Lost boys? : Secondary education and crime

Uusitalo, Roope

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Lost boys? Secondary education and crime

Kristiina Huttunen [a,*], Tuomas Pekkarinen [b], Roope Uusitalo [c], Hanna Virtanen [d]

[a] Aalto University, VATT Institute for Economic Research, Helsinki GSE and IZA, Finland
[b] VATT Institute for Economic Research, Aalto University, and IZA, Finland
[c] University of Helsinki, VATT Institute for Economic Research, Helsinki GSE and IZA, Finland
[d] ETLA Economic Research, Finland

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ABSTRACT

We study the effect of secondary education on criminal behavior of young men in Finland. We exploit admission cut-offs in over-subscribed programs and estimate the effect of gaining access to a) any secondary school vs no access, b) general vs vocational school, and c) selective vs less selective general school. Our results show that admission to any secondary school has a sizeable negative effect on the propensity to commit crime. There are no effects at the other two margins. The negative effects at the extensive margin are largest in the years following school admission and result in a reduction of the probability of ever committing crime rather than simply delaying the onset of crime. Our results suggest that keeping youth at school at a critical age has effects that last beyond years where effects on enrollment are observed.

1. Introduction

Individuals with low levels of educational attainment are vastly over-represented among criminal offenders.[1] Recent empirical literature has addressed the causality of the relationship between education and crime by relying on variation in compulsory schooling age across jurisdictions and cohorts (Anderson, 2014; Bell et al., 2022; Hjalmarsson et al., 2015; Lochner and Moretti, 2004; Machin et al., 2011), by exploiting events such as temporary school closures (Jacob and Lefgren 2003; Luallen 2006) or by using variation in the age at school entry (Cook and Kang, 2016; and Landersø et al., 2017). These studies have shown that schooling has a negative effect on crime both in the short run, by keeping potential criminals off the streets (incapacitation effect), and in the medium to long run by increasing returns to legitimate work (human capital effect).[2] More recently, the literature has recognized that not only the quantity but also the quality of schooling (Deming, 2011; Heckman et al., 2010) and teachers (Rose et al., 2022) can have an effect on criminal behavior in later life.

Most previous studies on crime and education focus on the effects of variation in the length and quality of compulsory schooling. Much less is known about the role of education and educational choices after compulsory schooling. Applying to secondary education is the first crucial educational choice that adolescents make in most education systems. This choice also usually coincides with the end of compulsory school and with the critical age when propensity to commit crime is rapidly increasing. Secondary education choices can also have large effects on the content of education and on the peer groups that the adolescents are faced with. Moreover, mistakes made in the transition to secondary education can result in higher likelihood of dropout at an age when labor market opportunities are still limited, and the consequences of these mistakes may therefore be costly.

In this paper, we use data on school admissions from Finland and employ regression discontinuity design (RDD) to study the effect of access to different secondary education programs on criminal behavior. We exploit admission cut-offs in over-subscribed programs to compare the outcomes of applicants who are just above the program cut-offs to the outcomes of those who applied to the same programs but just failed to be admitted. The central-

* Corresponding author at: Aalto University and VATT Institute for Economic Research, Arkadiankatu 7, 00101 Helsinki, Finland.

E-mail addresses: kristiina.huttunen@aalto.fi (K. Huttunen), roope.uusitalo@helsinki.fi (R. Uusitalo), hanna.virtanen@etla.fi (H. Virtanen).

[1] According to Harlow (2003), 68% of state prison inmates in the United States did not have a high school diploma in the late 1990s. In Finland, the country which we focus on this paper, 48% of all 25–40-year-old offenders and 75% of those sentenced to prison in 2011 had no education beyond compulsory schooling. The corresponding figure among the whole population in the same age range was only 15%.

[2] For the theoretical discussion, see Lochner and Moretti (2004) and Bell et al., (2022).

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ized admission mechanism allows us to examine the effect of access to secondary education programs on crime at several different margins. First, as compulsory schooling ends at the same time when the students apply to secondary education, we can focus on the individuals who are at a risk of not gaining access to any secondary education and may end up either in non-degree programs or outside education altogether. Second, as is typical in most European education systems, the choice of secondary education in Finland is a choice between academically oriented general and practically oriented vocational education. Focusing on the applicants who have applied to both general and vocational secondary education, we can estimate the effect of track choice on crime. Finally, there are some well-established "elite" institutions that receive a large number of high-quality applications and hence have very high admission thresholds. Results from this margin provide insights to the effects of school quality and peers on criminal behavior.

We link these admission data to administrative data on criminal convictions in order to estimate the causal effect of secondary school admissions on crime. As we are able to follow individuals up to ten years after the initial admission, we can also study the dynamic effects of admission on criminal behavior, as well as, on educational attainment and labor market outcomes. We claim that by focusing on the dynamics of the effect of admission at different margins we can learn about the mechanisms through which secondary education affects crime. Enrollment in secondary education, the characteristics of the peer groups, and labor market earnings are affected differently at each margin and over time. These effects can inform us whether the potential crime-reducing effects of secondary education are driven by incapacitation, human capital, or peer influences.

According to our findings, admission to any secondary education immediately after finishing compulsory schooling has a sizeable negative effect on the criminal behavior of young men. We find no effects on the criminal behavior of women and therefore focus exclusively on men in this paper. Men who are admitted to secondary schools are 52 % less likely to be convicted in a district court within 10 years after admission than men who are not admitted. This is a large effect compared to the effects of compulsory education reported in previous studies.[3] Admission to any type of secondary education also has a positive effect on enrollment and the composition of the peer group immediately after the admission as well as on the level of educational attainment and earnings 10 years after the admission.

In contrast, admission to general vs vocational secondary schools or admission to 'elite' schools has no effect on crime even though their effects on labor market prospects and 'peer quality' are comparable to the effects of getting into any secondary school. Thus, the content of secondary education does not seem to be important for crime outcomes. Furthermore, our results do not provide clear support for the simplest human capital mechanism where education increases the opportunity cost of crime. They also indicate that, at least linear, peer effects are not a sufficient explanation for the effect of schooling on crime.

The examination of the dynamics of the effect of admission to any type of secondary education reveals that whereas the enrollment effects are immediate, crime effects arise with a delay. These findings point to a more nuanced dynamic incapacitation mechanism, similar to the one described in Bell et al. (2022). It seems that post-compulsory schooling provides young individuals with skills and networks that reduce the probability of criminal behavior during the period when the risk of offending is otherwise high.[4] Our results also suggest that access to secondary education mainly reduces minor crimes that are common among young individuals rather than prevents the onset of criminal careers involving more serious crime.

We argue that our paper contributes to the literature on the effects of schooling on crime in the following ways. Whereas most of the earlier literature has exploited changes in compulsory schooling laws to examine the effect of the length of compulsory schooling on crime, we focus on individuals that have completed compulsory schooling and are apply secondary schools. We study the effect of access to secondary education at several margins that differ in their impacts on completed education and labor market outcomes as well as on the content of education and the peer groups that the individuals are subjected to. Following the criminal activity of individuals over time makes it possible to examine whether the effects on crime at these different margins are driven by incapacitation, human capital or peer effects. Finally, we estimate the effect of the access to secondary education on crime using a RD design which is closer to the experimental ideal than studies that rely on legislative reforms to generate exogenous variation in schooling.

The remainder of the paper is organized as follows. Section 2 describes the school application system in Finland. Section 3 describes the data used in the analysis. Section 4 presents our empirical strategy and section 5 the results. Section 6 concludes with some remaining comments.

2. Secondary education in Finland

In Finland compulsory schooling lasts for nine years and ends in May of the calendar year when the students turn sixteen. After this most students apply to secondary education which consists of vocational and general (more academic) tracks. Both tracks typically last for three years. The vocational track trains students for specific occupations, whereas the general track prepares students for tertiary education. Fig. 1a describes the structure of the Finnish education system.

Application to secondary education takes place through a centralized application system maintained by the Finnish National Board of Education. The application process is depicted in Fig. 1b. The process starts in February-March of the final 9th year of compulsory school. Individuals can apply to up to five different secondary school programs (programs in different schools or different programs within schools). Admission is based on program-specific admission scores. For most programs this score is solely based on the compulsory school grade point average (GPA). Some programs give extra points for experience and minority gender or use aptitude tests in addition to grades. Students receive their final grades in May and therefore do not know their exact admission points or admission cut-offs at the time of applying. Furthermore, as the cut-offs vary from year to year, students cannot accurately predict whether they will be admitted to a particular program, making strategic application behavior very difficult.

The supply of slots in each educational program is fixed and is announced before the application process begins. Student selection follows a student proposing deferred acceptance (DA) algorithm (Gale and Shapley, 1962) where each applicant is considered for

[3] Jacob and Lefgren, 2003 find that juvenile property crime decreases by 14 per cent on days when school is in session. Lochner and Moretti (2004) find that a one-year increase in average education levels in a state reduces state-level arrest rates by 11 per cent or more. Hjalmarsson et al. 2015 find that one additional year of compulsory schooling leads to a 6.7% reduction in convictions using Swedish compulsory school reform and data. A more recent paper by Following different compulsory school reforms, Bell et al. (2018) finds a 6% reduction in crime arrest rates in the US, and an 11% reduction in Australia.

[4] The pattern of our results is very similar to the results in Deming (2011), who studies the effect of school quality on crime.

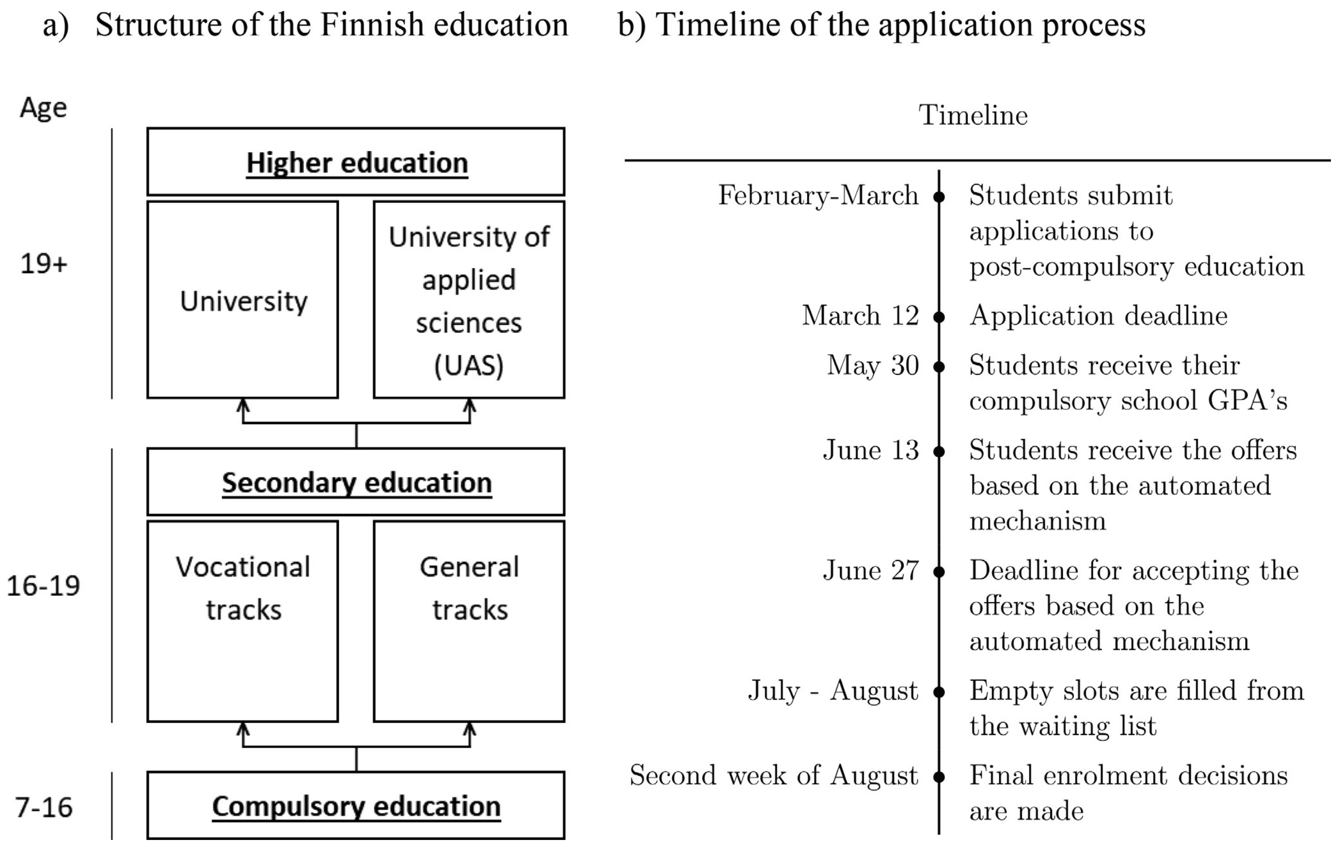

Fig. 1. Finnish education system.
Notes: Fig. 1a shows the possible pathways through education from compulsory education through higher education. Fig. 1b shows the detailed timing of events from application through the beginning of secondary education.

her preferred choice in the first round. Each program tentatively accepts applicants according to its selection criteria up to its capacity and rejects lower-ranking applicants. In the following rounds, the applicants rejected in the previous round are considered for their next preferred program. Each program compares these applicants to the tentatively admitted applicants from previous rounds, rejecting the lowest-ranking applicants exceeding its capacity. The algorithm terminates when every applicant is matched to a track, or every unmatched candidate is rejected by every track in his application.

In this paper we use data on full cohorts on men cohorts that apply to secondary education between 1996 and 2003. During these years over 98 percent of each cohort applied to secondary education upon graduation from compulsory education. Close to 50 percent of them applied only to general school, more than 30 percent only to vocational schools, and approximately 20 percent to both types of schools. Over 80 percent of the applicants received an offer to their first ranked program, whereas close to 5 percent of the applicants failed to obtain any offer to secondary education.[5] The main educational options for applicants not accepted to secondary education are an optional 10th grade of comprehensive school and preparatory training, after which students can apply again to secondary education. However, as failed applicants have already completed compulsory schooling, they are under no obligation to continue in education.[6]

While not all applicants enroll and complete a degree in the track in which they receive an offer, admission to secondary school track is highly predictive of enrollment and later completion of a program. According to our data, of those admitted to the vocational track, 90 percent enroll in vocational education immediately in the following academic year and 79 percent graduate from vocational track within 10 years from admission; of those admitted to the general track, 98 percent enroll in general education and 90 percent graduate from general track within 10 years. Furthermore, around 90 per cent of those admitted to secondary education upon graduation from compulsory schooling eventually complete a secondary school qualification within 10 years. The corresponding figure for those initially rejected is only 60 per cent.

3. Data and estimation sample

3.1. Data

Our primary dataset is the Finnish joint application registry, which contains information on all applicants to secondary education. We focus on eight cohorts of students who graduate from compulsory schooling between 1996 and 2003 and apply to secondary education immediately upon graduation. As nearly every one applies, these data cover practically all Finnish students leaving compulsory education. The data include information on com-

[5] There are more slots than the number of compulsory school graduates. However, older cohorts can also apply for secondary school places. Every year, around 30–40 per cent of all applicants to secondary schools had completed their compulsory schooling before the application year. Typically, these older applicants have been accepted in previous years but wish to switch to another program. Older applicants also include applicants who were rejected by the programs they applied to in previous years.

[6] Around 30 per cent of rejected applicants still find a study place in secondary education before the beginning of the semester. Another 40 per cent of rejected applicants enroll in other training programs and the remaining approximately 30 per cent do not enroll in any type of education or training in the first year.

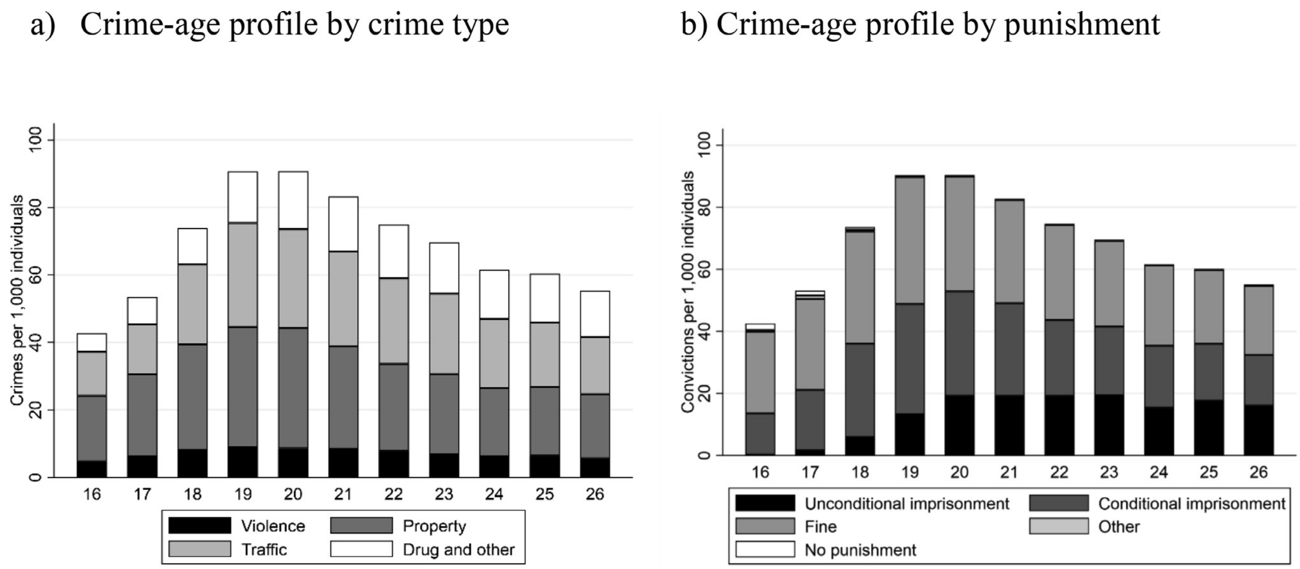

Fig. 2. Crime-age profile by crime type and punishment.
Notes: The figure plots convictions by age, crime type, and punishment for all secondary school applicants in Finland in years 1996–2003.

pulsory school performance, applications to secondary school programs in preference ranking, admission scores, as well as information on the admission results. The data also contain a unique personal identification code that allows us to link application data to other registers and follow individuals over time.

We merge these data with population-wide administrative registers from Statistics Finland for the years 1995–2013. First, we use the Finnish Longitudinal Employer-Employee Data (FLEED) to measure the labor market outcomes, such as employment status and annual earnings. Moreover, the FLEED data provide us with information on the demographic characteristics of the applicants and their parents. Furthermore, we link information on educational attainment from the Student Register and the Register of Completed Education and Degrees which contain information on all post-compulsory enrollments and completed qualifications. Finally, in order to measure criminal activity, we use data on Prosecutions, Sentences and Punishments based on the district court rulings. These data contain detailed information on the timing and type of crime as well as on type of punishment. Our main outcome measure for criminal activity, "Committed any crime", is an indicator of whether an individual has been convicted for any crime by a district court in a given year. In addition, we measure convictions by type of crime and by type of punishment.

In Fig. 2, we plot the crime-age profiles for the eight cohorts used in the analysis by crime type and by punishment. The minimum age of criminal responsibility in Finland is 15. Crime rates increase from age 16 and peak at ages 19 to 20, a common pattern in crime-age profiles reported in several earlier studies. Property crimes are clearly the most common crime category. The share of traffic violations increases from age 18, which is the age when Finns can obtain a driving license. At all ages, most convictions lead to fines or conditional imprisonment.[7] Unconditional imprisonment is rare before age 19 but the severity of crimes and, therefore, the share of imprisonment also increases with age.

3.2. Sample construction

We exploit admission cut-offs to secondary schools to estimate the causal effect of access to secondary education at three distinct margins: a) *extensive margin* that determines admission to any secondary school, b) *general margin* that determines admission to general vs vocational secondary school, and c) *selective margin* that determines admission to more vs less selective general secondary school. We use three parallel regression discontinuity designs that each focus on different admissions cut-offs and exploit a different estimation sample. We have data on a total of 480,000 first-time applicants to 17,000 education program-year combinations over eight years. In this paper we focus solely on the crime outcomes for men.[8] The data includes 240,000 first-time male applicants. The first column in Table 1 reports the descriptive statistics on the full sample of male applicants.

In order to employ our regression discontinuity design, we exclude applicants to programs for which we are unable to determine the admission cut-off. This restriction excludes applicants to special education programs that use alternative criteria for admission as well as applicants to under-subscribed programs that do not reject any applicants.[9] In addition, at each margin we focus on applicants for whom the cut-off is critical for determining access, i.e. the cut-off is the applicant's best chance of getting an offer from a secondary education program of a given type. We also limit the analysis to programs that have at least five such applicants within one grade point below and above the cut-off. We also perform robustness checks using larger estimation samples where we relax the limit of at least five candidates and require instead, that there are at least two applicants on both sides of the admission cut-off. These robustness checks yield very similar results.

[7] First-time offenders are usually not convicted to unconditional imprisonment. Conditional imprisonment leads to imprisonment if the convicted person commits further offences during the probationary period.

[8] In a working paper version (IZA DP 12084) of this paper we performed similar analysis by gender but found no effects for women. In general, women's crime rates are much lower and the estimated effect of admission on female crime is close to zero irrespectively of specification, sample restrictions or the length of the follow-up period.

[9] It is difficult to predict the admission cutoff or which of the education programs will have excess supply at any given year. Hence, applicants cannot manipulate their application behavior in a way that would lead to a guaranteed entry to secondary education.

Table 1
Background characteristics.

	Total data	Extensive margin	General margin	Selective margin
Individual characteristics				
GPA	7.3	7.7	7.5	8.3
Finnish speaker	0.935	0.950	0.937	0.933
Swedish speaker	0.050	0.030	0.050	0.051
Non-Finnish or Swedish speaker	0.015	0.020	0.013	0.016
Lives in a large city[a]	0.277	0.630	0.298	0.561
Crime pre-admission	0.024	0.016	0.009	0.004
Parental background				
Father's income	33,643	40,660	36,086	45,658
Father has secondary degree	0.673	0.727	0.724	0.805
Father has HE	0.160	0.264	0.186	0.369
Mother's income	22,739	25,864	24,273	29,286
Mother has secondary degree	0.751	0.779	0.793	0.864
Mother has HE	0.127	0.193	0.132	0.294
No of individuals	241,216	24,531	9171	8677

Notes: Table reports mean background characteristics for total data and for our three estimations samples. Total data refers to all secondary school applicants in Finland in years 1996–2003. The estimations samples consist of applicants to programs that are critical in determining access to any secondary school vs no offer (Margin of any offer), to general vs vocational secondary education (Margin of general school), and to more vs less selective general secondary school (Margin of selective school), and where the programs are critical thresholds for at least five applicants on both side of the threshold.
[a] Large city refers to top 15 most populated cities in Finland.

When focusing on the extensive margin we use admission cut-offs that are critical in determining the access to any type of secondary education. For each applicant we pick the program that has the lowest cut-off of the programs the applicant listed in his application. The applicants who are rejected at this margin have been rejected by all secondary schools that they applied to. They may enroll in optional 10th grade of comprehensive school or in preparatory training, or they may opt out of education altogether. Rejected applicants can also apply again to secondary education in the following years.

The descriptive statistics on the estimation sample at the extensive margin are reported in the second column of Table 1. The estimation sample represents approximately 10 percent of the full data and approximately-one third of all the rejected applicants. Perhaps somewhat surprisingly this estimation sample seems to come from a slightly more advantaged background than an average male applicant. Both parental education and income are clearly higher in this sample than in the full data and the applicants also seem to come from more urban areas. This positive selection is explained by the fact that the RDD strategy requires secondary school programs to be oversubscribed and therefore the programs that accept all the candidates are excluded from the estimation sample that we use in our extensive margins analysis.

The estimation sample that we use at the general margin focuses on the difference between general and vocational programs.[10] We pick applicants who apply both to the general and vocational tracks and who rank the general track first.[11] The sample is further restricted to those applicants who are above the admissions cut-off of at least one vocational track that they listed in their application. This is to ensure that we focus on a group of applicants for whom admissions cut-offs determine secondary school track type rather than access to secondary schools in general.

Our estimation sample at this margin includes all the applicants whose admission score falls within the bandwidth from the cut-off score of least selective general secondary school listed in the application. Rejected applicants at this margin have been rejected by all other general secondary schools that they applied to and are admitted to a vocational program instead. The estimation sample at this margin covers approximately 10 percent of the full sample of the applicants who applied to both types of tracks. The third column in Table 1 shows that the applicants in this sample are reasonably similar to an average male applicant.

Finally, we construct an elite school sample to examine the effect of admission to more selective general schools at the selective margin. We focus on the applicants that apply to the most selective general secondary schools and for whom the second ranked alternative is a less selective general school. Again, we only keep the applicants who are close to an elite school cut-off and for whom this admission cut-offs determine whether they are admitted to an elite school. We measure selectivity based on the average GPA among those admitted to the school over the eight-year period (1996–2003) and define the top 25 percent of all the general schools in the sample as elite schools. We require that the applicants in the estimation sample are above the cut-off to at least one less selective general school listed in their application. This provides us with effects of the exposure to different school and peer quality, while keeping the track type unchanged. The final column in Table 1 reports the mean background characteristics at this margin. As expected, the applicants in our selective margin sample have better prior school performance and more advantageous family background when compared to the full sample. Furthermore, they live more often in larger cities, where also the best general schools are located.

4. Empirical strategy

4.1. Admission cut-offs and the running variable

To identify the causal effect of access to different types of secondary education on crime we exploit a set of three parallel regression discontinuity designs with admission scores as the running variable. The admission score is based on the grade point average (GPA) of the applicants at the end of compulsory school. In the Finnish education system, all subjects are graded on a scale from 4 to 10 and the GPA is simply calculated as the unweighted average of final grades.

However, secondary schools sometimes apply different scales and subject-specific weights when calculating the admission score and can also use other criteria in addition to the compulsory school GPA to determine admission. To make the running variable comparable across different educational programs, we rescale the admis-

[10] Here we follow Silliman and Virtanen (2022).
[11] This comprises over 90 percent of all those who apply to both type of secondary tracks. Our results are not sensitive to this restriction (not reported here), however, focusing on this group alone will substantially simplify our analysis.

sion scores to GPA units.[12] The program-year-specific cut-off scores are then defined by the admission score of the lowest-scoring candidate admitted to a given program. We drop the observations that are used to define the cut-off score from the analysis. The running variable, r_{ikt}, for applicant i to track k in year t is defined as her distance to the cut-off point in GPA units. These program-specific running variables equal zero at the cut-off point for each program in a given year.

As described in section 3.2 our three regression discontinuity designs examine the effects of access to secondary education at three different margins and, thus, the critical admissions cut-offs also differ across the three RDDs. Namely, we focus on admission cut-offs, where applicant has her best chance of getting an offer to any secondary school, to general secondary school, and to selective general school. By construction each applicant can appear only once in the data.

Fig. 3 shows how crossing these three cut-offs affects the likelihood of admission to any secondary school, to any general secondary school, and to a selective general secondary school. At the margin of access to any secondary school, the extensive margin, crossing the cut-off increases the likelihood of admission to any secondary school substantially, by 60 percentage points. It also has a large, 33 percentage points, effect on the likelihood of admission to general secondary school (one of the two types of secondary schools), but only a minor, 3 percentage points, effect on the likelihood of admission to selective general secondary school. In contrast, at the margin of admission to general school, crossing the cut-off has no effect on the likelihood of admission to any secondary school, but has a significant, 66 percentage point effect on the likelihood of admission to the general track. Those who are below the cut-off at this margin are admitted to the vocational track. Again, we find only a small effect (4 percentage points) on the likelihood of admittance to a selective general school, indicating that the more selective general schools are not a common alternative at this margin. Finally, exceeding the admissions cut-off to the selective track barely affects the likelihood of admission to secondary education or general secondary schools (5 and 7 percentage points, respectively). Instead, crossing this cut-off has a large, 78 percentage point, effect on the likelihood of attending a selective school. Those who are below this admissions cut-off are admitted to less selective general secondary schools.

These figures suggest that the admissions margins affect the education paths in very distinct ways. At each margin, there is first and foremost, a clear, 60 to 80 percentage points, discontinuity in the likelihood of admission to the type of tracks most relevant at the given margin.[13] The three parallel RD designs therefore provide us with a great opportunity to explore different dimensions of the effects of access to secondary education on propensity to commit crime.

4.2. Regression analysis

Our aim is to estimate the causal effect of school admission at three different margins: at the margin of any offer, at the margin of general school vs vocational school, and at the margin of selective general school vs less selective school, on crime, labor market and educational outcomes. We estimate the following reduced form regression separately at each margin of admission:

$$y_{ikt} = \alpha_{kt} + \rho Z_{ikt} + \beta_{kt}(1 - Z_{ikt})(r_{ikt}) + \gamma_{kt} Z_{ikt}(r_{ikt}) + \sum_{x=1}^{5} \delta_x D_{x,ikt} + e_{ikt} \tag{1}$$

where y_{ikt} is the outcome variable (e.g. criminal conviction, income, employment) for applicant i to program k in year t. Z_{ikt} is a dummy variable indicating whether the applicant is above the cut-off of program k in year t, and r_{ikt} is the running variable centered around the cut-off point of each program.

We pool data on all the programs separately for each of the three estimation samples and include cut-off fixed effects α_{kt} and their interactions with the running variable in our regressions to allow the effects of the running variable to differ across programs and on either side of the cut-off. Hence our estimates compare the outcomes of the admitted applicants with the outcomes of the rejected applicants at program-specific admission cut-offs. The results can also be interpreted as weighted averages of the effects at each cut-off in the data that are pooled over all cut-offs. The error terms e_{ikt} are clustered at the program-year level.

A common challenge in RD designs that pool data on several thresholds is how to control for applicant type defined by her preferences over different programs when these preferences are too finely distributed to allow for full non-parametric conditioning. Here we follow Abdulkadıroğlu et al. (2022) and include the local DA propensity scores D_{ikt} as a control for the applicant type.[14] In Abdulkadıroğlu et al. (2022) the local DA propensity score will also restrict the sample to those applicants who have a non-degenerate risk of being assigned to a relevant school. Our sample restrictions and the way we limit the analysis to applicants who are within the bandwidth from the admission cut-off effectively excludes students that have a zero risk of being admitted to a relevant school as well as the applicants who are accepted with probability one to the school that determines the cutoff or to a preferred option. Hence, in our case DA propensity score is used merely to control for the applicant type among those whose education track is determined by the admission cutoffs. As shown in Appendix Table A2, our results are not sensitive to controlling for applicant type with local DA propensity scores.

We estimate the Equation (1) using non-parametric local linear regression with triangular kernel weights and the optimal bandwidth for each program-year combination following the procedure in Calonico et. al. (2014). Since the estimation sample with optimal bandwidth may vary by outcomes, we present our main results using a fixed bandwidth for all the programs and outcomes. This fixed bandwidth is chosen based on the mean of the program specific optimal bandwidths for the main crime outcomes. For robustness, we also report the results using outcome specific bandwidths as well as the sensitivity of our estimates to alternative fixed bandwidths in the appendix.

For our baseline results we focus on the reduced form specification. As discussed in section 4.1, crossing the admissions cut-off increases the likelihood of the relevant admission by 60 to 80 percentage points, yet not all applicants above the cut-offs are observed to be admitted to the given track. To provide insights into the magnitude of the effects of admission, we also use an instrumental vari-

[12] In practice we estimate program-specific regression models where admission scores are explained with the GPA and then divide the score with the coefficient of the GPA. This way, a one-unit change in GPA has the same effect on the rescaled scores in each program.

[13] It is also important to note that the likelihood of admission does not increase to one when exceeding the cutoff at the relevant margins in Fig. 3. The fuzziness of our setting is due to the way in which the offers to the applicants in the waiting list are made. Applicants in the waiting list can turn down offers and the offers can also be missed when the applicants cannot be contacted. We only observe offers accepted by the applicant in the waiting list.

[14] Abdulkadıroğlu et al. (2022) show that it is sufficient to control for the local DA propensity score defined as the applicant's probability of the relevant school assignment to control for the applicant type. In our case, where all schools base their admission on the same running variable and there are no lottery schools, the local DA propensity score reduces to the number of relevant schools in the applicant's preference ranking where she is within the RD bandwidth. Following Abdulkadıroğlu et al. (2022) we use nonparametric controls that are simply dummy variables D_x for each possible value of the DA propensity score that in our case can only take up to five distinct values.

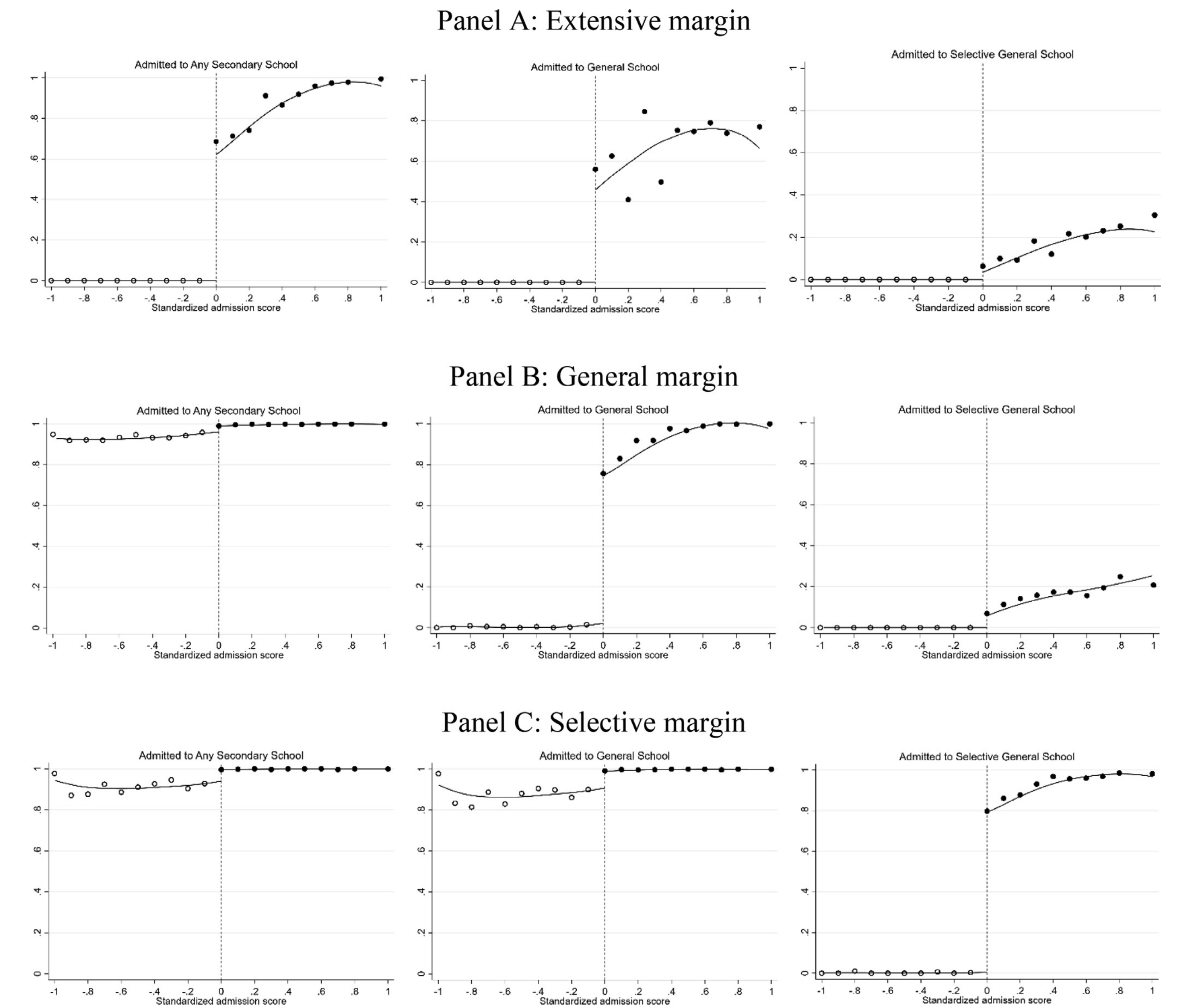

Fig. 3. Admission to secondary education by standardized admission score.
Notes: The figure shows the share of the applicants admitted to any secondary school, to any general school, and to any selective general school against the standardized running variable at the margin of admission to any secondary school (Panel A), to general vs vocational secondary school (Panel B), and to more vs less selective general secondary school (Panel C). The dots correspond to the sample means by 0.1 standardized admission score point bins**.** The lines represent estimated conditional mean functions smoothed using the local linear regression.

able (IV) strategy (fuzzy RDD) where we effectively scale the reduced form effect by the change in the relevant admissions probability. These estimates should be interpreted as local average treatment effects (LATE) of admission to a) any secondary school, b) to general secondary school, and c) to selective general school.

4.3. Defining treatment and counterfactual at each margin

In order to better understand the nature of the treatment and counterfactual at each margin of admission, we first investigate how enrollment and the characteristics of schools and peers change at each margin. We do this by running the model in Equation (1) with the probability of enrollment and peer characteristics as outcomes.

The results in Panel A of Table 2 show that crossing the cut-off to any secondary school increases the likelihood of enrolling in secondary school directly after the end of compulsory school by 35 percentage points. This amounts to over 90 percent increase in the probability of enrollment when compared with the mean enrollment below the admission cut-off. The increased enrollment is directed to both types of secondary tracks but more to the general than vocational track. We can see that crossing the cut-off decreases the likelihood of enrolling in non-degree studies (10th grade or preparatory training) and the likelihood of not enrolling in any type of education or being employed (NEET) substantially. These results suggest that the non-degree studies and NEET are the most relevant alternatives for the applicants who fail to gain access to any kind of secondary education. The Panel B of Table 2 shows that crossing the cut-off to the general track affects mainly the type of education track. However, crossing the admission cut-off to selective general secondary schools has no effect on the track type or on enrollment in secondary education.

As can be seen from Table 2, the characteristics of the peers also change substantially at each cut-off in the sense that admission

Table 2
RDD estimates on enrollment, NEET status, and peer characteristics.

	Reduced form		Mean below[a]
	Panel A: Extensive margin		
Enrollment and NEET status year 1			
Enrolled in secondary education	0.352***	(0.043)	0.322
Enrolled in general secondary education	0.217***	(0.039)	0.157
Enrolled in vocational secondary education	0.138***	(0.041)	0.166
Enrolled in non-degree studies	−0.213***	(0.038)	0.500
In NEET	−0.108***	(0.031)	0.152
Peer characteristics year 1			
Average GPA among peers	0.708***	(0.063)	6.4
Share of male students	−0.000	(0.016)	0.533
Share of peers with prior convictions	−0.012***	(0.002)	0.037
Share of peers with highly educated fathers	0.040***	(0.009)	0.119
Share of peers with highly educated mothers	0.033***	(0.006)	0.091
	Panel B: General margin		
Enrollment and NEET status year 1			
Enrolled in secondary education	0.043**	(0.020)	0.870
Enrolled in general secondary education	0.518***	(0.031)	0.107
Enrolled in vocational secondary education	−0.475***	(0.034)	0.763
Enrolled in non-degree studies	−0.017	(0.013)	0.083
In NEET	−0.026*	(0.015)	0.042
Peer characteristics year 1			
Average GPA among peers	0.943***	(0.045)	6.8
Share of male students	−0.150***	(0.013)	0.663
Share of peers with prior convictions	−0.014***	(0.001)	0.026
Share of peers with highly educated fathers	0.101***	(0.006)	0.070
Share of peers with highly educated mothers	0.071***	(0.004)	0.052
	Panel C: Selective margin		
Enrollment and NEET status year 1			
Enrolled in secondary education	−0.006	(0.011)	0.931
Enrolled in general secondary education	0.011	(0.017)	0.898
Enrolled in vocational secondary education	−0.018	(0.013)	0.035
Enrolled in non-degree studies	0.005	(0.006)	0.036
In NEET	−0.001	(0.010)	0.029
Peer characteristics year 1			
Average GPA among peers	0.434***	(0.039)	7.9
Share of male students	−0.052***	(0.010)	0.492
Share of peers with prior convictions	−0.002*	(0.001)	0.006
Share of peers with highly educated fathers	0.071***	(0.007)	0.248
Share of peers with highly educated mothers	0.069***	(0.006)	0.181

Notes: Table reports the effects of crossing the admission cutoff on enrollment, NEET status, and peer characteristics the first year after admission at the margin of admission a) to any secondary school, b) to general vs vocational secondary school, and c) to more vs less selective general secondary school. The results are from our baseline specification which includes cutoff fixed effects and their interaction with the running variable on both sides of the cutoff, as well as DA propensity scores. The local linear estimations employ an edge kernel and a fixed bandwidth that varies across the three margins: we use a fixed bandwidth of 0.4 standardized unit on each side of the cutoff at the margin of admission to any secondary school, 0.5 standardized unit on each side of the cutoff at the margin of admission to a general secondary school, and 0.7 standardized unit on each side of the cutoff at the margin of admission to a selective general secondary school. Standard errors (in parentheses) are clustered by cutoff.
[a] Mean below admission cut-off within the fixed bandwidth.

subjects the applicant to peers with higher GPA's and more highly educated parents.[15] Crossing the cut-off to any secondary school or general secondary school also significantly reduces the share of peers with prior criminal convictions whereas the gender composition of the peer group changes at the margin of general track and selective general schools.

4.4. Validity of research designs

The institutional features of the Finnish secondary education system make it an attractive context for our study. The centralized application and admissions systems for secondary education in Finland that uses deferred acceptance algorithm provides no incentives for strategic behavior and allow us to identify applicants at the three margins. Furthermore, the timing of the process (Fig. 1b) makes it impossible to know one's own admissions points or the admission cut-offs at the time of application.

We perform two types of checks to empirically examine the validity of our regression discontinuity designs. Fig. A1 displays the distribution of the standardized running variable in our three estimation samples. All the distributions are quite smooth around the cut-offs with no obvious signs of strategic behavior. Secondly, Table A1 shows that there are very few discontinuities in the background characteristics of applicants at the cut-offs. In particular, we observe no differences in pre-admission criminal behavior at the cut-offs. Furthermore, our results are robust for adding these background characteristics as controls (reported in Table A2).

5. The effect of post-compulsory education on crime

5.1. Main results

We begin with a graphical presentation of the reduced-form impact of crossing any of the three admissions margins on crime: admission to any secondary school, admission to a general secondary school and admission to a selective general secondary school. In Fig. 4, we plot the propensity to commit any crime leading to conviction in a district court within five and ten years after secondary school application as a function of the standardized admission score. Panel A of Fig. 4 suggests that admission to any secondary school lowers crime and that this effect is still visible ten years after initial application. However, crossing one of the other two cut-offs and being admitted to general rather than vocational school or to an elite school has no effect of crime.

The regression results reported in Table 3 and Fig. 5 confirm this visual impression. The first row of each panel in Table 3 reports the reduced form estimates of the effect of crossing the admission cut-offs on the crime outcomes. The second row reports the first stage i.e. the effect of crossing the admission cut-off to any secondary school (Panel A), to general secondary school (Panel B) and to selective general secondary school (Panel C). Local average treatment effects for the crime outcomes are reported on the third row of each panel.[16]

According to the regression results in Table 3 admission to any secondary school (vs no secondary school) has a substantial crime reducing effect. By year 10 the applicants who scored higher than the admission cutoff were 8.6p.p. less likely to have ever committed crime than those just below cut-off. An IV-estimate, that uses exceeding admission threshold as an instrument for admission, indicates that admission to any secondary school reduces the likelihood of having ever committed crime by 14.3p.p. Comparison to the average crime rate of those just below admission threshold

[15] Peers are defined as individuals who were admitted to the same school. Peers below the cut-off at the extensive margin are defined as individuals in the same cohort living in the same municipality who were not admitted to any secondary school.

[16] Table 3 and Fig. 5 report results of estimations that use fixed bandwidths which are based on the mean of the optimal bandwidths for the main crime outcomes estimated following the procedure in Calonico et a. (2014). The fixed bandwidth varies across the three margins. Results from estimations using the optimal bandwidths are reported in Table A2 In Fig. A3, we report the sensitivity of our main estimates with respect to alternative bandwidths ranging from 0.1 to 1.

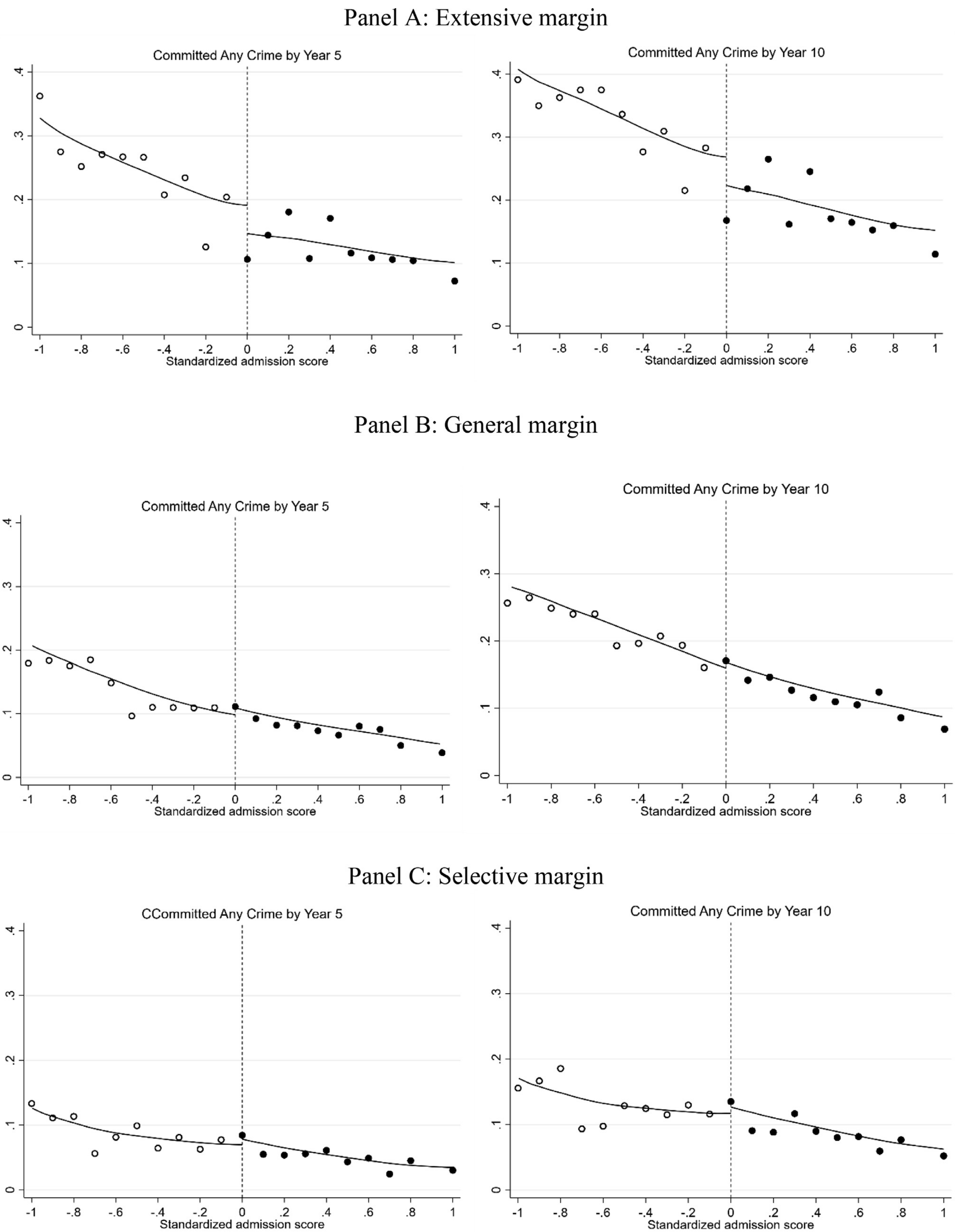

Fig. 4. Crime by standardized admission score.

Notes: The figure shows the share of applicants who committed any crime within five and ten years after admission to secondary education against the standardized running variable at the margin of admission to any secondary school (Panel A), to general vs vocational secondary school (Panel B), and to more vs less selective general secondary school (Panel C). The dots correspond to the sample means by 0.1 standardized admission score point bins**.** The lines represent estimated conditional mean functions smoothed using the local linear regression.

Table 3
RDD estimates on crime.

	Committed any crime by		
	year 1	year 5	year 10
	Panel A: Extensive margin		
Reduced form	0.012	−0.098***	−0.086**
	(0.019)	(0.034)	(0.039)
First stage	0.602***	0.602***	0.602***
	(0.043)	(0.043)	(0.043)
LATE: Admitted	0.020	−0.162***	−0.143**
	(0.032)	(0.057)	(0.066)
Mean below[a]	0.048	0.196	0.275
Observations	4395	4395	4395
	Panel B: General margin		
Reduced form	0.006	0.019	0.029
	(0.006)	(0.024)	(0.027)
First stage	0.664***	0.664***	0.664***
	(0.029)	(0.029)	(0.029)
LATE: Admitted	0.009	0.028	0.043
	(0.009)	(0.036)	(0.041)
Mean below[a]	0.012	0.109	0.184
Observations	4265	4265	4265
	Panel C: Selective margin		
Reduced form	−0.010	0.004	0.011
	(0.010)	(0.017)	(0.025)
First stage	0.772***	0.772***	0.772***
	(0.032)	(0.032)	(0.032)
LATE: Admitted	−0.013	0.006	0.014
	(0.013)	(0.022)	(0.032)
Mean below[a]	0.011	0.076	0.122
Observations	4262	4262	4262

Notes: Table reports the effects of crossing the admission cutoff on committing crime within one, five, and ten years after admission at the margin of admission a) to any secondary school, b) to general vs vocational secondary school, and c) to more vs less selective general secondary school. The results are from our baseline specification which includes cutoff fixed effects and their interaction with the running variable on both sides of the cutoff, as well as DA propensity scores. The local linear estimations employ an edge kernel and a fixed bandwidth that varies across the three margins: we use a fixed bandwidth of 0.4 standardized unit on each side of the cutoff at the margin of admission to any secondary school, 0.5 standardized unit on each side of the cutoff at the margin of admission to a general secondary school, and 0.7 standardized unit on each side of the cutoff at the margin of admission to a selective general secondary school. Standard errors (in parentheses) are clustered by cutoff. Estimates with outcome-specific optimal bandwidths are reported in Table A 2.

[a] Mean below admission cut-off within the fixed bandwidth.

(27.5 %) indicates that admission to secondary education can reduce crime rates by more than 50 %.

Interestingly, there are no effects immediately after admission. In Fig. 5, we examine the time profile of the effect of admission on crime in more detail. We report both effects on crimes in a given year (left columns) and cumulative fraction of applicants convicted at least once by a given year (right column). We find robust evidence on negative effect of admission to secondary education on crime. The effects are significantly negative from two to five years after admission. In subsequent years, the effect fades away and approaches zero by the end of the follow-up period. The right column of panel A indicates that admission to secondary education still results in a reduction in the likelihood of ever committing crime rather than simply delaying the onset of crime.

We repeat the same procedure to evaluate what happens at the other two thresholds: at margin of being admitted to general secondary school, and at the margin of being admitted to selective general school. The estimates reported in Table 3 and Fig. 5 are much smaller and far from being statistically significant irrespective of whether we examine the effects on committing crime within 1, 5, or 10 years. The only exceptions are the negative effect of general secondary school admission in year 3 and its positive effect in year 4 reported in left hand side Fig. 5. Although it is tempting to interpret these as effects on the length of secondary school (some vocational programs only last for two years) we are reluctant to draw strong conclusions based on these two point estimates.

We conclude that admission to general vs vocational education or admission to more vs less selective school has no additional effect on crime. The only margin at which we find reduction in crime is the margin of being admitted to any school. To better understand the mechanism beyond the crime reducing effect of school admission, we study next how enrollment, activity and income evolve over time at these three admission margins.

5.2. Effects on enrollment and labor market

Comparing the time-profiles of the effects of admission on crime with the effects on enrollment, inactivity, and labor market outcomes can provide useful insights into the mechanisms through which secondary education affects crime. If gaining admission to any secondary school reduces crime primarily during the years when the admitted applicants are more likely to be enrolled in post-compulsory education than the rejected applicants, we would argue that admission reduces crime mainly through its incapacitating effects. However, if the reduction in crime coincides with the positive effects of admission on labor market outcomes, the timing is more in line with the human capital mechanism.

In Fig. 6 we plot the effects of admission on enrollment rates and income - the two most reliably measured education and labor market related measures in our data.[17] Panel A in Fig. 6 shows that admission to secondary education has a large positive effect on enrollment in the first year after applying.[18] Admission also has a negative effect on enrollment rate in year four after admission. This is probably due to a delay in the entry of some of the rejected applicants to secondary education so that they have not yet completed their three-year program by year four. In the right column of Panel A, we show that admission to any secondary education has a positive effect on income ten years after admission. This income effect mainly reflects the fact that immediate admission has a long-lasting effect on the likelihood of eventually obtaining a secondary school degree (see appendix Table A3). Higher enrollment rates of the admitted applicants in the first year also do not lead to lower incomes. An increase of enrollment mainly decreases inactivity rates (see appendix Fig. A2, Panel A) with very little effect on employment. Apparently labor market prospects of rejected applicants are not very lucrative at age 16.

In Panel B of Fig. 6 we show that admission to general vs vocational secondary school also has significant effects on enrollment rates. For the first years these effects reflect lower drop-out rates in general secondary schools compared to vocational schools. After year 3 the effects are related to higher participation rates in tertiary education among general secondary school students. Consistent with Silliman and Virtanen (2022) we find that admission to general track has a negative effect on income from year four after admission. This effect is partly due to higher participation rates in tertiary education and, as a consequence of prolonged studies, lower employment rates. However, as in Silliman and Virtanen there are also long-term effects on income. For those at the margin vocational education often provides better labor market prospects than general education.

[17] In the appendix Fig. A2 we also analyze the effects on employment and inactivity rates. These employment rates are based on pension insurance contributions and are not fully consistent across age groups as pension insurance was not compulsory for employees under 18 until 2017. Inactivity (NEET) indicates being outside education, employment and training and hence cannot be any more reliable than employment data. Enrollment data comes directly from schools and income data from tax authorities and should be highly reliable.

[18] In order to make sure that we follow the same individuals throughout these 10 years, we use a fixed bandwidth instead of track- and year-specific optimal bandwidths in these regressions.

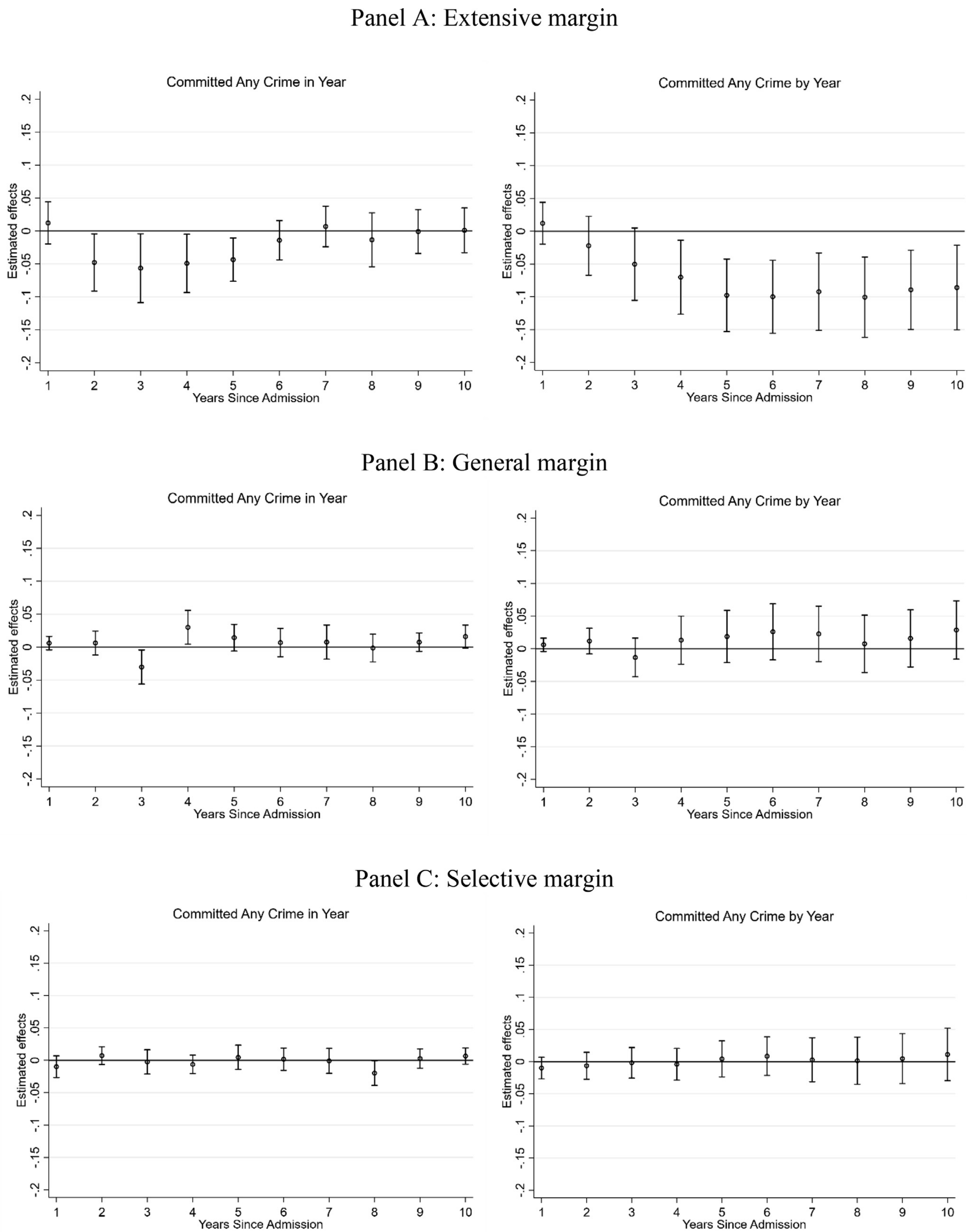

Fig. 5. RDD estimates on crime.

Notes: Figure reports the effects of crossing the admission cutoff on the propensity to commit any crime in the given year and by the given year estimated separately for each 10 years following the admission to secondary education. The effects are estimated at the margin of admission to any secondary school (Panel A), to general vs vocational secondary school (Panel B), and to more vs less selective general secondary school (Panel C). The results are from our baseline specification which includes cutoff fixed effects and their interaction with the running variable on both sides of the cutoff, as well as DA propensity scores. The local linear estimations employ an edge kernel and a fixed bandwidth that varies across the three margins: we use a fixed bandwidth of 0.4 standardized unit at the margin of any secondary school, 0.5 standardized unit at the margin of a general school, and 0.7 standardized unit at the margin of selective general school. Standard errors are clustered by cutoff. The graphs also show the 95 percent confidence intervals. Years are defined as August 15th-August 14th following year.

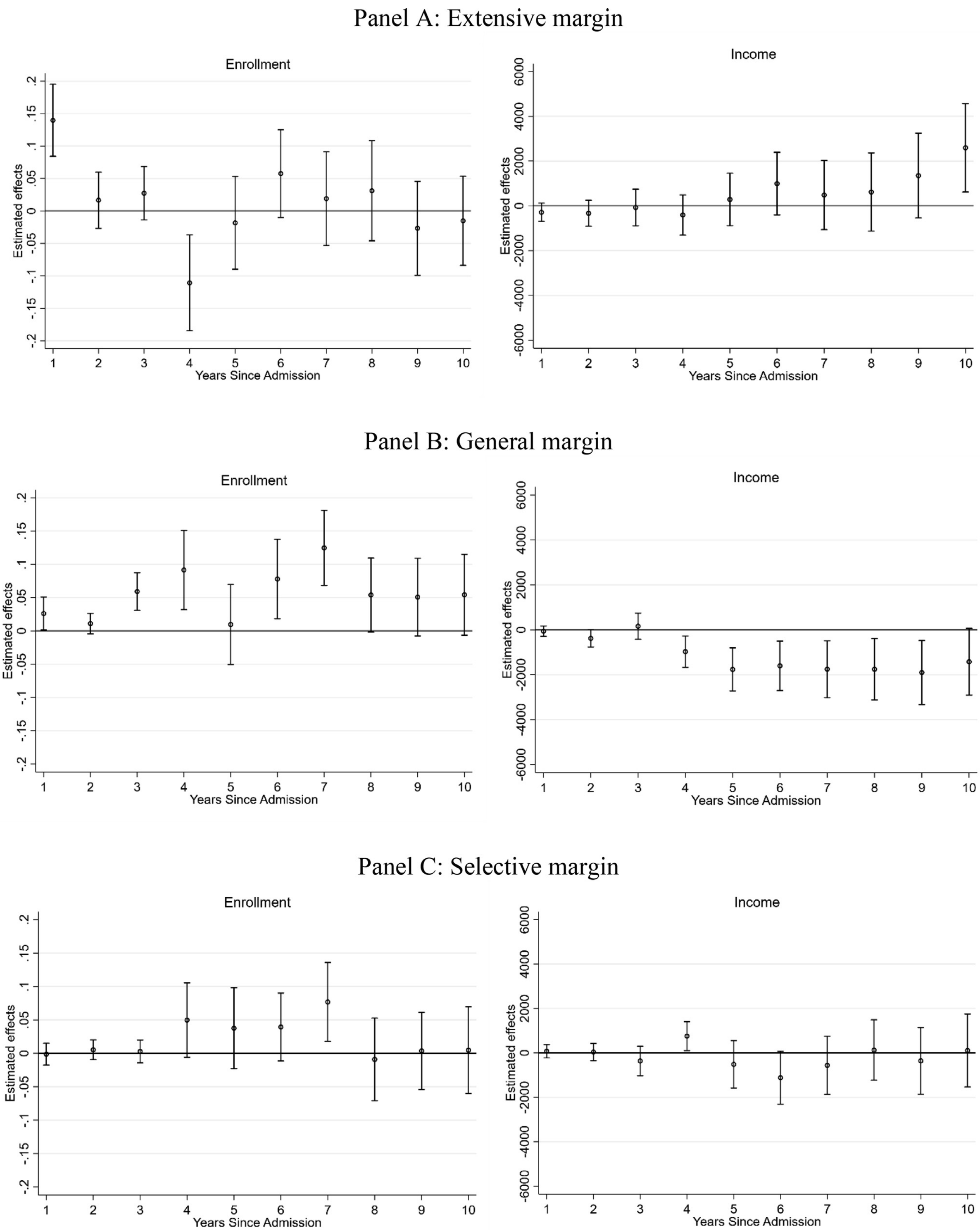

Fig. 6. RDD estimates on enrollment status and annual income.

Notes: Figure reports the effects of crossing the admission cutoff on enrollment and annual income estimated separately for each 10 years following the admission to secondary education. The effects are estimated at the margin of admission to any secondary school (Panel A), to general vs vocational secondary school (Panel B), and to more vs less selective general secondary school (Panel C). The specification includes cutoff fixed effects and their interaction with the running variable on both sides of the cutoff, as well as DA propensity scores. The local linear estimations employ an edge kernel and a fixed bandwidth that varies across the three margins: 0.4 standardized unit on each side of the cutoff at the margin of admission to any secondary school, 0.5 at the margin of admission to a general secondary school, and 0.7 at the margin of admission to a selective general secondary school. Standard errors are clustered by cutoff. The graphs also show the 95 percent confidence intervals. Years are defined as August 15th-August 14th following year. Incomes are indexed to 2010 euros.

Finally in Panel C of Fig. 6 we examine the effects of selective school admission on enrollment and earnings. Here we find some positive effects on enrollment but only after four years since admission i.e. mainly in participation in tertiary education. Effects on income seem to be small and not fully in line with enrollment effects.

5.3. Effects by crime type

In Table A4 we examine the effects on committing crime within five years after school admission by type of crime and by type of punishment. Again, we estimate the effects at all margins of admission. The results in Panel A show that the reduction of crime in this margin of any offer (extensive margin) is driven by relatively minor crimes that are punished by fines. We also find that the effects are negative on all major crime types, but that property, traffic, and drug related crimes (others) contribute most to the aggregate crime reducing effect of education.[19] The last columns of the table show the effect by total number of offences during the five-year period. We find that the effect of committing any crime (or at least one crime) during the period, is stronger and more precisely estimated, than the effect on committing at least two or at least three crimes during this period. This result indicates that the effect of access to secondary school on crime is driven by first offences and no additional effect is found when we use at least two or at least three convictions as the dependent variable. Panels A and B give no support that school admission to general vs vocational or to selective general vs less selective general affects any type of criminal behavior.

5.4. Discussion

The results presented in the previous section seem to contradict the simplest explanations of crime-reducing effects of education. According to the human capital explanation, education improves prospects in legal activities and hence increases the opportunity cost of crime. Using earnings are as a measure of labor market prospects, we should then expect to see effects on crime alongside positive earnings effects of admission to secondary education. We found that admission to secondary school has effects on earnings only in the long term, but effects on crime appear much sooner. We also found that admission to general (instead of vocational) secondary school lowers earnings but found no effects on crime in the years where the effects on earnings are largest. This pattern of results suggests that the traditional human capital mechanism cannot explain the reduction in criminal activity unless the students are forward-looking and react to long-term labor market prospects.

It also seems difficult to explain our results with peer-effects. Crossing the admission threshold to secondary education has a large effect on 'peer-quality' measured by parents' education, average grades of the peer group or prior criminal convictions. However, crossing the threshold to general vs vocational secondary education or crossing the threshold to elite vs less selective general secondary schools also affects peer groups. Yet, crossing either of these thresholds has no detectable effects on crime.

Our results do not lend support to the simplest incapacitation theories either. The effects on enrollment are largest immediately after application when those rejected from all secondary schools have substantially lower enrollment rates and higher inactivity rates than those admitted. Yet the effects on crime are only observed two to five years after admission. It is naturally possible that first crimes do not lead to conviction but this pattern of results is also consistent with the time pattern of crime effects observed in some previous studies.[20] These results suggest that rather than simply incapacitating potential offenders or by increasing the opportunity cost of crime, access to secondary education provides individuals with skills or attitudes that shelter them from criminal activity during the time in which they are making the transition from secondary school to further education or to the labor market. This transition coincides with ages at which young individuals are typically most likely to engage in criminal activity. Lochner and Moretti (2004) as well as Bell et al. (2022) call this mechanism dynamic incapacitation.

Finally, our results imply that access to secondary education is more successful in reducing crime among individuals whose criminal activity is likely to be confined to this period between adolescence and adulthood. As we find no effects on serious crime, it seems less likely that secondary education is an effective tool in reducing more serious crime committed by individuals who are more likely to end up in a criminal career.

5.5. Robustness

In Table A2 we examine the sensitivity of our main results to changes in the analysis sample and the specification. The first Panel A replicates the results of our main exercise, where we use cut-off fixed effects and their interactions with the running variable, fixed bandwidth and include indicators for DA propensity score as described in Section 4.2. Panel B drops these DA propensity score indicators from the specification. This has little impact on our results reflecting the fact that our main approach already restricts the comparison to observations with similar application behavior. Panel C reports the results without cutoff-specific interactions and Panel D show how sensitive the results are to adding controls for the applicant's mother tongue, age at graduation, gender, parental education and income, as well as for living in one of the 15 largest cities in Finland. Both specifications provide very similar results than the baseline results reported in the first panel.

Next, we examine how sensitive our results are to the choice of bandwidth. Panel E in Table A2 shows the results where optimal bandwidth is used for each outcome. These results are quite similar to the baseline estimates, where we use a fixed bandwidth that is chosen based on the mean of the program specific optimal bandwidths for the main crime outcomes. Fig. A3 reports the main results using alternative fixed bandwidths that vary between 0.1 and 1 standardized admissions units below and above the cut-offs. These results indicate that the results at the extensive margin are not sensitive to the choice of bandwidth as long as the bandwidth does not exceed 0.5. The effects get smaller when the bandwidth size gets very large, possibly due to the thinness of the data below the cut-off. In order to make sure that we have enough observations around each cut-off, we use fairly restrictive sample definitions and, as a result, lose a large share of the original data (as shown in Table 1). We check the sensitivity of our results to these restrictions by using a broader sample where we only require that there is at least two applicants on either side of the admission cut-off for whom the program was his or her best chance to get into post-compulsory schooling. Using this larger sample, the point estimates are smaller than in the baseline results, but the effects on crime remain statistically significant (Panel F).

6. Conclusions

While a large literature has documented the relationship between crime and compulsory education, we still know little

[19] These results are in line with Deming (2011), who finds evidence that being exposed to more crime-prone peers mainly increases drug-related offenses.

[20] See the papers by Bell et al. 2022, Deming (2011) and Landersø et al. (2017).

about the effects of secondary education on crime. In this paper we aim to fill this gap in the literature by exploiting admission cut-offs in different secondary education tracks in Finland. We examine the effect of secondary education on crime at three margins: 1) at margin of getting any offer vs no offer (the extensive margin), 2) the admission to academic general vs vocational track, and 3) the admission to selective vs less selective institutions. We follow secondary school applicants for several years after the admission date and examine the timing of the effects of admission on a rich set of crime outcomes in relation to its effects on enrollment and on labor market outcomes. By comparing evolution of outcomes after admission at different margins, we can better understand the mechanism through which education affects criminal behavior.

Our results show that being successful in gaining access to secondary education decreases the likelihood of committing crimes among young men. Men who are admitted to secondary schools are 52 % less likely to be convicted within 10 years after admission than men who are not admitted. The effect is sizeable when compared with previous estimates that have exploited variation in the length of compulsory schooling. However, the crime reducing effect of secondary education is restricted to the extensive margin. We find no effect on crime when examining admission to the general track or to the more selective general secondary schools.

Our results indicate that simple incapacitation, human capital, or peer group mechanisms cannot alone explain the effect of access to secondary education on crime. The largest effects of admission on crime do not coincide with its effects on enrollment or labor market outcomes. Furthermore, the fact that we find no effects on crime from gaining access to general track or to a selective secondary school even though peer groups change significantly at these thresholds suggests that simple peer effects are unlikely to be the underlying mechanism.

These results point to a more nuanced mechanism through which education affects crime. Secondary education seems to provide individuals with capabilities that ease the transition from secondary school to further education in a way that reduces the probability of engaging in criminal activity. The time pattern of results is consistent with several earlier papers that have studied the timing of the effects of education on crime.[21] However, the type of secondary education or the selectiveness of the programs do not seem to play an important role in shaping criminal behavior. The fact that we find no effect on more severe crimes, the probability of committing more than one criminal offense or crimes committed several years after admission may indicate that different measures are needed to prevent an individual becoming a career criminal. Our results however suggest that facilitating access to post-compulsory secondary education during the critical years when the risk of offending is high may be an effective way to prevent some individuals from ever engaging in criminal activity.

Data availability

The authors do not have permission to share data.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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Appendix

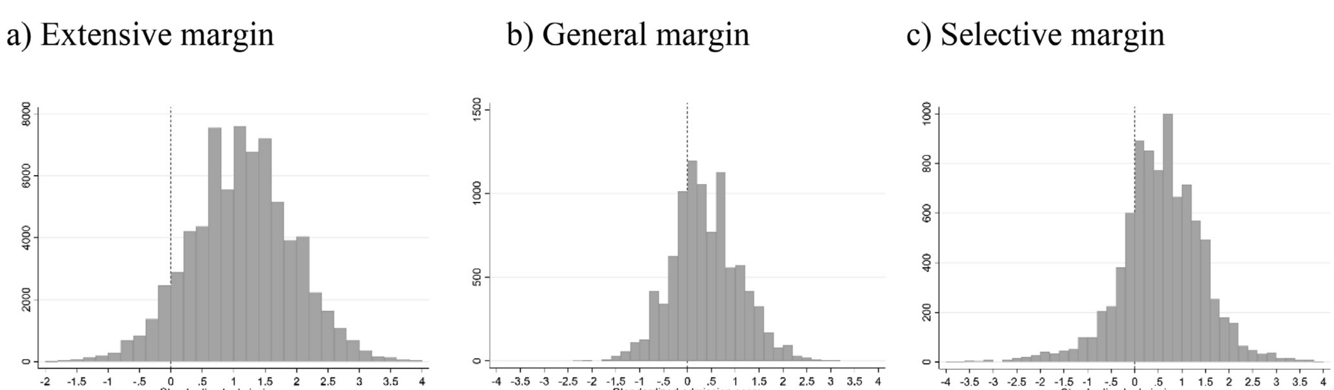

Fig. A1. Score distribution of applicants with respect to admission cut-offs.
Notes: The figure plots the frequency of applicants plotted against the standardized admission score in our three estimation samples. The standardized admission score is determined in respect to the cutoff that determines access to any secondary education (Fig. A1a), to a general secondary school (Fig. A1b), or to a selective general school (Fig. A1c).

[21] See Bell et al. 2022, Deming, 2011, and Landersø et al., 2017.

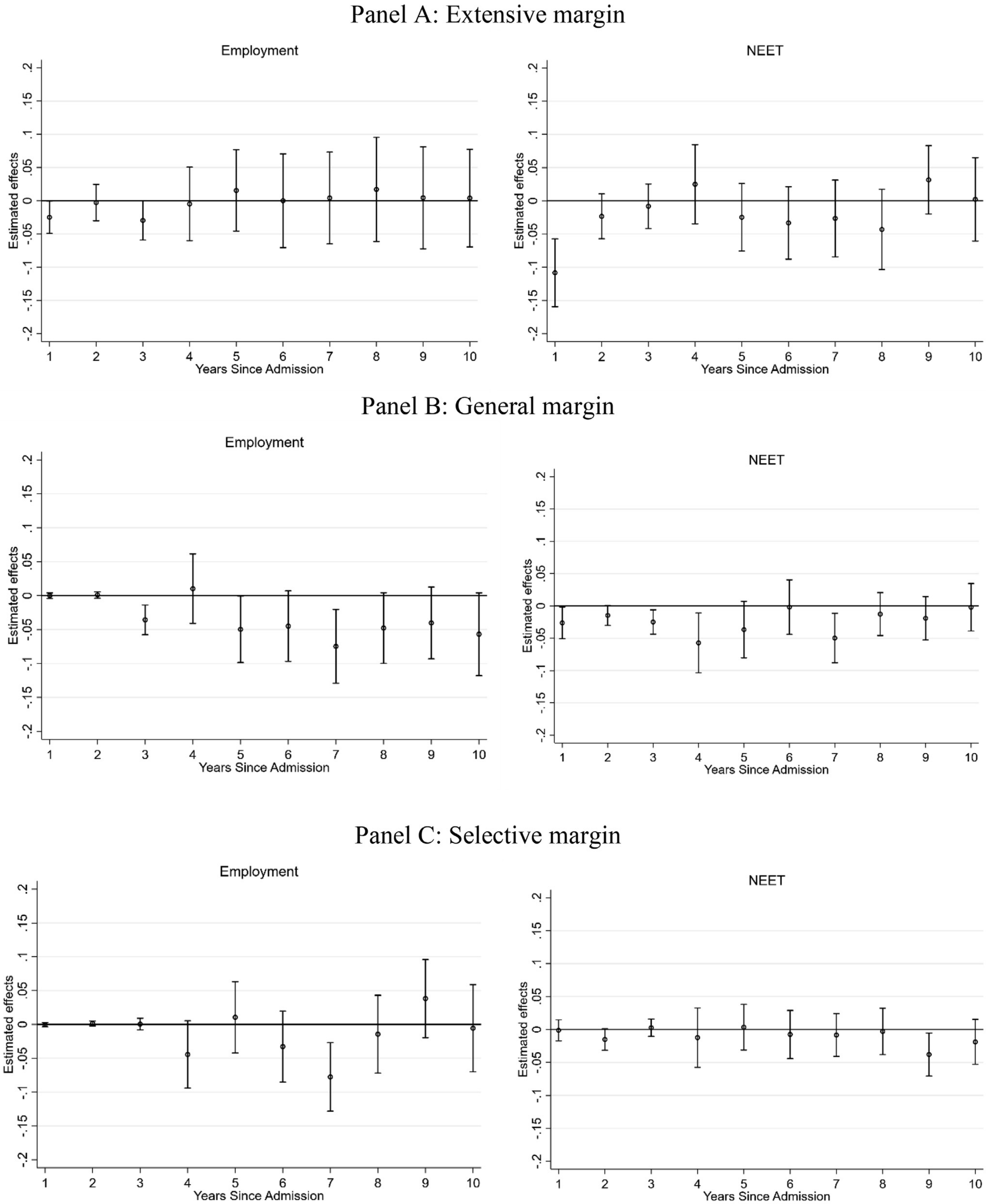

Fig. A2. RDD estimates on employment and inactivity.

Notes: Figure reports the effects of crossing the admission cutoff on employment and inactivity (not in employment education or training) estimated separately for each 10 years following the admission to secondary education. The effects are estimated at the margin of admission to any secondary school (Panel A), to general vs vocational secondary school (Panel B), and to more vs less selective general secondary school (Panel C). The results are from our baseline specification which includes cutoff fixed effects and their interaction with the running variable on both sides of the cutoff, as well as DA propensity scores. The local linear estimations employ an edge kernel and a fixed bandwidth that varies across the three margins: we use a fixed bandwidth of 0.4 standardized unit on each side of the cutoff at the margin of admission to any secondary school, 0.5 standardized unit on each side of the cutoff at the margin of admission to a general secondary school, and 0.7 standardized unit on each side of the cutoff at the margin of admission to a selective general secondary school. Standard errors are clustered by cutoff. The graphs also show the 95 percent confidence intervals. Years are defined as August 15th-August 14th following year.

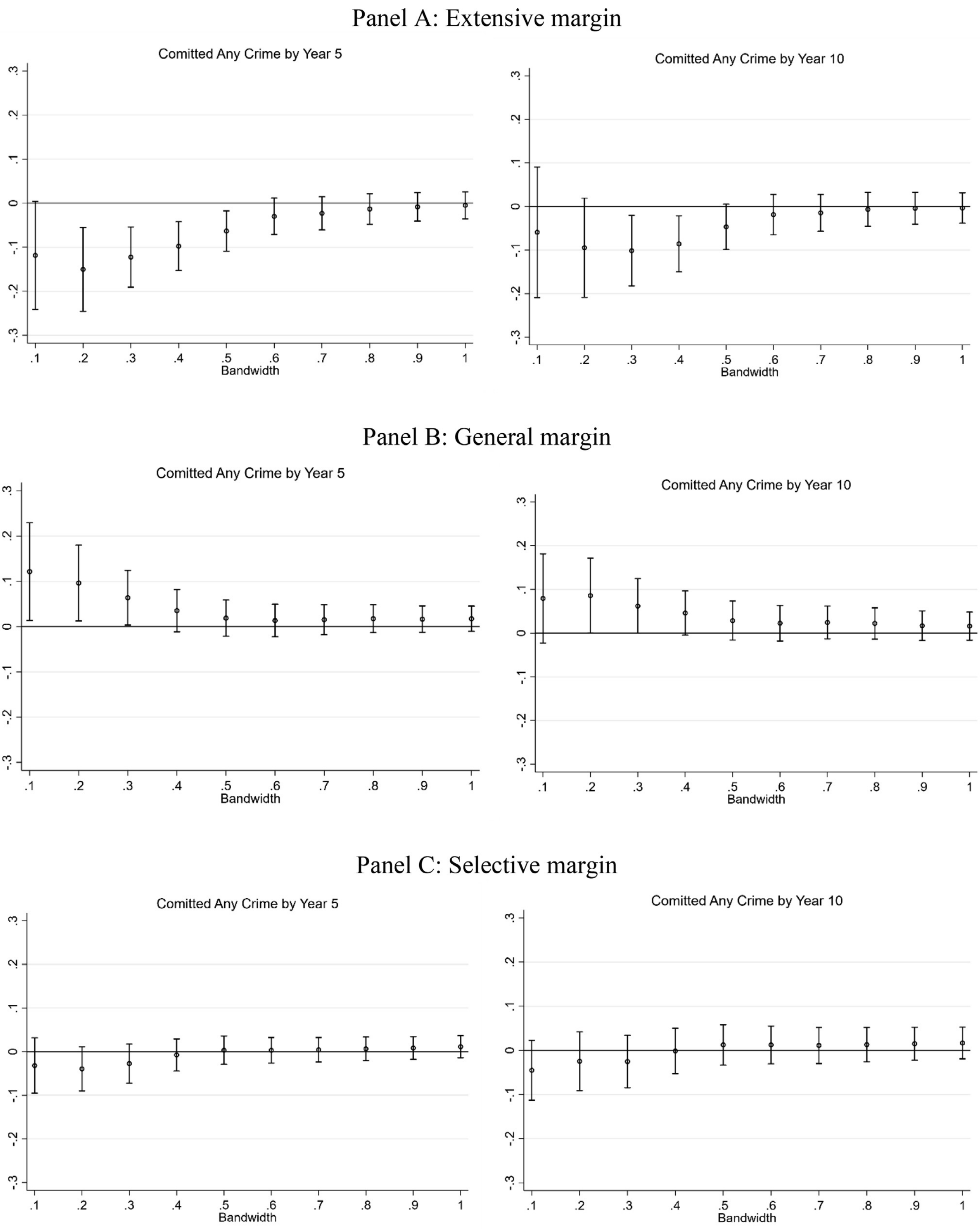

Fig. A3. Robustness to bandwidth choice.
Notes: Figure reports the effects of crossing the admission cutoff on propensity to commit any crime within five and ten years after admission to secondary education. The effects are estimated at the margin of admission to any secondary school (Panel A), to general vs vocational secondary school (Panel B), and to more vs less selective general secondary school (Panel C). The results are from our baseline specification which includes cutoff fixed effects and their interaction with the running variable on both sides of the cutoff, as well as DA propensity scores. The local linear estimations employ an edge kernel and a fixed bandwidth that varies from 0.1 to 1.0 standardized unit on each side of the cutoff. Standard errors are clustered by cutoff. The graphs also show the 95 percent confidence intervals. Years are defined as August 15th-August 14th following year.

Table A1
Covariate balance at admission cut-offs.

	Extensive margin		General margin		Selective margin	
Individual characteristics						
Crime before admission	−0.014	(0.014)	−0.004	(0.004)	0.001	(0.005)
GPA	0.016	(0.020)	−0.000	(0.000)	−0.001	(0.014)
Native language Finnish	0.005	(0.017)	0.019*	(0.010)	−0.005	(0.015)
Native language Swedish	−0.010	(0.007)	−0.003	(0.005)	−0.001	(0.008)
Age at graduation	0.023	(0.030)	−0.012	(0.016)	0.012	(0.016)
Lives in a large city[a]	−0.013	(0.026)	0.004	(0.011)	−0.020	(0.023)
Parents						
Information on father	0.019	(0.022)	0.000	(0.012)	0.011	(0.016)
Information on mother	0.008	(0.008)	0.012*	(0.007)	0.012	(0.009)
Information on both parents	0.031	(0.021)	0.010	(0.013)	0.025	(0.019)
Father's income	9335	(8661)	−2843	(6425)	3481	(2806)
Father in NEET	−0.033	(0.037)	−0.043	(0.028)	0.011	(0.026)
Father has secondary degree	0.020	(0.048)	0.036	(0.034)	−0.004	(0.027)
Father has HE	0.030	(0.033)	0.052*	(0.027)	−0.002	(0.039)
Mother's income	1896*	(1117)	944	(864)	−143	(1608)
Mother in NEET	−0.010	(0.034)	−0.037	(0.026)	0.012	(0.025)
Mother has secondary degree	0.022	(0.040)	0.027	(0.031)	0.002	(0.027)
Mother has HE	0.022	(0.028)	0.012	(0.024)	0.052*	(0.031)

Notes: Table reports the effects of crossing the admission cutoff on individual and parental characteristics at the margin of admission a) to any secondary school, b) to general vs vocational secondary school, and c) to more vs less selective general secondary school. The results are from our baseline specification which includes cutoff fixed effects and their interaction with the running variable on both sides of the cutoff, as well as DA propensity scores. The local linear estimations employ an edge kernel and a fixed bandwidth that varies across the three margins: we use a fixed bandwidth of 0.4 standardized unit on each side of the cutoff at the margin of admission to any secondary school, 0.5 standardized unit on each side of the cutoff at the margin of admission to a general secondary school, and 0.7 standardized unit on each side of the cutoff at the margin of admission to a selective general secondary school. Standard errors (in parentheses) are clustered by cutoff.
[a] Large city refers to top 15 most populated cities in Finland.

Table A2
Robustness checks.

	Extensive margin		General margin		Selective margin	
	Committed any crime by		Committed any crime by		Committed any crime by	
	year 5	year 10	year 5	year 10	year 5	year 10
	Panel A: Baseline estimates					
Reduced form	−0.098***	−0.086**	0.019	0.029	0.004	0.011
	(0.034)	(0.039)	(0.024)	(0.027)	(0.017)	(0.025)
Observations	4395	4395	4265	4265	4262	4262
	Panel B: Without DA propensity scores					
Reduced form	−0.096***	−0.083**	0.019	0.029	0.004	0.011
	(0.034)	(0.040)	(0.024)	(0.027)	(0.017)	(0.025)
Observations	4395	4395	4265	4265	4262	4262
	Panel C: Without cutoff specific interactions					
Reduced form	−0.060**	−0.056*	0.009	0.018	0.007	0.007
	(0.026)	(0.029)	(0.021)	(0.024)	(0.016)	(0.022)
Observations	4395	4395	4265	4265	4262	4262
	Panel D: With additional covariates					
Reduced form	−0.103***	−0.099**	0.029	0.044	0.004	0.008
	(0.035)	(0.041)	(0.025)	(0.028)	(0.018)	(0.026)
Observations	3955	3955	3939	3939	3922	3922
	Panel E: Optimal bandwidth					
Reduced form	−0.077***	−0.038	0.022	0.027	0.004	0.015
	(0.029)	(0.031)	(0.025)	(0.026)	(0.017)	(0.024)
Observations	4997	5826	4178	4744	4252	4576
	Panel F: Larger sample					
Reduced form	−0.088***	−0.061*	0.020	0.025	0.003	0.008
	(0.030)	(0.034)	(0.022)	(0.025)	(0.017)	(0.024)
Observations	8128	8128	7286	7286	5814	5814

Notes: Table reports the effects of crossing the admission cutoff on committing crime within five and ten years after admission at the margin of admission a) to any secondary school, b) to general vs vocational secondary school, and c) to more vs less selective general secondary school. Panel A reports our baseline results from Table 3. All the other specifications from Panel B to Panel F alter one dimension of the baseline specification. Panel B drops the DA propensity scores. Panel C drops the cut-off-specific interaction terms. Panel D adds individual and parental characteristics listed in Table 1 as additional controls. Panel E uses optimal bandwidths estimated according to the CCT2014 bandwidth selection rule. Panel F uses a larger estimation sample that requires that there are only at least two applicants on both sides of the cut-off.

Table A3
RDD estimates on completed degree.

	Completed degrees by year 3			Completed degrees by year 10		
	Any secondary degree	General secondary degree	Vocational secondary degree	Any secondary degree	General secondary degree	Vocational secondary degree
	Panel A: Extensive margin					
Reduced form	0.149***	0.094***	0.087***	0.080**	0.024	0.089**
	(0.036)	(0.031)	(0.028)	(0.039)	(0.038)	(0.041)
Mean below[a]	0.132	0.068	0.094	0.669	0.347	0.391
Observations	4395	4395	4395	4395	4395	4395
	Panel B: General margin					
Reduced form	−0.061	0.249***	−0.272***	0.027	0.288***	−0.243***
	(0.039)	(0.031)	(0.037)	(0.024)	(0.033)	(0.036)
Mean below[a]	0.497	0.092	0.472	0.838	0.221	0.714
Observations	4265	4265	4265	4265	4265	4265
	Panel C: Selective margin					
Reduced form	−0.039	−0.029	−0.009	−0.011	−0.015	0.011
	(0.033)	(0.036)	(0.009)	(0.021)	(0.026)	(0.025)
Mean below[a]	0.479	0.579	0.021	0.907	0.815	0.204
Observations	4262	4262	4262	4262	4262	4262

Notes: Table reports the effects of crossing the admission cutoff on completed degrees three and ten years after admission at the margin of admission a) to any secondary school, b) to general vs vocational secondary school, and c) to more vs less selective general secondary school. The results are from our baseline specification which includes cutoff fixed effects and their interaction with the running variable on both sides of the cutoff, as well as DA propensity scores. The local linear estimations employ an edge kernel and a fixed bandwidth that varies across the three margins: we use a fixed bandwidth of 0.4 standardized unit on each side of the cutoff at the margin of admission to any secondary school, 0.5 standardized unit on each side of the cutoff at the margin of admission to a general secondary school, and 0.7 standardized unit on each side of the cutoff at the margin of admission to a selective general secondary school. Standard errors (in parentheses) are clustered by cutoff.
[a] Mean below admission cut-off within the fixed bandwidth.

Table A4
RDD estimates on committing crimes within 5 years since admission by crime types.

	By crime type				By punishment type				By number of crimes		
	Violence	Property	Traffic	Other	Prison	Probation	Fine	Other	At least 1	At least 2	At least 3
	Panel A: Extensive margin										
Reduced form	−0.008	−0.035*	−0.067**	−0.052**	−0.006	−0.033	−0.066**	−0.015*	−0.098***	−0.016	−0.032*
	(0.022)	(0.020)	(0.030)	(0.021)	(0.008)	(0.025)	(0.032)	(0.008)	(0.034)	(0.020)	(0.018)
Mean below[a]	0.082	0.113	0.153	0.137	0.021	0.117	0.229	0.007	0.275	0.143	0.100
Observations	4395	4395	4395	4395	4395	4395	4395	4395	4395	4395	4395
	Panel B: General margin										
Reduced form	0.008	−0.004	−0.007	0.021	0.003	0.010	0.011	0.000	0.019	0.002	−0.003
	(0.011)	(0.010)	(0.021)	(0.013)	(0.002)	(0.012)	(0.023)	(0.003)	(0.024)	(0.013)	(0.009)
Mean below[a]	0.026	0.025	0.066	0.032	0.002	0.034	0.085	0.003	0.109	0.032	0.015
Observations	4265	4265	4265	4265	4265	4265	4265	4265	4265	4265	4265
	Panel C: Selective margin										
Reduced form	0.003	0.000	−0.013	0.005	0.001	−0.010	0.006	0.000	0.004	−0.010	−0.009
	(0.010)	(0.010)	(0.015)	(0.013)	(0.003)	(0.010)	(0.016)	(0.004)	(0.017)	(0.011)	(0.008)
Mean below[a]	0.021	0.026	0.064	0.050	0.002	0.030	0.096	0.004	0.117	0.035	0.018
Observations	4262	4262	4262	4262	4262	4262	4262	4262	4262	4262	4262

Notes: Table reports the effects of crossing the admission cutoff on committing crimes within 5 years since admission. Columns 1–4 report the estimates for different crime categories. Whether individual has committed at least one violent crime, at least one property crime etc. Columns 5–8 report the estimates on probability to commit at least one crime that resulted in a given punishment (prison, probation, fine or other). The last three columns report estimates on probability to commit at least 1 crime within the 5 years follow up period, at least 2 crimes, and at least 3 crimes. The results are from our baseline specification which includes cutoff fixed effects and their interaction with the running variable on both sides of the cutoff, as well as DA propensity scores. The local linear estimations employ an edge kernel and a fixed bandwidth that varies across the three margins: we use a fixed bandwidth of 0.4 standardized unit on each side of the cutoff at the margin of admission to any secondary school, 0.5 standardized unit on each side of the cutoff at the margin of admission to a general secondary school, and 0.7 standardized unit on each side of the cutoff at the margin of admission to a selective general secondary school. Standard errors (in parentheses) are clustered by cutoff.
[a] Mean below admission cut-off within the fixed bandwidth.

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