The (un)importance of school assignment

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Abstract

We combine data from the Amsterdam secondary-school match with register data and survey data to estimate the effects of not being assigned to one's first-ranked school on academic outcomes and on a wide range of other outcomes. For identification we use that secondary-school assignment in Amsterdam is based on the deferred acceptance mechanism with ties broken by lottery numbers. Losing the admission lottery for one's first-ranked school affects the characteristics of the assigned school, the home-school distance and the characteristics of teachers and peers. Despite the different school environment, we find no negative effects on academic outcomes, nor on any other outcome, including: time on homework, help with homework, attitudes towards school, awareness of parents, behavior inside school, behavior outside school, school satisfaction, civic engagement, having friends, and students' personality. It seems therefore that the concerns that parents of lottery losers express about their children's school assignment are based on the characteristics of schools, teachers and peers and not on academic or non-academic outcomes. *JEL*-codes: I21, I24, C26.

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1 Introduction

Since 2015, the secondary schools in Amsterdam use the deferred acceptance (DA) mechanism with ties broken by lottery numbers. Each year around 20% of the students lose the lottery and are assigned to another school than the one they rank first. Many parents of lottery losers express concerns about the result of the match, arguing that not being assigned to their first-ranked school would harm the outcomes of their children.[1] This often involves parents whose children are assigned to their second or third-ranked schools. This paper investigates whether the concerns of these parents are justified. Is not being assigned to the first-ranked school detrimental for outcomes that parents and students care about?

A number of studies find that placement in a preferred school has no, or sometimes even a negative, effect on students' academic outcomes (e.g. Cullen et al., 2006; Clark, 2010; Lucas and Mbiti, 2014; Abdulkadiroğlu et al., 2014; Angrist and Rokkanen, 2015; Abdulkadiroğlu et al., 2018; Barrow et al., 2020; Wu et al., 2019; Oosterbeek et al., 2023).[2] Cullen et al. (2006), Beuermann and Jackson (2022) and Beuermann et al. (2023) argue that parents and students may still prefer these schools because they also care about other outcomes. Cullen et al. find that winning the admission lottery in the Chicago Public School system lowers self-reported disciplinary incidents and arrest rates, while Beuermann et al. find that parents of secondary school students in Trinidad and Tobago prefer schools that raise achievement, reduce criminality and teen motherhood and increase formal labor market participation.

Because teenage motherhood, arrests and working in the formal sector are in many settings – including ours – not the main outcomes that most people will consider, we designed a survey with the goal to collect information about a wide range of variables that cover all the outcomes that students and parents are likely to care about when choosing a secondary school.[3] The survey contains questions about homework, attitudes towards school, parents' awareness, behavior inside and outside school, school satisfaction, civic engagement, personality traits and friends. We combine our survey data with data from the Amsterdam secondary-school match and register data from Statistics Netherlands.

When going from primary to secondary school, students in the Netherlands are directed by their primary school teacher to one of four ability tracks; vocational-elementary, vocational-theory,

[1] A place where concerns are expressed is the page Forum on the website of a parent organization advocating freedom of school choice in Amsterdam (Stichting Vrije Schoolkeuze Amsterdam; http://www.stichtingvsa.nl/guestbook.php).

[2] There are also studies that find positive effects of attending a preferred school; e.g. Jackson (2010), Pop-Eleches and Urquiola (2013) and Dobbie and Fryer (2014).

[3] In a short survey among 168 parents that we conducted in 2023, we asked respondents to divide 100 points between different school features that they care about. The average weights attached to different features are: 23 (over 100) to "feels good and has fun", 13 to "feels safe", 11 to "attention for personal development", 8 to "good academic outcomes", 8 to "short home-school distance", 5 to "similar peers", 5 to "clear communication to parents", and 4 to "diverse student population". This confirms that parents care about more than only academic achievement, teenage motherhood and crime arrests.

college, or university. In the student-proposing DA mechanism that the secondary schools in Amsterdam use, students submit a rank-ordered list (ROL) of schools that offer their ability track. Within coarse priority groups, ties are broken by lottery numbers. Conditional on priority status and ROL, being assigned to the first-ranked school is therefore random. We use this to estimate causal effects.

The data from the Amsterdam secondary-school match cover the six cohorts that participated in the Amsterdam secondary-school match between 2015 and 2020. These data contain for each student information about their ROL, their assigned school and some background variables. The data also have information about each school's capacity per track. Because the shortage of places at popular schools is largest in the university track and because of the high response rate (0.82) to the survey among students in this track, we restrict our analysis to the university track. Around 25% of the secondary-school students in Amsterdam are in that track. The register data from Statistics Netherlands contain annual information about students' progress in the educational system their family background and about the two non-academic outcomes that previous studies have focused on, namely teenage motherhood and being suspected of a crime.[4] In addition, the register data contain information about the teachers employed by each school.

Around 25% of the students in the university track are not assigned to the school they ranked first. Compliance with the assigned treatment is high. Only 2% of the students assigned to their first-ranked school do not enroll in it in the first year after the lottery. And only 7.5% of the students not assigned to their first-ranked school manage to enroll in it in the first year after the lottery. Four years later, these percentages are 14 and 9.5, respectively.

Not being assigned to the first-ranked school is a multidimensional treatment. It changes the home-school distance, the characteristics of the school and its teachers, and of the peers. For students in the university track losing the lottery implies an increase of the home-school distance by 213 meters, relative to a control mean of 4.15 kilometers. It also makes it more likely that a school is assigned that also offers one or more of the other (lower) tracks. Related to this, lottery losers are assigned to schools where teachers are on average less experienced, older, more often female, less often from Dutch descent, and in a lower pay scale.[5] Losing the lottery means assignment to a school where peers are on average less often from Dutch descent, more often from families with below median income and have a lower score on the nationwide exit test from primary school. Finally, lottery losers are assigned to a school that enrolls fewer students from their former primary school in the same track than lottery winners.

In spite of placement in a less attractive school, not being assigned to one's first-ranked school has no negative effects on any of the academic outcomes we observe. Lottery losers are equally likely as lottery winners to be in the expected grade and track three, four and five years after the

[4]The register data also include information on adult outcomes such as labor market status, earnings, marital status, fertility and health. In a few years, this information will be available for the cohorts that we study.

[5]Because we do not know in which track(s) teachers teach, we cannot compute these averages at the school×track level.

lottery, they are equally likely to graduate from the university track on time and equally likely to be enrolled in university. There is also no significant difference in GPA on the final exams. The only difference is that lottery losers are three percentage points *more* likely to specialize in the last three years of secondary school in the more challenging science or health fields as opposed to social sciences and humanities. For the non-academic outcomes that we observe in the register data, we find no effects from losing the admission lottery on being suspected of any crime.[6]

In the first year after the lottery, lottery losers are 18 percentage points more likely to report that they would rather have attended another school. This effect shrinks to around 10 percentage points in the second year and is no longer significantly different from zero in the fifth year. The key result of this paper is that we find no negative effects on any of the other outcomes measured through the survey: homework, attitudes towards school, parents' awareness, behavior inside and outside school, school satisfaction, civic engagement, personality traits and friends. We have enough precision to exclude modestly sized negative effects with high probability. There is also no evidence that specific groups of students defined by gender, ability, family income, migration background, or the rank of the assigned school on the ROL, are harmed. Overall, we conclude that parents' concerns about the negative effects on their children's outcomes when they are not assigned to their first-ranked school, are not warranted in the setting of our study.

This paper proceeds as follows. The next section describes the context. Section 3 describes the data. Section 4 provides details of the empirical approach. Section 5 presents results of the effects of losing the admission lottery on characteristics of school and peers. Section 6 presents and discusses the main findings. Section 7 concludes.

2 Context

2.1 Secondary education in the Netherlands

When students in the Netherlands move from primary school to secondary school, they are around 12 years old. At this stage, students are directed by their primary-school teacher to one of four ability tracks; the four-years vocational-elementary and vocational-theory tracks, the five-years college track and the six-years university track. The two vocational tracks prepare for subsequent vocational programs. The college track gives access to professional colleges (also referred to as universities of applied sciences). The university track gives access to university education, where most programs admit all applicants with a diploma from the university track.

Students can freely choose among the schools that offer their track. Schools do not accept students in a track above their level.[7] Some schools offer only one specific track (single-track

[6]The number of cases of girls giving birth while in secondary school is so small that we are not allowed to report it in accordance with the privacy regulations of Statistics Netherlands.

[7]Students are allowed to enroll in a track below their level. In practice this does not happen.

schools) while others offer multiple tracks (comprehensive schools).

Virtually all schools in the Netherlands are publicly funded and there are no tuition fees.[8] Schools with a large share of disadvantaged students receive extra funding from the government. Consequently, schools with a large fraction of disadvantaged students have more (not less) resources than other schools. The Dutch Education Inspectorate assesses the quality of schools and publishes its findings on the Internet. Schools that receive the lowest quality score for three consecutive years are shut down.

In the final years of secondary education, students specialize in one out of four study fields: science, health, social sciences or humanities. Secondary schools have to follow national curriculum guidelines and all students take national exit exams at the end of secondary school. These exit exams count for half of the final grades, the other half is determined by school-specific exams taken in the last two or three years of secondary education.

2.2 Secondary school choice in Amsterdam

Amsterdam is the largest city of the Netherlands. Every year around 7,500 students transfer from a primary school to one of the around 65 secondary schools. Since 2015 secondary schools in Amsterdam use the student-proposing DA mechanism to assign students to schools (Gale and Shapley, 1962).[9] Under this system students submit a rank-ordered list (ROL) of their preferences for schools. The length of this ROL can be as long as the number of available schools. There are no default schools for students who submit a short ROL and are not assigned to a school on their ROL.[10] Because DA with unrestricted ROL length is strategy proof, it is optimal for students to submit a ROL according to their true preferences. This property is emphasized in the communication to parents and students.[11]

There are only a few priority rules. These are based on having older siblings in the school, having a parent employed by the school, or a specific pedagogical relationship between the primary school a student attended and the secondary school on the student's ROL (Montessori, Dalton or Waldorf). The number of students with priority is quite small because most schools have been phasing out priority for siblings or children of personnel. Priority on the basis of home-school

[8]Public schools can request a voluntary parental contribution. The amounts suggested for these contributions are around 300 euros per year. This makes it unlikely that choices are restricted by financial motives.

[9]Before 2015 (starting in 2005) the secondary schools in Amsterdam used a version of the adaptive Boston mechanism. See De Haan et al. (2023) for an analysis of the different allocations under the Boston and DA mechanisms in Amsterdam. Before 2005, school assignment was decentralized.

[10]These students are assigned to another school in Amsterdam with remaining capacity. If the student rejects this assignment, she can apply to one of the other schools with remaining capacity.

[11]Some recent studies point to the fact that students sometimes make dominated choices under strategy-proof mechanisms (e.g. Rees-Jones, 2018; Fack et al., 2019; Shorrer and Sóvágó, 2023). Fack et al. (2019) report that conditional on both preferences and priorities being private information, truth telling is the unique equilibrium if there are no application costs and uncertainty about admission outcomes is large. These requirements are satisfied in our setting, where application costs are basically zero and admission for most applicants at oversubscribed schools is based on lottery outcomes instead of test scores or home-school distances.

distances or test scores is not allowed. Ties between students with the same (no) priority status are broken by lottery numbers. Around 80% of the applicants are assigned to the school of their first choice. This share varies between tracks, and is higher in the vocational tracks (around 90%) than in the college and university tracks (around 75%).[12]

3 Data

3.1 Data sources

The data come from three sources: the register of the secondary-school match from the city of Amsterdam, the register data from Statistics Netherlands, and the survey which we conducted among secondary-school students in Amsterdam.

The register of the match provides data of all students who participated in the secondary-school match in Amsterdam in the six years between 2015 and 2020. For each student, it has information about assigned ability track, the ROL, priority status, actual placement, age, sex, the score on the nationwide exit test from primary school and home address. Home addresses and the locations of schools result in distances between each student's home address and each of the schools. The register also contains information about the number of available places at each school per track by year. With the data from the match, we compute for each student the propensities to be assigned to the first-ranked school, to the second-ranked school, to the third-ranked school and so on. We also use the data from the match to compute for each student how many of the other first-year students in each secondary school come from the same primary school. In addition we compute the mean score on the nationwide test in primary school of the other first-year students in her track in each secondary school.

Using a personal identifier, we can link 99% of the students in the match register to the register data of Statistics Netherlands. The register data from Statistics Netherlands have annual information about students' position (school, track, grade) in the educational system on October 1st. This is four to six weeks after the start of the school year. We use this information to construct an indicator for enrollment in the first-ranked school. We also construct variables indicating academic performance: being in the expected grade and track after three, four and five years after the lottery, opting for the science or health specialization, graduation from the university track on time, grade point average on central exams, and being enrolled in university. The register data also contain information on two commonly used non-academic outcomes: being suspected of criminal offenses and teenage motherhood. Criminal offenses include violent crime, property crime, vandalism, drugs, and the category "other". Suspicion of criminal offenses is measured for the period between a student starting secondary school and 2021. While the register data also

[12]Placement in top choices was lower in 2015 when multiple tie breaking was used compared to after the switch to single tie breaking in 2016.

contain information on teenage motherhood, we can not report results in this study. The reason is that in our sample the number of girls given birth while in secondary school is so small that we are not allowed to report it in accordance with the privacy regulations of Statistics Netherlands. These regulations forbid reporting instances for groups of less than ten observations.

The data from Statistics Netherlands contain information about students' social background. Based on parents' country of origin, we construct an indicator for students with a Dutch (non-migrant) background. Based on parents' income, we define low-income students as students from families with household income below the national median (in the year prior to participation in the secondary-school match). In addition, the register data contain information about the teachers that are employed in each school. This includes their experience as a teacher and their experience in the school, their level, as well as their sex, age and migration background. The level of a teacher is measured on a scale from A to E. Level A is for primary school teachers, level B for (starting) secondary-school teachers that can be upgraded to level C if they are an example within the school and to level D if they are also an example for teachers working in other schools. Level E is mostly reserved for school management.[13] Teachers' salaries are tied to these levels. We translated these levels into a scale from running from 1 to 5.

In 2019 the secondary schools in Amsterdam gave us permission to survey their students and to merge the survey data to the data from the match and from Statistics Netherlands. To merge the datasets, we asked students for their postal code and their date of birth. The aim of the survey was to inquire whether not being assigned to the first-ranked school has detrimental effects on a range of outcomes.

To be able to compare lottery losers to lottery winners who submitted similar ROL's, it is vital to have high response rates in schools that reject many students and in schools to which many students are assigned who are rejected elsewhere. Out of the 64 secondary schools in Amsterdam, we selected 45 to be included in the survey. Instead of sending students a link to an online version of the survey, we went in-person to each class in each visited school and asked students to fill out the survey. We contacted schools beforehand to make an appointment for our visit. Once a date was set, we asked the school to make a schedule indicating which class could be attended at which hour and in which room. We also asked schools to inform parents and to point them to the possibility to deny permission to use data of their children.[14]

We collected data in 29 of the 45 selected schools before March 2020. In March 2020 secondary schools in Amsterdam were closed as a consequence of the Covid-19 pandemic. After the reopening of schools in September 2020, we managed to administer the survey in six more schools, so that we visited in total 35 out of the initially selected 45 schools. The coverage of visited schools is higher among schools that offer the university track. Thirty-five schools in Amsterdam offer this

[13]Source: https://www.poraad.nl/arbeidszaken-bedrijfsvoering/lerarenfunctie-wat-is-het-grootste-verschil-tussen-een-leraar-lb-lc-en

[14]Ethical approval was granted by the legal department of the municipality of Amsterdam. The parents of seventy-seven children used the opt-out option.

track, 31 of these were selected for a visit and 26 of these were actually visited. Because some students who started secondary school in the university track have switched to another track, it is important to also have data from students in other tracks. Below we discuss the representativeness of the survey respondents.

The questions included in the survey were discussed with and approved by representatives of the boards of the secondary schools. The number of questions included in the survey was restricted by the requirement that it should not take students more than 20 minutes to answer all questions. We piloted the survey in two schools that were not selected for a visit. This led to fine-tuning of some of the questions. We made clear to the respondents that the survey was not anonymous but that responses would be treated confidential. We told respondents explicitly that we needed their date of birth and postal code to merge the survey data to the data from the registers.

The survey contains 18 questions. A translation of the survey from Dutch is included in Appendix A. The first five questions ask identifying information: gender, date of birth, postal code (or street-name), school, class and ability track. We use the answers to these questions to merge the survey data to the register data from the match, and subsequently to the data from Statistics Netherlands. Twelve percent of the surveys do not have (complete) information about the (six digit) postal code or date of birth. Two percent of the surveys can be qualified as "complete nonsense". Using an iterative procedure we could merge 12,878 of the 14,768 surveys (87%) that were filled out.

We construct the following outcome variables to summarize respondents' answers to the other survey questions:

- Hours homework per week: This variable is based on the response to question 6 in the survey;
- Help with homework: This variable is an unweighted average of the responses to the six items in question 7, which run from 1 to 5. The items refer to help from teachers, the school, an external company, parents or others;
- Attitudes towards school: This variable is an unweighted average of the responses to the six statements in question 10, which run from 1 to 5. This includes statements like "I find it important to get good grades", "I enjoy going to school" and "Education is important for a good life later on";
- Parental awareness: This variable is based on the response to question 9 which asks how well informed parents are about the student's school experience on a scale from 1 to 10;
- Behavior inside school: This variable is an unweighted average of the responses to the three items in question 15, which run from 1 to 5. The items refer to skipping class, coming late for class and expulsion;

- Behavior outside school: This variable is an unweighted average of the responses to the four items in question 17, which run from 1 to 5. The items refer to consumption of alcohol, cigarettes and drugs, and having been in contact with the police;

- School satisfaction: This variable is an unweighted average of the responses to the six items in question 13, which run from 1 to 10. The items refer to satisfaction with their life in general, their school, their class, their mentor, the quality of the lessons and extracurricular activities;

- Civic engagement: This variable is an unweighted average of the responses to the five statements in question 12, which run from 1 to 5. The statements refer to i) listening to others even if opinions differ; ii) the importance of making an effort to improve the world; iii) redistribution; iv) interaction with people from other cultures or religions; and v) to strike for climate awareness;

- Personality traits: This variable is an unweighted average of the responses to the eight statements in question 18, which run from 1 to 5. The statements refer to self-esteem, perseverance, optimism, risk tolerance, self-confidence, competitiveness, procrastination and performance under time pressure;

- Friends: In question 16 we ask respondents to list the first names of up to six friends and indicate whether these are enrolled in the same school. We summarize the responses in three variables: number of friends, share of friends from the same school, and share of friends with a non-western name.

In all cases the variables are defined such that higher values represent better outcomes. Instead of unweighted averages of the items and statements we have also constructed outcome variables following the procedure proposed by Anderson (2008). This procedure weights the normalized variables underlying each index by the inverse of the covariance matrix of these variables. Outcomes that are highly correlated therefore receive lower weights than outcomes that provide independent information. Results are reported in the robustness section.

3.2 Descriptive statistics

Table 1 reports the total number of students in the university track by lottery year and subsequently the number of students with lottery risk, the number of students with lottery risk enrolled in a school that we visited and whose first-ranked school we visited (that is the potential number of surveys) and the number of merged surveys. Each year, around 2000 students in the university track participate in the Amsterdam secondary-school match. The total number of participants for the five cohorts that are covered by the survey (2015-2019) equals 10,225. Over 80% of these students are at risk of not being assigned to their first-ranked school (column (2)). This means

Table 1. Number of students in university track, by year, lottery risk and survey response

Sample:	Students	Students with lottery risk	Potential surveys	Merged surveys
Year:	(1)	(2)	(3)	(4)
2015	2,026	1,620	1,337	963
2016	1,968	1,554	1,336	1,072
2017	2,117	1,746	1,490	1,220
2018	2,019	1,618	1,434	1,276
2019	2,095	1,747	1,519	1,273
2020	1,945	1,672		
Total	10,225	8,285	7,116	5,804

Note: The first column reports the number of students in the university track participating in the Amsterdam secondary-school match by year. The second column reports the number of students from column (1) whose probability to be placed in their first-ranked school is strictly between zero and one. The third column reports how many of the students in column (2) are in a school that was visited for the survey and whose first-ranked school was visited. The fourth column reports how many of the students in column (3) responded to the survey and could be merged to the register data. Total refers to the totals for 2015 to 2019, it does not include 2020, which is in light-gray.

that their first-ranked school is not a safe choice. Of the 8,285 at-risk students, 7,116 are enrolled in a school that we visited to collect survey data. 5,804 of these students filled out a survey that we were able to merge to the register data from the school match. The rate of valid responses therefore equals 0.71 for at-risk students and 0.82 for at-risk students in visited schools. In light-gray, the table reports the number of (at-risk) students that participated in the match of 2020. This year is included in analyses based on register data only, but not in analyses that use the survey data.

The top panel of Table 2 reports descriptive statistics of characteristics of students in the university track.[15] It does so separately for all students, all students that are at risk, at-risk students for whom we visited both the current school and the first-ranked school, and the subset of students from this sample for whom we could merge the survey data to the register data. The first two columns also include students from the 2020 cohort, the last two columns do not. Average age in the year of application is just below 12 years, 50% of the students are female and around 60% are of Dutch descent. Students in the university track come from more affluent families with less than 30% from families with household income below the median. These students also score on average more than one standard deviation above the mean on the exit test from primary school.

The next panel shows that in the full sample, around 76% of students are assigned to their first-ranked school. This percentage is 71% for at-risk students. It remains almost the same when we restrict the sample to at-risk students in visited schools, and to at-risk students in visited schools with a merged survey. This indicates that survey response is very similar for lottery winners and losers. Panel C shows that only a minority of the students in the university track is assigned to a school that also offers lower tracks. For the full sample this share is 26% and this drops to

[15]Table B1 in the appendix lists all the schools that offer the university track, their capacities and the numbers of students on waiting lists.

18% and 14% in the samples of at-risk students and students in the survey sample. The average size of the assigned school is around 1,500 and students live around 4 kilometers away from the school to which they are assigned. Panel D shows that students are assigned to schools where teachers are close to 50 years old, have (since 2006) on average 8.3 years experience as a teacher and almost seven years as teacher in the school. Slightly more than half of the teachers is female and around 70% is of Dutch descent. The teachers are on average at level of pay scale C. Finally, panel E shows that students are assigned to schools where on average close to three of their former classmates from primary school with the same track advice are also assigned to. The share of low-income peers in the school is on average around 40%, close to 60% of the peers in the school are of Dutch descent, and the mean of the primary school test score is on average more than 0.7 standard deviation above the mean. When restricted to peers in the university track, the share of low-income peers is lower, the share of peers from Dutch descent is slightly higher and the average of the mean test score is higher. A common feature in all five panels is that the differences between samples (columns) are quite small.

4 Empirical approach

Our main specification is a model where we contrast not being assigned to the first-ranked school with being assigned to the first-ranked school. We estimate the following regression model for the outcome Y of student i for whom school s was the first-ranked school:

$$Y_{is} = \delta D_i + \alpha_s + U_{is} \tag{1}$$

where α_s are fixed effects for combinations of first-ranked schools, priority status and year of application, D_i equals one if student i was not assigned to her first-ranked school (i.e. lost the lottery) and zero otherwise, and U_{is} the error term. Due to the randomness of the lottery draws, which we will support below, the conditional mean independence assumption $E(U_{is}|D_i, \alpha_s) = E(U_{is}|\alpha_s)$ is satisfied. This implies that δ measures the causal effect of not being assigned to the first-ranked school. We restrict the analyses to at-risk students. This excludes students whose first-ranked school does not reject any student from the students' priority group, and students whose first-ranked school is not assigned to any student in their priority group (which is very rare).

In a subsequent analysis, we split "not being assigned to the first-ranked school" into being assigned to the second-ranked school, being assigned to the third-ranked school and being assigned to a school outside the top-3. To compare equals to equals, we condition on priority status and preferences that determine the probability to be assigned to schools at these different ranks. Because there are close to 40,000 possibilities to choose a top-3 of schools from the 35 schools that offer the university track, conditioning on fixed effects for these possibilities is unpractical. Instead

Table 2. Descriptive statistics

	All	At-risk	At risk + visited	At risk + survey
	(1)	(2)	(3)	(4)
A: Demographics				
Age in year of application (in years)	11.79	11.78	11.78	11.78
Female (0/1)	0.50	0.49	0.49	0.49
Dutch (0/1)	0.59	0.61	0.62	0.62
Single parent household (0/1)	0.20	0.20	0.19	0.19
Income below 50th percentile	0.28	0.26	0.25	0.25
Standardized test score	1.01	1.03	1.04	1.05
Test score missing	0.19	0.19	0.03	0.03
B: Treatment				
Assigned to 1st-ranked school	0.76	0.71	0.72	0.73
Adm. prob. to 1st-rank school	0.76	0.71	0.69	0.69
C: Characteristics assigned school				
Lower track available	0.26	0.18	0.16	0.14
School: total nr students	1,740	1,643	1,513	1,466
Home-school distance	4.20	4.21	4.18	4.17
D: Teacher characteristics				
Total experience since 2006	8.27	8.37	8.23	8.30
Experience at school since 2006	6.92	6.97	6.88	6.93
Year of birth	1972.4	1972.3	1972.8	1972.9
Share female	0.53	0.53	0.53	0.53
Share Dutch	0.69	0.70	0.70	0.71
Level (scale 1-5, 5 best)	3.11	3.15	3.18	3.19
E: Peers at assigned school				
No of former classmates (same track)	3.04	2.98	3.06	3.07
Low-income	0.43	0.39	0.39	0.38
Dutch	0.56	0.59	0.60	0.60
Test score	0.71	0.80	0.89	0.91
Same track: Low-income	0.40	0.37	0.37	0.37
Same track: Dutch	0.59	0.61	0.61	0.62
Same track: Test score	0.97	0.98	1.02	1.02
N	12,230	9,957	7,116	5,804

Note: Admission probabilities are simulated propensity scores (see Section 4). The variables that are student and school specific reflect the characteristics of the assigned school and are based on characteristics of students in grade 1 in the year that the student applies. For 2020 the primary school is missing, so the information about peers is based on cohorts 2015-2019. Columns (1) and (2) include the year 2020, while columns (3) and (4) exclude the year 2020. In 2020 no test score was administered because of the Covid-19 pandemic.

we condition on fixed effects for first-ranked schools and on the probabilities to be assigned to the second and third-ranked schools. Following Abdulkadiroğlu et al. (2017), we compute these probabilities (propensity scores) by running the DA mechanism for 1000 different lottery draws per year and taking the average second-ranked and third-ranked assignment rates over these draws. These propensity scores condense the information from the ROL's. In accordance with the actual mechanisms pertaining in these years, the propensity scores for 2015 are based on DA with multiple tie breaking and in 2016 to 2020 on DA with single tie breaking. The regression model is now:

$$Y_{is} = \beta_2 R_i^2 + \beta_3 R_i^3 + \beta_{>3} R_i^{>3} + f(p_i^2, p_i^3) + \alpha_s + U_{is} \tag{2}$$

where R_i^k equals 1 if student i was assigned to her k-th ranked school and zero otherwise, and p_i^k is the propensity score to be assigned to the k-th ranked school.[16] β_k is the causal effect of being assigned to the k-th ranked school instead of the first-ranked school on outcome Y.

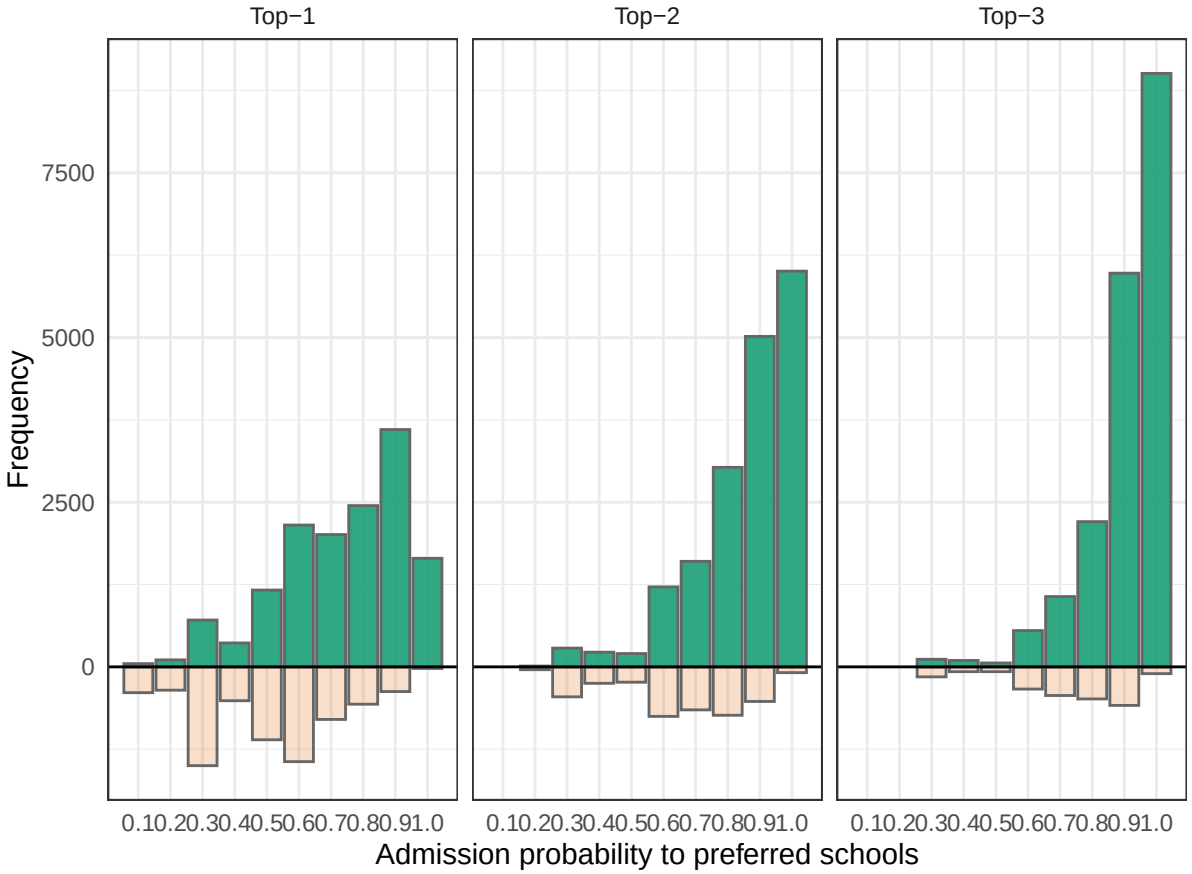
To illustrate the propensity scores, the left-hand graph of Figure 1 plots the distributions of the propensity to be assigned to the first-ranked school for all at-risk students. The bars above the horizontal axis represent students who were assigned to their first-ranked school. The bars below the horizontal axis represent students who were not assigned to their first-ranked school. The middle graph plots the distributions of the probability to be assigned to the first or second-ranked schools, while the graph on the right-hand side plots the distributions of the probability to be assigned to the first, second or third-ranked schools.

As an alternative for equation (1) we will in a robustness check report results where we replace fixed effects α_s by 50 dummies for different two-percentiles of the propensity score to be assigned to the first-ranked school. We will furthermore present results from regressions where instead of fixed effects for the first-ranked school we include fixed effects for the assigned school. This tells us whether students who are enrolled in the same school have different outcomes depending on the rank of that school on their ROL. In the main specifications we do not cluster standard errors, but in Subsection 6.5 we discuss results based on clustering at the level of the assigned school. That subsection also discusses results from specifications where we apply inverse probability weighting.

Table 3 reports balancing results. The left-hand panel shows the means of characteristics of students in the at-risk sample and of their first-ranked school, separately for students were assigned to their first-ranked school and for students who were not assigned to their first-school. Column (3) reports the differences without correcting for risk category fixed effects (α_s), and column (4) reports these differences taking account of the risk category fixed effects. While some of the differences in the third column are sizable and significantly different from zero, this is not the case in the final column. This supports our identifying assumption that conditional on risk category fixed effects, assignment to a first-ranked school is random. The right-hand panel repeats the same comparison

[16] In our analyses we specify $f(p_i^2, p_i^3)$ as 20 dummies for the ventiles of p_i^2 interacted with 20 dummies for the ventiles of p_i^3.

Figure 1. Probabilities to be assigned to top-n schools, for students who were (above x-axis) and were not (below x-axis) assigned



Note: Each graph plots the distribution probabilities to be assigned to the top-n schools, separately for students who were (above the x-axis) and were not (below the x-axis) assigned to their top-n schools. The left, middle and right-hand graphs correspond to $n = 1$, $n = 2$ and $n = 3$, respectively. Students with assignment probabilities equal to 0 or 1 are excluded.

for the sample of survey respondents. This shows that also the lottery winners and losers (and their schools) in this sample are balanced once we condition on risk category fixed effects (α_s). It furthermore shows that the characteristics of lottery winners are very similar in the full sample and in the survey sample. The same holds for the characteristics of lottery losers in the two samples.[17]

5 Treatment characteristics

Table 4 shows that not being assigned to the first-ranked school implies a significant change in many variables that describe the assigned school, their teachers and the peers. Column (1) presents results based on equation (1) and the other columns present results based on equation (2).

While only 12% of the lottery winners are assigned to a school that offers other (lower) tracks (column 5), this share is 26 percentage points higher for lottery losers. Lottery losers are assigned to a school that is on average 213 meters further from their home address and that has on average almost 600 students more than lottery winners.

There are differences in the characteristics of teachers in the schools to which lottery winners and losers are assigned (panel B). Compared to lottery winners, lottery losers are assigned to schools where teachers have on average less experience as a teacher (since 2006) but longer tenure in the assigned school. The teachers of the schools to which lottery losers are assigned are older, more often female and less often from Dutch descent than the teachers of the schools to which lottery winners are assigned. Also the pay scale of these teachers is significantly lower. The teacher characteristics are measured at the school level and not at the school×track level. The differences in teacher characteristics can therefore be (in part) due to the fact that lottery losers are more often assigned to schools that offer other tracks next to the university track.

Panel C shows that there are significant differences in the peer characteristics between the schools to which lottery winners and losers are assigned. Losing the lottery implies being assigned to a school with fewer students who come from the same primary school, with more peers from low-income families, with fewer peers from Dutch descent, and with peers who score on average lower on the exit test from primary school. These patterns hold at the school level and at the school×track level.

Most effects on characteristics of school, teachers and peers increase monotonically in size with the rank of the assigned school. That is: absolute effect sizes are in most cases larger for students assigned to a school outside their top-3 than for students assigned to their third-ranked school, which in turn are larger than for students assigned to their second-ranked school.[18]

[17]Table B2 in the appendix shows balance based on equation (2). The final three columns report differences relative to students assigned to their first-ranked school conditional on fixed effects for the first-ranked school, together with an indication of their p-value. A few differences are statistically significant, but these differences are very small in magnitude. Subsection 6.5 discusses further robustness tests of the balancing results.

[18]Subsection 6.5 discusses that effects on treatment characteristics are very similar for the survey sample.

Table 3. Balance

	Admin sample				Survey sample			
	Not top-1	Top-1	Difference		Not top-1	Top-1	Difference	
	mean (SD)	mean (SD)	without α_s	with α_s	mean (SD)	mean (SD)	without α_s	with α_s
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Student characteristics								
Age (in years)	11.79 (0.39)	11.78 (0.40)	0.014	0.009	11.79 (0.40)	11.78 (0.40)	0.016	0.008
Female (0/1)	0.49 (0.50)	0.49 (0.50)	-0.006	-0.001	0.48 (0.50)	0.50 (0.50)	-0.022	-0.013
Dutch (0/1)	0.63 (0.48)	0.60 (0.49)	0.025**	0.004	0.62 (0.49)	0.62 (0.49)	0.001	-0.016
Single parent HH (0/1)	0.19 (0.40)	0.20 (0.40)	-0.005	-0.011	0.18 (0.39)	0.19 (0.39)	-0.002	-0.008
Income below 50th percentile	0.26 (0.44)	0.26 (0.44)	-0.000	0.005	0.25 (0.43)	0.24 (0.43)	0.003	0.007
Standardized test score	1.02 (0.41)	1.03 (0.43)	-0.013	-0.012	1.04 (0.40)	1.05 (0.42)	-0.015	-0.010
Test score missing	0.16 (0.36)	0.21 (0.40)	-0.051***	0.000	0.03 (0.17)	0.03 (0.16)	0.003	-0.000
Characteristics of first-ranked school								
Assigned to 1st-rank school	0.00	1.00	X	X	0.00	1.00	X	X
Probability assigned to 1st-rank school	0.62 (0.14)	0.75 (0.15)	-0.123***	0.000	0.61 (0.13)	0.73 (0.15)	-0.115***	0.000
Nr students same 1st school/prim. school	5.19 (4.02)	4.75 (3.98)	0.442***	-0.002	5.38 (4.05)	4.68 (3.78)	0.702***	0.105
Nr students same track/1st school/prim. school	4.22 (3.52)	3.60 (3.17)	0.619***	-0.003	4.52 (3.58)	3.79 (3.28)	0.730***	0.092
Home-school distance 1st-rank school (in km)	4.25 (3.29)	4.15 (3.22)	0.101	0.059	4.16 (3.14)	4.10 (3.23)	0.058	0.030
N	2,872	7,085			1,558	4,246		

Note: Columns (1) and (2) present means and standard deviations. Results are based on the at-risk sample. Column (3) reports the difference between the means of columns (1) and (2). Column (4) reports the coefficients from regressions of each characteristic on an indicator for not being assigned to the first-ranked school and fixed effects for the first-ranked school. Columns (5) - (8) follow collumns (1) - (4) but using only the survey sample. Stars indicate whether the (conditional) differences in column (3),(4), (7) and (8) are significantly different from zero. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 4. Effects of not being assigned to first-ranked school on characteristics of school, teachers and peers

Assignment:	outside top-1 (1)	2nd ranked (2)	3rd ranked (3)	outside top-3 (4)	Control mean (5)
A: School characteristics					
Lower track available	0.259***	0.173***	0.225***	0.368***	0.12
	(0.010)	(0.013)	(0.019)	(0.016)	
School: total nr students	584***	359***	508***	902***	1536
	(26.6)	(33.1)	(52.0)	(46.8)	
Home-school distance	0.213***	0.170	0.342**	0.242**	4.15
(assigned school)	(0.077)	(0.106)	(0.168)	(0.113)	
B: Teacher characteristics					
Total experience since 2006	-0.177***	-0.032	0.009	-0.447***	8.46
	(0.022)	(0.028)	(0.044)	(0.038)	
Experience at school since 2006	0.130***	0.156***	0.436***	-0.069	6.99
	(0.028)	(0.042)	(0.060)	(0.047)	
Year of birth	-0.437***	-0.469***	-0.832***	-0.184**	1972
	(0.045)	(0.064)	(0.095)	(0.073)	
Share female	0.016***	0.009***	0.011***	0.028***	0.53
	(0.001)	(0.002)	(0.002)	(0.002)	
Share Dutch	-0.030***	-0.019***	-0.030***	-0.042***	0.70
	(0.002)	(0.002)	(0.003)	(0.003)	
Level (pay scale 1-5, 5 best)	-0.071***	-0.013*	-0.028**	-0.165***	3.17
	(0.006)	(0.008)	(0.012)	(0.010)	
C: Peers					
No of former classmates	-0.699***	-0.414***	-0.678***	-0.992***	3.13
(same track)	(0.059)	(0.088)	(0.125)	(0.081)	
Low-income	0.082***	0.053***	0.068***	0.122***	0.38
	(0.003)	(0.004)	(0.006)	(0.005)	
Dutch	-0.063***	-0.050***	-0.059***	-0.080***	0.60
	(0.003)	(0.004)	(0.005)	(0.005)	
Test score	-0.251***	-0.155***	-0.201***	-0.392***	0.85
	(0.007)	(0.009)	(0.015)	(0.012)	
Same track: Low-income	0.051***	0.034***	0.045***	0.076***	0.36
	(0.003)	(0.004)	(0.006)	(0.005)	
Same track: Dutch	-0.034***	-0.032***	-0.036***	-0.035***	0.61
	(0.003)	(0.004)	(0.005)	(0.006)	
Same track: Test score	-0.036***	-0.019***	-0.029***	-0.061***	0.98
	(0.003)	(0.004)	(0.005)	(0.004)	
N	9,957	9,957			

Note: Each cell in column (1) presents the coefficient of D from a separate regression of equation (1) where the variable indicated by the row entry is the dependent variable. The cells in columns (2)-(4) in each row present the coefficients of R^2, R^3 and $R^{>3}$ from a separate regression of equation (2) where the variable indicated by the row entry is the dependent variable. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

6 Results

This section presents estimates of the effects of not being assigned to the first-ranked school on enrollment (Subsection 6.1), on academic and other outcomes measured in the register data (Subsection 6.2) and on the outcomes measured in the survey (Subsection 6.3). Next it discusses results from heterogeneity analyses inquiring whether specific groups of students are harmed by not being assigned to their first-ranked school (Subsection 6.4) and from robustness analyses (Subsection 6.5).

6.1 Enrollment

The first row of Table 5 shows that not being assigned to the first-ranked school has a large effect on the probability to be enrolled in the first-ranked school in the first year after the lottery. Column (1) is based on equation (1) and columns (2) to (4) on equation (2). The control mean tells that 98% of the lottery winners are in the first year enrolled in the school they ranked first. The effect estimate of -0.905 means that 7.5% of the lottery losers are in the first year enrolled in the school they ranked first. Compliance with the assigned treatment is thus very high but not perfect. Two percent of the winners did not enroll in the school they ranked first. More than half of these never-takers moved out of Amsterdam, making moving a likely explanation for this form of non-compliance.[19]

The outcome of the lottery also has a large effect on the probability to be enrolled in the first-ranked school four years after the lottery. Eighty-six percent of the lottery winners are still enrolled in their first-ranked school. The effect estimate of -0.765 means that 9.5% ($= 0.86 - 0.765$) of the lottery losers are in the fourth year enrolled in their first-ranked school. Twelve percent of the lottery winners is thus changing schools between the first and fourth year. In that period, only two additional percent of the lottery losers manage to obtain a place in their first-ranked school. The results in columns (2) to (4) indicate that compliance with the treatment is somewhat smaller among students assigned to a school outside their top-3 compared to students assigned to their second- or third-ranked schools.

6.2 Academic outcomes

Table 6 presents results for the academic outcomes and for the crime outcome measured in the register data. Because many students in our sample have not yet completed their (secondary) school career, numbers of observations differ between outcomes. The results in column (1) show that three, four and five years after the lottery, losing the lottery has no effect on being in the

[19]The non-compliance by some of the lottery winners opens up 142 ($= 0.02 \times 7085$) places at oversubscribed schools. With some rippling down, this is probably sufficient to place 215 ($= 0.075 \times 2872$) lottery losers in their first-ranked schools.

Table 5. Effects on enrollment in first-ranked school by year since lottery

Assignment:	outside top-1 (1)	2nd ranked (2)	3rd ranked (3)	outside top-3 (4)	Control mean (5)
year 1	-0.905***	-0.915***	-0.926***	-0.884***	0.98
	(0.005)	(0.007)	(0.009)	(0.010)	
year 4	-0.765***	-0.786***	-0.781***	-0.731***	0.86
	(0.008)	(0.010)	(0.013)	(0.013)	
N year 1	9,957				
N year 4	8,285				

Note: The cells in column (1) present the coefficient of D from a separate regression of equation (1) where the variable indicated by the row entry is the dependent variable. The cells in columns (2)-(4) in each row present the coefficients of R^2, R^3 and $R^{>3}$ from a separate regression of equation (2) where the variable indicated by the row entry is the dependent variable. The cells in column (5) present the mean among the students who were assigned to the first-ranked school. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

third, fourth and fifth year of the university track. Also the share that passed the exam of the university track on time is not affected by losing the lottery. Nor are there effects on the GPA on the central exit exams from secondary schools or on the share that is enrolled in university. In all cases the estimated effects are close to zero and the standard errors are small enough to exclude modestly sized negative effects with 95% probability. The only academic outcome for which we find a statistically significant effect is that losing the lottery increases the probability to specialize in the science field or the health field instead of the fields of social sciences or humanities. The science and health fields are in Dutch education generally seen as more challenging (e.g. Buser et al., 2014). Columns (2) to (4) show that effect estimates are very similar for students being assigned to the school they ranked second, third or lower on their ROL. The final row of Table 6 shows results for being suspected of any crime. The control means indicate that the incidence of suspicion of crime is rather low. The effect estimates show that not assigning students to their first-ranked school does not make it more likely that they will be suspected of crime.

As a comparison, column (5) reports results from what we label a "naive approach". Here we regress outcomes on a dummy for not being assigned to the first-ranked school and fixed effects for assigned schools. This approach compares outcomes of lottery winners and losers with the same assigned school instead of the same first-ranked school, and should therefore not be interpreted causally. The results show that lottery losers are 2 to 3 percentage points more likely to be on track three to five years after the lottery than their peers in the same school who ranked that school first. Differences for later academic outcomes (exam passed and enrolled in university) point in the same direction, but lack statistical significance. These positive differences are consistent with the fact that losing the lottery has no effect on the student's own performance (column (1)) and assigns students to schools with peers who did worse on the exit test from primary school (see Table 4).

Table 6. Academic and other admin outcomes

	outside top-1 (1)	2nd ranked (2)	3rd ranked (3)	outside top-3 (4)	naive approach (5)	Control mean	N
In expected grade/track:							
- Year 3	-0.006 (0.007)	-0.014 (0.011)	-0.007 (0.015)	0.004 (0.011)	0.023 (0.008)***	0.88	9,957
- Year 4	-0.012 (0.010)	-0.010 (0.014)	-0.025 (0.020)	-0.008 (0.015)	0.029 (0.011)**	0.80	8,285
- Year 5	-0.015 (0.012)	0.002 (0.017)	-0.050 (0.025)**	-0.012 (0.018)	0.024 (0.014)*	0.74	6,538
Science or health track	0.027 (0.013)**	0.032 (0.018)*	0.017 (0.025)	0.027 (0.019)	0.028 (0.014)**	0.47	8,285
Exam passed on time/track	-0.011 (0.019)	0.015 (0.026)	-0.057 (0.037)	-0.024 (0.030)	0.027 (0.021)	0.62	3,174
GPA central exam	-0.052 (0.038)	-0.018 (0.051)	-0.096 (0.075)	-0.063 (0.060)	0.005 (0.041)	6.64	2,654
Enrolled in university	0.002 (0.019)	0.038 (0.025)	-0.043 (0.037)	0.001 (0.029)	0.013 (0.021)	0.45	3,174
Crime	0.006 (0.007)	0.010 (0.010)	-0.006 (0.012	0.006 (0.010)	-0.003 (0.009)	0.04	9,957

Note: Each cell in column (1) presents the coefficient of D from a separate regression of equation (1) where the variable indicated by the row entry is the dependent variable. The cells in columns (2)-(4) in each row present the coefficients of R^2, R^3 and $R^{>3}$ from a separate regression of equation (2) where the variable indicated by the row entry is the dependent variable. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. GPA on central exam is conditional on taking the exam, irrespective of the year and the track thereof. There are insufficient cases of teenage motherhood to be allowed to report the results.

6.3 Survey outcomes

We now turn to the effects on outcomes that we constructed on the basis of the survey responses. The first column of Table 7 presents results based on equation (1), columns (2) to (4) are based on equation (2), and column (5) presents results from the naive approach that includes fixed effects for the assigned school. In the first column, only one effect estimate differs significantly from zero; lottery losers spend per week half an hour more on homework. All the other estimates in the first column are close to zero and not significantly different from it. Not being assigned to the first-ranked school has no negative effects on: help with homework, attitudes towards school, parental awareness, behavior inside school, behavior outside school, school satisfaction, civic engagement, personality traits, number of friends, share of friends from school and share of friends with a non-western name.

While some of the estimates in columns (2) to (4) are significantly different from zero, these estimates do not systematically point to negative effects for lottery losers. Being assigned to the second-ranked school instead of the first-ranked school, improves attitudes towards school and personality traits. Being assigned to a school outside the top-3 instead of the first-ranked school worsens behavior outside school and reduces the number of friends from school. The magnitudes of these negative effects are quite small. We therefore conclude that not being assigned to the first-ranked school has no negative effects on any of the outcomes we measure through the survey.

Turning to the results from the naive approach in column (5), we observe only a few differences in the non-academic outcomes between lottery winners and losers who are assigned to the same school. Compared to lottery winners, losers do somewhat better on personality traits but their share of friends from school is 2 percentage points lower.

Table B3 in the appendix shows estimates of the effects on the variables underlying each of the index outcomes. This shows that for most indices also none of the underlying variables is significantly different from zero. This includes help with homework by parents or an external organization, suggesting the absence of a parental response to compensate for assignment to a lower-ranked school (e.g. Pop-Eleches and Urquiola, 2013). The noticeable exception occurs for the variables used to construct the index of civic engagement. Students not assigned to their first-ranked school are more in agreement with the statements regarding 'listening to others' and 'diversity' than students assigned to their first-ranked school, but they are less in agreement with the statement about 'striking for the climate'. A possible explanation for these results is that students who are not assigned to their first-ranked school are surrounded by more diverse peers who are less like them (fewer classmates from their primary school and more students from other tracks than the university track), and that this makes these students more openminded towards other people.

We have also examined whether effects on survey outcomes vary depending on the grade where students are in when they respond to the survey. Table 8 shows results for selected survey outcomes

Table 7. Survey outcomes

	outside top-1 (1)	2nd ranked (2)	3rd ranked (3)	outside top-3 (4)	naive approach (5)	Control mean
Hours homework per week	0.504 (0.184)***	0.623 (0.254)**	0.150 (0.339)	0.597 (0.301)**	0.051 (0.200)	7.44
Help with homework	-0.022 (0.019)	-0.026 (0.025)	-0.002 (0.036)	-0.022 (0.030)	-0.027 (0.020)	2.01
Attitudes towards school	0.011 (0.016)	0.056 (0.022)***	-0.032 (0.030)	-0.026 (0.024)	-0.015 (0.017)	4.01
Parental awareness	-0.003 (0.052)	0.077 (0.071)	-0.066 (0.104)	-0.040 (0.082)	0.032 (0.057)	8.47
Behavior inside school	-0.012 (0.019)	-0.001 (0.027)	-0.053 (0.040)	0.004 (0.031)	-0.014 (0.021)	4.41
Behavior outside school	-0.012 (0.016)	0.020 (0.022)	-0.012 (0.029)	-0.046 (0.026)*	-0.028 (0.017)	3.70
School satisfaction	-0.009 (0.037)	0.065 (0.052)	-0.066 (0.074)	-0.070 (0.058)	-0.065 (0.039)	7.47
Civic engagement	0.014 (0.018)	0.033 (0.026)	-0.022 (0.035)	0.007 (0.028)	-0.018 (0.020)	3.94
Personality traits	0.017 (0.018)	0.040 (0.024)*	-0.027 (0.034)	0.014 (0.028)	0.036 (0.019)*	3.45
Number of friends (max 6)	0.005 (0.045)	-0.007 (0.065)	0.026 (0.085)	0.021 (0.071)	0.069 (0.048)	5.38
Share friends from school	-0.012 (0.008)	0.006 (0.012)	-0.030 (0.016)*	-0.028 (0.013)**	-0.019 (0.009)**	0.68
Share friends non-western name	-0.003 (0.012)	0.007 (0.017)	-0.025 (0.022)	0.005 (0.018)	-0.001 (0.012)	0.24
N	5,804					

Note: Each cell in column (1) presents the coefficient of D from a separate regression of equation (1) where the variable indicated by the row entry is the dependent variable. The cells in columns (2)-(4) in each row present the coefficients of R^2, R^3 and $R^{>3}$ from a separate regression of equation (2) where the variable indicated by the row entry is the dependent variable. There is some item non-response, with the lowest number of observations for the share friends from school (5,575 observations). For most indices the number of missing observations is less than 50. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8. Survey outcomes over time

	Grade 1	Grade 2	Grade 3	Grade 4	Grade 5
Survey indices					
Attitudes towards school	-0.066*	0.072**	0.049	0.014	-0.021
	(0.035)	(0.034)	(0.033)	(0.040)	(0.033)
School satisfaction	-0.237***	0.139*	0.095	-0.022	-0.047
	(0.086)	(0.083)	(0.078)	(0.098)	(0.076)
Civic engagement	-0.006	0.108***	-0.029	0.031	-0.033
	(0.040)	(0.038)	(0.038)	(0.048)	(0.039)
Personality	-0.013	0.043	0.062*	-0.055	0.028
	(0.041)	(0.038)	(0.035)	(0.045)	(0.039)
Survey questions:					
Want to attend other school	-0.181***	-0.103***	-0.090***	-0.127***	0.011
(absolutely not)	(0.032)	(0.032)	(0.033)	(0.039)	(0.032)
Hours homework	-0.157	0.461	0.469	0.947**	0.849**
	(0.393)	(0.388)	(0.418)	(0.468)	(0.394)
Parents well informed	-0.197*	0.058	-0.125	0.179	0.098
	(0.110)	(0.103)	(0.112)	(0.132)	(0.127)
N	1,273	1,276	1,220	1,072	963

Note: Each row presents the coefficient, standard error and control mean from a separate regression of equation (1) where the variable indicated in the first column is the dependent variable. The table only presents results for selected outcome variables where effect estimates show noteworthy variation between grades. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. There is some item non-response, with on average 1.1% (0.9%) of responses missing for the first (fifth) grade.

for which effect estimates show noteworthy variation between grades. Losing the lottery leads only in the first grade to worse attitudes towards school, lower school satisfaction, and less informed parents. These negative effects disappear from the second grade onwards. Losing the lottery has positive effects on civic engagement in the second grade and on personality in the third grade, but not in any of the other grades.

Lottery losers are in the first grade 18 percentage points more likely than winners to affirm that they want to attend another school. This effect hovers around 10 percentage points in the next three grades and equals zero in the fifth grade of secondary school. While these may seem substantial effects, they also imply that the vast majority of the students who were not assigned to their first-ranked school do not want to attend another school. The positive effect of losing the lottery on hours of homework appears to be concentrated towards the end of secondary school (grades 4 and 5). A part of the effect on hours of homework can therefore be explained by lottery losers choosing more often for the more demanding science and health specializations in the final years of secondary school.

6.4 Heterogeneous effects

The results presented so far show that lottery losers are on average not harmed by not being assigned to their first-ranked school. This does, however, not exclude the possibility that specific groups of students are harmed. To inquire this, we consider separate effects for boys and girls, for students from low and high SES families, for students from Dutch and non-Dutch descent, and for students with below and above median scores on the exit test from primary school. Results are in Table B4 in the appendix. The table presents estimates for compliance (after four years), administrative outcomes and survey outcomes. There are only a few instances where effects for one of the groups give a different picture than that for the full sample. Male lottery losers are after four years somewhat less likely to be on track than male lottery winners. There is no such an effect of losing the lottery for women. The effect of losing the lottery on homework hours turns out to be entirely due to female lottery losers and lottery losers who scored below the median on the exit test from primary school. All in all, we conclude that there is no evidence that specific groups of students are harmed by not being assigned to their first-ranked school.

6.5 Robustness

Appendix B includes tables that show that the results we have presented so far are robust across a variety of different sample selections and specification choices. Specifically:

- To check the sensitivity of using fixed effects for first-ranked school $\times$ year $\times$ priority groups, column (4) of Table B5 reports differences that are based on regressions that include 50 bins for the propensity score to be assigned to the first-ranked school. The differences between these results and those in Table 3 in the main text are minimal.

- To check the sensitivity of using weighting observations equally, Tables B6 and B7 weight observations with the inverse of the propensity score to not be assigned to the first-ranked school (cf. Li et al., 2018; Goldsmith-Pinkham et al., 2022). Differences between results in these tables and those in Tables 3 and 6 in the main text are minimal.

- As an additional assessment of differences between the survey sample and the register sample, Tables B8 and B9 repeat the analyses of Tables 4 and 6 in the main text but are based on the survey sample instead of the full sample from the register. Most of the treatment differences and effects on administrative outcomes in the full sample are very similar in the survey sample.

- As another assessment of the accuracy of the survey sample, the results in Table B10 weights observations with the inverse of the propensity score of having responded to the survey. This propensity score is based on a logit regression of an indicator of response on observable characteristics. The results in this table are very similar to those in Table 7 in the main text.

- In the main analyses we have used outcomes that are constructed as unweighted averages of the responses to several survey questions. To assess the sensitivity to using equal weights, we have also constructed indices of the outcomes using the procedure described in Anderson (2008). The results in Table B11 are similar to those in Table 7 in the main text.
- To assess sensitivity to clustering of standard errors, Table B12 reports standard error terms that are clustered at the level of the assigned school. This leaves standard errors unchanged or reduces them slightly compared to Table 6 in the main text. The single significant effect (science and health track) remains significant.
- Table B13 is similar to Table 6 in the main text but now we control for the simulated probability to be assigned to the first-ranked school instead of fixed effects for first-ranked school $\times$ year $\times$ priority groups. Results are almost identical.
- Table B14 is similar to Table 7 in the main text but now observations are restricted to schools that we visited before the school closure due to the Covid crisis. This has almost no impact on the effect estimates.

7 Conclusions

Using a unique combination of data from the Amsterdam secondary-school match, register data and survey data, we find that students who are not assigned to their first-ranked school are not harmed by this. Neither their academic outcomes nor a broad range of non-academic outcomes are worse than they would have been, had they been assigned to their first-ranked school. Our finding for academic outcomes is in line with those of a number of previous studies (e.g. Cullen et al., 2006; Clark, 2010; Lucas and Mbiti, 2014; Abdulkadiroğlu et al., 2014; Angrist and Rokkanen, 2015; Abdulkadiroğlu et al., 2018; Barrow et al., 2020; Wu et al., 2019; Oosterbeek et al., 2023). The finding for non-academic outcomes is our novel contribution. Whereas some previous studies have looked at some specific non-academic outcomes (teenage motherhood and crime), we consider many of such outcomes, including homework, attitudes towards school, parents' awareness, behavior inside and outside school, school satisfaction, civic engagement, personality traits and friends. We have enough precision to exclude modestly sized negative effects. We also exclude negative effects for specific groups defined by gender, social background, migration status and academic ability. Our results cannot be explained by the fact that first-ranked schools and lower ranked schools are just too similar. Not being assigned to the first-ranked school implies meaningful differences in characteristics of schools, teachers and peers.

In the introduction we mentioned that parents have strong feelings about their child being assigned to their first-ranked school. Why are these feelings so strong if there are no measurable differences in outcomes? One explanation is that parents and students care about characteristics of

schools, teachers and peers, and are less interested in outcomes. First-ranked schools are closer to home (albeit just 5% of the average home-school distance) and provide peers from more advantageous backgrounds and with better scores on the exit test from primary school, than lower-ranked schools. In contrast to the findings in Abdulkadiroğlu et al. (2020), in our context the "better" peers do not generate better academic outcomes. If anything, it seems that exposure to better peers – i.e. the exposure to a less diverse peer group – is harmful for dimensions of civic engagement that are related to interaction with people from other groups ("people must listen carefully to each other, even if they disagree" and "I like to hang out with people who are different from me, for example with a different religion or culture"). Policymakers who care about these outcomes, may therefore consider to give less importance to the school preferences of parents and students and more importance to diversity within schools.

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A The survey

Below is a translation from Dutch of the questions included in survey.

You receive this questionnaire because we want to know about your experiences in your school. We kindly request you to fill out the questionnaire.

Your answers will be treated with confidentiality. By filling out the questionnaire, you give permission to merge your answers to administrative information and to use the data for research purposes. Your responses to the questionnaire are personal. Therefore, do not discuss with others while responding.

1. Personal data

   (a) Gender _____

   (b) Date of birth _____

   (c) Postal code _____O don't know (go to question 4)

2. In which school are you enrolled? School: _____

3. What is the name of your class? Class: _____

4. Name of street where you live _____

5. What is your current education level? If you are in a mixed level, you can indicate multiple levels.

   - vocational basic
   - vocational cadre
   - vocational theory
   - college
   - university

6. How many hours per week do you on average spend on homework? _____ hours

7. How often do you get help with homework from:

   1 = never

   2 = almost never

   3 = every now and then

   4 = regularly

5 = often

	1	2	3	4	5
Your mentor	O	O	O	O	O
Other teachers in school	O	O	O	O	O
Homework class in school	O	O	O	O	O
Homework class outside school	O	O	O	O	O
Parents	O	O	O	O	O
Other family members/acquaintances	O	O	O	O	O

8. Is it possible to get extra guidance in school if necessary?

Completely impossible [] [] [] [] [] [] [] [] [] [] Very possible
1 2 3 4 5 6 7 8 9 10

O Don't know

9. Are your parents well informed about how you are doing in school?

Completely uninformed [] [] [] [] [] [] [] [] [] [] Very well informed
1 2 3 4 5 6 7 8 9 10

10. Do you disagree or agree with the following statements?

1 = very much disagree

2 = disagree

3 = neutral

4 = agree

5 = very much agree

	1	2	3	4	5
I find it important to get good grades in school	O	O	O	O	O
I try my best in school	O	O	O	O	O
I like going to school	O	O	O	O	O
I feel safe in school	O	O	O	O	O
I feel that I can be myself in school	O	O	O	O	O
School is important to have a good life in the future	O	O	O	O	O

11. A. What is your current average grade for:

Dutch: O not applicable

Mathematics: O not applicable

B. What is your current grade point average?

GPA: O not applicable

12. Below are some statements. We ask you to indicate whether you disagree or agree with the statements.

    1 = very much disagree

    2 = disagree

    3 = neutral

    4 = agree

    5 = very much agree

	1	2	3	4	5
People must listen carefully to each other, even if they disagree	O	O	O	O	O
I think it's important that children and young people are committed to a better world	O	O	O	O	O
People who earn enough must take care of people who are less fortunate	O	O	O	O	O
I like to hang out with people who are different from me, for example with a different religion or culture	O	O	O	O	O
I think it's good that students go on strike for the climate	O	O	O	O	O

13. On a scale from 1 to 10, how satisfied are you with:

    Your life in general?

Very dissatisfied	[]	[]	[]	[]	[]	[]	[]	[]	[]	[]	Very satisfied
	1	2	3	4	5	6	7	8	9	10	

    Your current school?

Very dissatisfied	[]	[]	[]	[]	[]	[]	[]	[]	[]	[]	Very satisfied
	1	2	3	4	5	6	7	8	9	10	

    Your current class?

Very dissatisfied	[]	[]	[]	[]	[]	[]	[]	[]	[]	[]	Very satisfied
	1	2	3	4	5	6	7	8	9	10	

    Your mentor?

Very dissatisfied	[]	[]	[]	[]	[]	[]	[]	[]	[]	[]	Very satisfied
	1	2	3	4	5	6	7	8	9	10	

    The quality of the lessons?

Very dissatisfied	[]	[]	[]	[]	[]	[]	[]	[]	[]	[]	Very satisfied
	1	2	3	4	5	6	7	8	9	10	

The extracurricular activities the school organizes?

Very dissatisfied	[]	[]	[]	[]	[]	[]	[]	[]	[]	[]	Very satisfied
	1	2	3	4	5	6	7	8	9	10	

14. Would you rather attend another school in Amsterdam?

O yes, very much

O yes. probably

O rather not

O definitely not

15. How often:

1 = every day

2 = one or multiple times per week

3 = one or multiple times per month

4 = less often

5 = never

	1	2	3	4	5
have you skipped a class?	O	O	O	O	O
have you been too late for class?	O	O	O	O	O
have you been expelled from class?	O	O	O	O	O

16. What are the first names of the (maximum) six friends with whom you interact most often?

1. _____ attends the same school yes/no
2. _____ attends the same school yes/no
3. _____ attends the same school yes/no
4. _____ attends the same school yes/no
5. _____ attends the same school yes/no
6. _____ attends the same school yes/no

17. How often

1 = never

2 = almost never

3 = every month

4 = every week

5 = I don't know

	1	2	3	4	5
do you drink alcohol?	O	O	O	O	O
do you smoke cigarettes?	O	O	O	O	O
do you use drugs?	O	O	O	O	O
have you been in contact with the police?	O	O	O	O	O

18. Below are some statements. We ask you to indicate how well each statement applies to you.

    1 = does not apply at all

    2 = does not apply

    3 = neutral

    4 = applies

    5 = applies very well

	1	2	3	4	5
I'm satisfied with myself the way I am	O	O	O	O	O
I'm a go-getter	O	O	O	O	O
I think it will go well with me in the future	O	O	O	O	O
I like taking risks	O	O	O	O	O
I have confidence in myself	O	O	O	O	O
I like competition	O	O	O	O	O
I delay annoying things	O	O	O	O	O
I perform well under time pressure	O	O	O	O	O

B Additional tables and figures

This appendix presents tables with additional information and results.

Table B1 lists the schools that offer the pre-university track, together with their capacities, numbers of applicants, numbers of assigned places, numbers of places assigned to students that ranked the school first, and numbers of students on the waiting list. Data are for 2016.

Table B2 is similar to Table 3 in the main text but compares students who were assigned to their first-ranked school (column 4) to: students who were assigned to a school outside the top-3 (column 1), students who were assigned to their number 3 (column 2) and students who were assigned to their number 2 (column 3). The final three columns reported differences relative to students assigned to their first-ranked school conditional in fixed effects for the first-ranked school, together with an indication of their p-value. A few differences are statistically significant, but these differences are small in magnitude.

Table B3 shows estimates of the effects on the variables underlying each of the index outcomes. This shows that for most indices also none of the underlying variables is significantly different from zero. The noticeable exception occurs for the variables used for the index outcome civic engagement. Students who were not assigned to their first-ranked school are more in agreement with the statements regarding "listen to others" and diversity than students who were assigned to their first-ranked school, but they are less in agreement with the statement about striking for the climate.

Table B4 presents results from specifications that allow for heterogeneity of treatment effects along different dimensions. Columns (1) and (2) consider differences by sex. columns (3) and (4) consider differences by SES, columns (5) and (6) consider differences by parental migration background and columns (7) and (8) consider differences by students' test score on the nationwide exit test from primary school. The overall picture is that very few of the estimates of differential effects are significantly different from zero. Most noticeable are the findings in the first row, which indicate that students from low-SES families, students with a non-Dutch background, and students with a standardized test score below the median who were not assigned to their first ranked school, are a bit more likely to be enrolled in that school in the fourth year than their counterparts. Furthermore, the positive effect of not being assigned to the first-ranked school on hours of homework appears to be entirely due to the response of girls.

Table B5 is similar to Table 3 in the main text but now the differences in column (4) are based on regressions that include 50 bins for the propensity score to be assigned to the first-ranked school instead of fixed effects for first-ranked school $\times$ year $\times$ priority groups. The differences between both tables are minimal.

Tables B6 and B7 are similar to Tables 3 and 6 in the main text but now observations are weighted with the inverse of the propensity score to not be assigned to the first-ranked school (cf. Li et al., 2018; Goldsmith-Pinkham et al., 2022). Differences between both tables are minimal.

Table B8 is the same as Table 4 in the main text but is based on the survey sample instead of the full sample from the register. Most of the treatment differences in the full sample are also present in the survey sample.

Columns (1) and (2) of Table B9 repeats the results from Table 6 in the main text. These results are based on the full (register) sample. The subsequent columns report estimates based on the survey sample. This shows that we find very similar results for the survey sample as for the register sample.

Table B10 is similar to Table 7 in the main text but now observations are weighted with the inverse of the propensity score of having responded to the survey. This propensity score is based on a logit regression of an indicator of response on the interacted observable characteristics female, Dutch descent, low income, switched school by 2019 and the interaction of the first-ranked school and the placement rank (1, 2, 3, 3+). 5 The results in the two tables are very similar.

Table B11 is similar to Table 7 in the main text but now indices of the outcomes are constructed using the procedure described in Anderson (2008). This procedures weights the normalized variables underlying each index by the inverse of the covariance matrix of these variables. The table only includes other outcomes that are constructed from multiple underlying variables. The results in the two tables are very similar.

Table B12 is similar to Table 6 in the main text but now error terms are clustered at the level of the assigned school. This leaves standard errors unchanged or reduces them slightly. The single significant effect (science and health track) remains significant.

Table B13 is similar to Table 6 in the main text but now we control for the simulated probability to be assigned to the first-ranked school instead of fixed effects for first-ranked school $\times$ year $\times$ priority groups. Results are almost identical.

Table B14 is similar to Table 7 in the main text but now observations are restricted to schools that we visited before the school closure due to the Covid crisis. This has almost no impact on the effect estimates.

Table B1. The schools offering the university track – Data for 2016

School:	Capacity	Applicants	Assigned place	Assigned first ranked	On waiting list
	(1)	(2)	(3)	(4)	(5)
Barlaeus Gymnasium	145	174	145	139	131
Berlage Lyceum	84	17	30	17	0
Caland 2	150	9	10	9	0
Calandlyceum	108	67	68	67	0
Cartesius 2	56	17	24	17	0
Cartesius Lyceum	56	23	52	23	0
Comenius Lyceum	60	9	10	9	0
Cygnus Gymnasium	174	201	174	163	116
Damstede Lyceum	56	34	53	34	0
Fons Vitae Lyceum	70	26	70	25	22
Geert Groote College	42	34	42	34	1
Gerrit van der Veen College	60	28	45	28	3
Hervormd Lyceum West	92	4	5	4	0
Hervormd Lyceum Zuid	173	42	37	36	56
Het 4E Gymnasium	140	156	140	124	119
Het Amsterdams Lyceum	172	221	172	169	172
Hyperion Lyceum	145	243	139	137	185
IJburg College	15	8	8	8	0
Joodse SG Maimonides	45	5	6	5	0
Metis Montessori Lyceum	65	22	29	21	0
Montessori Lyceum Amsterdam	84	54	84	53	12
Open Schoolgemeenschap Bijlmer	144	21	24	21	0
Pieter Nieuwland College	84	70	80	66	8
Scholengemeenschap Reigersbos	50	6	7	6	0
Spinoza Lyceum	87	77	85	62	56
Spinoza20first	20	6	9	6	0
St. Ignatiusgymnasium	162	169	162	143	105
St. Nicolaaslyceum	140	140	132	104	131
Vinse School	22	3	1	1	4
Vossius Gymnasium	140	112	140	100	85

Note: The first column reports each school's capacity at the university track. The second column reports for each school the number of students assigned to the university track who first-ranked that school. The third column reports for each school the number of students who were assigned to that school. The fourth column reports per school the number of students who were assigned to that school and who first-ranked that school. The fifth column reports per school how many students are on the waiting list of that school, that is: were placed in a school that is lower on their ROL. For some schools, the reported numbers are summed over different programs (e.g. sports, bilingual, music, dance); this explains why Spinoza Lyceum and St. Nicolaaslyceum enroll fewer students than their (summed) capacities and still have students on their waiting lists.

Table B2. Balance table based on equation 2

	Out-3 mean (SD) (1)	Top-3 mean (SD) (2)	Top-2 mean (SD) (3)	Top-1 mean (SD) (4)	(1) vs (4) (5)	(2) vs (4) with α_s (6)	(3) vs (4) (7)	Joint p-value (8)
Student characteristics								
Age (in years)	11.79 (0.38)	11.80 (0.39)	11.79 (0.40)	11.78 (0.40)	0.017	0.023	-0.000	0.484
Female (0/1)	0.48 (0.50)	0.49 (0.50)	0.49 (0.50)	0.49 (0.50)	-0.013	0.006	0.002	0.730
Dutch (0/1)	0.64 (0.48)	0.65 (0.48)	0.61 (0.49)	0.60 (0.49)	0.006	0.027	-0.011	0.339
Single parent HH (0/1)	0.18 (0.39)	0.20 (0.40)	0.20 (0.40)	0.20 (0.40)	-0.013	-0.006	-0.012	0.951
Income below 50th percentile	0.23 (0.42)	0.27 (0.45)	0.28 (0.45)	0.26 (0.44)	-0.011	0.015	0.015	0.315
Standardized test score	1.05 (0.39)	0.97 (0.43)	1.02 (0.41)	1.03 (0.43)	-0.001	-0.043**	-0.006	0.222
Test score missing	0.20 (0.40)	0.13 (0.34)	0.13 (0.34)	0.21 (0.40)	0.002	0.005	-0.005	0.489
Characteristics of first-ranked school								
Assigned to 1st-rank school	0.00	0.00	0.00	1.00	X	X	X	
Probability assigned to 1st-rank school	0.67 (0.14)	0.61 (0.13)	0.59 (0.12)	0.75 (0.15)	0.000**	-0.000	0.000	0.146
Nr students same 1st school/prim. school	5.14 (4.07)	5.18 (4.05)	5.24 (3.94)	4.75 (3.98)	0.027	-0.109	-0.044	0.850
Nr students same track/1st school/prim. school	4.33 (3.69)	4.04 (3.36)	4.20 (3.41)	3.60 (3.17)	0.014	-0.185	0.004	0.617
Home-school distance 1st-rank school (in km)	4.25 (3.41)	4.34 (3.49)	4.20 (3.08)	4.15 (3.22)	0.206*	0.047	-0.018	0.312
N	1,031	593	1,248	7,085				

Note: Columns (1) to (4) present means and standard deviations. Column (5) to (7) report the coefficients from regressions of each characteristic based on equation 2. Stars indicate whether the conditional differences in columns (5) to (7) are statistically significant. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table B3. Survey outcomes - details

	coeff	s.e.	mean
Homework			
Help with hw mentor	0.004	(0.028)	1.59
Help with hw other teachers	-0.023	(0.035)	2.39
Help with hw organized by school	-0.006	(0.026)	1.26
Help with hw from external	-0.016	(0.039)	1.66
Help with hw from parents	-0.041	(0.035)	3.07
Help with hw from others	-0.035	(0.034)	2.02
Attitudes toward school			
Importance of good grades	0.029	(0.023)	4.22
I do my best in school	0.042	(0.026)	3.91
Enjoy going to school	0.014	(0.031)	3.35
Feel safe in school	-0.020	(0.024)	4.22
Can be myself in school	-0.008	(0.026)	4.11
Education is important for a good life	0.018	(0.027)	4.22
Behavior inside school			
Skip class	-0.018	(0.022)	4.59
Late	-0.009	(0.031)	4.13
Expelled	-0.007	(0.024)	4.51
Behavior outside school			
Drink alcohol	0.001	(0.025)	3.47
Smoke cigarettes	-0.011	(0.021)	3.80
Use drugs	-0.007	(0.020)	3.78
Contact with police	-0.018	(0.016)	3.78
School satisfaction			
Satisfaction with life	0.022	(0.048)	7.83
Satisfaction with school	-0.120**	(0.055)	7.60
Satisfaction with class	0.061	(0.054)	7.52
Satisfaction with quality lessons	-0.042	(0.047)	7.08
Satisfaction with mentor	0.028	(0.065)	7.30
Civic engagement			
Listen to others	0.055***	(0.021)	4.21
Young people committed better world	0.013	(0.025)	4.00
Redistribution	0.044	(0.029)	3.68
Diversity	0.057**	(0.026)	3.80
Strike for climate	-0.103***	(0.031)	4.01
Personality traits			
Satisfied	0.040	(0.029)	4.07
Perseverant	0.035	(0.029)	3.94
Future	0.022	(0.026)	4.15
Risk	0.025	(0.033)	3.49
Self-confidence	0.024	(0.031)	3.79
Competition	-0.029	(0.035)	3.84
Procrastinate	-0.030	(0.032)	3.89
Time pressure	0.004	(0.036)	3.20

Note: Each row presents the coefficient, standard error and control mean from a separate regression of equation (1) where the variable indicated in the first column is the dependent variable. Robust standard errors in parentheses. The sample is the survey sample at risk (5,804 observations) but there is some item non-response with the lowest number of observations for "Future" (5,520). The average number of responses missing over all questions is 1.8%. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B4. Heterogeneity

	Sex		SES		Parental background		Standardized test score	
	Males	Females	High SES	Low SES	non-Dutch	Dutch	below median	Above median
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Enrolled top-1 in year 4	-0.764 (0.011)***	-0.765 (0.011)***	-0.778 (0.009)***	-0.701 (0.018)***	-0.737 (0.013)***	-0.782 (0.010)***	-0.713 (0.011)***	-0.822 (0.011)***
Admin outcomes								
On track (yr 4)	-0.035 (0.015)**	0.012 (0.012)	-0.017 (0.010)	0.008 (0.026)	-0.009 (0.017)	-0.015 (0.012)	-0.007 (0.015)	-0.024 (0.012)**
Science/health track	0.018 (0.017)	0.038 (0.018)**	0.024 (0.014)*	0.041 (0.029)	0.013 (0.020)	0.036 (0.016)**	0.034 (0.018)	0.018 (0.018)
Exam on time on track	-0.023 (0.027)	-0.001 (0.025)	-0.019 (0.021)	0.009 (0.045)	0.025 (0.031)	-0.035 (0.024)	-0.008 (0.029)	-0.013 (0.024)
Exam grade	-0.083 (0.053)	-0.021 (0.053)	-0.044 (0.041)	-0.112 (0.092)	-0.081 (0.061)	-0.037 (0.047)	-0.054 (0.052)	-0.037 (0.052)
Enrolled in university	-0.015 (0.027)	0.019 (0.025)	0.000 (0.021)	0.004 (0.042)	0.013 (0.031)	-0.005 (0.023)	-0.024 (0.027)	0.024 (0.025)
Any crime	0.006 (0.011)	0.005 (0.007)	0.009 (0.007)	-0.012 (0.024)	-0.004 (0.011)	0.012 (0.009)	0.000 (0.013)	0.018 (0.012)
Survey outcomes								
Hours homework per wk	0.182 (0.227)	0.903 (0.270)***	0.515 (0.194)***	0.419 (0.473)	0.451 (0.315)	0.524 (0.213)**	0.705 (0.255)***	0.330 (0.258)
Help with homework	-0.021 (0.026)	-0.023 (0.025)	-0.025 (0.020)	-0.006 (0.045)	-0.018 (0.029)	-0.024 (0.023)	-0.034 (0.027)	0.003 (0.024)
Attitudes towards school	-0.019 (0.022)	0.045 (0.021)***	0.009 (0.017)	0.025 (0.040)	0.011 (0.024)	0.011 (0.020)	0.006 (0.022)	0.016 (0.021)
Parental awareness	0.029 (0.070)	-0.036 (0.074)	0.003 (0.056)	-0.032 (0.133)	0.036 (0.082)	-0.025 (0.065)	-0.013 (0.075)	0.028 (0.071)
Behavior inside school	-0.023 (0.028)	0.004 (0.025)	-0.004 (0.021)	-0.049 (0.050)	0.001 (0.030)	-0.020 (0.024)	-0.028 (0.028)	0.009 (0.027)
Behavior outside school	-0.024 (0.022)	0.002 (0.020)	-0.007 (0.017)	-0.039 (0.040)	-0.015 (0.023)	-0.012 (0.020)	-0.021 (0.021)	-0.003 (0.022)
School satisfaction	-0.024 (0.052)	0.006 (0.049)	-0.012 (0.040)	0.018 (0.096)	0.016 (0.059)	-0.021 (0.046)	-0.031 (0.055)	0.009 (0.048)
Civic engagement	0.039 (0.025)	-0.005 (0.024)	0.014 (0.020)	0.017 (0.044)	-0.017 (0.029)	0.032 (0.022)	0.019 (0.025)	0.014 (0.025)
Personality traits	-0.000 (0.023)	0.031 (0.024)	0.005 (0.019)	0.083 (0.045)*	0.048 (0.028)*	-0.001 (0.021)	0.032 (0.025)	0.011 (0.024)
Nr of friends	-0.064 (0.069)	0.085 (0.053)	-0.008 (0.049)	0.077 (0.111)	0.107 (0.075)	-0.051 (0.054)	-0.057 (0.064)	0.060 (0.063)
Share friends fr. school	-0.008 (0.012)	-0.015 (0.011)	-0.013 (0.009)	-0.005 (0.022)	-0.011 (0.014)	-0.012 (0.010)	-0.006 (0.012)	-0.015 (0.011)
Share friends non-Dutch	-0.016 (0.016)	0.010 (0.016)	-0.008 (0.012)	0.015 (0.031)	0.004 (0.020)	-0.011 (0.013)	-0.022 (0.016)	0.009 (0.016)

Note: Each pair of coefficients in an odd and even numbered column come per row from a separate regression of equation (1) where the variable indicated in the first column is the dependent variable. The regression is augmented by including an interaction between D and the variable indicated in the even numbered column as well as the main effect of that variable. Robust standard errors in parentheses. The number of observations differs for each panel, but the shares in the admin sample are 0.49 female, 0.17 low-SES, 0.61 Dutch and 0.49 above-median test score. For column (7) and (8) students with a missing test score are dropped, which includes all applicants in the Covid year 2020. Effects on enrolled in top-1 school are significantly different from the other group for low-SES (higher), Dutch (lower) and above median test score (lower). Students with above median test scores have larger effects on switched schools. The only significant difference for the admin outcomes is that females have a larger effect for on track in year 4. Effects for the survey outcomes are significantly different for females for homework (higher), attitude towards school (higher) and number of friends (higher). Effects on personality traits are higher for low-SES, effects for number of friends are lower for Dutch. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B5. Balance table using 50 propensity score bins

	Not top-1 mean (SD) (1)	Top-1 mean (SD) (2)	Difference without bins (3)	Difference with bins (4)
Student characteristics				
Age (in years)	11.79 (0.39)	11.78 (0.40)	0.014	0.011
Female (0/1)	0.49 (0.50)	0.49 (0.50)	-0.006	0.003
Dutch (0/1)	0.63 (0.48)	0.60 (0.49)	0.025**	0.003
Single parent HH (0/1)	0.19 (0.40)	0.20 (0.40)	-0.005	-0.008
Income below 50th percentile	0.26 (0.44)	0.26 (0.44)	-0.000	0.006
Standardized test score	1.02 (0.41)	1.03 (0.43)	-0.013	-0.010
Test score missing	0.16 (0.36)	0.21 (0.40)	-0.051***	-0.000
Characteristics of first-ranked school				
Assigned to 1st-rank school	0.00	1.00	X	X
Probability assigned to 1st-rank school	0.62 (0.14)	0.75 (0.15)	-0.123***	-0.000
Nr students same 1st school/prim. school	5.19 (4.02)	4.75 (3.98)	0.442***	0.015
Nr students same track/1st school/prim. school	4.22 (3.52)	3.60 (3.17)	0.619***	0.007
Home-school distance 1st-rank school (in km)	4.25 (3.29)	4.15 (3.22)	0.101	0.050
N	2,872	7,085		

Note: Columns (1) and (2) present means and standard deviations. Column (3) reports the difference between the means of columns (1) and (2). Column (4) reports the coefficients from regressions of each characteristic on an indicator for not being assigned to the top-1 school including controls for 50 bins of the propensity score of being assigned to the 1st-ranked school. Stars indicate whether the (conditional) differences in column (3) and (4) are statistically significant. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table B6. Balance table – Inverse probability weighting

	Not top-1 mean (SD) (1)	Top-1 mean (SD) (2)	Difference without α_s (3)	Difference with α_s (4)
Student characteristics				
Age (in years)	11.79 (0.39)	11.78 (0.40)	0.014	0.013
Female (0/1)	0.49 (0.50)	0.49 (0.50)	-0.006	0.006
Dutch (0/1)	0.63 (0.48)	0.60 (0.49)	0.025**	0.005
Single parent HH (0/1)	0.19 (0.40)	0.20 (0.40)	-0.005	-0.023**
Income below 50th percentile	0.26 (0.44)	0.26 (0.44)	-0.000	0.003
Standardized test score	1.02 (0.41)	1.03 (0.43)	-0.013	-0.013
Test score missing	0.16 (0.36)	0.21 (0.40)	-0.051***	0.002
Characteristics of first-ranked school				
Assigned to 1st-rank school	0.00	1.00	X	X
Probability assigned to 1st-rank school	0.62 (0.14)	0.75 (0.15)	-0.123***	0.000**
Nr students same 1st school/prim. school	5.19 (4.02)	4.75 (3.98)	0.442***	0.002
Nr students same track/1st school/prim. school	4.22 (3.52)	3.60 (3.17)	0.619***	-0.007
Home-school distance 1st-rank school (in km)	4.25 (3.29)	4.15 (3.22)	0.101	0.047
N	2,872	7,085		

Note: Columns (1) and (2) present (unweighted) means and standard deviations. Column (3) reports the (unweighted) difference between the means of columns (1) and (2). Column (4) reports the coefficients from regressions of each characteristic on an indicator for not being assigned to the first-ranked school and fixed effects for the first-ranked school, weighted with the inverse of the probability to be assigned to the first-ranked school. Stars indicate whether the (conditional) differences in column (3) and (4) are statistically significant. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table B7. Academic and admin outcomes – Inverse probability weighting

	coeff	s.e.	mean	N
In expected grade/track:				
- Year 3	-0.005	(0.008)	0.88	9,957
- Year 4	-0.007	(0.011)	0.81	8,285
- Year 5	-0.009	(0.013)	0.74	6,538
Science or health track	0.029**	(0.013)	0.47	8,285
Exam passed on time/track	-0.008	(0.020)	0.62	3,174
GPA central exam	-0.059	(0.040)	6.64	2,654
Enrolled in university	0.004	(0.020)	0.47	3,174
Crime	0.010	(0.007)	0.04	9,957

Note: Each row presents the coefficient, standard error and control mean from a separate regression of equation (1) where the variable indicated in the first column is the dependent variable. Observations are weighted with the inverse of the probability to be assigned to the first-ranked school. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $P < 0.01$.

Table B8. Effects of not being assigned to first-ranked school on characteristics of school, teachers and peers - Survey sample

Assignment:	outside top-1 (1)	2nd ranked (2)	3rd ranked (3)	outside top-3 (4)	Contr. mean (5)
School characteristics					
Lower track available	0.238***	0.173***	0.206***	0.351***	0.09
	(0.012)	(0.015)	(0.022)	(0.023)	
School: total nr students	326***	168***	250***	630***	1408
	-31.8	-39.1	-61.8	-62.1	
Home-school distance	0.297***	0.295**	0.743***	0.102	4.10
(assigned school)	(0.102)	(0.141)	(0.218)	(0.144)	
Teacher characteristics					
Total experience since 2006	0.026	0.095***	0.215***	-0.200***	8.35
	(0.028)	(0.036)	(0.054)	(0.054)	
Experience at school since 2006	0.346***	0.305***	0.636***	0.233***	6.90
	(0.040)	(0.057)	(0.081)	(0.071)	
Year of birth	-0.602***	-0.579***	-0.985***	-0.397***	1972
	(0.062)	(0.085)	(0.132)	(0.112)	
Share female	0.012***	0.006***	0.006*	0.024***	0.53
	(0.002)	(0.002)	(0.003)	(0.003)	
Share Dutch	-0.017***	-0.009***	-0.019***	-0.027***	0.71
	(0.002)	(0.003)	(0.004)	(0.004)	
Level (scale 1-5, 5 best)	-0.006	0.030***	0.035**	-0.092***	3.19
	(0.008)	(0.010)	(0.015)	(0.015)	
Peers					
No of former classmates	-0.795***	-0.497***	-0.795***	-1.165***	3.23
(same track)	(0.077)	(0.115)	(0.155)	(0.111)	
Low-income	0.065***	0.042***	0.062***	0.103***	0.37
	(0.004)	(0.005)	(0.007)	(0.007)	
Dutch	-0.054***	-0.045***	-0.055***	-0.068***	0.61
	(0.004)	(0.005)	(0.007)	(0.007)	
Test score	-0.183***	-0.104***	-0.148***	-0.320***	0.95
	(0.009)	(0.010)	(0.018)	(0.015)	
Same track: Low-income	0.041***	0.025***	0.040***	0.064***	0.36
	(0.004)	(0.004)	(0.007)	(0.008)	
Same track: Dutch	-0.031***	-0.030***	-0.035***	-0.031***	0.62
	(0.004)	(0.004)	(0.007)	(0.008)	
Same track: Test score	-0.018***	-0.001	-0.009	-0.048***	1.03
	(0.003)	(0.003)	(0.005)	(0.005)	
N	5,804				

Note: Each cell in column (1) presents the coefficient of D from a separate regression of equation (1) where the variable indicated by the row entry is the dependent variable. The cells in columns (2)-(4) in each row present the coefficients of R^2, R^3 and $R^{>3}$ from a separate regression of equation (2) where the variable indicated by the row entry is the dependent variable. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B9. Academic and admin outcomes; Survey sample.

	outside	2nd	3rd	outside	Control	N
	top-1	ranked	ranked	top-3	mean	
	(1)	(2)	(3)	(4)		
In expected grade/track:						
- Year 3	-0.012 (0.009)	-0.033 (0.013)***	-0.011 (0.017)	0.020 (0.012)	0.92	5,804
- Year 4	-0.014 (0.011)	-0.030 (0.016)*	-0.021 (0.022)	0.011 (0.017)	0.85	5,804
- Year 5	-0.020 (0.014)	-0.012 (0.019)	-0.052 (0.028)*	-0.002 (0.022)	0.80	4,531
Science or health track	0.045 (0.016)***	0.050 (0.022)**	0.036 (0.031)	0.047 (0.025)*	0.49	5,804
Exam passed on time/track	0.003 (0.024)	0.003 (0.031)	-0.050 (0.048)	0.022 (0.038)	0.69	2,035
GPA central exam	-0.050 (0.048)	-0.026 (0.065)	-0.118 (0.101)	-0.048 (0.075)	6.67	1,773
Enrolled in university	0.015 (0.024)	0.024 (0.031)	-0.006 (0.048)	0.049 (0.038)	0.48	2,035
Crime	-0.001 (0.007)	0.010 (0.011)	-0.020 (0.011)*	0.001 (0.012)	0.03	5,804

Note: Each cell in column (1) presents the coefficient of D from a separate regression of equation (1) where the variable indicated by the row entry is the dependent variable. The cells in columns (2)-(4) in each row present the coefficients of R^2, R^3 and $R^{>3}$ from a separate regression of equation (2) where the variable indicated by the row entry is the dependent variable. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B10. Survey outcomes – weighted by inverse probability of survey response

	coeff	s.e.	mean
Hours homework per week	0.523***	(0.189)	7.49
Help with homework	-0.015	(0.021)	2.02
Attitudes towards school	0.010	(0.016)	4.00
Parental awareness	-0.001	(0.055)	8.46
Behavior inside school	0.003	(0.020)	4.41
Behavior outside school	-0.009	(0.016)	3.70
School satisfaction	-0.012	(0.039)	7.44
Civic engagement	0.019	(0.019)	3.94
Personality traits	0.020	(0.018)	3.45
Total nr friends (max 6)	-0.015	(0.049)	5.37
Share friends from school	-0.013	(0.009)	0.68
Share friends non-western name	-0.004	(0.012)	0.24
N	5,799		

Note: Each row presents the coefficient, standard error and control mean from a separate regression of equation (1) where the variable indicated in the first column is the dependent variable. Observations are weighted with the inverse of the propensity score of having responded to the survey. This propensity score is based on a logit regression of an indicator of response on the interacted observable characteristics female, Dutch, low income, switched school by 2019 and the interaction of the first-ranked school and the placement rank (1, 2, 3, 3+). Five observations are dropped because their propensity score is equal to zero. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B11. Survey outcomes; indices constructed using the procedure of Anderson (2008)

	coeff	s.e.	mean
Help with homework	-0.017	(0.017)	0.02
Attitudes towards school	0.014	(0.019)	0.01
Behavior inside school	-0.016	(0.024)	0.01
Behavior outside school	-0.025	(0.024)	0.02
School satisfaction	0.008	(0.021)	0.02
Civic engagement	0.022	(0.021)	0.00
Personality traits	0.010	(0.016)	-0.01
N	5,804		

Note: Each row presents the coefficient, standard error and control mean from a separate regression of equation (1) where the variable indicated in the first column is the dependent variable. There is some item non-response, with the lowest number of observations (5,733) for the index on behavior outside school. Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B12. Academic and admin outcomes, standard errors clustered at level of assigned school

	coeff	s.e.	mean	N
In expected grade/track:				
- Year 3	-0.006	(0.007)	0.88	9,957
- Year 4	-0.012	(0.009)	0.80	8,285
- Year 5	-0.015	(0.012)	0.74	6,538
Science or health track	0.027**	(0.012)	0.47	8,285
Exam passed on time/track	-0.011	(0.016)	0.62	3,174
GPA central exam	-0.052	(0.0387)	6.64	2,654
Enrolled in university	0.002	(0.017)	0.45	3,174
Crime	0.006	(0.007)	0.04	9,957

Note: Each row presents the coefficient, standard error and control mean from a separate regression of equation (1) where the variable indicated in the first column is the dependent variable. Clustered standard errors on assigned school. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B13. Academic and admin outcomes controlling for 50 propensity score dummies

	coeff	s.e.	mean	N
In expected grade/track:				
- Year 3	-0.006	(0.007)	0.88	9,957
- Year 4	-0.012	(0.010)	0.80	8,285
- Year 5	-0.014	(0.012)	0.74	6,538
Science or health track	0.027**	(0.013)	0.47	8,285
Exam passed on time/track	-0.010	(0.020)	0.62	3,174
GPA central exam	-0.058	(0.039)	6.64	2,654
Enrolled in university	0.005	(0.020)	0.45	3,174
Crime	0.007	(0.007)	0.04	9,957

Note: Each row presents the coefficient, standard error and control mean from a separate regression of equation (1) where the variable indicated in the first column is the dependent variable. Instead of controls for first-ranked school fixed effects, include 50 bins for propensity scores* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B14. Other outcomes excluding schools surveyed after Covid

	coeff	s.e.	mean
Hours homework per week	0.483**	(0.192)	7.38
Help with homework	-0.022	(0.020)	2.01
Attitudes towards school	0.028*	(0.016)	4.01
Parental awareness	0.005	(0.055)	8.48
Behavior inside school	-0.018	(0.021)	4.41
Behavior outside school	-0.002	(0.016)	3.71
School satisfaction	0.033	(0.039)	7.49
Civic engagement	0.008	(0.019)	3.94
Personality traits	0.019	(0.018)	3.45
Total nr friends (max 6)	0.024	(0.046)	5.39
Share friends from school	-0.011	(0.009)	0.68
Share friends non-western name	-0.010	(0.012)	0.24
N	5,375		

Note: Each row presents the coefficient, standard error and control mean from a separate regression of equation (1) where the variable indicated in the first column is the dependent variable. Excluding respondents at schools surveyed after March 2020 or whose first-ranked school was surveyed after March 2020. There is some item non-response, with the lowest number of observations for hours homework per week (5,302). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.