Online Appendix: 'A track too far? The effect of general versus vocational upper secondary education on voter turnout'

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Supplementary material for online publication.

A General and vocational education in Europe

Figures 1 and 2 in the article and Table A.1 below use data from the European Social Survey (ESS) and from the OECD Education at Glance series. The ESS data are weighted using the ESS design weights, taking account of the stratified sampling procedure. We describe the variable definitions below.

- **Vocational enrolment**: Data from Education at Glance 2023. Table B1.3. *Profile of students enrolled in vocational programmes (2021)*: Vocational students as a share of all upper secondary students.

- **Vocational education in population**: Cumulative ESS data 2010–2020. Highest level of education according to the ESS detailed cross-national educational attainment variable: *edulvlb*. This variable is closely linked to ISCED 2011. For the the upper secondary (ISCED 3) and the post-secondary levels (ISCED 4), we follow the ESS documentation and use the second digit in *edulvlb* to identify educational orientation: 1=general, 2=vocational (Schneider 2020). This classification follows the ISCED 2011 definition of programme orientation. For the tertiary level (ISCED 5) we code *ISCED 5B short, advanced vocational qualifications* (edulvlb=520) as vocational but all other tertiary orientations as general. Doctoral degrees (ISCED 6) are coded as general.

  We only include respondents with at least upper secondary qualifications (ISCED 3). That is, respondents with only lower secondary education are missing-coded. The main reason for this sample restriction is that a large number of countries do not differentiate between general and vocational orientations on the lower secondary level, meaning all qualifications would be coded as general. Furthermore, this restriction is also in line with the empirical focus of our study on the upper secondary level.

  If we were to also include the lower secondary level in our measure of vocational education in the population, the corresponding share would be 42 per cent (compared to 49 per cent among those with at least upper secondary education).

- **Voter turnout**: We use the following ESS item: *Some people don't vote nowadays for one reason or another. Did you vote in the last [country] national election in [month/year].* We only see to responses from citizens between 18 and 35 years old.

- **Demonstrations**: We use the following ESS item: *Taken part in public demonstration last 12 months.* We only see to responses from citizens between 18 and 35 years old.

- **Boycotting**: We use the following ESS item: *Boycotted certain products last 12 months.* We only see to responses from citizens between 18 and 35 years old.

- **Contacting**: We use the following ESS item: *Contacted politician or government official last 12 months.* We only see to responses from citizens between 18 and 35 years old.

- **Petition**: We use the following ESS item: *Signed petition last 12 months.* We only see to responses from citizens between 18 and 35 years old.

Comparing different forms of political participation in Europe

Table A.1 show descriptive differences in political participation between individuals with general and vocational education in Sweden and in two country groups for five types of political participation (individuals aged 18–35, non-citizens excluded, 2010–2020 ESS data). The ESS data are weighted using the ESS design weights, taking account of the stratified sampling procedure.

Table A.1: Differences in political participation between individuals with general and vocational education in Sweden and in two country groups using ESS data, collapsed means

	Difference gen-voc	Difference gen-voc	Difference gen-voc
Turnout	0.049***	0.107***	0.103***
Demonstration	0.049**	0.042***	0.060***
Boycotting	0.026	0.071***	0.085***
Contacting	0.018	0.033***	0.035***
Petition	0.009	0.082***	0.100***
Countries	Sweden	EEA	EU15
N (turnout)	1965	45313	22645

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Denotes statistical significant differences in means between the two groups in OLS regressions.
EEA: The European Economic Area. All EU member states as well as Iceland, Norway and Switzerland.

B Additional information regarding Swedish upper secondary education and the admission process

Upper secondary education is not compulsory in Sweden but practically all children start studying at this level. The upper secondary programmes take three years to complete for a student who follows the curriculum.

Following the 2011 reform, there are 18 so-called national programmes: 6 general programmes and 12 vocational programmes. National programmes follow a standardized curriculum throughout the country. However, there are also a large number of so-called special programmes (*specialutformade program* or *särskild variant*). These programmes are typically local variants of the national programmes, which do not fully comply with the standardized curriculum and instead includes some type of specialization. There are also so-called preparatory programmes (introductory or individual programmes) for students who are not eligible for the standard national programmes. The idea is that these programmes should give the students eligibility for the national programmes. We describe how we categorize programmes in further detail below in the variable definitions in Appendix C.

Among students who are eligible for the national programmes, more than 99 per cent start studying at upper secondary school (SCB 2018). Among students who are not eligible for the national programmes, 94–97 per cent still start studying but then typically at the preparatory programmes (SCB 2018). See variable definitions in Appendix C for how we handle preparatory programmes. A relatively large share of students do not finish their studies at upper secondary school. Among all students who started in 2015 (including students on preparatory programmes), 72 per cent had finished upper secondary school after three years and 80 per cent after five years. In the case of students who started in 2015 on national programmes, 76 per cent had finished within three years. These figures refer to students who have done the required amount of coursework on the upper secondary level (2500 study points) and received a certificate of their studies (*studiebevis*). However, after the 2011 reform, a new type of degree was introduced at the upper secondary level (*gymnasieexamen*) with additional requirements. Before the reform, finishing upper secondary school meant getting a grade certificate (*slutbetyg*). There was no particular 'degree' from the upper secondary level at this time. We define 'graduation' from upper secondary school in the paper as doing the required coursework and getting a certificate of studies or a grade certificate as this definition is more comparable over time. See variable definitions in Appendix C below.

Reforms, curricula and grading

The institutional description in the manuscript portrays the upper secondary school instituted in Sweden after the 2011 reform. The reform affected students starting upper secondary school from 2011 and onwards. Students who started before 2011 followed the pre-reform upper secondary school throughout their studies.

The reform increased the differentiation between general and vocational programmes by reducing the amount of general subjects on vocational programmes with about one fifth, and instead added more coursework focused on specific occupations (Nylund and Rosvall 2016). This also meant that all students graduating from vocational programmes did not become eligible for tertiary studies, which was the case with the former system. However, vocational students could after the reform choose to take additional courses within their programmes to gain eligibility to tertiary schooling. The reform also made it harder to qualify for upper secondary schooling by requiring a pass grade in a larger number of subjects compared to the former system. Furthermore, new upper secondary school degrees were introduced with a separate vocational and academic degree, where the first is given to those who finish a vocational programme and the latter to those who finish a general programme. In 2022, one aspect of the 2011 reform was retracted and from 2023 all vocational programmes should give eligibility to tertiary studies. However, this change does not affect the cohorts we are studying.

Table B.1 describes the overall distribution of so-called programme wide courses on vocational and general programmes. Programme wide courses are courses that are not specific to a particular programme (i.e. general coursework).

Table B.1: Distribution of programme wide courses across vocational and general programmes in Sweden. Post the 2011 reform.

Subject	Vocational Programme	General Programme
Swedish	100p	300p
English	100p	200p
Mathematics	100p	100-300p
History	50p	50-200p
Social sciences	50p	100-200p
Religion	50p	50p
Natural sciences	50p	0-100p

50 points=50 study hours

In 2011 there was also a reform of the grades in lower secondary school in Sweden. The first cohort of children who graduated from lower secondary school with these new grades finished in 2013. Instead of a four-point grading scale (fail, pass, pass with distinction and pass with particular distinction), the reform implemented a six-point scale (A–F, where F equals fail). There was also a minor change in what subjects that could be included in a student's grade-point average (GPA) from lower secondary school. This reform implies two things that affect the calculation of our running variable, which is based on students' GPA from lower-secondary school. First, the change from a four-point to a six-point scale means that the minimal difference in a student's GPA changed from 5 GPA-points to 2.5. GPA is calculated by transforming the grades to points and summing together a student's 16 or 17 best subject grades. The old four-point scale was transformed to points in the following way: fail=0 points; pass=10 points; pass with distinction=15 points; pass with particular distinction=20 points. The new six-point scale instead applies the following scheme: F=0 points; E=10 points; D=12.5 points; C=15 points; B=17.5 points; A=20 points. This grade reform affects the distribution of the running variable as somewhat more than half of our sample of students starting upper secondary school between 2008 and 2016 have a more coarse GPA-measure than the latter cohorts (2013–2016). Second, the change in what subjects that could be included in one's GPA meant that the maximum GPA changed from 320 for 2008–2012 to 340 from 2013.

Further details on the admission system to upper secondary school in Sweden

The grade-based admission decisions are made sequentially and mechanically via computers. This computer-assisted process should reduce the risk of manipulation by case officers at admission offices. For each programme and school, the applicant with the highest GPA is admitted first, then the one with the second highest GPA, and so on until no free slots remain. Students who fail to gain admission to their preferred programme and school are tested for admission to their second preference. This continues until they are accepted into a programme or until no further programmes remain to which they have applied. This admission procedure has been shown to be strategy-proof, meaning that students have no reason to misreport their ranking of different programmes (Dahl, Rooth, and Stenberg 2023).

Neighbouring municipalities typically cooperate in administering admission to upper secondary school. This implies that students' applications are handled regionally by regional admission offices. Students can apply for both public municipal upper secondary schools and publicly funded voucher schools and they can apply to schools both within and outside of their admission region. However, if they apply for a programme on a public school outside of their admission region, they will only be allocated a slot if the programme is not oversubscribed by students from the admission region.

Sweden has a two-semester system with autumn and spring semesters. Students starts a new grade in the autumn semester. Students apply to upper secondary school during their last spring semester in lower secondary school (9th grade). Before the students submit their applications in May, there is a preliminary admission process that gets underway at the beginning of the spring semester. This preliminary process is based on provisional grades from the preceding autumn semester and functions in a similar way as the final procedure. That is, the students rank their preferred combinations of programme and school; slots are then allocated based on the students' grades. The preliminary process gives the students an idea of how their grades compare with others interested in the same programme and school.

The preliminary admission process gives the students some opportunity to adapt their choice of programme and school, as the students get an estimate of the required GPA for a specific programme. However, this process should not be an issue for the RD design due to that the preliminary process is not informative about the final cutoffs and the exact outcome of the final admission procedure. Their are three main reasons for this uncertainty. First, the preliminary admission process is based on students' provisional grades from the preceding autumn semester, whereas the final admission is done using the students' final grades. All students' grades will therefore differ between the preliminary and the final admission process. Second, students may change their minds between the preliminary and final round. Both of these factors contribute to that the required cutoff for oversubscribed programmes most likely will change between the preliminary and the final admission process. Third, when filling in their final applications in May, the students are also unaware of their own final grades, adding additional uncertainty. Cutoffs in the final admission also fluctuate annually, making them hard to predict for students.

While the preliminary admission process gives the students an idea overall about the chances to become admitted to a certain programme and school, the factors above imply that there is a high level of uncertainty regarding the outcome of the final admission procedure close to the cutoff. This is in line with what is required for the RD design.

C Descriptive statistics and notes on Swedish data

We have the necessary data on students' applications to the upper secondary level for the RD design from 2008 and onwards. The data are collected by regional admission offices and then reported to Statistics Sweden. These data cover practically the complete population of adolescents applying to upper secondary schooling in Sweden each year. We then add data from different administrative registers, including the students' socio-economic background in terms of their parents' education and earnings as well as the students' performance in upper secondary school. We can also follow students' labour market and educational trajectories during early adulthood. However, the most recent year we have such data for is 2021. We also have access to data on voter turnout in the national elections in 2010, 2018 and in 2022 as well as in the elections to the European parliament in 2009 and 2019. These data stem from digitized electoral rolls (Lindgren, Oskarsson, and Persson 2019). There are no population-wide data on turnout available for the national and EP elections in 2014. All of these different data are possible to combine thanks to that the administrative data include unique pseudonymized personal identifiers.

For the main RD analyses in the paper we focus on the cohorts applying to upper secondary school from 2008 to 2016. 2008 is the first year for which we have the necessary data for the RD design. We chose 2016 as the last year because the students starting 2016 will typically have finished upper secondary school at the time of 2019 EP election (although not at the time of the 2018 national election) and have turned 18 and be eligible to vote. However, an additional reason to not add additional cohorts beyond 2016 is that the proportion of oversubscribed programmes decreases for later cohorts, which makes these cohorts less suitable for our RD design.

The data quality of the administrative data from Swedish registers are of exceptional quality, in particular in relation to survey-based data. Socio-economic variables are collected from public registers where, for instance, wage income comes from the tax register and education from the official record of education (*Utbildningsregistret*) administered by Statistics Sweden. The data are typically collected annually in November and generally rely on automatized data collection, which almost eliminates 'non-response' (missing data) and minimizes measurement errors.

The detailed and sensitive character of these data imply that we are under ethical and contractual obligations to handle our data with great care and not distribute or make them accessible to others. The data are stored on secure servers administered by Statistics Sweden and we do all of our analyses using a secure remote desktop connection. We may only export aggregated results and data from these servers. Our project has gone through ethical review at the Swedish Ethical Review Authority (Dnr 2020-01550). For those researchers who want to replicate our results, it is possible to order the administrative data directly from Statistics Sweden (after ethical review). We will also make available a list all of the variables required for this project, together with the statistical code used for the analyses.

Sample limitations

While there are somewhat over 100,000 adolescents applying to upper secondary school each year in Sweden, the sample we can use for our RD models is much smaller. There are three main reasons for this. First, students who apply for both general and vocational programmes form a subsample of all students. In our data for 2008–2016, about 29 per cent of the students mix general and vocational programmes among their top three preferred programmes. In addition, we focus on students who have a general programme as one of their higher-ranked preferred programmes and a vocational programme as one of their lower-ranked alternatives. While this is more common than having a higher-ranked vocational programme and a lower-ranked general programme, it further reduces the sample. Second, a subtantial share of the students applying to upper secondary school in Sweden are not eligible for the standard national programmes and instead have to start preparatory programmes (so-called introductory or individual programmes, see variable definitions below). Third, we can only run the RD design on programmes that are oversubscribed so that there is a GPA cutoff and where some students are not admitted. Somewhat more than half of the students under our period of study apply to an oversubscribed programme as their first preferred programme.

We present figures and tables below with descriptive statistics on our full RD sample of students who are admitted to general and vocational programmes as well as separate tables and figures for students who start each programme type. We also present such statistics on the full population of upper secondary students.

A student may apply several times (years) to upper secondary school, for instance because of that they dropped out from the programme they first started. A small set of students also postpone starting in upper secondary school. However, since students who apply several times to upper secondary school form a somewhat special group – which tends to be negatively selected – we exclude these students from our analysis sample. That is, we limit our analysis sample to students who apply to upper secondary school for the first time and apply directly at the end of lower secondary school. However, we do include students who do not come directly from lower secondary school in the data when we derive our running variable, because they can affect the GPA cutoff for admission to a specific programme.

Variable definitions

A description of the general variables used in the analysis follow below.

- **Voter turnout in elections**: Coded as 1 for those that turn out to vote and 0 for those who abstain. Only defined for eligible voters. Students need to have had the chance to complete upper secondary school before we include data on their electoral participation in our analyses. That is, it is not until three years after they start upper secondary school their turnout is recorded in our data (even though students typically turn 18 and become eligible to vote before that). E.g., the turnout of a student beginning upper secondary school in 2016 will not be recorded for the national election of 2018 (September 9) but will be included for the EP election of 2019 (May 26).

  When we use the mean turnout between the 2018, 2019 and 2022 elections we also include individuals with data on only one of these elections. We choose to do so to maximize the number of observations and increase statistical precision. For mean turnout variables used in the population-level analyses, EP 2009/2019 and national 2010/18/22, we define them in the same way, only requiring at least one observation.

- **Year of birth**: Information is retrieved from the Swedish Population Register.

- **Age**: Calculated based on year of birth.

- **Female**: Equal to 1 if female, 0 if male. Information is retrieved from the Swedish Population Register.

- **Tertiary studies at age 22**: Equal to 1 if an individual has been registered to tertiary studies in Sweden in any year before the year in which he or she turns 23 years old, 0 if no experience of tertiary studies before the age of 23. Because 2021 is the most recent year for which we have access to socio-economic data, this indicator is only defined for cohorts applying to upper secondary school between 2008–2015. The 2015 cohort typically turns 22 years old in 2021.

- **Graduation within 3 years**: Coded as 1 for students who within three years of applying to upper secondary school graduate from this level. Up to 2013 (students following the pre-2011 reform system): graduation is defined as getting a grade certificate (*slutbetyg*). From 2014 (the post-reform system): graduation is defined as either getting a grade certificate (*slutbetyg*), upper secondary degree (*gymnasie-examen*) or a certificate of studies (*studiebevis*) with at least 2500 study points. Data from the register over students finishing upper secondary school (*Avgångna från gymnasieskolan*). Coded as 0 for students who do not meet these requirements (including those who are not at all included in this register – meaning that they have not finished upper secondary school).

- **Graduation within 5 years**: Defined in the same way as *graduation within 3 years* but allow for graduation within five years of applying to upper secondary school.

Data from the register over students finishing upper secondary school (*Avgångna från gymnasieskolan*).

- **Upper secondary GPA relative to year**: Grade point average in percentiles relative to graduation year. Standardized to run between 0 and 1. Data from the register over students finishing upper secondary school (*Avgångna från gymnasieskolan*). Data only available for students who have finished upper secondary school.

- **Upper secondary GPA relative to year, school and programme**: Grade point average in percentiles relative to graduation year, school and programme. Standardized to run between 0 and 1. Data only available for students who have finished upper secondary school.

- **Lower secondary GPA**: Grade point average from the last grade of compulsory school (9th grade). The sum of 16 subject grades for students who have not studied an additional modern language. The sum of 17 subject grades for students who have studied such a language. Ranging from 0 to 340. In our population-level analysis we define percentiles of this measure relative to year. The data come from the register over students applying to upper secondary school.

- **Test scores graduation courses**: Average test scores across national tests in courses necessary to graduate from the two programme types (general programme: Swedish 1–3, English 1–2 and Mathematics 1, vocational program: Swedish 1, English 1 and Mathematics 1). The average test scores are percentile ranked according to the year the students started upper secondary education. Test scores are not available for the full population of upper secondary students. There are some missing data for the earlier cohorts, especially 2008–2009. Some students may also drop out before taking these tests or for other reasons refuse to take them.

- **Inactivity between 18–22**: Proportion (0–1) of time spent between age 18 and 22 not employed (yearly wage less than 10 000 SEK) and not in any form of education (including upper secondary, post-secondary, tertiary, adult education and getting study allowances for studies abroad). This is largely equivalent to the status often referred to as NEET (Not in Education, Employment, or Training). Study participation is measured for the autumn semester. Each year neither in employment nor in education is coded as 1 and 0 otherwise. The proportion of years as inactive/NEET is calculated by taking the mean of these five annual indicators. Because 2021 is the most recent year for which we have access to socio-economic data, this indicator is only defined for cohorts applying to upper secondary school between 2008–2015. The 2015 cohort typically turns 22 years old in 2021.

- **Parental Education**: Seven educational levels according to the older Swedish SUN classification prior to the year 2000, which resembles the ISCED97-classification: 1: Primary education shorter than nine years; 2: Primary education and lower secondary education, in total nine years; 3: Upper secondary education, two years; 4: Upper secondary education, three years; 5: post-secondary or tertiary education shorter than three years: 6: post-secondary or tertiary education longer than three years; 7: PhD-level education. For some applications we take the mean between the parents to create a approximately continuous measure which we standardize to run between 0 and 1 (for instance in the falsification tests). In other cases we create dummies for each level of education separately for the mother and father (typically when using parental education as a control).

  Our indicator for tertiary education for parents takes the value 1 if any of an individual's parents have an education on at least level 5, and 0 otherwise.

  We measure parental education in the year when the students finish lower secondary school (and typically apply to upper secondary school).

- **Parental Earnings**: Annual gross wage labour income in deciles for the parents in the year when the students finish lower secondary school. For some applications we calculate deciles relative to year for the mother and father separately and then take the mean of these two values. The variable is then standardized to run between 0 and 1. For other usages we create dummy indicators for each decile separately for the mother and the father.

- **Foreign-born**: Born outside of Sweden. Information is retrieved from the Swedish Population Register.

- **Foreign-born parents**: Parents born outside of Sweden (available seprately for mother and father). Information is retrieved from the Swedish Population Register.

- **General/vocational programme**: We have access to data on the programme and school a student applies to in the main application to upper secondary school, the result of the main admission process (what programme and school a student became admitted to), as well as the programme (and school) to which a student is registered to in October during his or her first semester in upper secondary school.

  Most programmes are so-called national programmes and follow a specific nationally decided curriculum. These curricula also categorize the programmes as either having a general or vocational orientation. We adhere to this classification. However, there are also so-called special programmes (*specialutformade program* or *särskild variant*) that may diverge from the curricula of the national programmes. We also categorize the special programmes if they are similar to one of the national programmes, or in other ways clearly can be categorized as having a general or vocational orientation. However, special programmes that cannot readily be categorized as being general or vocational are excluded from our data. Note that the names of many of the programmes changed with the 2011 reform and that our classification below includes both pre and post reform programmes. The following programmes are categorized as general programmes:

– Aesthetics (*estetiskt program*).
– Economics (*ekonomiprogrammet*).
– Humanities (*humanistiskt program*).
– International baccalaureate (*IB*).
– Natural science (*naturvetenskapligt program*).
– Social science (*samhällsvetenskapligt program*).
– Technology (*teknikprogrammet*), including the option to study for a fourth year to become 'upper secondary school engineer' (*gymnasieingenjör*).

The following programmes are categorized as vocational programmes:

– Agriculture (*naturbruk*).
– Construction (*bygg och anläggning*).
– Child care (*barn och fritid*).
– Electricity and energy (*el och energi*).
– Commerce and administration (*handel och administration*).
– Food programme (*livsmedelsprogrammet*)
– Handcraft (*hantverk*).
– Heating and sanitation (*VVS och fastighet*).
– Health and socal care (*vård och omsorg*).
– Hotel and tourism (*hotell och turism*).
– Industrial technology (*industriteknik*).
– Media programme (*medieprogrammet*)
– Restaurants and food (*restaurang och livsmedel*)
– Vehicles and transport (*fordon och transport*).

There are two large programmes that we exclude from our analysis: the individual programme and the introductory programme. The former was a programme before the 2011 reform that was tailored for students who were not eligible for the standard national programmes in upper secondary school. This could be because they had not finished lower secondary school with a pass in the required subjects, or because they were newly arrived immigrants. The introductory programme is a similar type of programme implemented with the 2011 reform. Both of these programmes aim to make the students eligible and ready to study on a standard national programme. Thus, they are preparatory programmes and not distinct general or vocational programmes as such.

- **Finishing a general programme**: Coded 1 if a student graduated from one of the programmes above categorized as general programmes and 0 for students graduating from the programmes categorized as vocational. Graduation is defined as in the variables on graduation within 3 or 5 years above using the register over students finishing upper secondary school (*Avgångna från gymnasieskolan*). We only see to graduation within 5 years. Dropouts are missing-coded.

Descriptive statistics and figures

Figure 3 in the paper serves the purpose of giving an overview of the background characteristics of our RD sample and the population of students starting upper secondary school under our studied time period. Women are underrepresented in our sample because there are fewer women than men applying to vocational programmes overall, and therefore also fewer women who mix applying to general and vocational programmes.

Figure C.1 presents the overall distribution of compulsory school (lower secondary) GPA for the RD sample compared to the population. Figures C.2 and C.3 demonstrate the separate distributions for students starting vocational and general programmes: first for our RD sample and then for the full population.

Our RD sample is somewhat negatively selected compared to the full population because students who mix applying to general and vocational programmes tend have somewhat lower compulsory school grades on average than students who only apply for general programmes (GPA is 220.7 on average in the RD sample vs. 226.6 on average in the population). However, our RD sample overall is clearly positively selected compared to the population of students starting vocational programmes (mean GPA 220.7 vs. 195.9).

In Tables C.1–C.6 we present more detailed descriptive statistics on both the RD sample and the population of students starting upper secondary school during the years for which we define the RD sample (2008–2016). We include tables on the full sample (both general and vocational students) as well as separate tables for students starting general and vocational programmes. To make the population descriptives comparable to our RD sample we apply similar restrictions by only including students coming directly from lower secondary school and who have turnout data for at least one election.

Figure C.1: Distribution of compulsory school grades in the RD sample compared to the population of upper secondary applicants 2008–2016, 5 GPA point bins

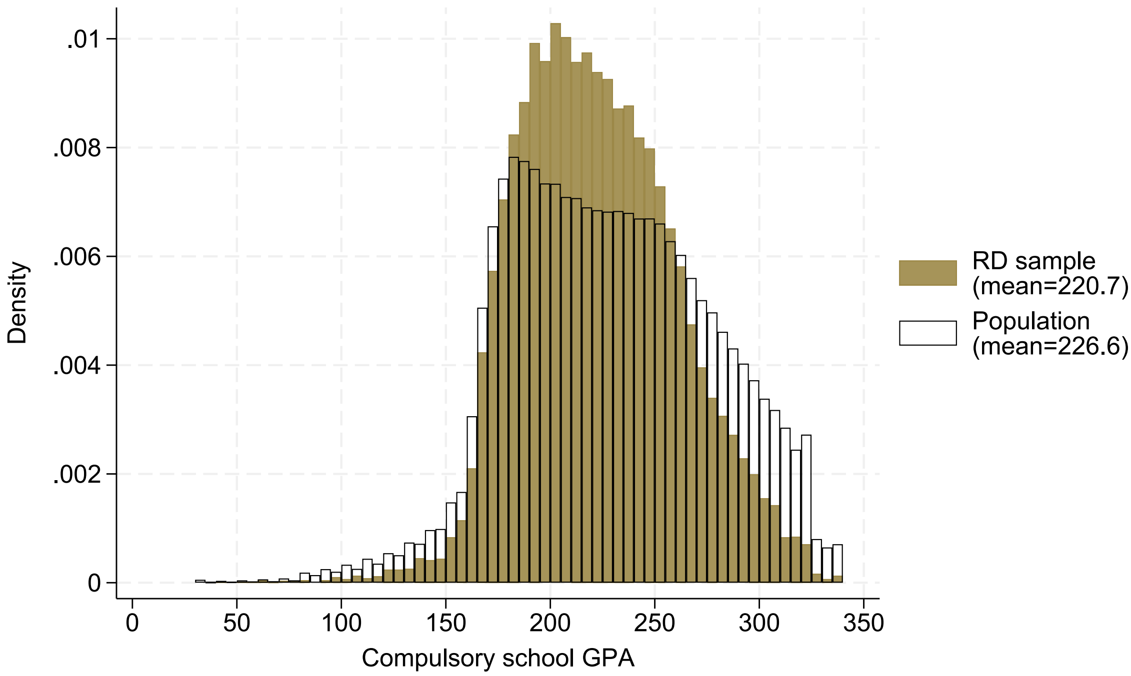


Figure C.2: Distribution of compulsory school grades in the RD sample: upper secondary applicants 2008–2016. Programme type separated, 5 GPA point bins.

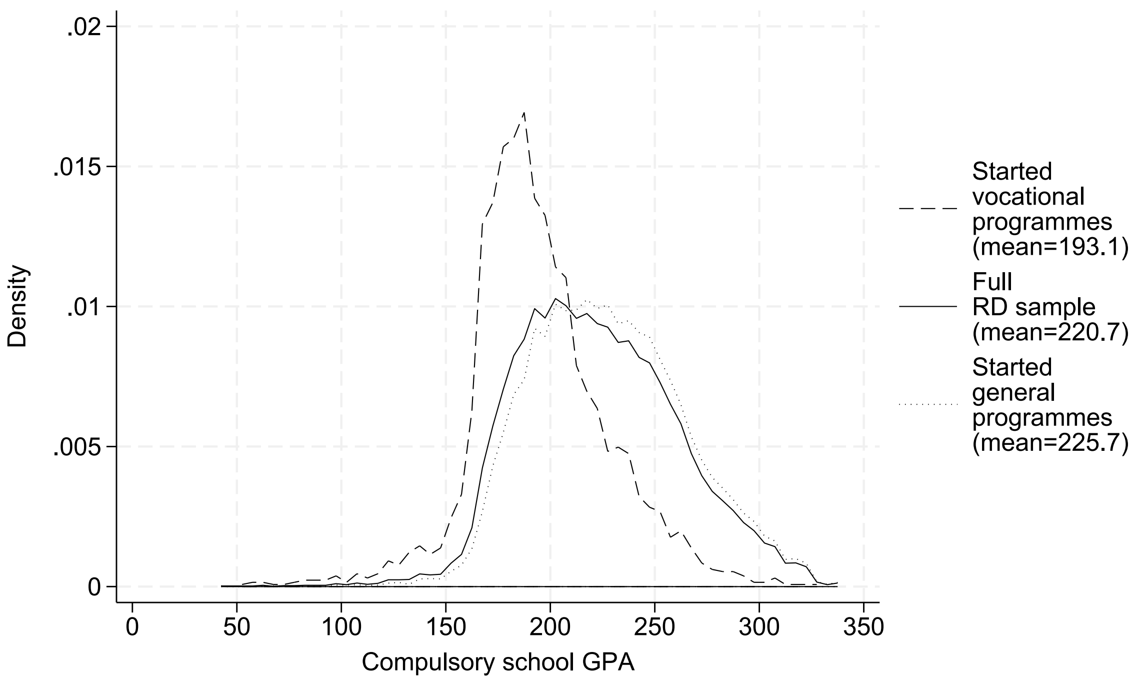


Figure C.3: Distribution of compulsory school grades in the population of upper secondary school applicants 2008–2016. Programme type separated, 5 GPA point bins.

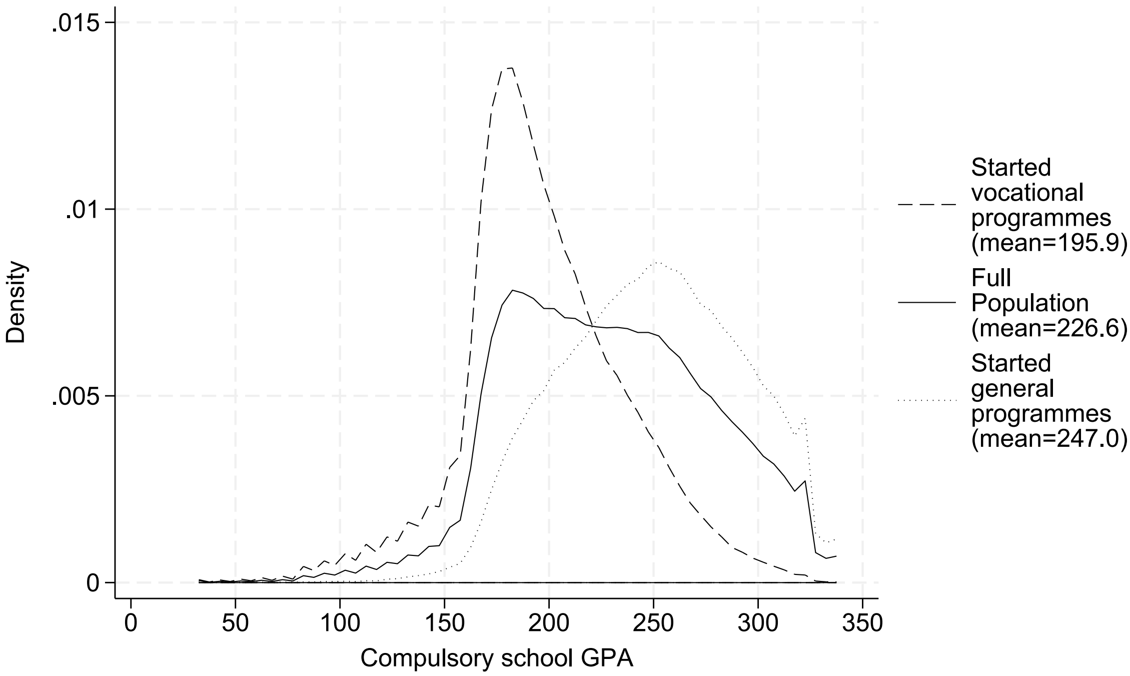


Table C.1: Descriptive statistics on students who start a general or vocational programme 2008–2016 (registered in October, first autumn semester), RD sample.

	count	mean	sd	min	max
Turnout in the national election 2018	15027	0.885	0.320	0	1
Turnout in the EP election 2019	17043	0.431	0.495	0	1
Turnout in the national election 2022	17024	0.852	0.355	0	1
Mean turnout in the 2018, 2019 and 2022 elections	17231	0.712	0.293	0	1
Female	17122	0.444	0.497	0	1
Parents with post-secondary education	16999	0.401	0.490	0	1
Mean turnout between parents in the 2009 EP elec	15525	0.421	0.493	0	1
Mean turnout between parents in the 2010 nat elec	15627	0.913	0.282	0	1
Any tertiary studies at age 22	15050	0.376	0.484	0	1
Finish general programme (instead of vocational)	12961	0.801	0.399	0	1
Graduate from upper secondary school within 3 yrs	17231	0.789	0.408	0	1
Graduate from upper secondary school within 5 yrs	17231	0.859	0.348	0	1
GPA at upper sec relative to year	14812	0.421	0.265	0	1
GPA at upper sec relative to year, school and prog	14812	0.398	0.274	0	1
Proportion of years as NEET between 18–22	15045	0.117	0.185	0	1
Foreign born	17144	0.0638	0.244	0	1
At least one foreign-born parent	17030	0.170	0.376	0	1
GPA, lower secondary school	17231	220.7	38.06	40	340
Age in 2018 (i.e. age at the end of the year)	17122	22.24	2.780	17	28
Running variable	17231	36.94	46.53	-160	270

Table C.2: Descriptive statistics on students who start a general programme 2008–2016 (registered in October, first autumn semester), RD sample.

	count	mean	sd	min	max
Turnout in the national election 2018	12918	0.895	0.307	0	1
Turnout in the EP election 2019	14480	0.452	0.498	0	1
Turnout in the national election 2022	14446	0.864	0.342	0	1
Mean turnout in the 2018, 2019 and 2022 elections	14620	0.728	0.286	0	1
Female	14522	0.460	0.498	0	1
Parents with post-secondary education	14440	0.415	0.493	0	1
Mean turnout between parents in the 2009 EP elec	13188	0.434	0.495	0	1
Mean turnout between parents in the 2010 nat elec	13322	0.919	0.272	0	1
Any tertiary studies at age 22	12917	0.412	0.492	0	1
Finish general programme (instead of vocational)	11060	0.926	0.261	0	1
Graduate from upper secondary school within 3 yrs	14620	0.799	0.401	0	1
Graduate from upper secondary school within 5 yrs	14620	0.870	0.337	0	1
GPA at upper sec relative to year	12709	0.435	0.265	0	1
GPA at upper sec relative to year, school and prog	12709	0.398	0.273	0	1
Proportion of years as NEET between 18–22	12912	0.112	0.179	0	1
Foreign born	14539	0.0607	0.239	0	1
At least one foreign-born parent	14467	0.159	0.366	0	1
GPA, lower secondary school	14620	225.7	36.87	40	340
Age in 2018 (i.e. age at the end of the year)	14522	22.40	2.769	17	28
Running variable	14620	46.41	41.86	-120	270

Table C.3: Descriptive statistics on students who start a vocational programme 2008–2016 (registered in October, first autumn semester), RD sample.

	count	mean	sd	min	max
Turnout in the national election 2018	2109	0.824	0.381	0	1
Turnout in the EP election 2019	2563	0.311	0.463	0	1
Turnout in the national election 2022	2578	0.783	0.412	0	1
Mean turnout in the 2018, 2019 and 2022 elections	2611	0.624	0.315	0	1
Female	2600	0.353	0.478	0	1
Parents with post-secondary education	2559	0.324	0.468	0	1
Mean turnout between parents in the 2009 EP elec	2337	0.346	0.475	0	1
Mean turnout between parents in the 2010 nat elec	2305	0.876	0.329	0	1
Any tertiary studies at age 22	2133	0.158	0.365	0	1
Finish general programme (instead of vocational)	1901	0.0747	0.263	0	1
Graduate from upper secondary school within 3 yrs	2611	0.731	0.443	0	1
Graduate from upper secondary school within 5 yrs	2611	0.802	0.399	0	1
GPA at upper sec relative to year	2103	0.335	0.243	0	0.990
GPA at upper sec relative to year, school and prog	2103	0.401	0.278	0	0.990
Proportion of years as NEET between 18–22	2133	0.148	0.214	0	1
Foreign born	2605	0.0810	0.273	0	1
At least one foreign-born parent	2563	0.233	0.423	0	1
GPA, lower secondary school	2611	193.1	32.35	50	327.5
Age in 2018 (i.e. age at the end of the year)	2600	21.35	2.673	17	27
Running variable	2611	-16.07	34.10	-160	175

Table C.4: Descriptive statistics on all students starting a general or a vocational programme: full population of upper secondary students 2008–2016.

	count	mean	sd	min	max
Turnout in the national election 2018	594717	0.880	0.325	0	1
Turnout in the EP election 2019	663182	0.455	0.498	0	1
Turnout in the national election 2022	664541	0.858	0.349	0	1
Mean turnout in the 2018, 2019 and 2022 elections	671065	0.724	0.294	0	1
Female	666853	0.486	0.500	0	1
Parents with post-secondary education	662021	0.425	0.494	0	1
Mean turnout between parents in the 2009 EP elec	607358	0.433	0.495	0	1
Mean turnout between parents in the 2010 nat elec	608329	0.908	0.288	0	1
Finish general programme (instead of vocational)	525948	0.618	0.486	0	1
Any tertiary studies at age 22	595123	0.401	0.490	0	1
Graduate from upper secondary school within 3 yrs	671065	0.812	0.391	0	1
Graduate from upper secondary school within 5 yrs	671065	0.875	0.331	0	1
GPA at upper sec relative to year	585115	0.500	0.287	0	1
GPA at upper sec relative to year, school and prog	585115	0.486	0.291	0	1
Proportion of years as NEET between 18–22	594966	0.105	0.179	0	1
Foreign born	667748	0.0655	0.247	0	1
At least one foreign-born parent	663871	0.180	0.385	0	1
GPA, lower secondary school	671065	226.6	48.52	30	340
Age in 2018 (i.e. age at the end of the year)	666853	22.12	2.616	17	29

Table C.5: Descriptive statistics on students starting a general programme: full population of upper secondary students 2008–2016.

	count	mean	sd	min	max
Turnout in the national election 2018	351610	0.906	0.292	0	1
Turnout in the EP election 2019	397888	0.542	0.498	0	1
Turnout in the national election 2022	399367	0.878	0.327	0	1
Mean turnout in the 2018, 2019 and 2022 elections	403042	0.769	0.283	0	1
Female	400028	0.528	0.499	0	1
Parents with post-secondary education	397873	0.521	0.500	0	1
Mean turnout between parents in the 2009 EP elec	361922	0.491	0.500	0	1
Mean turnout between parents in the 2010 nat elec	362795	0.925	0.263	0	1
Finish general programme (instead of vocational)	329752	0.971	0.169	0	1
Any tertiary studies at age 22	351072	0.585	0.493	0	1
Graduate from upper secondary school within 3 yrs	403042	0.833	0.373	0	1
Graduate from upper secondary school within 5 yrs	403042	0.897	0.303	0	1
GPA at upper sec relative to year	359237	0.560	0.286	0	1
GPA at upper sec relative to year, school and prog	359237	0.487	0.291	0	1
Proportion of years as NEET between 18–22	350969	0.0955	0.165	0	1
Foreign born	400664	0.0767	0.266	0	1
At least one foreign-born parent	398992	0.212	0.409	0	1
GPA, lower secondary school	403042	247.0	42.97	30	340
Age in 2018 (i.e. age at the end of the year)	400028	21.78	2.559	17	28

Table C.6: Descriptive statistics on students starting a vocational programme: full population of upper secondary students 2008–2016.

	count	mean	sd	min	max
Turnout in the national election 2018	243107	0.843	0.364	0	1
Turnout in the EP election 2019	265294	0.325	0.468	0	1
Turnout in the national election 2022	265174	0.828	0.378	0	1
Mean turnout in the 2018 and 2019 elections	268023	0.658	0.298	0	1
Female	266825	0.422	0.494	0	1
Parents with post-secondary education	264148	0.281	0.449	0	1
Mean turnout between parents in the 2009 EP elec	245436	0.349	0.476	0	1
Mean turnout between parents in the 2010 nat elec	245534	0.884	0.321	0	1
Any tertiary studies at age 22	244051	0.137	0.343	0	1
Finish general programme (instead of vocational)	196196	0.0242	0.154	0	1
Graduate from upper secondary school within 3 yrs	268023	0.781	0.413	0	1
Graduate from upper secondary school within 5 yrs	268023	0.841	0.365	0	1
GPA at upper sec relative to year	225878	0.403	0.261	0	1
GPA at upper sec relative to year, school and prog	225878	0.484	0.290	0	1
Proportion of years as NEET between 18–22	243997	0.120	0.196	0	1
Foreign born	267084	0.0486	0.215	0	1
At least one foreign-born parent	264879	0.133	0.339	0	1
GPA, lower secondary school	268023	195.9	39.40	30	340
Age in 2018 (i.e. age at the end of the year)	266825	22.62	2.619	17	29

D Details on RD setup

This section describes in further detail than in the paper how we carry out the RD analysis, including how we define the GPA cutoff, the running variable and how we do the RD estimation.

We rely on the RDRobust package (Cattaneo, Idrobo, and Titiunik 2019; also see Calonico et al. 2017) for our RD analyses. The RD plots are produced with the related RDplot package (Cattaneo, Idrobo, and Titiunik 2019).

GPA cutoff and non-compliance

To identify the GPA cutoff for a specific programme, we define groups specific to application year, school, programme code and regional admission office. Below we refer to unique combinations of these variables as a 'cell'. I.e. this is the group within which students are admitted to a programme in accordance with their GPA. Admission is specific to a certain combination of programme and shool (i.e. a student not getting admitted to a programme at one school is not transfered to the same programme at another school). The student with the highest GPA in each such cell is given the first slot on the specific programme, then the one with the second-highest GPA, and so on until there are no more available slots on this programme at this specific school during this year. The GPA of the last admitted student then defines the GPA cutoff for a specific cell. Alternatively, all students may get a a slot if there are more slots than students applying to a certain programme (cell). The programme code describes the programme – such as Social science – and if there is a certain sub-orientation to the programme. For instance, in the case of the social science programme, the sub-orientations may include behavioural science, media and communication, as well as only social science. The admission is separate for each such sub-orientation.

Since there is no GPA cutoff for cells where all students become admitted, we exclude these from our RD analysis data. More formally, we only include competitive (oversubscribed) cells in our analysis, meaning that at least one eligible student, who is not already admitted to a higher-ranked alternative, has not become admitted. Moreover, we rely on a variable describing whether a student is accepted on the basis of grades or on other grounds and only focus on the great majority whose admission is based on grades. While relying on grades is the standard admission procedure, it is possible for the students to appeal to the admission office about an exception from admission based on grades. For instance because of prolonged sickness that may have affected a student's grades negatively. In addition, a small number of programmes, mainly arts programmes, sports programmes, and gifted students' programmes, also allocate slots based on auditions and entrance exams.

Several students within a cell may have the same GPA, and sometimes the admission cutoff equals this GPA exactly. Regional admission offices resort to various routines in order to handle such ties. These include using a lottery, relying on the rank of an application, or prioritizing grades in specific subjects. Since we lack data on which routine is applied, we drop ties at the GPA cutoff in cells where some eligible students are accepted and some are not.

As mentioned in the main paper, there are instances of non-compliance in the data regarding students admission status. That is, there are cases where students with a GPA higher than the GPA cutoff still are not admitted to their preferred programme (there is a separate indicator in the data for whether a student is admitted or not). This is what in the RD literature typically is referred to as non-compliance (Cattaneo and Titiunik 2022; Lee and Lemieux 2010). We have conducted interviews with staff at Statistics Sweden and at the regional admission offices to get an understanding for why there are these instances of non-compliance in the admission data. While Statistics Sweden collects and administers the data on the national level, the data is reported to them from the regional admission offices. There are several reasons for why this type of errors may occur.

First, the reporting of the admission data is done by a large number of regional admission offices and the procedure is not fully standardized. It is therefore possible that coding errors occur. According to Statistics Sweden, there might be errors in the variable reporting whether a student is admitted on the basis of grades or on other grounds. It is, for instance, likely that educational programmes that rely on a combination of grades and entrance exams or auditions for admission are coded as just using grades for admission. Unfortunately, we have no data on whether such tests are used for admission to a programme. Second, on some schools there are several similar programmes with separate admission but for which we lack a detailed programme code to be able to differentiate the different programmes. In both of these cases our derived GPA cutoff will be incorrect and does not introduce exogenous variation in programme orientation.

If we include the non-compliant observations in our RD models, we tend to get significant effects in our falsification tests on predetermined covariates. There are also issues with precision. Against this background, we reason that relying on fuzzy RD estimation is not sufficient for handling the non-compliant observations. One option would be to drop all cells with any single instance of non-compliance but because most cells include hundreds of applicants, a single non-compliant application would lead to a large number of dropped observations. Since we have a quite limited sample already, such a strategy would worsen our precision issues. Instead, as we describe in the paper, we use a similar approach as Dahl, Rooth, and Stenberg (2023) and drop cells with more than 10 per cent non-compliant observations. In this way we should drop all cells where our derived GPA cutoff is just incorrect; for instance because of that we are unable to differentiate different programmes with separate admission due to that our programme codes are not detailed enough for some programmes. We also drop all single non-compliant observations where the non-compliance may be explained by single instances of coding errors.

In total we drop about four per cent of the students in our application data.

RD plots

The RD plots in the paper rely on quantile-spaced bins, and the number of bins is chosen using the data-driven 'mimicking variance' approach. This approach aims for mimicking the variation in the bins to the overall variation in the data. It is a method for selecting bins that tend to produce a larger number of bins to better portray the variation in the data. The alternative integrated-mean-squared-error method tends to result in a fewer bins and a more 'smooth-looking' plot but which may give a too simplified picture of the variation at the cutoff (Cattaneo, Idrobo, and Titiunik 2019).

The plots with a fourth order polynomials, Figure 4 and the left panel in Figure 6, do no apply any weights (a uniform kernel). Since the right panel in Figure 6 is supposed to demonstrate the local-linear estimation, these plots apply a triangular kernel for weighting, the standard approach in local-linear estimation.

Estimation: bandwidth, weighting and inference

While the idea behind a RD design with a cutoff is quite simple, the actual estimation involves a number of non-trivial decisions that may have major consequences for the final results. These decisions primarily revolve around the order of the polynomial for approximating the unknown regression function for the relationship between the running variable and the outcome, the choice of bandwidth around the cutoff, how to weigh observations and how to carry out statistical inference. However, recent methodological contributions offer guidance, and in regard to some issues, the literature also largely agrees on best practice. In this section we elaborate on how we handle these issues in relation to the somewhat condensed description in the paper.

We choose to apply the continuity-based approach to RD analysis in our main specification, even though our running variable is discrete (Cattaneo, Idrobo, and Titiunik 2019, 2023). That is, we mainly rely on local-linear RD estimation. We have three main reasons for this choice. First, the number of mass points amounts to 134, which is not a large number but also not a particularly small number (Cattaneo, Idrobo, and Titiunik 2023). Second, we do a number of falsification tests on pre-determined covariates and none of these become significant (Cattaneo, Idrobo, and Titiunik 2019). I.e., it does not seem to be the case that students with different pre-determined characteristics bunch up on different sides of the cutoff. Third, considering that there is such a strong overall relationship between the running variable and our outcomes (see Figure 6 in the paper), controlling for the linear trend around the cutoff becomes quite important. That is, if we use the local-randomization alternative for estimation, we can only use the observations that are very close to the cutoff as a broader bandwidth (without controlling for the trend) will mean that we pick up the overall correlation between grades and electoral turnout.

We also make use of the option in the RDRobust package to adjust the bandwidth according to the number of mass points. Furthermore, in the auxiliary models below, we carry out a number of robustness checks to substantiate the choice of relying on local-linear estimation. We also present results using the alternative local-randomization approach.

Moreover, we follow the common advice in the RD literature and do not apply higher-order polynomials as an alternative to local-linear estimation as such polynomials risk to lead to overfitting, noisy estimates and other issues (Cattaneo, Idrobo, and Titiunik 2019; Gelman and Imbens 2019). While a linear approximation may seem coarse for a relationship between the running variable and an outcome that often takes a non-linear shape, the important insight is that the linear approximation tends to work well and be robust *close* to the cutoff. This leads to the critical question how to select the bandwidth around the cutoff. This choice involves a trade-off between bias and precision, as narrower bandwidths typically reduce the misspecification related to the local polynomial approximation, whereas larger bandwidths imply more observations and usually less variance (Cattaneo, Idrobo, and Titiunik 2019). More recent contributions on the RD design generally recommend using a data-driven approach to choosing bandwidth to avoid that the RD estimates are dependent upon ad hoc and intransparent decisions by the researcher (Cattaneo, Idrobo, and Titiunik 2019; Cattaneo and Vazquez-Bare 2017; Imbens and Kalyanaraman 2012). We follow this advice and apply the widespread approach to choose the bandwidth that minimizes the mean squared error (MSE) of the local polynomial, i.e. an MSE-optimal bandwidth. We use one common bandwidth on both sides of the cutoff.

Apart from the choice of bandwidth some kind of weighting of the observations is typically applied as the observations close to the cutoff can be assumed to be more informative of the effect of the treatment. We use the often recommended triangular kernel function that applies symmetrical weights on both sides of the cutoff that are maximized at the cutoff and in a linear fashion taper off to the limits of the bandwidth (Cattaneo, Idrobo, and Titiunik 2019). Note that the weights affect the MSE and, hence, the bandwidth.

The local linear RD estimator, relying on an MSE-optimal bandwidth, is developed for point estimation and is inappropriate to use for conventional OLS-inference as the bias related to the local polynomial is not taken into account (Cattaneo, Idrobo, and Titiunik 2019). In other words, typical t-tests are likely to be invalid. To address this issue we complement the conventional standard errors with the so-called robust bias-corrected procedure for estimating confidence intervals (Calonico, Cattaneo, and Farrell

2020).

In summary, our RD models estimate the local average treatment effect (LATE) for students near the admission cutoff using the following equation:

$$E(\tau_i \,|\, x_i = x_c) = \frac{\tau_{ITT}}{\tau_{FS}} = \frac{\lim\limits_{x \to x_c^+} E(y_i \,|\, x_i = x) - \lim\limits_{x \to x_c^-} E(y_i \,|\, x_i = x)}{\lim\limits_{x \to x_c^+} E(D_i \,|\, x_i = x) - \lim\limits_{x \to x_c^-} E(D_i \,|\, x_i = x)} \tag{1}$$

where y_i is the outcome variable (e.g., voter turnout) for applicant i. D_i is the treatment indicator (starting a general programme or a vocational programme). x is the student's compulsory school GPA and x_c is GPA admission cutoff. $\lim_{x \to x_c^+}$ is the limit from the positive (right hand) side of the cutoff and $\lim_{x \to x_c^-}$ represents the limit from the negative (left hand) side. $E(D_i \,|\, x_i = x)$ is the probability of starting a general programme for individuals with compulsory school GPA x and $E(y_i \,|\, x_i = x)$ is the average turnout for individuals with compulsory school GPA x. This implies that the denominator, τ_{FS}, represents the first stage effect; i.e. the effect of being admitted to a general programme (treatment assignment) on the probability of starting a general programme (treatment). Moreover, the nominator τ_{ITT}, represents the intention to treat effect; i.e. the effect of treatment assignment on the outcome. In essence, the fuzzy RD estimates the jump (discontinuity) in the conditional mean of the outcome variable at the admission cutoff (ITT) and scales it by the jump (discontinuity) in the conditional treatment probability of starting a general programme at the admission cutoff (the first stage).

The limits are estimated by local linear regression where observations within the MSE-optimal bandwidth (h) are weighted by their proximity to the cutoff using a triangular kernel:

$$K_h(x_i - x_c) = \frac{1}{h} K\left(\frac{x_i - x_c}{h}\right) \tag{2}$$

E Regression tables on correlational analyses

In this section we present the complete regression tables on the correlational analyses in Figure 5 in the main paper. These models are run on the population of students who started upper secondary school between 2011 and 2016, applying the same sample restrictions as in our RD models. That is, we limit the sample to students eligible for the standard national programmes, who apply to upper secondary school for the first time and apply directly at the end of lower secondary school, and for whom we can categorize their programme as either general or vocational. The categorization of general and vocational programmes is done in the same way as in our RD models, see Appendix C.

Table E.1: The relationship between starting a general versus a vocational programme and turnout in European (EP) elections. Students starting upper secondary school 2011–2016.

	(1) Student turnout EP elec 2019	(2) Student turnout EP elec 2019	(3) Student turnout EP elec 2019	(4) Parental turnout EP elec 2009	(5) Parental turnout EP elec 2009
Gen prog	0.238***	0.192***	0.174***	0.148***	0.0930***
	(0.00450)	(0.00347)	(0.00321)	(0.00364)	(0.00273)
Constant	0.313***	0.343***	0.354***	0.324***	0.360***
	(0.00266)	(0.00248)	(0.00228)	(0.00242)	(0.00217)
Obs	377040	377040	377040	377040	377040
Controls	–	SE	SE + Par turnout	–	SE

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Robust standard errors clustered on schools.

The sample is restricted to be the same across all five models (i.e. only observations with data on all control variables).

The models correspond to the ones presented in Figure 5 in the paper on EP elections. Model (1): Student turnout in the EP election of 2019 without any controls (bivariate). Model (2): Student turnout in the EP election of 2019 with socio-economic controls. Model (3): Student turnout in the EP election of 2019 with socio-economic controls and controls for parental turnout in the EP election of 2009. Model (4): Parental turnout in the EP election of 2009 without any controls (bivariate). Model (5): Parental turnout in the EP election of 2009 with socio-economic controls.

Table E.2: The relationship between starting a general versus a vocational programme and turnout in national elections. Students starting upper secondary school 2011–2015.

	(1) Student turnout nat elec 2022	(2) Student turnout nat elec 2022	(3) Student turnout nat elec 2022	(4) Parental turnout nat elec 2010	(5) Parental turnout nat elec 2010
Gen prog	0.0579***	0.0480***	0.0442***	0.0464***	0.0326***
	(0.00201)	(0.00171)	(0.00168)	(0.00153)	(0.00119)
Constant	0.830***	0.837***	0.839***	0.875***	0.884***
	(0.00158)	(0.00141)	(0.00140)	(0.00122)	(0.00102)
Obs	391815	391815	391815	391815	391815
Controls	–	SE	SE + Par turnout	–	SE

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Robust standard errors clustered on schools.

The sample is restricted to be the same across all five models (i.e. only observations with data on all control variables).

The models correspond to the ones presented in Figure 5 in the paper on national elections. Model (1): Student turnout in the national election of 2018 without any controls (bivariate). Model (2): Student turnout in the national election of 2018 with socio-economic controls. Model (3): Student turnout in the national election of 2018 with socio-economic controls and controls for parental turnout in the election of 2010. Model (4): Parental turnout in the national election of 2010 without any controls (bivariate). Model (5): Parental turnout in the national election of 2010 with socio-economic controls.

Socio-economic controls add dummies for each category of the following variables (fixed effects): gender; birth year; foreign-born; foreign-born father; foreign-born mother; 7 levels of parental education (separately for mother and father); and parental earnings in deciles (separately for mother and father). Parental education and earnings are measured in the year when the student finishes lower secondary education (and typically apply to upper secondary school). See variable definitions in Appendix C.

The control for parental turnout adds two dummies for whether the parents turned out to vote in the EP election of 2009 (for student turnout in the EP election of 2019) or the national election of 2010 (for student turnout in the national election of 2022).

F Additional validity tests of the RD design

In this section we first demonstrate further tests of the validity of the RD design. These include presenting the distribution of the running variable, additional falsification tests and placebo cutoffs. Thereafter we test the robustness of our main turnout models by using a number of alternative model specifications.

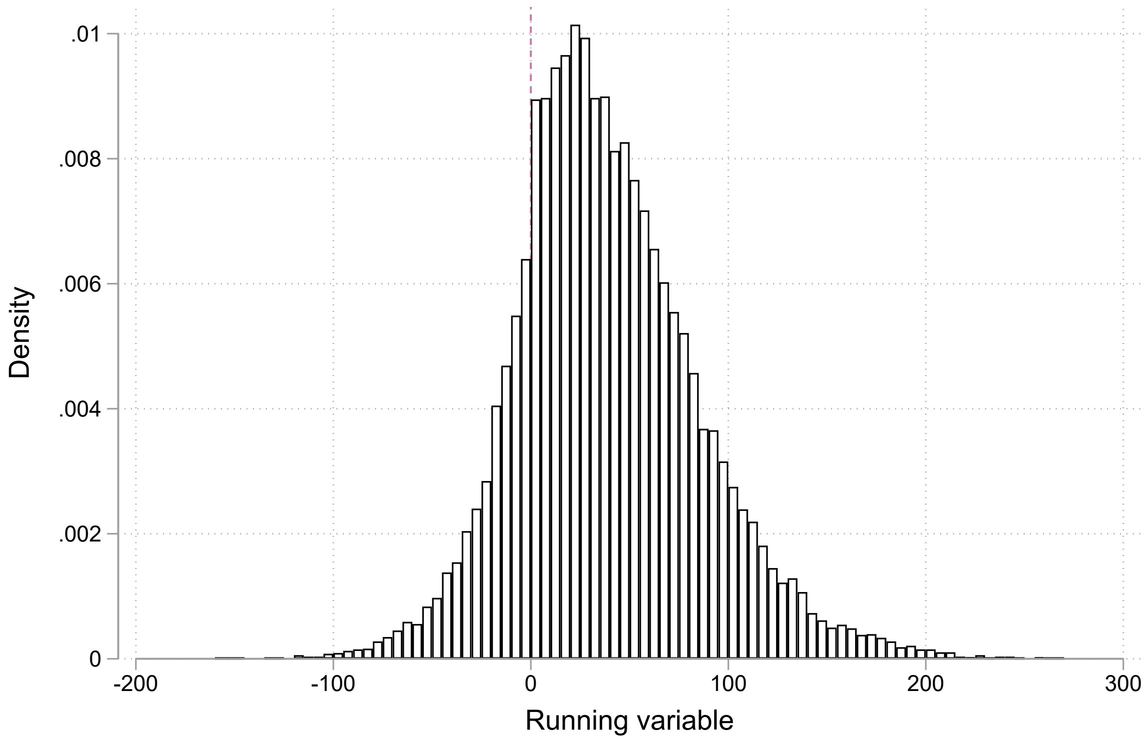
Distribution of the running variable

The fact that the reform of the grading system at the lower secondary level in 2013 (see Appendix B) meant that the minimal increment in a student's lower secondary GPA changed from 5 to 2.5 GPA points complicates checking the distribution of the running variable. That is, prior to 2013, the running variable is defined in steps of 5 GPA points and from 2013 in steps of 2.5 GPA points. This implies that somewhat more than half of our sample of students applying between 2008 and 2016 will consist of individuals where the minimal GPA increment is 5 GPA points, whereas the remaining part of the sample has a minimal GPA increment of 2.5 GPA points. I.e. the distribution will by definition not be continuous in terms of the minimal 2.5 point increments.

Against this background we first present histograms showing the distribution of the running variable in 5 GPA-points increments for our full sample for 2008–2016. Then we show separate plots of the distribution of the running variable in 2.5 GPA-points increments before and after 2013. We comment on each of the histograms below.

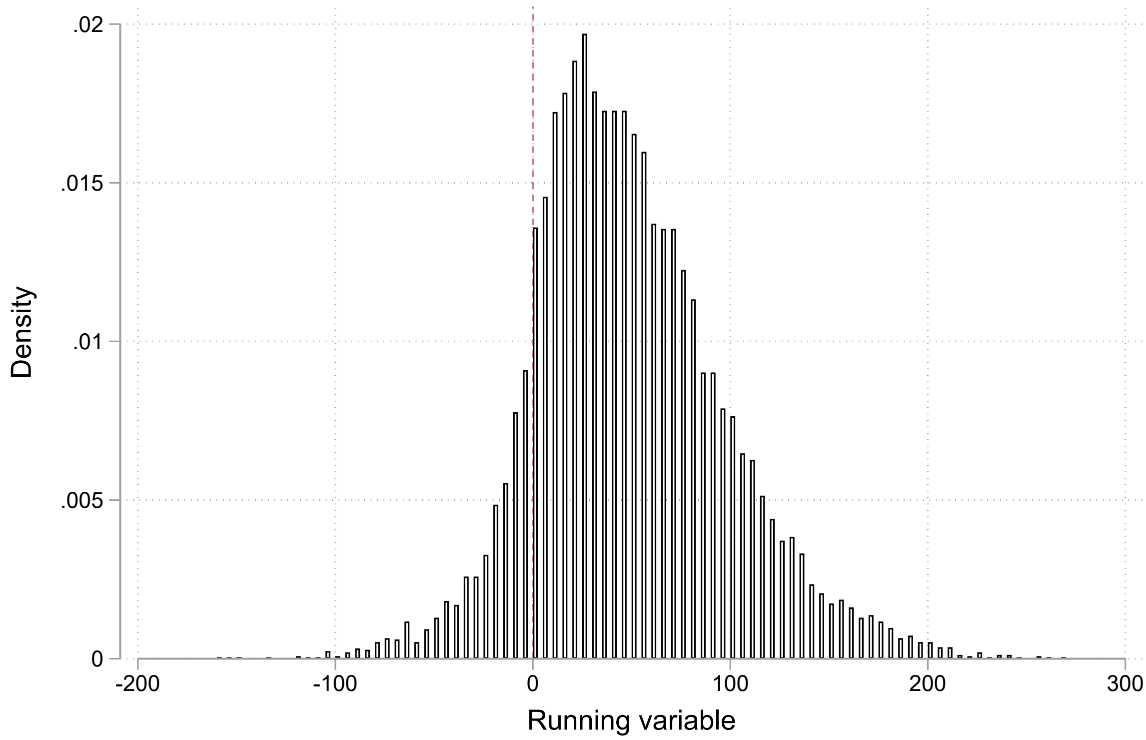
We present the distribution of the running variable only for observations with non-missing data on our essential variables for the RD models: data on if the student is registered to a vocational or a general programme during their first autumn semester on the upper secondary level and turnout data for at least one of the three elections we analyse (the national election of 2018, the EP election of 2019 or the national election of 2022).

Figure F.1: Distribution of the running variable 2008–2016, 5 GPA point bins



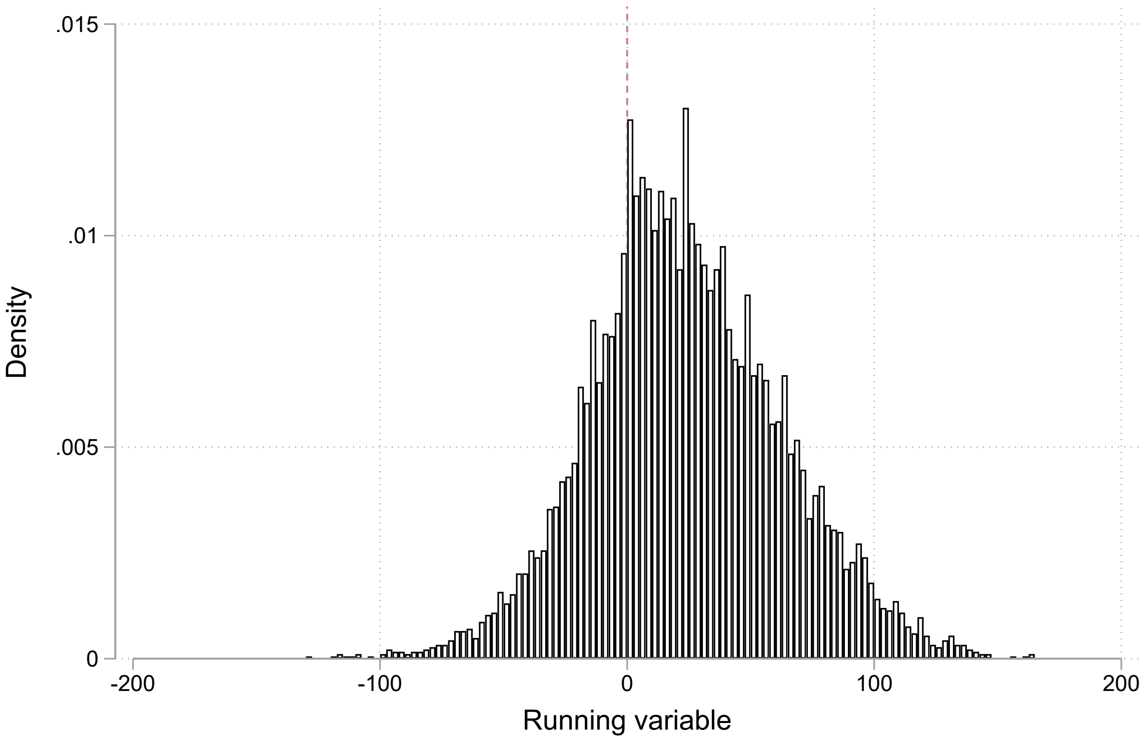
The distribution tends to resemble a normal distribution with the mean value being 30–35 GPA points above the GPA cutoff. This is not surprising as most students do have a GPA high enough for being accepted to their preferred programme. It is also possible that students adapt their applications such that they to a large extent apply to programmes where it is likely that they will pass the GPA cutoff (perhaps with some margin). This may be done on the basis of available information on the GPA cutoffs from previous years or in response to their own preliminary admission. This could be a reason for that there is a weak indication of bunching to the right of the cutoff. However, this should not present a problem to the RD design because the students will still not be aware of the exact GPA cutoff.

Figure F.2: Distribution of the running variable 2008–2012, 2.5 GPA point bins



The distribution of the running variable for the period 2008–2012 shows a similar pattern as the one for the full sample (Figure F.1). The distribution is largely normal with its peak above the GPA cutoff.

Figure F.3: Distribution of the running variable 2013–2016, 2.5 GPA point bins



The distribution of the running variable for the period 2013–2015 shows a somewhat different pattern than the one for the full sample and the one for the pre-2013 period (Figures F.1 and F.2). First of all, the smaller GPA point increments become clear and the distribution is also slightly more noisy with some different locally bunched observations. This could partly be explained by a somewhat smaller sample and the more details increments leaving more room for random variation. However, there is also a stronger indication of bunching just to the right of the GPA cutoff. Still there is not a negative jump in the number of observations just left of the GPA cutoff compared to lower values for the running variable. The distribution overall is still largely normal but more noisy. It might also be noted that there are several similar bunches like the one at the cutoff at other values of the running variable.

Further validity tests of the RD design

The tables below present complementary tests of the validity of the RD design. In Table F.1 we show corresponding sharp RD estimates of the fuzzy RD estimates in Table 1 in the paper. The RD estimate on graduating from a general upper secondary programme becomes much smaller, as expected, but remains significant at the 99 per cent level. We also add a model on how the treatment (starting a general rather than a vocational programme) affects students' likelihood of starting tertiary studies before the age of 23. The positive estimate suggests that treatment also affects students further educational pathway, as they become more likely to engage in tertiary studies, although the estimate is imprecise.

The falsification tests on the predetermined covariates parental education and parental turnout in 2010 remain non-significant. It should be noted that the latter outcome is not a predetermined outcome for our first cohorts as they start their studies at the upper secondary level in 2008. I.e., for our first three cohorts, 2008–2010, parental turnout is rather included as a placebo outcome. That is, an outcome taking place after the treatment but which is unlikely to be affected by the treatment (Cattaneo, Idrobo, and Titiunik 2019).

Table F.1: RD estimates on education and predetermined covariates: Sharp RD effect of being admitted to a general programme.

	(1) Grad gen prog	(2) Start HE	(3) Par edu	(4) Par turnout 2010
RD Estimate	0.178***	0.0410*	0.00668	0.0121
	(0.0482)	(0.0222)	(0.0118)	(0.0187)
Observations	13,035	15,380	17,342	15,946
Conventional CI	[.0836; .273]	[-.00252; .0845]	[-.0164; .0298]	[-.0245; .0488]
Robust CI	[.0354; .255]	[-.0178; .0844]	[-.0225; .0338]	[-.0334; .0583]
Robust P-value	.00951	.202	.695	.594

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

This table replicates Models (2)–(4) in Table 2 with sharp RD estimation instead of fuzzy RD, as well as adding tertiary studies as an additional outcome. Model (1): finishing a general programme. Model (2): starting higher (tertiary) education before the age of 23 (sample limited to cohorts 2008–2015). Models (3) and (4) report placebo effects on parental education and turnout in the 2010 election. These variables are defined as in Figure 4 in the paper.

Local linear estimates with triangular kernel weighting and MSE-optimal bandwidth. Conventional OLS-standard errors in parentheses. 95 per cent conventional confidence intervals, robust bias-corrected intervals and accompanying p-values are also reported.

In Table F.2 we present RD estimates on a set of additional predetermined covariates and placebo outcomes, complementing those in Table 1 in the paper. These outcomes relate to typical demographic and socio-economic factors that tend to have a strong relationship with voter turnout. We use fuzzy RD estimation as in the main results. The estimates vary around zero and none of them are anywhere near to become significant. Thus, this table gives further support for that the treated and control individuals are similar in their background characteristics, which in turn speaks against any manipulation around the cutoff.

Table F.2: Additional placebo outcomes. Fuzzy RD estimates of starting a general programme. Cohorts starting upper secondary 2008–2016.

	(1) ParInc	(2) ForeignBorn	(3) ForeignPar	(4) Female	(5) ParTurnout2009	(6) HiRelGPA
RD Estimate	0.0406	-0.0369	-0.0551	-0.0276	-0.00343	-0.0161
	(0.0482)	(0.0344)	(0.0567)	(0.0559)	(0.0608)	(0.0549)
Obs	17,030	17,144	17,030	17,122	15,525	17,191
Placebo cut	[-.054; .13]	[-.1; .031]	[-.17; .056]	[-.14; .082]	[-.12; .12]	[-.12; .091]
Conv CI	[-.057; .15]	[-.12; .037]	[-.18; .066]	[-.16; .082]	[-.16; .11]	[-.16; .091]
Rob CI	.39	.31	.37	.52	.67	.58

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Outcomes as follows. Model (1): Mean wage income between the parents when the student finishes lower secondary education, calculated in deciles relative to year, standardized to run between 0 and 1. Model (2): Foreign born (dummy). Model (3): At least one foreign-born parent (dummy). Model (4): Female (dummy). Model (5): Mean turnout between the parents in the 2009 EP election. Model (6): Dummy measuring whether the student belonged to the upper half of the GPA distribution at his or her lower secondary school (takes the value 1 if so).

Fuzzy RD local linear estimates with triangular kernel weighting and MSE-optimal bandwidth. Conventional OLS-standard errors in parentheses. 95 per cent conventional confidence intervals, robust bias-corrected intervals and accompanying p-values are also reported.

Lastly, Table F.3 presents a set of models where we manipulate the GPA cutoff, so called placebo cutoffs (Cattaneo, Idrobo, and Titiunik 2019). The idea is that if the real cutoff truly represents a discontinuity, there should not be any substantial effects to be found at fake cutoffs. We do this falsification test by manipulating the GPA cutoff in steps of 25 GPA points, from 25 up to 50 GPA points below, as well as above, the real cutoff. We examine whether these placebo cutoffs affect the likelihood of starting a general programme at the upper secondary level. In other words, we test whether there is an effect of the placebo cutoffs on the first stage in the fuzzy RD models. In accordance with the recommendation of Cattaneo, Idrobo, and Titiunik (2019), we only include observations above (positive placebo cutoff) or below (negative placebo cutoff) the real GPA cutoff in these models. Otherwise the real cutoff would have a strong impact on the estimates. However, since there are few observations to the left of the real cutoff, the estimates on this side become quite imprecise when they are in turn divided by a placebo cutoff. This is also the main reason for why we do not test placebo cutoffs even further away from the real cutoff (such as $+/-$ 75 GPA points). There would hardly be any observations to the left of such a placebo cutoff.

None of the estimates for the placebo cutoffs in Table F.3 are anywhere close to being significant significant. The estimates for Models (3) and (4) become rather imprecise due to the limited number of observations to the left of the real cutoff.

Table F.3: Placebo cutoffs for the first stage: Effect of placebo cutoffs on starting a general programme. Sharp RDD.

	(1)	(2)	(3)	(4)
RD Estimate	0.00730	-0.00630	-0.0536	0.00350
	(0.0142)	(0.0135)	(0.0888)	(0.0829)
Obs	14,157	14,157	3,074	3,074
Placebo cut	+25	+50	–25	–50
Conv CI	[-.021; .035]	[-.033; .02]	[-.23; .12]	[-.16; .17]
Rob CI	[-.028; .051]	[-.046; .021]	[-.28; .16]	[-.23; .18]
Rob P-v	.56	.47	.61	.84

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

These models present sharp RD estimates of the effect of placebo admission cutoffs on the likelihood of starting a general programme. Models (1)–(2) test positive placebo cutoffs to the right of the real cutoff. Models (3)–(4) test negative placebo cutoffs to the left of the real cutoff.

Local linear estimates with triangular kernel weighting and MSE-optimal bandwidth. Conventional OLS-standard errors in parentheses. 95 per cent conventional confidence intervals, robust bias-corrected intervals and accompanying p-values are also reported.

G Robustness checks of the RD models on voter turnout

In this section we present a set of robustness checks and auxiliary models related to our main RD models on voter turnout. To save space, we for some of these robustness checks only focus on mean turnout between the national elections in 2018 and 2022 and the 2019 EP election.

Sharp RD, controlling for covariates and altering the kernel

In Table G.1 we present sharp RD estimates of our main models in Table 2 in the paper. The RD estimate for the national election of 2018 becomes surprisingly large and almost of the same magnitude as the corresponding estimate in Table 2. This partly reflects that the bandwidth becomes narrower with sharp estimation, and in the case of the 2018 election the negative impact of getting admitted to a general programme is more pronounced close to the cutoff. The first stage also tends to be somewhat stronger for the earlier cohorts that represent a larger share of the sample for the 2018 estimate, meaning that the difference between the fuzzy and sharp estimates becomes less pronounced. The estimates for the EP election and the 2022 national election are instead, as expected with a somewhat weak first stage effect, much smaller than in the main results in Table 2. They are also, again as in the main results, not statistically significant.

However, the coefficient for mean turnout in Model (5) (with municipal FEs) is relative precise and significant at the 95 per cent confidence level. An average negative effect on voter turnout of five percentage points as a result of admission to a general programme must be considered quite substantial. In other words, the intention to treat effect is also significant and the overall conclusion of negative effects of general programme admission is not dependent upon fuzzy RD estimation.

Table G.1: The effect of being admitted to a general programme on turnout. Cohorts starting upper secondary school 2008–2016. Sharp RD (i.e. reduced form).

	(1) Nat 2018	(2) EP 2019	(3) Nat 2022	(4) 2018/19/22
RD Est	-0.0886***	-0.0293	-0.0282	-0.0498**
	(0.0269)	(0.0289)	(0.0220)	(0.0194)
Obs	15,363	17,391	17,374	17,582
Conv CI	[-.141; -.036]	[-.086; .0274]	[-.0712; .0149]	[-.088; -.0117]
Rob CI	[-.168; -.0431]	[-.108; .0348]	[-.0833; .0231]	[-.107; -.0134]
Rob P-v	.00092	.317	.267	.0117
Covs	–	–	–	Muni FEs

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.
The table replicates Table 2 in the paper but using sharp RD estimation instead of fuzzy RD.

In Table G.2 we rerun our preferred main models on mean voter turnout between the 2018, 2019 and 2022 elections, but adding the predetermined covariates birth year and a number of parental characteristics (parental education and earnings as well as whether foreign born). We also test to combine these with municipal fixed effects as we do in the main analyses.

Adding covariates (controls) to an RD design is not needed (or advised) for causal identification but is instead usually done to improve statistical precision or for robustness purposes (Cattaneo, Idrobo, and Titiunik 2019). We here primarily test to add covariates as a robustness check and for two main reasons. First, in the case of birth year, one might suspect that the varying time period for the different cohorts in between their upper secondary schooling and when we record turnout could affect the results. For instance, one could expect the effect to be weaker for earlier birth cohorts who are somewhat older, and who have had the opportunity to vote in previous elections, than for later cohorts who just have finished upper secondary school at the time of the elections we have data on between 2018 and 2022. Second, adding detailed predetermined characteristics on the parents serves the purpose of testing the balance around the cutoff in regard to these variables. If there is a large change in the estimates when adding these covariates, it would be an indication of that there might be imbalance or manipulation around the cutoff.

Table G.2: The effect of starting a general programme on turnout. Fuzzy RD estimates. Controlling for predetermined covariates.

	(1) 2018/19/22	(2) 2018/19/22	(3) 2018/19/22	(4) 2018/19/22	(5) 2018/19/22
RD Estimate	-0.0795*	-0.0894*	-0.104**	-0.0843**	-0.0969**
	(0.0460)	(0.0475)	(0.0466)	(0.0389)	(0.0387)
Observations	17,231	17,122	16,979	17,121	16,978
Conv CI	[-.17; .0108]	[-.183; .00375]	[-.195; -.0125]	[-.161; -.008]	[-.173; -.0209]
Rob CI	[-.187; .00667]	[-.199; -.00047]	[-.22; -.0225]	[-.174; -.0113]	[-.192; -.0268]
Rob P-value	.068	.0489	.0161	.0257	.00937
Covariates	–	Birth year	Par controls	Birth y Muni FEs	Par controls Muni FE

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

The table replicates the models on mean turnout between the 2018, 2019 and 2022 elections in Table 2 in the paper but controls for birth year and parental characteristics. Controls are specified as dummies for each category / FEs. The parental controls add indicators for seven levels of parental education, separately for the mother and father, parental earnings in deciles, also separately for the two parents, as well as dummies for whether the father and the mother is foreign born.

Model (1) in Table G.2 does not include any covariates and is included as a point of comparison. Model (2) demonstrates that controlling for birth year does little to the RD estimate compared to both Model (1) and the main results in Table 2 in the paper. Adding detailed covariates on parental characteristics in Model (3) (see table notes) also has a quite small impact on the estimate. If anything it becomes somewhat more negative but the change compared to main results is within what can be expected from random variation. The fact that these parental characteristics jointly do not affect the estimates on turnout strengthens the validity of the RD approach. In Models (4) and (5) we combine these covariates with municipal fixed effects. As in the main results, these fixed effects improve efficiency but the estimates as such are not affected much at all.

Table G.3 present our main turnout models using uniform weighting (kernel) within the MSE-bandwidth, as an alternative to the triangular weighting (kernel) around the cutoff in the main specification. Switching to a uniform kernel leads to somewhat smaller point estimates and the estimates are no longer statistically significant. This is likely to be a reflection of that the negative estimates are quite strongly driven by the observations closest to the cutoff (see Figure 6 in the paper). These are, on the other hand, the observations that typically are assumed to be the most informative about the causal effect of the cutoff (Cattaneo, Idrobo, and Titiunik 2019). The point estimates as such are also still quite substantial.

Table G.3: The effect of starting a general programme on turnout. Fuzzy RD estimates. Uniform kernel.

	(1) Nat 2018	(2) EP 2019	(3) Nat 2022	(4) 2018/19/22
RD Est	-0.0717 (0.0460)	-0.0528 (0.0612)	-0.0704 (0.0701)	-0.0660 (0.0416)
Obs	15,027	17,043	17,024	17,230
Conv CI	[-.162; .0185]	[-.173; .0672]	[-.208; .067]	[-.148; .0156]
Rob CI	[-.179; .0171]	[-.214; .0569]	[-.218; .0704]	[-.155; .0155]
Rob P-v	.106	.256	.315	.108
Covs	–	–	–	Muni FEs

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

The table replicates Table 2 in the paper but applies a uniform kernel for weighting observations close to the cutoff instead of the triangular kernel in the main specification (Cattaneo, Idrobo, and Titiunik 2019).

Splitting the sample before and after 2013

In Table G.4 we demonstrate models where we split the sample pre and post the grade-reform in 2013, where the grades at the lower secondary level were changed such that the smallest increment went from 5 GPA points to 2.5 GPA points. Since this reform affects the distribution of the running variable, it is relevant to explore how this change affects the estimates. Model (1) is equal to Model (4) in Table 2 in the paper and included for the sake of comparison. Models (2) and (3) show that the negative effect is stronger for applicants during the 2008–2012 period and that the estimate for applicants between 2013–2016 is non-significant, although still negative. While it is reassuring that both estimates are negative, it appears that the negative effects in the main results are mainly driven by the pre 2013 cohorts. However, this may partly be explained by the fact that the first stage tends to become weaker for later years. This is also reflected in that the confidence intervals become much wider in Model (3). The weaker first stage is in turn probably related to that the number of upper secondary schools increased during these years and fewer programmes therefore became oversubscribed towards the end of our study period.

Table G.4: The effect of starting a general programme on mean turnout 2018/19/22. Fuzzy RD estimates. Separate estimates pre and post the 2013 reform

	(1)	(2)	(3)
RD Estimate	-0.0795*	-0.0959**	-0.0508
	(0.0460)	(0.0486)	(0.0787)
Observations	17,231	9,889	7,342
Sample	2008–2016	< 2013	>= 2013
Conventional CI	[-.17; .0108]	[-.191; -.000606]	[-.205; .103]
Robust CI	[-.187; .00667]	[-.231; -.00746]	[-.232; .1]
Robust P-value	.068	.0366	.436

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Local randomization estimation

In Tables G.5 and G.6 we run a local randomization approach for estimating the RD effects. This approach is recommended for discrete running variables when there are a small number of mass points and assuming continuity at the cutoff might be questionable (Cattaneo, Idrobo, and Titiunik 2023; Sekhon and Titiunik 2017). While we reason above in Appendix D that local-linear estimation is preferable for our main models, the alternative local-random approach represents an important robustness check.

Pivotal to the local randomization approach is how to determine the bandwidth around the cutoff, as a wider bandwidth is positive for efficiency but risks to violate the assumption of local randomization. One way is to incrementally increase the bandwidth and test whether you get significant effects on predetermined covariates (Cattaneo, Idrobo, and Titiunik 2023). We run such models on parental education and earnings at the time of students' applications to upper secondary school, as well as on parental turnout in the 2009 EP election and in the 2010 national election. These models generally result in non-significant estimates centered around zero for the most narrow bandwidths but we find positive effects even for moderately wider bandwidths. In the case of parental education and earnings, we find significant positive point estimates for bandwidths equal to –7.5 to +5 GPA points and larger. In the case of parental turnout, we find significant positive estimates for the 2009 EP election from a bandwidth of –12.5 to +10 and for the 2010 election from a bandwidth of –10 to +7.5 GPA points. Against this background, we do not use broader bandwidths in the local randomization models than –7.5 to +5 GPA points but assess the models with narrower bandwidths as more credible.

Note that we do not apply symmetric bandwidths on each side of the cutoff as we have coded the running variable such that units with the value 0 are treated (admitted to a general programme). An equal number of mass points on each side the cutoff thus implies a bandwidth that is one 2.5 point step broader to the left of the cutoff (negative values).

In G.5 we run a simple bivariate OLS regression where turnout is regressed on a dummy indicator for whether a student is above the GPA cutoff (i.e. reduced form estimates). In Model (1) we start with the minimal possible bandwidth, only including the observations just below the cutoff and those just above/on the cutoff (remember that students with a zero value on the running variable just clear the GPA cutoff and are admitted to their preferred general programme). Model (1) shows a strong negative effect on turnout from just making the GPA cutoff. When we add additional observations by increasing the bandwidth with one additional mass point just below and above the cutoff, the point estimate is still negative but smaller and non-significant. In Model (3) we increase the bandwidth an additional step and the point estimate is still negative but close to zero. However, the fact that the estimate approaches zero is likely to be explained by that the wider bandwidth implies that the estimate is affected by differences between students related to their GPA. Remember that we get positive estimates on parental education and earnings at the widest –7.5 to +5 bandwidth in Table G.5.

Tables G.6 is similar to Table G.5 but uses IV-regression, where we instrument starting a general programme with whether a student is just above or below the GPA cutoff for admission. This is a way of mimicking the fuzzy RD estimation approach within the local random framework by estimating the effect on compliers of the admission (LATE). As expected, the effects become stronger than in Table G.5 and the effect for the narrowest bandwidth becomes exceptionally large, signifying an effect of minus 19 percentage points on turnout. The estimate for the –5 to +2.5 bandwidth is still sizable but not significant. Naturally the samples for these models become quite small, affecting precision.

Overall we believe that the local randomization models lend support to our main results as the estimates are quite similar also with this estimation approach; granted that we focus on observations close to the cutoff where the general impact of student GPA is small or negligible. For wider bandwidths the local random assumption does not hold and a comparison of students above and below the cutoff tends to also pick up general effects of differences in student GPA (or selection bias).

Table G.5: The effect of being admitted to a general programme on mean turnout 2018/19/22. Local randomization specification.

	(1)	(2)	(3)
Admitted to a general prog	-0.0558**	-0.0210	-0.00759
	(0.0271)	(0.0169)	(0.0141)
Obs	766	1357	2081
Bandwidth at cutoff	[–2.5, 0]	[–5, 2.5]	[–7.5, 5]

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Conventional standard errors.

Table G.6: The effect of starting a general programme on mean turnout 2018/19/22. Local randomization specification. IV.

	(1)	(2)	(3)
Starting a general prog	-0.187**	-0.0533	-0.0167
	(0.0905)	(0.0399)	(0.0312)
Obs	746	1322	2031
Bandwidth at cutoff	[–2.5, 0]	[–5, 2.5]	[–7.5, 5]

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Conventional standard errors.

Starting a general programme is instrumented with a dummy indicator for whether a student is admitted or not to his or her preferred general programme.

Placebo cutoffs and RD donut

In Table G.7 we present models on voter turnout testing placebo cutoffs. We run a sharp RD specification and similar placebo cutoffs in 25 GPA point increments as in Table F.3. We only include observations above (positive placebo cutoff) or below (negative placebo cutoff) the actual GPA threshold for admission. Otherwise the real cutoff would have a strong impact on the estimates (Cattaneo, Idrobo, and Titiunik 2019).

None of the estimates become significant. The point estimate in Model (4) is, however, quite large but this may be related to that there are few observations left of the –50 placebo threshold (only 348 units), making the estimate sensitive to single observations. Overall the placebo cutoffs do not indicate that there are any discontinuities in voter turnout similar to that at the real cutoff, which would cast doubt on the main results.

Table G.7: Placebo cutoffs for admission to a general programme. Effect of admission to general programme on mean turnout 2018/19/22. Sharp RDD.

	(1)	(2)	(3)	(4)
RD Est	0.0279	-0.00106	0.0000835	-0.105
	(0.0211)	(0.0226)	(0.0690)	(0.0751)
Obs	14,426	14,426	3,158	3,158
Placebo cut	+25	+50	-25	-50
Conv CI	[-.013; .069]	[-.045; .043]	[-.14; .14]	[-.25; .042]
Rob CI	[-.029; .095]	[-.062; .051]	[-.18; .18]	[-.31; .05]
Rob P-v	.29	.86	.97	.16

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Local linear estimates with triangular kernel weighting and MSE-optimal bandwidth. Conventional OLS-standard errors in parentheses. 95 per cent conventional confidence intervals, robust bias-corrected intervals and accompanying p-values are also reported.

Tables G.8, G.9 and G.10 demonstrate models where we exclude the observations closest to the cutoff. This is usually referred to as running an 'RD donut' (Cattaneo, Idrobo, and Titiunik 2019). This robustness check tests to what degree the estimates are sensitive to the observations close to the cutoff, which may be more vulnerable to manipulation. However, an issue with the donut approach is that it requires more extrapolation as the estimation will be done using observations further away from the cutoff. We start with excluding the mass points closest to the cutoff, –2.5 to 0, and then make the donut hole larger.

In Table G.8 we test the first stage; i.e. the effect of admission on what programme a student starts. This outcome is quite robust to excluding the observations close to the cutoff. However, the fact that the first stage becomes substantially stronger in Models (2) and (3) might signify that the donut approach also leads to biased estimates where the positive selection related to a higher GPA plays in. We explore this potential issue in Table G.9 by estimating models on the two pretermined covariates parental education and parental income – measured when students finish lower secondary school. These models show that the point estimates consistently become more positive for a larger 'donut hole'; whereas the parental education estimates border on significance, there are evidence of imbalance at the cutoff for parental income with clearly significant RD estimates. These results imply that the donut hole models need to be interpreted with great care.

In Table G.10 we present donut hole models on mean turnout between the elections in 2018, 2019 and 2022. The estimate for the smallest 'donut hole' is close to zero but for larger 'holes' the estimates turn positive, although imprecise and far from significant.

The donut hole results on turnout stand out in relation to most of the other RD models in the way that we do not find any negative effects on turnout from being admitted to a general programme. This may cast some doubt on our main results. However, the donut hole also appears to introduce imbalance around the cutoff regarding predetermined covariates (parental income and earnings). This implies that the donut approach may not be suitable for our data. It is also the case that observations close to the cutoff are typically regarded as the most informative on the causal effect of the RD treatment, in absence of manipulation around the threshold (Cattaneo, Idrobo, and Titiunik 2019). However, the fact that we find negative effects of voter turnout in our main models speaks against manipulation because it would most likely be more resourceful students (and parents) who engage in trying to acquire admission to their preferred general programme in non-standard ways (and possibly succeed). Such resourceful students would also likely be more prone to turn out to vote and not the other way around. I.e. we believe it to be unlikely that our main results could be driven by manipulation.

Table G.8: RD Donut: Admission to a general programme

	(1) Start Gen P 1st stage	(2) Start Gen P 1st stage	(3) Start Gen P 1st stage
RD Est	0.377***	0.467***	0.588***
	(0.0540)	(0.0684)	(0.0457)
Obs	16,485	15,909	15,200
Restriction at cutoff	[–2.5, 0]	[–5, 2.5]	[–7.5, 5]
Conv CI	[.27; .48]	[.33; .6]	[.5; .68]
Rob CI	[.22; .47]	[.28; .6]	[.46; .69]
Rob P-v	3.9e-08	1.2e-07	4.9e-24

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

The restriction at the cutoff excludes observations within the stated interval.

Table G.9: RD Donut: Predetermined covariates

	(1) Par edu	(2) Par edu	(3) Par edu	(4) Par income	(5) Par income	(6) Par income
RD Est	0.0334*	0.0645	0.0763	0.0328	0.113***	0.121***
	(0.0181)	(0.0448)	(0.0622)	(0.0221)	(0.0364)	(0.0465)
Obs	16,592	16,012	15,295	16,620	16,040	15,323
Restriction	[–2.5, 0]	[–5, 2.5]	[–7.5, 5]	[–2.5, 0]	[–5, 2.5]	[–7.5, 5]
Conv CI	[-.0021; .069]	[-.023; .15]	[-.046; .2]	[-.011; .076]	[.041; .18]	[.03; .21]
Rob CI	[-.0058; .084]	[-.017; .19]	[-.035; .25]	[-.017; .093]	[.044; .21]	[.03; .25]
Rob P-v	.088	.1	.14	.17	.003	.012

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

The restriction at the cutoff excludes observations within the stated interval.

Seven-level parental education indicator taking the mean between the parents. Parental income: earnings in deciles, the mean between the parents. Both indicators are standardized to run between 0 and 1. See Appendix C.

Table G.10: RD Donut: Mean turnout 2018/19/22

	(1) Turnout 181922 Fuzzy RD	(2) Turnout 181922 Fuzzy RD	(3) Turnout 181922 Fuzzy RD
RD Est	0.00270	0.0369	0.0337
	(0.0409)	(0.0596)	(0.0627)
Obs	16,485	15,909	15,200
Restriction at cutoff	[–2.5, 0]	[–5, 2.5]	[–7.5, 5]
Conv CI	[-.078; .083]	[-.08; .15]	[-.089; .16]
Rob CI	[-.085; .09]	[-.11; .17]	[-.14; .17]
Rob P-v	.96	.68	.83

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

The restriction at the cutoff excludes observations within the stated interval.

Collapsing the data to the available mass points

Applying local-linear estimation on a discrete running variable implies that the individual mass points become quite influential. Cattaneo, Idrobo, and Titiunik (2023) recommend to test the local-linear estimation on a dataset that is collapsed to the available mass points to assess to what degree the results are robust. We follow their recommendation and construct a dataset with only as many observations as mass points, where each observation represents the mean of the available original observations per mass point.

In Table G.11 we replicate our main fuzzy RD models in Table 2 in the paper, using such a collapsed dataset. The estimates are still negative but become smaller and are not statistically significant. This is both a reflection of the much smaller 'sample' directly affecting the confidence intervals and that the optimal bandwidth becomes much wider with so few observations (which gives less weight to the observations close to the cutoff where the effect on turnout is most discernable). To assess the impact of the changed bandwidth, we in Table G.12 present models on the collapsed dataset but for which we apply the same bandwidth as for the models in the main results. In this case the estimates are more similar to the main results, although not statistically significant.

The fact that the RD models on the collapsed dataset result in smaller and non-significant estimates suggest that some caution is warranted in interpreting our main results. Nonetheless, the point estimates as such are still clearly negative and represent substantial effects on turnout in a Swedish context with high levels of average electoral participation. There are still no indications of positive effects on turnout from starting a general programme. We also believe that these results must be interpreted in relation to the other robustness checks, in particular the local randomization models, which are recommended as the main alternative to local-linear estimation for datasets with mass points (Cattaneo, Idrobo, and Titiunik 2023). The local-randomization estimates do suggest negative significant effects close to the cutoff. While we believe the models on the collapsed dataset do not change our conclusion that there are no positive effects of starting a general programme on voter turnout, and that these effects even can be negative, they contribute to some uncertainty about the magnitude of such negative effects. This is one of the reasons why we in the paper write that we believe that the exact size of the estimates should not be taken at 'face value'.

Table G.11: The effect of being admitted to a general programme on voter turnout. Collapsing the data to the available mass points. Fuzzy RD and optimal bandwidth for collapsed data.

	(1) Nat 2018	(2) EP 2019	(3) Nat 2022	(4) 2018/19/22
RD Estimate	-0.0314	-0.0474	-0.0560	-0.0399
	(0.0628)	(0.0820)	(0.0390)	(0.0586)
Observations	134	134	133	134
Conventional CI	[-.154; .0916]	[-.208; .113]	[-.133; .0205]	[-.155; .075]
Robust CI	[-.163; .115]	[-.228; .126]	[-.143; .0396]	[-.167; .087]
Robust P-value	.736	.574	.267	.537

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Local linear estimates with triangular kernel weighting and MSE-optimal bandwidth. Conventional OLS-standard errors in parentheses. 95 per cent conventional confidence intervals, robust bias-corrected intervals and accompanying p-values are also reported.

Table G.12: The effect of being admitted to a general programme on voter turnout. Collapsing the data to the available mass points. Fuzzy RD and optimal bandwidth for original data.

	(1) Nat 2018	(2) EP 2019	(3) Nat 2022	(4) 2018/19/22
RD Estimate	-0.0879	-0.0501	-0.0653	-0.0581
	(0.108)	(0.0891)	(0.0635)	(0.0923)
Observations	134	134	133	134
Conventional CI	[-.299; .123]	[-.225; .125]	[-.19; .0592]	[-.239; .123]
Robust CI	[-.683; .0789]	[-.39; .222]	[-.284; .142]	[-.498; .146]
Robust P-value	.12	.592	.513	.283

$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Local linear estimates with triangular kernel weighting. The bandwidth is set to the optimal bandwidth for each model run on the uncollapsed original dataset. Conventional OLS-standard errors in parentheses. 95 per cent conventional confidence intervals, robust bias-corrected intervals and accompanying p-values are also reported.

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