & Survey

2.1 Education in Sweden

In Sweden, the vast majority of students complete three years of high school (*gymnasium*) after compulsory school (grades 1-9). High school programs are specialized, both for broad academic subjects (e.g., social science, natural science), and vocational tracks. Academic programs contain coursework preparing the students for university, but if students are not qualified for their university program of choice at the end of high school (by completed courses), they can top-up their education with an additional year of high school.

There is a single application for all colleges and universities in Sweden.[14] For degree granting programs and courses beginning in the fall semester, students apply in the spring of the same year. There are no tuition fees for Swedish citizens or permanent residents, and when studying full-time, students are given generous grants and low-interest loans which cover living costs. University programs in Sweden can be divided into the following eight broad categories:

Table 1: Categories of Education and particular programs

[14] In Sweden there is a technical difference between a university and a college (högskola), in that the former is legally permitted to award PhDs. Universities are typically more prestigious and selective, but many types of bachelors and masters-level educations can be completed at either type of institution. We use the words college and university interchangeably in this paper.

These eight categories are adopted from the classification of education that Sweden has used since the 1960s (*SUN*). We use the broadest category (first digit of a possible 3-4). Within each category there are more specific university programs, some of the most common are listed in the right column.

2.2: Survey implementation

For the purpose of this project, we have collected survey data on a sample of high school students in the municipality of Stockholm. To be part of our population, the students must have attended the third year of a municipal high school in 2014 and lived in the municipality of Stockholm. Although the fraction of independent (non-municipal) high school students is high in Stockholm, the majority of the students in academic programs attend municipality schools. The municipality of Stockholm includes many suburbs, some well-off and some much less so.

A concern with eliciting preferences from survey data is that the result may differ from what would be found in real-world situations. As our study design is quite similar to a stated preference experiment, advice from the stated preference literature was used when designing the survey. We describe the motivation in designing the survey in more detail in Appendix E, and a translation of the survey in Data Appendix A. The survey timing was chosen carefully to be before the university applications closed, but late enough so that they had likely put considerable thought into their educational path. Hence, we hope to limit the issue of cognitive dissonance/ex-post rationalization, where students provide biased responses because their field of study selection is already set in stone.[15] Our study differs from most other studies using subjective expectations data, in that it is drawn from a region rather than a single school, and the students are in high school and not in university.[16] And, since the sampling is done at the high school level, the students can end up at any university or college, including technical colleges and business schools, also located outside Stockholm.

To maximize saliency and sample size we hired a professional interview company to contact the students and do in-person interviewing. As is typical with voluntary surveys, differences in

[15] Zafar (2011a) tests for this issue in his sample and shows that those students do not appear to exhibit cognitive dissonance when reporting their beliefs.
[16] Boneva & Rauh (2020) is a notable exception that uses high school students from 37 English schools.

ease of contact (primarily due to no listed phone number) and willingness to participate mean that the final sample is not representative of the population. However, we do still have a sample composed of students from over 40 different high schools. We compare the surveyed sample to the population in terms of demographics in Appendix E and Table A1, and their expectations in Section 2.4.

After an introduction, the students were first shown the eight fields of study (listed in Table 1) and asked to think about and choose which of the most common programs in that field they would pursue if they had to pick. As these classifications are standard in Sweden, they should be meaningful to the surveyed students. Then, they were asked to provide their expectations and beliefs about these hypothetical educational choices on 10 different dimensions.[17] They were instructed to imagine their most preferred specific program within each field when answering questions about the broad field-category (e.g. computer science within the natural science category). Two of these ten dimensions were their expected earnings at ages 30 and 40 assuming they had graduated with a degree in the respective field. Then, the students were asked if they expected to go to pursue postsecondary education, and if so, to which field. The field or no-college option chosen in these two questions is our primary measure of chosen education from the survey. Since the previous questions referred only to college fields, they were finally asked about their expected earnings at age 30 and 40 should they not attend college.[18]

2.3 Data

Our sample includes all 498 individuals who completed the survey and were matched to administrative data by Statistics Sweden. We have matched the students to their parents' incomes and educations (to capture socio-economic status), as well as to demographics such as immigrant background and gender. We observe the high schools the students attended, as well as their coursework and grades, which we use to construct proxies for student ability.

[17] These 10 dimensions are, in survey order: probability of passing the degree, probability of liking the coursework, expected hours per week of studying, probability that family will approve of choice, probability of finding a job directly after graduation, probability of job satisfaction at age 30, probability of being able to combine work and family life at age 30, expected hours per week of work at age 30, expected earnings at age 30, expected earnings at age 40, and, social status (separate from salary) they associate with the education. See Angelov et al. (2019) for more details, and later sections in this paper were we look at these other outcomes.

[18] At the end of the survey the students were randomized into an offer to see actual average earnings for each of the fields. We found no impact of the treatment on their subsequent application and enrollment behavior. Thus, we feel confident ignoring the experiment for the purposes of this study.

Importantly, we have also linked the early 2014 survey to follow up data through early 2019. In these five years we observe all applications to university, as well as enrollment, and eventually graduation and labor market earnings.

A key variable we use throughout is choice of education. We define this separately for the stated choice as answered in the survey, and revealed choice both through applying as well as through enrolling in a program. When using the administrative follow-up data to define choice of education, we include only degree-granting programs. For field of application, we assign the student to the field of the degree-ranking program that they ranked as 1st top choice. If they did not apply to university in Sweden between 2014-2019, or they applied by never ranked a program as their top choice (just a course), they were assigned to the no-college choice option. Likewise, for enrollment, if the student is never enrolled in a program in Sweden, we assign them to the no-college choice option.

2.4 Descriptive Statistics and validation of the earnings expectation and education measures

Table 2 summarizes the students' responses to the two questions on anticipated earnings after graduating with a degree in each of the eight field of study categories, and the two questions on earnings without going to university. Although we focus on the choice between any college field and no-college, looking at field-specific earnings is useful to evaluate how the students responded to the survey. The table uses the mean of each student's expectation at ages 30 and 40, which we think of as a proxy for expected lifetime earnings. At the low end, students expect average earnings of 26,600 SEK per month on average in the world where they do not attain more education. At the high end, they expect 43,700 SEK per month in the world where they attain a Social Sciences degree.

A common critique of subjective expectations data is that survey respondents may not exert effort in their responses, yielding data that is not a true reflection of their beliefs.[19] If this were the case in our data, we would expect to see expected earnings that did not line up well with reality. In fact, we find that students have reasonable expectations, suggesting that they have

[19] Cognitive dissonance is also a potential issue with the potential to bias estimates. We discuss this separately in Section 3.4.

thought about this question and acquired relevant information. We can see this in Figure 1, which plots the survey expected earnings on the horizontal axis and population averages (in 2018 for workers around age 40) for each field on the vertical axis. The correlation is 0.862, with the students clearly separating the low-earning categories (no college, teaching, humanities, animal/agriculture, services) and the high-earning categories (health, social sciences, engineering and sciences). The levels of earnings the students expect are generally higher than what we observe in the full population in 2018, however when we restrict the population earnings to only workers with jobs in Stockholm, the expected earnings levels are nearly a perfect fit. Additionally, they expect a fairly wide spread of earnings between fields, which tells us that they perceive meaningful differences in renumeration based on education.

Table 2: Mean Expected Earnings by Field

Figure 1: Comparing Expected Earnings to Population Earnings

Another way to show that students took the survey seriously and responded with their true expectations is to use the follow-up data described in the previous section. With this data, we can compare what they said they planned to do in the survey, with what they actually did do over the subsequent 4.5 years. This time horizon covers most of the students' entry into university, but is not far enough to capture their full labor-market potential. Thus, we focus on comparing their stated intention to go to college, to whether they do actually apply or enroll and in which field.

Table 3 summarizes the correspondence between the eight fields of study plus no-college in expectation and the administrative data. We see that roughly 47% of students pursue the same education category that they said they planned to. This is higher, at 55%, if we only look at university fields and those who apply to university. The discrepancy is that while only 23 individuals in the survey said they did not plan to go to college, roughly 100 do not apply to a program (by our definition) by 2019. The correspondence between survey and enrolling in a degree program is similarly high. Of those who enroll in a college program, 51.5% enroll in the same field they said they expected to. There are an additional 58 people who apply to college, but are not enrolled by 2019. This makes the overall match (including no-college) somewhat lower at 38%. These shares are much higher than random allocation to fields, and suggest strong informational content of expected studies.[20]

Table 3: Correspondence between stated and revealed educational choices

3 Conceptual framework and Estimation issues

With our collected information on each individual's expected earnings in two educational states S={0,1} we can characterize the ex-ante earnings levels and returns by estimating the means and distributions of the potential outcomes y_{0i}, y_{1i}, and hence the gains $y_{1i} - y_{0i}$. We do this both unconditionally and conditional on treatment status, i.e., choosing S=0 or S=1, where S can represent the stated choice (in the survey), application and enrollment (in administrative data). In our main analysis, and in the framework presented in this section, we frame the choice between university and high school as the two educational states. However, the reasoning is applicable for any binary categorization, such as high and low paying fields of university study.

We will first discuss our parameters of interest and then turn to estimation issues. Note that we always think of the potential outcome variables y_{0i} and y_{1i} as expressed in *log* expected earnings, where y_{1i} is the log expected earnings in the chosen university field. Although our goal is to estimate means as well as the distributions, the discussion mostly focuses on means.

3.1 Treatment Effects

Some ex-ante average treatment effects of interest are:

$$ATE = E[y_{1i} - y_{0i}] \tag{1}$$

$$TT = E[y_{1i} - y_{0i} | S = 1] \tag{2}$$

[20] Arcidiacono et al. (2020) and Wiswall and Zafar (2020) are able to use subsamples where they can track actual earnings and compare to expectations and find positive and sizable correlations. This is also something we will be able to do soon for all the survey respondents, through the use of administrative earnings registers and the survey respondents' personal identifiers.

$$TUT = E[y_{1i} - y_{0i}|S = 0] \quad (3)$$

where ATE is the ex-ante average treatment effect; TT is the ex-ante average treatment effect for those taking up the treatment, and; TUT is the ex-ante average treatment effect for those not taking up the treatment. If $TT \neq TUT$, the gain from treatment differs between individuals taking up treatment versus individuals not taking up treatment. This means that individuals sort themselves between educational states in a non-random way and that the ATE parameter will be of limited value for treatment effect evaluations (Heckman and Robb, 1985). With our data the ex-ante treatment effects from choosing university can be directly estimated using our counterfactual outcomes and calculating mean expected earnings. To fully understand the gains and sorting pattern we also estimate the counterfactual distributions across educational states.[21]

3.2 Characterizing sorting

To connect the various ex-ante treatment effects with parameters for the degree of sorting across states, we closely follow the setup in Heckman, Lochner and Todd, 2006, (henceforth HLT, 2006) who lay out a framework for interpreting treatment effects and sorting parameters when estimating the returns to college using observational data.

The (log) earnings expectations associated with the two educational states S={0,1} are specified as $y_{0i} = \alpha + u_{0i}$ and $y_{1i} = \alpha + \bar{\beta} + u_{1i}$. The terms u_{0i}, u_{1i} are random variables with $E(u_{0i}) = E(u_{1i}) = 0$ so that the means of the potential outcomes are $E(y_{0i}) = \alpha$, $E(y_{1i}) = \alpha + \bar{\beta}$. Hence:

$$\beta_i = y_{1i} - y_{0i} = \bar{\beta} + u_{1i} - u_{0i} \quad (7)$$

is the individual expected earnings gain from choosing university over high school, and

[21] The distinction between ex-ante and ex-post treatment effects are discussed extensively in Heckman and Vytlactil (2007). These are estimated using observational data in Cunha, Heckman and Navarro (2005) and further surveyed in Cunha and Heckman (2007). Arcidiacono et al., 2020, and Wiswall and Zafar, 2020, are the first that use subjective expectations data to estimate ex-ante treatment effects.

$$ATE = E[\beta_i] = E(y_{1i} - y_{0i}) = \bar{\beta} \quad (8)$$

is the ex-ante average treatment effect (ATE) in the population. Since β_i is heterogeneous in this framework (as long as $u_{1i} \neq u_{0i}$) the average treatment effects can differ in sub-populations, including the one defined by treatment status S. The ex-ante average treatment on the treated effect $TT = E[\beta_i|S = 1]$ and the ex-ante average treated on the untreated effect $TUT = E[\beta_i|S = 0]$ can be expressed as:

$$TT = E[y_{1i} - y_{0i}|S = 1] = \bar{\beta} + E[u_{1i} - u_{0i}|S = 1] = \bar{\beta} + SE_1 \quad (9)$$

$$TUT = E[y_{1i} - y_{0i}|S = 0] = \bar{\beta} - E[u_{0i} - u_{1i}|S = 0] = \bar{\beta} - SE_0 \quad (10)$$

where the first sorting effect, SE_1, is how much *more* those who actually choose S=1 expect to get in returns to college over an average person (labelled Sorting Gain in HLT, 2006). Likewise, SE_0 is how much *less* those who actually choose S=0 expect to get in returns from college, compared to an average person.[22] These sorting parameters are of particular interest to us, as they are informative in characterizing ex-ante sorting behavior, and (as we will see below) of the sources of bias in random coefficient models relating schooling to earnings.

If the ex-ante returns vary with choice of treatment, this suggests that individuals sort themselves among treatment states due to their expected earnings returns (or something correlated with these). If individuals simply choose the S in which they expect to earn the most, we would have that $y_{1i} > y_{0i}$ for those selecting S=1 and $y_{1i} < y_{0i}$ for those selecting S=0. This implies $TT = E[y_{1i} - y_{0i}|S = 1] > 0$ and $TUT = E[y_{1i} - y_{0i}|S = 0] < 0$, and the degree to which this holds in the population determines the fraction of individuals that sort based on absolute earnings advantage. If individuals sort themselves to the S in which they expect to have an earnings advantage compared to other individuals, we would have that $y_{1i} - y_{0i}$ is larger for those selecting S=1 than those selecting S=0.[23] [24] Such sorting based on

[22] Obviously, SE_0 can also be viewed as the extra return the S=0 types get from no-college relative to the average person, if we think of no-college as the treatment.

[23] Which also implies that $y_{0i} - y_{1i}$ is larger for those selecting S=0 than those selecting S=1.

[24] The theory of comparative advantage connects earnings and ability formally in the following way (see Sattinger, 1993 and Kirkeboen, Leuven and Mogstad, 2016). Earnings, $Y_i^S = \pi^S q_i^S$, is the product between price (per unit worker output) π and productivity q where prices differ only between educational states S={S,S'}. In this simple model, an individual i is said to have a comparative advantage (i.e., relative productivity advantage)

comparative advantage in expected earnings implies $TT = E[y_{1i} - y_{0i}|S = 1] > E[y_{1i} - y_{0i}|S = 0] = TUT$. Hence, sorting based on absolute advantage implies sorting based on comparative advantage. The degree to which comparative advantage holds at the mean in the population can then be estimated by the parameter $CA = TT - TUT = SE_1 + SE_0$, which is the combined sorting effect calculated as the sum of the additional gains from choosing each educational state, compared to the return for an average person.[25]

If we find evidence of comparative advantage, but not absolute advantage, it indicates a deviation from a pure earnings maximization framework since the gain from university is positive also for those that select high school, on average. This finding can be reconciled with earnings maximization in a generalized Roy model including barriers to entry such as costs of schooling, or if the object of maximization is a broader utility measure. We will return to this in Sections 4.2 and 5.2.

3.3 Connection to OLS estimates of the returns to education

HLT, 2006, discuss how to identify various treatment effects and sorting parameters in a generalized Roy model allowing for heterogeneous returns to be correlated with educational choice, using observational data and OLS and IV estimation techniques. In principle, unless $u_{1i} - u_{0i}$ is independent of S,[26] the various treatment effects might differ, and are not separately identified. Hence, neither are the sorting parameters. With our data, we can directly estimate ex-ante versions of the treatment and sorting effects and hence, which we, given some assumptions, can use to infer consequences for how to infer ex-post returns from observational data.

Since the surveyed earnings expectations can never include the part of actual earnings which individuals cannot forecast at the time when they make their schooling choices, the validity of our ex-ante parameters for ex-post data depends on the degree to which actual ex-post earnings

over individual i' in state S, and the individual i' has a comparative advantage over individual i in state S', if the earnings return from state S for individual i is higher than for i': y_i^S-$y_i^{S\prime}$>$y_{i\prime}^S$-$y_{i\prime}^{S\prime}$, where y equals $\log(Y)$. In our ex-ante case, we might think of q as perceived productivity and y as expected earnings, which then depends on perceived productivity and expected wages, associated with a certain choice.

[25] Kirkeboen, Leuven and Mogstad, (2016) estimates the sum of these sorting effects across multiple fields of study choice using observational data for Norway. We adapt their framework to multiple educational choices and subjective expectations data in Angelov, Johansson, Lindahl and Pihl (2021).

[26] For instance because $u_{1i} = u_{0i}$ so that returns are constant.

is due to predictable earnings (at time of choice) and the degree to which the unforeseen part (due to shocks to the education-earnings distribution) is evenly distributed across educational states. We discuss this further in Section 3.4.

Here, we continue to follow the framework in HLT, 2006, but with the purpose to use their framework and show that our data on earnings expectations can directly identify not only the ex-ante treatment effects, but also various ex-ante sorting parameters.

Assuming no ex-ante general equilibrium effects,[27] we can relate expected earnings to the potential earnings outcomes as $y_i = Sy_{1i} + (1-S)y_{0i}$.[28] The ex-ante equivalent of the descriptive university earnings education gap (equivalent to the difference-in-means or OLS estimator from a regression of log earnings on a university dummy) is therefore:

$$\begin{aligned} OLS = E[y_{1i}|S=1] - E[y_{0i}|S=0] \\ &= \bar{\beta} + \{E[u_{0i}|S=1] - E[u_{0i}|S=0]\} + E[u_{1i} - u_{0i}|S=1] \\ &= ATE + SB + SE_1 \end{aligned} \tag{11}$$

where $E[u_{0i}|S=1] - E[u_{0i}|S=0]$ is the Selection Bias (SB) and $E[u_{1i} - u_{0i}|S=1]$ is the sorting gain from choosing S=1 for those choosing S=1 (SE_1). By using that $ATE = TT - SE_1 = TUT + SE_0$ we therefore also have that:

$$OLS = TT + SB \tag{12}$$

$$OLS = TUT + SB + SE_0 + SE_1 \tag{13}$$

Equations (11)-(13) specify the relationships between an OLS estimator, the various treatment effects (ATE, TT and TUT) and the sorting parameters (SE_1, SE_0 and SB).[29] With

[27] I.e., a surveyed individuals' expected earnings should not be affected by the educational choices of other individuals.

[28] This can be rewritten as $y_i = y_{0i} + (y_{1i} - y_{0i})S = \alpha_i + \beta_i S_i = \alpha + \beta_i S_i + u_{0i}$ which is equivalent to a random coefficient model. In a constant coefficient model, we have implicitly assumed $u_{0i} = u_{1i}$, since $y_i = \alpha + \bar{\beta}S + u_{0i} + S(u_{1i} - u_{0i})$, so that the only source of bias in an estimate from an OLS regression would be the correlation between u_{0i} and S, the traditional selection (or ability) bias. With heterogeneous returns, however, we need stronger assumptions (Card, 1999; Heckman, Lochner and Todd, 2006).

[29] Note that Equation (13) can also be expressed as $OLS = TUT + E[u_{1i}|S=1] - E[u_{1i}|S=0] = TUT + rSB$, where the term rSB can be thought of as a sort of "reverse SB" since it is the selection bias with respect to

heterogeneous returns the various treatment effects differ and so does the sources of biases in OLS estimates of these various treatment effects. However, given knowledge about the OLS, ATE, TT and TUT parameters the sorting parameters are all identifiable from this system of equation (as $SB = OLS - TT$, $SE_1 = TT - ATE$ and $SE_0 = ATE - TUT$). Hence, the degree, and sources, of bias when estimating the various ex-ante treatment effects using OLS can be identified.

What can we learn from observational studies regarding ex-post treatment and sorting effects? At least since Griliches (1979) many researchers (see Card, 1999, for a survey), using proxy controls for ability, twins fixed effects and various IV-strategies, have argued the existence of ability bias, i.e. SB>0, and therefore that OLS estimates of returns to schooling have produced overestimates of the average returns to schooling.[30] On the other hand, several papers who have explicitly modelled the choice of education and allowed for heterogeneous returns, in combination with imposing strong econometric identification assumptions, find support for *SB*<0, (Willis and Rosen, 1979; HLT, 2006; Carneiro, Heckman and Vytlacil, 2011, who all use data for US, and Nybom, 2017, who uses data for Sweden), so that high school students earn more in high school than what college goers would, had they chosen high school. This would go against a single skill/ability model and instead support a multidimensional skills model.[31]

There is less evidence available on the sign of the bias due to heterogeneous returns and of the sorting effects (the SEs) from observational studies of returns to college or university. Card (2001) models the return to be heterogeneous and showed that this would be expected to lead to an upward bias of the average marginal return (the ATE), even if SB=0, with the bias increasing in the degree of importance for comparative advantage (higher returns to schooling for those selecting more schooling).[32] In HLT (2006), Carneiro, Heckman and Vytlacil (2011) estimates of SE_1 are quite large. The difference between ATE and TUT is positive and large,

u_{1i} instead of u_{0i}. From these formulas also follows that $CA = SE_1 + SE_0 = rSB - SB$, so that a lower SB (higher rSB) is mechanically associated with higher (lower) CA.

[30] In a framework were the individual returns to education are approximately constant in the population (so that $u_{1i} = u_{0i}$), or if $cov(u_{1i} - u_{0i}, S)$=0.

[31] Nybom (2017) finds TT>OLS in the semiparametric model but not in the parametric normal model, and also that observable ability measures leads to a OLS estimate when they are included as control variables, which is in line with a positive ability bias.

[32] In the framework of HLT (2006) this follows from the $OLS = ATE + SB + SE_1$ equation above since a larger SE_1 will be equivalent to a higher degree of comparative advantage (holding SE_0 constant).

suggesting a positive SE_0 as well. Hence, both sorting effects are important. In Nybom (2017), TT is significantly larger than ATE, suggesting a positive SE_1 as well. The difference between ATE and TUT is smaller, but still positive, suggesting a positive SE_0.

3.4 Estimation issues

Before we turn to the estimation results it is important to clarify what we are more and less likely to be able to estimate accurately with data on earnings expectations. To estimate ex-ante treatment effects and sorting parameters we need to assume that our data on surveyed earnings expectations are informative about actual earnings expectations. In order to infer that these estimates are relevant for ex-post treatment effects and sorting, we need to assume that our data on surveyed earnings expectations are informative about actual earnings.

The ex-ante treatment effects and sorting parameters discussed above are summarized in Table 4. They are all constructed from the mean expected earnings in the two education states, and hence straightforward to estimate. However, for them to be unbiased estimates, we must make assumptions about the students' expectations errors associated with each educational state. These issues are discussed at length in Arcidiacono et al. (2020). Swedish degree programs are more specialized than American ones (lacking the broad liberal arts foundation). This means that field of study in Sweden is very highly predictive of ultimate occupation (Björklind et al., 2016).

Table 4 about here

If y_{0i}, y_{1i} and S are measured without error, it is straightforward to estimate all the ex-ante treatment effects and sorting parameters from our data. In fact, ex-ante ATE is still identified even in the presence of measurement errors, as long as the measurement errors have the same mean across educational states (Arcidiacono et al., 2020). For the ex-ante TT and TUT we also need that the income expectation errors are independent of potential miss-classification of S. Since we have information on S at different stages (survey to enrollment), we have less of an issue with classification error if the treatment of interest is whether an individual graduates or not, although our data do not extend long enough to see the end of most university spells.

Estimating the distribution of treatment effects correctly in the presence of measurement errors in expected income requires stronger assumptions (see Arcidiacono et al., 2020, Appendix A.5). If the measurement errors are classical, the dispersion of the true ex-ante distributions are overestimated. Hence, our estimated distributions are likely to be inflated as they partly include measurement errors. For our most important results regarding the distributions, we compare the distribution of treatment effects for treated and untreated. Hence, if measurement errors are similar across treated and untreated, the patterns are likely to be similar, even in the presence of classical measurement error.

Previous research has worked to establish that the type of subjective expectations data that we use is not subject to cognitive bias. Specifically, Zafar (2011a; 2011b) collected expectations for the same individuals twice after they chose a major, and does not find that students rationalize their choice by becoming more positive in their expectations of their chosen field (relative to their not chosen fields) over time. This is contrary to a story of cognitive dissonance (i.e., individuals overestimate the benefits of their choice relative to the things they don't choose). Zafar (2011a) also provides evidence that students exert sufficient mental effort in their response, and that their expectations are well formed and that measurement error in their responses is classical. These other findings are consistent with what we see in our data, although we cannot repeat his test for cognitive dissonance with a single period's observation of expectations.

Inferring what the ex-ante treatment effects say about ex-post treatment effects require much stronger assumptions, since this also requires rational expectations and no unanticipated earnings shocks (Arcidiacono et al., 2020). This becomes especially important when we attempt to use our data on earnings expectations to learn about the sources of bias inherent in OLS estimates of treatment effects when returns are heterogeneous and correlated with educational choice. A distinction between what individuals cannot forecast at the time when they make their schooling choices (*uncertainty*)[33] and what they can predict (*heterogeneity*) is made in the literature attempting to distinguish ex-ante and ex-post returns using observational data (see Carneiro, Hansen and Heckman, 2003; Cunha and Heckman, 2007; and HLT, 2006).[34] Ex-post earnings data capture both the unpredictable and predictable components, whereas ex-

[33] Say because of an exogenous shock that changes the returns to university ex-post.
[34] See also Heckman and Vytlacil (2007) who distinguish between the true earnings beliefs and true earnings.

ante data only capture the predictable component.[35] Hence, if our estimated biases differ from those in well-designed observational studies, it could be because schooling choices are correlated with the earnings components that cannot be foreseen at the time when the schooling choices are made.

In Section 2.4, we showed that the expected earnings is highly correlated with the observed average earnings across educational choices (Figure 1). However, we still want to emphasize that our results are only suggestive for those conclusions that require ex-post validity. Additionally, our questions about future earnings are asked conditional on graduating and finding employment, so our estimates are not intended to take risk of drop-out or unemployment into account.

Finally, our main estimates of returns are calculated based on the average of individuals' earnings expectations at age 30 and at age 40, rather than lifetime earnings. These could differ because of foregone earnings due to education duration (especially in the college/no-college comparison), and because of different earnings growth rates. However, previous research using Swedish administrative data has shown that life-cycle bias is quite low for workers if their earnings are measured at these ages (Böhlmark and Lindquist (2006); Nybom and Stuhler (2016)), at least for men. Discount rates could also differ between individuals, but since our estimates are within individual, they should not impact our results.

Section 4: Estimated ex-ante earnings levels and returns distributions: University versus High School

In this section we describe the distributions of ex-ante earnings levels and returns. We begin by describing them overall (Section 4.1), followed by between and within (Section 4.2) the high school and university educational states.

Section 4.1: Ex-ante earnings levels and returns distributions: overall

[35] HLT, 2006, surveys the literature and finds that both are important.

The overall earnings distributions for high school and university fields of study are shown in Figure 2 for high school (the solid red line) and stated university field of choice[36] (the dashed blue line).[37] In the figure we also show the eight university fields of study alternatives (as solid grey lines). Each distribution consists of all individuals in our sample (regardless of their intent to go to college or not).

Figure 2 about here

The individuals' expectations of high school earnings, i.e., if they were to choose not to continue to university, show that most of responses lie between 20,000 and 35,000 SEK in monthly pre-tax earnings, with a mean of 26,400 SEK (roughly €2,640) and a standard deviation of 6.9. The individuals' expectations of university earnings, i.e., if they were to attend their preferred university field, show a distribution that is located more to the right and is more dispersed. The bulk of the responses are between 25,000 and 50,000 SEK in monthly pre-tax earnings with a mean of 38,600 SEK and a standard deviation of 11.9. The mean is about 46% higher and the standard deviation (SD) is 72% higher for preferred fields of university compared to the high school distribution.

By using the distribution of counterfactual earnings for high school and (preferred) university fields of choice, we can construct the distribution of ex-ante university premiums. Figure 3 shows distributions for preferred university field relative to high school earnings (the dashed red line) and for the average of all university fields relative to high school earnings (the solid blue line).[38] As the former distribution is located more to the right, individuals' expected earnings for their preferred university field is higher than in non-preferred university fields of study. Hence, prospective students sort into fields where they expect higher earnings. We focus on comparing the preferred university fields with choosing to stop at high school. This captures ex-ante returns to university education which is most comparable to the ex-post observable university return. The mean return is 36%, but varies quite a lot. We note that 9.5% of the prospective students expect negative returns to university.

[36] Here we use survey-stated chosen field. If we use chosen field based on application or enrollment, the university distribution (Figure 2) and returns to education distribution (Figure 3) look very similar.
[37] Students who chose the no-college option in the survey don't have a preferred "university field". For them we use a weighted average of all 8 college fields, where the weights are the popularity of these fields among the rest of the sample, as their expected university earnings.
[38] The detailed field of choice premium distributions (relative to high school) are shown in Appendix Figure A1.

Figure 3 about here

Next we investigate what characteristics correlate with an individual's ex-ante returns to university. We want to know if observable characteristics are predictive of expected returns, and to what extent. To do this we perform OLS regressions of the individual return ($\beta_i = y_{1i} - y_{0i}$) on *gender*, a *socio-economic status (SES) index* (having mean zero and standard deviation equal to one) based on parents education and income, if individual is a first- or second-generation immigrant (*foreign*), and on two high-school performance variables determined prior to the survey: *math score*, *English score*. In addition, in some regressions we include the grade point average (*GPA*) at the end of high school, as it is a broader achievement measure and is important for admission to popular university fields of study. The high school achievement variables are all standardized to have mean zero and standard deviation one in the estimations. Table 5 reports the resulting estimates, from OLS regressions on each variable separately (columns 1-6), on all variables combined (columns 7 and 8), with the last column also including field of study indicators.[39] We also checked to see if the sorting patterns differed within high school program specialization, and found no meaningful change relative to column 8.

Table 5 about here

Individual ex-ante returns to university education are correlated with some observable characteristics, including being higher for females, those with higher SES backgrounds and those with higher math scores.[40] However, the relationship to previous school performance measures are mixed, as the estimates for English and GPA both are statistically insignificant.[41] Including all these characteristics simultaneously gives an R2 of 0.061, hence a lot of the heterogeneity in the individual return remains unexplained. In the last column, we see that the estimates remain similar using only variation within fields of choice, even though the R2

[39] The results using the average of all university fields (rather than the expected earnings in chosen field) relative to high school are qualitatively similar.
[40] The result for SES is in line with estimates in Boneva and Rauh (2020) for UK.
[41] Our results are in line with Card's (1999) review in the handbook of labor economics who suggests that return to education is positively related to SES, but only possibly to measured ability, but less in line with results in Nybom (2017) who finds that the returns to education is strongly positively associated with ability, but only with parental earnings unconditional on ability.

increases to 0.236. Hence these results are not due to students with different characteristics making different educational choices.

In theory it could also be possible that the differences in expected returns are due to differences in information quality between groups. One piece of evidence against this explanation is an information experiment that we conducted on the same sample at the conclusion of the survey. Students were randomly offered the chance to see true average population earnings within field. If poor information in some groups (e.g. low SES) were the cause of differential expected returns, then we would expect that the information would induce more field switching for these individuals in the follow-up data. We find no evidence of this: no group responded to the information by differentially changing their application and enrollment behavior over the subsequent years.[42]

Section 4.2: Ex-ante earnings levels and returns distributions: by educational states

In this section we estimate the means and the distributions of expected earnings levels and returns separately by choice of level of education. This includes estimating various ex-ante treatment effects and sorting parameters (as summarized in Table 4), and to infer what these imply for models of educational choice and for returns to schooling estimations. We estimate these separately by stated choice, application and enrollment (as discussed in Section 2).

Distributions

We start by looking at the potential earnings distributions. These distributions are shown separately by the two educational states in Figure 4a-4c. The solid lines are the distributions of earnings for those that stop at high school (blue line) or continue with university (red line). The dashed lines are the counterfactual earnings distributions. Hence, in each figure we illustrate two "actual" (i.e., for the level of education they chose) ex-ante earnings distributions and two counterfactual earnings distributions, for the treated and untreated groups, respectively. The means (in logs) of these four distributions are shown in the first Panel of Table 6.

[42] A report summarizing the (non) result of the experiment will be available at sites.google.com/view/arielpihl shortly.

Figure 4 about here

Table 6 about here

We can infer at least two interesting findings from these figures. First, those who choose university have the highest university earnings expectation, higher than what those that stop at high school would have had if they would have chosen university.[43] However, as we see in the figures, there is still a lot of overlap of the distributions. There are a sizable number of high school graduates that expect to do better than many university graduates, had they chosen university. Second, those that prefer high school have a similar earnings distributions to what those that choose university would have had, had they chosen high school. On average, the expected earnings for high school is slightly higher for those choosing high school, between 2-7%, across the stated, applied and enrolled divisions, although not statistically significantly so.[44]

It is notable that our pattern of results presented in these figures are similar to results in Cunha, Heckman and Navarro (2005) who use factor models applied to representative observational data for the US to create counterfactual earnings distributions.[45] They find that high school graduates are somewhat more successful than college graduates, if the latter would have stopped education at high school. However, the overlap of the distributions are substantial. Additionally, they find that college graduates are more successful than high school graduates, if the latter would have gone to college.

If we relate expected earnings in high school (y_{0i}) and expected earnings in (preferred) university field of study (y_{1i}) we can directly estimate the association between perceived

[43] This finding holds even if we use a simple mean of all university fields for the high-school choosers.
[44] This can be seen from the "Selection Bias" ($E[y_1|S=0]-E[y_0|S=0]$) estimate in the first row in Panel C of Table 6.
[45] See figures 6.1-6.4 in Cunha, Heckman and Navarro (2005). They also find that results for ex-ante and ex-post distributions are similar, which they argue is the in line with heterogeneity (as opposed to uncertainty) explaining most measured variability in earnings. The literature using observational data to analyze counterfactual earnings distributions is surveyed in Cunha and Heckman (2007). This literature is distinguishing between ex-post counterfactual distributions, as well as ex-ante counterfactual distributions, the latter estimated from their ex-post data.

earnings ability in high-school and university, respectively.[46] If $Cov(u_{0i}, u_{1i}) > 0$ it suggests that earnings ability is (more or less, depending on the magnitude) unidimensional: those expected to be most “able” in high school occupations are also the most able ones in university occupations. If $Cov(u_{0i}, u_{1i}) < 0$ earnings ability is (more or less) multidimensional: those expected to be more “able” in high school occupations are the least able in university occupations. In our data we estimate a correlation coefficient of 0.4.[47] [48] Hence, individuals who expect to do well in one educational state also expect to do well in the other educational state, on average. This is at odds both with results in papers by Carneiro, Heckman and Vytlacil (2011) and Cunha, Heckman and Navarro (2005), who test for this using observational data for US applied to “factor models” and find evidence of a negative correlation, but also deviates from unitary ability models assuming the individuals who do better in one state necessarily do better in the other state.

Average treatment effects and sorting patterns

In Figures 4d-4f, we use the expected earnings data and calculate the distributions of returns to university separately for those choosing university (S=1, TT) and for those choosing high school (S=0, TUT). Table 6 reports the estimates of the average treatment effects overall and for these two groups (Panel B). Although it is evident from the figures that there is a lot of distributional overlap, the estimates of average treatment effects provide us with some clear findings. The average treatment effects are estimated as 0.36, which implies about 9% per year of college. We find that the ex-ante average treatment on the treated effects are estimated to be only slightly higher, around 0.38, whereas the ex-ante average treatment on the untreated effects are estimated lower but still positive as 0.18 to 0.30, so about 5-7% per year, with larger TUT estimates if choices are revealed instead of stated. That TT>TUT>0 implies sorting with respect to comparative advantage and that those that choose high school have positive expected earnings gains from choosing university, even though they do not choose university. We now turn to a discussion about the implication of these results.

[46] Since $Cov(y_{0i}, y_{1i}) = Cov(u_{0i}, u_{1i})$ using the framework in Section 3.
[47] This magnitude is very similar if we estimate correlations within educational choice categories (if we ignore the estimated correlation coefficient for the small group of untreated in the survey, which is very imprecisely estimated).
[48] These correlations are slightly larger if we use the simple mean of college fields (rather than the field that students actually chose. So, they are not driven by using actual chosen field.

First, our finding of a positive and large TUT goes against the pure Roy model of earnings maximization and instead suggests that there are other factors affecting these students' choices. One such candidate is direct cost of schooling. However, since all universities in Sweden are free and living expenses are covered by generous grants and subsidized loans, this is very unlikely to be the explanation. Another candidate is psychic costs of schooling, which has been shown to be important in rationalizing college education choice behavior in the US (HLT, 2006). As part of the survey we asked the students about their expected enjoyment of study, the probability of graduating on time, the expected number of study hours, and the degree of parental approval associated with a choice of fields of university study, and we use these measures for the preferred field choice as proxies for psychic costs of university education. In Appendix Table A2 we regress the expected university earnings returns on these measures separately for those choosing high school and university. Ignoring the results in the first column (which are very imprecise) there is clear evidence that these measures are unrelated to expected returns for those that do not choose university, whereas they are somewhat related to returns for those that choose university (for which longer expected study hours and (possibly) higher parental approval is associated with higher earnings return). Hence, psychic costs, at least as captured through these measures, are unlikely to explain our finding of a positive TUT.

Another possibility is compensating non-pecuniary factors: that giving up earnings from choosing high school might be expected to benefit individuals later in life in ways other than earnings. Unfortunately, we do not have access to direct counterfactual information about non-pecuniary outcomes in the choice to discontinue education after high school, but we return to this explanation in Section 5.2 using when we investigate ex-ante treatment effect for high versus low paying university fields of choice.

Second, the fact that we find on average $TT > TUT$ means that the expected relative gain from choosing university is higher for those choosing university than for those choosing high school, on average. This is consistent with students sorting based on their comparative advantage in respective educational state. As we discussed in Section 3.3., we can use the estimated ex-ante treatment effects and the ex-ante OLS estimates (in Panel B) to calculate the sorting parameters, whose magnitude have implications for bias in ex-ante OLS estimates of returns to university education. We report these in Panel C of Table 6, using the equations (11)-(13). Since the selection bias (SB) is estimated small or negative, the ex-ante OLS estimate is fairly similar to an ex-ante TT estimate. The sorting gain from choosing university, for those choosing university (compared to the average person) (SE_1), is very small, whereas the sorting loss from choosing high school, for those choosing high school (SE_0), is very large.[49] Hence, the bias in an OLS estimate of ATE due to heterogeneous returns is positive but small (in line with what was argued in Card, 1999, 2008), whereas the selection bias tends to lead to a slight underestimate of ATE. Since they go in opposite directions, the OLS estimate is very similar to the ATE estimate. Inferring TUT from OLS, however, leads to a large downward bias, driven mostly by SE_0.

Note that if we are to compare our results for the estimated ex-ante treatment effects in Panel B with those from observational studies, we find that they are similar to those in Carneiro, Heckman and Vytlacil (2011) and HLT (2006) in that $TT > ATE > TUT$, where OLS is a downward biased estimate of TT. However, they differ in other dimensions, since they find that TUT to be roughly zero, that OLS is an upward biased estimate of ATE and that the difference between TT and TUT is due to a positive sorting gain (SE1>0). If we instead compare our ex-ante estimates to Nybom (2017), who uses observational administrative data for Sweden, the results are remarkably similar to what we find. Nybom (2017) finds that $TT > ATE > TUT$, where TUT is positive, and that the OLS estimate, without controls, is very similar to the ATE estimate. As OLS<TT, the sign on the selection bias is found to be negative.[50]

Third, the finding that the ex-ante TUT estimates increase when we use data on application or enrollment, compared to stated choices, suggests that the students who are switched into non-treatment when preferences are revealed, have higher expected returns than those who already during the survey stated that they will not continue to university. This suggests that expected returns vary across prospective students depending on their propensity to enroll at university education.

To see this more clearly, we first divide those that stated that they would choose university into those that later did apply and those that did not, and those that applied to university into those

[49] As shown in section 3.3, the difference between the ex-ante TT and TUT estimates consists of the two sorting effects SE_0 and SE_1.

[50] We compare our estimates to the semiparametric estimates reported in Nybom (2017). The only deviation in the results is that the sorting effect in our case is almost entirely driven by SE_0, something which explains why our ex-ante estimates of TT is closer to (the lower) ATE, whereas in Nybom (2017) the TUT estimate is closer to (higher) ATE estimate.

that later did enroll and those that did not. In this way we can estimate average treatment effects for those that are more or less likely to keep being treated, the latter group constituting prospective students that eventually opted out of treatment but likely were closer to actually take up treatment than those that neither stated nor applied to university. These results are shown in Appendix Table A4, and discussed further in Appendix D.[51] We see that the expected return to university among those that stated that they would go to university but did not apply is estimated as 0.263, which is much lower than the 0.400 among those that did apply. A similar pattern is found among those that applied with a higher expected return for those that eventually enroll than for those that did not (0.405 versus 0.342). A division of individuals into only treated and untreated groups therefore gives an incomplete picture of the heterogeneity of returns, and future work using subjective expectations data to estimate ex-ante treatment effects should probably continue to explore the distribution of returns with respect to the likelihood of enrolling or choosing treatment.[52]

Section 5: Estimating ex-ante levels and returns distributions across high- versus low paying fields of study choice

In this section we describe similar estimates of means and distributions of treatment effects and sorting parameters as in Section 4, but instead look within university at high- versus low paying fields. The horizontal choice dimension is interesting in its own right, as a comparison with our results for the level dimension in the previous section, as well as to make comparison with the results in Arcidiacono et al., (2020) and Wiswall and Zafar (2020) who estimated ex-ante treatment effects across occupational fields and college majors using data collected from two colleges in the US. As we also possess counterfactual data on non-pecuniary outcomes associated with different university fields of study, we are also able to provide ex-ante treatment effects for these outcomes, as well as to reconcile them with the results for earnings, which can help rationalize the positive TUT found earlier.

[51] In Appendix Figure A2, we elaborate on this by first predicting the propensity to enroll at university, using all the information from the stated, applied and enrollment choices. Relating the expected returns to this predicted propensity to enroll at university, show a positive relationship which further collaborates these results.
[52] As, for instance, through more explicit estimation of ex-ante versions of treatment effects for those at the margin of participation (Björklund and Moffitt, 1987; Heckman and Vytlacil, 2007; Carneiro, Heckman and Vytlacil, 2011).

Section 5.1: Ex-ante earnings levels and returns distributions

The estimates of the means and distributions for high- and low paying fields are shown in Table 7 and Figure 5. We see (perhaps unsurprisingly) that expected earnings are higher for the high paying fields, for both those who pursue them and those that don't: the second row of Panel A contains larger mean log expected earnings than those in the first row. The estimated ex-ante ATEs are large and positive (about 0.38) and very similar to the ATEs estimated for university versus high school in the previous section. The estimated ex-ante TTs are larger than the TUTs, but the TUTs are still large and positive. Sorting gain from choosing high paying fields (SE_1) is positive but small, and sorting effects from choosing low-paying fields (SE_0) is positive and large. Hence, these results also support sorting based on comparative advantage. There is no evidence of positive selection bias. The estimated SBs are negative, even though only statistically significant for stated choice. Hence, the earnings in low paying fields are expected to be low, also by those choosing high-paying fields had they instead chosen low paying fields.

Overall, the results here are very similar to those in the previous section where we compare university and high school choice. This is true for the estimates of the average treatment and sorting effects, as well as the distributions. The positive and large TUTs suggests that there are probably other (i.e., non-pecuniary) factors that are very important for this decision. As we collected expectations data on non-pecuniary benefits for field of study, we can investigate this hypothesis directly, something which we do below. We can also compare our results to those in Wiswall and Zafar (2020) who compare TT and TUT for Science/Business versus Humanities/Social Science fields, and find TT>TUT, but that TUT>0, very much in line with what we find here, although their TT estimates are larger than ours.

Figure 5 about here

Table 7 about here

Section 5.2: Ex-ante non-pecuniary returns

In previous sections we showed that the TUTs are positive both for the college choice and high-paying university field choice. This means that individuals systematically leave money on the table when they make their educational choices. One potential reason for this is that there are negative non-pecuniary returns to these same choices which offset the earnings returns in the students' utility function. In this section we investigate if this is the case. We do so by repeating the analysis from Section 5.1. Hence, we estimate ex-ante treatment effects and sorting parameters for these outcomes, and compare the results for those using earnings.

In the survey we asked the students questions about expectations and beliefs about some non-pecuniary outcomes: the probability of finding a job directly after graduation, the probability of job satisfaction at age 30, the probability of being able to combine work and family life at age 30, the social status (separate from salary) they associate with the field of study (and later occupation)[53] and expected hours per week of work at age 30. The work hours question is asked in a scale from 0 to 80 hours, whereas the answers to the other outcomes are provided on a scale from 0-100.[54] Note that we carefully explained what is meant by a probability. For a more detailed description of the general instructions and the specific questions in the survey see Appendix Section A**.** In Appendix Table A3 we show summary statistics in the expected amenities for all fields.[55]

To facilitate comparison between the sizes of the coefficients across variables, we have standardized all expectations amenities to have mean zero and standard deviation one in the full sample. We also reverse hours of work such that a larger value corresponds to a positive outcome (less time spent). These questions were only asked for the college fields because we did not think that imagining the type of job they would get at age 30 or later would be tangible enough for the no-college option.

[53] When we asked the students about perceived social status of the field of study (and resulting occupation) they were specifically instructed to answer independently of the associated earnings level. Because social status is a key concept within sociology we wanted to be able to gauge its importance separately from earnings.
[54] For instance, for each hypothetical choice we asked "How high is the probability that your parents and other family members would approve of your choice of major?" The average response to this question for males was 72.3, meaning that on average they expected that there was a 72.3% chance that their parents would approve the choice.
[55] Expected hours of work per week are high. The mode is 40 hours (28% of the respondents), but over 40% of the respondents provide figures between 41 and 60 hours. This might be due to the survey question which asked about the work hours they need to work, which could be interpreted as full-time work plus overtime etc., and/or that they need to work a lot of hours to keep up in fields where they deem themselves uninterested or untalented.

To test whether these non-earnings outcomes explain the positive TUT, we examine returns similarly as we did for the earnings measure in Section 5.1 (and as described in section 3). In Table 8 we condense the presentation of these estimates to the ATEs, TTs and TUTs, as well as for results using data from survey and from enrollment as treatment status.

We find mostly positive and large TT estimates. For instance, those choosing high paying fields of choice expect 1.39 SD higher social status, 0.92 SD higher probability of finding a job, and 1.01 SD higher probability of enjoying the job in a high paying field, compared to what they would have expected to have experienced had they choose a low paying field. Using enrollment in high-paying fields give similar estimates. We also note that the TT estimates for hours worked are positive with those choosing high paying fields expecting to work two-quarter of an hour more.

These TT estimates are notably higher than the TUT estimates for all outcomes. Also, the sign of the TUT estimates are sometimes positive (as for status) and sometime negative (as for enjoying the job and work-life balance). It seems like part of the story for why the TUT estimates for earnings where positive for high versus low paying fields of choice is that prospective students of low-paying fields expect to experience less enjoyment on the job and to be less able to balance work and life, if they would have chosen a high paying field. They give up earnings to instead be compensated in some aspects of non-pecuniary benefits. We also note that given the similarity between the level of education (university/high school) results and the high/low paying fields results, our results here may also be valid for the level results.

Section 6: Conclusions

In this paper we have estimated means and distributions of ex-ante treatment effects for university education relative to high school, as well as to high- and low paying university field-of choice, using elicited earnings expectations associated with counterfactual educational choices. We have shown that average ex-ante returns to university are substantial, with treatment effects for those choosing and enrolling to university being larger than for those who did not choose or enroll at a university. We have also put our results into a framework for estimating the returns to education typically associated with ex-post returns, and found that ex-ante selection bias is small, and possibly even negative, and that although individuals choose in accordance with their expected comparative advantage in earnings, the resulting ex-ante bias due to heterogeneous returns are fairly small.

The usage of ex-ante data is not without problems. It requires high-quality survey data so that elicited expectations are informative about expected outcomes. This is especially true if it is to be compared to future ex-post outcomes. However, it is therefore remarkable how similar our findings are to some of the studies using observational ex-post data and, sometimes, strong econometric identification assumptions, to estimate various means and distributions of treatment effects. For instance, qualitatively, our results from estimating ex-ante treatment effects are much in line with Nybom (2017) who used the MTE framework and observational data in Sweden to estimate ex-post returns to university. Although we find higher average ex-ante returns compared to the estimated ex-post returns in Nybom (2017), we also find agreement with respect to TT to university being larger than the TUT, that the TUT is positive and sizable, and that selection bias is small and negative.

We also estimated ex-ante returns to high- versus low university fields of study (as in Wiswall and Zafar, 2020). We found positive estimates, where TT are larger than TUT and the results in general are qualitatively similar to those for university versus high school. As we also elicited subjective expectations for a set of non-pecuniary outcomes, we find that the choice with respect to traditional high- and low earnings fields, where TUT was found to be mostly positive, can be reconciled with negative TUT returns in some non-earnings factors, such as enjoyment of work and combining family and work. This is in line with an extended Roy model where individuals take into account broader utility when making their educational choices. This is similar to those results in Arcidiacono et al., 2020, and Wiswall and Zafar, 2020, using elicited subjective expectations data for US. Hence, in this way, their results for the US are in line with our results for Sweden, despite the large existing difference in the degree of earnings inequality and system of higher education.

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1 Tables

Table 1: Categories of education and particular degree programs

Category	Common programs
Low-paying fields	
Education and teacher training	Subject teacher training; Pedagogy and didactics
Humanities and Art	Media production; History and archeology
Agriculture, Forestry and animal health	Veterinary care; Agriculture and forestry
Services	Tourism and travel; Police training
High-paying fields	
Social science, Law, Business, etc.	Psychology; Business administration; Law
Natural science, Mathematics and Data	Biology; Computer science; Mathematics
Engineering and Manufacturing	Civil engineering; Technical Engineering (mechanical, electrical)
Healthcare and social care	Medical training; Social work and guidance

Table 2: Mean expected earnings by field

Broad Field of Study	E(Earn) 30-40 (1000SEK/mo)
High school	26.6
Teaching	29.5
Humanities	30.6
Animal/Agro	31.7
Services	36.1
Social Sci	43.7
Sciences	39.6
Engineering	38.3
Health	37.4

Table 3: Correspondence between stated and revealed educational choices

	Expected Field
Panel A: Comparison with application data	
Match including everyone (many assigned to “no college”)	46.8% (233/498)
Match for just college fields	55.3% (218/394)
Match for just those with HS in the survey	65% (15/23)
Panel B: Comparison with enrollment data	
Match including everyone (many assigned to “no college”)	38% (189/498)
Match for just college fields	51.5% (173/336)
Match for just those with HS in the survey	70% (16/23)

Table 4: Parameters

Panel A: Returns to schooling estimates		
Observed earnings and education OLS	$\beta_{OLS} =$	$E[y_{i1}\|_{S_i=1}] - E[y_{i0}\|_{S_i=0}]$
Treatment on the treated	$\beta_{TT} =$	$E[y_{i1} - y_{i0}\|_{S_i=1}]$
Treatment on the un-treated	$\beta_{TuT} =$	$E[y_{i1} - y_{i0}\|_{S_i=0}]$
Average treatment effect	$\beta_{ATE} =$	$E[y_{i1} - y_{i0}] = p\beta_{TT} + (1-p)\beta_{TuT}$
Panel B: Other returns to schooling estimates		
Individual i's returns	$\beta_i =$	$y_{i1} - y_{i0}$
Treatment effect at margin	$\beta_{EOTM} =$	$E[y_{i1} - y_{i0}\|_{imarginal}]$
PanelC: Parameters deriving from Panel A		
Selection bias	$SB =$	$\beta_{OLS} - \beta_{TT} = cov(Y_{i0}, S_i)/var(S_i)$ (typical AB: $E[y_{i0}\|_{S_i=1}] > E[y_{i0}\|_{S_i=0}]$)
Sorting effect 1	$SE_1 =$	$\beta_{TT} - \beta_{ATE}$
Sorting effect 0	$SE_0 =$	$\beta_{ATE} - \beta_{TUT}$
Comparative advantage	$CA =$	$SE_1 + SE_0 = cov(\beta_i, S_i)/var(S_i)$ ($CA > 0 \implies \beta_{TT} > \beta_{TuT}$)
Earnings ability correlation	$=$	$corr(u_0, u_1) = corr(Y_{i0}, y_{i1}$

Note: y_{ic} is individual i's expected earnings in $c = 1$ (college) or $c = 0$ (no college). S_i is their actual expected choice to pursue college or not. p is the share of the population that intends to pursue college.

Table 5: How β_i varies with demographics

	(1) Beta_i	(2) Beta_i	(3) Beta_i	(4) Beta_i	(5) Beta_i	(6) Beta_i	(7) Beta_i	(8) Beta_i
Female	0.0563*						0.0624+	0.0650*
	(0.0274)						(0.0342)	(0.0324)
SES Index		0.0294**					0.0367**	0.0260*
		(0.00938)					(0.0118)	(0.0109)
Foreign			0.0476				0.142**	0.119**
			(0.0365)				(0.0475)	(0.0450)
Math Score				0.0320*			0.0475+	0.0365
				(0.0157)			(0.0269)	(0.0247)
English Score					0.00697		-0.0351	-0.0238
					(0.0171)		(0.0289)	(0.0266)
HS GPA						0.00221	-0.0124	-0.0216
						(0.0180)	(0.0375)	(0.0330)
Choice FE								Yes
R^2	0.008	0.024	0.004	0.012	0.001	0.000	0.061	0.236
N	498	498	434	397	438	434	343	343

Note: $\beta_i = y_{i1} - y_{i0}$, an individual's expected college premium where y_{i1} is the earnings in stated choice college field for college choosers. For those who do not plan to go to college, we use a weighted average of all the college fields, where the weights are based on popularity among those who stated they intended to go to college in the survey. Both scores and GPA are standardized to mean 0, standard deviation 1.

Table 6: College vs. no-college, college-earnings defined using chosen fields and weighted averages

	Stated		Applied		Enrolled	
	(1) S=0	(2) S=1	(3) S=0	(4) S=1	(5) S=0	(6) S=1
Panel A: Conditional expected log earnings						
c=0	3.313	3.239	3.260	3.238	3.258	3.235
c=1	3.455	3.613	3.533	3.633	3.556	3.615
N	23	475	104	394	162	336
Panel B: Ex-ante treatment effects						
β_{ols}	0.300***		0.373***		0.357***	
	(0.0490)		(0.0290)		(0.0252)	
β_{ate}	0.366***		0.370***		0.353***	
	(0.0137)		(0.0132)		(0.0125)	
β_{tt}	0.374***		0.396***		0.380***	
	(0.0138)		(0.0146)		(0.0150)	
β_{tut}	0.182*		0.273***		0.298***	
	(0.0746)		(0.0295)		(0.0219)	
Panel C: Sorting parameters						
sb	-0.0741		-0.0230		-0.0236	
	(0.0484)		(0.0279)		(0.0240)	
SE_1	0.00887*		0.0257***		0.0269**	
	(0.00385)		(0.00719)		(0.00879)	
SE_0	0.183**		0.0975***		0.0559**	
	(0.0702)		(0.0260)		(0.0179)	
ca	0.192**		0.123***		0.0828**	
	(0.0736)		(0.0328)		(0.0265)	
$corr(u_0, u_1)$	0.370***		0.377***		0.420***	

Note: $E[y_{i1}|S_i = 1]$ uses on $i's$ chosen field. Because $S = 0$ individuals do not have a chosen field (they 'chose' no college) $E[y_{i1}|S_i = 0]$ is a weighted average of the eight fields, where the weights are the popularity of the fields among the $S = 1$ individuals. These weights are redefined for every time period.

Table 7: High pay vs. low-pay, defined using chosen fields and weighted averages

	Stated		Applied		Enrolled	
	(1) S=0	(2) S=1	(3) S=0	(4) S=1	(5) S=0	(6) S=1
Panel A: Conditional expected log earnings						
High Pay=0	3.399	3.293	3.334	3.302	3.350	3.297
High Pay=1	3.618	3.670	3.590	3.683	3.580	3.663
N	100	375	38	322	52	284
Panel B: Ex-ante treatment effects						
β_{ols}	0.272***		0.349***		0.313***	
	(0.0338)		(0.0393)		(0.0364)	
β_{ate}	0.344***		0.368***		0.345***	
	(0.0113)		(0.0133)		(0.0132)	
β_{tt}	0.378***		0.381***		0.366***	
	(0.0125)		(0.0141)		(0.0141)	
β_{tut}	0.220***		0.256***		0.229***	
	(0.0223)		(0.0347)		(0.0317)	
Panel C: Sorting parameters						
sb	-0.106**		-0.0321		-0.0529	
	(0.0327)		(0.0385)		(0.0353)	
SE_1	0.0332***		0.0132**		0.0211***	
	(0.00612)		(0.00438)		(0.00595)	
SE_0	0.125***		0.112***		0.115***	
	(0.0203)		(0.0330)		(0.0291)	
ca	0.158***		0.125***		0.137***	
	(0.0255)		(0.0369)		(0.0343)	
$corr(u_0, u_1)$	0.526***		0.453***		0.463***	

Note: $E[y_{i1}|S_i = 1]$ and $E[y_{i0}|S_i = 1]$ use $i's$ chosen field. $S = 0$ individuals do not have a chosen high-paying field, so $E[y_{i1}|S_i = 0]$ is a weighted average of the four high-paying fields, where the weights are the popularity of the fields among the $S = 1$ individuals. We define chosen low-paying field weights similarly for $S = 1$ individuals. These weights are redefined for every time period.

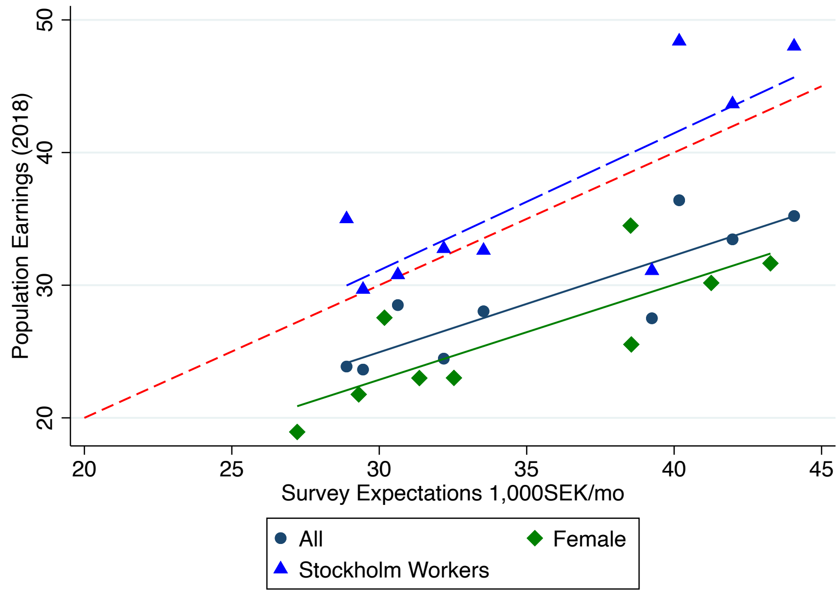
Table 8: Non-earnings returns to high paying versus low paying fields

	(1) Social Status	(2) Find Job	(3) Enjoy Job	(4) Work Hours	(5) Work-Life Bal
Panel A: Survey Choice					
β_{tt}	1.391***	0.916***	1.014***	0.664***	0.0610
	(0.0449)	(0.0494)	(0.0490)	(0.0439)	(0.0533)
β_{tut}	0.488***	-0.00540	-1.021***	0.281**	-0.667***
	(0.0826)	(0.108)	(0.0777)	(0.0885)	(0.0984)
Panel B: Application Choice					
β_{tut}	1.409***	0.864***	0.928***	0.584***	-0.0903
	(0.0478)	(0.0570)	(0.0553)	(0.0476)	(0.0596)
β_{tut}	0.609***	0.148	-0.737***	0.141	-0.628***
	(0.151)	(0.159)	(0.174)	(0.203)	(0.163)
Panel C: Enroll Choice					
β_{tut}	1.331***	0.750***	0.845***	0.444***	-0.0973
	(0.0534)	(0.0577)	(0.0602)	(0.0513)	(0.0596)
β_{tut}	0.761***	0.146	-0.396*	0.134	-0.315*
	(0.122)	(0.123)	(0.170)	(0.170)	(0.152)

Note: Standard errors in parentheses. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001. We calculate returns as we have before but replace earnings with the noted variable. The outcomes have been standardized to mean zero, standard deviation 1.

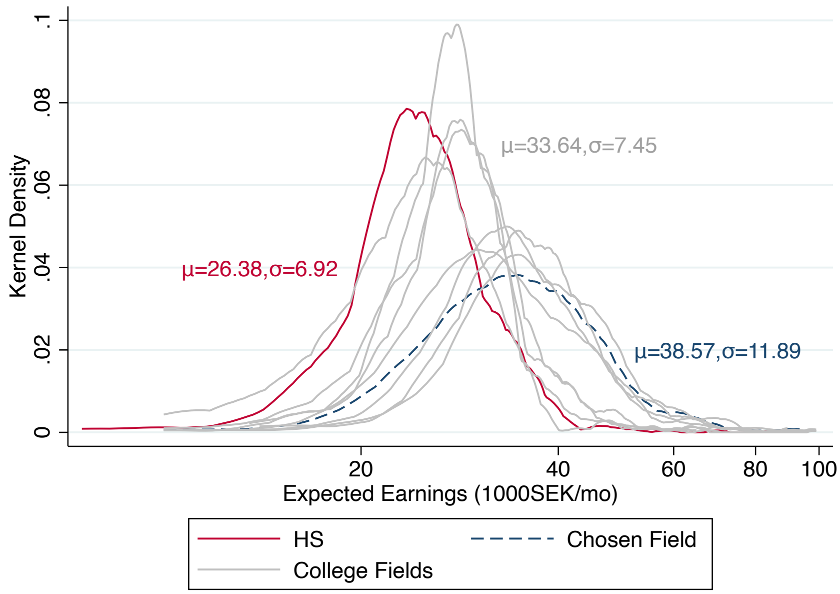
2 Figures

Figure 1: Comparing expected earnings to population earnings



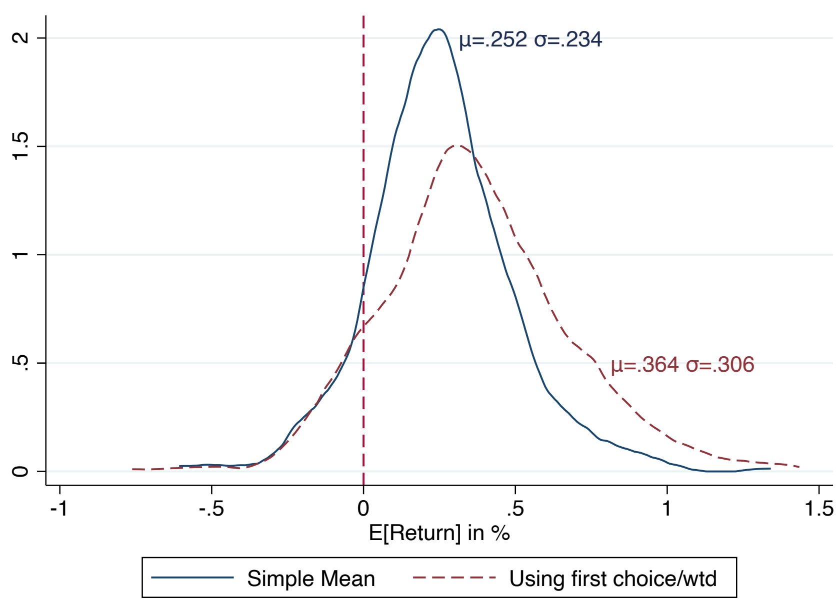
Note: Red dashed line is the 45 degree line. Plots the surveyed mean expected earnings in each field against population mean earnings in administration data (for those aged 40 in 2018), along with linear fit lines. The survey data is the full sample for both "All" and "Stockholm Workers", and the female sample respondents for "Female." The population data for "Stockholm Workers" is all those aged 40 and registered as working in Stockholm municipality in 2018.

Figure 2: Distribution of earnings by university and field



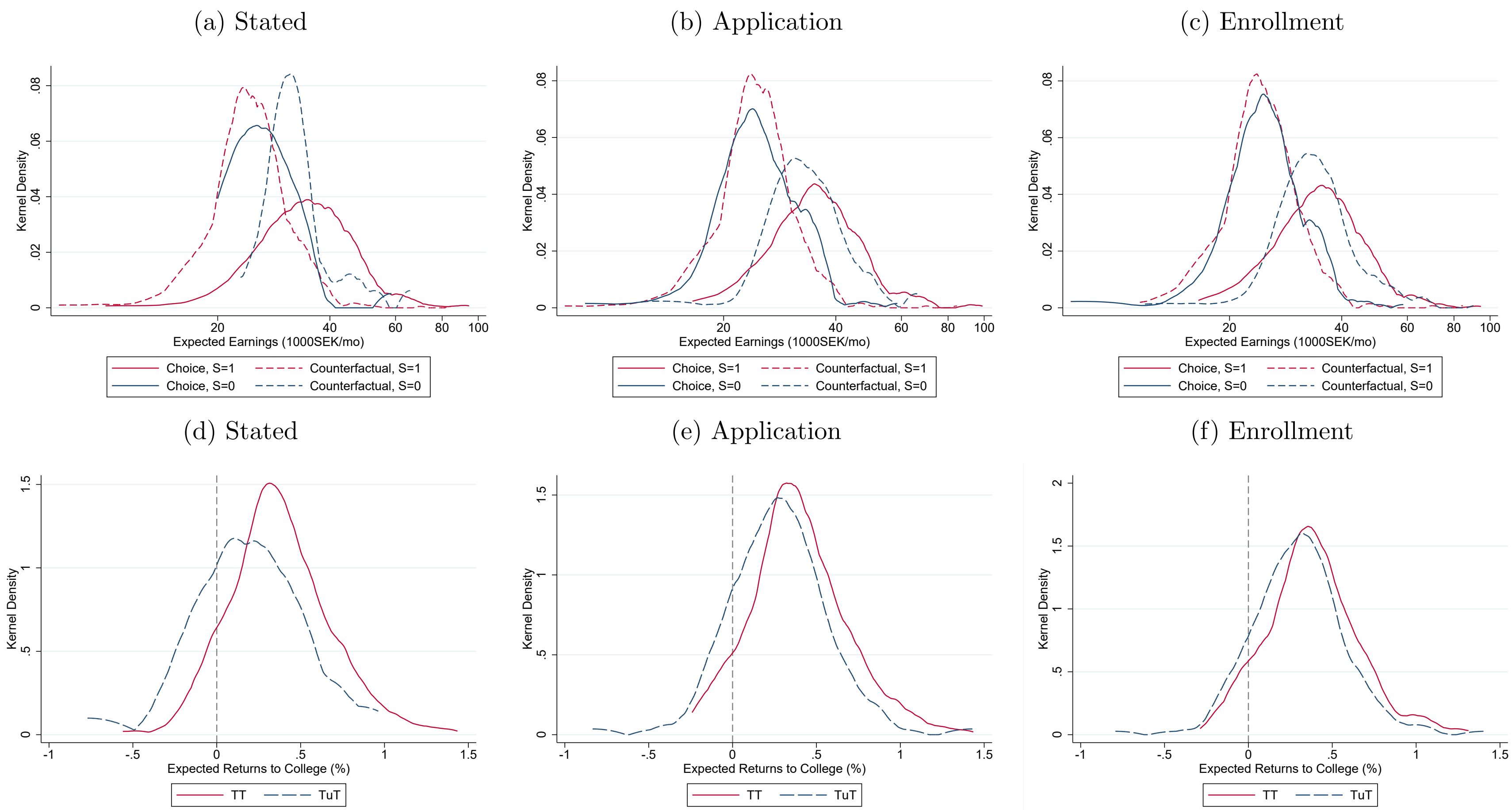
Note: Plots the distributions of expected earnings for no college degree/HS (y_{i0}) in red, and for each individual field (y_{ij}) in grey. The blue dashed line is expected earnings for the stated (survey) chosen field for all individuals.

Figure 3: Distribution of university premiums (β_i)



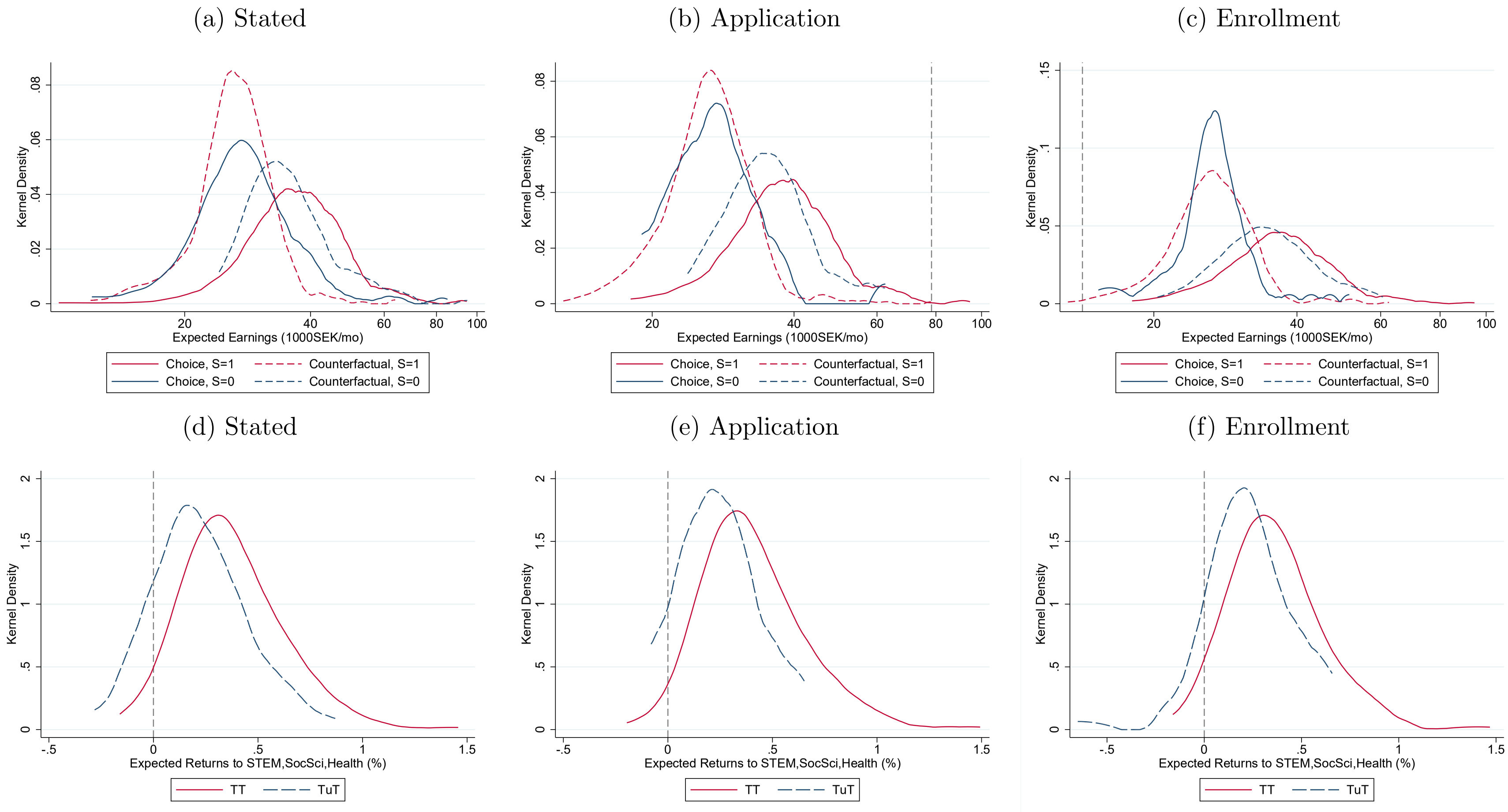
Note: Shows how the returns to college change when we use the returns to actual chosen college field (rather than the average of all college fields). Since those who don't plan to go to college don't have a "chosen" college field, their returns are the average of the college fields weighted by their popularity in the whole sample.

Figure 4: Expected earnings in college vs no-college by treatment status



Note (a)-(c): The solid lines are the distributions of earnings for those that stop at high school (blue line) and continue with university (red line). The dashed lines are the counterfactual earnings distributions. Hence, the blue dashed line is the university earnings distribution for those that choose high school, and the red dashed line is the high school earnings distribution for those that choose university. S=1 are people who chose a college field (stated in the survey, or by applying/enrolling), S=0 those did not. Earnings in college uses actual chosen field expected earnings for S=1, and an average of all fields weighted by their sample popularity (amount stated in survey/application/enrollment) for S=0.
Note (d)-(f): Returns to college calculated as log expected college earnings (for either expected field if treated, or a weighted average of all fields if untreated), minus log expected non-college earnings.

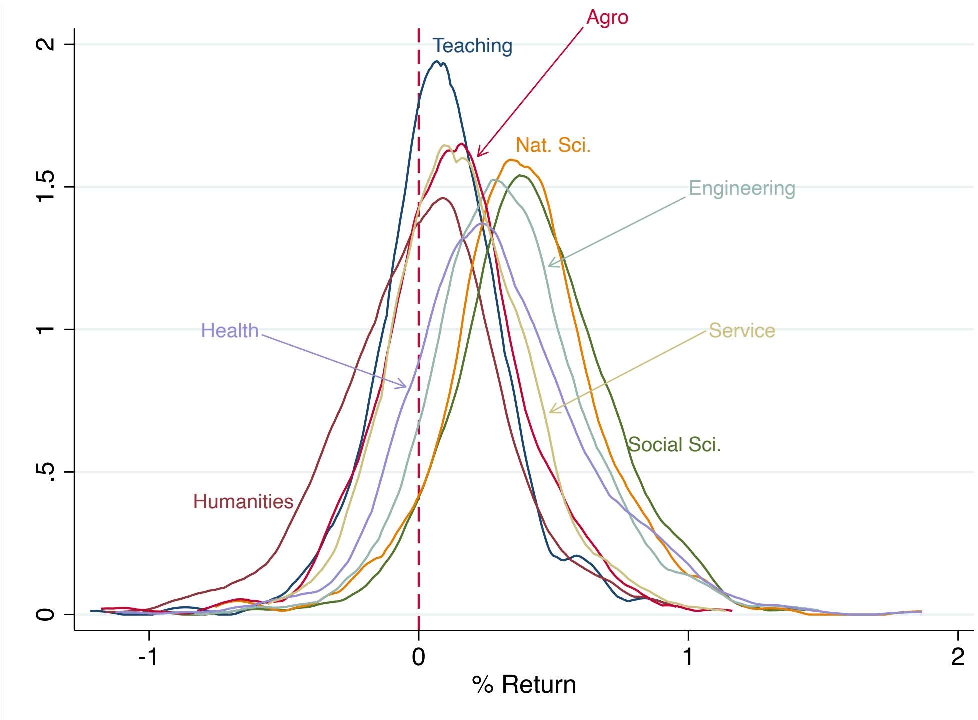
Figure 5: Expected earnings in high-paying vs. low-paying fields by treatment status



Note (a)-(c): High-paying field categories are: Social Science, Sciences, Engineering and Health care. Low-paying fields are Humanities, Teaching, Agriculture and Services. S=1 is people who chose a high paying field field (stated in the survey, or by applying/enrolling), S=0 those did not. Earnings in high-paying fields uses actual chosen field expected earnings for S=1, and an average of the high paying fields fields weighted by their sample popularity (amount stated in survey/application/enrollment) for S=0. Likewise for low-paying fields.
Note (d)-(f): Returns to high-paying fields calculated as log expected college earnings (for either expected field if treated, or a weighted average of high-paying fields if untreated), minus log expected low-paying field earnings (for either expected field if un-treated, or a weighted average of the low-paying fields if treated.

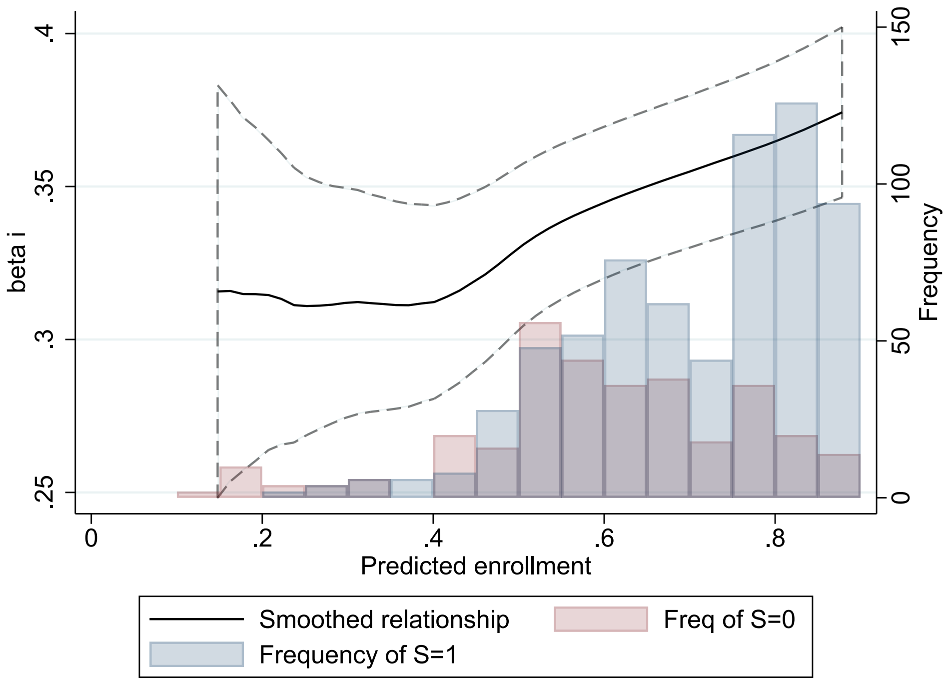
A Appendix Tables and Figures

Figure A1: Unconditional expected returns to field of study relative to no college



Note: X-axis is difference in log earnings between the field and the no college options.

Figure A2: Treatment effect size versus propensity to enroll



Note: X-axis is predicted propensity to enroll in university, where the explanatory variables are from the individual's average ranking of low-paying fields and chosen field in the survey. The line plots a local polynomial for expected returns to college, along with a 95% confidence interval (dashed lines). The histogram uses the right y-axis and shows the number of individual who actually enroll in college (S=1) and don't (S=0) for different predicted enrollment probabilities.

Table A1: Summary statistics on family and high school variables

	Surveyed Sample			Population	
	(1)	(2)	(3)	(4)	(5)
	Male	Female	Total	Stockholm	All
Background Variables:					
Foreign background	0.174	0.208	0.191	0.300	0.174
	(0.380)	(0.407)	(0.394)	(0.458)	(0.379)
Mom went to university	0.510	0.488	0.499	0.390	0.250
	(0.501)	(0.501)	(0.501)	(0.488)	(0.433)
Father went to university	0.500	0.478	0.489	0.378	0.178
	(0.501)	(0.501)	(0.500)	(0.485)	(0.383)
Parent(s) annual income (1000s SEK)	877.6	844.8	865.4	738	661.4
	(789.8)	(537.5)	(680.1)	(677.2)	(403.7)
School Variables:					
Avg. English Test Score (/20)	16.15	15.95	16.06	15.31	13.74
	(3.620)	(3.457)	(3.538)	(3.915)	(4.211)
Avg Math Score (/20)	12.88	12.52	12.70	10.05	8.289
	(5.225)	(5.093)	(5.157)	(5.983)	(6.084)
College Prep Program	0.878	0.910	0.894	0.886	0.625
	(0.328)	(0.287)	(0.309)	(0.317)	(0.484)
STEM Specialized Program	0.504	0.361	0.434	0.361	0.215
	(0.501)	(0.481)	(0.496)	(0.480)	(0.411)
Total Observations	254	244	498	2949	98936
N with all Vars	162	159	321	1600	56635

Table A2: Costs of choosing to go to college and expected returns.

	Survey		Applied		Enrolled	
	S=0	S=1	S=0	S=1	S=0	S=1
	(1)	(2)	(3)	(4)	(5)	(6)
Constant	0.0967	0.349***	0.264***	0.354***	0.309***	0.331***
	(0.0806)	(0.0265)	(0.0412)	(0.0223)	(0.0272)	(0.0206)
Grad Prob	-0.225	0.00258	-0.0267	-0.0120	0.0322	-0.0258
	(0.182)	(0.0230)	(0.0610)	(0.0281)	(0.0405)	(0.0262)
Enjoy Studies	0.223	0.00841	0.0181	0.0150	-0.0497	0.0361
	(0.157)	(0.0260)	(0.0705)	(0.0262)	(0.0546)	(0.0253)
Study Hours	-0.199*	0.0387**	-0.00504	0.0361*	0.0102	0.0314+
	(0.0783)	(0.0145)	(0.0349)	(0.0157)	(0.0262)	(0.0160)
Fam Approve	0.0259	0.0121	0.00780	0.0496+	-0.0191	0.0603*
	(0.0922)	(0.0248)	(0.0598)	(0.0275)	(0.0384)	(0.0240)
Observations	23	475	104	394	162	336
R^2	0.369	0.019	0.002	0.034	0.010	0.057

Note: Standard errors in parentheses. + p<0.10, * p<0.05, ** p<0.01, *** p<0.001. Outcome is individual expected return (β_i) in all columns. Cost measures are taken from the individual's expectations in the event that they attend college, regardless of whether they did ($S = 1$) or did not ($S = 0$) choose college.

Table A3: Means for all questions by field of study

	Edu	Hum	Soc Sci	Sci/Math	Tech/Eng	Agro	Health	Serv	Total
Prob of passing the degree	76.17	67.40	75.83	64.02	66.53	67.21	69.18	73.40	69.97
	(25.68)	(28.97)	(23.19)	(28.41)	(26.12)	(28.36)	(24.93)	(24.51)	(26.67)
Prob of enjoying coursework	54.88	55.90	67.72	55.72	55.69	44.05	59.95	54.27	56.02
	(25.17)	(29.71)	(24.77)	(29.63)	(27.20)	(28.68)	(26.12)	(25.13)	(27.78)
Expected study hrs/wk	34.79	33.45	44.16	47.47	42.11	33.37	44.90	32.91	39.15
	(17.19)	(18.63)	(19.48)	(20.42)	(19.22)	(16.74)	(20.18)	(17.42)	(19.54)
Parental approval	69.94	61.03	82.64	83.72	75.91	59.35	82.34	62.69	72.20
	(27.43)	(31.18)	(21.55)	(21.34)	(25.70)	(32.37)	(22.60)	(29.48)	(28.43)
Prob find a job	68.87	42.85	64.36	67.11	66.83	55.89	69.74	60.87	62.07
	(25.00)	(25.49)	(21.92)	(23.32)	(22.15)	(25.40)	(23.38)	(22.19)	(25.08)
Prob enjoy job (age 30)	53.13	55.61	67.53	57.23	56.68	47.14	60.33	53.24	56.36
	(25.20)	(29.12)	(22.90)	(27.28)	(25.60)	(28.20)	(25.00)	(23.91)	(26.54)
Expected hrs/wk (age 30)	47.32	39.14	49.24	47.43	45.76	45.06	52.60	44.51	46.38
	(11.53)	(13.33)	(12.18)	(11.74)	(11.09)	(13.26)	(13.40)	(11.45)	(12.80)
Expected earnings at 30	26.29	24.47	37.14	35.71	34.16	27.86	32.85	28.49	30.87
	(5.744)	(8.005)	(11.14)	(10.64)	(10.29)	(8.623)	(11.09)	(8.232)	(10.36)
Expected earnings at 40	30.63	29.45	44.07	41.98	40.17	32.19	39.25	33.54	36.41
	(6.966)	(9.422)	(12.99)	(12.35)	(12.17)	(9.598)	(12.96)	(9.591)	(12.13)
Perceived status for degree	44.94	46.26	78.03	73.27	64.82	40.36	71.09	48.51	58.41
	(20.49)	(21.35)	(14.60)	(17.71)	(18.86)	(20.94)	(23.10)	(20.61)	(24.26)

Table A4: Treatment on the margin

	Stated (S=1)		Applied	
	(1)		(2)	
Panel A: Treatment on the treated and untreated				
β_{TT}	0.374***		0.396***	
	(0.0138)		(0.0146)	
β_{tut}	0.182*		0.273***	
	(0.0746)		(0.0295)	
N_1	475		394	
N_0	23		104	
	(3)	(4)	(5)	(6)
	S=1 Apply=0	S=1 Apply=1	Apply=1 Enroll=0	Apply=1 Enroll=1
Panel B: Separating TT into two margins				
β_{TT}	0.263***	0.400***	0.342***	0.405***
	(0.0346)	(0.0147)	(0.0438)	(0.0153)
N	89	386	58	336

Standard errors in parentheses. *** p<0.001

Note: Panel A repeats a portion of Table 6. N_1 is the sample size for $S = 1$, e.g. the treated individuals, likewise N_0 is the number of untreated individuals.

Online Appendix

Nikolay Angelov[1], Per Johansson[2], Mikael Lindahl[3], and Ariel Marek Pihl[4]

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August 27, 2021

A Survey Details

A.1 General Instructions

The survey began by going through each of the eight categories of university study (field) and asking students to pick their most preferred degree program from between 3 and 10 options (see next section). After the students had a particular program in mind, the survey on subjective expectations began. General instructions for how to answer the questions were repeated before each question they were relevant for. To save space, we are only including them once. All questions and information were provided in Swedish, this is a translation.

- All questions:
  - When answering the question below, try to consider the (possibly) hypothetical situation where you have studied in this particular field. In other words, if you have not considered pursuing this field or are not planning to study at university, you should try to imagine what it would be like if you ended up in this major, or were forced to choose it.
- Any question where the answer is in terms of probability:
  - For some questions, you need to specify the probability in % that something happens. The probability in % is a number that must be between 0 and 100. For example, a probability of 2 or 5% means an almost non-existent chance of something occurring, 19% means that the chance is quite small, 47 or 54% means that it is about as likely that something occurs as that it does not occur, 80% means the chance is quite large, and 95 or 98% means that the event will almost certainly happen.
- Questions 14-18:

- Look forward to when you are 30 (40) YEARS OLD. Think about what kind of work will be available to you and that you will accept if you have successfully obtained a degree in any of the following education fields.
- NOTE that you can get certain types of employment no matter what program you have completed. For example, you can work as a property manager regardless of subject matter. On the other hand, you cannot find work as a doctor if you have studied architecture.
- Your answers below should take into account any further education over the bachelor level that you think you would achieve after a degree in each field.

A.2 Survey Questions

1-8. PROGRAM CHOICE WITHIN FIELD If you chose to study [FIELD] in college, which of the following options would you most likely choose? (*See following section for the full list of options for each field category.*)

9. PASSING What is the probability in % that you would be able to pass a degree in the respective educational field within the normal study time? Think about the subject you chose in each respective field. Try to imagine the amount and difficulty of the lectures, course literature, exams, assignments and other elements that you think would be required.

10. COURSE WORK What is the likelihood in % that you would enjoy course work related to the program? Enjoying the course work means that lectures, course literature, exams, laboratory sessions, assignments, or other elements that you think would be required would be quite stimulating and exciting.

11. STUDY TIME (HOURS PER WEEK) How many hours per week do you think you would need to study for each education? Try to imagine the amount and difficulty of the lectures, course literature, exams, laboratory sessions, assignments or other elements that you think exist in the respective education.

12. WHAT WOULD YOUR FAMILY THINK? If you were to study the respective field, what is the likelihood in % that your parents and other family members would approve of your choice of education? If you think the views are different, try to weigh the different opinions. Alternatively, you can specify what the most important person for your choice of education would think.

13. WILL YOU GET JOB AFTER THE EXAM? If you obtain a college degree in the respective field, how much is the probability in % that you find a job that you can accept immediately after graduation?

14. WILL YOU ENJOY YOUR JOB? Look forward to when you are 30 YEARS OLD. If you have a degree in this field and a job, what is the likelihood in % that you enjoy your job?

15. WORK AND PRIVATE LIFE Look forward to when you are 30 YEARS OLD. If you have a degree in this field and a job, what is the likelihood in % that you can combine your work and private/family life in a satisfying way?

16. WORKING HOURS AFTER GRADUATION (HOURS PER WEEK) Look forward to when you are 30 YEARS OLD. If you have a degree in this field, how many hours a week do you think you would need to work?

17. MONTHLY EARNINGS WHEN YOU ARE 30 YEARS OLD How much do you think you will earn per month when you are 30 YEARS OLD and have a degree in this field? Enter monthly salary in SEK before tax.

18. MONTHLY EARNINGS WHEN YOU ARE 40 YEARS OLD How much do you think you will earn per month when you are 40 YEARS OLD and have a degree in this field? Enter monthly salary in SEK before tax.

19. STATUS What social status do you think a degree in this field has? Try to completely ignore salary, and only think about the part of the education status that is NOT related to pay. To make it easier, you can imagine that you earn the same regardless of education. Enter Status as a number between 0 and 100, where 0 is lowest and 100 has the highest status.

20. FUTURE PLANS: Are you planning to apply for college after high school?
    Yes/No

21. CHOICE: What educational field do you intend to apply for?
    The eight college field options

22. RANKING: Imagine that you have to choose between the eight college fields and upper secondary school (i.e. no college education but only completing gymnasium). Sort these nine options by how likely it is that you choose each of them.
    Implemented through dragging and dropping the 9 options into the preferred order

23. NO COLLEGE: Look forward to when you are 30 YEARS OLD. Think about what kind of work will be available to you and which you can accept if you did not attend university. How much do you think you will earn in the month when you are 30 YEARS OLD and do not have a university education? Enter monthly salary in SEK before tax.

24. NO COLLEGE: Now look forward to when you are 40 YEARS OLD. Think about what kind of work will be available to you and you can accept if you did not attend university. How much do you think you will earn in the month when you are 40 YEARS OLD and do not have a university education? Enter monthly salary in SEK before tax.

25. ACTUAL AVERAGE WAGES: Are you interested in obtaining information about actual average wages for people with different types of university degrees? You do not have to answer any more questions.

B Potential Programs within Fields of Study

Choice set (potential programs within field listed from most to least popular):

1. Pedagogy and teacher education
   - Teacher training: grades 7-12
   - Pedagogy and didactics
   - Teacher training: vocational subjects
   - Teacher training: elementary
   - Teacher training: preschool
2. Humanities and arts
   - Media Production
   - History and Archeology
   - Fine arts and design
   - Music, dance or drama
   - Foreign Language
   - Swedish Literature
   - Religion
3. Social science, law, trade, administration
   - Psychology
   - Business, Commerce, Administration
   - Law and Jurisprudence
   - Journalism and Media Studies
   - Economics
   - Political science
   - Sociology, Ethnology and Cultural Geography
   - Library studies
4. Sciences, mathematics and data
   - Biology, Biochemistry
   - Computer science and systems science
   - Mathematics and statistics
   - Physics
   - Environmental Sciences, Geosciences and Natural Geography
   - Chemistry
5. Technology and Manufacturing
   - Civil engineering and building engineering (e.g. architectural education or civil engineer in civil engineering, civil engineering, construction)
   - Engineering and engineering industry (e.g. engineering engineer, energy, electronics, chemistry, automotive engineering, industrial economics)
   - Materials and manufacturing (e.g. civil engineer in food, textile technology, materials technology, wood, paper, rock and mineral technology)
6. Agriculture, forestry and animal health care
   - Veterinary Care

- Agriculture and Forestry
- Fishing and Aquaculture

7. Healthcare and Social Care
   - Doctor Training
   - Social work
   - Therapy, rehabilitation and nutrition
   - Technically oriented health education
   - Nursing
   - Dental School
   - Pharmacy
   - Dental hygienists
8. Services
   - Tourism, travel and leisure
   - Security in society (e.g. police, fire protection, rescue services)
   - Sport and wellness
   - Military training
   - Hotel, restaurant and large household
   - Transport services (e.g. sea captain, pilot, flight attendant)
   - Work environment and occupational training (e.g. ergonomics)

C Background Variables

C.1 Ability Variables

Surveyed students have been matched to their high school records, both course grades and final test grades (these are a component of course grade). Both course grades and test grades are given by the students' teachers, however we expect test score grades to be less biased by the relationship between student and teacher. We use only grades given before the survey was administered (2011-2013). For various reasons (transferring from one type of program to another, in-migration, absence on test day) students are not always matched to test scores. Thus conditioning on test scores reduces our sample size considerably.

During this period students are graded on a 0-20 point scale, where 0 is an F (fail), 10 is an E (lowest pass), and 20 is an A.

English Ability: All high school students in Sweden are required to take English courses. The two courses that they typically completed in high school before the time of the survey are English 4 and English 5. Of the 498 students who are matched to their administrative data, some are missing one of the two courses. To maximize the sample with a valid english ability measure, we use the average of Eng4 and Eng5 for students with both, or the value for one exam if the other is not available. This yields a sample of 438 (88% of the overall sample).

Math Ability: All college-track students in Sweden should take one of two Mathematics tracks. Students in high school programs with a math/engineering focus take Math C, and students in other types of academic programs take Math B. Grades for the less intensive Math B course are lower on average than those for Math C. The data also contains a few individuals who have grades for Math A, which should be only for non-academic high school programs (not college-prep). We think this Math track is too different to compare and do not include it. To create a measure of Math ability that is valid for the most possible students, we use the mean math grade earned.[1]

C.2 Family Variables

Surveyed students are matched to information on their mother and father in 2013 national register data.[2] Of the 498 students for whom we have surveys, 486 are matched to mother's information and 472 are matched to father's information. Failed matches are a combination of two things: no information on who the parent is, and the parent not appearing in 2013 data. Of the 13 students not matched to mother characteristics, 9 have a mother's personal number. These mothers may be deceased or emigrated from Sweden.

Of the 26 students not matched to father characteristics, 10 have a father's personal number. The remaining 16 probably have not been legally connected to their biological father.

Parent Education: We create years of education for each parent using *SUN2000Niva* 2-digit codes. The lowest category is "below 9 years of education", and we code it as 7 years. The remaining categories have close correspondence with number of years of academic education (9-20 years).

Parent Income: We use *ForvErs*, which captures all employment and business related income, including things like parental leave and unemployment benefits. This is annual income in 2013 for the individual parent.

Parent Field of Study: We use the first digit of *SUN2000Inr* codes to categorize the parents' education into the same nine fields of education as the students chose between. There is also an "other" category for parents.

C.3 Socio-Economic Status

In order to compare more advantaged to less advantaged students, we combine multiple aspects of family background into a single variable to proxy for socio-economic status (SES). We use the Principle Component Analysis Index in the paper, but compare it to a simple index below for completeness.

Principle Component Analysis: If we think of SES as a latent variable which is correlated with things like income, education and family composition, we can use principle components to measure SES. To maximize the sample we run the PCA estimation on variables which

[1]Using max score achieved yields a measure that is 94.7% correlated.

[2]This is primarily biological parents, but the small number of adopted children are matched to their adoptive parents.

have been altered to replace missing parent information. For parent education, we use the mean for fathers and mothers. For parent income, we replace missing with zeros (since the child should have no access to income from this parent). To adjust for these modifications the PCA also includes indicator variables to signify each missing parent. Education and income load positively, with father's variables receiving more weight than mothers, and education more weight than income. Parent missing loads negatively (having a parent missing decreases SES). 42% of the variance in the six variables is explained by the first component we use to stratify individuals.

Simple Index: To combine the information contained in family variables, we standardize each parent's education and income to have mean 0 and standard deviation 1. Then we average them, along with an indicator for having both parents present in the data, allowing for missing information to be ignored in the average. This yields an SES variable for any student who has any information about either parent, where the only penalty for missing parents is through the indicator for both parents present.

Table 1: First Component of SES PCA

Variable	Component 1 Eigenvector
Mother's Years of Ed	0.4439
Father's Years of Ed	0.5003
Mother's Income	0.379
Father's Income	0.3888
Farther Missing	-0.4037
Mother Missing	-0.308
Explained Variance	0.42

Comparison: These two measures are 96% correlated, and only 3 individuals are above median in one measure and below median in the other.

Appendix D: Survey design and implementation

For the purpose of this project, we have collected survey data on a sample of high school students in the municipality of Stockholm. To be part of our population, the students must have attended the third year of a municipal high school in 2014 and lived in the municipality of Stockholm. Although the fraction of independent (non-municipal) high school students is high in Stockholm, the majority of the students in academic programs attend municipality schools. The municipality of Stockholm includes many suburbs, some well-off and some much less so.

A concern with eliciting preferences from survey data is that the result may differ from what would be found in real-world situations. As our study design is quite similar to a stated preference experiment, advice from the stated preference literature was used when designing the survey.[1] In the stated preference experiment literature it is emphasized that the experiments should be preceded by interviews, focus groups and pre-tests, that the respondents' incentives to answer truthfully should be thoroughly analyzed, and that the questions should be relevant or realistic for the respondents. In the spring of 2013, we piloted the survey in four iterations in different high schools for a total of 53 students. At the end of each pilot round, informal focus groups were held with the respondents, which enabled us to improve marginally on the next survey. These focus groups reassure us that the final version of the survey contained questions that were easy to understand and that the students felt they had the preparation and sufficient information to answer them.

In general terms, one of our most important guidelines when designing the survey was to put the students in a choice situation which is as close as possible to a real-life choice of university education. While this is obviously not entirely possible, we took several steps to ensure that the survey mimics as much as possible the optimizing behavior of well-informed rational agents. For instance, the ordering and wording of the questions was chosen such that the students were put in

[1] A number of methods for reducing this bias have been suggested in the stated preference literature (cf. List, 2001, Murphy *et al.*, 2004, Carson, 2012, and Kling *et al.*, 2012), and several recent studies show that stated and revealed preferences often coincide (cf. Murphy *et al.,* 2010, and Jacquemet *et al.*, 2011). The results in these studies suggest that the importance of hypothetical bias depends on the experimental setting (cf. Taylor *et al*., 2001, and Ajzen *et al*., 2004). As is emphasized in the recent survey by Kling *et al.* (2012), there is a 'current best practice for survey design'.

a choice situation that approximates the real-life choice of higher education that many of them would face about a month ahead. Specifically, to enable the students to be able to answer questions about their future wages under different counterfactual education majors, we asked them first to state which particular education they would choose if they chose humanities, social sciences, etc.

In order for the respondents to be as well-informed as possible, we focused on the students during their last year in high school when many are to apply for college. As we mention in Section 2.2, survey timing was chosen carefully to be before the university applications closed, but late enough so that they had likely put considerable thought into their educational path. This should limit the issue of cognitive dissonance/ex-post rationalization, where students provide biased responses because their field of study selection is already set in stone.

In order to elicit reliable expectations and beliefs on counterfactual outcomes, we wanted to have an expert interviewer present during the interview, something that is quite costly. At the same time, it was desirable to have a large sample to increase the precision of the estimates. This is especially important since the subjective expectations data sets used in many previous papers are based on small and narrow samples. A professional survey company (SKOP) was hired to contact and conduct interviews with students, after we provided them with a list of the names and addresses from the Stockholm municipality authorities (N=3,368).

Of the 3,368 individuals in their final year of secondary school, SKOP was able to match 1682 to phone numbers. 66 had stopped studying, finished school or left Stockholm, 258 stated they did not want to participate, 791 never responded to contact and 62 scheduled a time for the survey, but didn't follow through. SKOP completed 505 surveys with students, and a total of 498 of these where successfully matched to administrative data and comprise our primary sample. This final sample is not representative of the starting population, nor is Stockholm representative of Sweden as a whole. This is clear in Appendix Table A1, where we see that those who responded to the survey are less foreign, come from higher SES backgrounds and have generally higher test scores than the starting population. However, as samples of convenience are standard in this literature (typically students from a particular selective US college), we think that our sample (with students from >40 high schools who ultimately apply to dozens of educational institutions) still provides a much more diverse sample.

The questionnaires were answered in person using computer-assisted personal interviewing through home visits and meetings in cafes or similar locations. A full translation of survey questions is provided in Appendix A.

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Appendix E: Estimating treatment effects for marginal students

E.1 Treatment effects

The parameters TT and TUT in Equations (2) and (3) provide effects of treatments for two broad populations defined by treatment status. Estimates of such treatment effects are useful for policymakers as guidance regarding effects of cancelling existing educational programs or making them compulsory. However, they can be misleading if we were to interpret such estimates as representing effects for marginal students, for instance if policy makers would like to change the scale of existing programs, or infer what happens to the demand for program slots with a general increase in the returns to the program (Björklund and Moffitt, 1987; Heckman and Vytlacil, 2007). Estimating such parameters using observational data can be challenging. Here we use our data on potential outcomes, in combination with stated and revealed educational choices, to estimate the treatment effect for those closest to the margin of taking up treatment. This average treatment effect is of policy interest, as this is a treatment effect for those individuals most likely to react to policies affecting incentives to enroll. It is related to the treatment effects for individuals at the margin of participation (the effect of treatment for people at the margin of indifference, or EOTM; see Heckman and Vytlacil, 2007).

To formulate an estimable version of this parameter we separate the treatment on the treated effect for those that applied into those that eventually did enroll and those that chose not to enroll, where the latter group is likely closer to enrolling than those included in the treatment on the untreated group which already at the survey stated that they would not choose university. This exercise is essentially comparing treatment effects between sub-groups with discretely different enrollment/application propensities, and we expect that they will differ somewhat on both observables and unobservables. Specifically, we estimate:

$$TUT_{enrol|appl} = E\left[y_{1i} - y_{0i} | S_{applied} = 1, S_{enrolled} = 0\right] \qquad \text{(A1)}[1]$$

[1] Note that $TT_{applied} = E\left[y_{1i} - y_{0i} | S_{applied} = 1\right] = (1-w) \cdot E\left[y_{1i} - y_{0i} | S_{applied} = 1, S_{enrolled} = 0\right] + w \cdot E\left[y_{1i} - y_{0i} | S_{applied} = 1, S_{enrolled} = 1\right] = (1-w) \cdot TUT_{enrol|applied} + w \cdot TT_{enrol|applied}$ where w is the fraction choosing to enroll of the population of applicants.

which then can be compared to the average treatment effect for those enrolled, i.e., $E[y_{1i} - y_{0i}|S_{enrolled} = 1]$ and the average treatment effect for those not enrolled, i.e., $TUT = E[y_{1i} - y_{0i}|S_{enrolled} = 0]$. Similarly, we estimate

$$TUT_{appl|chose} = E\left[y_{1i} - y_{0i}|S_{chose} = 1, S_{applied} = 0\right] \quad \text{(A2)}$$

which is the average treatment effect on the untreated (the non-applicants) among the pool that stated in the survey that they would choose S=1.[2] This sub-group is likely closer to the group of applicants than the untreated group that already in the survey stated that they would not choose university.

E. 2 Estimates

To provide estimates of equations (A1) and (A2) we use that we have data on three different levels of choice, and estimate treatment effects for subsamples of these individuals. We split up the sample of those that applied to college into those that did enroll and did not enroll, as well as split up the sample of those that stated that they would choose university, into those that applied and those that did not apply.

Results are shown in Appendix Table A4. Panel A repeats the TT and TUT for applied and enrolled from Table 6 for reference. In Panel B, we split up these into the two groups. We see that those stated that they would apply, but then ultimately did not have lower returns than those that did apply, 0.26 versus 0.40, and that those that applied, bud did not enroll have lower returns than those that did enroll, 0.34 versus 0.40. This suggests that even though those who opted out have lower returns, the returns are still positive. If we compare these returns to those estimated for the untreated groups (in the second row of Panel A), we see that the treatment effects on the untreated are estimated to be 0.18 for those that did not state college, and 0.27 for those that did not apply, which are lower than the 0.26 and 0.34 estimated here. Hence, although the estimates are not very different, results suggest that incentivizing high school students to go to college will likely require a higher return than what is estimated from using treated on the untreated estimates.[3]

[2] An estimate for this group can be compared with estimate the average treatment effect for those applied, i.e., $TT_{appl|chose} = E\left[y_{1i} - y_{0i}|S_{chose} = 1, S_{applied} = 1\right] = E\left[y_{1i} - y_{0i}|S_{applied} = 1\right] = TT_{appl}$.

[3] We also used all the survey responses regarding respondents' choices and ranks of high school and fields of study choice and predict the probability to enroll at university. We then estimate the average treatment effect for

Since, individuals sort based on their comparative advantage, CA may differ in subpopulations where the propensity to choose enrollment (or ability) is more similar. We can back out CA in enrollment for the subpopulation who does apply to college (removing the less similar non-appliers) from the numbers in columns 5-6 of Appendix Table A4. The estimate then becomes $CA_{appl} = TT_{enrol|appl} - TUT_{enrol|appl}$ = .405-.342=0.063, which is somewhat smaller than the 0.083 in Table 6, but not significantly so. [4]

each predicted treatment probability. We illustrate these in Online Appendix Figure A2, where we have related the individual returns to this predicted propensity to enroll. The relationship at the bottom 0.40 of the probabilities should be interpreted with care, as these are based on few observations. However, we see that the returns vary from around 0.30 up to 0.38 for those with the highest treatment probabilities. This positive relationship confirms the pattern shown in Appendix Figure A4 and discussed above.

[4] We have also calculated this separately for those above and below the median math score, and those with above and below median SES and found little difference.