Field of Study, Earnings, and Self-Selection

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Abstract: Why do individuals choose different types of post-secondary education, and what are the labor market consequences of those choices? We show that answering these questions is difficult because individuals choose between several unordered alternatives. Even with a valid instrument for every type of education, instrumental variables estimation of the payoffs require information about individuals' ranking of education types or strong additional assumptions, like constant effects or restrictive preferences. These identification results motivate and guide our empirical analysis of the choice of and payoff to field of study. Our context is Norway's post-secondary education system where a centralized admission process covers almost all universities and colleges. This process creates credible instruments from discontinuities which effectively randomize applicants near unpredictable admission cutoffs into different fields of study. At the same time, it provides us with strategy-proof measures of individuals' ranking of fields. Taken together, this allows us to estimate the payoffs to different fields while correcting for selection bias and keeping the next-best alternatives as measured at the time of application fixed. We find that different fields have widely different payoffs, even after accounting for institutional differences and quality of peer groups. For many fields the payoffs rival the college wage premiums, suggesting the choice of field is potentially as important as the decision to enroll in college. The estimated payoffs are consistent with individuals choosing fields in which they have comparative advantage. We also test and reject assumptions of constant effects or restrictive preferences, suggesting that information on next-best alternatives is essential to identify payoffs to field of study.

Keywords: Field of study, earnings, self-selection, treatment effects; unordered choice

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1 Introduction

According to OECD data, the majority of young adults in developed countries enroll in post-secondary education. One of the decisions that virtually all these students have to make is to choose a field of study or college major.[1] The field of study choice is potentially as important as the decision to enroll in college, since the earnings differences we observe across fields rival college earnings premiums. Yet, there is little evidence on why individuals choose different fields of study, and the labor market consequences of those choices. Altonji et al. (2012) review the literature and conclude that "there is a long way to go on the road to credible measures of the payoffs to fields of study".

In this paper, we investigate why individuals choose different fields of study, and the payoffs to those choices. We begin by showing that answering these questions is difficult because individuals choose between several unordered alternatives. We show that even with a valid instrument for each field of study, instrumental variables (IV) estimation of the payoffs requires information about individuals' ranking of fields or strong additional assumptions, like constant effects or restrictions on preferences. Otherwise, IV does not identify the payoff to any individual or group of the population from choosing one field of study as compared to another.

These identification results motivate and guide our empirical analysis of the choice of and payoff to field of study. In particular, we use instruments to correct for selection bias and measures of next-best alternatives to approximate individuals' margin of choice. Taken together, this allows us identify the payoff to a chosen field relative to a particular next-best alternative, without assuming constant effects or making strong preference restrictions. For example, we are able to examine whether the gains in earnings to persons choosing Science instead of Teaching are larger or smaller than the gains in earnings to those choosing Business instead of Teaching.

The information on next-best alternatives also allows us to examine the pattern of sorting to fields. For example, a random member of the population might achieve a

[1] In most OECD countries, students typically enroll in a specific field of study upon entry to a university. In the United States, however, students only specialize in a major during the last year(s) of college.

negative payoff from a Science degree, yet those with appropriate talents who chose Science might obtain a positive payoff. We use our estimates to assess whether individuals tend to choose fields in which they have comparative advantage. In contrast to much of the existing literature on education and self-selection, we do not make strong assumptions about selection criterion, information sets or the distribution of unobservables.

The context for our analysis is the Norwegian post-secondary education system. For several reasons, Norway provides an attractive context for this study. It satisfies the requirement for a large and detailed data set that follows every student through the layers of the education system and into their working career.[2] It also has a centralized admission process that covers almost all universities and colleges. Norwegian students apply to a field and institution simultaneously (e.g. Teaching at the University of Oslo). In their application, they can rank up to fifteen choices. The applicants are scored by a central organization based on their high school GPA. Applicants are then ranked by their application score after which places are assigned in turn: The best ranked applicant gets her preferred choice; the next ranked applicant gets the highest available choice for which she qualifies, and so on. This process creates credible instruments from discontinuities which effectively randomize applicants near unpredictable admission cutoffs into different fields of study. At the same time, it provides us with strategy-proof measures of individuals' ranking of fields.

Our empirical findings may be summarized with three broad conclusions. First, different fields have widely different payoffs, even after accounting for institutional differences and quality of peer groups. For example, by choosing Science instead of Humanities, individuals almost triple their earnings early in their working career. By comparison, choosing Science instead of Engineering or Business has little payoff. Second, individuals tend to choose fields in which they have comparative advantage. Third, we reject assumptions of constant effects or restrictive preferences, suggesting that information on next-best alternatives is essentially to identify payoffs to field of study.

[2] We therefore avoid the problem of non-response bias in previous studies relying on survey data. Hamermesh and Donald (2008) show that non-response bias can lead to misleading conclusions about the payoffs to post-secondary education.

Taken together, our findings can inform ongoing debates over government intervention to address apparent mismatches and market frictions in the supply and demand for post-secondary field of study. For example, the U.S. President's Council of Advisors on Science and Technology (2012) is the latest in a series of reports that call for education reforms to increase the number of college graduates in STEM fields. Using our estimates, we simulate the effects on earnings from a policy that lowers the admission cutoffs to Science education, a change which could be achieved by increasing the number of slots to this field. There are two components to the total change in earnings from the policy change: A direct effect on individuals who are shifted into Science, and an indirect effect as slots are freed up in other fields. Our simulation makes clear that the effect of a policy that changes the field people choose depends inherently on the next-best alternatives, both directly through the payoffs and indirectly through the fields in which slots are freed up. Without information on next-best alternatives, it is difficult to predict if the effects of a policy that increases the number of graduates in STEM fields will be large or small, positive or negative.[3]

Our paper is primarily related to a small but growing literature on the payoffs to different types of post-secondary education, reviewed in Deming et al. (2012) and Altonji et al. (2012). To date, most studies perform OLS estimation, and thus assume that all selection is on observables. The two papers most closely related to our study both use Chilean data. Hastings et al. (2013b) make important progress over previous research by addressing selection on unobservables. Hastings et al. use discontinuities from a centralized, score-based admissions system to estimate the earnings effects of crossing the threshold for admission to a preferred institution-field (called degree) relative to a weighted average of next-best degrees.[4] Assuming that heterogeneity in treatment effects

[3]For the same reasons, predictions about externalities or social returns to field of study will depend on the next-best alternative. For example, some studies suggests lower social returns to high-paying professions (e.g. business) than low-paying professions (e.g., teaching). See e.g. Lockwood et al. (2014).

[4]Discontinuities in admission thresholds have also been used in other contexts than field of study, such as the effect of admission to particular institutions (e.g. Saavedra, 2008; Hoekstra, 2009; Zimmerman, 2014), the impact of another year of college (Öckert, 2010), the marriage market consequences of admission to higher ranked university (Kaufmann et al., 2013), the effect of admission to higher quality primary and secondary schools (see e.g. Jackson, 2010; Duflo et al., 2011; Abdulkadiroglu et al., 2012; Pop-Eleches and Urquiola, 2013), and the consequences of affirmative action in engineering colleges in India (Bertrand et al., 2010).

only depends on observable characteristics, they also estimate the impacts of crossing the admission cutoff to a particular degree relative to not being admitted to any university. By comparison, Reyes et al. (2013) estimate a parametric model of post-secondary schooling choice and examine the distribution of payoffs to different degrees according to years of study and private versus public institution. Their estimates point to the importance of allowing for unobserved heterogeneity in effects when analyzing the payoffs to different post-secondary degrees.[5]

We complement the literature on the payoffs to post-secondary education in several ways. First and foremost, we provide evidence on the payoff to a chosen field relative to a particular next-best alternative, without assuming constant effects or making strong assumptions about preferences or the distribution of unobservables. Our approach allows us to estimate the payoffs to different fields while correcting for selection bias and keeping the next-best alternatives as measured at the time of application fixed. Second, we examine heterogeneity in the levels of potential earnings by field of study. Not only does this help in interpreting the magnitude of the estimated payoffs, it also allows us to quantify the role of next-best alternatives in explaining earnings differences among the high educated. Third, the admission system we study creates exogenous variation in both field and institution choice, which helps interpret the estimated payoffs. Fourth, because we can track individuals through each step of the education system, we are able to estimate the impact of completing a field of study rather than the intention-to-treat effect of crossing the admission cutoff to a field. This is potentially important as completion rates are sometimes low and vary systematically across fields, which complicates interpretation of intention-to-treat estimates.[6]

Our paper is also related to a literature on the sorting pattern of individuals to post-secondary education. Our findings of selection on comparative advantage are consistent with previous work that use observational data to study how individuals select into college

[5]See also Arcidiacono (2004) who estimated a dynamic model of college and major choice. His estimates suggest that large earnings premiums exist for certain majors.

[6]See Altonji (1993) for a discussion of the ex ante return associated with starting a particular major, which includes the probability of dropping out entirely and switching majors, and the ex-post return to the completed major.

(see e.g. Willis and Rosen, 1979; Carneiro et al., 2011). To date, most of what we know about why individuals choose different fields of study comes from surveys or informational experiments. The evidence is mixed. Some studies suggest that students often base educational choices on limited or inaccurate information on labor market returns.[7] Others suggest that students' subjective expectations of earnings and self-assessed abilities are key determinants of educational choices.[8] We find that in naturally occurring data, students tend to act as if they possess knowledge of idiosyncratic earnings gains when choosing field of study. Our findings also highlight a challenge to interpreting the results from surveys or informational experiments: The earnings observed in each field will generally be non-random samples of population potential earnings, and therefore have no significance as guides to the social or private profitability of field choices.

Finally, our paper builds and extends on a literature on identification of treatment effects in unordered choice models. Heckman et al. (2006) and Heckman and Urzua (2010) discuss the challenges to identification and interpretation of treatment effects in such models. They show that individuals induced into a state by a change in an instrument may come from many alternative states, so there are many margins of choice. They conclude that structural models can identify the earnings gains arising from these separate margins,[9] while this is a difficult task for IV without invoking strong assumptions. In this paper, we make precise what IV can and cannot identify when there are multiple, unordered treatments. We find that information on next-best alternatives is essential to identify treatment effects in such settings, and we reject the alternatives of assuming constant effects or imposing strong restrictions on preferences.

While our empirical findings are specific to the context of post-secondary education, there could be lessons for other settings. Examples can be found in observational studies that use IV to study workers' selection of occupation, firms' decision on location, or fami-

[7]See e.g. Hastings et al. (2013a), Betts (1996), Wiswall and Zafar (2014), and Reuben et al. (2013).

[8]See e.g. Arcidiacono et al. (2012), Attanasio and Kaufmann (2009), and Stinebrickner and Stinebrickner (2014). See also Carneiro et al. (2003) who show that uncertainty coming from forecast errors seem to have little effect on schooling choices.

[9]For example, Kline and Walters (2014) estimate a semi-parametric selection model to learn about the effects of Head Start as compared to no preschool or competing preschool programs. See also Dahl (2002) who develop a semi-parametric method to study migration across U.S. states.

lies' choice of where to live. Our study highlights key challenges and possible solutions to understanding why agents choose different alternatives and what the causal effects of these choices are. Another example is the frequent use of encouragement design in evaluation studies, where programs are made available but take up is not universal (see e.g. Duflo et al., 2008). Researchers then use OLS and IV to estimate intention-to-treat and local average treatment effects (LATE) parameters, respectively. We show what assumptions and information that are required to draw causal inference from encouragement designs in settings with multiple, unordered treatments.

The remainder of the paper is organized as follows. Section 2 discusses identification of payoffs to field of study. In Section 3, we describe the admission process to post-secondary education in Norway. Section 4 describes our data and presents descriptive statistics. Section 5 provides a graphical depiction of our research design, before Section 6 turns to the formal econometric model. Section 7 describes our main findings on payoffs to field of study, explores possible mechanisms, and reports results from specification checks. In Section 8, we test assumptions of constant effects and restrictive preferences, and quantify the role of next-best alternatives in explaining the variation in payoffs across fields. Section 9 uses our estimates to simulate the impact on earnings from a policy which increases the supply of slots to Science. In Section 10, we explore the pattern of selection to fields. The final section offers some concluding remarks.

2 Identifying payoffs to field of study

2.1 Regression model, potential earnings, and field choices

To formalize ideas, consider the case in which students choose between three fields, $d \in \{0, 1, 2\}$. For notational simplicity we suppress the individual index, and also abstract from any control variables. Our interest is centered on how to interpret OLS and IV estimates of the following equation:

$$y = \beta_0 + \beta_1 d_1 + \beta_2 d_2 + \epsilon \tag{1}$$

where y is observed earnings, and $d_j \equiv \mathbb{1}_{[d=j]}$ is an indicator variable that equals one if an individual completed field j and zero otherwise.

Suppose that individuals are assigned to one of three groups, $Z \in \{0, 1, 2\}$, and let $z_j \equiv \mathbb{1}_{[Z=j]}$ be an indicator variable that equals one if an individual is assigned to group j and zero otherwise. One can think of Z as a multi-valued instrument that shifts the relative cost or benefits of choosing different fields. For each individual, this gives three potential field choices, d^z, and nine potential earnings levels, $y^{d,z}$.

Throughout the paper, we make the standard IV assumptions:

Assumption 1. (Exclusion): $y^{d,z} = y^d$ *for all* d, z

Assumption 2. (Independence): $y^d, d^z \perp Z$ *for all* d, z

Assumption 3. (Rank): $E[\mathbf{z}'\mathbf{d}]$ *has full rank.*

Note that we do not restrict the heterogeneity in the payoffs to field of study: For a given individual, the payoff may vary depending on the fields being compared (e.g. $y^1 - y^0$ differs from $y^2 - y^0$); and for a given pair of fields, the payoff may vary across individuals (e.g. $y^1 - y^0$ differs between individuals).

We link observed and potential earnings and field choices as follows:

$$y = y^0 + (y^1 - y^0)d_1 + (y^2 - y^0)d_2 \tag{2}$$

$$d_1 = d_1^0 + (d_1^1 - d_1^0)z_1 + (d_1^2 - d_1^0)z_2 \tag{3}$$

$$d_2 = d_2^0 + (d_2^1 - d_2^0)z_1 + (d_2^2 - d_2^0)z_2 \tag{4}$$

where $d_j^z \equiv \mathbb{1}_{[d^z=j]}$ is an indicator variable that tells us whether an individual would choose field j for a given value of Z. For example, d_j^0 gives the status of field of study choice j when $Z = 0$ ($z_1 = 0$ and $z_2 = 0$), d_j^1 is the status when $Z = 1$ ($z_1 = 1$ and $z_2 = 0$), and d_j^2 is the status when $Z = 2$ ($z_1 = 0$ and $z_2 = 1$).

As in the usual LATE framework with a binary treatment (see Imbens and Angrist, 1994), we assume that switching on z_j does not make it less likely an individual chooses field j:

Assumption 4. (Monotonicity): *$d_1^1 \geq d_1^0$ and $d_2^2 \geq d_2^0$*

Note that Assumption 4 puts no restrictions on the possibility that z_j affects the costs or benefits of field k relative to field l (l, $k \neq j$). For example, it is silent about whether an individual's choice between field 2 and 0 is affected by whether z_1 is switched on or off.

Because there are many fields, the data demands for IV are high: For each field it is necessary to find a variable that is conditionally random, shifts the probability of choosing that field relative to the other options, and does not directly affect y. As a result, most of the research to date uses OLS to estimate the payoffs to field of study.[10] We therefore begin with a brief discussion of how to interpret OLS estimates of equation (1) before turning to what IV can and cannot identify.

2.2 OLS estimation of payoffs to field of study

In equation (1), the OLS estimate of the payoff from choosing, say, field 2 instead of 0 is the sample analogue of $E[y|d = 2] - E[y|d = 0]$. As usual, we can write the OLS estimand of β_2 in terms of potential outcomes as follows:

$$E[y|d = 2] - E[y|d = 0] = \underbrace{E[\Delta^2|d = 2]}_{\text{Payoff}} + \underbrace{E[y^0|d = 2] - E[y^0|d = 0]}_{\text{Selection Bias}} \tag{5}$$

where $\Delta^2 \equiv y^2 - y^0$ is the individual level payoff to completing field 2 instead of field 0, and $E[\Delta^2|d = 2]$ is the average payoff for those who completed field 2 instead of 0.

The first key challenge to estimate payoffs to fields of study is to correct for selection bias, $E[y^0|d = 2] \neq E[y^0|d = 0]$. Early and ongoing research adds many observable characteristics to equation (1), hoping that any remaining bias is small. Dale and Krueger (2002), Black and Smith (2004), Lindahl and Regner (2005), Hamermesh and Donald (2008), and Dale and Krueger (2011) show the difficulty in drawing causal inferences about the payoffs to post-secondary education from observational data.

The second key challenge is that individuals who choose the same field may differ in their next-best alternatives while researchers usually only observe the chosen field. Let

[10] See Altonji et al. (2012) and the references therein.

$d_{/j}$ denote an individual's next-best alternative, namely the field that would have been chosen if j is removed from the choice set. Expanding the first term on the right-hand side of (5), we get:

$$\begin{aligned} E[\Delta^2|d=2] &= E[\Delta^2|d=2,\, d_{/2}=0]\,\Pr(d_{/2}=0|d=2) \\ &\quad +E[\Delta^2|d=2,\, d_{/2}=1]\,\Pr(d_{/2}=1|d=2) \end{aligned} \tag{6}$$

Equation (6) illustrates that even in the absence of selection bias, it is difficult to interpret the OLS estimate of β_2 because it is a weighted average of payoffs to choosing field 2 instead of 0 across persons with different next-best alternatives. The average payoffs across individuals with different next-best alternatives will differ (i.e. $E[\Delta^2|d=2,\, d_{/2}=0] \neq E[\Delta^2|d=2,\, d_{/2}=1]$), if Δ^2 varies across individuals and they base their ranking of fields, in part, on these idiosyncratic payoffs.

One limiting case that illustrates the difficulty in interpreting $E[\Delta^2|d=2]$ is when everybody who completed field 2 has field 1 as next-best alternative, so that $\Pr(d_{/2}=1|d=2)=1$. In this case, $E[\Delta^2|d=2]$ is the average payoff of choosing field 2 instead 0 for individuals for whom field 2 versus 1 is the relevant choice margin: $E[\Delta^2|d=2,\, d_{/2}=1]$. In more realistic cases, $E[\Delta^2|d=2]$ will be a weighted average of payoffs to choosing field 2 instead of 0 for individuals coming from separate margins: field 2 versus 1, and field 2 versus 0. The weights depend on the proportion of people at each margin, and are unobserved unless researchers have information on next-best alternatives.

Because individuals who choose different fields may differ in their next-best alternatives, it is also difficult to compare different payoffs. For example, it could be that the average payoff to field 2 over 0 is larger than the average payoff of field 1 over 0:

$$E[\Delta^2|d=2] > E[\Delta^1|d=1]$$

even when the opposite is true for individuals at the relevant choice margins:

$$E[\Delta^2|d=2,\, d_{/2}=0] < E[\Delta^1|d=1,\, d_{/1}=0].$$

This can happen because the weights on next-best alternatives may vary by chosen field.

More generally, OLS estimates of the payoffs to field of study can vary either because of selection bias, differences in potential earnings across fields, or differences in weights across the next-best alternatives.

2.3 IV estimation of payoffs to field of study

To address selection bias, it is sufficient to have instruments that satisfy Assumptions 1-4. However, it turns out that identifying economically interpretable parameters remains difficult, because there is no natural ordering of the alternative fields of study and researchers rarely observe the individual's next-best alternative. We now show that even with a valid instrument for each field, identification of payoffs to field of study require information about individuals' ranking of fields or strong additional assumptions, like constant effects or restrictive preferences.

2.3.1 What IV cannot identify

IV uses the following moment conditions

$$E[\epsilon z_1] = 0 \tag{7}$$

$$E[\epsilon z_2] = 0 \tag{8}$$

$$E[\epsilon] = 0 \tag{9}$$

which can be expressed in terms of potential outcomes and treatments by rewriting the residual of equation (1) in terms of (2)-(4) as follows:

$$\begin{aligned}
\epsilon &= (y^0 - \beta_0) + (\Delta^1 - \beta_1)d_1 + (\Delta^2 - \beta_2)d_2 \\
&= (y^0 - \beta_0) + (\Delta^1 - \beta_1)(d_1^0 + (d_1^1 - d_1^0)z_1 + (d_1^2 - d_1^0)z_2) \\
&\qquad + (\Delta^2 - \beta_2)(d_2^0 + (d_2^1 - d_2^0)z_1 + (d_2^2 - d_2^0)z_2)
\end{aligned} \tag{10}$$

After substituting this expression in (7)-(9) and using the independence assumption we obtain the following moment conditions, now in terms of potential outcomes and treatments:

$$E[(\Delta^1 - \beta_1)(d_1^1 - d_1^0) + (\Delta^2 - \beta_2)(d_2^1 - d_2^0)] = 0 \tag{11}$$

$$E[(\Delta^1 - \beta_1)(d_1^2 - d_1^0) + (\Delta^2 - \beta_2)(d_2^2 - d_2^0)] = 0 \tag{12}$$

Solving these two equations for β_1 and β_2 leads to Proposition 1.[11]

Proposition 1. *Suppose Assumptions 1-4 hold. From solving equations (11)-(12) for β_1 and β_2, it follows that β_j for j=1,2 is a linear combination of the following three payoffs:*

i) Δ^1: Payoff of field 1 compared to 0

ii) Δ^2: Payoff of field 2 compared to 0

iii) $\Delta^2 - \Delta^1 \equiv y^2 - y^1$: Payoff of field 2 compared to 1

Proof. The proof is given in Appendix A. □

Proposition 1 shows that without further restrictions, IV estimation of equation (1) does not identify the payoff to any individual or group of the population from choosing one field of study as compared to another. For example, it would not tell us whether the gains in earnings to persons choosing Engineering instead of Business are larger or smaller than the gains in earnings to those choosing Law instead of Business. It is possible that persons choosing Engineering gain while those choosing Law lose; IV under Assumptions 1-4 only identifies an weighted average of the payoffs to different fields, which could large or small, positive or negative.

2.3.2 What IV can identify

The basic problem with IV estimation of equation (1) is that individuals who are induced to choose, say, field 2 if z_2 is switched on may select either field 0 or field 1 if z_2 is switched off. The standard IV assumptions ensure that switching on z_2 shifts some individuals into field 2, but they say nothing about the fields these compliers are shifted away from. Auxiliary assumptions are therefore necessary to identify the payoff from choosing one field of study as compared to another. Proposition 2 makes precise what IV identifies

[11] After solving (11)-(12) for β_1 and β_2, the intercept β_0 is identified from (9).

under three alternative assumptions: (i) constant effects; (ii) restrictive preferences; and (iii) irrelevance and information on next-best alternatives.

Proposition 2. *Suppose Assumptions 1-4 hold. Solving equations (11)-(12) for β_1 and β_2, we observe the following results:*

(i) If Δ^1 and Δ^2 are common across all individuals (Constant effects), then

$$\begin{aligned}\beta_1 &= \Delta^1 \\ \beta_2 &= \Delta^2\end{aligned}$$

(ii) If $d_2^1 = d_2^0$ and $d_1^2 = d_1^0$ (Restrictive preferences), then

$$\begin{aligned}\beta_1 &= E[\Delta^1 | d_1^1 - d_1^0 = 1] \\ \beta_2 &= E[\Delta^2 | d_2^2 - d_2^0 = 1]\end{aligned}$$

(iii) If $d_1^1 = d_1^0 = 0 \Rightarrow d_2^1 = d_2^0$, $d_2^2 = d_2^0 = 0 \Rightarrow d_1^2 = d_1^0$ and we condition on $d_1^0 = d_2^0 = 0$ (Irrelevance & next-best alternative), then

$$\beta_1 = E[\Delta^1 | d_1^1 - d_1^0 = 1,\ d_2^0 = 0]$$

$$\beta_2 = E[\Delta^2 | d_2^2 - d_2^0 = 1,\ d_1^0 = 0]$$

Proof. The proofs are given in Appendix A. □

In (i), Δ^1 and Δ^2 are common across all individuals and IV estimation of equation (1) identifies the payoff to each field. This constant effect assumption is, however, at odds with a large body of evidence which suggests the effect of education is heterogeneous and individuals choose schooling levels based on their idiosyncratic individual returns (see e.g. Carneiro et al., 2011).

Instead of assuming constant effects, identification can be achieved by making restrictions on individuals' preferences. One possibility is to impose the assumption in (ii), which implies that changing z from 0 to 1 (2) does not affect whether or not an individual chooses treatment 2 (1). Behaghel et al. (2013) show that this assumption allows for a causal interpretation of IV estimates in situations with multiple unordered treatments, as in regression model (1). In many settings, however, it is difficult to justify this assumption as it imposes strong restrictions on preferences. For example, it implies that an individual who chooses field 2 if the cost of field 1 is low ($z = 1$) must also choose field 2 if the cost of field 0 is low ($z = 0$).

Another possibility is to combine information about individuals' next-best alternatives with weak assumptions about individuals' preferences. In (iii), we assume that if changing z from 0 to 1 (2) does not induce an individual to choose treatment 1 (2), then it does not make her choose treatment 2 (1) either. In our context, for example, this assumption means that if crossing the admission cutoff to field 1 does not make an individual choose field 1, it does not make her choose field 2 either. On its own, this irrelevance condition does not help in resolving the identification problem posed by heterogeneous effects under Assumptions 1-4. But together with information about individuals' next-best alternatives, it is sufficient to identify LATEs for every field. The intuition is straightforward: By conditioning on the next-best alternative, individuals who are induced to complete a field by a change in the instrument come from a particular alternative field.

2.4 Empirically addressing the challenges to identification

The identification result in part (iii) of Proposition 2 motivates and guides our empirical analysis of the payoffs to field of study below. The key to our research design is twofold: We use instruments to correct for selection bias, and measures of next-best alternatives to approximate individuals' margin of choice. As discussed in greater detail later, our data provides us with strategy-proof measures of individuals' ranking of fields. These measures are designed to elicit the applicants true ranking of fields at time of application. We use this information to condition on individuals' next-best alternatives in the IV estimation of a model like equation (1). As a result, we can estimate the payoffs to different fields while correcting for selection bias and keeping the next-best alternatives as measured at the time of application fixed. We also test (and reject) the alternative auxiliary assumptions of constant effects or restrictive preferences in (i) and (ii).

3 Institutional details and identification strategy

In this section, we describe the admission process to post-secondary education in Norway, documenting in particular that the process generates instruments which can be used to correct for selection bias, as well as information about individual's next-best alternatives that allows us to approximate individuals' choice margin.

3.1 Admission process

During the period we study, the Norwegian post-secondary education sector consisted of eight universities and 25 university colleges, all of which are funded and regulated by the Ministry of Education and Research. A post-secondary degree normally lasts 3-5 years. The four main universities (in Bergen, Oslo, Trondheim and Tromsø) all offer a wide selection of fields. By comparison, the university colleges rarely offer fields like Law, Medicine, Science, or Technology, but tend to offer professional degrees in fields like Engineering, Health, Business, and Teaching. There are generally no tuition fees for attending post-secondary education in Norway, and most students are eligible for financial support (part loan/part grant) from the Norwegian State Educational Loan Fund.

The admission process to post-secondary education is centralized. Applications are submitted to a central organization, the Norwegian Universities and Colleges Admission Service, which handles the admission process to virtually all universities and colleges. Students apply to a field and institution simultaneously (e.g. Teaching at the University of Oslo). The unit in the application process (course) is the combination of detailed field and institution.

Every year in the late fall, the Ministry of Education and Research decides on funding to each field at every institution, which effectively determines the supply of slots. While some slots are reserved for special quotas (e.g. students from northernmost part of Norway), the bulk of the slots are for the main pool of applicants. For many courses, demand exceeds supply. Courses for which there is excess demand are filled based on an application score derived from high school GPA. Individual course grades at high school range from 1 to 6 (only integer values), and GPA is calculated as 10 times the average grade (up to two decimal places). A few extra points on the application score are awarded for choosing specific subjects in high school. For some courses, the application score can also be adjusted based on ad-hoc field specific conditions unrelated to academic requirements (e.g. two extra points for women at some male-dominated fields). Finally, applicants can also get some compensation in their application score depending on their age, previous education and fulfillment of military service.

On applying, students rank up to fifteen courses. Information about what courses are offered by the different institutions is made available in a booklet that is distributed at high schools. The deadline for applying to courses is mid-April. This is the applicants' first submission of course rankings. They can adjust their rankings until July. New courses cannot be added, but courses can be dropped from the ranking. Once the rankings are final in July, offers are made sequentially where the order is determined by the applicants' application score: the highest ranked applicant receives an offer for her preferred course; the second highest applicant receives an offer for her highest ranked course among the remaining courses; and so on. This is repeated until either slots run out, or applicants run out. This allocation mechanism corresponds to a so-called sequential dictatorship, which is both Pareto efficient and strategy-proof (Svensson, 1999). By design, this mechanism should elicit the applicants true ranking of fields at time of application.[12]

This procedure generates the first set of offers which are sent out to the applicants in late July. Applicants then have a week to accept the offer, choose to remain on a waiting list or withdraw from the applicant process. The slots that remain after the first round are then allocated in a second round of offers in early August among the remaining applicants on the waiting list. New offers are generated following the same sequential dictatorship mechanism as in the first round, and sent out. Since applicants in this second round can only move up in the offer sequence, second round offers will either correspond to first

[12]A possible caveat to the strategy-proofness is the truncation of the application list at 15 courses. This truncation may induce individuals to list a safe option as the 15th choice to make sure they receive any offer of post-secondary education. In practice, this seems unlikely to matter for our findings: During the period our application data cover, less than 0.07% of all applicants are offered a 15th choice.

round offers, or be an offer for a higher ranked field. In mid-August, the students begin their study in the accepted field and institution. If the students want to change field or institution, they usually need to participate in next year's admission process on equal terms with other applicants.

3.2 Instruments, next-best alternatives, and separability

For courses with excess demand, this admission process generates a setup where applicants scoring above a certain threshold are much more likely to receive an offer for a course they prefer as compared to applicants with the same course preferences but marginally lower application score. This creates discontinuities which effectively randomize applicants near unpredictable admission cutoffs into different fields and institutions.

To see this, consider Table 1a which shows a stylized example of a typical application where the applicant is on the margin of getting different field offers from the same institution. Suppose the applicant has an application score of 49. In this case, she would receive an offer for her 3rd ranked course. This defines her *preferred field* in the local course ranking around her application score, namely field 2. In this local ranking, her *next-best alternative* is field 3, the field she would prefer if field 2 would not be feasible. We can now compare her to an applicant with the same ranking of fields, but who has a slightly lower application score of 47. This applicant has the same preferred field and next-best alternative in the local ranking around her application score, but receives an offer for field 3 instead of 2. The intuition behind our identification strategy is that by comparing the outcomes of these applicants we can estimate the effect of getting an offer of field 2 instead of 3, while ruling out that differences in their outcomes are driven by unobserved heterogeneity in preferences, ability and other confounders.

Table 1b gives another example where two applicants are on the margin of getting an offer for the same field but from different institutions. One applicant has a application score of 49 and receives an offer from institution A, whereas the other receives an offer from institution B because she has a slightly lower application score of 47. By comparing the outcomes of these applicants we can estimate the effect of getting an offer of institution A

Table 1. Illustration of identification of payoffs

(a) Fields

Course Ranking	Inst.	Field	Cutoff
1st best	A	1	57
2nd best	B	1	52
3rd best	A	2	48
4th best	A	3	45

	Application score = 49		
Local Course Ranking	Inst.	Field	Offer
Preferred	A	2	Yes
Next-best	A	3	No

	Application score = 47		
Local Course Ranking	Inst.	Field	Offer
Preferred	A	2	No
Next-best	A	3	Yes

(b) Institutions

Course Ranking	Inst.	Field	Cutoff
1st best	B	1	52
2nd best	A	2	48
3rd best	B	2	46
4th best	B	3	43

	Application score = 49		
Local Course Ranking	Inst.	Field	Offer
Preferred	A	2	Yes
Next-best	B	2	No

	Application score = 47		
Local Course Ranking	Inst.	Field	Offer
Preferred	A	2	No
Next-best	B	2	Yes

instead of B, while ruling out that differences in their outcomes are driven by unobserved heterogeneity.

In the two examples of Table 1, the applicants either receive offers for different fields from the same institution or from different institutions for the same field. This illustrates that we have independent variation in field and institution choices. In principle, we could therefore estimate the payoff to field of study separately for each institution, but sample sizes prevent us from such an estimation procedure.

In our baseline 2SLS model, we abstract from differences in institutional quality, recognizing that changing field could involve changes in institution of study. Indeed, the baseline estimates of the payoffs to field of study will capture any effect that is linked to the change in fields because of crossing the admission cutoff between his preferred field and next-best alternative. We therefore think of the baseline estimates as measures of earnings gains from completing one field of study as compared to another, with the understanding that these gains may not necessarily arise only from occupational specificity of human capital.

To examine the role of institutional quality in explaining the estimated payoffs, we

Table 2. Illustration of separability in identification of selection patterns

(a) Cohorts (t, s) and fields

Local Course Ranking	Inst.	Field	Cutoff (t)
Preferred	A	2	48
Next-best	A	3	45
Local Course Ranking	**Inst.**	**Field**	**Cutoff (s)**
Preferred	A	3	47
Next-best	A	2	44

(b) Fields and institutions

Local Course Ranking	Inst.	Field	Cutoff
Preferred	A	2	48
Next-best	A	3	45
Local Course Ranking	**Inst.**	**Field**	**Cutoff**
Preferred	B	3	47
Next-best	B	2	44

impose separability between field and institution. Such separability assumptions are frequently imposed in empirical analysis of payoffs to different types of post-secondary education. In our setting, separability allows us learn about the role of any course characteristic that differs across admission cutoffs, and that is correlated but not perfectly collinear with field. In particular, we complement the baseline 2SLS results with estimates of the payoffs to field of study where we control for a full set of indicator variables for the institution that applicants are predicted to attend given their course ranking and application score. In addition, we use the separability to explore other explanations for the payoffs to field of study, such as differences in peer quality.

Separability also plays a role in our analysis of the pattern of selection to fields. For example, consider a comparison of the payoff to preferred field 2 over next-best field 3 and the payoff to preferred field 3 over next-best field 2. To identify both these payoffs, it is necessary that field 2 has a higher admission cutoff for some individuals, whereas field 3 has a higher admission cutoff for other individuals. In Table 2a, we show how separability between cohort and field allows us to exploit variation over time in admission cutoffs to learn about selection patterns: In one year the application threshold for field 2 was higher than for field 3, while in another year this was reversed. Another example is shown in Table 2b, which illustrates the case where admission cutoffs for a pair of fields are reversed across institutions. Taken together, the variation in admission cutoffs across institutions and over time allows us to assess self-selection to fields, while controlling for direct effects of cohort and predicted institution in a separable model.

Table 3. Classification of broad fields with examples of more detailed fields

Science:	Biology; Chemistry; Computer science; Mathematics; Physics
Business:	Administration; Accounting; Business studies
Social Science:	Sociology; Political science; Anthropology; Economics; Psychology
Teaching:	Kindergarten teacher; School teacher
Humanities:	History; Philosophy; Languages; Media
Health:	Nursing; Social work; Physical therapy
Engineering (BSc):	Electrical; Construction; Mechanical; Computer
Technology:	MSc engineering; Biotechnology; Information technology
Law:	Law
Medicine:	Medicine; Dentistry; Pharmacology

4 Data and descriptive statistics

4.1 Data sources and sample selection

Our analysis combines several sources of Norwegian administrative data. We have records for all applications to post-secondary education for the years 1998 to 2004. We retain the individuals' first observed application, also requiring that they have no post-secondary degree at that moment of application. We aggregate specific fields into 10 broad fields of study, listed in Table 3. We retain all applicants who apply for at least two broad fields of study, where the most preferred field needs to have an admission cutoff, and the next-best alternative must have a lower cutoff (or no binding cutoff). This ensures that we have information on both the preferred and the next-best field, and a source of identification (potentially binding admission cutoffs) in our analysis.

In a next step, we link this application information for the 1998–2004 cohorts to the Norwegian population registry in order to retain information on their socio-economic background. In particular, we have information about parental education (both for the mother and father), income of the father, and the immigrant status of the family. This information is pre-determined in the context of our analysis, and refers to the year when the applicant was 16 (fathers' earnings are averages at ages 16 and 19).

For our treatment variables, we have information for all applicants on their completed field and education. This information comes from the national education register for the years 1998 to 2012. We recode the information on individuals' educational attainment to match our broad field classification.[13] Our measure of annual earnings comes from the Norwegian tax registers over the period 1998 to 2012. This means that every cohort is observed for at least eight years after their application. The measure of earnings encompasses wage income, income from self-employment, and transfers that replace such income like short-term sickness pay and paid parental leave (but excludes unemployment benefits). Earnings are deflated using the CPI with 2011 as the base year, and are converted to US Dollars (USD) using exchange rates.[14]

In our main analysis, we estimate the payoff to field of study among individuals who complete post-secondary education in terms of their earnings 8 years after application. Relating the moment at which we measure earnings to the year of application (rather than year of completion) avoids endogeneity issues related to time to degree. Another advantage is that by positioning earnings 8 years after applying most individuals will have made the transition to work. As a result, the estimated payoffs should be interpreted as earnings gains early in the working career, rather than internal rates of return on the investment in human capital. In a robustness analysis, we show that the estimated payoffs change little if we include individuals who do not complete post-secondary education or measure earnings one year earlier or later.

4.2 Descriptive statistics

The first column of Table 4 shows descriptive statistics for our sample of first-time applicants who applied for at least two broad fields of study, and whose most preferred field had an application threshold. We standardize application score in our sample to have zero mean and standard deviation one. The majority of applicants, about 64 percent, is female. The applicants are, on average, between 21 and 22 years old when we observe them applying for the first time.[15] Father's earnings (average of earnings at applicant's age 16 and 19) is, on average, USD 66,000, and about 50 percent of the applicants have

[13]Norwegian Universities and Colleges Admission Service classifies specific fields into broad fields. This classification is different from the one used by the national eduction registry (http://www.ssb.no/a/publikasjoner/pdf/nos_c617/nos_c617.pdf). Our classification matches the two.

[14]We use a fixed exchange rate of 6.5 Norwegian kroner per US dollar.

[15]In Norway, students graduate from high school in the year they turn 19, after which many serve in the military, travel, or work for a year or two before enrolling in post-secondary education.

Table 4. Descriptive statistics of applicants

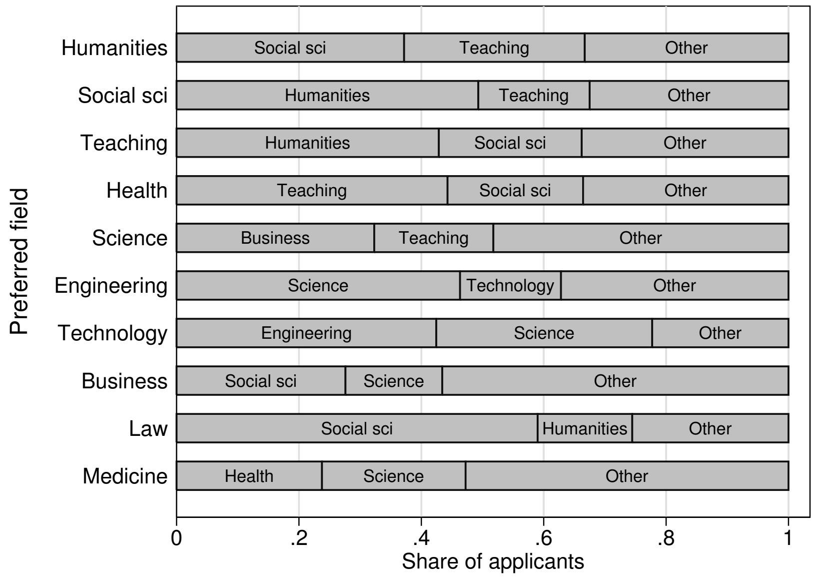
	Sample		All	
	Mean	SD	Mean	SD
Age	21.59	(4.36)	22.99	(5.79)
Female	0.64		0.62	
Application score	-0.00	(1.00)	-0.23	(1.05)
Earnings 8 yrs after appl. (USD 1,000)	55.52	(31.24)	52.84	(30.83)
Parents are immigrants	0.04		0.04	
Mother has higher educ.	0.37		0.30	
Father has higher educ.	0.40		0.33	
Father's earnings (USD 1,000)	65.61	(56.40)	58.00	(51.94)
Fields ranked	3.01	(1.11)	2.16	(1.24)
Inst. ranked	3.70	(2.36)	3.18	(2.45)
Rank of best (final) offer	2.50	(2.00)	1.82	(1.62)
Offered rank=1	0.40		0.58	
Offered rank=2	0.25		0.15	
Offered rank=3	0.13		0.07	
No offer	0.01		0.11	
Observations	50,083		218,824	

Note: Columns 1-2 and 3-4 display descriptive statistics of our estimation sample of applicants and of all applicants, respectively. Earnings of the applicants are measured eight years after application. All other characteristics are measured before or at the time of application. 'Offered rank = X' is a dummy variable for whether an individual is offered her Xth ranked choice.

a high-educated mother or father.

On average, an applicant receives an offer for her second or third ranked course. Around 40 percent receives an offer for the first ranked course, and close to 80 percent receives an offer for one of the three highest ranked courses. Less than one percent receive no offer at all. The applicants rank, on average, 3 different fields and 4 different institutions. Figure 1 reports the two most common next-best fields for every preferred field. For example, this figure shows that almost half of the individuals whose preferred field is Technology have Engineering as their next-best alternative. By comparison, individuals with Engineering as preferred field typically have Science as next-best alternative. It is also clear that Humanities, Social Science and Teaching tend to be close substitutes.

The second column of Table 4 reports observable characteristics for the whole population of applicants. As can be seen from the table, our sample is younger, has somewhat higher application score and a slightly more advantaged family background. Compared to our sample, the population average applicant is more likely to receive her first ranked



Note: We use our sample of applicants to compute the probability of a next-best field given a preferred field. For each preferred field, we report the share of applicants for the two most common next-best fields.

Figure 1. Most common next-best fields by preferred fields

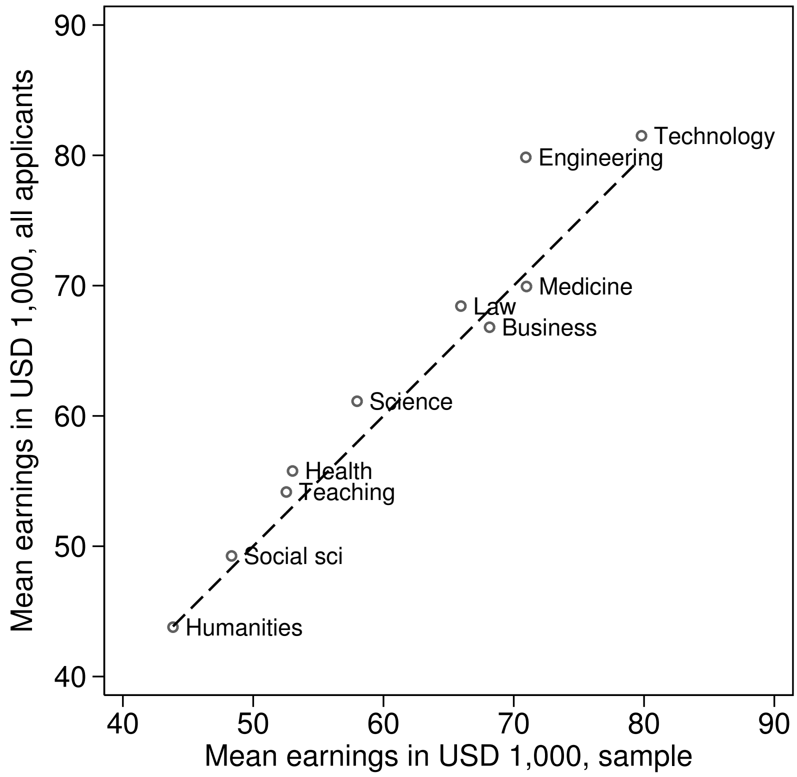
course; at the same time, the fraction that does not receive any offer is higher in the population of applicants. We can also compare average earnings across fields in our sample to those in the overall population of applicants. Figure 2 shows that average earnings across fields are closely aligned along the 45 degree line, suggesting our sample is very comparable to the other applicants in terms of levels of earnings by field.

5 Graphical illustration of research design

A virtue of our research design is that it provides a transparent way of illustrating how the payoffs to field of study are identified. To this end, we begin with a graphical depiction before turning to the formal econometric model and the regression results.

5.1 Admission cutoffs and field of study

As explained above, our research design uses the admission cutoffs as instruments for completed field of study. Figure 3 pools all the fields and admission cutoffs and illustrates how crossing the cutoffs affects i) the chance of receiving an offer to enroll in the preferred field, and ii) the probability of obtaining a degree in the preferred field. The data is



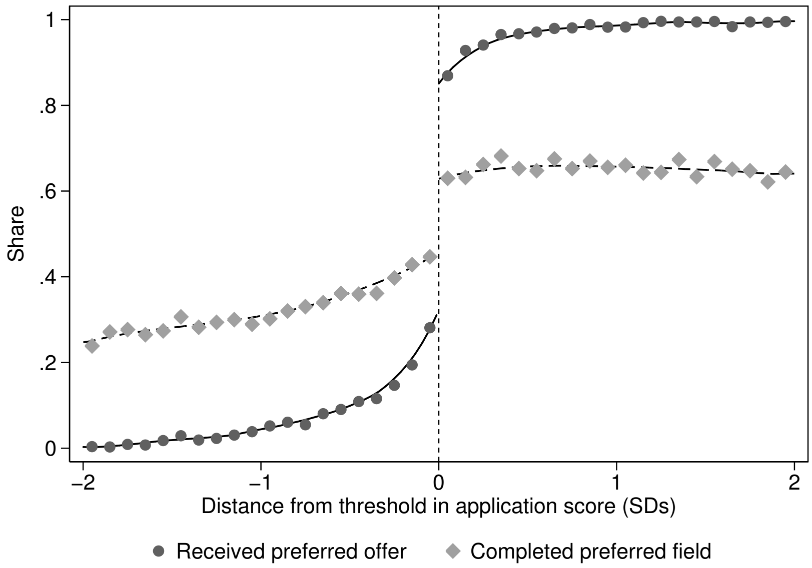
Note: This figure reports mean earnings by field for our sample of applicants and for all applicants. Earnings are measured eight years after application. The measures of earnings are regression adjusted for year of application.

Figure 2. Mean earnings by field: Sample and all applicants

normalized so that zero on the x-axis represents the admission cutoff to the preferred field, and observations to the left (right) of this cutoff have therefore an application score that is lower (higher) than the cutoff. We plot the unrestricted means in bins and include estimated local linear regression lines on each side of the admission cutoff.

The probability of being offered the preferred field increases by about 50 percentage points at the admission cutoff.[16] There is also a sharp jump in the probability of graduating with a degree in the preferred field at the cutoff, with graduation rates increasing from roughly 0.46 to 0.62. There are two reasons why the jump in the offer rate is larger than the jump in the graduation rate: Some individuals are offered but never complete their preferred field; others do not initially get an offer but they re-apply in the following

[16]Because some slots are reserved for special quotas and some fields have ad-hoc conditions unrelated to academic requirements, the probability of being offered the preferred field is not a deterministic function of application score. See the discussion of institutional details in Section 3.



Note: This figure shows the sample fraction that is offered or complete the preferred field by application score. We pool all admission cutoffs and normalize the data so that zero on the x-axis represents the admission cutoff to the preferred field. We plot unrestricted means in bins and include estimated local linear regression lines on each side of the cutoff.

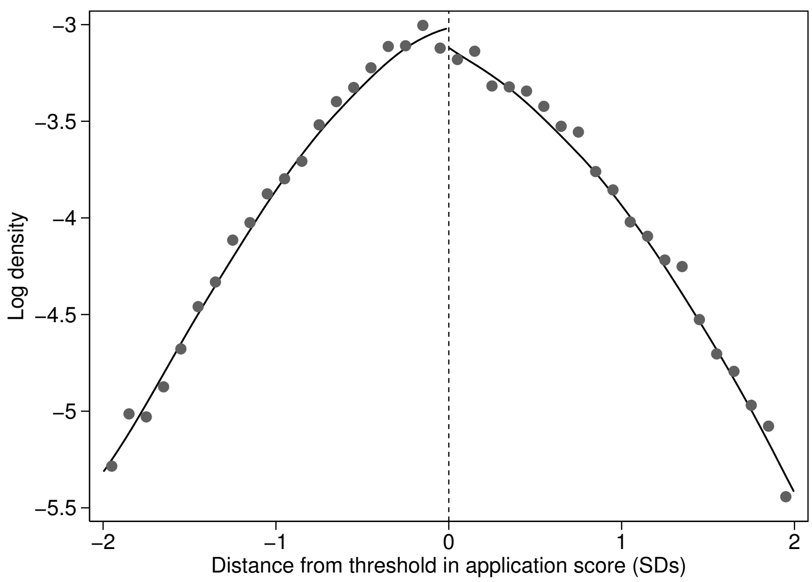
Figure 3. Admission thresholds and preferred field offer and completion

years and end up graduating with a degree in the preferred field. Since our treatment variables are defined as graduating with a degree in a given field, the former group of individuals are never takers (i.e. they do not complete their preferred field even when the instrument is switched on) while the latter group are always takers (i.e. they complete their preferred field even when the instrument is switched off). Our IV estimates are not informative about the payoffs to field of study for never or always takers.

5.2 Admission cutoffs and sorting

A potential threat to our research design is that people might try to sort themselves to the right of the cutoff in order to receive an offer for their preferred field of study. If such sorting occurs, we would expect to see discontinuities in the observed characteristics and in the density of applicants around the cutoffs.

Figure 4 shows the estimated density when we pool all the fields and admission cutoffs. What matters for our research design is that there is not a discontinuous jump in probability mass at zero, since that would point to sorting. As can be seen in Figure 4,



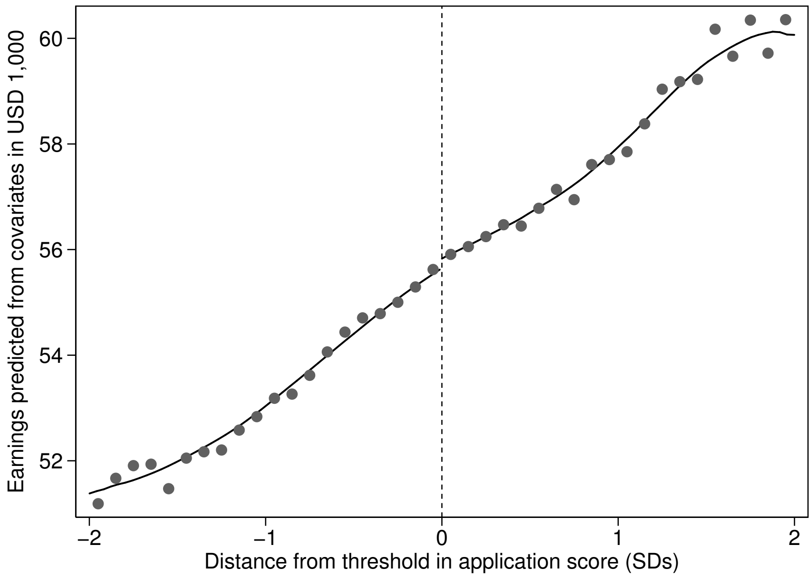
Note: This figure shows the log density of applicants in our sample by application score. We pool all admission cutoffs and normalize the data so that zero on the x-axis represents the admission cutoff to the preferred field. We plot unrestricted means in bins and include estimated local linear regression lines on each side of the cutoff.

Figure 4. Bunching check around the admission cutoffs

there is no indication that applicants are able to strategically position themselves around the application boundary, and the test proposed by McCrary (2008) is insignificant and does not reject the null hypothesis of no sorting.

A complementary approach to assess the validity of our instruments is to investigate covariate balance around the cutoffs. We consider several individual characteristics that correlate with earnings: gender, age, application score, parental education and parental income. We construct a composite index of these pre-determined characteristics, namely predicted earnings, using the coefficients from an OLS regression of earnings on these variables. Figure 5 shows average predicted earnings in small intervals on both sides of the pooled application cutoffs and a local linear regression fit. There is no indication that applicants on different sides of the application boundaries are different on observables. Indeed, a formal test is highly insignificant and we do not reject continuity in predicted earnings at the cutoff.

Taken together, Figures 4 and 5 suggest that students do not sort themselves around the admission cutoffs. The absence of sorting around the cutoffs is consistent with key



Note: This figure shows the predicted earnings by application score. Earnings are predicted from an OLS regression of earnings on gender, age, application score, parental education and parental income. We pool all admission cutoffs and normalize the data so that zero on the x-axis represents the admission cutoff to the preferred field. We plot unrestricted means in bins and include estimated local linear regression lines on each side of the cutoff.

Figure 5. Balancing check around the admission cutoffs

features of the admission process. First, the exact admission cutoffs are unknown both when individuals do their high school exams and when they submit their application. Second, the admission cutoffs vary considerably over time, in part because of changes in demand, but also because changes in funding cause variation in the supply of slots. Third, there is limited scope for sorting around the cutoff during the last semester of high school when students do their final exams and apply for post-secondary education. In our setting, the application scores depend on the academic results over all three years in high school, unlike countries in which admission is based only on how well the students do in final year exams or college entrance tests.

5.3 Admission cutoffs and post-graduation earnings

The figures above show that individuals on each side of the cutoff are similar in predetermined characteristics, but differ in the field in which they get an offer and ultimately graduate. Figure 6 examines whether the abrupt changes in field of study at the cutoffs are associated with discontinuous changes in post-graduation earnings. In particular, this figure illustrates how the changes in earnings depend both on individuals' preferred field and their next-best alternatives.

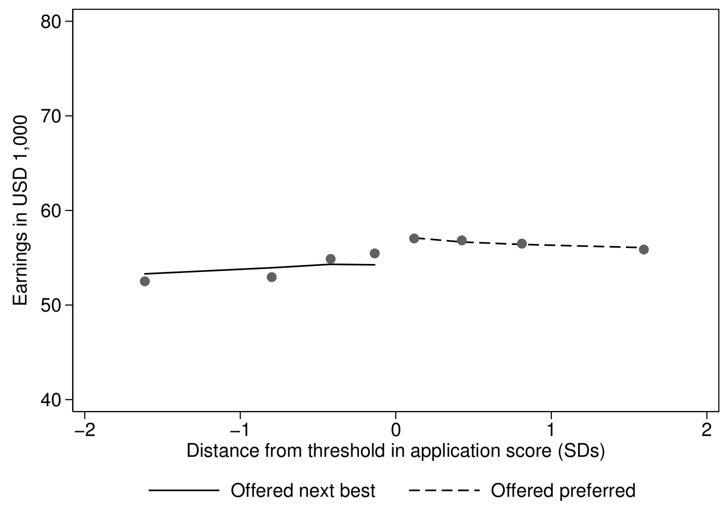
To construct this figure, we estimate the following regression separately for every next-best field k:

$$y = \sum_{j \neq k} \alpha_{jk} z_j + x' \xi_k + \nu_{jk} + \epsilon \qquad (13)$$

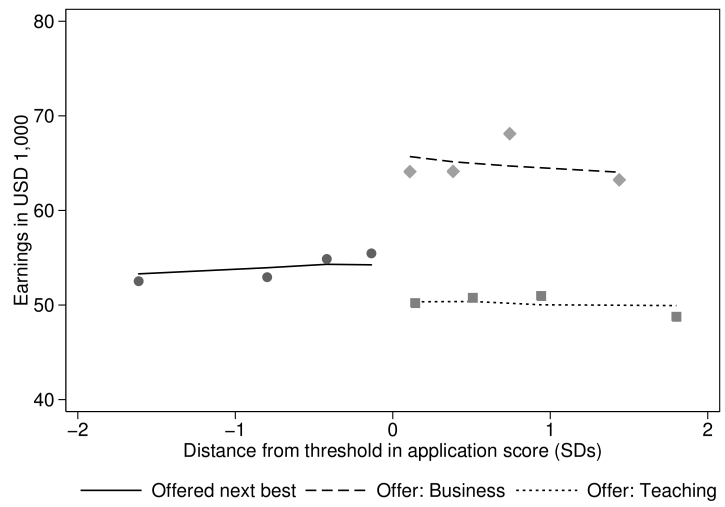
where y is earnings eight years after application, z_j is the predicted offer for field j which is equal one if j is the individual's preferred field and her application score exceeds the admission cutoff for field j, ν_{jk} is a fixed effect for preferring field j and having k as the next-best field, and x is a vector of controls that includes the running variable (application score), gender, cohort and age at application.

Figure 6 plots the predicted jumps (the $\hat{\alpha}$'s) from this regression model. The lines shows cell averages of predicted earnings when all covariates are set equal to their (global) means. To show the data, the figure also adds the average residual for different quartiles of the application score on each side of the threshold. The first graph of Figure 6 aggregates the estimates across all fields and cutoffs and shows the average change in earnings when individuals cross the cutoff for admission to their preferred field. While there is a sharp jump in the probability of completing the preferred field at the cutoff, there is only a small increase in average earnings. For those above the cutoff for their preferred field, the second graph aggregates only across next-best fields. Thus, the graph displays how earnings change when individuals cross the cutoff for admission to particular preferred fields, namely business and teaching. This figure shows that the earnings of those offered Business as a preferred field is much higher than the average of those offered their next-best field, while the earnings of those offered Teaching is much lower.

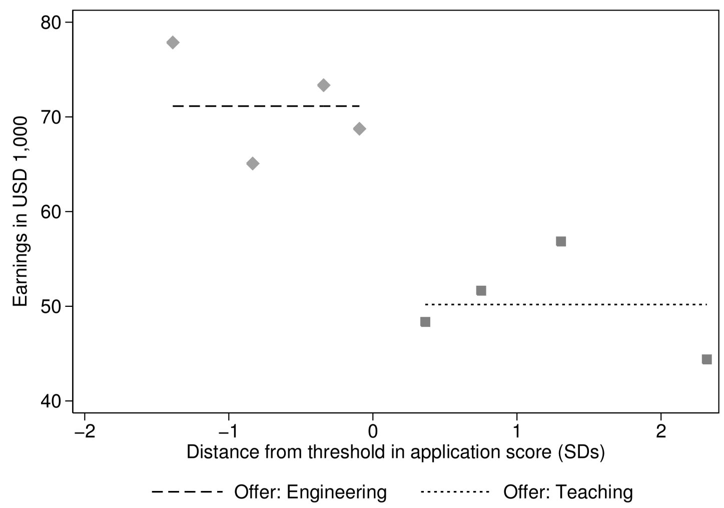
As shown formally in Section 2, the basic problem with interpreting the changes in earnings from crossing the admission cutoffs to the preferred fields is that individuals may come from different alternative fields, so the margins of choice will vary. The third and fourth graph of Figure 6 illustrate how the change in earnings from crossing the admission cutoff to Teaching can be positive or negative depending on the next-best alternative. In



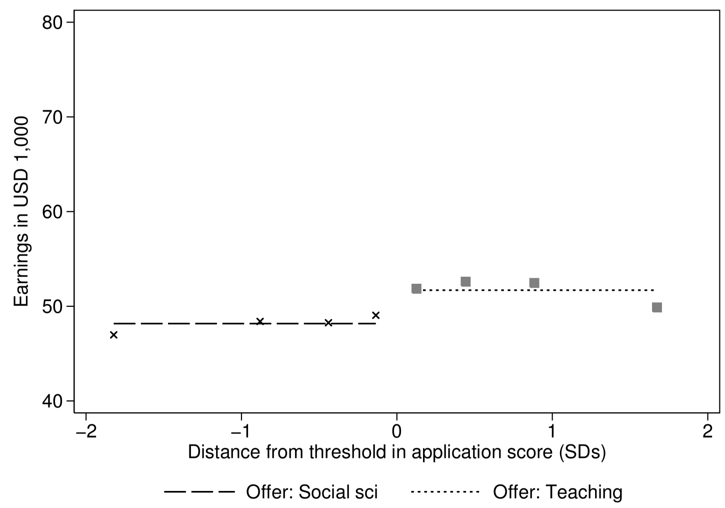
(a) Offered preferred (pooled) vs. next-best (pooled)



(b) Offered business or teaching vs. next-best (pooled)



(c) Offered Teaching vs. next-best Engineering



(d) Offered Teaching vs. next-best Social Science

Note: To construct the graphs, we estimate (13) separately for every next-best field. We plot the predicted jumps $\hat{\alpha}$'s. The lines show cell-averages of predicted earnings when all covariates are set equal to their global means. We display the average residual for different quartiles of the application score on each side of the cutoff. The first graph aggregates the estimates across all fields and cutoffs. The second graph aggregates only across next-best fields. The third and fourth show estimates for specific combinations of preferred and next-best fields.

Figure 6. Average earnings around admission cutoffs

particular, crossing the admission cutoff to teaching is associated with a sharp drop in earnings if the next-best alternative is Engineering, while we see a small jump in earnings if the next-best alternative is Humanities.

Taken together, these graphs highlight how information on next-best alternatives is key to identifying the payoffs from choosing one field of study as compared to another. In the next section, we detail how the admission cutoffs and the measures of next-best alternatives are used in IV estimation of the payoffs to different fields.

6 Estimation approach

Proposition 2 shows that we can identify the LATE from choosing one field of study as compared to another if our instruments satisfy an irrelevance condition, and if we can condition on individuals' next-best fields.[17] We implement the conditioning by specifying the following system of equations separately for every next-best field k:

$$y = \sum_{j \neq k} \beta_{jk} d_j + x'\gamma_k + \lambda_{jk} + \epsilon \tag{14}$$

$$d_j = \sum_{j \neq k} \pi_{jk} z_j + x'\psi_{jk} + \eta_{jk} + u, \quad \forall j \neq k \tag{15}$$

where (14) is the second stage equation, and (15) are the first-stage equations, one for each field. Our outcome variable y is earnings 8 years after applying, $d_j \equiv \mathbb{1}_{[d=j]}$ equals 1 if an individual completed field j and is 0 otherwise, λ_{jk} and η_{jk} are fixed effects for preferring field j and having k as the next-best field, and x is a vector of controls that includes the running variable (application score), gender, cohort and age at application.

From 2SLS estimation of equations (14)-(15), we obtain a matrix of the payoffs to field j compared to k for those who prefer j and have k as next-best field. Our estimation approach exploits the fuzzy regression discontinuity design implicit in the admission process described above, where individuals with application scores above the cutoff are more likely to receive an offer. Although the identification in this setup is ultimately local, we

[17] If one believes the irrelevance condition does not hold or that individuals do not understand the allocation mechanism, our estimation approach should be interpreted as relaxing the constant effects assumption in part (i) of Proposition 2 to allow for heterogeneity in payoffs according to next-best fields.

use 2SLS because our sample sizes do not allow for local non-parametric estimation. All equations therefore include controls for the running variable. While our baseline specification controls for the application score linearly, we report results from several specification checks, all of which support our main findings. To gain precision, we estimate the system of equations (14)-(15) jointly for all next-best fields, allowing for separate intercepts for preferred field and for next-best field (i.e. $\lambda_{jk} = \mu_k + \theta_j$ and $\eta_{jk} = \tau_k + \sigma_j$). In a robustness check, we show that our estimates are robust to allowing for separate intercepts for every interaction between preferred and next-best field.

Finally, following Imbens and Rubin (1997) and Abadie (2002), we also decompose the LATEs that results from estimation of (14)-(15) into the complier average potential earnings with field j and the complier average potential earnings with field k. This decomposition helps in interpreting the magnitude of the payoffs.

7 Payoffs to field of study

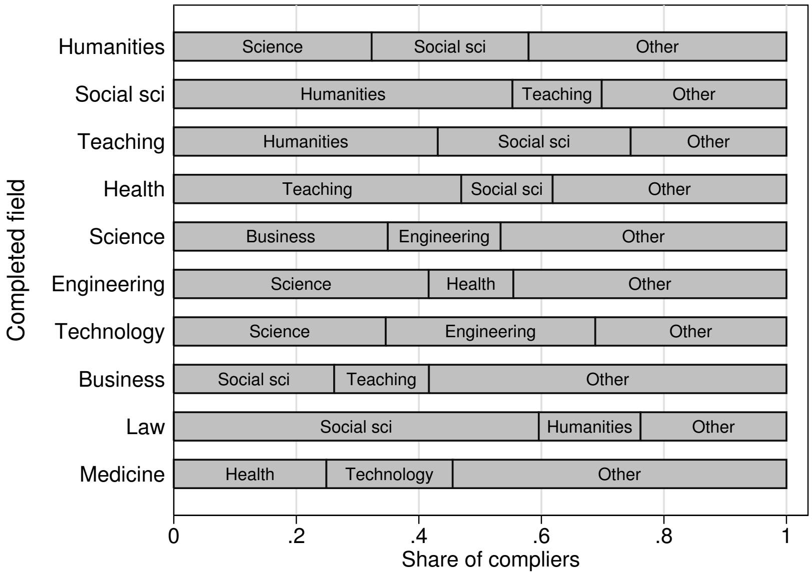
7.1 First stage estimates

We begin by estimating the first stage regressions. Since we condition on next-best fields, individuals who are induced to complete a field by a change in an instrument come from a particular alternative field. As a result, the first stage estimates are informative about the substitution pattern across fields from small changes in admission cutoffs.

Appendix Table A1 presents first stage estimates and the corresponding F-statistics. For brevity, we focus in these tables on the own instrument of each completed field: For the dependent variable d_j, we report the estimated coefficient of z_j and its F-statistic. The first stage estimates confirm that crossing the admission cutoffs between next-best alternatives and preferred fields lead to jumps in the probability of completing the preferred fields. The F-statistics are generally large, suggesting that weak instruments are not a key concern.[18]

From the first stage estimates, we can compute the proportion of compliers for whom the relevant choice margin is preferred field j versus next-best alternative k. Figure 7

[18]For example, the F-statistic is above 10 in 72 of 81 cases. Even in the other cases with weaker instruments, our just-identified 2SLS estimates are median-unbiased.



Note: We use our first stage estimates to compute the probability among our compliers of a next-best field given a completed field. For each preferred field, we report the share of compliers for the two most common next-best field.

Figure 7. Complier weights of alternative fields by completed fields

displays the two most common next-best fields for every preferred field. For example, this figure reveals that Science is the typical next-best field for compliers who prefer Technology or Engineering. It is also clear that Humanities, Social Science, and Teaching tend to be close substitutes. By comparing Figure 7 to Figure 1, we can see that the compliers to our instruments are similar to non-compliers in terms of their preferred and next-best field.

By computing the proportion of compliers by preferred and next-best field, we also learn that certain combinations of fields are rare. In particular, few compliers have Law as their next-best field, and virtually no one have Medicine as the next-best field. This means that we do not have support in our data to identify the effect of choosing field j instead of Medicine, and that we have too few compliers to obtain precise estimates of the payoff to choosing field j instead of Law.

7.2 2SLS estimates

Table 5 reports the 2SLS estimates from the model given by equations (14) and (15). By conditioning on next-best field, we are able to estimate the payoffs to different fields while correcting for selection bias and keeping the next-best alternatives as measured at the time of application fixed. For example, the first column reports estimates of the payoffs to different fields as compared to Humanities. This column shows significant gains in earnings to all fields as compared to Humanities. The payoffs are largest for Medicine, followed by Engineering, Science, Business, Law, and Technology. By way of comparison, choosing Health, Social Science or Teaching instead of Humanities has substantially lower but still significant payoff. To better understand the magnitude of the estimated payoffs, the final row of Table 5 computes the weighted average of the levels of potential earnings for compliers with their next-best field. For each next-best field, the weights sum to one and reflect the proportion of compliers by preferred field. This row shows, for example, that by choosing Science instead of Humanities, individuals almost triple their earnings early in their working career.

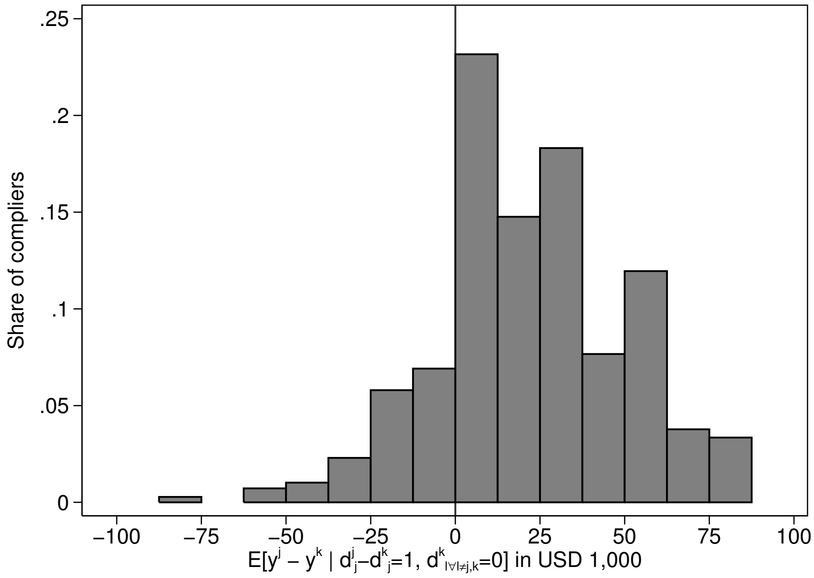
Figure 8 summarizes the results, showing the distribution of payoffs among the compliers for every combination of preferred field and next-best alternative. This figure illustrates that most compliers earn more in the preferred field than they would have earned in the next-best alternative. For many fields the payoffs rival the usual estimates of college earnings premiums, suggesting that the choice of field is potentially as important as the decision to enroll in college. In our data, for example, individuals who did not complete any post-secondary education were, on average, earning USD 43,200 at age 30, whereas the average earnings of individuals with a post-secondary degree was USD 54,700 at the same age.

Figure 9 graphs the weighted averages of payoffs to different completed fields across next-best fields. For each completed field, the weights sum to one and reflect the proportion of compliers by next-best field. Figure 8 illustrates that the compliers tend to prefer fields that give them higher earnings than their next-best alternatives. Indeed, this is true even in fields for which earnings are relatively low, like Teaching and Health. The only exception is Humanities, for which there is a negative average payoff.

Table 5. 2SLS estimates of the payoffs to field of study (USD 1,000)

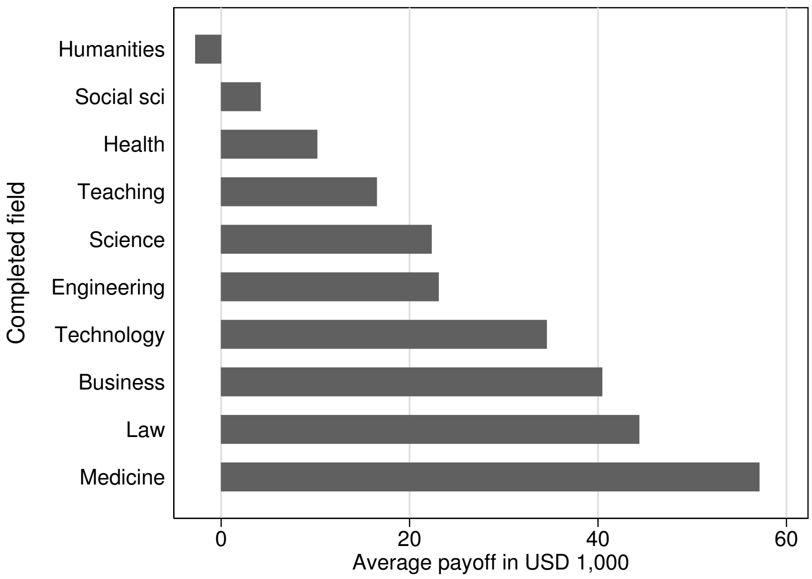
	Next best alternative (k):								
	Humanities	Soc Science	Teaching	Health	Science	Engineering	Technology	Business	Law
Completed field (j):									
Humanities		21.38*	-4.72	-22.93*	4.97	-38.51**	6.87	-42.21**	-156.33
		(10.97)	(9.85)	(12.12)	(11.86)	(14.72)	(48.29)	(10.56)	(437.28)
Social Science	18.72**		9.84	-10.82	55.46**	-55.36**	-110.38	-28.37**	-76.07
	(6.73)		(11.55)	(13.00)	(21.45)	(20.60)	(102.97)	(10.66)	(86.42)
Teaching	22.25**	31.37**		1.82	23.46**	-33.94**	-35.32	-21.08**	22.78
	(4.96)	(7.88)		(6.55)	(9.45)	(12.54)	(37.07)	(7.12)	(127.87)
Health	18.75**	30.69**	7.72**		28.87**	-27.87**	-43.38**	-17.39**	-55.19
	(6.25)	(7.56)	(2.82)		(7.64)	(10.35)	(20.84)	(3.97)	(97.68)
Science	53.71**	69.59**	38.58**	29.63**		-2.21	16.81	-4.92	148.26
	(18.37)	(22.36)	(14.20)	(11.53)		(14.60)	(18.07)	(10.51)	(276.20)
Engineering	59.81	-5.53	75.24**	0.16	52.35**		-46.00	-13.03	-57.66
	(50.59)	(58.17)	(37.50)	(16.36)	(20.98)		(43.89)	(23.70)	(166.60)
Technology	41.87**	58.69**	22.08*	32.45**	68.07**	-5.56		7.03	-53.07
	(10.84)	(10.09)	(12.44)	(10.09)	(9.63)	(11.95)		(9.49)	(147.53)
Business	48.13**	61.93**	31.02**	30.22**	58.01**	-3.42	28.54*		3.53
	(11.25)	(12.03)	(8.78)	(10.86)	(10.48)	(12.61)	(15.61)		(83.04)
Law	46.34**	55.62**	36.60**	21.49*	40.07**	-27.53	-15.55	-1.36	
	(7.16)	(8.34)	(11.56)	(11.46)	(9.68)	(18.29)	(17.96)	(8.66)	
Medicine	83.34**	79.39**	62.62**	45.57**	81.31**	21.07	40.07**	23.34**	14.82
	(9.76)	(10.65)	(9.02)	(7.01)	(9.71)	(20.67)	(11.72)	(8.79)	(83.61)
Average y^k	30.01	23.40	46.15	51.79	27.31	87.85	78.37	75.61	105.83
Observations	8,391	11,030	10,987	3,269	6,422	3,085	1,245	4,403	1,251

Note: From 2SLS estimation of equations (14)-(15), we obtain a matrix of the payoffs to field j as compared to k for those who prefer j and have k as next-best field. Each cell is a 2SLS estimate (with st. errors in parenthesis) of the payoff to a given pair of preferred field and next-best field. The rows represent completed fields and the columns represent next-best fields. The row labeled average y^k reports the weighted average of the levels of potential earnings for compliers in the given next-best field. The final row reports the number of observations for every next-best field. Stars indicate statistical significance, * 0.10, ** 0.05.



Note: This figure graphs the complier weighted distribution of estimates in Table 5.

Figure 8. Distribution of estimated payoffs (USD 1,000) to field of study



Note: This figure graphs the weighted averages of payoffs to different completed fields by next-best field. The payoffs come from Table 5. For each field, the weights sum to one and reflect the proportion of compliers by next-best field.

Figure 9. Average estimated payoffs (1,000 USD) by completed field

7.3 Robustness analysis

Before turning to the interpretation of the estimated payoffs, we present results from several robustness checks, all of which support our main findings.

Specification checks. In the baseline model, we used a linear specification for the control of the running variable (application score). In graphs (a) and (b) of Appendix Figure A1, we show that our results barely move if we instead use a quadratic or a cubic specification of the running variable in the 2SLS estimation. In graphs (c) and (d), we use separate quadratic and cubic trends on each side of the admission cutoff. The estimates are very similar to the baseline results.

In Appendix Figure A2, we show that the baseline estimates are robust to adding more control variables or being more flexible in the specification of the controls. In graph (a), we add interactions between preferred field and the pre-determined characteristics of the applicant. Graph (b) includes interactions between preferred field and separate trends on each side of the cutoffs, while graph (c) adds a full set of indicator variables for the interactions between preferred and next-best field. In graphs (d), we control for pre-determined measures of parental education and earnings.

Because of data availability, our baseline specification estimated the payoffs to field of study in terms of earnings 8 years after application. In graphs (a) and (b) of Appendix Figure A3, we examine the sensitivity of the estimates to measuring earnings one year later or one year earlier in the working career. The results suggest the payoffs to field of study do not change appreciably if we use earnings 7 or 9 years after application as the dependent variable in the 2SLS estimation.

Completing post-secondary education. Our baseline sample excludes individuals who do not complete any post-secondary education. This sample selection helps in reducing the residual variance, leading to improved power and precision. A possible concern is that our instruments might not only affect field of study but also whether an individual completes any post-secondary education. To address this concern, we perform two specification checks. In both cases, we add individuals who did not complete any post-secondary education to the baseline sample.

Using the extended sample, we first examine how the probability of completing any post-secondary education change when individuals cross the cutoff for admission to their preferred field. Graph (a) in Appendix Figure A4 shows that crossing the admission cutoffs to preferred fields matters little, if anything, for whether an individual completes any post-secondary education.

The second check expands the model given by equations (14) and (15) to account for non-completion, and introduces a new endogenous variable in the second stage, namely a dummy variable for completing post-secondary education. Since the original equation was exactly identified, an additional instrument is needed. To achieve identification, we extend our baseline sample to include individuals who have a preferred field with a binding admission cutoff, and whose next-best alternative is no post-secondary education. The additional instrument is an indicator variable for crossing the admission cutoff from not receiving any offer of post-secondary education to receiving an offer. Graph (b) of Appendix Figure A4 shows the estimated payoffs barely move if we account for endogeneity in completing post-secondary education.

Exclusion restriction. The exclusion restriction requires that crossing the admission cutoffs do not affect the individual's earnings if it does not change her field of study. One could, for example, think that being admitted to the preferred field in and of itself affects students confidence or motivation. Even if such direct effects were empirically important, the reduced form effects can still be given a causal interpretation (which is sufficient to perform the policy simulation below). Appendix Table A2 displays the full set of reduced form effects of crossing the admission cutoff to the preferred field from a particular next-best field. As is clear from Appendix Figure A5, these reduced form estimates show a qualitatively similar pattern in terms of earnings gains as compared to the 2SLS estimates.

7.4 Interpreting the payoffs

As discussed in Section 3, our baseline 2SLS estimates capture any effect which operates through whether the individual changes field of study because of crossing the admission cutoff between his preferred field and next-best alternative. We therefore think of the baseline estimates as measures of earnings gains from completing one field of study as compared to another, with the understanding that these gains may not necessarily arise only from occupational specificity of human capital.[19] While quantifying the relative importance of possible mechanisms is beyond the scope of this paper, our data allows us explore a few of the alternative explanations for the payoffs to field of study.

One possibility is that payoffs to field of study reflect differences in institutional quality. The student composition by fields varies across institutions, and changing field of study may involve changes in the institution of study. To examine the role of institutional quality in explaining the estimated payoffs, we exploit that we observe the institutional identifiers of an individual's preferred field and her next-best alternative. Depending on whether the individual's application score is higher (lower) than the admission cutoff, the individual is predicted to attend the institution of the preferred (next-best) field. By including a full set of indicator variables for the predicted institution in the first and second stage equations,[20] our 2SLS estimates are identified from the variation in predicted field of study that is orthogonal to predicted institution. Graph (a) of Figure A6 shows the 2SLS estimates do not change appreciably if we control for predicted institution in the first and second stages. The correlation between the estimated payoffs with and without controls for predicted institution is 0.84.

Another possibility is that payoffs to field of study reflect differences in the quality of peer groups.[21] The composition of peers varies across fields, and changing field may involve changes in the quality of peer groups. To examine the role of peer quality in explaining the estimated payoffs, we exploit that we observe the application scores of an individual's peer

[19]While the evidence base is scarce, some studies point to the importance of occupational specificity of post-secondary field of study. For example, Altonji et al. (2014b) relate field-specific task measures to earnings, and find that labor market skills associated with a major can account for a large fraction of earnings inequality among college educated. See also Altonji et al. (2014a), Arcidiacono et al. (2014), and Pavan and Kinsler (2012) on the importance of major-specific human capital.

[20]Predicted institution is an exogenous variable conditional on the controls for the running variable, application score. The same is true for our measures of predicted peer quality and predicted experience, discussed below.

[21]There is limited evidence on peer effects in college and the results are mixed. Some studies point to little if any influence of peer academic ability, while others suggest it can play a role in shaping study habits and beliefs. See e.g. Stinebrickner and Stinebrickner (2006).

students in her preferred field and in the next-best alternative. Depending on whether the individual's application score is higher (lower) than the admission cutoff, the individual is predicted to be exposed to the peers in the preferred field (next-best alternative). By including a control variable for average application score of the predicted peer group in the first and second stage equations, our 2SLS estimates are identified from the variation in predicted field of study that is orthogonal to average application score of the predicted peers. Graph (b) in Figure A6 shows the 2SLS estimates barely move if we control for predicted peer quality in the first and second stages. We find a correlation of 0.98 between the estimated payoffs with and without controls for predicted peer quality.

A third possibility is that differences in labor market experience at the time we measure earnings contribute to the estimated payoffs. In Norway, post-secondary degrees typically last 3-5 years. To examine the role of experience in explaining the estimated payoffs, we exploit that we observe the expected duration of each field of study. Depending on whether the individual's application score is higher (lower) than the admission cutoff, the individual is predicted to have an experience level of eight years minus the expected duration of study. By controlling for the predicted experience level, our 2SLS estimates are identified from the variation in predicted field of study that is orthogonal to predicted experience. Graph (c) of Figure A6 shows the 2SLS estimates do not change appreciably if we control for predicted experience in the first and second stages.

8 The role of next-best alternatives

The identification result in part (iii) of Proposition 2 motivated and guided our empirical analysis in which we exploited information on next-best alternatives to estimate payoffs to field of study. Proposition 2 also pointed out alternatives to using information on next-best alternatives: One could identify payoffs by assuming constant effects or imposing strong restrictions on preferences.

To investigate the constant effect assumption, we pool individuals with different next-best fields and re-estimate the 2SLS model given by equations (14) and (15). If we reject equality between the estimated payoffs by next-best alternatives and the corresponding estimates based on pooled 2SLS, then we reject the null-hypothesis of constant effects. Appendix Table A3 reports the differences between the pooled estimates and the estimates by next-best alternative. We find significant differences in the estimated payoffs; a joint test of equality is strongly rejected (p-$value = 0.014$). In Appendix Figure A7, we plot the estimated payoffs by next-best alternative against the differences between the pooled estimates and the estimates by next-best field. This figure suggest the constant effect assumption leads to severe biases in the estimated payoffs to field of study.

The restrictive preference assumption of Behaghel et al. (2013) also has testable implications. In particular, the assumption implies that for the first stage with dependent variable d_j and omitted comparison field k, the coefficient of z_l should equal zero for all $l \neq j, k$. The corresponding formal test has $H_0 : \pi_{lj} = 0, \forall l \neq j, k$. Appendix Table A4 reports the test statistics for this null hypothesis separately for each next-best field k, as well as pooled across all next-best fields. In all cases we strongly reject the restriction on preferences.

Taken together, the rejections of constant effects as well as restrictions on preferences suggest that information on next-best alternatives is essential to identify payoffs to field of study. Appendix Table A5 provides a complementary perspective on the role of next-best alternatives. The first column regresses the estimated payoffs reported in Table 5 on indicator variables for completed fields. We find that completed field accounts for 45 percent of the variation in payoffs. The third column adds next-best fields to the regression, increasing the R-squared to 96 percent. This highlights the quantitative importance of accounting differences in next-best fields when examining the payoffs to field of study choices.

The fourth and sixth column of Appendix Table A5 perform the same regressions, but now with potential earnings in the completed field as the dependent variable. While most of the variation is explained by differences in completed field, individuals' next-best fields do explain some of the variation. The seventh and ninth columns repeat these regressions but use potential earnings in the next-best field as dependent variable. Whereas the majority of the variation is explained by individuals' next-best fields, completed field also account for a small part of the variation.

9 Policy simulation: Supply of Science slots

The payoffs we estimate are LATEs of instrument-induced shifts in field of study. Since our instruments are admission cutoffs, they pick out individuals who are at the margin of entry to particular fields. We therefore need to be cautious in extrapolating the payoffs we estimate to the population at large. Despite the local nature of our estimates, the payoffs among the compliers to our instruments are informative about policy that (marginally) changes the supply of slots in different fields.

To illustrate the policy relevance of our findings, we simulate the effects on earnings from a policy which lowers the admission cutoffs to Science, a change which could be achieved by increasing the number of slots to this field. The total change in earnings resulting from the policy change will be driven by two channels: A direct effect on individuals who are shifted into Science, and an indirect effect as slots are freed up in other fields.

To be concrete, we consider a policy change that adds one hundred slots to Science. The additional slots are offered to individuals whose application score is just below the pre-reform admission cutoff to Science. In our application data, we observe who these individuals are as well as their next-best fields. Additionally, we know which individuals that will be offered the slots that are freed up and what their next-best fields are. By combining these pieces of information with the reduced form estimates reported in Appendix Table A2, we can compute the direct and indirect effects of the policy change.[22]

We begin by adding up the direct effects of the one hundred additional slots to Science. As shown in Table 6, the policy change is predicted to directly increase earnings by about USD 19,400. The positive and substantial direct effect is driven by gains in earnings among individuals at the margin of entry to Science whose next-best field is Teaching, Humanities, Health or Social Science. To compute the overall effect on earnings, we

[22]Figure 3 illustrated that we have a fuzzy regression discontinuity design. To simulate the effect of additional slots to Science, we therefore use the reduced form effects instead of the 2SLS estimates.

Table 6. The payoffs to adding 100 new slots in Science

	Direct effect		Indirect effect	
	New slots	Payoff in USD 1,000	Freed up slots	Payoff in USD 1,000
Humanities			19	-88.7
Social sci			9	-0.2
Teaching			19	55.8
Health			16	-22.9
Science	100	19.4		
Engineering			7	6.5
Technology			3	4.8
Business			27	90.8
Law				
Medicine				
Total	100	19.4	100	46.1

Note: This table shows the simulation results of a policy change that adds one hundred slots to Science. The second column adds up the direct effects on individuals who are offered the additional slots to Science. To compute the direct effects, we use the reduced form estimates reported in Appendix Table A2 and our information on which individuals that will be offered the additional slots and what their next-best fields are. The third column shows the next-best fields of these individuals. The fourth column reports the indirect effects of the slots that are freed up in other fields. To compute the indirect effects, we use the reduced form estimates reported in in Appendix Table A2 and our information on which individuals that will be offered the slots that are freed up and what their next-best fields are.

add up the indirect effects of the slots freed up. Table 6 displays the results from this calculation, showing a total indirect effect of USD 46,000. The positive and large indirect effect is largely driven by gains in earnings from individuals who were offered the slots that were made available in Business and Teaching. However, these gains are partly offset by earnings losses from the individuals who were offered the slots that were freed up in Humanities and Health.

In interpreting the results from this policy simulation, it should be noted that we make a couple of simplifying assumptions. For brevity, we only consider one round of indirect effects. In reality, however, the original change in the supply of slots would cascade through the education system as new slots are freed up every time individuals are admitted to higher ranked fields. We also abstract from any equilibrium type of changes in the behavior of individuals or labor markets to small increases in the supply of slots.

Despite these simplifications, our simulation serves to highlight how the effect of a policy that changes the field people choose depend inherently on the next-best alternatives, both directly through the payoffs and indirectly through the fields in which slots are freed up. Yet next-best alternatives play little if any role in policy discussions that are concerned with mismatches and market frictions in the supply and demand for post-secondary field of study. For example, the U.S. President's Council of Advisors on Science and Technology (2012) is the latest in a series of reports that call for education reforms to increase the number of college graduates in STEM fields. Advocates of such reforms claim that earnings would increase and matches between skills and job requirements would improve if more people were educated in STEM fields. However, without information on next-best alternatives it is difficult to project if the impact on earnings will be large or small, positive or negative.

10 Sorting pattern to fields

In Table 5, we presented a matrix of the payoffs to field j compared to k for those who prefer j and have k as next-best field. The matrix is not symmetric, implying that (a) the payoff to field of study are heterogeneous across individuals, and (b) the selection into fields is non-random. Taken together, this motivates our analysis of the sorting pattern to fields where we exploit the information on next-best fields to answer the following questions: Do individuals sort into fields in which they have comparative advantage? Is the sorting pattern consistent with earnings maximization or are non-pecuniary factors necessary to rationalize individuals' choices?

The answer to the first question tells us whether individuals differ not only in their productivities in a particular field, but also in their relative productivities in different fields. Describing how individuals with different abilities sort into different fields is important to understand the determinants of earnings inequality and the aggregate output for the economy as a whole (see e.g. Sattinger, 1993). The answer to the second question in informative about whether individuals behave as predicted by the Roy model of self-selection. We think of this as an important first step towards understanding what economic models that can help explain why individuals choose different fields of study.

10.1 Defining comparative advantage in the context of field of study

The term 'comparative advantage' is used in different ways by different authors. We follow the seminal work of Sattinger (1978; 1993) in our definition of comparative advantage. To be precise, let q_i^l denote individual i's productivity in field l and let π^l denote the price per unit of worker output in that field. Her potential earnings in field l is then given by $y_i^l = \pi^l q_i^l$.

Consider individuals who are on the margin between two fields j and k. Individuals are potentially heterogenous in their productivity in these fields, and are each characterized by a pair (q_i^j, q_i^k). If

$$\frac{q_1^j}{q_2^j} > \frac{q_1^k}{q_2^k} \Leftrightarrow \log y_1^j - \log y_1^k > \log y_2^j - \log y_2^k$$

then individual 1 is said to have a comparative advantage in field j and individual 2 has a comparative advantage in field k.

Our goal is to examine whether individuals tend to prefer fields in which they have comparative advantage. If individuals prefer fields in which they have comparative advantage, then the relative payoff to field j as compared to k is larger for individuals who prefer j over k ($j \succ k$) than for those who prefer k over j ($k \succ j$):

$$E[\log y_i^j - \log y_i^k | j \succ k] > E[\log y_i^j - \log y_i^k | k \succ j] \tag{16}$$

By way of comparison, the inequality would be reversed if individuals prefer fields in which they have comparative disadvantage (e.g. due to non-pecuniary factors), while random selection into fields would make (16) hold with equality.

10.2 Evidence on comparative advantage among compliers

While a complete characterization of the pattern of selection would require a number of strong assumptions, we can use the estimated payoffs to learn about the comparative advantages of the compliers to our instruments.

To examine whether compliers tend to prefer fields in which they have comparative advantage, we re-estimate the model given by equations (14) and (15), but now with log earnings as the dependent variable. This gives us the LATE counterparts to the testable implication of equation (16), namely

$$E[\log y^j - \log y^k | d_j^j - d_j^k = 1,\, d_{\forall l \neq j,\, k}^k = 0] > E[\log y_i^j - \log y_i^k | d_k^k - d_k^j = 1,\, d_{\forall l \neq j,\, k}^j = 0]$$

Appendix Table A6 reports the estimates of the payoffs to field of study with log earnings as the dependent variable.

Figure 10 provides evidence on comparative advantage among compliers. This figure shows the distribution of the differences in relative payoffs to field j versus k between individuals whose preferred field is j and next-best alternative is k and those with the reverse ranking. As is apparent from the figure, most of these differences are positive, which suggests that compliers tend to prefer fields in which they have comparative advantage. Indeed, the differences in payoff are sometimes considerable depending on whether j is the preferred field or the next-best alternative, suggesting that sorting on comparative advantage could be an empirically important phenomenon in the choice of field of study.

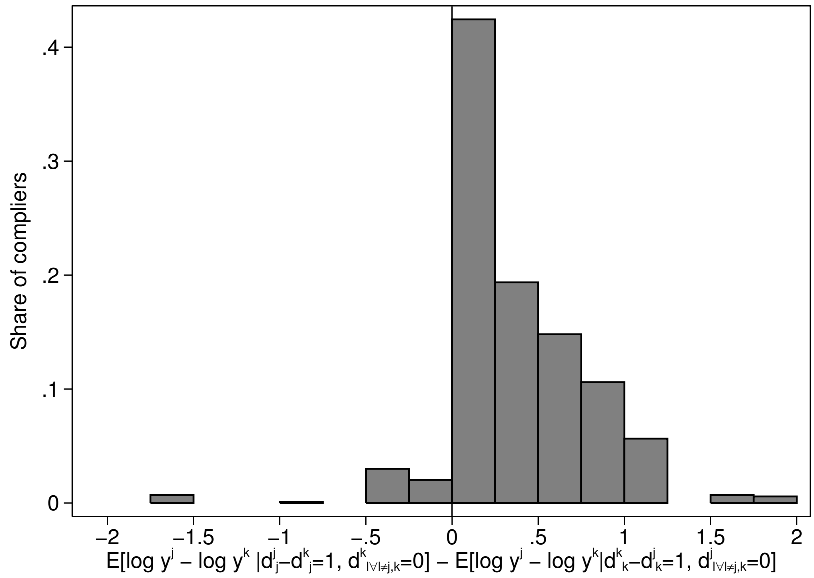
10.3 Robustness of comparative advantage.

There are at least three concerns about the conclusion of compliers preferring fields in which they have comparative advantage.

The first is that field of study may affect employment probabilities, which could bias the estimates with log earnings as dependent variable. However, we find fairly small impacts of field of study on employment. Furthermore, if marginal workers have lower potential earnings, any bias coming from employment effects should make it less likely to find evidence of comparative advantage.

A second concern is that we rely on variation in admission cutoffs to fields across institutions. It is therefore reassuring to find in Figure A6 that the estimated payoffs to fields change little if we account for quality differences across institutions.

Lastly, one might be worried the conclusions drawn about selection patterns are driven by heterogeneity across subfields within our broader definition of fields (see Table 3). To



Note: This figure graphs the distribution of the differences in relative payoffs to field j versus k between individuals whose preferred field is j and next-best alternative is k and those with the reverse ranking. To construct this graph, we use the complier weighted distribution of estimates in Appendix Table A6.

Figure 10. Comparative advantage

address this concern, we have re-estimated the model given by equations (14) and (15) with treatment variables defined according to subfields instead of broader fields. The estimates are shown in Appendix Figure A8. They suggest that aggregation to broader fields is not driving the conclusion that compliers tend to prefer fields in which they have comparative advantage.

10.4 Sorting pattern and economic models

The above results suggest that self-selection and comparative advantage are empirically important features of field of study choices. These findings have implications for the type of economic models that can help explain the causes and consequence of individuals choosing different types of post-secondary education.

Much economic analysis and empirical work relies on an efficiency unit framework where there is only one type of human capital which individuals possess in different amounts.[23] While the presence of comparative advantage is at odds with models based

[23] A prominent example is the Ben-Porath model, which assumes efficiency units so different labor skills are perfect substitute. Heckman et al. (1998) extend the standard Ben-Porath model by relaxing

on the efficiency unit assumption, it is consistent with the basic Roy model. The Roy model has a simple selection rule: Individual i chooses field j over k when $y_i^j > y_i^k$, which means her relative productivity advantage in field j (q_i^j/q_i^k) exceed the relative prices (π^k/π^j). Although the majority of the estimated payoffs in Table 5 are positive, and therefore consistent with the basic Roy model where individuals maximize earnings, for a subset of field pairs the estimated payoffs are negative. These choices can be rationalized by a generalized Roy model where idiosyncratic individual returns correlate positively with the valuation of the non-pecuniary factors of fields (see e.g. Heckman and Vytlacil, 2007).

11 Conclusion

Why do individuals choose different types of post-secondary education, and what are the labor market consequences of those choices? In this paper, we showed that answering these questions is difficult because individuals choose between several unordered alternatives. Even with a valid instrument for every type of education, IV estimation of the payoffs require information about individuals' ranking of types or strong additional assumptions, like constant effects or restrictive preferences.

These results motivated and guided our empirical analysis of the choice of and payoff to field of study. Our context is Norway's post-secondary education system where a centralized admission process covers almost all universities and colleges. This process creates discontinuities which effectively randomize applicants near unpredictable admission cutoffs into different fields of study. At the same time, it provides us with strategy-proof measures of individuals' ranking of fields. Taken together, this allowed us to estimate the payoffs to different fields while correcting for selection bias and keeping the next-best alternatives as measured at the time of application fixed.

Our results showed that different fields have widely different payoffs, even after accounting for institutional differences and quality of peer groups. For many fields the payoffs rival the college wage premiums, suggesting the choice of field is potentially as

the assumption of efficiency units for labor services.

important as the decision to enroll in college. We also found that information on next-best alternatives is essential to identify payoffs to field of study, strongly rejecting the alternatives of assuming constant effects or imposing strong restrictions on preferences. Moreover, differences in next-best alternatives are quantitatively as important as differences in preferred fields when examining the variation in payoffs to field of study choices. Finally we found that our data are consistent with field of study choice based on comparative advantage.

While our empirical findings are specific to the context of post-secondary education, there could be lessons for other settings. One example is the observational studies that use IV to study workers' selection of occupation, firms' decision on location, and families' choice of where to live. Our study highlights key challenges and possible solutions to understanding why agents choose different alternatives and what the causal effects of these choices are. Another example is the frequent use of encouragement design in development economics, where treatments are made available in the entire study area but take up is not universal. Researchers then use OLS and IV to estimate intention-to-treat and LATE parameters, respectively. We show what assumptions and information that are required to use encouragement design in settings with multiple, unordered treatments.

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A Proofs of Propositions

Proof Proposition 1

Developing the moment conditions (11) and (12) further, we obtain

$$
\begin{aligned}
&E[(\Delta^1-\beta_1)(d_1^1-d_1^0)+(\Delta^2-\beta_2)(d_2^1-d_2^0)] = \\
&\qquad E[\Delta^1-\beta_1|d_1^1-d_1^0=1,\, d_2^1-d_2^0=0]\Pr(d_1^1-d_1^0=1,\, d_2^1-d_2^0=0) \\
&+E[(\Delta^1-\Delta^2)-(\beta_1-\beta_2)|d_1^1-d_1^0=1,\, d_2^1-d_2^0=-1]\Pr(d_1^1-d_1^0=1,\, d_2^1-d_2^0=-1) \\
&\qquad +E[\Delta^2-\beta_2|d_1^1-d_1^0=0,\, d_2^1-d_2^0=1]\Pr(d_1^1-d_1^0=0,\, d_2^1-d_2^0=1) \\
&\qquad -E[\Delta^2-\beta_2|d_1^1-d_1^0=0,\, d_2^1-d_2^0=-1]\Pr(d_1^1-d_1^0=0,\, d_2^1-d_2^0=-1)=0
\end{aligned}
$$

and

$$
\begin{aligned}
&E[(\Delta^1-\beta_1)(d_1^2-d_1^0)+(\Delta^2-\beta_2)(d_2^2-d_2^0)] = \\
&\qquad E[\Delta^2-\beta_2|d_1^2-d_1^0=0,\, d_2^2-d_2^0=1]\Pr(d_1^2-d_1^0=0,\, d_2^2-d_2^0=1) \\
&+E[(\Delta^2-\Delta^1)-(\beta_2-\beta_1)|d_1^2-d_1^0=-1,\, d_2^2-d_2^0=1]\Pr(d_1^2-d_1^0=-1,\, d_2^2-d_2^0=1) \\
&\qquad +E[\Delta^1-\beta_1|d_1^2-d_1^0=1,\, d_2^2-d_2^0=0]\Pr(d_1^2-d_1^0=1,\, d_2^2-d_2^0=0) \\
&\qquad -E[\Delta^1-\beta_1|d_1^2-d_1^0=-1,\, d_2^2-d_2^0=0]\Pr(d_1^2-d_1^0=-1,\, d_2^2-d_2^0=0)=0
\end{aligned}
$$

Solving these equations without further restrictions will result in expressions for β_1 (and β_2) that are weighted averages of three different returns: $\Delta^1 \equiv y^1 - y^0$, $\Delta^2 \equiv y^2 - y^0$ and $\Delta^2 - \Delta^1 \equiv y^2 - y^1$, and involves eight different compliers groups.

Proof Proposition 2

Case (i), Δ^1 and Δ^2 are the same for all individuals (Constant Effects)

The constant effect assumption simplifies equations (11) and (12) to

$$
P(\Delta-\beta) \equiv \begin{pmatrix} E[d_1^1-d_1^0] & E[d_2^1-d_2^0] \\ E[d_1^2-d_1^0] & E[d_2^2-d_2^0] \end{pmatrix} \begin{pmatrix} \Delta^1-\beta_1 \\ \Delta^2-\beta_2 \end{pmatrix} = \begin{pmatrix} 0 \\ 0 \end{pmatrix}
$$

which is true if $\beta_1 = \Delta^1$ and $\beta_2 = \Delta^2$.[24]

Case (ii), $d_1^2 - d_1^0 = 0$ and $d_2^1 - d_2^0$ (Restrictive Preferences)

The restrictive preferences assumption simplifies (11) and (12) to

$$E[(\Delta^1 - \beta_1)(d_1^1 - d_1^0)] = 0 \tag{17}$$

$$E[(\Delta^2 - \beta_2)(d_2^2 - d_2^0)] = 0 \tag{18}$$

and by monotonicity and independence we have

$$E[(\Delta^1 - \beta_1)(d_1^1 - d_1^0)] = E[\Delta^1 - \beta_1 | d_1^1 - d_1^0 = 1] \Pr(d_1^1 - d_1^0 = 1) = 0 \tag{19}$$

$$E[(\Delta^2 - \beta_2)(d_2^2 - d_2^0)] = E[\Delta^2 - \beta_2 | d_2^2 - d_2^0 = 1] \Pr(d_2^2 - d_2^0 = 1) = 0 \tag{20}$$

From the rank condition and monotonicity, it follows that

$$\beta_1 = E[\Delta^1 | d_1^1 - d_1^0 = 1]$$

$$\beta_2 = E[\Delta^2 | d_2^2 - d_2^0 = 1]$$

Case (iii), $d_1^1 = d_1^0 \Rightarrow d_2^1 = d_2^0$, $d_2^2 = d_2^0 \Rightarrow d_1^2 = d_1^0$ (Irrelevance), and condition on $d_1^0 = d_2^0 = 0$ (Next Best Alternative)

Irrelevance, monotonicity, and independence simplify the moment conditions above to

$$\begin{aligned} E[(\Delta^1 - \beta_1)(d_1^1 - d_1^0) + (\Delta^2 - \beta_2)(d_2^1 - d_2^0)] = \\ E[\Delta^1 - \beta_1 | d_1^1 - d_1^0 = 1,\ d_2^1 - d_2^0 = 0] \Pr(d_1^1 - d_1^0 = 1,\ d_2^1 - d_2^0 = 0) \\ + E[(\Delta^1 - \Delta^2) - (\beta_1 - \beta_2) | d_1^1 - d_1^0 = 1,\ d_2^1 - d_2^0 = -1] \Pr(d_1^1 - d_1^0 = 1,\ d_2^1 - d_2^0 = -1) = 0 \end{aligned}$$

[24]Under our assumptions

$$P = \begin{pmatrix} \Pr(d_1^1 - d_1^0 = 1) & \Pr(d_2^1 - d_2^0 = 1) - \Pr(d_2^1 - d_2^0 = -1) \\ \Pr(d_1^2 - d_1^0 = 1) - \Pr(d_1^2 - d_1^0 = -1) & \Pr(d_2^2 - d_2^0 = 1) \end{pmatrix}$$

and the rank condition ensures that P is invertible.

and

$$
\begin{aligned}
&E[(\Delta^1 - \beta_1)(d_1^2 - d_1^0) + (\Delta^2 - \beta_2)(d_2^2 - d_2^0)] = \\
&\qquad E[\Delta^2 - \beta_2 | d_1^2 - d_1^0 = 0,\, d_2^2 - d_2^0 = 1] \Pr(d_1^2 - d_1^0 = 0,\, d_2^2 - d_2^0 = 1) \\
&+ E[(\Delta^2 - \Delta^1) - (\beta_2 - \beta_1) | d_1^2 - d_1^0 = -1,\, d_2^2 - d_2^0 = 1] \Pr(d_1^2 - d_1^0 = -1,\, d_2^2 - d_2^0 = 1) = 0
\end{aligned}
$$

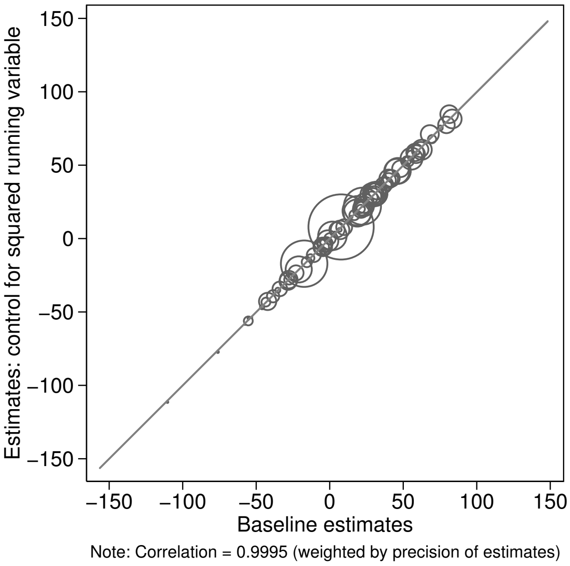
Stratifying on $d_1^0 = d_2^0 = 0$, implies that $\Pr(d_1^1 - d_1^0 = 1,\, d_2^1 - d_2^0 = -1) = \Pr(d_1^2 - d_1^0 = -1,\, d_2^2 - d_2^0 = 1) = 0$, and as a consequence the moment conditions simplify to

$$
\begin{aligned}
E[\Delta^1 - \beta_1 | d_1^1 - d_1^0 = 1,\, d_2^1 - d_2^0 = 0] \Pr(d_1^1 - d_1^0 = 1,\, d_2^1 - d_2^0 = 0) = 0 \\
E[\Delta^2 - \beta_2 | d_1^2 - d_1^0 = 0,\, d_2^2 - d_2^0 = 1] \Pr(d_1^2 - d_1^0 = 0,\, d_2^2 - d_2^0 = 1) = 0
\end{aligned}
$$

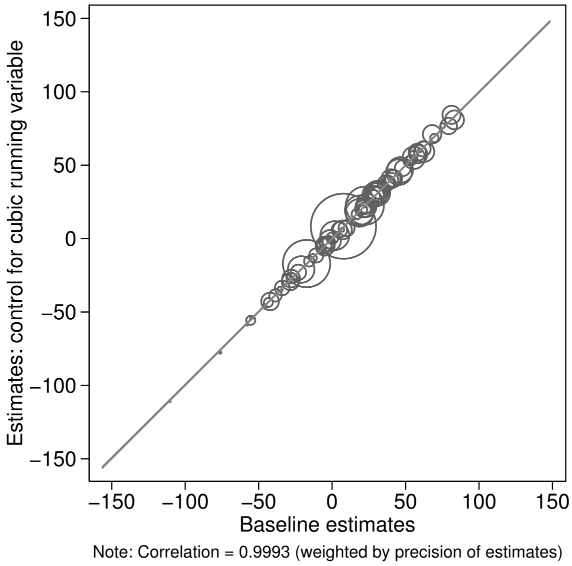
From the rank condition and monotonicity, it follows that

$$
\begin{aligned}
\beta_1 = E[\Delta^1 | d_1^1 - d_1^0 = 1,\, d_2^1 - d_2^0 = 0] \\
\beta_2 = E[\Delta^2 | d_2^2 - d_2^0 = 1,\, d_1^2 - d_1^0 = 0]
\end{aligned}
$$

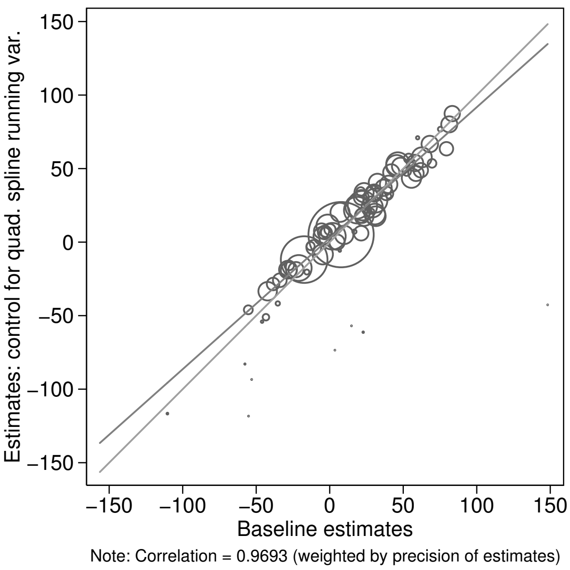
B Appendix tables and graphs



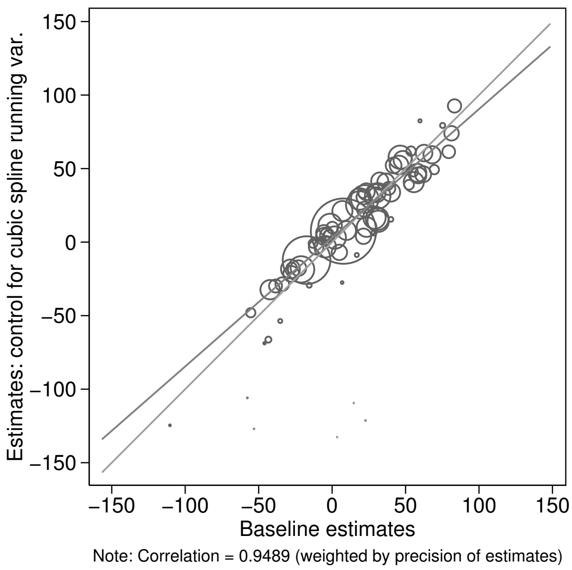
(a) Quadratic application score



(b) Cubic application score



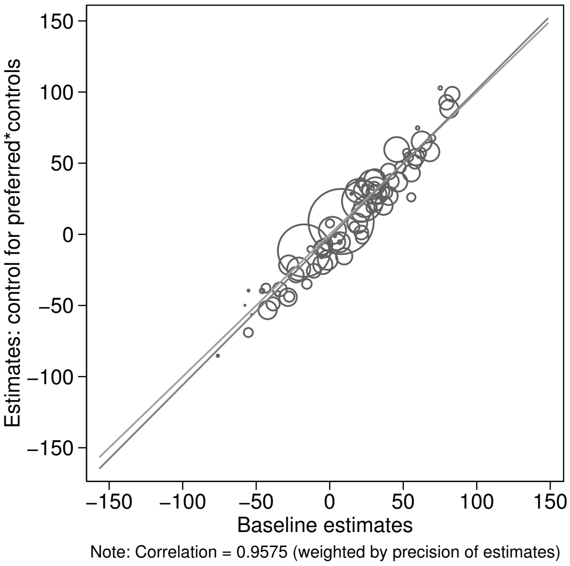
(c) Quadratic spline application score



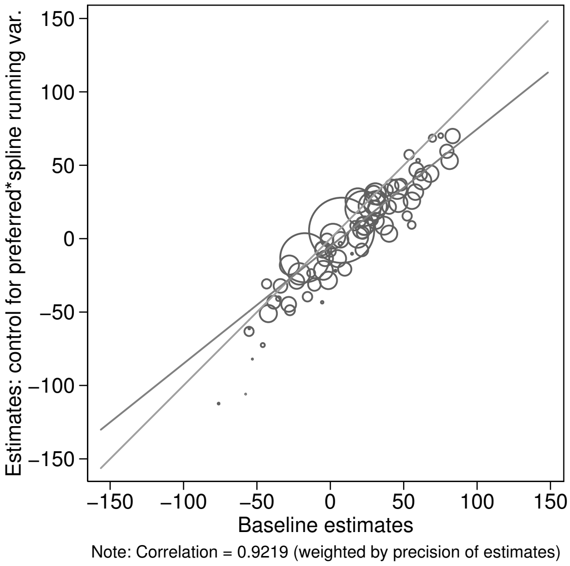
(d) Cubic spline application score

Note: In each graph, the horizontal axis shows the payoffs in Table 5, which we obtain by estimating equations (14)-(15) with our baseline specification. In each graph the vertical axis shows the estimated payoffs when extending the vector of control variables with extra variables: In graph (a) we add application score squared. In graph (b) a we include a cubic in application score. In graph (c) we include a quadratic in application score with separate coefficients for applicants who scored above and below the admission threshold. In graph (d) we include a cubic in application score with separate coefficients above and below the threshold. The correlations reported below each graph are weighted with the inverse sum of the squared estimated standard errors of the payoffs, which is also indicated by the size of the markers.

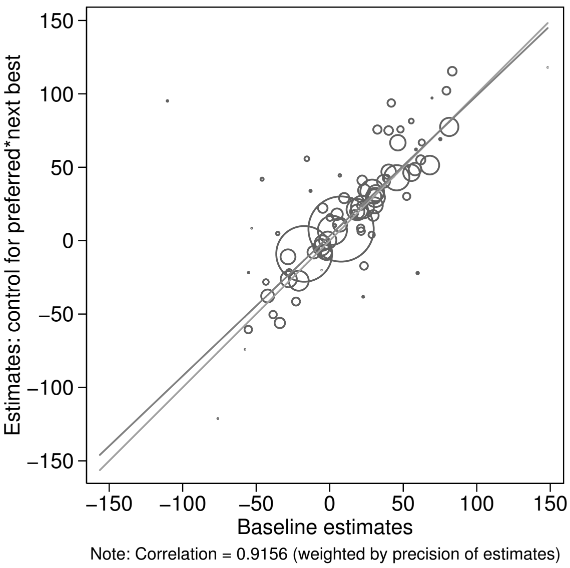
Figure A1. Robustness checks: Specification of the running variable



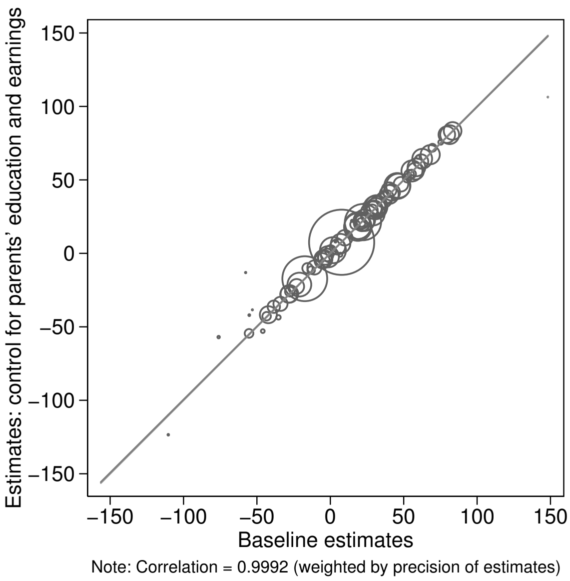
(a) Separate effect of control variables by preferred field



(b) Linear spline application score by preferred field



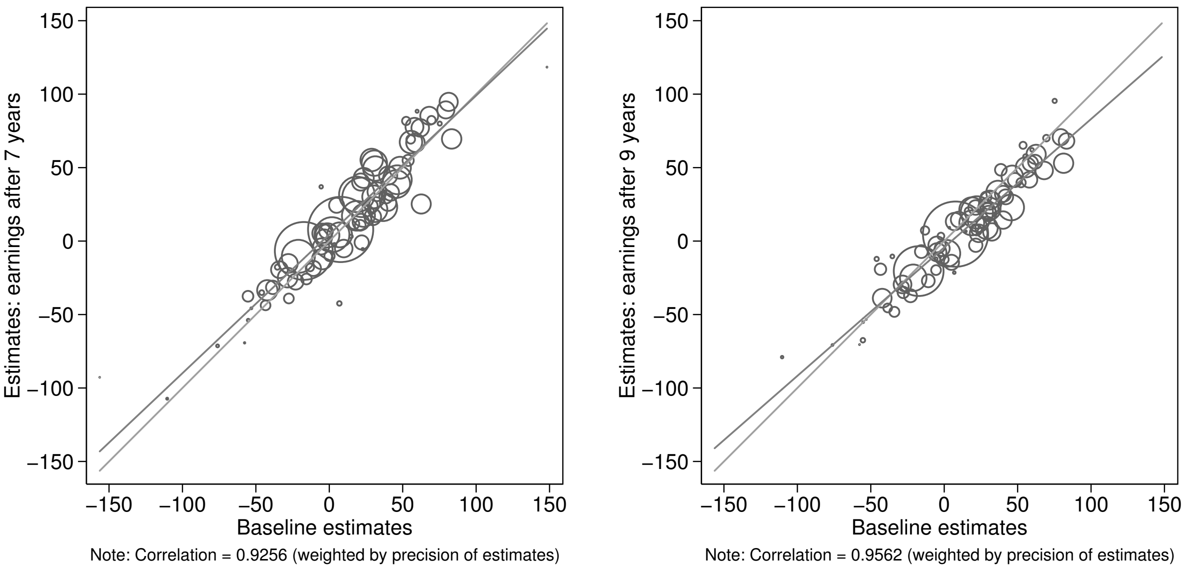
(c) Preferred field interacted with next best specific intercepts



(d) Controlling for parents' education and earnings

Note: In each graph, the horizontal axis shows the payoffs in Table 5, which we obtain by estimating equations (14)-(15) with our baseline specification. In each graph the vertical axis shows the estimated payoffs adding extra control variables: In graph (a) we add a set of interactions between our baseline control variables and preferred field. In graph (b) we include interactions between application score and preferred field, with separate coefficients for applicants above/below the admission threshold. In graph (c) we include the full sets of interactions λ_{jk} and η_{jk} between preferred field j and next-best k. In graph (d) we extend the vector of control variables (x) with parental education and earnings. The correlations reported below each graph are weighted with the inverse sum of the squared estimated standard errors of the payoffs, which is also indicated by the size of the markers.

Figure A2. Robustness checks: Parental characteristics and controls for preferred field



(a) Measure earnings one year earlier

(b) Measure earnings one year later

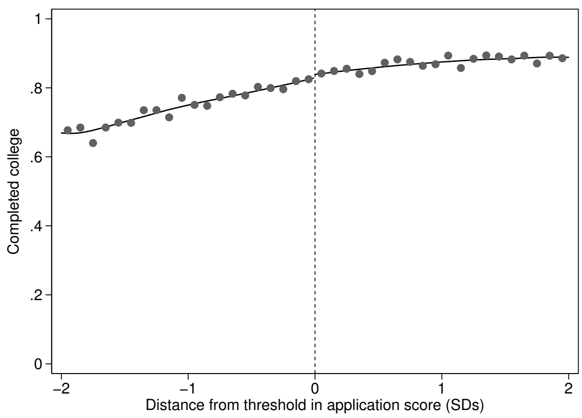
Note: In each graph, the horizontal axis shows the payoffs in Table 5, which are measured 8 years after application, and which we obtain by estimating equations (14)-(15) with our baseline specification. In each graph the vertical axis shows the estimated payoffs using earnings observed at different times after application. In graph (a) we observe earnings one year earlier, i.e, 7 years after application. In graph (b) we observe earnings one year later, i.e, 9 years after application. In graph (b) we are not able to observe the earnings of the last application cohort, such that the sample size is reduced to 41,570 people. The correlations reported below each graph are weighted with the inverse sum of the squared estimated standard errors of the payoffs, this is also indicated by the size of the markers.

Figure A3. Robustness checks: Timing of earnings measurement

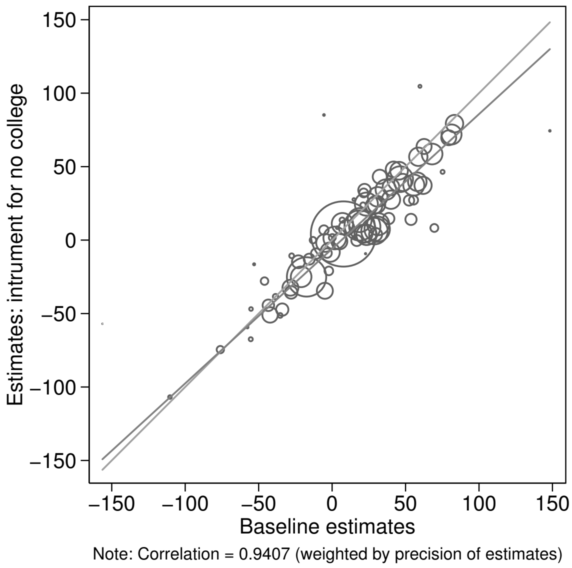
Table A1. First stages – own instruments

	Next best alternative (k):								
	Humanities	Soc Science	Teaching	Health	Science	Engineering	Technology	Business	Law
Preferred field (j)									
Humanities		0.335	0.293	0.366	0.407	0.679	0.392	0.514	0.416
		[147.4]	[101.1]	[49.6]	[102.1]	[77.2]	[7.9]	[70.2]	[1.4]
Social sci	0.251		0.265	0.243	0.204	0.497	0.167	0.347	0.318
	[186.0]		[80.7]	[46.3]	[31.8]	[13.0]	[3.2]	[62.4]	[27.8]
Teaching	0.412	0.506		0.482	0.524	0.631	0.448	0.479	0.531
	[576.6]	[552.5]		[359.8]	[236.2]	[89.5]	[7.2]	[188.7]	[13.5]
Health	0.490	0.400	0.485		0.455	0.590	0.431	0.549	0.479
	[823.6]	[1064.4]	[2630.7]		[642.8]	[353.3]	[29.3]	[1030.1]	[53.6]
Science	0.281	0.224	0.257	0.378		0.406	0.587	0.310	0.104
	[52.9]	[35.0]	[58.1]	[61.3]		[57.1]	[41.8]	[107.4]	[1.0]
Engineering	0.221	0.197	0.247	0.519	0.257		0.240	0.311	0.503
	[4.6]	[2.0]	[5.5]	[14.0]	[40.2]		[12.5]	[12.8]	[4.2]
Technology	0.542	0.567	0.497	0.543	0.458	0.244		0.540	0.567
	[110.1]	[200.1]	[49.4]	[93.7]	[560.6]	[107.9]		[170.6]	[57.6]
Business	0.435	0.430	0.485	0.455	0.462	0.630	0.591		0.410
	[345.3]	[547.8]	[316.0]	[199.0]	[365.7]	[290.9]	[180.8]		[145.4]
Law	0.490	0.431	0.518	0.340	0.455	0.563	0.644	0.441	
	[343.8]	[692.4]	[97.7]	[40.9]	[100.2]	[18.4]	[47.9]	[103.3]	
Medicine	0.750	0.665	0.766	0.559	0.548	0.646	0.527	0.786	0.724
	[230.0]	[317.8]	[130.1]	[338.9]	[406.7]	[34.5]	[177.2]	[320.3]	[121.1]

Note: From estimation of equation (15) for each next-best field k, we obtain a full matrix of first-stage coefficients giving the effect getting an offer for field l has upon completing j. While each column conditions on a given next-best field k, each row gives the own-field coefficients, i.e., the effect getting an offer for field j has on completing field j. F-statistics for the own-field first-stage coefficients in brackets.



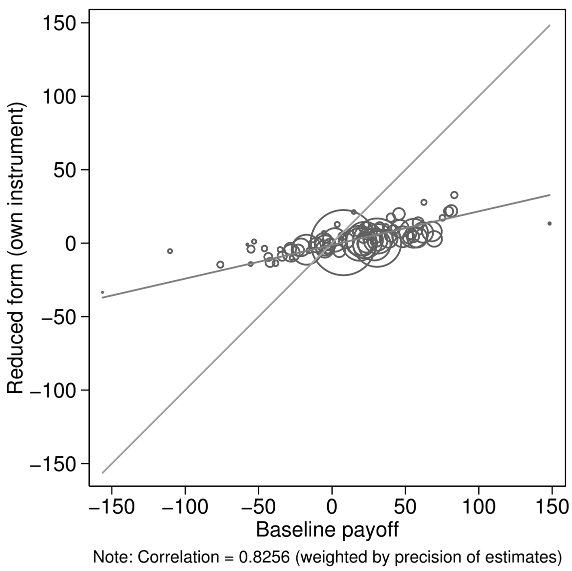
(a) Admission cutoff and share completing college



(b) Payoff estimates including non-completion

Note: Graph (a) extends the baseline estimation sample to also include applicants that have not completed any higher education 8 years after application. The dots show bin means, while the line is a local linear regression. In graph (b) we compare the baseline payoff estimates, presented in Table 5, with payoffs estimated when we also include no college as an additional field. In these estimations applicants who prefer field j and who have no higher education as their second best are used to construct an instrument for the payoff field j relative to no college. The correlation reported below graph (b) is weighted with the inverse sum of the squared estimated standard errors of the payoffs, which is also indicated by the size of the markers in graph (b).

Figure A4. Robustness checks: College non-completion



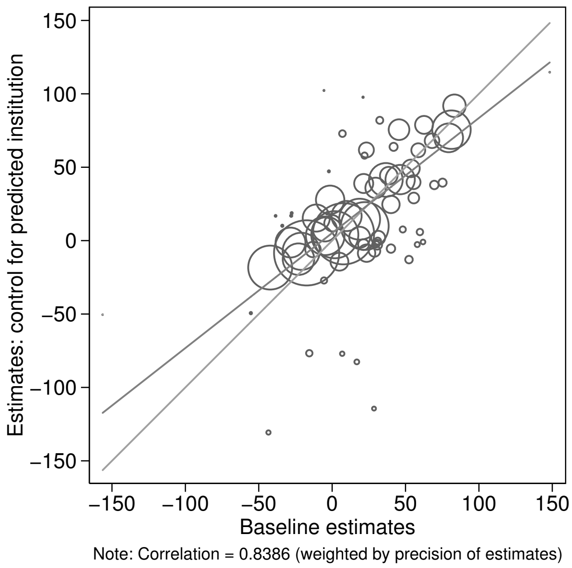
Note: The graph compares the baseline estimated payoff to field j given next-best field k, presented in Table 5, with reduced form payoffs to getting an offer for preferred field j given next-best field k, presented in Appendix Table A2. The correlation reported is weighted with the inverse sum of the squared estimated standard errors of the payoffs, which is also indicated by the size of the markers.

Figure A5. Baseline payoffs and (own instrument) reduced form payoffs

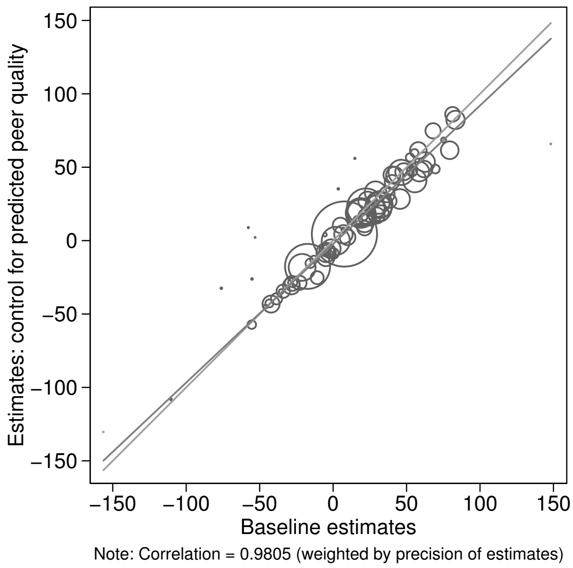
Table A2. Reduced forms – Own instruments

	Next best alternative (k):								
	Humanities	Soc Science	Teaching	Health	Science	Engineering	Technology	Business	Law
Preferred field (j):									
Humanities		-1.54	-1.51	-4.86*	-5.35*	-13.59**	3.32	-13.13**	-33.48
		(1.61)	(1.65)	(2.92)	(2.74)	(5.67)	(12.32)	(3.66)	(29.64)
Social Science	0.89		1.74	-1.25	4.52	-14.18*	-5.41	-3.75	-14.61**
	(1.21)		(2.21)	(2.28)	(2.96)	(8.33)	(8.28)	(2.80)	(5.40)
Teaching	2.13**	2.99**		2.21	-0.11	-8.82**	-4.36	-2.86	1.73
	(0.91)	(1.12)		(1.41)	(1.80)	(3.56)	(6.38)	(1.88)	(10.25)
Health	-0.07	0.36	0.38		-0.78	-6.72**	-9.36**	-4.46**	-4.04
	(0.95)	(0.69)	(0.51)		(1.03)	(1.93)	(4.13)	(1.11)	(4.60)
Science	6.52**	2.92	2.93	4.59*		-4.24	7.62*	-3.96**	13.40
	(3.02)	(2.12)	(1.88)	(2.49)		(3.75)	(4.38)	(1.92)	(13.87)
Engineering	7.69	6.90	17.24**	1.14	3.81		-3.64	-1.29	-0.91
	(6.75)	(8.03)	(5.76)	(6.34)	(2.79)		(5.90)	(5.66)	(16.14)
Technology	9.14**	13.88**	6.86	11.21**	7.96**	0.85		4.69	1.12
	(3.44)	(3.02)	(4.55)	(3.62)	(1.69)	(1.86)		(4.25)	(7.88)
Business	4.39**	7.28**	1.56	1.52	3.95**	-3.46	7.21		12.74*
	(1.59)	(1.63)	(1.68)	(2.84)	(1.64)	(3.08)	(5.56)		(6.58)
Law	9.82**	6.76**	11.04**	3.91	4.92*	-10.01	-5.03	-0.63	
	(1.88)	(1.15)	(5.54)	(3.00)	(2.85)	(8.13)	(6.17)	(2.99)	
Medicine	32.75**	21.69**	27.83**	19.91**	21.96**	11.36	17.39**	12.47**	21.20**
	(5.16)	(3.59)	(6.31)	(2.83)	(2.83)	(10.43)	(3.93)	(5.74)	(8.67)
Observations	8,391	11,030	10,987	3,269	6,422	3,085	1,245	4,403	1,251

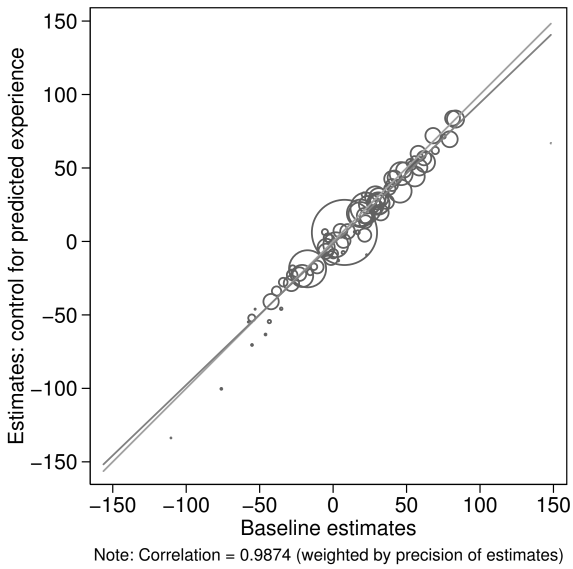
Note: From OLS estimation of the reduced form of equations (14)-(15), we obtain a full matrix of the payoffs to the instrument of a predicted offer to field j as compared to a predicted offer for k for those who prefer j and have k as next-best field. The rows represent preferred fields and the columns represent next-best fields. Each cell is a reduced form estimate (with st. errors in parenthesis) of the payoff to being predicted to get an offer for a given preferred field given the next-best field. The final row reports the number of observations for every next-best field. Stars indicate statistical significance, * 0.10, ** 0.05.



(a) Control for predicted institution



(b) Control for predicted peer quality



(c) Control for predicted experience

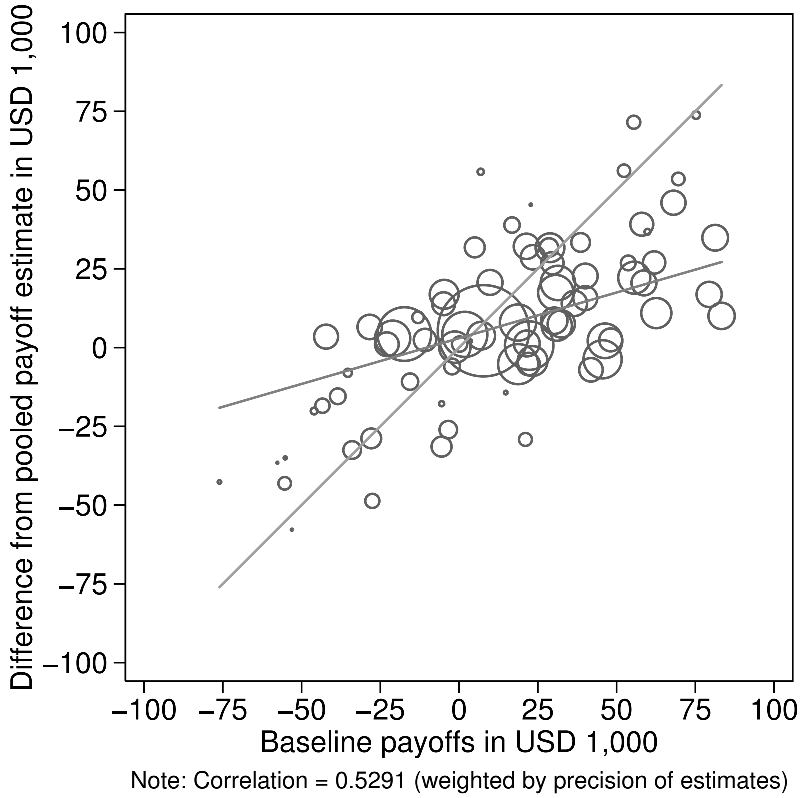
Note: In each graph, the horizontal axis shows the payoffs in Table 5, which we obtain by estimating equations (14)-(15) with our baseline specification. In each graph the vertical axis shows the estimated payoffs when extending the vector of control variables (x) with extra variables that may mediate an effect on earnings of completing field j versus k: In graph (a) we add dummies for the institution the applicant is predicted to attend. In graph (b) a we include the leave-out mean application score of those predicted admitted to each field by institution, excluding the applicant. In graph (c) we include predicted experience, constructed as 8 minus the nominal duration of the predicted offered field. The correlations reported below each graph are weighted with the inverse sum of the squared estimated standard errors of the payoffs, which is also indicated by the size of the markers.

Figure A6. Interpreting the payoffs: Adding controls for predicted institution, peers and experience

Table A3. Difference between payoffs from baseline and pooled specification

	Next best alternative (k):								
	Humanities	Soc Science	Teaching	Health	Science	Engineering	Technology	Business	Law
Completed field (j):									
Humanities		31.71**	11.41	-2.74	38.73**	-18.96	36.39	1.16	-109.16
		(10.55)	(7.39)	(10.30)	(13.72)	(18.95)	(42.97)	(11.06)	(448.87)
Social Science	14.10		18.38*	0.54	90.09**	-45.89*	-68.03	7.30	-24.52
	(9.53)		(10.08)	(11.99)	(21.54)	(24.57)	(92.89)	(10.83)	(74.63)
Teaching	5.77	24.88**		5.85	43.41**	-32.06*	-7.27	8.19	57.54
	(7.27)	(8.13)		(5.05)	(10.93)	(16.88)	(32.31)	(7.04)	(133.35)
Health	0.41	21.21**	6.67		45.74**	-27.62*	-21.30	9.01	-14.27
	(6.35)	(7.60)	(4.27)		(10.91)	(16.15)	(18.85)	(6.19)	(92.41)
Science	11.12	31.23**	9.94	10.08		-18.37	19.32	-1.11	105.22
	(12.68)	(14.16)	(8.19)	(8.65)		(18.55)	(17.12)	(10.11)	(245.44)
Engineering	32.45	20.66	81.93**	3.54	72.66**		-22.28	23.19	9.99
	(42.98)	(44.15)	(34.11)	(15.27)	(19.90)		(35.93)	(20.34)	(128.71)
Technology	1.67	25.30**	-7.53	6.84	57.06**	-27.78		6.45	-34.49
	(9.20)	(8.92)	(12.24)	(8.89)	(13.66)	(17.88)		(10.36)	(141.81)
Business	0.64	24.11**	-0.33	1.74	46.65**	-31.41*	18.52		9.00
	(6.87)	(8.86)	(5.71)	(7.35)	(11.22)	(17.34)	(14.48)		(87.27)
Law	8.96	26.52**	15.10	3.85	38.89**	-45.35**	-18.65	5.80	
	(7.00)	(9.53)	(11.38)	(10.72)	(11.84)	(22.18)	(17.53)	(9.74)	
Medicine	17.99**	21.60**	13.28	-2.44	50.58**	-24.01	11.42	1.75	-1.26
	(8.80)	(9.62)	(8.66)	(7.53)	(12.16)	(24.52)	(12.61)	(9.08)	(84.79)
Joint test (χ^2_9)	17.10	14.11	17.03	7.94	22.75	11.96	12.05	10.56	7.79
p-value joint test	0.047	0.118	0.048	0.540	0.007	0.215	0.210	0.307	0.556

Note: Here we report the interaction between completed and next-best field. Each column reports the interaction coefficients from a separate estimation of the payoff to each field unconditional on next-best and conditional on the next-best field being k. If payoffs do not differ by next-best, we expect this interaction to be zero. The last two rows in each column report the χ^2-statistic and p-value from a joint test of the interactions between next-best field k and all completed fields j. A joint test of whether the payoff differ by some next-best field k for any completed field j gives the a test statistic χ^2_{72}=101.0, with p-value=0.014.



Note: The graph compares the baseline estimated payoff to field j given next-best field k, presented in Table 5, with payoff estimates that don't condition on next-best field. We estimate the unconditional payoff to each field j, $\hat{\beta}_j$ (relative to an arbitrary reference field), and calculate the unconditional payoff to field j over k as $\hat{\beta}_j - \hat{\beta}_k$. The correlation reported is weighted with the inverse sum of the squared estimated standard errors of the payoffs, which is also indicated by the size of the markers.

Figure A7. Difference between payoffs from baseline and pooled specification

Table A4. Testing for restrictive preferences

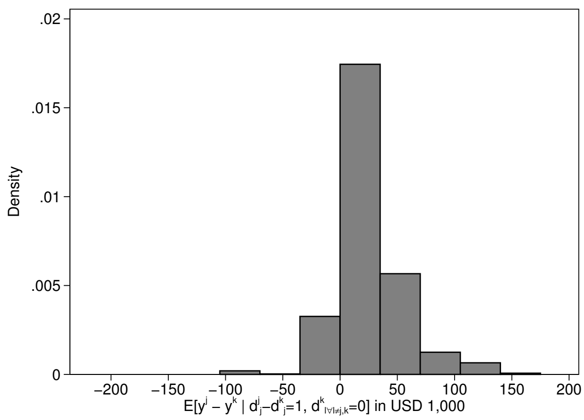
	χ^2 test statistic	p-value
Next best alternative (k):		
Humanities	747.9	<.001
Social Science	1080.2	<.001
Teaching	546.3	<.001
Health	288.1	<.001
Science	560.7	<.001
Engineering	611.8	<.001
Technology	473.9	<.001
Business	257.4	<.001
Law	317.5	<.001
Overall	5083.7	<.001

Note: The table reports the results from testing whether all first stage coefficients not associated with the own-field instrument or the next-best field are zero. We test $H_0 : \pi_{lj} = 0, \forall l \neq j, \forall j \neq k$ separately for each next-best field k (72 degrees of freedom), as well as the joint test across all next-best fields (648 degrees of freedom).

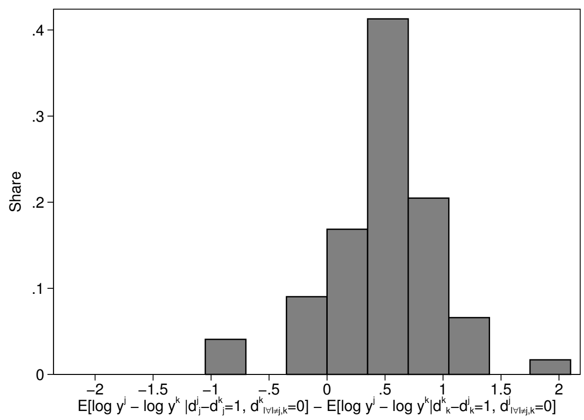
Table A5. OLS regressions of payoffs and potential earnings on completed and next best field

				Potential Earnings					
	Payoff			Completed field (j)			Next-best (k)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
R^2	0.446	0.552	0.955	0.933	0.119	0.951	0.032	0.904	0.935
Field Controls:									
- Completed	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes
- Next best	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
F Completed	6.4		62.1	110.6		118.4	0.3		3.4
p-value	<.001		<.001	<.001		<.001	0.984		0.002
F Next best		11.1	88.3		1.2	2.8		84.6	110.0
p-value		<.001	<.001		0.301	0.010		<.001	<.001

Note: Payoffs are those reported in Table 5, potential compliers' earnings are estimated with the method of Imbens and Rubin (1997). All regressions weigh with the inverse squared estimated standard error of the dependent variable.



(a) Distribution of estimated payoffs to subfields



(b) Comparative advantage for subfields

Note: The graphs display results from 2SLS estimation of equations (14)-(15) with j and k indexing subfields. The estimation sample is restricted to combinations of subfields where we have at least 50 individuals, giving a total sample size of 30,135. Graph (a) corresponds to Figure (8) and shows the complier-weighted distribution of the estimated payoff to subfield j over subfield k. Graph (b) corresponds to Figure (10) and shows the difference in relative payoffs for the pairs of subfields where we have both relative payoff estimates.

Figure A8. Payoffs to subfields

Table A6. Log payoffs

	Next best alternative (k):								
	Humanities	Soc Science	Teaching	Health	Science	Engineering	Technology	Business	Law
Completed field (j):									
Humanities		0.30	-0.19	-0.27	0.07	-0.38*	-0.17	-0.56**	0.33
		(0.19)	(0.16)	(0.20)	(0.20)	(0.20)	(0.68)	(0.16)	(0.21)
Social Science	0.33**		-0.08	-0.43	0.75**	-0.60	-2.60	-0.30**	-0.47
	(0.12)		(0.18)	(0.28)	(0.35)	(0.37)	(2.74)	(0.15)	(0.40)
Teaching	0.41**	0.46**		0.13	0.43**	-0.22	-0.35	-0.12	0.05
	(0.09)	(0.14)		(0.08)	(0.14)	(0.15)	(0.61)	(0.08)	(0.40)
Health	0.34**	0.46**	0.07		0.46**	-0.29**	-0.65*	-0.19**	0.05
	(0.09)	(0.13)	(0.05)		(0.12)	(0.13)	(0.39)	(0.05)	(0.25)
Science	0.62**	0.92**	0.49**	0.44**		0.02	0.13	0.03	1.16
	(0.26)	(0.30)	(0.21)	(0.18)		(0.19)	(0.26)	(0.13)	(1.51)
Engineering	0.78	0.82	1.28**	0.04	0.80**		-0.43	0.11	-0.55
	(0.81)	(1.55)	(0.62)	(0.23)	(0.33)		(0.53)	(0.30)	(1.03)
Technology	0.84**	0.85**	0.27	0.38**	1.01**	0.10		0.23**	0.31
	(0.14)	(0.16)	(0.19)	(0.16)	(0.15)	(0.14)		(0.10)	(0.37)
Business	0.76**	0.86**	0.43**	0.46**	0.86**	0.09	0.35		0.53**
	(0.12)	(0.16)	(0.11)	(0.11)	(0.14)	(0.15)	(0.25)		(0.22)
Law	0.76**	0.84**	0.40**	0.50**	0.63**	-0.27	-0.33	0.11	
	(0.10)	(0.13)	(0.12)	(0.16)	(0.15)	(0.27)	(0.34)	(0.11)	
Medicine	1.24**	1.13**	0.83**	0.65**	1.18**	0.53**	0.44**	0.40**	0.76**
	(0.12)	(0.16)	(0.12)	(0.11)	(0.15)	(0.20)	(0.17)	(0.09)	(0.23)
Observations	7452	10129	10308	3010	5825	2905	1165	4140	1152

Note: From 2SLS estimation of equations (14)-(15) with log earnings as dependent variable, we obtain a full matrix of the relative payoffs to field j as compared to k for those who prefer j and have k as next-best field. The rows represent completed fields and the columns represent next-best fields. Each cell is a 2SLS estimate (with st. errors in parenthesis) of the relative payoff to a completed field given the next-best field. Stars indicate statistical significance, * 0.10, ** 0.05.