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Identification of Causal Education Effects Using a Discontinuity in School Entry Tests: First Results from a Pilot Study*

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Abstract

We use a credible regression discontinuity design to estimate causal education effects. Pupils in the Swiss education system had to pass a centrally organized exam that classified them into different levels of secondary school, and that ultimately determined their educational degree. A major feature of this exam was the local randomization around the classification threshold due to the impossibility of strategic sorting. Our preliminary results suggest large and significant effects on earnings, political interest, and attitudes toward immigrants. The extension to a wider set of data is part of ongoing research.

JEL Classification: D72, I21, J15, J31

Keywords: Returns to education, causality, endogeneity, regression discontinuity.

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1 Introduction

The importance of education in the development of our life has long been recognized. Most fundamentally, education provides us with knowledge and skills. It teaches us how to think, act, and decide, and it shapes our opinions and beliefs. Given this view, the effects of education are as diverse as the sciences studying them, including the effects on employment prospects, health, fertility, child education, political activity, and social capital. Extensive reviews of the costs and benefits of education are provided by Card (1999), Emler and Frazer (1999), Wolfe and Haveman (2001), Hillygus (2005), Grossman (2006), Huang *et al.* (2009), Oreopoulos and Salvanes (2011), and the OECD (2006, 2010).

While there is little doubt about the significance of education for a variety of outcomes, the quantification of causal channels is difficult due to at least two reasons. First, education is a choice variable driven by numerous factors, some of which are observed by the analyst, some of which are not. Second, education effects are likely heterogenous over different educational levels and across individuals. If potential confounders and heterogeneity are neglected in a regression-type model, then standard estimators like ordinary least squares (OLS) are biased. A common solution is to use instrumental variables (IV) estimators that explore, for example, changes in the compulsory schooling legislation; see Angrist and Krueger (1999), Card (2001), and the references above for details. A major drawback of IV, however, is the assumption of exogenous instruments that affect the regressand only through education, but that are otherwise unrelated to the outcome process.

Our study approaches the estimation of causal education effects from a different angle. We use a unique feature of the Swiss education system until the late 1990s, where pupils at the age of about 12 had to pass a centrally organized exam (the so-called "Sekprüfung"). The exam result determined the level of secondary school pupils could attend and ultimately whether they could access university education. Thus, the exam is expected to have a large impact on the highest education achieved. For estimation purposes, we can take advantage of an almost sharp discontinuity in the classification scheme. Pupils above a certain threshold were classified in upper level secondary school, pupils below the threshold in lower level secondary school. By design, teachers were unable to strategically sort, because grading was done by external experts. This, allows us to analyze the education outcomes of pupils near the discontinuity as if they would come from a randomized experiment.

The present paper reports the results from a pilot study that we conducted in order to check

the feasibility of our approach. The structure is as follows. First, we briefly describe the Swiss education system and the particularities of the entry test. Second, we provide details about the data collection (identification of schools and pupils, follow-up survey). Third, we provide initial evidence for the credibility of the regression discontinuity (RD) design, and estimate causal education effects for three different outcomes: political interest, attitudes toward foreigners, and earnings. While these outcomes are clearly selected according to our own interests and background, our results indicate the high potential of the proposed identification strategy and the possible generalization to a number of other outcomes. This and the extension to a larger, representative dataset constitute current research efforts.

2 Swiss education system and school entry tests

2.1 General information

The authority over the Swiss education system is divided between the federal state, cantonal, and municipal level. The Swiss constitution (art. 62/1) guarantees that every canton can set up its own system while being in line with the federal rules. As a consequence, we observe a substantial cantonal variation, e.g., in the age of school enrollment, the division of primary and secondary school, and the quality of education.[1] The recent 2010 report of the Swiss Coordination Centre for Research in Education (SKBF-CSRE 2010) provides an excellent overview of the Swiss education system.

Despite these differences, the basic structure of the system is the same for all cantons. It is given by four levels: primary, lower secondary, upper secondary, and tertiary (see Figure 1). Primary and lower secondary school are compulsory and free of charge. In most cantons, the former lasts six years, the latter three years (some cantons also know the five/four-model). The lower secondary school prepares for basic vocational and general education. Pupils are classified into different groups according to their performance. While *Werk-, Real-* and *Sekundarschule* prepare for different levels of vocational training, the *Vor-* and *Untergymnasium* constitute the basis for general education and university.

The upper secondary school consists of vocational training and general education. Vocational training takes place in companies while spending one day in a school offering specialized courses. It lasts two to four years depending on the complexity of the job. General educa-

[1] See the report of the Forschungsgemeinschaft PISA Deutschschweiz/FL (2005: 163).

tion consists of "matura" schools or special middle schools and takes three to four years. A "matura" degree from general upper secondary schools allows for free admission to university.[2] Cantons generally differ in the design of "matura" schools. The traditional way in most cantons is a 6-or 7-year high-school after completing primary school, but recent reforms since 1995 offer more flexibility for students on the vocational track to switch to upper secondary education.

Universities as well as higher vocational education and training build the two branches of the tertiary level. The latter provides expertise to graduates of a 3- to 4-year basic vocational training. It is a Swiss particularity rarely seen in other countries (but also exists in Germany and Austria). The universities offer a wide range of curricula and are publicly funded. There are ten cantonal universities and two Federal Institutes of Technology. Furthermore, there are seven universities of applied science which offer more practice-oriented courses for individuals with a vocational background. Beside this, teacher training colleges educate primary and secondary teachers.

After compulsory school about two thirds of pupils start vocational training, compared to about one third in general education. Since 1995, there is an increasing number of students pursuing a professional matura career; about 11.8% in 2007 according to the Swiss Federal Office for Statistics (2010). This certificate allows entering the universities of applied science. There has also been an increase in regular matura degrees which are a prerequisite to start university (from around 14.9% of total graduates in 1990 to 19.4% in 2007). As a consequence, the total amount of university students has almost doubled from 1990 to 2010 (Swiss Federal Office for Statistics 2011). Currently, around 18% of the working population hold a university degree which is slightly below the average of other OECD countries (SKBF-CSRE 2010).

2.2 Entry tests

An important feature of the transition from primary to secondary school has been a centralized exam at the end of primary school (the "Sekundar-" or "Gymnasialprüfung").[3] This test has served as a central instrument to differentiate individual skill levels. In addition, cantonal matura schools were limited in financial means and capacity to take all candidates.[4] Pupils

[2] Access to some university degrees, such as medicine, is restricted and require an additional centralized exam. Herren (2008) and the SKBF-CSRE (2010) provide additional information about the tertiary system.

[3] As a consequence of the increased possibilities to change from the vocational to the university track, some cantons (such as Zurich) require an exam for individuals who want to pursue the university track.

[4] This was especially true for cantons where pupils were sent to convent schools. Moreover, until the mid 1970s there were few cantonal matura schools (Meylan 1996).

were usually around 10 to 12 years old when taking the exam (depending on the cantonal education system), and in most cases, they could retake the test in the following year.

The entry test created a discontinuity in the assignment to secondary school that was very important for both the length and the quality of education. We expect the classification to be especially sharp up until the 1990s for three reasons. First, it was very difficult to change from the vocational track to the university track, and thus the entry test was very decisive in restricting the access to higher education. Second, the entry test was often the unique decision rule for entering secondary school. It was only in the late 1980s when many cantons decided to consider both the test and pre-test achievement. Third, due to economic constraints we expect that, on average, back then pupils were less likely to retake the exam than today.

The exam as well as the assignment rule were designed by the ministry of education, sometimes in cooperation with the head of the secondary school. Grading was done by external experts and thus could not be manipulated by class teachers. The threshold for passing the exam was determined either by the relative rank in a given school and year in order to account for potential capacity constraints, or by the absolute number of points. In particular for the former case, we expect no sorting bias by the pupils because the threshold could not be anticipated in advance. By official regulations, the exam results had to be conserved for at least 10-15 years either in the archives of the school district or in the cantonal archives.

Due to the cantonal and sometimes municipal heterogeneity there is no complete picture about the entry test in Switzerland. We know that most cantons did have this entry test and abolished it around 1985 ($\pm$ 10 years). Some cantons, cities and municipalities such as the canton of Zurich, or municipalities in the cantons of Schwyz and Schaffhausen still rely on an entry test. It is part of our current research efforts to conduct a large-scale survey of municipalities in Switzerland to obtain detailed information about the "Sekprüfung".

3 Data

We collected individual test scores from entry exams in different schools in Switzerland. Multiple steps were necessary in order to collect these data. First, we collected data on all schools that had an entry test and still have the results stored in their archives. Then we consulted the cantonal data protection officers to get permission for the access to individual exam information. The negotiating process was relatively demanding since access to non-anonymous information on individuals is strongly restricted in Switzerland. An official agreement with

the canton allowed us to collect the addresses of former pupils. We worked with different municipalities to trace current residence.

In this pilot study, we solely focus on pupils from Neuhausen in the canton of Schaffhausen. Meanwhile, we have collected data from 12 other municipalities, and we are negotiating with about 50 more. The entry tests in Neuhausen decided between *Real-* and *Sekundarschule* and pupils were between 12 and 16 years at the date of the exam. Since only *Sekundarschule* allowed for a later change to university track, the test was critical in determining higher education prospects. The exam included reading, writing, and math and the threshold was 20 out of 50 points. It was possible to retake the test, and about 18.3% of our sample did so. Around 75% of them passed in the second exam but none of them acquired higher education.

We confine our analysis to pupils close to the cutoff because they are assumed to be similar in characteristics. We collected addresses of pupils who were at maximum three points away from the threshold (in absolute value). In a telephone survey, we asked them about their educational degrees, professional activity, political attitudes, and labor market outcomes.[5]

Our final dataset consists of 263 individuals.[6] About half of them (49.8%) are male and the mean age is 27.95 years. Table 1 shows that about 5% hold a university degree. This share is relatively low in Swiss-wide comparison, but to be expected given the rural sampling region we focus on. We notice that among those who passed the entry test about 8% graduated from university, while nobody from those who did not pass the exam holds a university degree. This reflects the fact that barriers to higher education are extremely high for people in classes preparing for mechanical vocational education (*Realschule*). Moreover, the fraction of men is slightly higher among those who passed, but the difference is not significant.

Considering our political variables, only about 11% state to be "very interested" in politics. Among pupils who passed the entry test this number is significantly higher than among non-passers (14.8% vs 3.4%). We focus on the highest of four categories because we think that this is the more precise measure for interest in politics in general. But still, if we compare the upper two categories, the difference in means is significant (66.9% vs 53.4% with p-value of 0.017). Looking at attitudes toward foreigners, the fraction of individuals stating that they find foreigners "enriching" is higher among entry test takers who succeeded (19.6% vs 9.4%).[7]

[5]The complete survey is available for download from http://staff.vwi.unibe.ch/boes/research.html.

[6]Initially, we had addresses of around 500 pupils, but we could not contact them all during our survey period of three weeks. Among the contacted pupils, we achieved a response rate of more than 90%.

[7]Here we also take the highest category because we think that the middle categories are not very meaningful due to possible social responsiveness.

As a final outcome, we calculated a variable representing yearly wages after social security payments including possible additional end-of-year salaries. Then we dropped all individuals with average income lower than 40'000 SFr which is about the average minimum wage across different sectors.[8] Finally, we constructed a variable indicating wages in the top quartile of the wage distribution. Individuals who passed the test are significantly over-represented in the high wage group (26.5% vs 15.5%). In the next two sections, we analyze to which extent the variations in these outcomes can be causally related to different educational achievements.

4 Identification strategy

Conceptually, we think of education effects in terms of the following model

$$Y = \pi_0 + \pi_1 E + \varepsilon \tag{1}$$

where Y is the outcome variable (e.g., political interest, attitude toward foreigners, earnings, as considered here, or any other outcome of interest), E is the individual's highest education achieved, and ε is an error term; π_0 and π_1 are parameters. In this framework, the causal education effect is given by π_1 because it measures the change in Y for a *ceteris paribus* change in E. Equation (1) must be understood as a simple approximation to $Y = f(E, \varepsilon)$, the choice of f depending, among other things, on the scaling of the outcome variable and the properties of the underlying theoretical model. Estimation of (1) by ordinary least squares (OLS) methods will provide a consistent estimator of the causal education effect if E and ε are independent, and the education effect π_1 is constant across individuals

Both assumptions, independence and constant effects, have been rigorously questioned in the literature, a common source of bias being innate ability, and heterogeneity, of individual decision-makers. In a nutshell, the bias arises because different individuals make different educational choices, and thus variations in outcomes can be due to a causal education effect, and/or due to different individual characteristics. We can correct for *observed* background factors by including control variables (or use more sophisticated matching or weighting methods in doubt of functional form misspecification). Differences in *unobserved* factors can be coped with instrumental variables (IV) methods. Such methods explore variation in E exogenous to the individual. For a discussion of these and related aspects see Angrist and Krueger (1991),

[8] Since our sample includes students and young mothers we have a lot of part-time workers in the pilot data. The minimum wage depends on the general contracts in the sector and varied between 3000 and 4000 SFr. in 2008 (Oesch 2008: 26).

Card and Krueger (1992), Card (1995), Angrist *et al.* (1996), Ashenfelter and Rouse (1998), Card (1999), and more recently the special issues of the Journal of Economic Literature (2010: 48, 2) and the Journal of Economic Perspectives (2010: 24, 2).

In this study, we take advantage of school entry tests in Switzerland in order to infer a causal education effect. As outlined above, a single exam determined the level of secondary school pupils could attend according to their—in some municipalities relative, in other absolute result—in a given school/year. Methodologically, this design is referred to as regression discontinuity (RD) design; see Imbens and Lemieux (2008) and Lee and Lemieux (2010) for recent surveys. The validity of the RD design crucially depends on the condition that individuals are unable to precisely manipulate the assignment mechanism, which we believe is a credible assumption in our case because neither pupils nor teachers were able to strategically sort. As a consequence, even while some pupils were more likely near the threshold than others, the probability of being just above or just below the threshold was approximately the same for all of them. This allows us to analyze the education outcomes of pupils near the threshold as if they were generated from a randomized experiment. We will provide some evidence for the plausibility of local randomization below.

Empirically, we deal with a fuzzy RD design (FRD). The key characteristic of FRD is that the educational degree is non-deterministic in the test score. This implies that the probability of obtaining a higher educational degree is larger for individuals just above the threshold than for individuals just below. The jump is discrete, but not equal to one. The FRD fits our data well because i) some individuals may pass the exam in the next year, or ii) change school levels at a later stage, and iii) most pupils who did pass did not attend university later. Hence, compliance is far-away from perfect, but almost one-sided. Let τ denote the threshold and S the test score, then the causal education effect is given by

$$\Delta_{FRD} = \frac{\lim_{s\downarrow\tau} \mathrm{E}(Y|S=s) - \lim_{s\uparrow\tau} \mathrm{E}(Y|S=s)}{\lim_{s\downarrow\tau} \mathrm{E}(E|S=s) - \lim_{s\uparrow\tau} \mathrm{E}(E|S=s)} \tag{2}$$

The estimand in (2) is equivalent to an IV estimand if the functional form specification of S, and the data window, are the same in the first and second stages, specified as

$$E = \alpha_0 + \alpha_1 Pass + f^-(S-\tau) + f^+(S-\tau) + \epsilon \tag{3}$$

$$Y = \beta_0 + \beta_{FRD}\widehat{E} + g^-(S-\tau) + g^+(S-\tau) + \nu \tag{4}$$

The variable $Pass$ is equal to one if the individual passed the exam, and thus $S \geq \tau$, and zero otherwise. The functions f and g are same degree polynomial functions to the left $(-)$

and to the right (+) of the threshold, with argument normalized to zero at the threshold. In the simplest case, these could be linear functions with different slopes on either side. $\widehat{E}$ is the prediction from the first stage. The estimator of β_{FRD} is a two-stage least squares (2SLS) estimator. Note that this estimation is based on the subpopulation of compliers, i.e., those who would complete a higher educational degree if they had passed the exam, and would not complete a higher educational degree if they had not passed the exam. By construction, this is confined to the population of pupils at the threshold.

The identification strategy generalizes to non-linear models, e.g., if the first and second stage outcome variables are binary, or ordinal. In this case, we may specify

$$E^* = \gamma_0 + \gamma_1 Pass + f^-(S-\tau) + f^+(S-\tau) + \epsilon \tag{5}$$

$$Y^* = \delta_0 + \delta_1 E + g^-(S-\tau) + g^+(S-\tau) + \nu \tag{6}$$

where E^* and Y^* are latent variables, and the observed variables are given by $E = 1(E^* > 0)$ and $Y = 1(Y^* > 0)$ in the case of a binary variables, for example. Although, we might in principle follow a semi- or non-parametric approach, the limited sample size in our pilot study leads us to specify a bivariate standard normal distribution of (ϵ, ν). See Greene (2007) and Wooldridge (2010) for a detailed description of the bivariate probit model, and Ozier (2010) for a related approach in the RD context.[9] Alternatively, we may allow for a non-linear model of the outcome variable by using non-linear least squares and apply the weighting approach of Abadie (2003) in order to identify the causal effect for the subgroup of compliers; see also Angrist (2001), Abadie *et al.* (2002), and Frandsen *et al.* (2010).

5 Results

5.1 First stage discontinuity and local randomization

For our pre-study, we selected a symmetric data frame of ±3 points around the threshold. The discontinuity in university degrees by distance to the threshold is illustrated in Figure 2. In the pilot sample, none of the individuals just below the threshold subsequently obtained a university degree. About 8% of the pupils who marginally passed the exam finished university.

[9] Ozier (2010) employs a similar identification strategy as in this paper. He uses administrative data on test scores and survey data, and shows that self-reported test scores are subject to substantial misreporting. We avoid this problem, too, by collecting administrative records of entry tests. The key advantages of our data are the detailed information about the institutional assignment mechanisms without relying on estimated break points in the probability of "passing" as a function of the test scores, and that we can plausibly rule out strategic sorting.

Table 2 provides linear regression estimates of the discontinuity, allowing for a non-zero slope in distance to the right of the threshold. The shift of 9.5 percentage points is significantly different from zero at the 1% level (column 1), but the slope is small in magnitude and insignificant. If we compare mean ages and the proportion of men to the left and to the right of the threshold, then we observe neither a significant shift at the discontinuity point, nor a significant relationship to the distance measure (compare also Figures 3 and 4).

While these findings are indicative of the validity of the RD design, we are clearly limited by the scale of our pilot study. Additional tests need to be carried out as soon as the whole data are collected, following the recommendations of Lee and Lemieux (2010). Parental education, for example, may be a potential confounder of the shift in educational degrees at the discontinuity point, and we will gather detailed background information in order to rule out such alternative explanations.[10] Furthermore, our small sample did not allow for a powerful test of the change in the test score distribution near the threshold, which would question local randomization. Some preliminary results, however, do not support this hypothesis.

In Table 3, we report the regression estimates of various specifications of the first stage. Column (1) reproduces the results of Table 2 with the constant and the slope in distance to the left of the threshold set to zero. The latter restriction will be imposed in all regressions because it remains to hold empirically even when adding controls (i.e. no pupil attained a university degree if she did not pass the threshold). Column (2) adds gender and age, but the results do not change compared to (1), as expected. Columns (3) and (4) evaluate the sensitivity of the first stage to the choice of data frame, restricting the observations to the distance interval (-2,2) and (-1,1) around the threshold, respectively. The estimated shift becomes somewhat larger the tighter the range of observations, but we lose statistical significance due to the smaller sample size. For the remaining analyses, we will keep the (-3,3) interval as a reasonable choice given the trade-off between validity of local randomization and sample size.

5.2 Education effect on political interest

The first causal effect we look at is the impact of education on political interest. The dependent variable is binary and indicates whether the respondent is interested in politics, and political topics, in general (as opposed to little or no interest). The fraction of positive outcomes in the sample is about 11%, and we observe a significant difference between individuals with a

[10] See Urquiola and Verhoogen (2009) in a related context with class size as underlying running variable.

university degree and individuals without, the former reporting an about 33 percentage points higher probability of being interested in politics (see columns 1 and 4 in Table 4). The question is whether the difference can be related to a causal relationship, for example because the better educated have additional knowledge about their influence on policy-making, or are more likely to work in jobs directly related to political topics, or whether the difference is confounded by unobserved common factors (such as preferences, tastes, or ability).

In order to separate a causal effect from confounding changes, we explore the discontinuity in test scores and apply three different estimation methods: linear 2SLS, causal IV probit, and bivariate probit. Columns 2, 3 report the first and second stage estimates for the linear probability model. As before, the shift in university degrees at the threshold is positive and significant in the first stage. It is somewhat smaller than the estimates reported in Table 3 because we abstract from the distance measure. The education effect substantially increases compared to OLS. A Hausman test clearly rejects the null hypothesis of exogenous education. The comparison of IV and OLS indicates a downward bias in OLS. Possible explanations for this observation are a negative relation between confounders and education, measurement error, or the restriction to local average education effects. Although the latter explanation, that the compliers above the threshold have particular high returns to education seems plausible and is in line with previous studies (e.g. Angrist and Krueger 1991, Card 1999, 2001), the present data do not allow for a deeper analyis of this issue, but we will pick up the discussion when we re-estimate the models using the full sample.

A major concern about the linear IV probability model is that the 2SLS, or FRD, estimand is the ratio of two probability effects. Depending on the relative magnitude, this ratio might well exceed the limits of zero and one for a probability, as in our case. We therefore provide two alternative estimates that account for possible non-linearities. The causal IV probit estimator is based on a probit specification for the conditional mean of the dependent variable, estimated by non-linear least squares, and weights imposed according to the Abadie (2003) procedure in order to identify the local average education effect; see also Angrist (2001) for a related application. The estimates of the causal IV probit model are provided in column 5. Compared to the standard probit model, the education coefficient substantially increases, as in the linear model. The discrete probability effect (evaluated at mean values) reported in the last row allows for a direct comparison of results. The probit model predicts a 32.4 percentage point increase in the probability of being interested in politics, the causal IV probit model predicts

a 42.8 percentage point increase.

As a final alternative, we provide estimates of a bivariate probit model. We note that the first stage is specified as a probit, and we do not observe individuals with a university degree just below the threshold. In order to avoid perfect prediction, we randomly recode the education variable for 3% of the sample (see also Angrist, 2001). This generates measurement error such that the reported estimates are somewhat attenuated. We evaluated the bias under the exogeneity assumption, which was of magnitude 10-15%. Despite this fact, for the present analysis we deem this approach reasonable because (i) the bivariate probit seems a natural choice for the binary regressand/regressor case, (ii) perfect prediction will likely disappear in the full sample, (iii) we have a limited sample in our pilot, and (iv) the attenuation bias will vanish with larger the sample size. The estimates of the bivariate probit model are in line with 2SLS and causal IV probit. The results indicate that the probability of being interested in politics increases by 58.5 percentage points for individuals with university degree, which is a lower bound on the causal education effect given attenuation bias.

5.3 Attitude toward foreigners

As a second outcome we look at the self-reported attitude of individuals towards foreigners. The dependent variable is again binary and indicates whether foreigners are a valuable addition or rather a thread to society. The fraction of people strictly in favor with this statement is about 16%. However, we observe a significantly larger support of individuals with a university degree (about 54% as opposed to about 12% for those without a university degree), even after controlling for gender and age. Part of this difference can be explained by the educational differences of individuals, but those individuals, in general, also differ in other factors, such as family background, personal environment, preferences, and the like. The extent to which differences are related to education can only be judged by exploring an exogenous source of variation.

Table 5 reports the results of three different models that account for the potential endogeneity of education (in addition OLS and Probit estimates which treat education as exogenous). Like before, we take advantage of the discontinuity of educational degrees in test scores to construct a first stage estimate. The linear probability 2SLS model suggests that the difference in attitude toward foreigners between individuals with and without a university degree is even larger (about 94 percentage points). The estimate is significant at the 10% level, but violates

the logical requirement for a probability effect to provide predictions within the unit range.

The causal IV and bivariate probit estimators (columns 5-7) solve this problem by imposing a non-linear model structure. The former predicts an education differential of 52.7 percentage points, the latter a differential of 80.7 percentage points. These estimates are significantly different from zero, and indicate a large gap in attitudes toward foreigners between high and low educated individuals. Our results are in line with Chandler and Tsai (2001) and also Hainmueller and Hiscox (2007, 2010) who show that education is the most important factor in explaining views towards immigrants. The comments regarding complier effects, random recoding, and attenuation bias apply as before. We want to point out, however, that we find large and significant education effects despite these facts and despite the small sample.

5.4 Return to education

As a final exercise, we evaluate the causal effect of education on yearly earnings. The monetary returns to education are among the most thoroughly studied economic effects in the literature; see Card (1999, 2008) for a review. Due to data limitations, we confine ourselves to an indicator for the work income being in the top quartile. We apply the same model specifications as for the political outcomes; the results are reported in Table 6. Under the exogeneity assumption, the probability of a top quartile income is predicted to be about 28.1 percentage points higher for individuals with a university degree than for those without. This prediction is significant at the 5% level and in line with other studies, for example the 2010 report of the Swiss Coordination Centre for Research in Education (SKBF-CSRE 2010).[11] Swiss evidence based on exogenous variation is very limited, though, due to the lack of credible instruments. Using the discontinuity in test scores, we fill this important gap.

When we account for potential endogeneity of education, then the estimated return substantially increases. The causal IV probit estimator, for example, predicts an increase by 45.1 percentage points, the bivariate probit model an even higher increase by 79.7 percentage points, which is about twice the size of the effect under exogeneity. The results are largely consistent with the international evidence on causal education effects with IV estimates up to 50-80 percent larger than OLS for the years of schooling (see e.g. Angrist and Krueger (1991),

[11] See Wolter and Weber (2003, 2005) for a summary of the Swiss evidence. Much of the Swiss literature is concerned with the complexity of the Swiss education system, and its opportunities to complete an educational degree. Most importantly, the approximation of education by the years of schooling is shown to be poor. In the present analysis, we avoid this problem by restricting attention to a university degree. In the full sample, however, we will extend the RD framework to estimate a whole set of returns for each of the major degrees.

and Card (2001)).

Two remarks apply regarding the results. First, the bias in OLS and Probit is somewhat counterintuitive to the common notion of ability bias, which states that the more able individuals are more likely to complete a university degree, and have a higher probability of getting an income in the top quartile (which would bias the estimator upward). While we cannot rule out this type of bias, alternative explanations such as non-trivial measurement error and the restriction to compliers are equally relevant, in particular when looking at the discontinuity in test scores. We expect that these individuals, on average, have a higher return because i) if they pass, they attain a university degree and therefore expect to have a higher than average return to education and ii) if they would not pass the entry test, they would only achieve a relatively low degree.

Second, the heterogeneity of returns is highly relevant because of the complexity of the Swiss education system. The above cited report (SKBF-CSRE 2010) indicates that wage differentials are a non-linear function of educational degrees, with a particular distinction between "traditional" secondary and tertiary education, (higher) vocational education, and continuing education and training. Depending on the final sample size, and the time frame of accessible results for the entry exams, we are able to differentiate at this level and provide, for the first time, a large set of causal effects for different educational degrees. These estimates will be relevant not only in the Swiss context but for other countries, too, especially in Europe, where vocational education is part of the current educational policy debate.

6 Conclusion and Outlook

The paper identifies secondary school entry tests in Switzerland as a reliable, and arguably exogenous source of shifts in educational degrees, and provides striking RD evidence of causal education effects for three outcomes: political interest, attitudes toward foreigners, and earnings. Our analyses so far are limited by the size of the pilot sample, and the next step will be to collect more, and more representative, data. The outcomes we have in mind are demographic variables (such as civil status, family situation, the number of kids), employment characteristics, several health indicators (such as height, weight, number of doctor visits, smoking status), child outcomes (such as birthweight, school attendance), preference parameters (such as risk and time discounts), political outcomes (such as party identification, participation, attitudes), and indicators of social capital (such as altruism, trust, social participation).

At the end, we expect to have a unique dataset that for the first time combines rich socio-economic and political-psychological information with a powerful source of exogenous variation. In contrast to previous studies that instrumented education by features of the compulsory schooling legislation, or the proximity to schools, the secondary school entry tests in Switzerland generate a variation in the education of pupils near the threshold that can be analyzed as if it would come from a randomized experiment. Our pilot sample is indicative of the local randomization. This aspect is the key advantage of our data over all previous work.

Our analyses will profit in many respects from the originality of the data. We contribute to a better understanding of common education effects, such as those on earnings, employment, health, or turnout. But we also enhance our knowledge on causal education effects that have not been thoroughly studied before (such as those on preferences, attitudes, and social capital). From a methodological point of view, we contribute to the literature by applying modern RD methods. Since many of our outcomes are discrete, we are interested in features of the target distribution beyond common mean effects (such as quantiles, probabilities, or spread parameters). Thus, the experiences we gain from the analysis of our data will provide important input for other empirical work taking advantage of RD designs.

Finally, our results will inform policy-makers about major educational policy concerns in Switzerland and elsewhere. In many Western countries, large reforms of the education system are under way, but this far-reaching policy changes are more often than not based on a thin empirical foundation. Fully understanding the causal effects of education is crucial for a systematic, and well-founded policy discussion, and we will feed this discussion with extensive empirical evidence.

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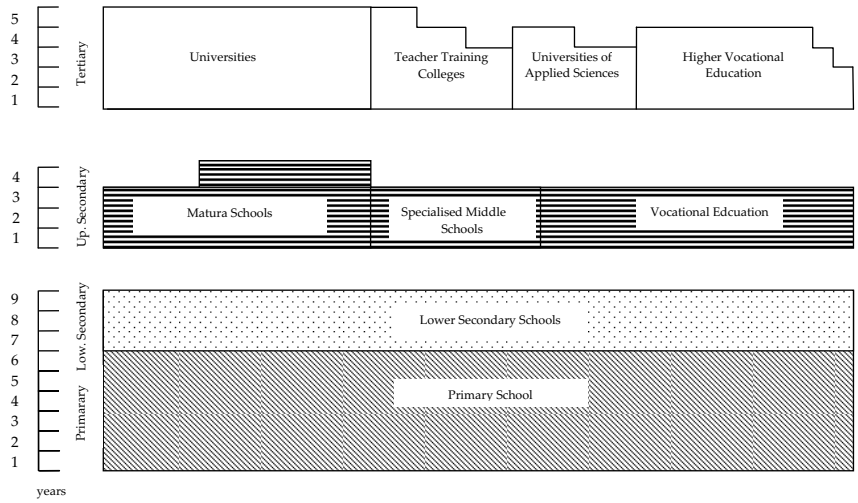
Tables and Figures

Figure 1: Swiss education system



Notes: A simplified overview about the Swiss education system (modified version of the figure in the yearbook of the Swiss Federal Office for Statistics (2010: 355).

Table 1: Characteristics of the pilot sample

	Overall	N	Pass	N	No pass	N
University degree	0.053	263	0.080	175	0	88
	(0.224)		(0.272)			
Political interest	0.110	263	0.148	175	0.034	88
	(0.313)		(0.356)		(0.183)	
Attitude toward foreigners	0.163	165	0.196	112	0.094	53
	(0.371)		(0.399)		(0.295)	
Income in top quartile	0.229	175	0.265	117	0.155	58
	(0.421)		(0.443)		(0.365)	
Age	27.95	263	28.28	175	27.31	88
	(3.91)		(3.79)		(4.11)	
Male	0.498	263	0.514	175	0.465	88
	(0.501)		(0.501)		(0.501)	

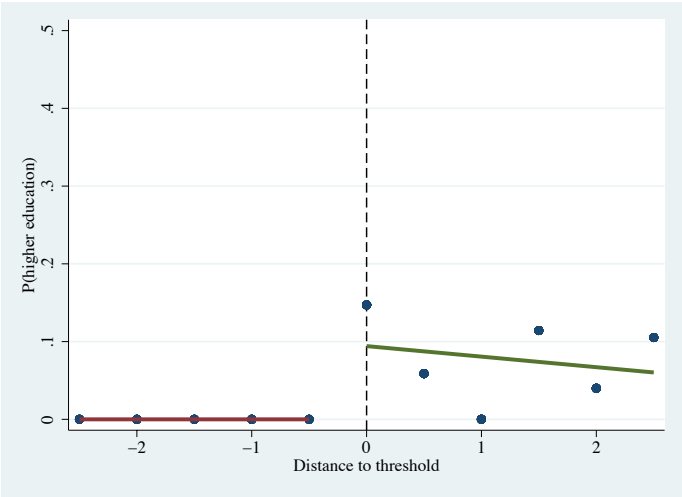
Notes: University degree (yes=1/no=0). Interest in politics in general (yes=1/no=0). Positive attitude toward foreigners (yes=1/no=0). Income in top quartile (yes=1/no=0). Reported numbers are mean values. Standard deviation in parentheses.

Table 2: Linear regression checks for validity of RD design

	University degree	Age	Male
Pass	0.095***	0.048	0.014
	(0.028)	(1.119)	(0.144)
Distance * No pass	0	0.384	0.018
		(0.609)	(0.078)
Distance * Pass	-0.014	0.305	0.006
	(0.021)	(0.363)	(0.047)
Constant	0	27.89***	0.493***
		(1.00)	(0.129)

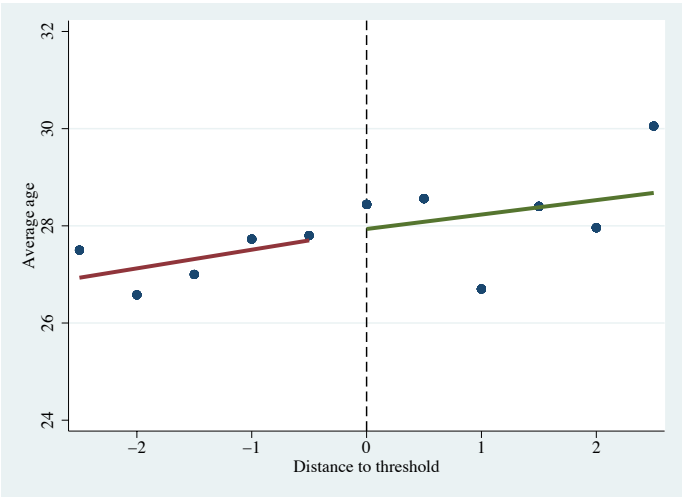
Notes: Pass is equal to one if the test score is weakly larger than the threshold, and zero otherwise. Distance equals the test score minus the value of the threshold. Standard errors in parentheses. *** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$

Figure 2: University degrees by test scores



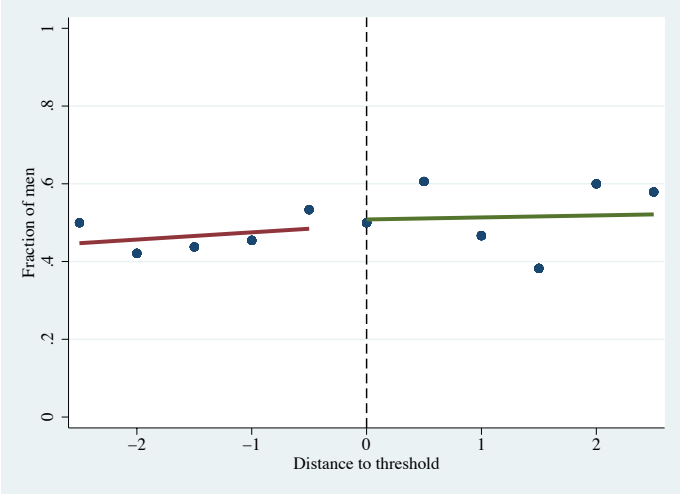
Notes: Scaling on horizontal axis for distance to threshold. Students pass the exam for distance weakly larger than zero (to the right of the dashed vertical line).

Figure 3: Average age by test scores



Notes: Scaling on horizontal axis for distance to threshold. Students pass the exam for distance weakly larger than zero (to the right of the dashed vertical line).

Figure 4: Fraction of men by test scores



Notes: Scaling on horizontal axis for distance to threshold. Students pass the exam for distance weakly larger than zero (to the right of the dashed vertical line).

Table 3: First stage linear regressions

	All		$\lvert Distance \rvert < 2$	$\lvert Distance \rvert < 1$
Pass	0.095***	0.093***	0.101**	0.146*
	(0.028)	(0.037)	(0.047)	(0.087)
Distance * Pass	-0.014	-0.014	-0.024	-0.187
	(0.021)	(0.021)	(0.036)	(0.137)
Age		0.001	0.001	0.004
		(0.004)	(0.004)	(0.008)
Male		0.027	0.033	0.062
		(0.027)	(0.035)	(0.063)
Constant	0	-0.050	-0.035	-0.133
		(0.099)	(0.129)	(0.242)
Number of observations	263	263	184	82

Notes: Columns 1,2 with absolute distance to threshold less than 3 points. Columns 3,4 with distances less than 2 and 1 points, respectively. Standard errors in parentheses.
*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$

Table 4: Causal effect of education on political interest

	OLS	2SLS		Probit	Causal IV	Bivariate probit	
		University	Interest		probit	University	Interest
University degree	0.330***		1.474**	1.121***	2.142***		1.818**
	(0.084)		(0.674)	(0.359)	(0.662)		(0.893)
Age	-0.002	0.001	-0.004	-0.009	-0.108	-0.023	0.001
	(0.004)	(0.004)	(0.006)	(0.028)	(0.090)	(0.029)	(0.027)
Male	-0.045	0.027	0.011	0.243	0.856*	0.461**	0.133
	(0.038)	(0.028)	(0.053)	(0.214)	(0.500)	(0.230)	(0.221)
Pass		0.077***				0.830***	
		(0.029)				(0.286)	
Constant	0.114	-0.046	0.143	-1.200	0.272	-1.504*	-1.466**
	(0.136)	(0.099)	(0.177)	(0.796)	(1.108)	(0.826)	(0.763)
University degree DPE				0.324**	0.428**		0.585*
				(0.134)	(0.204)		(0.322)

Notes: University degree (yes=1/no=0). Interest in politics in general (yes=1/no=0). Pass equals one if the test score is weakly larger than the threshold, zero otherwise. Coefficients of latent model for probit-type models. Bivariate probit with random recoding of university degree in order to avoid perfect prediction (3% of sample). DPE = Discrete probability effect. $N = 263$. Standard errors in parentheses.
*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$

Table 5: Causal effect of education on attitude toward foreigners

	OLS	2SLS		Probit	Causal IV	Bivariate probit	
		University	Attitude		probit	University	Attitude
University degree	0.421***		0.943*	1.315***	1.946***		2.607***
	(0.102)		(0.551)	(0.382)	(0.742)		(0.210)
Age	-0.008	0.002	-0.009	-0.039	0.009	-0.006	-0.023
	(0.007)	(0.005)	(0.008)	(0.034)	(0.090)	(0.034)	(0.030)
Male	-0.077	0.029	0.092	-0.327	-0.897	0.185	-0.423*
	(0.056)	(0.041)	(0.061)	(0.248)	(0.684)	(0.232)	(0.218)
Pass		0.114**				0.872**	
		(0.045)				(0.349)	
Constant	0.387*	-0.070	0.401	-0.111	-1.548	-1.720*	-0.399**
	(0.201)	(0.152)	(0.214)	(0.933)	(1.254)	(0.958)	(0.842)
University degree DPE				0.439***	0.527***		0.807***
				(0.143)	(0.156)		(0.135)

Notes: University degree (yes=1/no=0). Positive attitude toward foreigners (yes=1/no=0). Pass equals one if the test score is weakly larger than the threshold, zero otherwise. Coefficients of latent model for probit-type models. Bivariate probit with random recoding of university degree in order to avoid perfect prediction (3% of sample). DPE = Discrete probability effect. $N = 165$. Standard errors in parentheses.
*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$

Table 6: Causal effect of education on income in top quartile

	OLS	2SLS		Probit	Causal IV	Bivariate probit	
		University	Income		probit	University	Income
University degree	0.381***		1.101	1.137**	1.340**		2.552***
	(0.130)		(0.885)	(0.454)	(0.618)		(0.246)
Age	0.033***	0.003	0.030***	0.155***	0.082	-0.043	0.156***
	(0.008)	(0.004)	(0.009)	(0.038)	(0.064)	(0.034)	(0.037)
Male	0.165***	0.037	0.138*	0.726***	0.851	0.104	0.603**
	(0.059)	(0.034)	(0.072)	(0.263)	(0.622)	(0.255)	(0.239)
Pass		0.074**				0.777**	
		(0.035)				(0.349)	
Constant	-0.815***	-0.098	-0.764***	-5.864***	-3.955***	-0.712	-5.902***
	(0.218)	(0.127)	(0.243)	(1.172)	(1.216)	(0.983)	(1.143)
University degree DPE				0.381**	0.451**		0.797***
				(0.177)	(0.166)		(0.042)

Notes: University degree (yes=1/no=0). Income in top quartile (yes=1/no=0). Pass equals one if the test score is weakly larger than the threshold, zero otherwise. Coefficients of latent model for probit-type models. Bivariate probit with random recoding of university degree in order to avoid perfect prediction (3% of sample). DPE = Discrete probability effect. $N = 175$. Standard errors in parentheses.
*** $p < 0.01$ ** $p < 0.05$ * $p < 0.1$