SCHOOL CHOICE IN AMSTERDAM: WHICH SCHOOLS ARE CHOSEN WHEN SCHOOL CHOICE IS FREE?

Nienke Ruijs
(corresponding author)
Dutch Inspectorate of Education
3544 AC Utrecht
The Netherlands
n.m.ruijs@owinsp.nl

Hessel Oosterbeek
Amsterdam School of Economics
University of Amsterdam
1018 WB Amsterdam
The Netherlands
h.oosterbeek@uva.nl

Abstract

Using discrete choice models, this paper investigates the determinants of secondary school choice in the city of Amsterdam. In this city, there are many schools to choose from and school choice is virtually unrestricted (no catchment areas, low or no tuition fees, short distances). We find that school choice is related to exam grades and the quality of incoming students, but not to progression in lower grades, no delay in higher grades, and a composite measure of quality published by a national newspaper. Furthermore, students appear to prefer schools that are close to their home and schools that many of their former classmates in primary school attend.

https://doi.org/10.1162/edfp_a_00237

1. INTRODUCTION

This paper uses conditional and mixed logit models to analyze the determinants of secondary school choice in the city of Amsterdam. Students in this city can choose any school that offers education at their academic level, tuition fees are low, and schools are easy to reach because of relatively short distances. These features create an ideal environment to examine whether school choices are affected by different measures of school quality. We do so using data of individual school choices for the school years starting in 2007, 2008, 2009, and 2010. Analyzing individual school choices (instead of aggregate school enrollment data) allows us to compare the importance of school quality for school choices to the importance of other factors, including traveling distance, whether the school was oversubscribed in the previous year, and the number of classmates from primary school who attend the secondary school.

Analyzing the importance of school quality for school choices is important from the perspective of the debate on school choice and competition. The main arguments in favor of these policies are that by having more choice students can find a school that better matches their specific needs and exposure to competition gives schools an incentive to improve education quality.[1] The direct evidence of the impact of school competition on school quality and student achievement is mixed (Hoxby 2000a, 2007; Cullen, Jacob, and Levitt 2006; Hsieh and Urquiola 2006; Rothstein 2007; Böhlmark and Lindahl 2015).[2] A prerequisite for such an impact to emerge is that school choices are at least partially driven by school quality. If this is not the case, improvement of school quality does not raise student inflow and it is then not sensible for schools to compete on this dimension.

In a recent study, Koning and Van der Wiel (2013) examine whether published information on school quality affects secondary school enrollment in the Netherlands. They find higher quality scores are associated with a small but significant increase in enrollment. There are, however, reasons to reexamine the findings of this study. First, Koning and Van der Wiel analyze data from the whole country, thereby also including more remote areas where most schools are in fact local monopolists. By analyzing data from a city where the supply of schools at any academic level is abundant, we focus on a situation where students have more scope to respond to variation in quality.

Second, because of data limitations, Koning and Van der Wiel use enrollment in the third year of secondary school as a proxy for applications in the first year. Since on average 3.9 percent of Dutch secondary school students repeat a grade in a given year in the first three years of secondary school (CBS 2012), this implies that in the third year, 7.8 percent of the enrollees have been replaced by 11.7 percent repeaters from previous

1. We will often phrase as if students are the decision makers. We acknowledge that in reality their parents have an important say in this.
2. In a famous study, Hoxby (2000a) finds that more school competition (measured as the number of school districts in a metropolitan area) boosts student achievement in the United States. Identification is based on variation in the number of school districts caused by the numbers of streams (rivers). Rothstein (2007) has shown that Hoxby's results are sensitive to the way in which streams are counted (see also Hoxby 2007). Hsieh and Urquiola (2006) find no evidence that choice, triggered by the provision of vouchers, improved student achievement in Chile. Using a comparable reform in Sweden, however, Böhlmark and Lindahl (2015) find a positive effect of choice on student achievement. Using data from admission lotteries from the Chicago public school system, Cullen, Jacob, and Levitt (2006) find that students who lost the lottery—and therefore have a restricted choice set—are not harmed in their achievement.

cohorts. These repeaters from previous cohorts have based their school choices on published information on school quality from earlier years. In addition, a non-negligible fraction of 23 percent of secondary school students are transferred to a higher or lower academic track by the time they reach the third year or have moved to another school (Onderwijsinspectie 2007). Moreover, because the most popular schools are oversubscribed and cannot admit all applicants, school choice in response to school quality is better measured by applications rather than enrollment. Our data on school choice in the city of Amsterdam contain information on first-year applications.

Third, the analysis of Koning and Van der Wiel does not include students in the lowest levels of prevocational education, and thereby omits 25 percent of Dutch secondary school students. Our analysis covers all academic levels. Koning and Van der Wiel (2013) focus on a single composite measure of school quality that is published each year in a national newspaper in the Netherlands. In our main analysis, we report results based on the same measure. Additionally, we inquire about the importance of the underlying components and we investigate whether the quality of the incoming students influences school choices.[3]

Related to our paper are also the studies of Hastings, Kane, and Staiger (2009), Burgess et al. (2015) and Glazerman and Dotter (2016). Hastings, Kane, and Staiger (2009) use data from primary and middle school choices from a school district in the United States. In that district, a school choice program was introduced where parents could list their top three schools. Using exploded mixed logit models, the authors find that the weight parents place on school characteristics is heterogeneous. Parents, especially those of high socioeconomic status, tend to prefer schools with high test scores. Parents also tend to prefer schools in which the majority of the students are of the same race as their children. These results imply that minority parents face a trade-off between high performance and ethnic composition. Further, the distance to schools and the availability of transportation are relevant determinants of school choice. Burgess et al. (2015) combine survey and administrative data to estimate conditional logit models and find that most families strongly prefer the academic performance of schools. Socioeconomic composition and distance are also important drivers of school choice. More advantaged parents have stronger preferences for academic performance. Glazerman and Dotter (2016) use information from rank-ordered lists to estimate school preferences of 22,000 applicants to 200 schools in Washington, DC. They find that distance, school demographics, and academic indicators play important roles in school choice. There is also heterogeneity of preferences.[4]

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3. In another recent study using Dutch data, Borghans, Golsteyn, and Zölitz (2015) investigate the determinants of school choice of the parents of primary school students in the southern part of the southern province Limburg. They find that parents tend to avoid the roughly 5 percent of the schools the Dutch Education Inspectorate assesses to be weak. Primary schools with higher average scores on the national exit exam attract more students.
4. Other related studies analyzing revealed preferences include Weiher and Tedin (2002) who analyze data from 1,000 charter households in Texas and find that race is a good predictor of their revealed (and stated) preferences; Harris and Larsen (2015) use application data from students in New Orleans to examine how school choices differ before and after Hurricane Katrina; and Nathanson, Corcoran, and Baker-Smith (2013) analyze application and admission data of five cohorts of high school students in New York City and focus on the differences in choices and placement between low-achieving students and others. Nathanson, Corcoran, and Baker-Smith find that low-achieving students are matched more often to lower-performing schools than other students. This is mainly because of differential choices, not to differential rates of being placed in their top choices. Studies that analyze stated preferences obtained through surveys include Collins and Snell (2000),

In the years in our study that we analyze, the secondary schools in Amsterdam used a version of the adaptive Boston mechanism to assign students to schools. More specifically, each student applied to one school that offers the academic track advised to the student.[5] If the number of applicants for a school (and track) does not exceed the number of available seats, all applicants are placed. If a school is oversubscribed, it organizes a lottery. Students who lose the lottery can subsequently only choose from the schools that then still have vacant seats. This matching algorithm potentially gives rise to strategic behavior where students do not report their truly preferred school (Abdulkadiroğlu and Sönmez 2003; Calsamiglia and Guell 2014). In section 2 we discuss this in more detail and explain why we think the parameters estimated by the logit models will closely reflect students' true preferences for school characteristics.

In addition to measures of school quality and oversubscription in the previous year, we also consider the impact on school choice of the number of classmates from primary school who attend a specific secondary school. Peer effects are intrinsically difficult to identify. De Giorgi, Pellizzari, and Redaelli (2010) find that peer groups do not fully overlap to identify peer effects in the choice of college majors. Lacking such a source of variation and in the absence of random assignment of peers (as in Sacerdote 2001), we restrict ourselves to testing the null-hypothesis of no peer effects in school choice.[6]

The main results of this paper are threefold. First, school choices are related to measures of quality. In particular, students prefer schools with higher exam grades and with a better quality of incoming students. This is especially true for students in the higher academic tracks. Choices are not driven by other dimensions of school quality (progression in lower grades and no delay in higher grades) that are included in the composite quality-measure published by a national newspaper. As a result, the composite measure is not a consistent predictor of school choice in Amsterdam. Second, we find some evidence that when a school conducts an admission lottery in the previous year, students are deterred from choosing that school, suggesting that strategic behavior occurs. Third, our results reject the null-hypothesis of no peer effects in school choice. When a larger share of a student's primary school peers chooses a certain secondary school, the student is more likely to pick that school as well. This is even true when we correct for the systematic component in the popularity of the specific secondary school among the students in the student's primary school.[7] Although this result does not prove the importance of peer effects in school choices, it is consistent with it.

This paper proceeds as follows. The next section describes the context of secondary school choice in Amsterdam. Section 3 describes the data. Section 4 provides details of the empirical strategy that we utilize. Section 5 presents and discusses the empirical findings. Section 6 summarizes and concludes.

who look at 101 parents who sent their children to one of two neighboring schools in the United Kingdom; Denessen, Driessen, and Sleegers (2005), using data from 10,000 parents in the Netherlands who were asked which factors are driving the choice of primary school for their children; and Jochim et al. (2014), who surveyed 4,000 public school parents in eight "high-choice" cities in the United States and find that parents do exercise their choice but want better options.

5. Section 2 provides details about the tracking system in Dutch secondary education.
6. Angrist (2014) gives a critical assessment of the peer effects literature.
7. Further details are provided in section 4.

2. CONTEXT

This section begins by describing the key features of school choice in the Netherlands. Next it explains the structure of secondary education in the Netherlands. The final subsection gives information about some specificities of secondary school choice in the city of Amsterdam.

School Choice in the Netherlands

The Netherlands has a long history of free school choice. The constitution of 1848 guarantees the freedom to provide education. In 1917, this freedom was extended with the amendment that all schools receive state funding. In current practice, this means that privately run schools (either with a religious background or subscribing to specific pedagogical approaches such as Montessori or Dalton) are publicly funded at the same level as publicly run schools. In return, schools have to adhere to certain rules. In particular, they are subject to quality inspections by the Dutch Education Inspectorate.

For students, these regulations imply that they are free to choose the school they want; they are not restricted by measures such as catchment areas.[8] Dutch students make extensive use of this option: 70 percent of students in primary education and 75 percent of students in secondary education are enrolled in a publicly funded, privately run school (CBS 2009). Privately funded schools are virtually nonexistent—in 2009, only 0.3 percent of students in secondary education attended a privately funded school (Onderwijsinspectie 2010).

The government funding of schools is to a large extent dependent on student numbers, in which the money follows the student. There are additional funding schemes for disadvantaged students. In primary education there is the system of weighted student funding. For disadvantaged students, in terms of low parental education, schools can get additional funding up to 1.2 times the regular per-student funding. In this paper we use the weighted student funding to identify disadvantaged students.

Secondary Education in the Netherlands

Dutch secondary education starts around age 12 years and lasts four to six years. The length of secondary education depends on the school track (the Netherlands has a tracked secondary school system). The lowest track (pre-vocational secondary education) lasts four years and gives access to vocational education programs. Within the pre-vocational track, there are four different levels, each giving access to different levels of vocational education programs. In this paper, they are indicated with the numbers I to IV, with IV being the highest level.[9] The intermediate track (senior general secondary education) takes five years, and gives access to professional colleges. The highest track (pre-university education) takes six years, and gives access to university education.

Which school track a student should take is mainly decided at the end of primary education. It is partly determined by standardized tests (in most cases the nationwide exit test, called the “citotoets” [citotest]), and partly determined by the assessment of the

8. A small number of municipalities have put restrictions on primary school choice to foster desegregation (Ladd, Fiske, and Ruijs 2011). In those projects, preferences are important in placement decisions as well.
9. Pre-vocational III will not be taken into account in the analyses, because only 2 percent of students receive a pre-vocational III advice.

primary school teacher. Not all secondary schools offer all school tracks, so the track advice is an important factor in secondary school choice. When offering more than one school track, schools are allowed to teach children of different tracks together. Depending on student achievement and school policies, students may change tracks during secondary education. Also, they may enroll in a higher track after finishing a lower track.

Subject to some conditions, students may choose which courses they want to take during their last years in secondary school. Secondary schools have to follow national curriculum guidelines. Students take centrally determined national exams at the end of secondary school. The national exams count for 50 percent of students' final grades, and the other 50 percent is determined by school-specific exams taken in the last two or three years of secondary education.

Secondary School Choice in Amsterdam

Amsterdam is the capital of the Netherlands and is its largest city with 750,000 inhabitants. Each year, 5,500 to 6,000 students transfer from primary education to secondary education. In the city of Amsterdam, there are about fifty-four secondary schools, excluding schools for students with special educational needs. Not surprisingly, some schools are more popular than others. Each year some schools are oversubscribed and conduct lotteries to allocate the available places.

In the years that we analyze, the secondary schools in Amsterdam use a version of the adaptive Boston mechanism to assign students to schools. More specifically, each student applies to one school that offers the academic track that was advised to the student. There are no default schools—primary schools ensure that every student submits an application form. If the number of applicants for a school (and track) does not exceed the number of available seats, all applicants are placed. Students who perform on the standardized test in line with the primary school teachers' advice cannot be rejected by the secondary school, which effectively means that it is hard for schools in Amsterdam to select students.[10]

If a school is oversubscribed, it organizes a lottery. Some schools have oversubscription for some school tracks but not for others. In that case, the lottery is conducted for each school track separately. Schools are allowed to use a limited number of priority rules, that is, they can grant priority to siblings of current students, children of staff members, and students from a primary school with similar special programs (for example, Montessori secondary schools can grant priority to children from a Montessori primary school). These priority rules need to be announced before the application date, so they are known to parents. In the years that we study, around 5 percent of the students could not be placed in the school where they applied in the first round because they lost the lottery.

10. The definitions for corresponding test scores are strictly prescribed. When a student has a lower score than can be expected from the teacher's advice, the secondary school should discuss the student with the primary school and/or conduct an extra standardized test. In these cases, the secondary school has some discretion in rejecting the student, which happens in about 5 percent of the cases. For most school tracks, the majority of the students (52 percent) have a test score in line with the primary school advice. An exception to this are the vocational levels with additional support. Here, all students are placed after discussions with the primary school.

Students who lose the lottery can subsequently only choose from the schools that then still have vacant seats. This matching algorithm potentially gives rise to strategic behavior where students do not report their truly preferred school (Abdulkadiroğlu and Sönmez 2003; Calsamiglia and Guell 2014). This implies that parameters estimated by the logit models may deviate from the parameters of the utility function. We have reason to believe, however, that such deviations are small. In recent years, the secondary schools in the city of Amsterdam switched from the adaptive Boston mechanism to the truth-telling Deferred Acceptance mechanism. A study that compares school choices under the two mechanisms in Amsterdam estimates that only 8 percent of the students do not apply to their most-preferred school under the Boston mechanism (see De Haan et al. 2016). Moreover, the students who choose strategically do not choose schools that are very different from the schools they rank first. This implies that 92 percent of the students choose the school they prefer most, whereas the other 8 percent choose a school that is similar to their most preferred school.[11] This concurs with results reported by Burgess et al. (2015). These authors use data from England in which roughly two-thirds of their observations are exposed to a truth-telling mechanism and about one-third to a strategic mechanism. When they restrict their sample to the parents exposed to the truth-telling mechanism, their results are robust. To further accommodate concerns about strategic choices, we include as covariate an indicator that equals one if the school/track organized a lottery in the previous year.

3. DATA

This section describes the data sources used in this paper. It first describes the data about students, then the data about schools.

Student Information

Data come from the centralized application and placement system of the city of Amsterdam. This database has information on 21,117 Amsterdam students choosing a secondary school in Amsterdam in the four years from 2007 to 2010. The database provides information on student background characteristics, such as sex and ethnicity (but not income), and on primary school achievement, such as school track advice, score on the standardized test ("citoscore") at the end of primary school, and grade repetition in primary school.

For each student, we know at which school he or she applied in the first round and whether the student was enrolled at that school. For students who are not placed at the school where they apply in the first round, subsequent choices are also registered. This information, however, is not used here because the choice sets after the first round are not sufficiently clear. Moreover, because not many students lose a lottery in the first round, this only involves a small number of observations.

Using information on students' school track advice and information on the school tracks offered by each secondary school, we can create the choice set for each student.

11. Switching schools during secondary school is not forbidden but relatively infrequent. Dependent on the school track, 71 percent to 84 percent of the secondary school students in Amsterdam are at the same school as where they started in the fourth year of education (Gemeente Amsterdam 2013). Note that these numbers include students who switch schools because they change school tracks or prefer pre-vocational courses that are not offered at their school.

Table 1. Student Characteristics

	N Students	Share of Boys	Both Parents Born in the Netherlands		Belonging to Disadvantaged Group		Citoscore		
			Yes	Missing	Yes	Missing	Mean	SD	Missing
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Pre-university	4,374	0.50	0.60	0.04	0.14	0.12	544.1	7.4	0.01
Senior general/pre-university	2,819	0.49	0.45	0.05	0.27	0.11	540.5	6.1	0.01
Senior general	3,133	0.50	0.39	0.05	0.34	0.12	537.7	6.4	0.02
Pre-vocational IV/senior general	2,028	0.49	0.28	0.06	0.47	0.10	534.4	6.2	0.02
Pre-vocational IV	2,881	0.50	0.22	0.05	0.54	0.10	531.5	6.7	0.03
Pre-vocational II	2,130	0.49	0.16	0.06	0.67	0.09	526.7	8.1	0.19
Pre-vocational I/II	1,039	0.48	0.14	0.05	0.65	0.15	524.7	9.0	0.33
Pre-vocational I	2,713	0.47	0.14	0.07	0.64	0.19	520.4	10.6	0.58
Total	21,117	0.49	0.34	0.05	0.42	0.12	535.7	10.1	0.12

Notes: Columns 2 to 6 report shares. The "No" category is omitted.

Although schools outside of Amsterdam can also be chosen, we limit the choice set to schools in the city of Amsterdam. Schools outside of Amsterdam do not follow the same enrollment rules and their students are not registered in the Amsterdam enrollment system. Similarly, children outside of Amsterdam can choose schools in Amsterdam. Because their data are not consistently registered, these children are omitted from the analysis as well. Because the enrollment procedures are different for special educational needs schools, we drop those schools from the sample. Moreover, we drop some students who have missing values on key variables.[12]

Table 1 reports descriptive statistics on student characteristics. The first column shows that the pre-university track attracts around 21 percent of students. Another 28 percent of students enter secondary school at the combined senior general secondary/pre-university track or the senior general secondary track. The remaining 51 percent of students start secondary education at the pre-vocational tracks. The second column shows that the share of boys is fairly constant across the different secondary school tracks. They are only somewhat underrepresented at the two lowest tracks.

12. Overall, 7,432 students are dropped out of the initial sample of 28,549. We drop 3,913 of them because they are living outside of Amsterdam or going to primary schools outside of Amsterdam. These students prefer schools in Amsterdam over schools in their own municipalities. We drop these students because we only have information about students applying to schools in Amsterdam, so we cannot compare characteristics and choice preferences of students who decide to travel to Amsterdam with students choosing schools in these other municipalities. Vice versa, we do not have information on students living in Amsterdam choosing schools outside of Amsterdam. This group, however, is very small: 0.8 percent of the pre-university students choose a school outside of Amsterdam (De Haan et al. 2016). We drop 1,488 students because they attend special education needs schools; 1,123 students are dropped because their primary school advice (e.g., practical education, pre-vocational III) is not taken into account; 103 students are dropped because their primary school is unknown; and 236 students are dropped because their address is not registered. We drop 416 students because they chose schools that should not be in their choice set given their primary school advice. This group is distributed over all primary school advice. Within school advice, small groups of students go to the same secondary school that offers either tracks just above or just below their primary school advice. Their citoscores are generally either relatively low or relatively high. Given that we do not know the exact choice set of these students, they are dropped from the sample. Finally, 153 students are dropped because they go to a few very small secondary schools that do not have enough observations to take into account in this study (these are mainly religious schools, such as Jewish or Islamic schools).

Table 2. Descriptive Information on Students' Choice Set

		Distance (in kilometers)				Actual Share of Peers		Predicted Share of Peers	
	N Schools	Chosen School M (SD)	Nearest School M (SD)	All Schools M (SD)	Share of Schools with Lotteries	Chosen School M (SD)	All Schools M (SD)	Chosen School M (SD)	All Schools M (SD)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Pre-university	27	3.09 (2.13)	1.04 (0.71)	5.00 (2.90)	0.08	0.12 (0.12)	0.03 (0.06)	0.11 (0.11)	0.03 (0.06)
Senior general/pre-university	21	3.17 (2.15)	1.20 (0.78)	5.60 (3.08)	0.10	0.14 (0.14)	0.03 (0.07)	0.13 (0.12)	0.03 (0.06)
Senior general	23	3.08 (2.16)	1.14 (0.73)	5.57 (3.04)	0.10	0.14 (0.14)	0.03 (0.07)	0.14 (0.13)	0.03 (0.06)
Pre-vocational IV/senior general	18	3.01 (2.10)	1.24 (0.71)	6.03 (3.28)	0.11	0.17 (0.16)	0.03 (0.07)	0.16 (0.14)	0.03 (0.07)
Pre-vocational IV	23	2.85 (2.08)	1.14 (0.72)	6.03 (3.20)	0.16	0.18 (0.17)	0.03 (0.07)	0.17 (0.15)	0.03 (0.07)
Pre-vocational II	22	2.98 (2.29)	0.96 (0.59)	6.28 (3.40)	0.05	0.13 (0.15)	0.02 (0.06)	0.12 (0.12)	0.02 (0.06)
Pre-vocational I/II	21	2.85 (2.23)	1.00 (0.66)	5.82 (3.16)	0.06	0.12 (0.15)	0.02 (0.06)	0.11 (0.12)	0.02 (0.05)
Pre-vocational I	22	2.82 (2.12)	1.05 (0.69)	6.00 (3.15)	0.06	0.12 (0.13)	0.02 (0.07)	0.11 (0.11)	0.02 (0.06)

Notes: Column 3 reports the distances to the nearest school within a students' choice set, column 4 reports the average distance to all schools within a students' choice set. Column 6 describes the average share of peers from primary school that also chose the chosen secondary school. The predicted share of peers is a linear prediction of the actual share of peers in a primary school that chooses for a certain secondary school over the four years of the study.

Column 3 demonstrates the segregation of secondary school tracks in Amsterdam along the lines of migrant status. Although the share of students in the population with both parents born in the Netherlands is only around one-third, their share in the highest track is 0.6, and this decreases monotonically to 0.14 in the lowest track. This carries over to column 5, which shows the share of students with a disadvantaged background by school track. Column 7 shows the monotonic relationship between students' scores on the exit exam in primary school and their track in secondary school (the scale for this variable runs from 500 to 550).

School Information

Table 2 reports descriptive information on the choice set and the chosen school for the different school tracks. The first column reports the numbers of schools at each track from which students can choose. Many schools appear multiple times in this column because they offer more than one secondary school track.

Using information on students' home addresses, we calculated the distances from their house to Amsterdam schools in their choice set. These distances are calculated in a straight line based on GPS coordinates.[13] Columns 2 to 4 provide means and standard deviations of the distances that students have to travel to the school they choose (column 2), to the school offering their advised track nearest to where they live (column 3) and the average over all schools at their advised track (column 4). The mean distance to

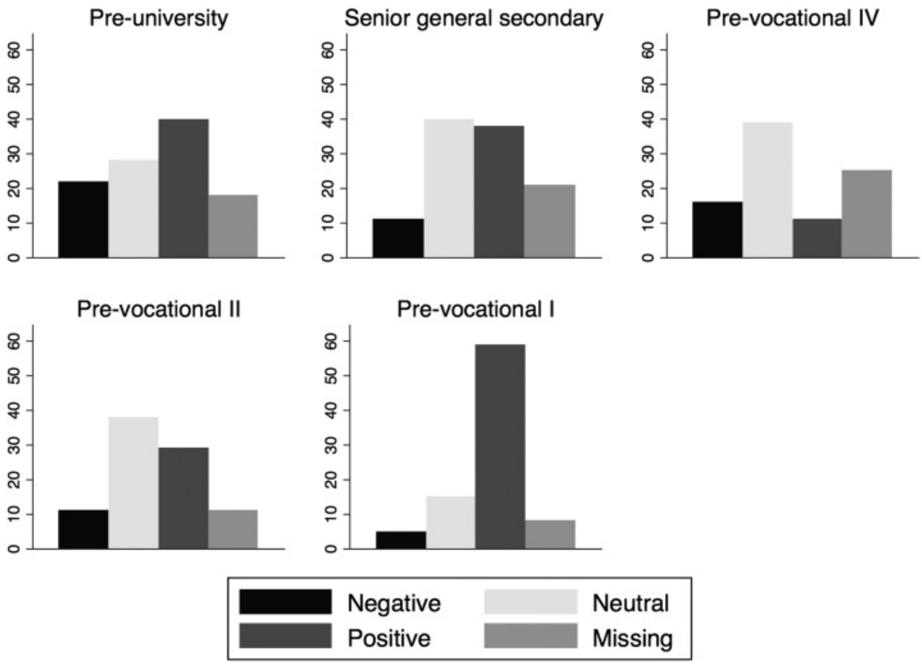
13. For a random sample of one hundred students, we calculated the road distance as well. The road distance turned out to be very closely related to the distance in a straight line ($r = 0.99$).

the nearest school is close to one kilometer and this is very similar for the different school tracks. The mean distance to the school that is actually chosen is about three kilometers at all school tracks, whereas the mean of the average distance to all schools at a given track is five to six kilometers, again with little variation across school tracks. We constructed an indicator that equals one for tracks in schools that had a lottery in the year prior to the year of application.[14] Column 5 of table 2 shows the share of schools that conducted a lottery, averaged over the four years of observations.

To study the relationship between a student's own school choice and that of his or her classmates, columns 6 to 9 provide information about the actual and predicted shares of peers in primary school who go to the same secondary school, and the shares of classmates who would go to the same school if students chose their secondary schools randomly. For each student, we calculated the actual share of classmates in primary school who apply to each of the secondary schools.[15] Because some secondary schools may always be more popular among students from a specific primary school (because of distance or pedagogy), we also construct predictions to investigate classmates' choices for secondary schools in deviation from primary-school-specific trends. For these predictions we use the actual share of peers in the different years to estimate a linear trend over the four years of our study, and the predicted values of those regressions as a measure of predicted popularity. This is akin to the population variation used by, for example, Hoxby (2000b). These shares show that classmates from the same primary school tend to choose the same secondary school (the actual share of peers in chosen schools exceeds the actual share of peers in all schools) even when corrected for the usual popularity of secondary schools among students from a given primary school (the actual share of peers in chosen schools exceeds the predicted share of peers in chosen schools).

As described in section 2, Dutch schools are subject to quality inspections by the Dutch Education Inspectorate. Their quality information is based on several aspects, such as exam results and school visits. Since the 1990s, the Inspectorate's quality assessment of schools is public information, published on a yearly basis. A national newspaper called *Trouw* makes the quantitative part of the information accessible to the broader public, by publishing exam grades and other characteristics, and by computing an overall quality score measured on a 5-point scale ("- -," "-," "0," "+," "++").[16] The inputs for the newspaper's quality scores are: final exam grades in the school track, the percentage of students getting a degree without grade repetition in the higher classes, and the performance of the school in the lower years. For this last number, changes in school track play a role: students enrolled in a lower track than their initial advice reduce the score, students enrolled in a higher track increase it. The overall quality score

14. The information we use for the lottery indicator is from a booklet published annually by the municipality of Amsterdam. This booklet includes one page of information on each secondary school, together with information on the general enrollment procedure. It is handed out to all students in the last year of Amsterdam primary schools. From our contacts with schools, we noticed that the booklets have a few errors on the lotteries. Because students will visit the school before subscribing, and they will receive the correct information there, we decided to adjust these cases to match the information from the schools.
15. Because we do not have information on classes within primary schools, we define classmates as all other students in the final grade of a students' primary school.
16. In 2001, a news magazine called *Elsevier* also started publishing school quality information based on the Inspectorate information. We choose to use the *Trouw* data because they were the first to publish this information and because it has a wider circulation than *Elsevier*.



Note: This figure shows the occurrence of different *Trouw* quality scores for Amsterdam schools published in December 2006 to December 2009.

Figure 1. Variation in *Trouw* Quality Scores

is calculated and published for each school track separately, so within a school, there may be different quality scores. Very small schools or schools that miss values on the underlying scores (for instance, because a school is new) do not get an overall quality score.

The quality information we use for each student is the quality information published in December before the student's school choice in April. In 2009—the last publication relevant in our data—the 5-point scale was abolished and only the underlying numbers were published. Since the 5-point scale was computed in the same way every year, we are able to reconstruct the scores. We will study the 2009 quality scores separately in the analyses.

Because only Amsterdam schools are included in this study, there are not enough schools with the extreme quality scores "- -" and "++" in the sample. Therefore, we transformed the 5-point scale into a 3-point scale. As can be seen in figure 1, there is substantial variation in the quality scores of the Amsterdam schools.[17] The quality scores are not stable over time. Table 3 shows the variation of the quality scores within schools in the four-year period of the study. For example, only ten out of the

17. Four schools (with seven overlapping school tracks) are registered formally as two separate schools. These two pairs of schools cooperate very closely, and only receive one quality score for the pair. Therefore, we used this quality score for both schools.

Table 3. Changes in School Quality Scores over Time

School Track	*N* Schools	*N* Stable Schools	Quality Scores in Current Year	Quality Scores in Previous Year: Missing	Negative	Neutral	Positive
			Pre-university				
Pre-university	27	10	Missing	9	0	0	0
Senior general/pre-university	21	6	Negative	4	11	2	0
			Neutral	2	8	8	2
			Positive	0	1	9	24
			Total	15	20	19	26
			Senior General Secondary				
Senior general/pre-university	21	7	Missing	13	0	0	0
Senior general	23	9	Negative	1	0	3	1
Pre-vocational IV/senior general	18	7	Neutral	0	6	21	4
			Positive	1	4	5	22
			Total	15	10	29	27
			Pre-vocational IV				
Pre-vocational IV/senior general	18	4	Missing	14	0	1	1
Pre-vocational IV	23	5	Negative	0	3	6	2
			Neutral	4	8	16	2
			Positive	1	1	6	2
			Total	19	12	29	7
			Pre-vocational II				
Pre-vocational II	22	5	Missing	3	0	3	1
Pre-vocational I/II	21	5	Negative	0	4	2	0
			Neutral	2	5	17	8
			Positive	2	1	5	13
			Total	7	10	27	22
			Pre-vocational I				
Pre-vocational I/II	21	11	Missing	3	0	0	3
Pre-vocational I	22	11	Negative	0	3	0	0
			Neutral	1	1	6	4
			Positive	1	0	7	36
			Total	5	4	13	43

Notes: In this table, we include all relevant schools that have a quality score for a certain school track. We do not take into account the fact that some schools are not in the choice set for combined tracks (a student with combined senior general secondary/pre-university advice cannot go to a school that only offers pre-university education). The number of stable schools indicates the number of schools that have the same quality score over the four included years.

twenty-seven schools that offer the pre-university track keep the same quality score for four consecutive years.

4. EMPIRICAL STRATEGY

The description of the empirical approach is divided into two parts. It begins with a description of conditional and mixed logit models that we use for the main analysis. Next, it provides details about the approach we use to say something about the influence that peers have on school choices.

Main Analysis

To study the determinants of secondary school choice we use discrete choice models. Specifically, we will estimate conditional and mixed logit models. These models follow from a random utility framework in which a student is assumed to choose the school that maximizes her utility. The utility that student i choosing in year t derives from secondary school s is defined as $U_{its} = V_{its} + \varepsilon_{its}$, where V_{its} is the observed portion of utility and ε_{its} is the unobserved portion of utility. ε_{its} is assumed to be an independently, identically distributed extreme value, in which the independence means the unobserved utility for one alternative is unrelated to the unobserved utility for another alternative (Train 2009). We will use different specifications of V_{its}, thereby imposing different assumptions on the model. In all cases, we investigate school choice separately for each group of students with the same school track advice because the students' choice sets differ by advised school track.

First, we analyze whether school choice can be predicted by student and school characteristics, in which $V_{its} = x'_{its}\beta + w'_{ts}\delta + z'_{it}\gamma_s$. Here x_{its} are characteristics that are specific for a student–school combination. In the main analysis this will only be the distance from home to school. In additional analyses it will also include the share and the predicted share of a students' primary school peers going to that secondary school. w_{ts} are secondary school–specific factors: the school quality characteristics and whether the school had a lottery for that school track in the previous year. z_{it} are student-specific factors, included such that the coefficients of the variables of interest are conditional on these variables. The included student specific factors are gender, ethnicity, test score, student weight, and indicators for missing values on these characteristics. The probability (p_{its}) that student i in year t chooses for secondary school s is given by:

$$p_{its} = \frac{\exp\left(x'_{its}\beta + w'_{ts}\delta + z'_{it}\gamma_s\right)}{\sum_l^m \exp\left(x'_{itl}\beta + w'_{tl}\delta + z'_{it}\gamma_l\right)}, \qquad (1)$$

The parameters β, δ, and γ are estimated using maximum likelihood estimation, in which the log likelihood function is given by $LL(\beta, \gamma_s, \delta) = \sum_{s=1}^{S} \sum_i y_{its} \ln p_{its}$, where y_{its} is a binary indicator equal to one if student i in year t chooses school s, and zero otherwise.

In specification 1, we do not take into account differences between secondary schools, apart from school quality and having a lottery in the previous year. It is likely, however, that unobserved characteristics (in our database) of the secondary school, such as the quality of the building and the availability of extracurricular activities, are important in secondary school choice. In specification 2, we therefore add secondary school fixed effects, d_{sj}, as a secondary school dummy, which has $d_{sj} = 1$ if $s = j$ and $d_{sj} = 0$ if $s \neq j$. Adding secondary school dummies changes the interpretation of the quality scores—we no longer use the absolute scores of a secondary school but study how school choices change when school quality scores change.

$$p_{its} = \frac{\exp\left(x'_{its}\beta + w'_{ts}\delta + z'_{it}\gamma_s + \sum_{j=2}^{J} d'_{sj}\alpha_j\right)}{\sum_l^m \exp\left(x'_{itl}\beta + w'_{tl}\delta + z'_{it}\gamma_l + \sum_{j=2}^{J} d'_{lj}\alpha_j\right)}, \qquad (2)$$

Models 1 and 2 are conditional logit models, which assume that the errors ε_{its} are independent of each other, such that the unobserved portion of utility for one alternative is unrelated to the unobserved portion of utility for another alternative. This lack of correlation gives rise to the property of independence of irrelevant alternatives (IIA). IIA means that for any two alternatives j and k, the relative odds of choosing j over k are the same, such that the ratio is not dependent of the presence or attributes of other alternatives (Train 2009). In practice, this means that two schools (of the same track) will be equally affected by the opening of a new competing school. This is not very likely—the school that is more closely related in terms of distance or school policy will probably be affected more. Mixed logit models do not have the IIA property. Therefore, we have also estimated mixed logit models for secondary school choice. As can be seen in equation 3, we estimated random coefficients for the student–school-specific characteristics and the school-specific characteristics (quality, lottery in the previous year, and distance). $\phi(\beta, \delta|b, d, W)$ indicates the mixing distribution, in this case the normal density with means b and d and covariance W.[18]

$$p_{its} = \int \left(\frac{\exp\left(x'_{its}\beta + w'_{ts}\delta + z'_{it}\gamma_s + \sum_{j=2}^{J} d'_{sj}\alpha_j\right)}{\sum_l^m \exp\left(x'_{itl}\beta + w'_{tl}\delta + z'_{it}\gamma_l + \sum_{j=2}^{J} d'_{lj}\alpha_j\right)} \right) \phi\,(\beta, \delta|b, d, W)\, d\beta. \qquad (3)$$

Another advantage of the mixed logit model is that it allows for variation in preferences for school characteristics. It might be that some students put a high weight on the published quality scores, whereas others do not. Or some risk-averse students may avoid a school that had a lottery in the previous year, whereas others do not care so much. In the mixed logit model, we do not only estimate the means of the preferences for school characteristics but also their standard deviations, which allows us to see to what extent the preferences for secondary school characteristics vary across students.

Peer Effects in School Choices

The school choice literature typically assumes that students make their choices individually and ignores possible influences of peers. Casual observation suggests this is unrealistic. Students who go from a primary school to a secondary school seem to be influenced by the choices of their classmates in primary school. They may coordinate their school choices, or some students may follow the choices of others. There may, in other words, be peer effects in school choices.

Peer effects are intrinsically difficult to analyze (Manski 1993; Angrist 2014). The following identification problems are present. The first is the reflection or simultaneity problem. In the presence of peer effects, it is not always clear how the causality runs—do peers affect the respondent, or does the respondent affect peers? The second is the self-selection problem. Group membership is endogenous—peers select themselves on the basis of similar characteristics. Further, in the presence of peer effects, it is not always clear whether effects are driven by behavior, or by unobserved characteristics

18. Rather than a normal density, Hastings, Kane, and Staiger (2009) assume a negative lognormal distribution for distance, which imposes that all students dislike commuting. Without explicitly imposing this restriction, we consistently find that students have a negative preference for distance.

that are correlated with it. Finally, there is the errors-in-variables problem—it is not always clear how to define the relevant peer group.

The economics of education literature has so far been unsuccessful in addressing all these problems in a satisfactory way. We follow Sacerdote (2001) and include the fractions of primary school classmates choosing for each secondary school in the logit model of students' secondary school choice. The results are subject to the reflection problem and the correlated errors problem, and can therefore not be given a causal interpretation. The results are, however, informative about the degree of correlation in the choices of classmates in primary school.

To address the correlated errors problem, we include two variables capturing peer preferences. We include the predicted share of peers in which we account for the (trend in the) general popularity of a secondary school within a primary school. This captures systematic feeder patterns related to factors, such as pedagogy and distance. The second variable, the actual share of peers, captures whether the choices of students' primary school classmates deviate from the choices of students from other cohorts in that primary school. If inclusion of the first component solves the correlated errors problem, the estimate of the effect of the idiosyncratic component in classmates' choices is only biased due to the reflection problem. According to Sacerdote (2001), this allows us to test the null hypothesis of no peer effects, which predicts no relationship between a student's own school choice and the idiosyncratic school choices of her classmates in primary school.

5. RESULTS

The empirical results are presented in four subsections. The first subsection presents the main findings from the conditional logit models with and without school fixed effects. The next subsection explores the sensitivity for the inclusion of other measures of school quality than the composite measure published by the national newspaper *Trouw*. We then present the results from the mixed logit model that allows for heterogeneous preferences across students. The final subsection covers the findings with respect to the influence of peers.

Main Findings

Table 4 presents estimates from the conditional logit model without school fixed effects separately for each secondary school track. The top part of the table reports the estimates of the impact of school quality scores on students' choices. For students with a mixed advice (e.g., senior general secondary/pre-university) the quality scores of both tracks matter and estimates of the impact of two quality scores are reported, where the first estimate pertains to the highest of the two tracks.[19] Neutral quality scores are the reference category. As described before, the quality scores of 2009 are reported separately because only the underlying scores were published by *Trouw* in that year. The estimated coefficients for the quality scores provide no support for the hypothesis that

19. Consider, for example, the estimates for a negative quality score for students with a mixed senior general secondary/pre-university advice. The estimate of −0.380 is the effect of a negative quality score of the pre-university track in a school, and the estimate of 0.304 is the effect of a negative quality score of the senior general secondary track in a school.

Table 4. Estimates from Conditional Logit Models without School Fixed Effects

	Pre-university	Senior General/ Pre-university	Senior General	Pre-vocational IV/ Senior General
Negative quality	0.371***	−0.380***	0.106	0.001
	(0.111)	(0.127)	(0.084)	(0.101)
		0.304***		−0.307***
		(0.108)		(0.109)
Positive quality	0.477***	0.474***	0.122	−0.055
	(0.093)	(0.105)	(0.077)	(0.124)
		0.046		−0.321**
		(0.114)		(0.143)
Missing quality scores	−0.414***	−0.377**	−0.775***	−0.602
	(0.136)	(0.181)	(0.169)	(0.413)
		−0.705**		0.363
		(0.298)		(0.393)
Negative quality 2009	−0.011	0.696**	−0.058	0.108
	(0.229)	(0.268)	(0.383)	(0.459)
		−0.503		0.384
		(0.420)		(0.289)
Positive quality 2009	−0.293**	0.062	−0.274**	0.182
	(0.119)	(0.188)	(0.107)	(0.159)
		−0.100		0.181
		(0.162)		(0.208)
Missing quality scores 2009	−0.118	0.549	−0.006	−0.222
	(0.270)	(0.590)	(0.173)	(0.263)
		−0.768		
		(0.484)		
Distance	−0.535***	−0.552***	−0.520***	−0.553***
	(0.020)	(0.020)	(0.021)	(0.024)
Lottery	0.335***	−0.078	0.035	0.462***
	(0.067)	(0.064)	(0.067)	(0.137)
N students	4,374	2,819	3,133	2,028
N schools	27	21	23	18
Log likelihood	10,375.3	−5,991.1	−7,068.0	−3,703.4
Pseudo R^2	0.15	0.24	0.24	0.31
	Pre-vocational IV	**Pre-vocational II**	**Pre-vocational I/II**	**Pre-vocational I**
Negative quality	−0.222***	0.080	−0.115	−0.355
	(0.085)	(0.123)	(0.212)	(0.555)
			−1.037	
			(0.735)	
Positive quality	−0.189*	0.034	0.171	0.069
	(0.098)	(0.089)	(0.131)	(0.105)
			0.190	
			(0.169)	
Missing quality scores	−0.260**	0.160	−0.382	0.058
	(0.129)	(0.145)	(0.556)	(0.139)
			0.380	
			(0.587)	
Negative quality 2009	0.354**	−0.605	−0.763	0.385
	(0.169)	(0.482)	(0.578)	(0.404)
Positive quality 2009	−0.393**	−0.317*	−0.657**	0.238
	(0.159)	(0.170)	(0.259)	(0.218)
			0.039	
			(0.293)	
Missing quality scores 2009	−0.567***	−0.568**	−0.538	−0.375
	(0.191)	(0.258)	(0.650)	(0.377)
			−0.257	
			(0.777)	

Table 4. Continued.

	Pre-vocational IV	Pre-vocational II	Pre-vocational I/II	Pre-vocational I
Distance	−0.546***	−0.482***	−0.531***	−0.582***
	(0.022)	(0.017)	(0.023)	(0.017)
Lottery	0.633***	0.113	0.219	0.049
	(0.134)	(0.338)	(0.334)	(0.183)
N students	2,881	2,130	1,039	2,713
N schools	23	22	21	22
Log likelihood	−5,446.1	−4,365.7	−2,059.1	−5,400.9
Pseudo R^2	0.32	0.29	0.31	0.33

Notes: Standard errors are clustered at the primary school level and reported in parentheses. Included student level controls are gender, Dutch, missing ethnicity, test score, missing test score, student weight, and missing student weight.

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

school choice is systematically influenced by published quality scores. Two coefficients for a positive quality score are significantly positive and three coefficients for a negative quality score are significantly negative. At the same time, two coefficients for a positive quality score are significantly negative, and two coefficients for a negative quality score are significantly positive. Just focusing on the sign of the coefficients, it turns out that fourteen of twenty-two coefficients have a sign that is consistent with the hypothesis that a positive (negative) quality score makes it more (less) likely that a school is chosen. We cannot reject that this number of correct signs is generated by chance ($p = 0.86$). By and large we can therefore not reject the hypothesis that published quality scores have no systematic impact on students' school choices.

Distance turns out to be a consistent and strong predictor of school choice. This is in accordance with results from other studies (e.g., Hastings, Kane, and Staiger 2009; Koning and Van der Wiel 2013). One way to interpret the size of the coefficients is in terms of odds ratios. The logit coefficient for distance to pre-university schools in table 4 can be expressed as an odds ratio by taking the exponent: $\exp(-0.535) = 0.59$. This coefficient indicates that students with a pre-university advice are 41 percent less likely to choose a school that is one kilometer farther away, as compared to an identical school one kilometer closer. The odds ratios for the other school tracks indicate that students are 38 percent to 44 percent less likely to choose a school one kilometer more distant. Alternative specifications for distance also consistently indicate that students prefer schools closer to their home.[20]

The coefficients for the lottery dummy are positive and significant in three of eight cases, but they have the "wrong" sign in the sense that they suggest that students are more likely to choose a school that had a lottery in the previous year. This result is

20. Specifically, we have estimated models using the log of distance, adding a quadratic term, using bins, distance rank, and a closest school indicator. Compared with the lowest bin and closest school, all distance bins and ranks have a negative and significant coefficient. The closest school indicator is positive and significant when not including distance, but becomes negative and significant in three (lower) tracks when including distance, implying that after taking into account distance, students do not prefer the school closest to their house. When adding a quadratic term, the linear term remains negative and significant. Squared distance is always positive and significant in four out of eight school tracks, implying that the negative value put on distance is nonlinear and shaped like a parabola that opens upward.

robust for different specifications of the lottery and for using different samples—we obtain similar results when (1) we use the percentage of students losing the lottery in the previous year instead of the binary indicator; (2) we account for the school making an extra class in the year of school choice; and (3) we drop students who have priority for admittance to their school of first choice. Results are reported in table A.1 in Appendix A, which is available in a separate online appendix that can be accessed on *Education Finance and Policy*'s Web site at https://www.mitpressjournals.org/doi/suppl/10.1162/edfp_a_00237.

Table 5 presents estimates from the conditional logit model, including secondary school fixed effects. This alters the interpretation of the quality scores—we no longer use the absolute scores of a secondary school but study how school choices respond to changes in school quality scores. The estimation results are rather similar to those in the previous table without school fixed effects. For the quality scores, fewer coefficients are significant and some coefficients have changed their sign. As before, however, we cannot reject the hypothesis that quality scores have no systematic impact on students' school choices. The estimated effects for distance are also very similar to those in the previous table. The inclusion of school fixed effects does, however, change the estimates of the coefficients of the lottery indicator. Now, five of eight coefficients have the expected negative sign, with two coefficients being significant at the 10 percent level and one coefficient being significant at the 5 percent level. In the models with school fixed effects, schools are compared with themselves in years in which there is no lottery. Therefore, these results are more likely to capture a pure lottery effect. There is thus some evidence that students avoid schools that conducted a lottery in the previous year. This finding carries over to different specifications of the lottery (see table A.1 in online Appendix A). The odds ratios for the significant coefficients range from 0.51 to 0.89, implying that students are 11 percent to 49 percent less likely to choose a school when it conducted an admission lottery in the previous year. Another way to interpret these coefficients is in terms of willingness to travel: $Wtt = \beta_{lottery}/\beta_{distance}$. The results imply that students are willing to travel 0.22 to 1.41 kilometers to avoid a school that conducted an admission lottery in the previous year.

We have also investigated whether preferences differ over time. For instance, as the centralized system started in 2005, it could be that students responded differently to lotteries in 2006 than to lotteries in 2009. We have therefore interacted the lottery dummy and quality indicators with year dummies. The results are reported in table B.1 in online Appendix B. For the quality variables in the specifications without fixed effects, the results show no consistent pattern of increasing or decreasing preferences for school quality scores. For the lottery, the results in the models without fixed effects slightly shift over the years. For 2006, four coefficients are negative and insignificant, two are positive and insignificant, and two are positive and significant. For 2009, only one coefficient is negative and significant, three are positive and insignificant, and four are positive and significant. Thus, there is some evidence that parental preference for schools that conducted school admission lotteries in the previous year increases over time.[21]

21. For some track/year combinations the lottery indicator cannot be estimated because no single school in that track conducted a lottery in that year. For senior general/pre-university there were no schools conducting

Table 5. Estimates from Conditional Logit Models with School Fixed Effects

	Pre-university	Senior General/ Pre-university	Senior General	Pre-vocational IV/ Senior General
Negative quality	0.288**	−0.097	0.071	0.022
	(0.123)	(0.146)	(0.088)	(0.101)
		0.174*		−0.079
		(0.105)		(0.116)
Positive quality	0.397***	0.284***	0.006	0.014
	(0.102)	(0.108)	(0.076)	(0.129)
		0.065		−0.321**
		(0.114)		(0.153)
Missing quality scores	−0.252	−0.092	−0.069	−0.151
	(0.156)	(0.189)	(0.236)	(0.433)
		−0.225		0.168
		(0.332)		(0.409)
Negative quality 2009	0.321	0.236	0.634	−0.265
	(0.248)	(0.280)	(0.466)	(0.493)
		0.439		0.523*
		(0.492)		(0.308)
Positive quality 2009	−0.350***	−0.032	−0.210*	0.100
	(0.125)	(0.191)	(0.113)	(0.171)
		−0.069		−0.119
		(0.166)		(0.248)
Missing quality scores 2009	0.442	0.349	0.132	−0.334
	(0.378)	(0.630)	(0.182)	(0.274)
		−0.419		
		(0.473)		
Distance	−0.588***	−0.609***	−0.555***	−0.576***
	(0.023)	(0.023)	(0.021)	(0.024)
Lottery	0.066	−0.103	−0.113*	0.003
	(0.064)	(0.069)	(0.065)	(0.131)
N students	4,374	2,819	3,133	2,028
N schools	27	21	23	18
Log likelihood	−10,112.1	−5,891.0	−6,916.4	−3,557.8
Pseudo R^2	0.17	0.25	0.25	0.34
	Pre-vocational IV	**Pre-vocational II**	**Pre-vocational I/II**	**Pre-vocational I**
Negative quality	−0.153*	0.195	0.082	1.698***
	(0.091)	(0.121)	(0.212)	(0.646)
			−0.651	
			(0.976)	
Positive quality	−0.174*	−0.082	0.014	0.052
	(0.102)	(0.094)	(0.141)	(0.111)
			0.167	
			(0.195)	
Missing quality scores	0.022	−0.028	−0.367	0.027
	(0.163)	(0.161)	(0.555)	(0.145)
			0.109	
			(0.612)	
Negative quality 2009	0.578***	0.165	−0.861	0.419
	(0.189)	(0.520)	(0.598)	(0.408)
Positive quality 2009	−0.528***	−0.296*	−0.360	0.217
	(0.172)	(0.163)	(0.283)	(0.220)
			−0.215	
			(0.338)	
Missing quality scores 2009	−0.563***	−0.329	−0.085	−0.438
	(0.196)	(0.264)	(0.676)	(0.387)
			−0.874	
			(0.832)	

Table 5. Continued.

	Pre-vocational IV	Pre-vocational II	Pre-vocational I/II	Pre-vocational I
Distance	−0.557*** (0.022)	−0.495*** (0.018)	−0.552*** (0.024)	−0.588*** (0.017)
Lottery	−0.291* (0.175)	−0.678** (0.287)	−0.151 (0.320)	0.026 (0.187)
N students	2,881	2,130	1,039	2,713
N schools	23	22	21	22
Log likelihood	−5,272.8	−4,240.6	−2,003.0	−5,344.0
Pseudo R^2	0.34	0.31	0.33	0.33

Notes: Standard errors are clustered at the primary school level and reported in parentheses. Included student level controls are gender, Dutch, missing ethnicity, test score, missing test score, student weight, and missing student weight.

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

For the models with fixed effects (results not shown) most coefficients are insignificant and the signs are inconsistent.[22]

Alternative Measures of Secondary School Quality

In the previous subsection, we concluded that school quality scores do not clearly predict school choice. A potential explanation is that parents value the underlying quality measures described in section 3 over the composite quality measure. Therefore, in specification 1 in table 6, the underlying quality measures are added to the models in table 4. Descriptive statistics about these underlying quality measures are reported in table C.1 in online Appendix C. It turns out that exam grades are positively related to school choice: nine of eleven coefficients are positive, of which five are significant (at the 10 percent level). The significant coefficients are mainly at the higher school tracks. The willingness-to-travel ratios imply that students (ceteris paribus) are willing to travel up to 1.41 kilometers more to attend a school with a one-point higher average exam grade (or 0.48 kilometers more to attend a school with a 1 standard deviation (SD) higher exam grade). The percentage of students getting a degree without delay in the higher classes seems to be negatively related to school choice: five of eleven coefficients are negative, of which four are significant. Willingness to travel ratios indicate that students are willing to travel up to 0.39 kilometers to attend a school with a 1 SD lower percentage of students getting a degree without delay in the higher classes. This could be because more delay in the higher grades is related to better exam grades. Grade progression in the lower grades does not predict school choice. When adding school fixed effects to

lotteries in 2008. For the lowest three tracks, there was only one school conducting a lottery, which was also the only school that missed quality scores for these tracks in 2008.

22. As a falsification test, we have also used future (changes in) quality and lottery indicators on current year decisions. For the quality variables, the majority of coefficients is insignificant, although the number of coefficients with an unexpected and significant sign decreases, whereas the number of coefficients with an expected and significant sign slightly increases. For the future lottery indicators, the coefficients for the models without fixed effects are positive and significant for seven of eight school tracks. For the models with fixed effects, the coefficients are also more often positive, but generally insignificant. However, as the future lottery indicator is an indicator for having a lottery in the year of school choice (instead of a lottery in the previous year), and schools only have a lottery when they are oversubscribed for that school track, it makes sense that this lottery indicator is positively related to number of applicants for a school.

Table 6. Models Including Underlying School Characteristics (without school fixed effects)

	Pre-university	Senior general/ Pre-university	Senior general	Pre-vocational IV/ Senior General
(1) Index grade progression in lower grades	0.008* (0.004)	0.000 (0.005)	−0.002 (0.004)	−0.004 (0.004)
Exam grades	0.761*** (0.157)	0.069 (0.214)	0.322** (0.159)	0.168 (0.196)
		0.490** (0.200)		0.516* (0.299)
Degrees without delay	0.005 (0.004)	0.004 (0.004)	−0.010** (0.004)	−0.003 (0.006)
		−0.012** (0.005)		0.003 (0.006)
N students	4,374	2,819	3,133	2,028
(2) Index grade progression in lower grades	0.004 (0.005)	0.000 (0.005)	−0.001 (0.004)	−0.005 (0.004)
Exam grades	0.612*** (0.155)	0.074 (0.213)	0.244 (0.166)	−0.112 (0.209)
		0.407** (0.208)		0.705** (0.314)
Degrees without delay	0.005 (0.004)	0.004 (0.004)	−0.010** (0.004)	−0.004 (0.006)
		−0.012** (0.005)		0.013** (0.006)
Primary school test scores of inflow	0.060*** (0.015)	0.032 (0.022)	0.045*** (0.017)	0.049** (0.023)
N students	4,374	2,819	3,133	2,028

	Pre-vocational IV	Pre-vocational II	Pre-vocational I/II	Pre-vocational I
(1) Index grade progression in lower grades	−0.006* (0.003)	0.002 (0.006)	−0.005 (0.010)	0.002 (0.004)
Exam grades	−0.063 (0.196)	0.367* (0.191)	−0.107 (0.316)	0.105 (0.160)
			0.310 (0.271)	
Degrees without delay	0.003 (0.005)	−0.010* (0.005)	−0.017** (0.007)	0.003 (0.006)
			0.001 (0.011)	
N students	2,881	2,130	1,039	2,713
(2) Index grade progression in lower grades	−0.005 (0.003)	0.005 (0.006)	−0.005 (0.009)	0.003 (0.004)
Exam grades	0.040 (0.200)	0.461** (0.178)	−0.234 (0.317)	0.007 (0.163)
			0.300 (0.271)	
Degrees without delay	0.010** (0.005)	−0.008 (0.005)	−0.013** (0.007)	0.007 (0.006)
			−0.006 (0.011)	
Primary school test scores of inflow	0.031** (0.014)	0.027 (0.018)	−0.009 (0.016)	−0.005 (0.012)
N students	2,881	2,126	1,022	2,643

Notes: Standard errors are clustered at the primary school level and reported in parentheses. Models also include indicators for school quality, distance, and a lottery indicator as in table 4. Included student level controls are gender, Dutch, missing ethnicity, test score, missing test score, student weight, and missing student weight.

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

specification 1 in table 6 (results not shown) the coefficients become nonsignificant and inconsistent in sign, indicating that parents and students do not respond to changes in the underlying quality measures.[23]

The models in table 6 also include the composite quality measures,[24] distance, and the lottery indicator. The results for distance and the lottery indicator do not change when including the underlying quality measures. For the composite quality measures, the pattern of inconsistent signs remains and fewer coefficients are significant. Out of the three significant coefficients for positive quality scores, two have a positive sign and one a negative sign. For negative quality scores, there are two significant coefficients with a positive sign and two with a negative sign. For the models with school fixed effects, the composite quality measures remain insignificant.

To investigate whether the positive preference for schools with higher exam grades is driven by a preference for a higher-achieving student population, specification 2 in table 6 adds information about the (demeaned) average primary school test scores of the current inflow to the models in specification 1. We computed the average citoscore of the current inflow for each specific school track advice in a school in a certain year.[25] We only compute models without school fixed effects, because a model with school fixed effects would assume that parents respond to (small) changes in average primary school test scores in the year in which the student applies. This information is not available to parents.

The results indicate that students and parents also prefer schools with higher average primary school test scores in the advised school track. Six of eight coefficients are positive, of which four are significant. The two negative (n.s.) coefficients are at the lowest tracks. Willingness-to-travel ratios imply that students are willing to travel up to 0.54 kilometers to attend a school with a 1 SD higher average of primary school test scores. When including the primary school test scores of the current inflow, exam grades remain a positive predictor of school choice: nine of eleven coefficients are positive of which four are significant. Degree without delay in the higher grades becomes less consistent: six coefficients are negative, of which three are statistically significant, and five coefficients are positive, of which two are statistically significant. Grade progression in the lower grades remains unrelated to school choice. The conclusions for distance, the lottery indicator, and the composite quality measures do not change.

Heterogeneous Preferences

Table 7 presents estimation results of mixed logit models. In addition to the mean of the preference weights for school characteristics, the mixed logit model also produces estimates of their standard deviation. This allows us to see to what extent the preferences for secondary school characteristics vary across students. Again, the estimates indicate

23. Including different specifications of enrollment (log of enrollment, enrollment, lagged enrollment, log of lagged enrollment), indicates that (lagged) enrollment is positively and significantly related to school choice. On average, students and parents seem to prefer larger schools. When adding school fixed effects, the coefficients are generally insignificant and inconsistent in sign.
24. Removing the composite measures of school quality yields similar results.
25. Almost 14 percent of all students miss information on their citoscore. In computing the average citoscores, we do not take these students into account. This leads to losing some observations, as twenty-three school–year–track combinations only have missing citoscores. The combinations that have no citoscores are mainly lower tracks and early years.

Table 7. Estimates from Mixed Logit Models

	Pre-university		Senior General/ Pre-university		Senior General	
	Mean	SD	Mean	SD	Mean	SD
Negative quality	−0.105	1.445^{***}	−0.196	0.603	−0.006	0.660
	(0.184)	(0.301)	(0.193)	(0.450)	(0.225)	(0.753)
			−0.003	1.025^{**}		
			(0.241)	(0.517)		
Positive quality	0.458^{***}	0.889^{**}	0.299^{***}	0.854^{**}	0.026	0.418
	(0.118)	(0.348)	(0.116)	(0.352)	(0.081)	(0.328)
			0.090	0.769^{**}		
			(0.134)	(0.343)		
Missing quality scores	-1.122^{*}	1.851^{**}	-1.275^{**}	2.486^{***}	-2.431^{**}	3.102^{***}
	(0.617)	(0.726)	(0.513)	(0.523)	(0.976)	(0.777)
			-5.073^{*}	5.077^{**}		
			(2.635)	(1.970)		
Negative quality 2009	0.314	0.553	0.023	1.656^{***}	0.139	1.973^{***}
	(0.349)	(0.480)	(0.395)	(0.517)	(0.747)	(0.575)
			0.453	0.898		
			(0.648)	(0.554)		
Positive quality 2009	−0.192	1.164^{**}	0.087	0.371	-0.218^{*}	0.394
	(0.199)	(0.585)	(0.249)	(0.933)	(0.121)	(0.365)
			−0.076	0.495		
			(0.195)	(0.621)		
Missing quality scores 2009	−1.695	2.599^{**}	1.739^{*}	0.500	0.484	0.313
	(1.170)	(1.053)	(0.925)	(0.599)	(0.331)	(0.899)
			-4.547^{**}	4.680^{***}		
			(1.995)	(1.679)		
Distance	-0.660^{***}	0.163^{**}	-0.738^{***}	0.174^{***}	-0.649^{***}	0.251^{***}
	(0.031)	(0.071)	(0.035)	(0.059)	(0.031)	(0.042)
Lottery	0.028	0.450	−0.115	0.287	−0.107	0.035
	(0.086)	(0.399)	(0.073)	(0.246)	(0.068)	(0.126)
N students	4,374		2,819		3,133	
N schools	27		21		23	
Log likelihood	10,197.6		−5,936.0		−7,007.5	
Pseudo R^2	0.16		0.25		0.24	

	Pre-vocational IV/ Senior General		Pre-vocational IV		Pre-vocational II	
	Mean	SD	Mean	SD	Mean	SD
Negative quality	0.037	0.027	−0.176	0.387	0.181	0.086
	(0.109)	(0.176)	(0.109)	(0.307)	(0.126)	(0.242)
	−0.128	0.020				
	(0.127)	(0.200)				
Positive quality	−0.095	0.899^{**}	-0.184^{*}	0.004	-0.272^{*}	1.002^{***}
	(0.178)	(0.413)	(0.106)	(0.113)	(0.144)	(0.351)
	-0.351^{**}	0.156				
	(0.163)	(0.182)				
Missing quality scores	−0.133	0.550	−0.122	0.786	0.005	0.266
	(0.473)	(0.443)	(0.277)	(0.496)	(0.180)	(0.195)
	−0.356	1.520^{***}				
	(0.565)	(0.552)				
Negative quality 2009	−0.628	1.253^{***}	0.214	1.645^{**}	−2.138	2.512
	(0.585)	(0.404)	(0.434)	(0.694)	(4.150)	(2.481)
	0.643^{*}	0.215				
	(0.358)	(0.352)				
Positive quality 2009	0.134	0.464	-0.718^{*}	1.075	−0.706	1.808
	(0.247)	(1.359)	(0.379)	(0.928)	(0.520)	(1.247)
	−0.165	0.893				
	(0.303)	(0.684)				

Table 7. Continued.

	Pre-vocational IV/ Senior General		Pre-vocational IV		Pre-vocational II	
	Mean	SD	Mean	SD	Mean	SD
Missing quality scores 2009	−0.873	1.364	−1.077	1.259	−0.497	0.349
	(0.828)	(1.222)	(0.738)	(1.168)	(0.318)	(0.539)
Distance	−0.671***	0.232***	−0.634***	0.192***	−0.558***	0.152***
	(0.036)	(0.032)	(0.025)	(0.033)	(0.026)	(0.035)
Lottery	−0.021	0.282	−0.393	0.968	−1.804***	1.958***
	(0.146)	(0.289)	(0.285)	(0.589)	(0.666)	(0.522)
N students	2,028		2,881		2,130	
N schools	18		23		22	
Log Likelihood	−3,649.4		−5,358.8		−4,227.0	
Pseudo R^2	0.32		0.33		0.31	

	Pre-vocational I/II		Pre-vocational I	
	Mean	SD	Mean	SD
Negative quality	0.011	0.690	−0.272	2.955***
	(0.336)	(0.945)	(1.387)	(0.615)
	−3.989	3.570		
	(4.330)	(3.042)		
Positive quality	−0.089	0.777	0.050	0.078
	(0.218)	(0.690)	(0.113)	(0.241)
	0.160	0.334**		
	(0.206)	(0.142)		
Missing quality scores	−0.364	0.232	0.012	0.087
	(0.612)	(0.611)	(0.155)	(0.506)
	−0.506	1.410**		
	(0.826)	(0.580)		
Negative quality 2009	−3.000	2.810*	0.244	1.333**
	(2.028)	(1.674)	(0.664)	(0.676)
Positive quality 2009	−0.575	1.138*	0.212	2.017***
	(0.431)	(0.658)	(0.234)	(0.712)
	−0.232	0.240		
	(0.380)	(0.319)		
Missing quality scores 2009	−0.593	0.599	−1.373*	0.158
	(0.754)	(0.414)	(0.778)	(0.487)
	−0.862	0.995		
	(1.000)	(0.737)		
Distance	−0.653***	0.178***	−0.615***	0.054
	(0.044)	(0.055)	(0.018)	(0.035)
Lottery	−0.852	1.946***	−0.017	0.258
	(0.690)	(0.668)	(0.202)	(0.677)
N students	1,039		2,713	
N schools	21		22	
Log likelihood	−1,984.8		−5,324.8	
Pseudo R^2	0.34		0.33	

Notes: Standard errors are clustered at the primary school level and reported in parentheses. Included student level controls are gender, Dutch, missing ethnicity, test score, missing test score, student weight, and missing student weight for pre-vocational I, I/II, and II. For the pre-vocational IV to pre-university tracks, the missing indicators could not be included for computational reasons. When using Wald tests to compare the coefficients for the variables in this table in conditional logit models with and without the missing indicators, it turns out that the null hypothesis of equal coefficients cannot be rejected for the pre-vocational IV to pre-university tracks.

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

that students have a lower preference for schools that are farther from their home address. At the same time, however, we see the aversion for distance varies significantly across students. Regarding school quality, we again see inconsistent coefficients over the different school tracks; we still cannot reject the hypothesis that composite quality scores have no systematic impact on school choice. Some of the standard deviations for the quality scores come out significantly, indicating that the preferences for certain quality scores vary across students. Regarding the lottery indicator, it can be seen that only the mean coefficient for pre-vocational II comes out significantly. For the other school tracks, the coefficients are mainly negative but not significantly different from zero.

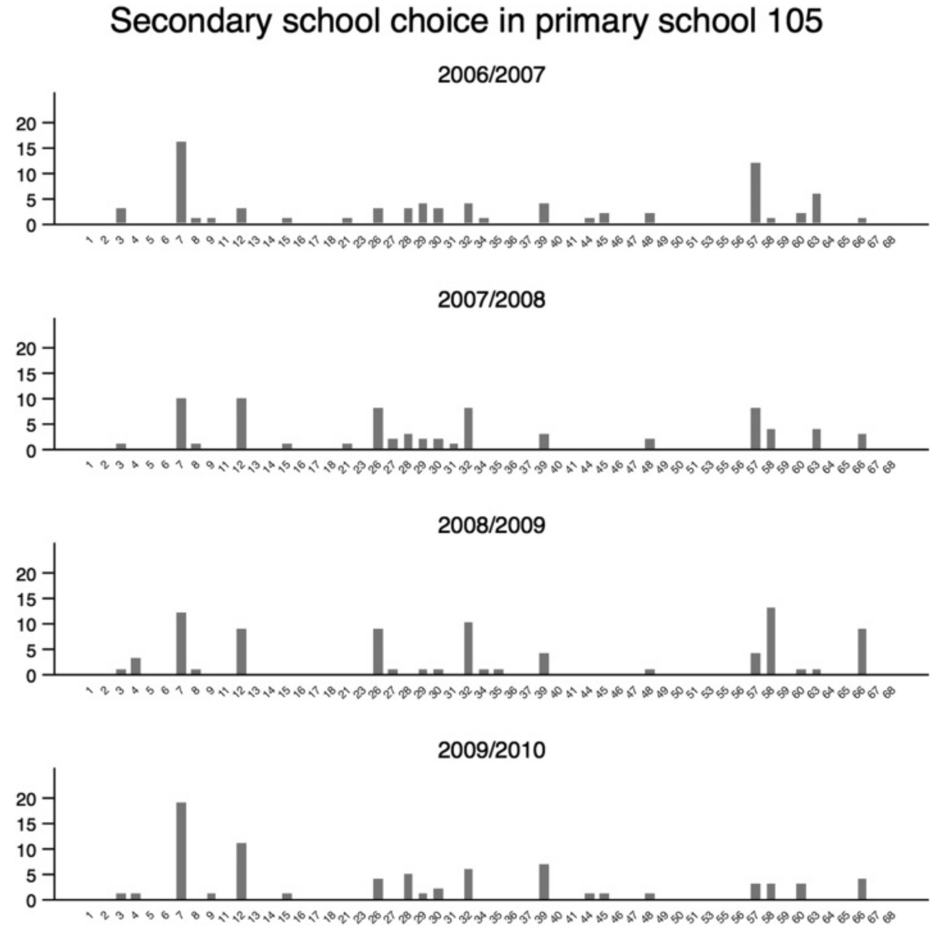
The standard deviations in the mixed logit models only describe whether secondary school preferences vary across families. They do not provide information on which characteristics are valued by different types of families. Therefore, we have also interacted the quality, distance, and lottery variables with the disadvantaged student indicator using conditional logit models. The results in table D.1 in online Appendix D show some interesting patterns. As before, distance is negatively valued. The interaction of distance and student weight is also negative and significant, implying that the negative preference for distance is stronger for disadvantaged students. The interaction coefficient of lottery and weight, on the other hand, is mainly negative and significant in five school tracks, implying that parents of disadvantaged students do not have a positive preference for schools that conducted school admission lotteries in the previous year. For the quality variables, the main coefficients remain very similar. Ten of the thirty-three interactions of the quality and weight variables are significant. All significant coefficients have the opposite sign of the corresponding main effect, which implies that for disadvantaged students, the preferences for positive or negative quality indicators are close to zero.

For similar models with school fixed effects (reported in table D.2 in online Appendix D) home-to-school distance and distance interacted with weight again have negative and significant coefficients, implying that disadvantaged students put a significantly stronger negative value on the distance to school. For the admission lotteries, nearly all coefficients and the interactions with weight are insignificant. For the quality indicators, the pattern that changes in quality scores are not consistently related to choice remains, and none of the interactions with student weight is significant.[26]

Peer Effects

Figure 2 depicts for one single primary school from year-to-year how the students spread out across the available secondary schools. It shows that some secondary schools (such as schools 7, 12, 32, and 57) are in general more popular than other schools for students in this primary school. But we also see some strong fluctuations. For example, school 58 is not a popular destination in 2006–07 and 2007–08, is the most popular destination in 2008–09, and is back to its original level in 2009–10. The same is true

26. We have also inquired whether students are more likely to apply to a school that offers multiple tracks. This analysis is limited to students who have schools with single and multiple tracks in their choice set. The results reveal that students at the pre-university level have a preference for single-track schools and this preference is stronger for students with higher citoscores. Students at the pre-vocational (II and IV) level instead prefer multiple-track schools.



Note: Frequency in the number of students choosing each school is depicted on the vertical axis, the horizontal axis shows secondary schools.

Figure 2. Secondary School Choice in One Primary School

for school 66. Similar graphs for other primary schools reveal comparable patterns: Students who leave the same primary school in the same year tend to choose the same secondary schools. To some extent this can be attributed to a general popularity of a specific secondary school among the students from certain primary schools. But there is also idiosyncratic clustering at—apparently random—destination schools.

Table 8 reports results from conditional and mixed logit models of school choice that also include the shares of classmates and predicted shares of classmates who choose each secondary school as variables in x_{its}. The table only reports the coefficients of these two variables. Consistent with the pattern we observed in figure 2, all coefficients are significantly positive. Not surprisingly, we see a positive and significant coefficient for the predicted share of peers in the primary school. This coefficient captures the general popularity of secondary schools in the primary school. On top of the predicted share of peers, however, we see that the actual share of peers has a positive and significant coefficient as well. This indicates that when a higher share of the primary school peers

Table 8. Estimates of Peer Coefficients: Various Logit Models

	Pre-university	Senior General/ Pre-university	Senior General	Pre-vocational IV/ Senior General	Pre-vocational IV	Pre-vocational II	Pre-vocational I/II	Pre-vocational I
				Conditional Logit Without School Fixed Effects				
Predicted share of peers	3.706***	3.063***	4.643***	3.062***	3.625***	2.025***	5.573***	3.642***
	(0.586)	(0.684)	(0.595)	(0.653)	(0.625)	(0.723)	(1.192)	(0.642)
Share of peers	3.464***	4.259***	3.013***	5.127***	3.669***	4.554***	2.743***	3.131***
	(0.485)	(0.539)	(0.465)	(0.566)	(0.509)	(0.663)	(0.988)	(0.535)
				Conditional Logit With School Fixed Effects				
Predicted share of peers	4.135***	3.537***	4.797***	2.659***	3.382***	1.618**	5.108***	3.530***
	(0.564)	(0.681)	(0.593)	(0.640)	(0.621)	(0.707)	(1.233)	(0.635)
Share of peers	3.336***	4.282***	2.916***	5.131***	3.635***	4.506***	2.741***	3.135***
	(0.477)	(0.530)	(0.460)	(0.568)	(0.500)	(0.650)	(1.000)	(0.535)
				Mixed Logit				
Predicted share of peers	4.513***	4.022***	5.957***	4.417***	4.970***	2.786***	7.191***	3.918***
	(0.644)	(0.785)	(0.699)	(0.862)	(0.760)	(0.838)	(1.705)	(0.783)
Share of peers	3.720***	5.721***	3.586***	6.117***	4.318***	5.654***	4.529***	3.808***
	(0.564)	(0.649)	(0.564)	(0.742)	(0.594)	(0.831)	(1.305)	(0.718)
SD (predicted share of peers	1.526	−0.012	2.897	−5.846***	2.063	3.545***	1.033	−0.820
	(1.699)	(3.647)	(2.954)	(0.965)	(2.792)	(1.122)	(5.083)	(2.728)
SD (share of peers)	−1.489	3.965***	−3.399	−1.657	−3.886**	4.977***	7.189***	−3.925***
	(1.174)	(0.750)	(2.294)	(2.134)	(1.717)	(0.779)	(1.730)	(0.810)
N students	4,374	2,819	3,133	2,028	2,881	2,130	1,039	2,713
N schools	27	21	23	18	23	22	21	22

Notes: Standard errors are clustered at the primary school level and reported in parentheses. All models also include indicators for school quality, lottery, and distance as in table 4. Included student level controls are gender, Dutch, missing ethnicity, test score, missing test score, student weight, and missing student weight. For the pre-vocational IV to pre-university tracks, the missing indicators could not be included in the mixed logit specification for computational reasons. When using Wald tests to compare the coefficients for the peer characteristics and the indicators for quality, lottery, and distance in conditional logit models with and without the missing indicators, it turns out that the null hypothesis of equal coefficients cannot be rejected for these levels.

$^{***}p < 0.01$; $^{**}p < 0.05$.

chooses for a certain secondary school, the student himself is more likely to pick this school as well. We find this result for all specifications and also when we correct for secondary school times year fixed effects, which makes it unlikely that this result is driven by sudden changes in the attractiveness of secondary schools. The school times year fixed effects results are reported in online Appendix E.

Because of the reflection problem and correlated unobservables, these coefficients cannot be interpreted as causal effects of the school choices of classmates. The coefficients do not inform us about the social multiplier. Does the choice of one student influence all others, or is everyone affected by everyone? The coefficients also do not exclude that the correlated school choices are caused by a third unobserved factor, such as active recruitment by a specific secondary school in certain primary schools in a particular year. The results in table 8 are therefore no proof of the importance of peer effects—they are, however, consistent with it.

To get an idea on the magnitude of the peer coefficients, we can compute willingness to travel coefficients if we interpret the peer coefficients as the actual preference to attend the same secondary school as primary school classmates do: $Wtt = \beta_{peers}/\beta_{distance}$. For pre-university schools, we get a willingness to travel of $3.464/-0.371 = -9.334$, indicating that students are willing to travel 9.3 kilometers for a 100 percent increase in the percentage of peers, or 943 meters for a 10 percent increase in the percentage of peers. Given that the average class size in primary schools is about twenty-five, a 10 percent increase in the percentage of peers means an increase of two or three classmates. For the other school tracks, the willingness to travel for a 10 percent increase in the percentage of peers varies from 674 to 1,429 meters.

6. CONCLUSIONS

We investigate which secondary schools students choose when school choice is not restricted by catchment areas, high tuition fees, or large distances. This provides a good opportunity to examine whether school choices depend on school quality, which is a key assumption of advocates of school choice and competition. Our findings lend some support for this assumption. Students prefer schools where graduating students have higher exam grades and schools that attract incoming students with higher scores on the exit test of primary education. This is especially true for students who are advised into the higher academic tracks. The result of this is that good students cluster together in good schools, leaving the not-so-good students behind in the not-so-good schools.

Our results reject the null-hypothesis of no peer effects in school choice. When a larger share of a student's primary school peers chooses for a certain secondary school, the student is more likely to pick that school as well. This is even true when we correct for the systematic component in the popularity of the specific secondary school among the students in the student's primary school.[27] Although this result does not prove the importance of peer effects in school choices, it is consistent with it. If students prefer to enroll in the same secondary school as their primary school classmates, school assignment mechanisms may facilitate this by permitting students to opt for combined lottery numbers. This may be particularly useful when the deferred acceptance mechanism is in place, especially with multiple-tie breaking when substantial shares of students will not be assigned to their most-preferred school.

ACKNOWLEDGMENTS

We gratefully acknowledge valuable comments from two anonymous referees. We also thank Adam Booij, Monique de Haan, Inge de Wolf, Pierre Koning, Geert Ridder, Jesse Rothstein, Adriaan Soetevent, Karen van der Wiel, and seminar participants at various places for useful comments and suggestions. We thank Herman Ozinga and Kees Waijenberg from the Amsterdam municipal office for supplying the data.

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