Are Parents Uninformed?

- The Impact of Additional School Quality Information on School Choice Behavior, School Placement and School Segregation

Dany Kessel[*]   Elisabet Olme[†]

November 28, 2017

PRELIMINARY AND INCOMPLETE
PLEASE DO NOT CITE!

Abstract

In this paper, we study the effects of providing information about school performance on school choice behavior and general equilibrium outcomes. In 2016, we conducted a randomized experiment in a Swedish municipality among the population of households with children about to start the 7th grade. Households selected to be part of the treatment received information about the absolute performance on standardized tests of all schools as well as performance adjusted for the student composition.

We find a reaction to the information on adjusted performance but not to the absolute performance. The share of households that choose a top performing school (in terms of adjusted performance) increase with 5.4 percentage points (corresponding to 16%) due to the treatment. This effect is concentrated to high-skilled households with a non-foreign background. Using simulations, we study at the effect on assignment. We find that the changes in choice behavior translates into changes in assignment but that the effects are muted due to institutional constraints. We do however also find that the initial changes in assignments are propagated though the system. Finally, we find that there is a significant reduction in school segregation in terms of foreign background.

JEL Codes: I21; I24; I28; D10; D82; D83
Keywords: Education; School Choice; School Segregation; Asymmetric and Private Information; Mechanism Design

---

[*]Stockholm University, dany.kessel@ne.su.se
[†]Stockholm University, elisabet.olme@ne.su.se

1   Introduction

During the last 30 years or so, there has been a rapid and large expansion of school choice programs all over the world (Musset, 2012). Due to this tens or potentially even hundreds of millions of households are now, each year, faced with the decision on where to send their children to school. On of the main motivations behind these school choice reforms has been to increase school performance through competition. The mechanism in mind is that when parents get the opportunity to choose schools they will choose higher preforming schools. Schools will therefore have to work to improve their performance in order to attract students. However, for this mechanism to work households needs to be able to observe performance. School performance can be hard to observe and as education is usually free and publicly funded households cannot rely on the price mechanism to guide them. The empirical evidence of the effects of introducing and/or expanding school choice programs on school performance and student outcomes is mixed.[1]. This begs the question, is lack of accessible information on school performance a reason for why we have not seen bigger gains? Can a simple information intervention affect school choice behavior so to strengthen the link described above and increase the competitive pressure on schools to improve performance?

Further, there are indications that introducing and/or expanding school choice opportunities increase school segregation in various dimensions.[2]. In line with this there is evidence that suggest that choice of school is dependent on the households socioeconomic characteristics and ethnic background. More disadvantaged households seem to choose schools of lower quality (usually measured by the school's performance on standardized tests) compared to more advantaged households.[3] One explanation for this could be that more advantaged households may face a lower cost of obtaining the relevant information compared to more disadvantaged households. Is is therefore also relevant to ask if providing households with accessible and reliable school performance information would reduce schools segregation.

To shred further light on these questions we conducted a large scale randomized controlled trial in the Swedish municipality of Linköping. Households with children about to start 7th grade in the autumn of 2016 were randomly selected into a treatment or control group. Prior to the school choice period, treated households received a letter containing information on the absolute and adjusted (for student composition) performance of the available schools. Performance was measured by the test scores on standardized test taken in 9th grade. Using this variation in access to school performance information, we study the effect on the school choice behavior of households and on the corresponding general equilibrium assignment of students.

It has previously been shown that information about school performance can affect choice behavior. Hastings & Weinstein (2008) uses two experiments that provided direct information about school test scores to lower-income families in the public-school choice plan in Charlotte-Mecklenburg Public School District. They find that receiving information on test scores significantly increases the fraction of parents choosing higher-performing schools. Gallego et al. (2012) reports results from a field experiment where report cards describing, among other things, the performance of the nearby schools was handed out to parents in school meetings in the final grade of preschools in poor urban areas in Chile. They also find economically and statistically significant effects on the test scores of the schools attended by students of the treated preschools.

Both these studies focus on disadvantaged households who's default schools are, in general, very low performing. Hence, it is not obvious how large the effects on school choice behavior in the general population and, by extension, the competitive pressure on schools to improve performance are. There are some semi-

---

[1]See Hoxby (2000), Cullen et al. (2006), Hsieh & Urquiola (2006), Lavy (2010), Böhlmark & Lindahl (2012), Lavy (2015), Abdulkadiroglu et al. (2017) and Epple et al. (2017)

[2]See Hsieh & Urquiola (2006), Bifulco & Ladd (2007) and Söderström & Uusitalo (2010)

[3]See Hastings et al. (2007) and Burgess et al. (2016)

structural work that examines if these results can be generalized to the population as a whole but the evidence is mixed. Hussain (2013) using school choice data from London, estimates a conditional logit model and find that households seem to internalize ratings from school inspections when making their choices. In contrast, using data from Amsterdam, Ruijs & Oosterbeek (2014) estimates a similar model but do not find that that published indicators of school quality are consistent predictors of school choice. Our design helps to answer this question. Our treatment group is a random sample of the entire population in a rather well-educated and rich Swedish municipality and hence we can estimate the effect over the entire distribution of households in a setting where differences between schools are not as large. This is our first main contribution.

Further, is has also been shown that information can affect the general equilibrium outcomes in a school system. The main outcome that has been examined has been the effect on enrollment. Hussain (2013) exploits variation in timing of the publicizing of the results of school inspections in the UK and finds that enrollment increases in schools with top rankings while it decreases in schools with bottom rankings. Koning & van der Wiel (2013) find that negative (positive) school-quality scores reported in a national newspaper decrease (increase) the number of first-year students who enroll in a school after the year of publication. In a recent study Andrabi *et al.* (2017) distributed report cards describing the local schools in a random sample of villages in Pakistan. They document effects not only on enrollment but also on other equilibrium outcomes such as average test scores and prices charged by private schools. Using high-quality Swedish register data we can study the effects on an additional general equilibrium outcome, namely segregation. Segregation is an outcome that, to our knowledge, has not been studied in this context. This is our second main contribution.[4]

Finally, previous studies have not had much to say about the link between the observed changes in choice behavior and the effects on general equilibrium outcomes. Figure 1 visualizes how we think about this link. We know that information on school performance can affect choice behavior. Given that a household alters their choice behavior their school placement can be affected. To what extent this will happened will depend on how choice behavior is altered but also on the demographic and geographic composition of the municipality as well as institutional factors, such as the allocation mechanism[5], the priority structures that determine who gets accepted to an oversubscribed school[6] and the amount of free capacity in the system. Calsamiglia & Guell (2014) points to Barcelona as an example where the design of the school choice system creates an environment where choice hardly matter at all. Further, in a system with limited capacity, changes in the assignment of some students can affect the assignment of others. If a new group of students select into an oversubscribed school they might displace students that would previously have been accepted to that school. Also, when student move to another school they free up capacity at their old school. If these schools are oversubscribed this will create room for other students to move in. The size of the effects will, again, depend on the overall choice patterns and the demographic, geographic and institutional factors described above. This potential reassignment of students can then, in turn, displace other students and/or free up capacity at other, potentially oversubscribed, schools. Hence, large effects on choice behavior can have small or no general equilibrium effects and small effects on choice behavior can have large general equilibrium effects.

Understanding how these mechanisms work can be very beneficial when constructing school choice systems in general and school performance information interventions in particular. We can, given our design, where we treat a random sample of the entire population, and our knowledge on how the allocation mechanism in
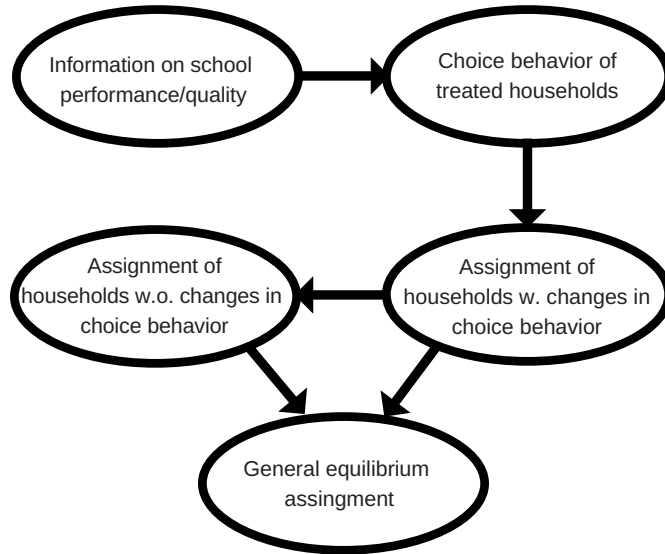
---

[4]We should note that the general equilibrium outcomes discussed here are short term. Initial effects could create secondary effects that we, as well as previous studies, can not capture given our designs. To capture more long term economic outcomes one would have to treat several cohorts in a random sample of municipalities/villages/towns.

[5]See Abdulkadiroglu & Sönmez (2003) for a detailed overview of the different allocations mechanisms and a discussion of their properties.

[6]See Fack & He (2015) for discussion on the role of priority structures

Figure 1: **From Information to General Equilibrium Outcomes**

Information on school performance/quality → Choice behavior of treated households

Assignment of households w.o. changes in choice behavior ← Assignment of households w. changes in choice behavior

Choice behavior of treated households → Assignment of households w. changes in choice behavior

Assignment of households w.o. changes in choice behavior → General equilibrium assingment

Assignment of households w. changes in choice behavior → General equilibrium assingment

Linköping functions, uncover these mechanisms. This is our third main contribution.[7]

We confirm that added information about school performance can affect choice behavior, not only among disadvantaged households in failing schools but also in a setting where households are, on average, well educated and where differences between schools are not especially large. We document a 5.4 percentage point increase in the probability of listing one of the top five performing[8] schools in the treatment group compared to the control group. 5.4 percentage points is not a large effect in absolute terms but in relative terms it corresponds to a 16 % increase in applications[9] to the top performing schools and a 8 % decrease in applications to the other schools. This suggest that information about school performance can increase the competitive pressure of school performance.

We do find that households mainly seem to react to the measure of adjusted performance. We find no indications of households choosing schools with higher absolute performance. This could be because that households have already internalized the absolute performance when making their choices, either because they actually have access to the information on absolute performance directly or because they had good enough proxy (student composition or reputation). It does however indicate that the changes in choice behavior we observe are not driven by households looking for a stronger (in terms of observable characteristics) peer group, but rather something else.

We also find that the effects on choice behavior are highly heterogeneous and almost entirely driven by high-skilled households with a non-foreign background. This is in contract to most previous studies that have found significant effects among more disadvantaged households. We also find that all the action is

---

[7]We should also note that there is a small literature that looks at the effect of information on school quality on house prices. This literature includes Figalo & Maurice (2004), Fiva & Kirkeboen (2011) and Imberman & Lovenheim (2016)

[8]In terms of adjusted performance.

[9]The baseline in control group is 33,3 percentage points

on the margin between the top ranked schools and the schools with a low share of students with a foreign background (catering mainly to students that live in areas where the fraction of households with foreign background is low).

These very specific and concentrated effects indicates that there is something more going on than just a general lack of information. We speculate that it might be due to differences in priors and preferences but given that we do not have information on either we can not say anything definite. It does however speak to the external validity of our study, in a setting where there are no high performing schools with a higher than average share of visible minorities we should not necessarily expect the same effects.

Using simulations we study the assignment of students in two scenarios, one where everyone is acting as the control group (where no one has access to performance information) and one where everyone is acting as in the treatment group (where everyone has access to performance information). We find that the share of students admitted to one of the top performing schools increases with 2 percentage points (or 6.6 % from a baseline of 30 percentage points in the control group). This implies that in the scenario when no one have access to information on adjusted school performance there would be free capacity on the top performing schools but, in a the case where everyone have access these schools would be oversubscribed. This is a clear example of the muting effect that institutional factors can have on the effect of altered school choice behavior on school assignment.

In line with the results on choice behavior we do find that these changes in assignment are driven by high-skilled and native households from whom the increase is 3 percentage points. The corresponding increase in the other groups is 0.7 percentage points. The fact that the effect is positive for the other groups does however imply that there is no displacement, rather the new students are simply occupying free capacity.

The corresponding loss of students at other schools is correlated with the unpopularity of the school with the two most unpopular schools experiencing more than 75 % of the decrease. Interestingly the students at these school are not students for whom we observed changes in choice behavior. This implies that this reduction is not due to students from these school moving directly to the top performing schools but rather of them, directly or indirectly, occupying the free capacity created at other schools when students from there moved into the top preforming schools. This is strong evidence for the propagating effect discussed above and implies that an intervention of this kind can benefit not only those that react to it by altering their behavior but also those that did not.

Lastly, we find that school segregation in terms of foreign background significantly decreases. The Duncan dissimilarity index drop more than 6 points. About half of this drop is due to the increase of children from high-skilled and non-foreign households that moves from the schools with a low share of students with foreign background into the top ranked schools. The other half is due to the free capacity their departing creates at the schools with a low share of students with a foreign background, being occupied, at least in part, by students with a foreign background.

The remainder of this paper is organized as follows. Section 2 outlines the institutional setting and describes the experiment, Section 3 describes the data and the empirical strategy, Section 4 presents results and Section 5 concludes.

2 The Institutional Setting and the Experiment

The Swedish compulsory school system consists of nine grades with children, in general, starting 1st grade in the year they turn seven. In 1991, Sweden decentralized its school system by making the municipalities responsible for the provision of compulsory education. In 1992, voucher schools and public school choice was introduced. Given this school choice can, in practice, mean different things in different municipalities.

Our experiment was conducted in Linköping municipality. In this section we will first, in Section 2.1 we describe the school choice system in Linköping and the institutional framework surrounding it. We will then, in Section 2.2 describe the experiment we conducted in this setting.

2.1 School Choice in Sweden and Linköping

Linköping is the fifth largest municipality in Sweden with a total of 158 000 inhabitants. Most of these inhabitants live in the town of Linköping (109 000 inhabitants) located in the middle of the municipality, the rest live in smaller villages or rural areas surrounding the town. The average size of a student cohort is about 1700 students. In Linköping most schools are either elementary schools (covering kindergarten up to 6th grade) or middle schools (covering 7th to 9th grade).[10] We will, in this paper, focus on the choice of middle school. In total there are 19 middle schools in Linköping. They are concentrated in the town of Linköping with the highest concentration in the city centre. Figure 2 shows a map over Linköping municipality with the schools marked.

Figure 2: **School location** (©Statistics Sweden)
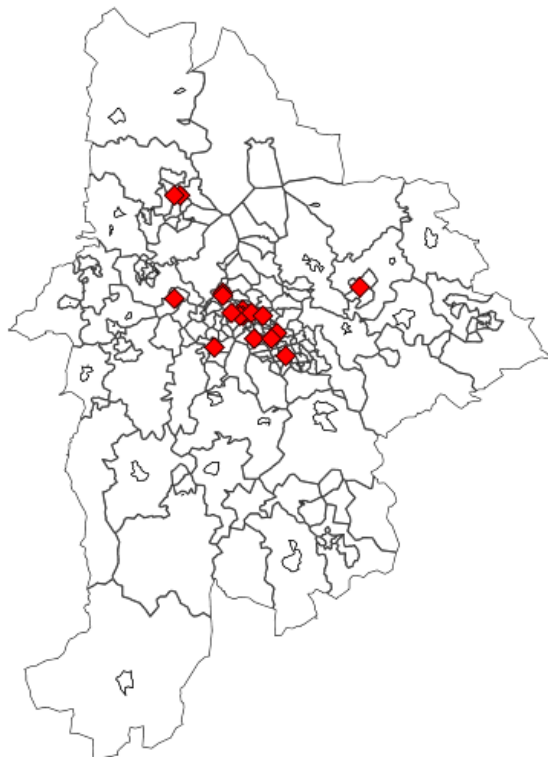
In the beginning of January of each year the municipality sends out a letter to all households with children about to start 7th grade. The letter informs the parents that it will soon be time to choose a school for the upcoming academic year. The letter also contains information on which school is, what the municipality refers to as, the households guaranteed school. This is the school that the student will be placed in if another school is not actively chosen or if they are not accepted to the schools they have applied for. It is usually the school closest to the households residence. The letter also contains a user name and a password to an

---

[10]There are a few voucher schools that depat from this pattern by offering 4th to 9th grade or all grades from kindergarten or 1st to 9th grade.

online tool where the parents can log in and apply for schools.[11] Further, the letter contains information on how priority to oversubscribed schools is determined (more on this later), information on where one where can find more information on the available schools and where one can find information in other languages then Swedish.

Parents have a little more than two weeks to log into the tool and submit their choices. They can list up to three ranked choices. There are some middle schools in Linköping where there are special tracks such as drama, science, soccer and so on. If one wants to apply for one of these it has to be ranked as one of the three choices one is allowed. Applications to the voucher schools are are done using the online tool.

Once the online tool closes, in the end of January the municipality starts by placing everyone in their guaranteed school. Once this is done the municipality sends the lists of applicants to the voucher schools so they can rank the applicants by their priority ordering.[12] The municipality also determine what priority each student has at each of the public schools. Priority is determined by proximity unless one applies for a specific track for which there are special tests or tryouts.

Figure 3: **Timeline of the school choice process**
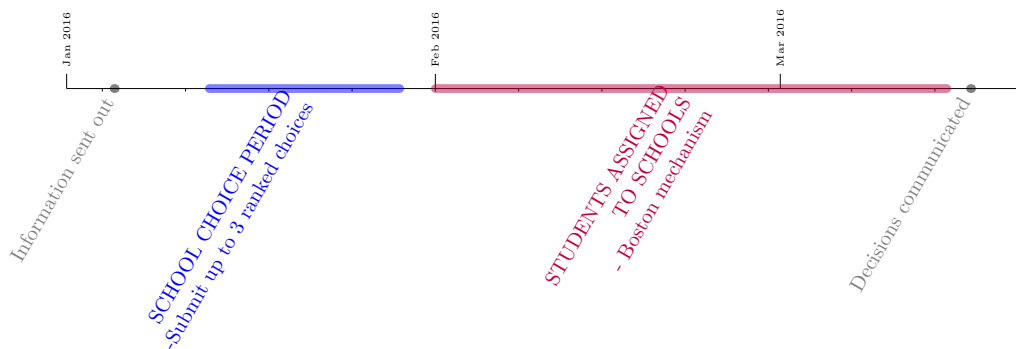
When priority is established the municipality starts placing the students using the Boston mechanism.[13]. The municipality starts by placing everyone in their guaranteed school. They then allow everyone to apply to their highest ranked school. This is done by hand and in several rounds as once a student is accepted at a school other than their guaranteed school a spot at that school is freed up making it possible to place another student on a their highest ranked school. Once they have placed everyone they can on their highest ranked school they repeat this process using the second highest ranked school for them that did not get accepted to their top choice and submitted a second choice. When that is done the process is repeated again using the third highest ranked school. Using the Boston mechanism means that households have to take strategic incentives into consideration when choosing schools. This is because the process drastically reduces the chances of being accepted in any particular school after being rejected from ones highest ranked school. Once everyone is assigned to a school the municipality send out a second letter informing the households where their child have been placed. This is usually happens in the middle/end of March. Figure 3 visualizes this process.

When making their choices the households have access to a number of different sources of information. The main one is the municipalities own online tool where one can compare schools. This tool is available on the municipality website and referred to in the letter the households receives when they are first informed

---

[11]They can also log in and accept the spot in their guaranteed school, this is however equivalent with not making a active choice at all, as this would also result in the child being placed at the guaranteed school.

[12]All voucher schools in Linköping uses time in school specific queues to establish priority.

[13]The Boston mechanism is described in detail in Abdulkadiroglu & Sönmez (2003)

Figure 4: **Extract of the Letter**

Skola School	Genomsnittspoäng Average Score (max=20)	Över/underprestation Over/underperformance (poäng/points)
Arenaskolan	-	-
Berzeliusskolan	14,4	+0,2
Björkö Friskola	13,2	-1,2
Dar al Uloum	-	-
Ekholmsskolan	12,6	-1,0

that it is time to choose schools. The tool includes both the public schools and the voucher schools in Linköping municipality. In this tool one can find the location of the school, the number of students in different grades, a short text authored by the school itself and results from the municipalities own surveys where students and parents rate their school in terms of learning environment, facilities, food and so on. This service does not contain any information on grades, test scores, graduation rates, student-teacher ratios or proportion of certified teachers. This information can be found online in the Swedish National Agency for Educations database. The existence of this database is however not common knowledge and it not to easy to navigate. The schools also offers open houses where one can come and visit the school before submitting ones final choices. Given the number of schools that are available this is however not an efficient way for parents and/or children to get a clear picture of their options. We think that it is fair to say that, compared to Swedish municipalities in general, information on school performance in Linköping is lacking.

2.2   The Experiment

The experiment was conducted in Linköping in 2016. All households in the municipality with children about to start grade 7th grade was randomly selected to be part of either the control or treatment group. In January, at the same time as the municipality sent out their letter to the households about to choose schools (see Figire 3), households belonging to the treatment group received a letter with additional information about the schools in their municipality.

This letter included information about the available schools' performance on standardized tests. More specifically we used average test scores on the standardized test in 9th grade in Swedish, Mathematics and English.[14] We also provided the households with a measure on how the school performs in relation to how it is expected to perform given its student composition.[15]. We translated the information to nine different languages to ensure that everyone would be able to understand it. We also provided them with our contact details so they could contact us if there they had any questions. Figure 4 shows an extract of the table that was included in the letter.

3   Data and Empirical Strategy

In this Section we start with a description of the data we are using (Section 3.1), then, in Section 3.2 where we report and discuss some descriptive statistics and finely, in Section 3.3 we discuss our empirical strategy

---

[14]In Sweden standardized tests are taken in 3rd, 6th and 9th grade
[15]We will describe in detail how this measure is constructed in section 3.1

and evaluate our identifying assumption.

3.1 Data

There are two main sources of data used in this study. The first is school choice data provided by the municipality of Linköping and the second is register data from Statistics Sweden. For each student, we have an indicator for whether the household received our letter with additional information on school performance or not. This is used to define the control and treatment group.[16] The municipal data also contains information about each household's school choices. Children in this data set are linked to register data using individual identifiers and to their parents using the multi-generational register available from Statistics Sweden.[17] From register data, we extract demographic and geographic information on all school starters as well as the students already attending schools in Linköping. A more detailed description of the data follows.

Definition of study population   We depart from the population of student's that, in January 2016, was expected to start 7th grade in Linköping in August 2016. Within this population, individuals were randomly assigned to either the treatment or control group. We drop 2 individuals as we do not observe their school choices.[18] Further, we drop 41 individuals that were never assigned to the control or treatment group although we do observe their school choices.[19] We end up with a population of 1615 households with children about to start 7th grade for whom we can both observe choices and can categorize them as treated or not.

School choices   From the municipality, we obtained data on all school choices made by households in our population. We observe up to 3 choices for each household. In addition, we observe each student's guaranteed school. In one case, students are guaranteed a spot at one of two different schools, in which case we observe both of these.[20] We also construct a variable indicating whether they choose a school other than their guaranteed school. If no school was actively chosen we recode the the top choice to be the guaranteed school. Finally, this data contains information about which school the student was assigned to by the municipality.

School performance   We have access to the performance on the standardized tests in 9th grade on all schools in Sweden. This data was used to construct the measures of school performance included in the letter sent out to households in the treatment group. We observe the scores (on a scale from 0 to 20, where 10 points is generally considered a passing score) in Swedish, Mathematics and English. For each school, we calculate the mean test score over all subjects and take average over the years 2013-2014. We do this to reduce the noise that comes from the fact that some schools have a limited number of students in each grade.[21] We use this measure as our measure of absolute performance. As mentioned we also provided the treated households with a measure on how the school performs in relation to how it is expected to perform

---

[16]In addition, we have an indicator for households where the letter was sent back to us because the recipient could not be located. This happened in three cases and all these where cases where the students parents where not living at the same address. When parents where not living at the same address we sent out one letter to each parent. Hence, at least one parent in all treated household received our letter.

[17]If the student was adopted, we use the characteristics of the adopting parents instead of the biological parents. We do this as we are interested in how households react to information.

[18]These individuals were not assigned a school by the municipality, which makes it likely that they moved out of Linköping and thus did not take part in the school choice process

[19]These are individuals either who moved to Linköping before the start of the school year, but after January 2016 when we defined our population or students from other municipalities that wanted to go school in Linköping.

[20]When this is the case we will use the average characteristics of these guaranteed schools to describe the the households default option. The two schools are very similar in observed characteristics and hence we do not think this is a problem.

[21]See Kane & Staiger (2002). We would like to have included additional years but due to data limitations this was not possible.

given its student composition. We calculate this by estimating the following equation on the full population of middle schools in Sweden, separately for 2013 and 2014:

$$Y_i = \alpha + \boldsymbol{X}_i'\boldsymbol{\beta}_x + \varepsilon_i \tag{1}$$

where Y_i is school's i:s the scores on the standardized tests in 9th grade and $\boldsymbol{X_i}$ is a vector of variables describing the student composition on school i.[22] We take the residuals from the schools in Linköping, average them over the years 2013-2014 and use that as a measure on how the school performs in relation to how it is expected to perform.

We do lack these outcomes for three of the schools chosen by households about to start 7th grade. In two cases it is because the school was opened after 2014 and hence no tests had been written on these schools at the time we formulated our letter. The last school is a small religious school (with between five and ten students in each grade) where we do not have access to data [23]. This means that we only observe the performance of the chosen and reserved schools for 1439 households with children starting 7th grade.

Demographic characteristics   From Statistics Swedens LISA register, we obtain information on, among other things, gender, region of birth and level of education of all parents of school choosers and students enrolled in a school in Linköping. We use this to create an indicator variable for foreign background, equal to one if the student is born abroad and/or both the students parents are born abroad. We also create a dummy variable for high-skilled parents, equal to one if at least one of the parents have an education beyond upper secondary school. We use the compulsory school register, containing all students in Linköping in combined with the information from the LISA register to construct measures of the student composition of each school in Linköping.[24]

Geographical data   We observe the geographical location of both students and schools. Residential location of students is observed the first of January 2016. School location is observed in 2013. For both students and schools, location is mapped out using a 250m×250m grid for which we observe the coordinates of the midpoint of each cell[25] Using this, we calculate the distance between each student and each school.[26]. In total we observe distance for 1427 households with children starting 7th grade. This is due mainly to the previously mentioned school openings but also because we lack information on a few households residual location.

3.2   Descriptive statistics

Table 1 show the attributes of the schools that the middle school starters in Linköping had available to them. We observe performance and composition of a total of 16 schools in 7th grade. The schools average score is 13.32 (on a 20 grade scale). Looking at the adjusted measures we can see that Linköpings middle schools are

---

[22]In doing this we follow the Swedish National Agency for Education's method used in their so called SALSA-measure.

[23]Statistics Sweden do not provide data on students in religious schools in order to protect religious freedom

[24]The the latest compulsory school register we have access to is from 2014 witch means that we only observe students who were enrolled in 2014. Our measure of the student composition at the schools available to choose from in 2016 is thus based on students enrolled in those schools in 2014. In general we do not think this is an issue because the student compositions of the schools do not vary much over time. This does however mean that we lack information on the student composition for the two schools opening after 2014. Further, we also know that the municipality, during this time, due to increasing cohort size, converted a school that was previously mainly aimed at teaching newly arrived immigrants Swedish to a regular schools. We therefore recode the student characteristics of schools with more than 95 % where immigrants to missing to avoid drawing wrongful conclusions. We also lack this information on the previously mentioned religious school.

[25]For those living in the rural areas of Linköping the gird is 1000m×1000m.

[26]The distance is calculated as $distance = \sqrt{(xcoord_{student} - xccord_{school})^2 + (ycoord_{student} - ycoord_{school})^2}$

under-performing somewhat compared to the rest of Sweden given their student composition. The average school has 22 % students with a foreign background and 62 % students with high-skilled parents. In all these measures there is considerable spread. For example, the middle school with the worst absolute scores the average score is 9.75 which is not considered a passing grade while in the best performing school the average score is 16.10 which is well over the threshold for a pass with distinction.

Table 1: **Characteristics of Available Schools**

	Mean	SD	Min	Max
Performance				
Absolute performance	13.32	1.62	9.75	16.10
Adjusted performance	-0.42	0.79	-2.03	1.03
Student composition				
Foreign background (fraction)	0.22	0.21	0.03	0.86
High-skilled parents (fraction)	0.62	0.16	0.15	0.86
Observations	16			

Note: The absolute performance is the average scores on the standardized tests in Swedish Mathematics and English 9th grade during 2013-2014. These scores ranges from 0 to 20. The adjusted performance is the difference between the actual and expected score, where the expected score is the score predicted by the regression in Equation (1). We classify a student as having foreign background if a student is born abroad or if both parent are born abroad. We classify students as having high-skilled parents if at least one of their parents have an educational level higher than upper secondary.

Table 2 show descriptive statistics of our population of school choosing households. We note that their demographic characteristics are very similar to the student population at large. We can also see that their guaranteed schools are, perhaps, under-performing somewhat compared to the population of schools in general. They also seem to have a somewhat higher proportion of students with a foreign background and low-skilled parents.

We do also see that, on average, most households have several schools within a reasonable distance from their home. Households with children starting 7th grade have, on average, 6 schools within 5 kilometers. The variance in the number of school close to home is quite large with some households having no schools within 5 kilometers while others have more then 10. This reflects the fact that some households reside in the town of Linköping where the schools are concentrated while other reside in smaller villages and the rural area surrounding the town. On average the spread in performance of the schools within 5 kilometers from home is large. The average difference in scores between the highest performing and the lowest performing school is over 4 points. The same goes for the student composition of the schools within 5 kilometers of the school choosers. For those starting 7^{the} grade the average difference in fraction of students with a foreign background (high-skilled parents) between the school with the highest fraction and the school with the lowest fraction is 46 (43) percentage points. Just like with the number of schools close to home there is a lot of variation in these measures depending on if one lives in or outside the town of Linköping.

That there are multiple schools with varying characteristics close to ones home can be considered a necessary condition for being able select into a higher performing school. It is however not sufficient, it will also depend on the assignment mechanism and priority structures used. In Linköping we have knowledge on how this mechanism operates and how priorities are determined and can therefore, for each student, examine their actual choice opportunities. Specifically, what we do, is that we, using the mechanism and the priority

structures used in Linköping, for each student's determine which schools they would get accepted to if they where to apply for it (keeping all other households choices constant). We then examine if any of the schools they would get accepted would mean that they improved at least one standard deviation in terms of either absolute or adjusted performance compared to their guaranteed school. Almost 80 % of all students can unilaterally improve in terms of absolute performance and 84 % of all students can unilaterally improve in terms of adjusted performance. The few that can't improve are mainly students that have one of the top schools as their guaranteed school. This in combination with the fact that most schools are located in the city center (see Figure 2) where most of the municipalities bus lines connect implies that most households do, in practice, have an actual choice and can react to our treatment.

Table 2: **Background Characteristics of Households**

	Mean	SD	Min	Max
Predetermined variables				
Foreign background	0.22	0.41	0.00	1.00
High-skilled parents	0.66	0.47	0.00	1.00
Characteristics of guaranteed school				
Absolute performance (guaranteed)	13.16	1.43	9.75	16.10
Adjusted performance (guaranteed)	-0.41	0.65	-2.03	1.03
Share foreign background (guaranteed)	0.23	0.19	0.03	0.86
Share high-skilled parents (guaranteed)	0.60	0.14	0.15	0.86
Geographic variables				
No. of schools within 5000m	6.00	4.74	0.00	13.00
Diff (max-min), absolute performance (within 5000m)	4.05	2.30	0.00	6.35
Diff (max-min), adjusted performance (within 5000m)	2.12	1.09	0.00	3.06
Diff (min-max), fraction foreign background (within 5000m)	0.46	0.32	0.00	0.81
Diff (min-max), fraction high-skilled parents (within 5000m)	0.43	0.25	0.00	0.71
Observations	1615			

Note: The absolute performance is the average scores on the standardized tests in Swedish Mathematics and English 9th grade during 2013-2014. These scores ranges from 0 to 20. The adjusted performance is the difference between the actual and expected score, where the expected score is the score predicted by the regression in Equation (1). We classify a student as having foreign background if a student is born abroad or if both parent are born abroad. We classify students as having high-skilled parents if at least one of their parents have an educational level higher than upper secondary.

Finally, in Table 3 we describe the choice behavior in the control group. In panel (a) we can see that about 50 % of households are making an active choice. In general households with a non-foreign background and high-skilled households are more active compared to households with foreign background and low-skilled households. Non-foreign and high-skilled households also choose schools with better absolute performance, a larger fraction of students with high-skilled parents and fewer students with a foreign background compared to households with a foreign background and low-skilled households. When it comes to adjusted performance this pattern is not as clear, high-skilled households choose higher performing schools compared to low-skilled households but households with a foreign background choose higher performing schools compared to households with a non-foreign background.

In panel (b) we report the difference between the top ranked school and the guaranteed school in various school attributes for the subset of households that makes an active choice. All groups do, on average, with their top choice, improve on the absolute and adjusted performance compared to their guaranteed school. All groups so also, on average, choose schools with a larger fraction of students with high-skilled parents

Table 3: **Choice Behavior in the Control Group**

	All Mean	Native Mean	Foreign Mean	High-skilled Mean	Low-skilled Mean
(a) Whole Sample					
Active choice	0.50	0.56	0.24	0.56	0.39
Distance (top)	4345	4840	2690	4439	4240
Average absolute performance (top)	13.81	13.91	13.47	14.05	13.38
Average adjusted performance (top)	-0.21	-0.23	-0.13	-0.17	-0.29
Fraction w. foreign background (top)	0.21	0.16	0.36	0.18	0.24
Fraction w. high-skilled parents (top)	0.67	0.69	0.61	0.70	0.62
Observations	783	615	166	510	265
(b) Conditional of Being Active					
Distance (top-guaranteed)	1956	2047	1296	1931	2037
Average absolute performance (top-guaranteed)	0.90	0.92	0.78	0.89	0.92
Average adjusted performance (top-guaranteed)	0.49	0.53	0.22	0.52	0.40
Fraction w. foreign background (top-guaranteed)	-0.07	-0.06	-0.13	-0.06	-0.11
Fraction w. high-skilled parents(top-guaranteed)	0.17	0.17	0.15	0.16	0.18
Observations	318	280	37	244	74

Note: The absolute performance is the average scores on the standardized tests in Swedish Mathematics and English 9th grade during 2013-2014. These scores ranges from 0 to 20. The adjusted performance is the difference between the actual and expected score, where the expected score is the score predicted by the regression in Equation (1). We classify a student as having foreign background if a student is born abroad or if both parent are born abroad. We classify students as having high-skilled parents if at least one of their parents have an educational level higher than upper secondary. Active choice is equal to one if the household made an active choice of non-reserved school is equal to one if the household chose a school other than their guaranteed school.

and a lower fraction of students with a foreign background than their guaranteed schools. In the case of adjusted performance we see that high-skilled households and non-foreign households seem to improve more (in absolute terms) compared to low-skilled households and households with a foreign background. Households with a foreign background and low-skilled households also seem to select schools with a smaller share of students with foreign background compared to their guaranteed schools than the other groups. Finally, we should also note that households with foreign background choose schools closer to their home. This is likely due to the fact that they are more likely to live in the town of Linöping.

3.3 Empirical strategy

Estimating treatment effects in a randomized controlled trial is straightforward. Let T_i be an indicator equal to 1 if household i is as assigned to the the treatment group and zero otherwise. The effect of the treatment on any outcome Y_i can then be estimated using a linear regression model specified as:

$$Y_i = \alpha + \beta T_i + \epsilon_i \tag{2}$$

where ϵ_i is a (heteroskedasticity robust) error term.[27] We are interested in the effects of treatment on

---

[27]As neither the sampling process nor the assignment of treatment was clustered, we choose not to report clustering adjusted standard errors. See Abadie *et al.* (2017) for a discussion about when to use clustered standard errors. All statistical tests in

the quality of the chosen schools, measured by β.

Y_i is our outcome of interest. In our main specification this will be a dummy indicating if the top ranked school is one of the top performing schools in the information we sent out.[28] We will also run the analysis on a set of alternative outcome variables to investigate whether the information sent out affected school choice behavior in dimensions other than school performance. In these specifications Y_i will be the the difference in the outcome of interest between the household i:s top ranked school and their guaranteed school.

For β to have a causal interpretation, we need the randomization of households into the control and the treatment group to have been successful. To confirm random assignment, we run a regression with the treatment indicator (T_i) as the dependent variable and include all predetermined variables (from Table 2) as independent variables. If randomization was successful, we do not expect the predetermined variables to predict treatment. Table 4 show the results from this regression, as well as the p-value of a F-test of joint significance. One out of eleven of the included predetermined variables is significant, and the F-tests confirms the null hypothesis. We conclude that randomization was successful and hence, that whether a household received treatment or not is truly random.

Another potential threat to identification is that information could have spread between households in the treatment and control group. There is no perfect test of this and hence it is not possible to exclude this possibility. However, the time window for school choice it not that long (less than three weeks). During this time we covered all local media and there was no mention of the experiment. Anecdotal evidence from households contacting us also suggest that the information sent out was not subject to any discussions between parents.[29] Further, we have access to the choices made by households with children about to start 7th grade in Linköping in 2014. If information has not spread from the treatment group to the control group these households should have access to about the same information about the schools as our control group in 2016 and hence make similar choices. For each school that was available to choose in both 2014 and 2016 we estimate the differences in probability of it being chosen by running the following regression:

$$S_i = \alpha + \beta 2016_i + \epsilon_i \tag{3}$$

where S_i is a indicator variable taking the value of 1 if the school in question was household i:s top ranked school and zero otherwise, 2016_i is an indicator variable if the household is part of the control group in our experiment in 2016 and zero if the household was a school chooser in 2014 and ϵ_i is a (heteroskedasticity robust) error term. β should then be interpreted as the difference in the total fraction of students choosing school i in the control group in 2016 and in the general population in 2016. In Figure 5 we plot these β:s and their confidence intervals for each school. We order the schools by the performance measures we included in our letter. If the information has spread from the treatment group to the control group we would expect the β:s to be increasing. We only have one significant effect; one of the low performing schools is considerably less chosen in the control group of our experiment compared to the general population of households 2014. The effect on this school is very large, the fraction of students choosing the school is reduced by almost 8%. This school did go from over a hundred applicants in 2014 to only 11 in 2016. We do know that one middle school in Linköping was evacuated in 2015 due to extremely poor working conditions. The school is now

---

this paper has also been done using randomization inference (see Young (2017) for more details on this). Doing this does not affect any of the results.

[28]We will focus on the top choice because it is the natural focus in the Boston Mechanism. Most households don't list a second or third choice (63%). Further, the second choices we do observe are not very useful for our purposes. In about half of the cases the second choice is some sort of special track, one fifth of all households lists the same school as their first choice (after listing a special track as their first choice) and 15% are listing their reserved school as their second choice (poetically believing that they could loose their spot there when actively choosing another school)

[29]Of 30 individuals contacting us regarding the information sent out, one had discussed it with another parent and one had heard another parent mentioning the letter.

Table 4: **Test of randomization**

	Treated
Foreign background	0.0317
	(0.0381)
Highly educated	-0.0101
	(0.0319)
Absolute performance (guaranteed)	0.0440
	(0.0452)
Adjusted performance (guaranteed)	-0.0714
	(0.0513)
Share foreign background (guaranteed)	0.0529
	(0.137)
Share high-skilled parents (guaranteed)	-0.187
	(0.383)
No. of schools within 5000m	-0.00853
	(0.0113)
Diff (max-min), absolute performance (within 5000m)	-0.0307
	(0.0437)
Diff (max-min), adjusted performance (within 5000m)	0.0777*
	(0.0471)
Diff (min-max), fraction foreign background (within 5000m)	-0.105
	(0.241)
Diff (min-max), fraction high-skilled parents	0.286
	(0.227)
Constant	-0.0460
	(0.443)
Ftest	0.601

*Note: Standard errors in parenthesis. Significance levels indicated by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable is the treatment indicator (T_i). The absolute performance is the average scores on the standardized tests in Swedish Mathematics and English 9th grade during 2013-2014. These scores ranges from 0 to 20. The adjusted performance is the difference between the actual and expected score, where the expected score is the score predicted by the regression in Equation (1). We classify a student as having foreign background if a student is born abroad or if both parent are born abroad. We classify students as having high-skilled parents if at least one of their parents have an educational level higher than upper secondary. F-test reports the p-value from an F-test of joint significance of all variables included in the model.*

Figure 5: **Test of Information Dispersion**

(a) Absolute scores (7th grade)

(b) Adjusted scores (7th grade)
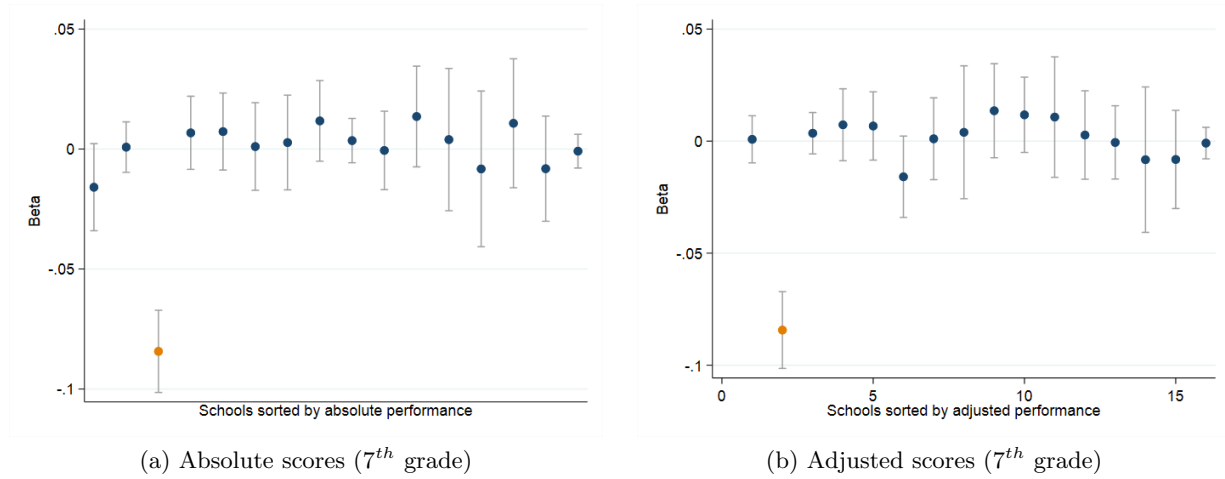
temporarily housed in the cellar of a nearby high school and local media has reported that students and teachers alike are fleeing the school. This could explain this extreme outlier. Except for this single school we do see a upwards slope (or indeed any other pattern that would suggest that information had spread between from the treatment group to the control group). Given this we are comfortable with assuming that the spread of information from the treatment group to the control group has been limited.

4   Results

In this section we will present and discuss our results. In Subsection 4.1 we discuss the effects of our treatment on choice behavior, we then move on to the effects on assignment (in Subsection 4.2 assignment and finally, in Subsection 4.3 the effects on school segregation.

4.1   Effects on Choice Behavior

Table 5 reports the effect on making an active choice (what one can think of as the extensive margin) and the effects on the composition of those making an active choice. In the first column we can see that there is no aggregate treatment effect on choosing another school then ones guaranteed school. The same is true for the composition of active households.

Table 6 reports the effects of our treatment on the performance of the chosen schools. Panel (a) reports the results for absolute performance while Panel (b) reports the results for adjusted performance.

In the first column the dependent variable is a indicator variable taking the value one if the household top choice is one of the top 5 performing schools (for absolute and adjusted performance respectively) as they where ranked in our letter. In the second and third column we repeat this analysis but with the top 4 or top 6 performing schools instead. This is to show that the results are not knife-edge in the sense that they are dependent of us using the top five schools. In the forth column the dependent variable is the actual performance measure (as it was printed in the letter) of each households top ranked school minus the performance measure of their guaranteed school. This measure is standardized with the school standard deviation in the relevant performance measure.

For absolute performance we find no effects. In this our results differ from most previous studies. There can be several reasons for this. Firstly, we couple the information on absolute performance with information

Table 5: **Effects on the Extensive Margin & Composition**

	Active	Share active, high-skilled	Share active, foreign background
Treated	0.0141	0.0251	0.0314
	(0.0249)	(0.0307)	(0.0227)
Constant	0.496***	0.732***	0.103***
	(0.0179)	(0.0225)	(0.0155)
Observations	1615	812	810

*Note: Significance levels indicated by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. We classify a student as having foreign background if a student is born abroad or if both parent are born abroad. We classify students as having high-skilled parents if at least one of their parents have an educational level higher than upper secondary. Active choice is a indicator variable taking value one if the households is opting out of their guaranteed school and zero otherwise.*

on adjusted performance which they did not. It could therefore be that they would have found similar results as us if they would have included adjusted performance. Secondly, the absolute differences between the schools in Sweden are, in general, not as large as the differences in the US and Chile. This means that there could be less reason to base ones school choice on absolute performance. Further, the households in our sample are, on average, quite well-educated in comparison to the households in the previous studies. More well-educated households could potentially already have internalized the information on absolute performance or, alternatively, they could know that measures absolute performance does not give much real information on how productive a school is when it comes to producing knowledge.

When it comes to adjusted performance we can see in the first column that households in the treatment group are about 5.4 percentage points more likely to choose one of the top five schools compared to the control group. The second and third column confirm these results. In the forth column, where the dependent variable is the difference in performance between the households top ranked school and their guaranteed school, we get insignificant results. The coefficient (0.0438) is however similar to what we get from a back of the envelope calculation where we assume that 5.4 % of the students move from the average performing school among the non-top performing schools to the average performing school among the top performing schools (0.0531). The reason why the numbers do not match up perfectly is because, as we will show later, the flow into the group of top performing schools is not coming from the lowest performing schools but rather from the schools in the middle of the distribution. Hence it seems like the loss of significance is not due to the fact that the effect is not there but rather to a loss of power and precision.

5.4 percentage points might not seem like a big effect, however, in the control group only 33.3 % list one of the top performing schools as their top choice. Hence, our information intervention increases the applications to the top performing schools with about 16 %. At the same time the subscription to the non-top performing schools decreases with 8 %. This is not insignificant and could potentially increase the competitive pressure to improve performance in the system. How strong this effect would be is something we can not answer in this study. To so this we would need to treat a random sample of municipalities and track how performance evolves over time.

Table 7 shows the effects for different subgroups. We see that it is mainly driven by high-skilled households with a non-foreign background whom, if treated, are 7.5 percentage points more likely to list one of the top five schools (in terms of adjusted performance) as their top choice.

In Table 8 we report the effects on other school attributes of the chosen schools, namely the student composition and the home-school distance. The first columns reports the effects on distance; there are no significant effects. The same is true for the fraction of students from high-skilled households in the schools

Table 6: **Effects on Performance of Schools Chosen**

(a) **Absolute performance**

	Top 5	Top 6	Top 4	Performance
Treated	-0.0134	0.0296	0.00674	-0.0291
	(0.0237)	(0.0248)	(0.0205)	(0.0346)
Constant	0.350***	0.517***	0.212***	0.410***
	(0.0171)	(0.0179)	(0.0146)	(0.0254)
Observations	1615	1615	1615	1439

(b) **Adjusted performance**

	Top 5	Top 6	Top 4	Performance
Treated	0.0537**	0.0459*	0.0558**	0.0438
	(0.0239)	(0.0248)	(0.0229)	(0.0390)
Constant	0.333***	0.446***	0.277***	0.225***
	(0.0169)	(0.0178)	(0.0160)	(0.0278)
Observations	1615	1615	1615	1439

*Note: Standard errors in parenthesis. Significance levels indicated by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Top 4/5/6 is an indicator variable taking the value one if the household has chosen one of the top 4/5/6 performing schools (for absolute and adjusted performance respectively) as they where ranked in our letter. Absolute performance is average score on the standardized tests in Swedish, Mathematics and English in 9th grade. Adjusted performing is the difference between the actual and expected score, where the expected score is the score predicted by the regression in Equation (1).*

Table 7: Effects on Adjusted Performance of Schools Chosen, Subgroups

	Top 5	Top 5	Top 5	Top 5	Top 5	Top 5
Treated	0.0553*	0.0361	0.0682**	-0.00616	0.0751**	0.0256
	(0.0301)	(0.0393)	(0.0265)	(0.0534)	(0.0319)	(0.0360)
Constant	0.363***	0.283***	0.301***	0.452***	0.339***	0.325***
	(0.0213)	(0.0277)	(0.0185)	(0.0387)	(0.0224)	(0.0256)
Observations	1053	547	1260	350	921	694
Subgroup	High-skilled	Low-skilled	Non-foreign background	Foreign background	High-skilled & non-foreign background	Low-skilled &/| foreign background

*Note: Standard errors in parenthesis. Significance levels indicated by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. We classify a student as having foreign background if a student is born abroad or if both parent are born abroad. We classify students as having high-skilled parents if at least one of their parents have an educational level higher than upper secondary.*

(the second column). In the third column, where we report the effects on the fraction of students with foreign background we do see a large and statistically significant positive treatment effect. On average, treated households top ranked schools have 1.4 percentage points more students with a foreign background compared to the non-treated households top ranked schools.

Table 8: **Effects on Other School Characteristics**

	Distance	High-skilled parents (fraction)	Foreign background (fraction)
Treated	149.2	-0.00737	0.0141**
	(134.9)	(0.00577)	(0.00625)
Constant	879.2***	0.0764***	-0.0317***
	(91.11)	(0.00427)	(0.00460)
Observations	1427	1439	1439

*Note: Standard errors in parenthesis. Significance levels indicated by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Distance is the home-school distance. We classify a student as having foreign background if a student is born abroad or if both parent are born abroad. We classify students as having high-skilled parents if at least one of their parents have an educational level higher than upper secondary.*

This deserves further investigation. In order to do this we divide the schools into four groups. The first group contains the five schools with the best adjusted performance (From now on refereed to as the top performing schools), the second included non-top performing schools with a low share of students with a foreign background[30] (From now on refereed to as the schools with a low share of students with a foreign background), the third group includes the non-top performing schools with a high share of student with foreign background[31] (From now on refereed to as the schools with a high share of students with a foreign background) and the forth group includes the schools for which we do not have measures for performance and composition (From now on refereed to as the non-ranked schools).

In Table 9 we report some summary statistics for these groups of schools. We see that the top performing schools are clearly outperforming the schools in the other groups. We further note that 46% of all students have one of the schools with a low share of students with a foreign background as their guaranteed school. The corresponding number is 30% for the top performing schools of schools is and 23% for the schools with a high share of students with a foreign background. There are virtually no students with one of the non-ranked as their guaranteed school.[32] Of the students that have a top performing school as their guaranteed school 70% come from high-skilled households. This is comparable to the schools with a low share of students with a foreign background (67%) and considerably higher than the schools with a high share of students with a foreign background (58%). 30% of the students from the top ranked schools have a foreign background, this is significantly higher than the schools with a low share of students with a foreign background (10%) but about the same as the schools with a high share of students with a foreign background (31%).

In Figure 6 we see visualize the choice patterns between these groups in the control group. What we show in the figure is the net flow of choices between the groups. In most cases the net flow between two group will be the same as the gross flow. This is because, more or less, no one selects into the schools with a high share of students with foreign a background and no one selects out of the non-ranked schools. Hence, the only case where flow goes both ways is between the top performing schools and the schools with a low share of students with foreign a background.

---

[30]Below the median
[31]Above the median
[32]As mentioned, these schools include two newly opened schools (that mainly focuses on special tracks) and a small religious school.

Table 9: **Description of the School Groups**

	Top 5	Low share of FB	High share of FB	Not ranked
Share of guaranteed spots	0.30	0.46	0.23	0,00
Share of students w. foreign background (guaranteed)	0.3	0.1	0.31	1,00
Share of students w. high-skilled parents (guarantied)	0.7	0.67	0.58	0.42
Average absolute performance	14.2	13.3	11.7	-
Average adjusted performance	0.3	-0.6	-0.9	-

Note: We classify a student as having foreign background if a student is born abroad or if both parent are born abroad. We classify students as having high-skilled parents if at least one of their parents have an educational level higher than upper secondary.

The only group with a negative net flow overall is the group of schools with a high share of students with a foreign background. More than 50 % of the students that have one of these schools as their guaranteed school are trying to opt out to another group. They move to all groups but the largest flow is to the top ranked schools (7.3 % of the student population). There is also a positive net flow from the top performing schools to the schools with a low share of students with foreign background. This flow corresponds to 3 % of the student population. Finally, we also see a general flow into the non-ranked schools from all other groups. The size of the flow into this group from the other groups corresponds quite well with the the relative size[33] of the groups.

In Figure 7 we show the change in these choice patterns between the control and treatment group. The net flow from the top performing schools to the schools with a low share of students with foreign background is reversed. The change corresponds to 5.5 % of the student population, more or less the exact size of our estimated treatment effect reported in Table 6. Further, back of the envelope calculation indicates that this change in net flow explains 80% of the increase in the share of students with a foreign background in the treatment groups top ranked schools (documented in Table 8). We do not document any other economically or statistically significant changes in the choice patterns between the groups.

Hence it seem like the change in net flow between the schools with a low share of students with foreign background and the top performing schools is the only significant effect of our treatment. To make sure however, in Table 10 we look at the effects on the percentile adjusted performance rank (within group) of schools left and chosen. Column one reports the effects on the percentile rank of adjusted performance of schools left conditional of making an active choice. The effects are small and insignificant. This implies that the households choosing to opt out of their guaranteed school in the control group and the treatment group are not opting out of schools that perform significantly different. Column two reports the same thing but conditional of making an active choice and staying in the same group. Again the estimate is insignificant and rather small. This pattern is repeated in the third column where we report the effect on the percentile performance rank of the school left conditional of being active and leaving ones group. In the forth and last column we report the percentile rank of the school chosen conditional of being active and staying in ones group. Again the coefficient is small and insignificant. All in all this confirms the suspicion that there is not much going expect for the reverse of the new flow from the schools with a low share of student with a foreign background to the top performing schools.

The fact that the effects are concentrated in this way implies that there is more going on than just a general lack of information on adjusted performance. One explanation is that is consistent with what we observe is that high-skilled non-foreign households, especially those residing in areas where the share

---

[33]Measured by the number of students that have a school in the group as their guaranteed school
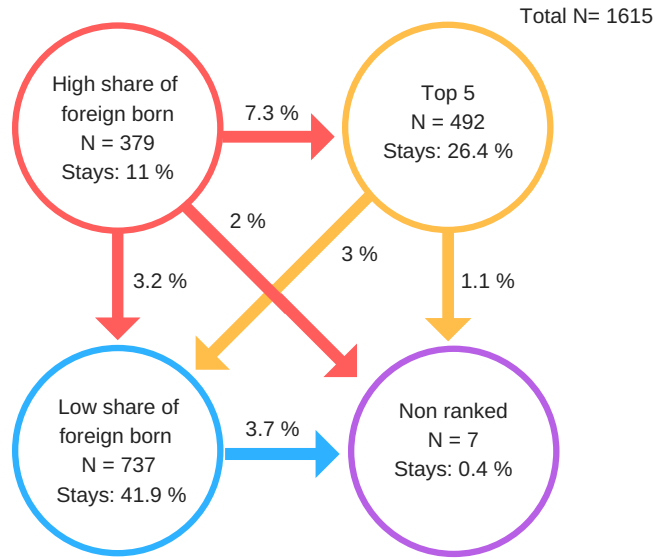
Figure 6: **Choice Patterns Between Groups (Net Flows)**

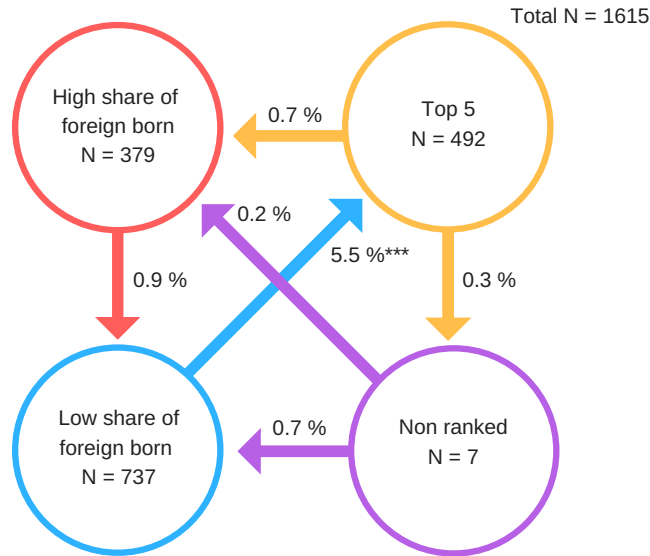Figure 7: **Choice Patterns Between Groups (Net Flows)**

Table 10: **Performance of Schools Left and Chosen**

| | School performance percentile of: | | | |
	School left	School left	School left	School chosen
Treated	0.016	0.030	0.012	-0.009
	(0.024)	(0.042)	(0.028)	(0.039)
Constant	0.398***	0.334***	0.417***	43.44***
	(0.017)	(0.030)	(0.021)	(0.029
Observations	801	185	616	185
Sample restriction:	Being active	Being active & staying in group	Being active & changing group	Being active & staying in group

*Note: Standard errors in parenthesis. Significance levels indicated by * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.*

of households with a foreign background is small, have exaggerated negative priors of the performance on schools with a high share of students with a foreign background and that our information updated these priors enough for some of these households to change their choice behavior. Another explanation could be that the priors are similar between groups but that high-skilled non-foreign households have a stronger relative preference for performance compared to student composition and therefore are the ones that react to the performance information in our letter. These explanations are not mutually exclusive and given that we do not have access to the data on the prior beliefs or on preferences we can not disentangle them. It does however speak to the external validity of our study, in a setting where there are no high performing schools with a higher than average share of visible minorities we should not necessarily expect the same effects.

4.2 Effects on assignment

We now move on to the question of how these changes in choice behavior affect assignment. In order to explore this we run a simulation where we, to the best of our ability, try and replicate the assignment mechanism and the priority structure used in Linköping. What we do is the following:

1. We approximate each schools capacity with the either the number of guaranteed spots or the number of students assigned to the school, whichever is higher.[34]

2. We draw, with replacement, from the control group, a cohort of the same size of our observed cohort.

3. We place them at their guaranteed school.

4. We allow everyone to apply for their top choice.

5. We accept students applying to school where there is free capacity in order of their priority.[35] Students not accepted at their preferred school are refereed back to their guaranteed school.

6. We document the number of students that leaves each school and readjust the free capacity accordingly.

7. We repeat step 4-6 until no students are placed in new schools.

8. We document relevant measures of the assignment.

9. We repeat step 2-9 1000 times.

10. We repeat step 2-10 for the treatment group.

Notice that we abstract from the fact that the households can list multiple choices. This is done for the sake of convenience but we do not think this takes much away from the analysis because, as previously mentioned, most people do not list a second or third choice and when they do they are often listing special tracks, the same schools as they have previously listed or their guaranteed school.

Figure 8 shows the simulation results for the share of students that are admitted to a top performing school. We can see that this share increases with two percentage points from our treatment (from 30 percentage points when we sample from the control the group to 32 percentage points when we sample from the treatment group). This is a little less than 40% of the effects we documented on choice behavior in

---

[34]Note that this might mean that we underestimate the capacity of the schools that do not get filled to their capacity. Given that these are the less popular schools we do not think this will be a big issue and, if anything, it is likely to bias the difference between the control and treatment group downwards.

[35]For public schools we use distance to establish priority while we for voucher schools use a random number. If a school is a students guaranteed school we give them top priority.

Table 6. This implies that in the setting where no one has access to information on adjusted performance there would be free capacity on these schools while in a setting where this information was available to all households the top performing schools would be oversubscribed. Hence, the effect of our treatment on assignment of those that react to the treatment is muted by institutional factors, in this case capacity constraints.
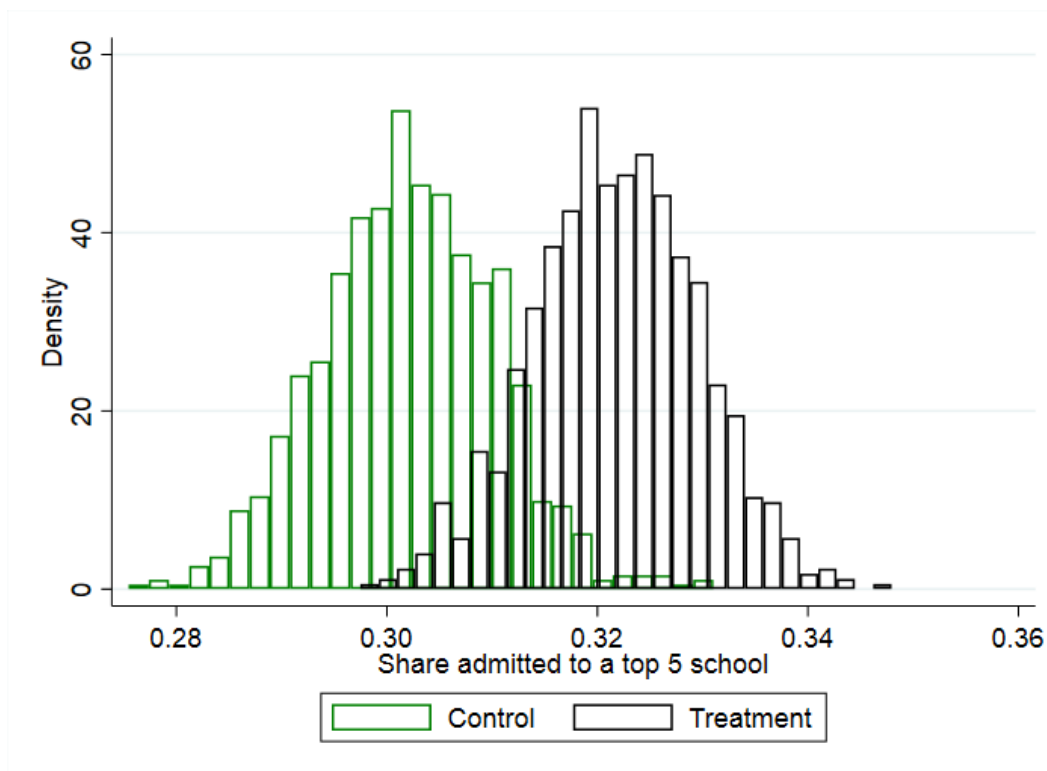
Figure 8: **Share Attending a Top Performing School**

Figure 9 we report the effects on assignment to a top performing school on high-skilled and non-foreign households and the other households separately. We see that the overall effect on assignment is driven by the non-foreign students with high skilled parents. The share of these students attending a top performing school increases with 3 percentage points, from 34.3% to 37.3%. In the other groups we see small but positive effect (0.7 percentage points, from 24.5% to 25.2%). This is what we should expect as it was in the group of non-foreign and high-skilled households where we, in Table 7, documented significant effects on choice behavior. The fact that the effect is positive for the other groups does however indicate that they are not being displaced by the change in choice behavior among the high-skilled households with a non-foreign background.

As the share of student assigned to the top performing schools increases it decreases in the other schools. Figure 10 shows how this decrease is distributed over the schools. In Subfigure (a) we show the relationship between the share of the total student population lost and the unpopularity of the school in the control group.[36] We can see a clear positive relationship. In Subfigure (b) we show the relationship between the share of the schools on student population lost (comparing to the assignment when we are sampling from the control group) and the unpopularity of the school in the control group. We see the same pattern but, disregarding a few outliers, with a clearer linear relationship. This finding is interesting as the most unpopular

---

[36]measured by the share of the students that have the school as their guaranteed school that tries to opt out

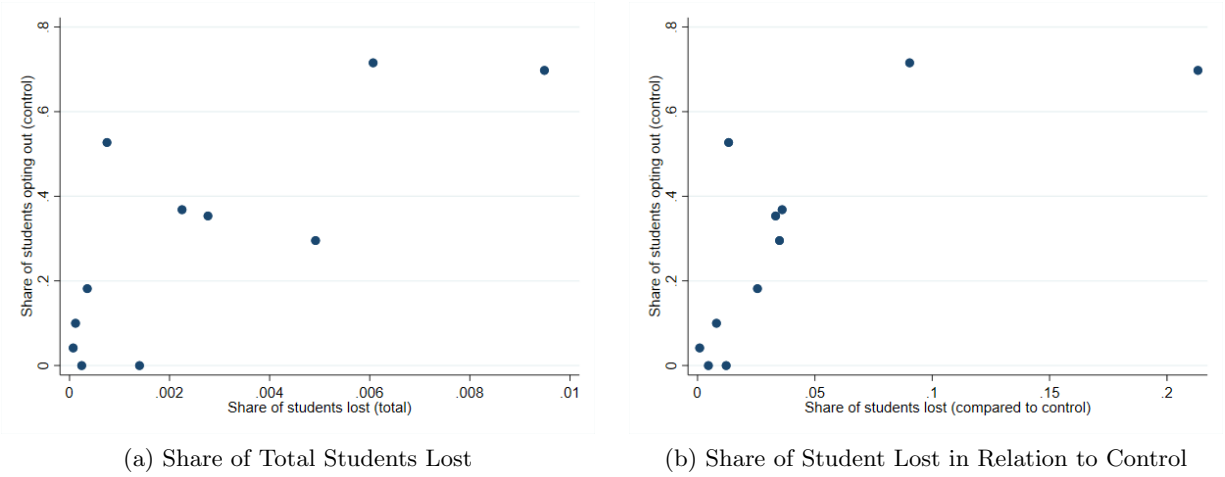Figure 9: **Share Attending a Top Performing School (Subgroups)**
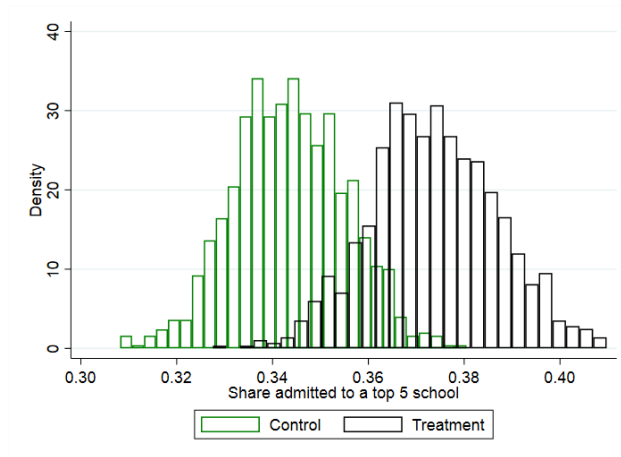
(a) High-skilled and Native
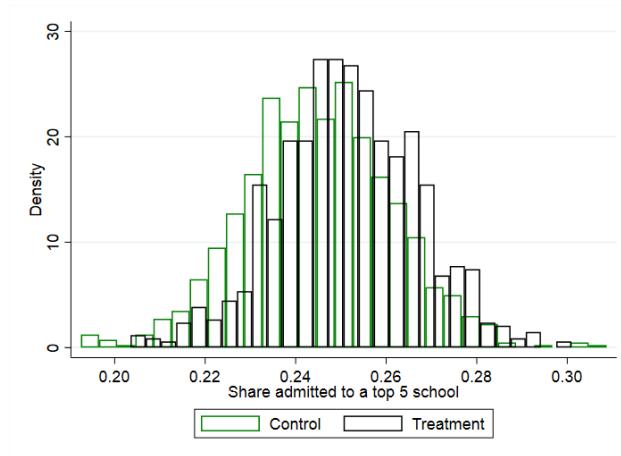
(b) Other

Figure 10: **Loss of students**

(a) Share of Total Students Lost

(b) Share of Student Lost in Relation to Control

schools are not, in general, the schools with a low share of students with a foreign background from from which we, in Figure 7, documented a increased flow of chices into the top performing schools. Rather they are schools with a high share of students with a foreign background for witch we did not document such an increase. In fact, the two most unpopular schools that together account for about 75% of the total decrease both belong to the group of schools with a high share of students with foreign background. Hence, the decrease in enrollment in these schools can not be driven in changes in choice behavior. Rather, what is happening is that the increased flow of students from the schools with a low share of students with foreign background to the top performing schools caused by our treatment frees up capacity. This free capacity is then occupied, either directly or in the next step, by students trying to opt out if the unpopular schools. This clearly exemplifies the propagating effect that changes in assignment of some students can have on other students. This illiterates that an intervention like this does not only have to benefit the households that actively react.

4.3 Effects on Segregation

In this section we will look at the effects of our treatment on school segregation. Segregation between groups divided into organizational units (such as schools) can be measured using either measures of evenness or measures of exposure (Massey & Denton, 1988). Exposure measures are sensitive to the share of the minority in the population, something that evenness measures are generally not. As Allen & Vignoles (2007) points out the share of minority's is not something that educational policy can directly affect and therefore it seems reasonable to use a measure of evenness when evaluating the effects of information about school performance on school segregation.

We will follow Massey & Denton (1988) and use the Duncan Dissimilarity Index as our main measure of evenness. The Dissimilarity Index (D) is given by:[37]

$$D = \frac{1}{2} \sum_{i=1}^{n} \mid \frac{a_i}{a} - \frac{b_i}{b} \mid$$ (4)

Where N is the number of organizational units (in this case schools), a_i is the number of students from

---

[37]Duncan & Duncan (1955)

group A in school i, a is the total number of student from group A in all schools, b_i is the number of students from group B in school i and b is the total number of students from group B in all schools. The Dissimilarity Index ranges from zero to one and has a clear interpretation as the percentage of one of the two groups that would have to move to a different organizational unit (school) in order to produce a distribution in each organizational unit that matches the distribution of the entire population.

The Duncan Dissimilarity index fails the transfer principal (James & Taeuber (1985) and White (1986)). This means that it is insensitive to the redistribution of minority group members among organizational units with minority proportions above or below the overall minority proportion. Only transfers of minority members from units where they are over represented to units where they are under represented affect the value of the index. Given the flows of students that we observe we do not think that this should be a concern. To be sure we will however also report the Thiel Entorpy Index[38], another measure of evenness for which the transfer principle holds. The Thiel Entropy Index can be interpreted as the difference between the diversity (entropy) of the system and the weighted average diversity of individual organizational units, expressed as a fraction of the total diversity of the system Reardon & Firebaugh (2002).

Carrington & Troske (1997) show that the evenness measures are sensitive to randomness when the average size of the organizational unit is small. As we are interested in the difference in unevenness between two scenarios where the size of the organizational units are more or less the same we do not think this should be a big problem. As a safety measure we will however simulate a random assignment of students from the treatment group and the control group separately and compare the distribution of the D in these two cases to make sure that there is no underlying difference due to randomness. We also include a measure of exposure, isolation[39] to confirm that our results are not driven by this property of the evenness measures.

Figure 11 shows the results from our simulations on the school segregation in terms of foreign background. We can see that the Duncan Dissimilarity Index is considerably lower when we sample from the treatment group compared when we sample from the control group. It drops a total of 6.6 points from from 0.456 to 0.390. Figure 14 in 5 confirms that these effects are not dependent on our choice of the Duncan Dissimilarity Index as our measure of segregation and that they are not due to randomness.

Given the interpretation of the Duncan Dissimilarity Index this drop of 6.6 points can not be explained by the three percentage point increase of non-foreign students assigned to the top performing schools. In Section 4.2 we did however establish that there was a secondary effect on assignment as these student moved out of their schools (with a low share of students with foreign background) and freed up capacity there. If this free capacity was, at least in part occupied by students with a foreign background it could explain part of the observed drop in segregation. In Figure 12 we report the simulation results for how the share of students with foreign background assigned to schools with a low share of students with a foreign background is affected by our treatment. We see a significant increase of 3.3 percentage points (from 25,6 % to 28,9 %). This effect combined with the increased share of non-foreign students in the group of top performing school corresponds well to the total observed decrease in the the Duncan Dissimilarity Index.

We now move on to school segregation in terms of parental skill level. Figure 13 displays the effects on the Duncan dissimilarity index. There is a small decrease of 1.3 points (from 0.276 to 0.263). This effect is however not robust to the choice of segregation measure as can be seen in Figure 15 in 5.

Table 11 summarizes these findings.

---

[38]Originally proposed by Thiel & Finezza (1971)
[39]See Lieberson (1980) for a description

Figure 11:

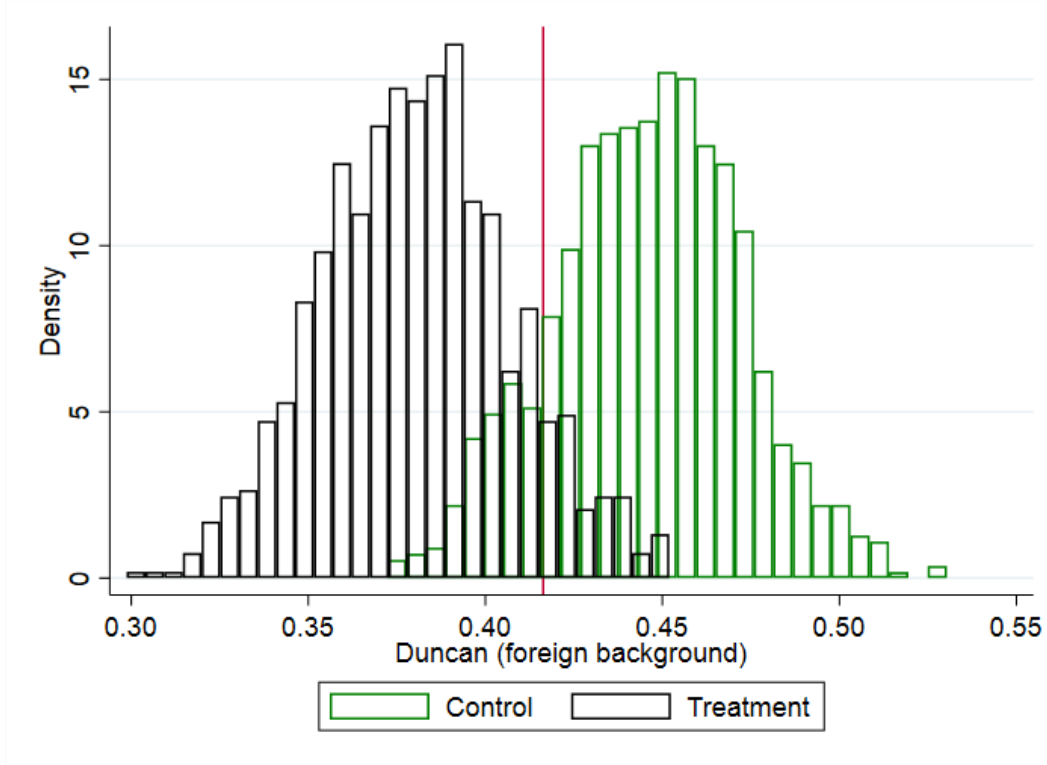Duncan Dissimilarity Index (Foreign Background)

Table 11: **Effects on Segregation**

	Control	Treatment	Difference (Treatment - Control)
(a): Foreign background			
Duncan	0.445	0.380	-0.066
Thiel	0.223	0.176	-0.057
Isolation	0.407	0.371	-0.036
(a): Parental Skill-Level			
Duncan	0.276	0.262	-0.013
Thiel	0.08	0.091	0.011
Isolation	0.306	0.300	-0.006

Figure 12: **Share of Students with Foreign Background Attending Schools with a low Share of Students with Foreign Background**
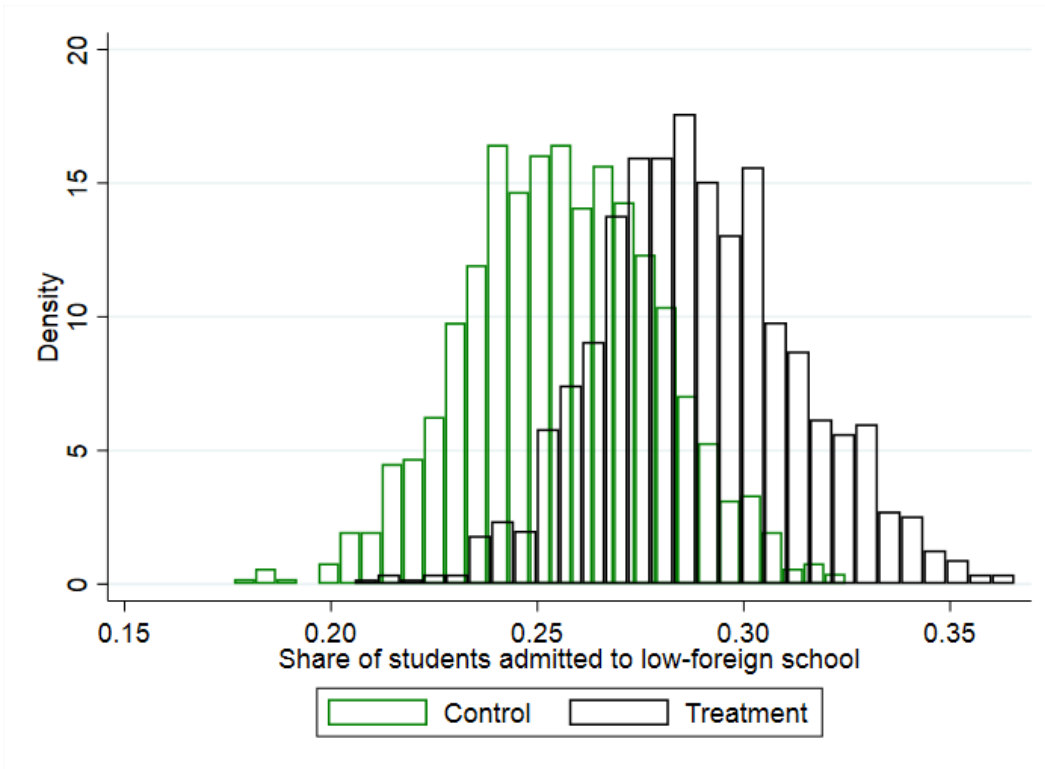
Figure 13: **Duncan Dissimilarity Index (Parental Skill-level)**
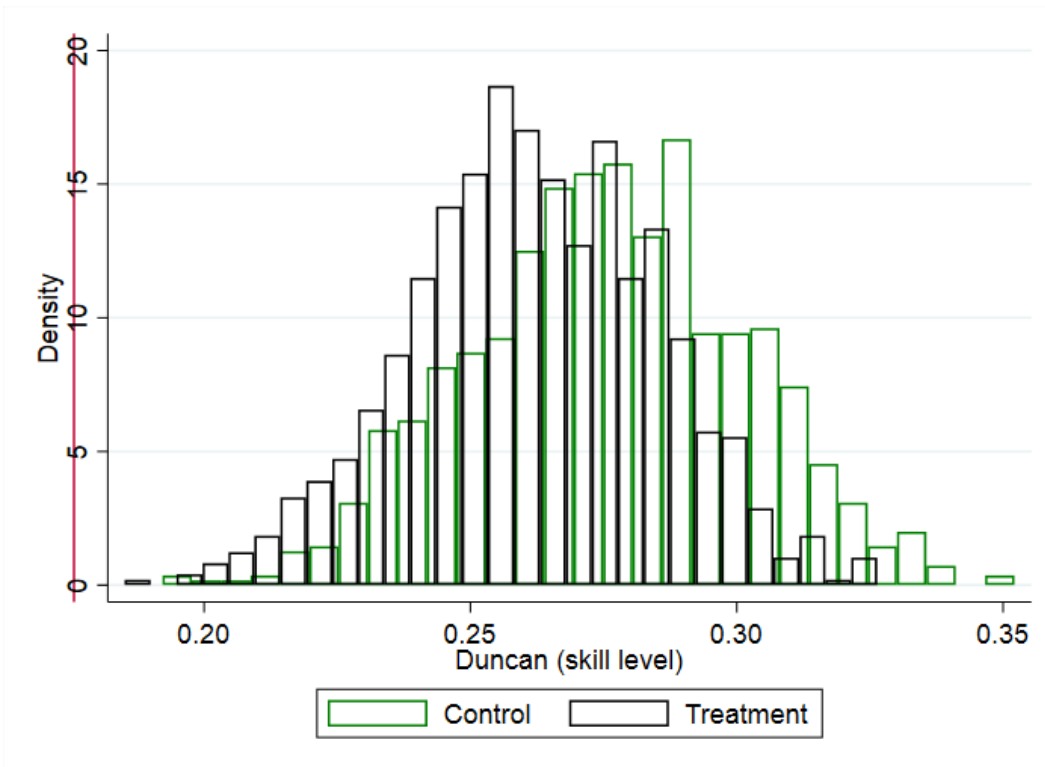
5 Conclusion

In this paper we report the results from a randomized control trial where we distributed information on absolute and adjusted (for student composition) school performance to households in Swedish municipality about to choose a middle school.

We find that this information does affect school choice behavior. The effects are heterogeneous and mainly driven by high-skilled households with a non-foreign background. The changes in the choice behavior of these households also only seems to take place on the margin between the high performing schools and the schools with a low share of students with foreign background. Further, we only find a reaction in terms of adjusted performance of the schools chosen. This indicates that school performance information can have a effect on the competitive pressure on schools to improve performance. It also suggests that there is something more going on than just a general lack of information. It could be explained by differences in strength of priors between groups but also by differences in relative preferences for performance and composition. In this paper we do not have the tools to uncover this but we do think that future studies on this subject would be benefited by a strategy that allows them to elicit credible measures of preferences, prior beliefs on how schools perform or both.

Using simulations we find that the documented changes in school choice behavior translates into effects on the assignment of the households that did alter their choice behavior. Limited capacity in the schools do however seem to mute this effect. This suggest that it can be important to couple these kinds of information interventions with a readiness to, in different ways, expand capacity at the schools for which demand increases. We find no indications of students being displaced but we find that the changes in assignment that take place propagates through the system as capacity frees up, thereby also potentially benefiting students that did not react to the information. Further, we find that our intervention, through similar mechanisms, significantly reduces segregation in terms of foreign background.

The external validity of these results can be discussed as our results might be dependent on their being high preforming schools with a considerable share of visible minorities in the system. We are also evaluating the role of information on school choice behavior in a setting where there are strategic incentives for parents to consider when choosing schools. This might dull the effect of our treatment on the choice behavior. Studies in other settings would therefore be valuable. However, providing households with information on how schools perform is cheep and non-intrusive intervention with no documented negative effects and hence should not be avoided by policy-makers.

.

References
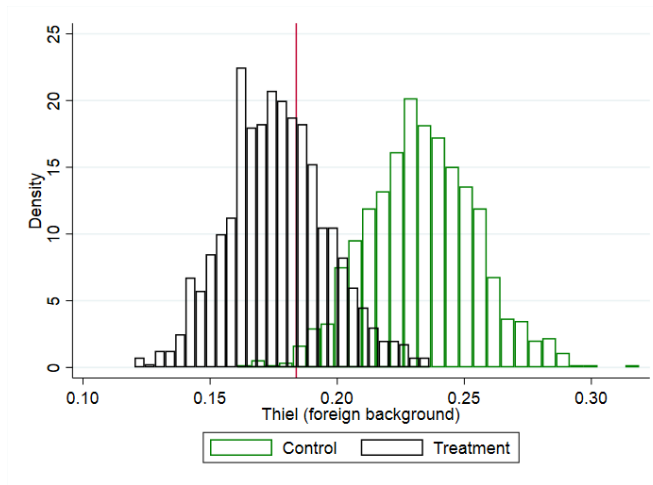
Abadie, A., Athey, S., Imbens, G. W., & Woolridge, J. 2017. When Should You Adjust Standard Errors for Clustering? *Unpublished Manuscript, MIT.*

Abdulkadiroglu, A., & Sönmez, T. 2003. School Choice: A Mechanism Design Approach. *American Economic Review*, **93**(3), 729–747.

Abdulkadiroglu, A., Pathak, P. A., & Walters, C. R. 2017. Free to Choose: Can School Choice Reduce Student Achievement? *American Economic Journal: Applied Economics (Forhtcoming).*

Allen, R., & Vignoles, A. 2007. What should an index of school segregation measure? *Oxford Review of Education*, **33**(5), 643–668.

Andrabi, T., Das, J., & Khwaja, A. I. 2017. Report Cards: The Impact of Providing School and Child Test Scores on Educational Markets. *American Economic Review*, **6**(107), 1535–1563.

Bifulco, R., & Ladd, H. F. 2007. School choice, racial segregation, and test-score gaps: Evidence from North Carolina's charter school program. *Journal of Policy Analysis and Management*, **26**(1), 588–638.

Böhlmark, A., & Lindahl, M. 2012. Independent schools and long-run educational outcomes evidence from Sweden s large scale voucher reform. *IZA Discussion Paper*, **6683**.

Burgess, S., Greaves, E., Vignoles, A., & Wilson, D. 2016. What Parents Want: School Preferences and School Choice. *The Economic Journal*, **125**(587), 1262–1289.

Calsamiglia, C, & Guell, M. 2014. The Illusion of School Choice: Evidence from Barcelona. *IZA Discussion Paper*, **8202**.

Carrington, W. J., & Troske, K. R. 1997. On measuring segregation in samples with small units. *Journal of Business & Economic Statistics*, **15**(4), 402–409.

Cullen, J. B., Jacob, B. A., & Levitt, S. 2006. The Effect of School Choice on Participants: Evidence from Randomized Lotteries. *Econometrica*, **74**(5), 11911230.

Duncan, O. D., & Duncan, B. 1955. A Methodological Analysis of Segregation Indexes. *American Sociological Review*, **20**(2), 210–217.

Epple, D.., Romano, R., & Urquiola, M. 2017. School Vouchers: A Survey of the Economics Literature. *Journal of Economic Literature*, **55**(3), 441–492.

Fack, G., Grenet J., & He, Y. 2015. Beyond Truth-Telling: Preference Estimation with Centralized School Choice. *Paris School of Economics Working Paper*, **35**.

Figalo, D. N., & Maurice, E. L. 2004. What's in a Grade? School Report Cards and the Housing Market. *American Economic Review*, **94**(3), 591–604.

Fiva, J. H., & Kirkeboen, L. J. 2011. Information Shocks and the Dynamics of the Housing Market. *The Scendinavian Journal of Economics*, **113**(3), 525–552.

Gallego, A. F., Lagos, F, & Y., Stekel. 2012. Types of Information and School Choice: An Experimental Study in the Chilean Voucher System. *Unpublished Manuscript, Pontificia Universidad Catolica de Chile.*

Hastings, J. S, & Weinstein, J. M. 2008. Information, School Choice, and Academic Achievement: Evidence from Two Experiments. *Quarterly Journal of Economics*, **123**(4), 1373–1414.

Hastings, J. S., Kane, T. J., & Staiger, D. O. 2007. Heterogeneous Preferences and the Efficacy of Public School Choice. *NBER working paper*, **12995**.

Hoxby, C. 2000. Does Competition Among Public Schools Benefit Students and Taxpayers? . *The American Economic Review*, **90**(5), 1209–1238.

Hsieh, C-T., & Urquiola, M. 2006. The effects of generalized school choice on achievement and stratification: Evidence from Chile's school voucher program. *Journal of Public Economics*, 1477–1503.

Hussain, I. 2013. Not Just Test Scores: Parents' Demand Response to School Quality Information. *Unpublished manuscript, University of Sussex.*

Imberman, S. A., & Lovenheim, M. F. 2016. Does the market value value-added? Evidence from housing prices after a public release of school and teacher value-added . *NBER Working Paper*, **19157**.

James, D. R., & Taeuber, K. E. 1985. Measures of Segregation. *Sociological Methodology*, **15**, 1–32.

Kane, T. J., & Staiger, D. O. 2002. The promise and pitfalls of using imprecise school accountability measures. *Journal of Economic Perspectives*, **16**(4), 91–114.

Koning, P., & van der Wiel, K. 2013. Ranking the Schools: How School-Quality Information Affects School Choice in the Netherlands. *Journal of the European Economic Association*, **11**(2), 466–493.

Lavy, V. 2010. Effects of Free Choice Among Public Schools. *The Review of Economic Studies*, **77**(3), 1164–1191.

Lavy, V. 2015. The Long-Term Consequences of Free School Choice. *NBER Working Paper*, **20843**.

Lieberson, S. 1980. A piece of the pie: Blacks and white immigrants since 1880. *Berkeley and Los Angeles: University of California Press.*

Massey, D. S, & Denton, N. A. 1988. The Dimensions of Residential Segregation. *Social Forces*, **67**(3), 281–315.

Musset, P. 2012. School Choice and Equity: Current Policies in OECD Countries and a Literature Review. *OECD Education Working Papers*, **66**.

Reardon, S. F., & Firebaugh, G. 2002. Measures of MultiGroup Segregation. *Sociological Methodology*, **31**(1), 33–67.

Ruijs, N. M., & Oosterbeek, H. 2014. School Choice in Amsterdam: Which Schools are Chosen when School Choice is Free? *Education Finance and Policy*, **0**, 1–64.

Söderström, M., & Uusitalo, R. 2010. School Choice and Segregation: Evidence from an Admission Reform. *The Scandinavian Journal of Economics*, **112**(1), 55–76.

Thiel, H., & Finezza, A. J. 1971. A note on the measurement of racial integration of schools by means of informational concepts. *Journal of Mathematical Sociology*, **1**, 33–67.

White, M. J. 1986. Segregation and Diversity Measures in Population Distribution. *Population Index*, **52**, 198–221.

Young, A. 2017. Channelling Fisher: Randomization Tests and the Statistical Insignificance of Seemingly Significant Experimental Results. *Unpublished Manuscript, LSE.*
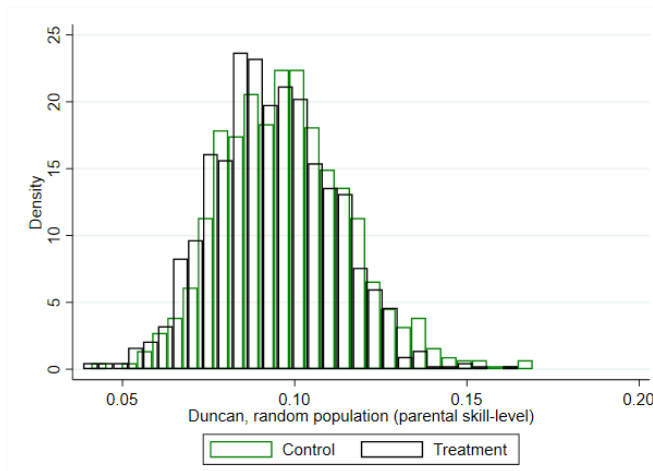
Appendix A

Figure 14: **Robustness (Foreign Background)**
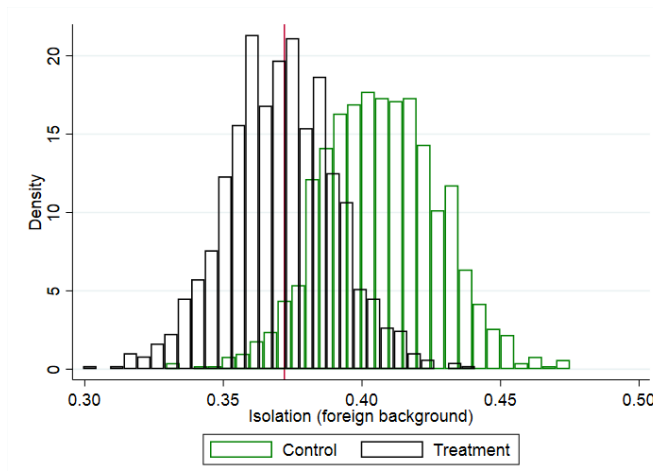
(a) Thiel Entropy Index

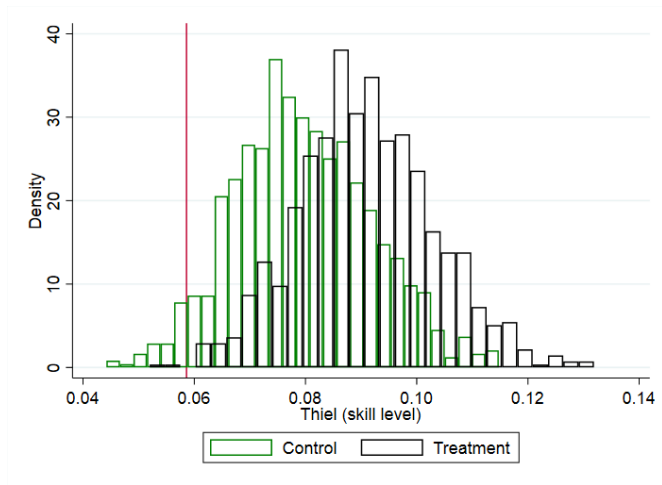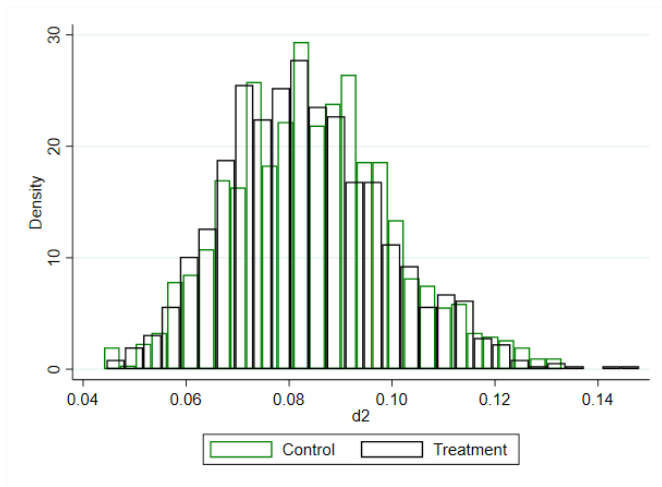## (b) Duncan (random population)

(c) Isolation

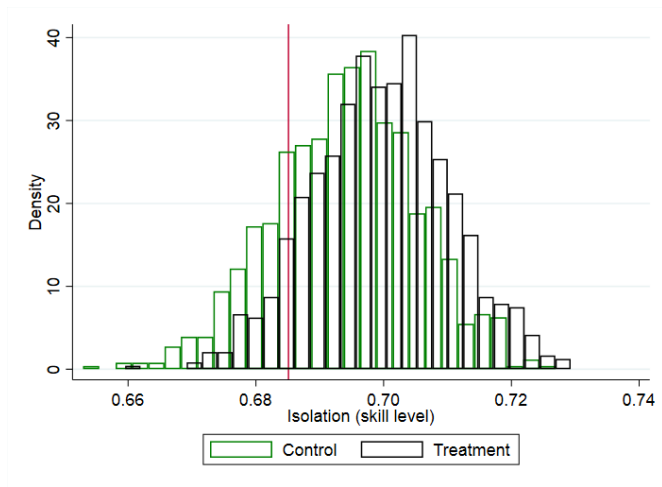Figure 15: **Robustness (Parental Skill-level)**

(a) Thiel Entropy Index

a

(b) Duncan (random population)

a

(c) Isolation

a